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

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April 2022

Abstract

We present a test of Coasean theories of efficient separations. We study a cohort of jobs from the introduction through the repeal of a large age- and region-specific unemployment benefit extension in Austria. In the treatment group, 18.5% fewer jobs survive the program period. According to the Coasean view, the destroyed marginal jobs had low joint surplus. Hence, after the repeal, the treatment survivors should be more resilient than the ineligible control group survivors. Strikingly, the two groups instead exhibit identical post-repeal separation behavior. We provide, and find suggestive evidence consistent with, an alternative model in which wage rigidity drives the inefficient separation dynamics.

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We thank the editor and three anonymous reviewers, as well as Daron Acemoglu, Josh Angrist, David Autor, Mark Bils, Steve Davis, Mike Elsby, Leo Kaas (discussant), Philipp Kircher, Patrick Kline, Moritz Kuhn, Guido Menzio, Thijs van Rens (discussant), as well as audiences at conferences at the ASSA Meeting, briq Workshop, ECB/CEPR Labour Market Workshop, London-Paris Public Economics Conference, CEPR/IZA Annual Symposium, CEPR Public Economics Symposium, MILLS, Ammersee Workshop, NBER SI Macro Perspectives, NBER Labor Studies, NBER Public Economics, NBER Aging Program, NBER Longer Working Lives Workshop, NYU Search Theory Workshop, SED, CESifo Venice, and at seminars at Banca d'Italia, the Philadelphia Fed, Harvard, IAB, INSEAD, U Mannheim, UC Berkeley, UC Santa Barbara, University College London, and U Penn. Karl Aspelund, Carolin Baum, Nikhil Basavappa, Dominik Egloff, Raymond Han, Andreas Kettemann, René Livas, Nelson Mesker, Shakked Noy, Damian Osterwalder, Nina Roussille, Philippe Ruh, Martina Uccioli, Samuel Young, and Dalton Zhang provided excellent research assistance. Jäger and Schoefer acknowledge financial support from the Boston Retirement Research Center, the National Science Foundation, and the Sloan Foundation. The RBC grant requires the following disclaimer: "The research reported herein was performed pursuant to a grant from the U.S. Social Security Administration (SSA) funded as part of the Retirement Research Consortium. The opinions and conclusions expressed are solely those of the authors and do not represent the opinions or policy of SSA or any agency of the Federal Government. Neither the United States Government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of the contents of this report. Reference herein to any specific commercial product, process or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply endorsement, recommendation or favoring by the United States Government or any agency thereof."

1 Introduction

Coasean theories of jobs assume that an employer and worker exploit all gains from trade and reach bilaterally efficient outcomes, splitting joint job surplus through unrestricted transferable-utility compensation arrangements. All job separations are mutually preferable, occurring if and only if joint surplus would otherwise turn negative. Due to its theoretical appeal, bilateral efficiency remains the dominant assumption in labor market models. Conversely, non-Coasean frictions such as wage rigidity that can cause inefficient separations are often dismissed *a priori* exactly due to the plausibility of efficient bilateral contracting (starting with Barro, 1977; Becker, Landes, and Michael, 1977), although departures from bilateral efficiency can be microfounded (e.g., Hall and Lazear, 1984). The same properties that underlie the theoretical appeal of the Coasean hypothesis have shielded it from empirical tests. First, the abstract concept of surplus is not observable, let alone the counterfactual surplus of a terminated job. Second, the observable consequences of separations need not be informative about bilateral efficiency. For example, although layoffs leave workers dramatically worse off than quits, both labels can reflect efficient separations (McLaughlin, 1991). Third, even fixed flow wages can reflect efficient bargaining, which, in theory, can involve complicated, e.g., present value, payments, and only requires adjustment if the flow wage falls outside of the parties' reservation wages (MacLeod and Malcomson, 1993; Hall, 2005; Cahuc, Postel-Vinay, and Robin, 2006).[1]

We overcome these challenges with a revealed-preference test of group-level separations using a quasi-experimental research design. We study a transitory treatment that, while active, reduces joint job surplus and thereby causes separations of initially low-surplus jobs. The treatment is then sharply repealed. Post-repeal, the group of surviving, formerly treated jobs lacks a mass of marginal (low-surplus) matches. Under the Coasean view, this group of treatment survivors should subsequently exhibit resilience to *any* kind of shock compared to a control group, in which this set of low-surplus jobs has remained.

Our treatment reducing joint job surplus is an unemployment insurance (UI) benefit extension, which boosted workers' outside option (nonemployment). Specifically, the program raised potential benefit duration from originally one to four years in Austria in 1988. Since eligibility was determined by a sharp age cutoff (age 50 and up) and the program was region-specific, we implement a difference-in-differences design comparing age groups and regions in the universe of Austrian social security data. Crucially, the program was abruptly repealed in 1993, which permits our test: *after the program repeal*, the group of formerly treated job survivors should be more resilient—i.e., have fewer

[1]Thus, although wages in Austria may appear insensitive to (nonemployment) outside options (Jäger, Schoefer, Young, and Zweimüller, 2020), such insensitivity need not be allocative for separations.

separations—in response to *any* future shocks, compared to the control group.

Our first step documents that the program raised separations by 11.0ppt (27%) over its five-year period: 51.7% of jobs in the treatment group separated (largely into long-term nonemployment), compared to a counterfactual separation rate of 40.8% absent the reform.[2] That is, 18.5% of the surviving jobs in the control group would have separated had the group also been exposed to the UI extension.

In our second step, we exploit the abrupt repeal of the policy in 1993. We track the jobs active both already at the onset of the program in 1988 and still active at the repeal ("survivors"). The repeal realigns the surplus distributions among survivors between the former treatment and control groups—except that the treatment group now features a missing mass of marginal matches, the additional separators, who are still present in the control group. By the Coasean view, these marginal jobs have joint surplus ranging between zero and a cutoff value (equal to that of the UI extension). They should be the first to separate in the control group, ahead of any inframarginal treatment group survivors.

Strikingly—and inconsistent with the Coasean prediction—the two groups exhibit *identical* post-repeal separation behavior in the data. The absence of resilience holds unconditionally as well as in response to negative labor demand events.

To quantify the gap between the Coasean prediction and the data, we construct benchmarks for post-repeal separations. Our simplest benchmark exploits the Coasean pecking order of jobs given by their ranking according to joint job surplus. For small post-repeal aggregate surplus shocks, separations should occur in the control group but not the former treatment group. There, separations should only start once the control group post-repeal separation rate crosses the threshold given by the treatment effect size of the initial UI extension. The treatment effect was large, so this Coasean benchmark predicts substantial resilience, which the data reject.

We also consider Coasean alternatives in which, after the repeal, idiosyncratic surplus shocks may partially replenish the mass of marginal jobs in the treatment group. In the most extreme theoretical case of "reshuffling," no resilience emerges because these shocks fully realign surplus across both groups already within the first post-repeal year—an implausibly strong assumption. More realistic cases, such as large idiosyncratic shocks or processes calibrated to match control group separations, still predict substantial resilience.

To account for the observed separation dynamics, we propose a non-Coasean model

[2] Important existing work has documented the initial separations effect of the reform we study. Winter-Ebmer (2003) and Lalive and Zweimüller (2004) study inflow effects of the program. Lalive, Landais, and Zweimüller (2015), who primarily focus on job finding spillovers among the unemployed during the policy period, also include separations as an outcome (Table 3). However, the existing literature on Austrian UI has not documented our core new fact (the post-repeal resilience of surviving matches), or assessed the efficiency properties of the separations induced by the reform.

featuring wage rigidity, specifically, frictions that prevent wage differentiation between similar workers (above versus below the age threshold for policy eligibility).[3] By preventing flexible transfers of utility, wage rigidity leads to separations when either worker *or* firm surplus would turn negative, rather than joint surplus (as in the Coasean world). Here, the UI reform would have destroyed matches with initially low *worker* surplus, while potentially leaving behind many matches with low *firm* surplus. The model rationalizes identical post-repeal separation dynamics in the treatment and control groups if, for example, post-repeal separations are largely driven by shocks to firm surplus and the correlation between worker and firm surplus is limited. This configuration of surplus is particularly plausible in our setting and sample of older workers, for example under models of compensation back-loading or employer competition (Lazear, 1979, 1981; Cahuc, Postel-Vinay, and Robin, 2006; Frimmel, Horvath, Schnalzenberger, and Winter-Ebmer, 2018), and given that Austria mandates generous severance payments for long-tenured workers that are foregone in unilateral quits, which raises workers' inside job value.[4]

Consistent with the model with wage rigidity, our key findings (the policy's initial separation effects followed by non-resilience) stem from high wage rigidity pockets of the labor market (e.g., firms with homogeneous wage growth). That said, separation-relevant wage rigidity is hard to measure (which motivated our approach to begin with) and our proxies are correlated with other potentially relevant variables (e.g., tenure, blue collar). This analysis also highlights that our diagnosis of inefficient separations is limited both to our specific sample (i.e., older workers with high tenure) and to the compliers therein (e.g., with rigid wages), rather than extending to all separations in the Austrian labor market.

Section 2 reviews the institutional context, policy, and data. Section 3 presents our Coasean and non-Coasean benchmark models. Section 4 reports the large separation effects from the UI extension. Section 5 reports our core test comparing the post-repeal separations in the former treatment and control groups. Section 6 discusses alternative Coasean models. Section 7 explores wage rigidity as a resolution. Section 8 concludes.

2 Institutional Context, the Policy Variation, and Data

We review the UI reform, other aspects of the institutional context, and our data.

[3]Models with wage posting or pay equity norms feature such frictions, consistent with empirical evidence (Albrecht and Axell, 1984; Card, Mas, Moretti, and Saez, 2012; Card, Cardoso, Heining, and Kline, 2018; Saez, Schoefer, and Seim, 2019; Flinn and Mullins, 2018; Dube, Giuliano, and Leonard, 2019; Di Addario, Kline, Saggio, and Solvsten, 2020; Drenik, Jäger, Plotkin, and Schoefer, 2020; Jäger, Schoefer, Young, and Zweimüller, 2020).

[4]The associated prediction that smaller UI shifts should not trigger separations even among older workers during the 1980s in Austria is documented in Jäger, Schoefer, Young, and Zweimüller (2020).

2.1 The Austrian UI System and the UI Benefit Extension

In 1988, the Austrian government enacted a regional extended benefit program (REBP), a large region- and age-specific expansion of the potential benefit duration (PBD) of UI benefits. PBD increased from 20-30 weeks (pre-reform) to 209 weeks (post-reform) for affected workers.[5] Since the gross (not taxed) replacement rate of UI benefits both before and after the reform was between 40 and 48% of salary for most employees (see Jäger, Schoefer, Young, and Zweimüller, 2020), we ballpark the cash present value of the extension to about 71% of a typical worker's annual salary in Appendix A. Figure 1 Panel (a) summarizes the reform by plotting PBD by age group and region over time.

The Austrian UI system and the program make for a particularly suitable setting for our purposes. First, the program cleanly shifted the outside option of affected workers by substantially increasing PBD. Importantly, Austrian workers are *fully eligible for UI benefits upon quitting* after a four-week waiting period. The reform left other institutional features, such as UI payroll taxes, unchanged (and there is no experience rating).

Second, REBP's eligibility criteria induced variation along two dimensions (age and region), permitting a difference-in-differences (DiD) design: workers had to (i) be age 50 or older (at the beginning of the unemployment spell); (ii) have worked at least 780 weeks during the 25 years prior to the spell; (iii) have resided in the REBP districts for at least 6 months prior to the claim; and (iv) start their new unemployment spell after June 1988 or have a spell in progress in June 1988. Our DiD design controls for unobservable confounders at the region and cohort level. We net out regional shocks (including market-level effects of the reform) by comparing workers narrowly above or below the age threshold *in the same region*. We net out age- or cohort-specific factors by comparing the same cohorts across REBP and non-REBP regions.[6]

REBP aimed to mitigate the labor market consequences of a crisis in the steel sector (iron, steel, and other heavy industries), including the restructuring of the large, state-owned Oesterreichische Industrie AG (OeIAG). The REBP regions—depicted in Figure 1 Panel (b)—were selected due to their larger share of employment in the steel sector, about 17%, compared to around 5% in the non-REBP regions. Importantly, REBP eligibility did

[5]The PDB during the 1980s was 30 weeks, provided the worker had been employed (and paid UI contributions) for at least three out of the last five years; otherwise, 20 weeks. After exhaustion of UI, both before and after the REBP reform, the unemployed could apply for unemployment assistance (UA, "Notstandshilfe"), capped at 92 percent of UI benefits (detailed in Appendix A).

[6]The cross-regional difference also nets out a 1989 reform that *nationally* raised PBD to 39 (52) for workers aged 40 to 49 (50 and above) weeks and with 312 (468) weeks of employment in the last 10 (15) years. For job losers from August 1989 onward, REBP's incremental effect on PBD was then 3 years (from a 52 week baseline) and before August 1989 it was 3.44 years (from 30 weeks). The reform also increased the replacement rate from 41 to 47% for monthly incomes 5,000 to 10,000 ATS (400 to 800 USD at the time).

not include any industry requirement. Nevertheless, to minimize UI policy endogeneity concerns, our empirical analysis excludes steel sector employees. Moreover, the second difference (between slightly younger, ineligible cohorts in the REBP versus non-REBP regions) nets out any potential spillovers from the steel sector decline, or other region-specific shocks or trends. We further evaluate potential spillovers in Section 6.2.

Repeal of the Program REBP was initially in effect until December 1991 before it was extended in January 1992.[7] REBP was then repealed on August 1, 1993, stopping acceptance of new entrants yet also grandfathering in claimants in ongoing spells who had previously established eligibility. In addition, a grandfathering clause (§81) covered separations occurring post-repeal due to an advance notice period; empirically, we thus analyze post-repeal resilience starting in 1994q1. The repeal decision was formally announced in June 1993, and implemented only two months later. The program ended abruptly: as late as January 1993, the Austrian government had considered expanding the program to older workers in the entire country, along with changes in the eligibility requirements.[8]

2.2 Other Institutional Features

Wage Setting While collective bargaining coverage in Austria is nearly universal, it leaves substantial room for decentralized, flexible wage setting. Bargaining agreements, often concluded at the industry-by-occupation level, regulate wage floors for worker categories, usually by experience or tenure (but not age). However, actually paid wages substantially exceed the wage floors, e.g., by more than 20% in manufacturing during our reform period (Leoni and Pollan, 2011). There is also substantial scope for wage differentiation between firms within an industry, as evidenced by individual firms sharing rents with workers and large pay dispersion between firms (Jäger, Schoefer, Young, and Zweimüller, 2020). At the individual worker level, downward nominal wage rigidity appears lower or similar compared to, e.g., Germany or the United States (Dickens, Goette, Groshen, Holden, Messina, Schweitzer, Turunen, and Ward, 2007; Elsby and Solon, 2019). In our empirical analysis, we include a heterogeneity analysis by wage flexibility proxies.

Interaction of UI with Other Social Policies By interacting with other policies, REBP

[7]For new spells, the 1992 extension repealed eligibility in 6 of the 28 regions—which we exclude from our analysis. It also tightened eligibility criteria from residence to previous employment in a treated region.

[8]We confirm this course of events in a newspaper analysis. For instance, a major newspaper (*Der Standard*) reported in an article entitled "*Länger Geld für alle Altersarbeitslosen* (Longer benefits for all older 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 [...]." (Our translation.)

could serve as a bridge into permanent nonemployment. In the absence of REBP, unemployed men could effectively retire early at age 58 by claiming UI for one year, special income support (equivalent to UI but 25 percent higher and paid for at most 12 months) for another, and then drawing a regular public pension at age 60 (male workers with at least 35 years of contributions). Hence, since REBP extended UI PBD by three years, eligible workers 55 and older could permanently withdraw from the labor force.

Disability insurance (DI) can also interact with UI to influence labor supply (Staubli, 2011). During the study period, relaxed access to a DI pension from age 55 onward allowed job losers in REBP regions to retire at age 51 while being on some kind of benefit until claiming their public pension at age 60. (Employability also played a role, as DI applicants below (above) age 55 received a DI pension when a health impairment reduced the work capacity by more than 50 percent in all (their original) occupation(s).) Inderbitzin, Staubli, and Zweimüller (2016) study effects of the program on DI entry.

Advance Notice for Layoffs, Works Councils, and Severance Pay While employment protection was not as stringent as in many other countries, layoffs were subject to a set of rules. At the time of REBP, the firm's advance notice requirement was 5 (4, 3, 2, 1.5) months for workers with at least 25 (15, 5, 2, 0) years of tenure, and the firm had to inform and consult the works council (potentially present in establishments with 5 or more workers) about planned layoffs. Severance payments (further discussed in Section 6.2 and Appendix B) were mandated for all separation types except for dismissals for cause, unilateral worker quits, and quits into retirement with fewer than ten years of tenure. The amount was a step function of worker tenure: < 3 (3, 5, 10, 15, 20, 25) years of tenure mapped into 0 (2, 3, 4, 6, 9, 12) monthly salaries.

2.3 Data and Sample

Our main dataset is the Austrian Social Security Database (ASSD), matched employer-employee data covering the universe of private-sector, dependently employed and non-tenured public sector employees from 1972 onward (Zweimüller, Winter-Ebmer, Lalive, Kuhn, Wuellrich, Ruf, and Buchi, 2009; Austrian Social Security Database, 1972-2016). Our sample are workers born between 1933 and 1948, as older cohorts had already reached the regular retirement age at the repeal of REBP. Our slightly younger control cohorts are born between 1943 and 1948, and are younger than 50 at the repeal in 1993. We drop women, because their experience data (below) are unreliable, and they could already retire at age 55. Table 1 reports summary statistics.

We assign workers to REBP or control regions by the location of their establishment and, if missing, their residence (based on data from the Austrian employment agency). We drop the six regions where REBP was repealed early, in 1991 rather than 1993 (partial treatment regions in Figure 1 Panel (b)). We also drop the steel sector, which the reform targeted. To broadly rule out remaining concerns related to the steel sector, we show that our results extend to a variety of industries in Appendix Figure A.13, and study growing and shrinking industries separately in Appendix Figure A.14. Moreover, the difference-in-difference design compares slightly older and younger workers in the same region and thus nets out region-specific shocks. We further discuss potential spillovers in Section 6.2. To measure worker experience with pre-1972 data, we draw on data from the Austrian Ministry of Social Affairs (AMS). The vast majority of our sample fulfilled the experience requirement (see the last two columns in Appendix Table A.1); since this sample restriction does not affect our estimates, we present the unconditional results.

3 Deriving the Test of the Coasean Model: Resilience from Missing Mass of Marginal Matches

We set up the Coasean framework and derive its key prediction: resilience to shocks following the repeal of the large, separation-inducing UI extension. We also sketch an alternative, non-Coasean model with wage rigidity that accommodates non-resilience.

3.1 Coasean Benchmark

We provide the setup and the main derivations here, with details in Appendix C.

3.1.1 Bilaterally Efficient Bargaining

Jobs and Surplus Jobs (worker-firm matches) carry worker surplus S^W and firm surplus S^F, each of which consists of the party $i \in \{W, F\}$'s inside job value V^i_{In} (amenities, productivity,...), plus/minus wage w (with which the parties transfer utility in terms of, e.g., present values), minus the outside value from separating V^i_{Out} (unemployment, retirement, working for another firm, the value of a vacancy and hiring another worker,...):

$$S^W(w, \mathbf{V}^W) = V^W_{\text{In}} + w - V^W_{\text{Out}} \geq 0, \tag{1}$$

$$S^F(w, \mathbf{V}^F) = V^F_{\text{In}} - w - V^F_{\text{Out}} \geq 0, \tag{2}$$

where $\mathbf{V}^i = (V^i_a)_{a \in \{\text{In,Out}\}}$, and we also use $\mathbf{V} = (\mathbf{V}^i)_{i \in \{W,F\}}$.

At a *given* wage level, a job is feasible if both parties enjoy non-negative surplus. If worker surplus is negative while firm surplus is non-negative, the job will end in a quit; in the reverse case, the job will end in a layoff; and if both surpluses are negative the job will end in a mutual separation. Figure 2 illustrates these intuitions with various case studies (listed in the figure note) of jobs characterized by different worker surplus (x-axis) and firm surplus (y-axis) combinations. The *solid* circles (•) denote *gross-of-wage surpluses*, i.e., $V^W_{\text{In}} - V^W_{\text{Out}}$ for the worker and $V^F_{\text{In}} - V^F_{\text{Out}}$ for the firm. This term is the surplus combination these job "fundamentals" would carry 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 achieve transfers of utility that move net surpluses of the parties along 135-degree, iso-joint-surplus lines.

Figure 2 also partitions jobs into four regions: feasible jobs (top right, solid lines), quits (top left, dashed lines), layoffs (bottom right, dotted lines) and mutual separations (bottom left, dot-dash-patterned line). For a job to be viable net of the wage, it must be—at least after adjusting the wage—in the top right corner, providing positive surplus to both parties; separations occur in the other corners.

Coasean Bargaining The essence of the Coasean framework is bilateral efficiency through bargaining: all jobs with non-negative *net* surplus will be feasible because the parties can find a wage that transfers utility such that both worker and firm surplus end up non-negative. Formally, the parties choose a wage within the bargaining set of reservation wages $w \in [\underline{w}^W, \overline{w}^F]$, where $\underline{w}^W$ and $\overline{w}^F$ are such that $S^W(\underline{w}^W, \mathbf{V}^W) = 0$ and $S^F(\overline{w}^F, \mathbf{V}^F) = 0$. Such a choice is possible as long as joint surplus is non-negative (i.e., whenever $\overline{w}^F \geq \underline{w}^W$).[9] As a result, the two-dimensional surpluses that determine job viability and separations, Equations (1) and (2), collapse to a one-dimensional, single allocative concept of *joint* job surplus $S(\mathbf{V})$, defined as:

$$S(\mathbf{V}) = \overbrace{V^W_{\text{In}} + V^F_{\text{In}} - V^W_{\text{Out}} - V^F_{\text{Out}}}^{S^W(w,\mathbf{V}^W)+S^F(w,\mathbf{V}^F)}. \tag{3}$$

Any and only jobs with non-negative joint surplus are feasible with efficient bargaining; the wage splits the surplus to satisfy both participation constraints. Figure 2 illustrates how

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

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

such bargaining renders feasible all jobs born upwards or to the right of the marginal-jobs frontier, by moving jobs along the iso-joint-surplus curve.

Efficient Separations With Coasean bargaining, separations occur if and only if joint surplus becomes negative. To capture idiosyncratic shocks to specific matches, we assume job values evolve following a Markov process $k(\mathbf{V}'|\mathbf{V})$, where, going forward, x' denotes the next-period value of x. Then, for a job of value vector $\mathbf{V}$, the probability of separating next period is the probability of transitioning to job values $\mathbf{V}'$ that yield negative joint surplus. To consider *aggregate* (homogeneous) shocks (like the UI reform described below), we define $\widetilde{S}(\mathbf{V}')$ as the short-hand for the surplus level gross of some given aggregate surplus shifter $-\varepsilon' < 0$, such that, for an aggregate shock, $\widetilde{S}(\mathbf{V}', \varepsilon' = 0) = S(\mathbf{V}', \varepsilon') - \varepsilon'$ and $\widetilde{S}(\mathbf{V}) < \varepsilon' \Leftrightarrow S(\mathbf{V}', \varepsilon') < 0$. That is, a positive ε denotes a *negative* surplus shock, and separations occur if $\widetilde{S}(\mathbf{V})$ falls short of ε—the separation cutoff. Due to Coasean bargaining, the incidence of shocks on the worker or firm does not matter, so we consider the sum of the shocks $\varepsilon' = \varepsilon^{W'} + \varepsilon^{F'}$. Hence, the job-level separation probability in the face of idiosyncratic shocks k and an aggregate shock ε' is:

$$\widetilde{\mathbb{d}}(\mathbf{V}, \varepsilon') = \int_{\mathbf{V}'} \mathbb{1}(\widetilde{S}(\mathbf{V}') < \varepsilon') k(\mathbf{V}'|\mathbf{V}) d\mathbf{V}'. \tag{4}$$

Group-Level Separations Figure 3 Panel (a) plots an example distribution of joint surplus for intuition. Without loss of generality, we have normalized $\varepsilon' = 0$ for aggregate shocks absent REBP. Separations occur in the black portion, where jobs would yield negative surplus. Formally, the group-level separation rate is, for a given idiosyncratic shock distribution, a given aggregate shock and a given distribution of job attributes $f(.)$:

$$\delta = \int_{\mathbf{V}} \widetilde{\mathbb{d}}(\mathbf{V}, \varepsilon') f(\mathbf{V}) d\mathbf{V}. \tag{5}$$

3.1.2 The UI Extension (REBP)

Modeling UI Generosity We think of the REBP treatment as lowering joint surplus $\varepsilon_b^{W\prime} = V_{\text{Out}}^{W\prime}(b_0 + \Delta b) - V_{\text{Out}}^{W\prime}(b_0)$ primarily by improving the worker's outside option $V_{\text{Out}}^{W}(b)$, which is a function of UI generosity b. In the Austrian context described in Section 2, this approach is suitable as even quitting workers receive full benefits (after a brief waiting period), there is no experience rating, and UI take-up is high. We ballpark the cash value of extended benefits to 71% of a typical annual salary in Appendix A. In Section 6.2, we empirically evaluate whether heterogeneous valuations of UI could shroud resilience.

Treatment and Control Groups Our quasi-experimental study features a treatment ($Z = 1$) and a control ($Z = 0$) group, with UI generosity $b_Z = b_0 + Z \times \Delta b$ deviating from baseline b_0. Initial distributions of job values in each group, $f^Z(\cdot)$, are assumed to be the same for $Z = 1$ and $Z = 0$:[10] $f^0(\cdot) = f^1(\cdot)$.

Netting Out Equilibrium Effects In fact, our empirical DiD design has multiple control groups: eligible cohorts in the control region, and slightly younger (ineligible) workers in both regions. The slightly younger, untreated control group in the same region permits us to net out any equilibrium effects of REBP. The treatment is the *differential* exposure to the program on the outside option of treated workers, net of market-level effects. In our notation, we therefore suppress market-level or spillover effects. Additionally, as discussed in Section 6.2, we can test for and reject such effects on our results.

Separation Effects The incremental separations caused by REBP should stem from jobs with joint surplus between zero and the size of the REBP surplus shift. Figure 3 Panel (a) illustrates this logic. During REBP, all jobs with negative joint surplus $\widetilde{S}(\mathbf{V}') < 0$ (in the left, black area) separate in both regions. The gray set of *marginal jobs* have surplus $0 \leq \widetilde{S}(\mathbf{V}') < \varepsilon_b^{W\prime}$, and hence separate only if exposed to REBP. The remaining jobs—which survive in either group—have surplus $\widetilde{S}(\mathbf{V}') \geq \varepsilon_b^{W\prime}$. The figure also references separation rates for the treatment and control groups (δ^1 and δ^0).

3.1.3 Post-Repeal Prediction: Resilience

The repeal of REBP restores each surviving, treated match's surplus to the level of its peer in the control group. Except, the repeal does not bring back to life the previously destroyed jobs (since we track survivors only). We depict the surplus distributions of REBP survivors right after the repeal in Figure 3 Panels (d) and (g), separately for the former treatment and control groups. The former treatment group features a *missing mass of marginal matches*. By contrast, these low-surplus jobs remain in the former control group. This missing mass will persist until idiosyncratic shocks to joint surplus—discussed in Section 6.1—possibly replenish it by reshuffling the surplus distribution.

The testable prediction characterizing the Coasean view is that right after the REBP repeal, the formerly treated REBP survivors should exhibit fewer separations—*relative resilience*—in response to post-repeal shocks compared to the control group, where the marginal, low-surplus jobs have remained. Appendix Figure A.15 illustrates separations by group as a function of shock ε''. We denote post-repeal functions with capital letters:

[10] In our DiD design, this condition need not hold in levels but in between-cohort differences across regions. The original working paper featured an analysis of complier characteristics, empirically substantiating this assumption. See also Table 1 for summary statistics.

Δ for δ, K for k, and $\widetilde{\mathbb{D}}$ for $\widetilde{\mathbb{d}}$. Post-repeal aggregate shocks and job values are denoted by $''$ rather than $'$. Post-repeal separation rates in the treatment (control) group $Z = 1(= 0)$ are:

$$\Delta^Z = \int_{\mathbf{V}'} \underbrace{\int_{\mathbf{V}''} \mathbb{1}(\widetilde{S}(\mathbf{V}'') < \varepsilon'') K(\mathbf{V}''|\mathbf{V}') d\mathbf{V}''}_{\equiv \widetilde{\mathbb{D}}(\mathbf{V}', \varepsilon'')} f^Z_{\text{post}}(\mathbf{V}') d\mathbf{V}', \tag{6}$$

where $\widetilde{\mathbb{D}}(\mathbf{V}', \varepsilon'')$ denotes the post-repeal separation probability out of a job with attributes $\mathbf{V}'$ and given aggregate post-repeal shock ε''. Differential post-repeal separations reflect post-repeal distributional differences, f^Z_{post}, induced by selective separations from REBP—the missing mass of low-surplus matches.

3.1.4 Coasean Benchmark Without Idiosyncratic Shocks

The Coasean resilience prediction is especially tractable under the assumption that jobs experience only common aggregate shocks and no idiosyncratic changes in surplus during the post-repeal period. Intuitively, in this setting, the treatment group is perfectly resilient (exhibits no separations) as long as the subsequent aggregate shock size ε'' is smaller than the size of REBP, i.e., for $\varepsilon'' \leq \varepsilon^{W}_{b}{}'$. For larger shocks $\varepsilon'' > \varepsilon^{W}_{b}{}'$, separations start emerging even in the former treatment group, with the marginal REBP survivors carrying $\widetilde{S}(\mathbf{V}') = \varepsilon^{W}_{b}{}'$ being the first to separate. The leftmost panels of Figure 3, and Appendix Figure A.15, illustrate the intuitions. Appendix C details the derivations. In Section 6, we assess robustness to specific, plausible idiosyncratic shock processes.

Assumptions Formally, assuming no idiosyncratic shocks post-repeal means assuming the post-repeal surplus innovation process $K(.|.)$ is an identity matrix. In practice, we study post-repeal horizons as short as a single year (1994-95). Aggregate shocks ε'' drive post-repeal separations. Crucially, we place no restrictions on the idiosyncratic shock $k(.|.)$ *during* the five-year REBP period (although this discrete time setup permits only one shock). We also assume equality of initial job distributions, discussed in Section 3.1.2.

Predicted Separation Rates If we directly observed the REBP shock size $\varepsilon^{W}_{b}{}'$ and the post-repeal aggregate (homogeneous) surplus shocks ε'', we could simply compare realized post-repeal separations in the former treatment group against this Coasean benchmark. Yet, surplus and aggregate shocks are not directly observable. Instead, our empirical strategy draws inferences from the control group post-repeal separation rates, which encode the size of post-repeal shocks, and the (differential) during-REBP separation rates, which encode the size of the REBP surplus shock $\varepsilon^{W}_{b}{}'$.

In fact, for this case of no idiosyncratic shocks, we can express the post-repeal former treatment group separation rates (Δ^1) as a kinked, piece-wise linear function of that of the former control group (Δ^0), with slopes and kink positions given by (δ^0, δ^1):

$$\Delta^1(\Delta^0(\varepsilon''), \delta^0, \delta^1) = \max\left\{0, \frac{1-\delta^0}{1-\delta^1}\left[\Delta^0(\varepsilon'') - \frac{\delta^1-\delta^0}{1-\delta^0}\right]\right\}. \tag{7}$$

The position of the kink is given by $\Delta^0 = \frac{\delta^1-\delta^0}{1-\delta^0}$. As long as the control group post-repeal separation rate Δ^0 is lower than the fraction of marginal matches among the survivors $\frac{\delta^1-\delta^0}{1-\delta^0}$, no separations should occur in the treatment group, because these matches are missing. Once control group separations cross that threshold, separations commence in the former treatment group (with a slope steeper than one, $\frac{1-\delta^0}{1-\delta^1}$, because the incremental separator count is over a smaller count of survivors there). Both groups will have, on average, indistinguishable separation rates if all control jobs dissolve or if the initial REBP treatment effect is zero. Hence, the design has power if the initial treatment effect during REBP is large—shifting the kink far away from zero—and if Δ^0 is smaller than one. Appendix Figure A.15 illustrates this relationship.

Comparing the Coasean benchmark given by Equation (7) with the actual post-repeal separation rates constitutes our revealed-preference test—the contribution of our paper.

3.1.5 Preview of Alternative Coasean Benchmarks

To rationalize our findings of non-resilience, in Section 6 we will consider—but the evidence will ultimately reject—extensions of the Coasean model that allow idiosyncratic shocks, which may replenish the marginal jobs in the former treatment group. Our preferred explanation, outlined below, studies *inefficient* bargaining, due to wage rigidity.

3.2 A Non-Coasean Model With Wage Rigidity

Resilience need not emerge in non-Coasean models. Here, frictions prevent the efficient (re-)bargaining. We consider perfectly rigid (fixed) wages. Intuitively, in Figure 2, wage rigidity prevents the parties from moving the wage of some of the positive joint surplus jobs towards the feasible-jobs frontier, thereby shrinking the set of feasible jobs to the upper right quadrant. We present key equations here, and draw on Figure 3 for intuition. We assume no post-repeal idiosyncratic shocks. The full model is in Appendix D.

Separations Separations occur if at least one of worker or firm surplus turns negative at the given wage, since due to fixed wages both participation constraints in Equations (1) and (2) matter. Hence, inefficient separations—i.e., terminations of jobs with positive joint surplus—can emerge. We now think of wage w as one additional job attribute that can evolve or be fixed, such that jobs are now characterized by $(w, \mathbf{V})$. We define unilateral worker and firm surpluses net of the (fixed) wage and net of the aggregate shock $S^W(w, \mathbf{V}^W, \varepsilon^{W\prime})$ and $S^F(w, \mathbf{V}^F, \varepsilon^{F\prime})$, and their gross counterparts as $\widetilde{S}^W(w, \mathbf{V}^W)$ and $\widetilde{S}^F(w, \mathbf{V}^F)$. Formally, the job-level separation probability is given by:

$$\widetilde{\mathbb{d}}(w, \mathbf{V}; \varepsilon^{W\prime}, \varepsilon^{F\prime}) = \int_{(w', \mathbf{V}')} \mathbb{1}\Big(\underbrace{\widetilde{S}^W(w', \mathbf{V}^{W\prime}) < \varepsilon^{W\prime}}_{\text{Quit}} \overbrace{\quad \vee \quad}^{\text{Mutual Sep.: } \wedge} \underbrace{\widetilde{S}^F(w', \mathbf{V}^{F\prime}) < \varepsilon^{F\prime}}_{\text{Layoff}}\Big) k((w', \mathbf{V}')|(w, \mathbf{V})) d(w', \mathbf{V}'), \tag{8}$$

where separations can be labeled as quits (negative worker surplus but positive firm surplus), layoffs (reversed), or mutual separations (both negative). Here, the initial *incidence* of a shock matters for separations, since worker and firm values are no longer "fungible" and we must separately track $\varepsilon^{W\prime}$ and $\varepsilon^{F\prime}$. Analogously to the Coasean case, group level separation rates are $\delta = \int_{(w,\mathbf{V})} \widetilde{\mathbb{d}}(w, \mathbf{V}; \varepsilon^{W\prime}, \varepsilon^{F\prime}) f(w, \mathbf{V}) d(w, \mathbf{V})$.

REBP Effects in a Non-Coasean Setting Since participation constraints cannot be collapsed into joint surplus, as in the left panels of Figure 3, we now plot example contour maps of the joint distribution of firm (y-axis) and worker (x-axis) surpluses *net of wages and shifters*, $S^W(w, \mathbf{V}^F, \varepsilon^{F\prime})$ and $S^W(w, \mathbf{V}^W, \varepsilon^{W\prime})$. We do so in the right panels of Figure 3. The axes are the participation constraints. Panel (c) illustrates how REBP improved workers' outside options (i.e., lowered worker surplus), so the treated jobs shift left. For comparison, in the middle panels we also plot the Coasean analogs, in the form of contour maps of *gross*-of-wage surpluses, expanding one-dimensional joint surplus from the left panels; there, separations occur only for jobs that fall below the zero-joint-surplus diagonal.

Post-Repeal (Non-)Resilience After the repeal, Figure 3 Panel (f) depicts the former treatment group at the original position but with a missing mass of matches. This gray set of missing matches have low *worker* surplus—the dimension along which REBP selected them into separation—but not necessarily low firm surplus, compared to the control group (Panel (i)). This set is defined by $\{(w', \mathbf{V}') : 0 \leq \widetilde{S}^W(w', \mathbf{V}^{W\prime}) < \varepsilon_b^{W\prime} \wedge \widetilde{S}^F(w', \mathbf{V}^{F\prime}) \geq 0\}$. Thus, resilience does arise for shocks to worker surplus. Resilience need not arise to *firm* shocks, where separations can be very similar in both groups (e.g., if if worker and firm surpluses are independently distributed; see also Appendix Figure A.15 Panel (d)). Non-resilience can therefore arise even without strong assumptions about post-repeal idiosyncratic shocks, unlike in the Coasean model, as we discuss in Section 6.1.

4 Large Separation Effects of the UI Benefit Extension

In this section, we estimate that the REBP reform increased job separations by 11.0ppt among initial matches over the five year program horizon (relative to a 40.8% baseline separation rate among the peer cohorts in the control region). We obtain this estimate using a difference-in-differences design exploiting the reform's sharp eligibility variation by region and age. Interpreted through the lens of our Coasean model, $\frac{\delta^1-\delta^0}{1-\delta^0} = \frac{0.110}{1.0-0.408} =$ 18.5% of surviving matches in the control group are marginal low-surplus matches that would not have survived the extension. Most of the excess separations went into long-term nonemployment, perhaps followed by early retirement.[11]

Plotting Raw Data: Cohort Gradients of Separations We first present visual evidence using raw data in Figure 4 to assess the parallel trends assumption, before turning to regression estimates. Panel (a) plots the share of workers who separated from their 1998 job by 1993q3 (the first quarter after the repeal of REBP), sorted by month-of-birth cohort along the x-axis. We start with the right-hand section of the panel, representing younger workers (born before 1943) who turned 50 only after the repeal and were therefore never eligible for extended benefits. These cohorts exhibit homogeneous separation rates of roughly 40% in both regions, supporting our identification assumption that control cohorts exhibited parallel cohort trends (and even identical *levels*) across regions. Appendix Figure A.16 confirms this overlap among even younger cohorts.

The middle section of Panel (a) represents intermediate cohorts born between 1933 and 1943, who were exposed to the reform in REBP regions; exposure was maximal for workers born in 1938, who turned 50 at the onset of the reform and were still eligible at the repeal 5 years later. Among these cohorts, separations are markedly higher in REBP regions than in non-REBP regions. This vertical difference represents the treatment effect of REBP, and is about 20 percentage points at its peak, as displayed in Panel (b).

Finally, the left-hand section of Panel (a) represents older cohorts (born before 1933) who, while eligible for REBP, were older than 55 at its onset. Consequently, they already had access to more generous disability/early retirement benefits with relaxed entry conditions, as described in Section 2, and reached the retirement age of 60 before the repeal of REBP. A slight treatment effect emerges for these workers, who were eligible for extended benefits, but, regardless of region, had mostly retired by 1993 anyway.

By comparing slightly older and younger cohorts within the same region, our research design nets out any differences between regions that are constant across cohorts (including

[11]Prior studies have documented separation effects of REBP (Winter-Ebmer, 2003; Lalive and Zweimüller, 2004; Lalive, Landais, and Zweimüller, 2015), but have not examined the post-repeal separation dynamics of surviving matches or their efficiency properties.

market-level effects of the program). Potential remaining confounders are shocks or unobservables varying 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 separations among our cohorts during a fixed age window, 50 to 55, rather than between points in time (years 1988 to 1993).[12] This robustness check is in Appendix Figure A.17, Panels (a) (levels) and (b) (differences), which show a similar treatment effect and similar support of the parallel trends assumption for this separation definition. By construction, this figure also eliminates the age trends.

Finally, Appendix Figure A.17 mirrors Figure 4 but studies quarters nonemployed (Panels (c) and (d)) and unemployed (UI/UA benefit receipt) (Panels (e) and (f)), between 1988q2 and 1993q3. Trends in control cohorts again lie on top of each other. Among the eligible cohorts, a treatment effect for both nonemployment and unemployment opens up. Similar results emerge for the 50-55 age horizon, in Appendix Figure A.18.

Regression Estimates In Table 2, we report the estimated average treatment effect from a difference-in-differences (DiD) regression specification among pre-reform, 1988 job holders, for various outcomes D_{rci}, for worker i in region r in birth cohort c:

$$D_{rci} = \alpha + \beta \cdot \text{REBP Region}_r + \gamma \cdot \text{Treated Cohort}_c + \mu \cdot \underbrace{\text{REBP Region}_r \times \text{Treated Cohort}_c}_{Z_{rc}} + \chi_{rci}. \quad (9)$$

The coefficient of interest μ captures the effect of REBP eligibility Z_{rc}, defined by region and birth cohort. Interpreted through the lens of the model, μ captures the (subsequently, post-repeal missing) mass of marginal matches, $\delta^1 - \delta^0$. We set $Z_{rc} = 1$ for workers in the REBP region born before August 1943, such that they were older than 50 at some point during REBP, and zero for other workers (our control groups). Here and in subsequent regression analyses, we exclude workers born before August 1933 because an overwhelming majority had retired by August 1993 anyway. The model includes baseline effects for REBP region and eligible cohort. Our regression specification thus exploits within-region, within-cohort variation. We cluster standard errors at the level of administrative regions (groups of districts, *Arbeitsamtsbezirke*), but we have also assessed robustness for clustering at other levels. Table 2 reports results from the cohort-based design (1998-93 outcomes); we report the age-based estimates (50-55) in Appendix Table A.12, finding similar results. We keep the young control cohorts up to a five-year range. We also assess outcomes for even younger cohorts, which we discuss in Section 6.2.

[12]We measure separations between the quarter before 50 (REBP eligibility), and the quarter before 55 (when disability and early retirement incentives change).

Table 2 Column (1) reports a treatment effect of 11.0ppt on separations from the 1988q2 employer by 1993q3. This effect represents a 27% increase from a counterfactual separation share of 40.8% in the absence of REBP (regression constant plus the baseline effects for treatment region and old cohorts). The 95% confidence interval ranges from 3.0 to 18.9ppt. In turn, our estimates imply that $\frac{\delta^1-\delta^0}{1-\delta^0} = \frac{0.110}{1.0-0.408} = 18.5\%$ of surviving matches in the control group are marginal, low-surplus matches that would not have survived the extension.

Column (2) shows that the REBP-induced separations are largely into persistent nonemployment, i.e., without another employer 1988-93 (12.1ppt, SE 4.3ppt). Column (3) reports a positive effect of 1.48 quarters (SE 0.38) on quarters nonemployed during 1988-93; quarters unemployed (UI/UA receipt) increased by 0.97 (SE 0.53) (Column (4)). Column (5) shows that these effects reflect a reduction of 1.06 quarters (SE 0.37) in continuous employment with the initial employer.

5 Puzzle for Coase: No Resilience After the Repeal

The sudden repeal of the reform in August 1993 (described in Section 2) allows us to test the core prediction of the Coasean model derived in Section 3.1.4: that REBP survivors—jobs that existed before the onset of the reform in 1988 through its repeal in 1993—should subsequently exhibit lower separation rates. This test has power thanks to the large missing mass of low-surplus jobs in the former treatment group: by the end of REBP, an additional 11.0ppt of treated workers had separated. The older control group had a 40.8% separation rate, so among its survivors, $\frac{0.110}{1.0-0.408} = 18.5\%$ are marginal, low-surplus jobs. Intuitively, separations among REBP survivors should be low as long as the control group separation rates do not exceed 18.5%. Yet as we now show, the survivors exhibit exactly the same post-repeal separation behavior as the control group, both unconditionally and in response to negative labor demand shifts.

5.1 Empirical Post-Repeal Separation Behavior

Our sample consists of 1998-93 survivors in the former treatment and control regions: jobs already active right before the onset of REBP in 1988 that continued through its repeal in 1993. To account for potential cross-time REBP spillovers attributable to layoff notices and explicit grandfathering (as the law permitted for pre-scheduled layoffs, see Section 2), our cutoff survival date defining the post-repeal survivor sample is 1994q1. Barring this sample restriction, the strategy mirrors that in Section 4. Our outcome variable is the fraction of 1998-93 survivors subsequently separating at various post-repeal horizons.

Plotting Raw Data: Post-Repeal Separation Rates by Cohort In Figure 5 (1994-96 horizon) and Appendix Figure A.19 (other horizons), we plot the post-repeal separation rates among the surviving jobs, for the former control region (blue solid line) and the former treatment region (red short-dashed line), for levels (Panel (a)) and differences between regions (Panel (b)), by cohort. These raw data convey nonparametric evidence for our main finding, the absence of resilience. There are no post-repeal separation differences between surviving jobs previously exposed to REBP and surviving control jobs, despite the policy's large separation effects during 1988-93.

Quantifying the Differences in Separation Behavior Figure 5 Panel (b) also reports the average DiD estimate for the effect on post-separation behavior, analogous to specification (9) for the survivor sample. The 0.8ppt (SE 1.0ppt) estimate indicates that the former treatment group, if anything, had a slightly *higher* separation rate in the post-repeal period, rather than exhibiting resilience. The tight confidence intervals include zero and allow us to rule out effects more negative than −1.2ppt. Full results are in Table 3 Column (1), along with results for the other outcomes assessed last section (separations into non-employment, etc.) for 1994-96; Appendix Tables A.13-A.15 report on the other horizons.

In the other columns of Table 3, we continue to find no resilience on other margins (nonemployment, time in nonemployment, time on unemployment benefits or assistance, and continuous employment with the original employer). We also report a version dropping workers close to the retirement age, in Appendix Table A.16.

5.2 Coasean Benchmark for Post-Repeal Separations

Predicted Separation Rates By Cohort To gauge the gap between the former treatment group's post-repeal separations in the data and the Coasean prediction, we compute the *predicted* separations according to a Coasean benchmark without post-repeal idiosyncratic shocks, as presented in Equation (7) above in Section 3.1.4. Specifically, for each (monthly) birth cohort c, we collect during-REBP separation rates in the control and REBP regions to proxy for (δ_c^0, δ_c^1) (the blue solid and red dashed lines respectively in Figure 4 Panel (a)). We feed in post-repeal cohort-specific separation rates from the peer cohorts in the control group Δ_c^0 (blue solid line in Figure 5). Intuitively, this benchmark predicts smaller separation effects for larger initial treatment effects of REBP in a given cohort (Figure 4 Panel (b)), due to a larger mass of missing marginal matches.

We plot these predicted Coasean separation rates as a yellow dashed line in Figure 5. The gap between this Coasean prediction and the observed separation rates in the control group is large, confirming that our test has power. For instance, by 1996, the benchmark predicts close to *zero* separations for most of the formerly treated cohorts, whereas the control group's actual post-repeal separation rate is 20% or higher.

Even multiple years later, the design retains power but the differences shrink (since Δ^0 grows), as Appendix Figure A.19 clarifies. Yet, at those multi-year horizons such as from 1994 to 1998, the assumption underlying the benchmark, of no idiosyncratic post-repeal shocks replenishing the mass of marginal jobs, is less plausible.

Quantitative Benchmark We also calculate this Coasean benchmark for the DiD regression coefficient. We aggregate the yellow predicted line across cohorts, weighting cells by their 1994 employment. The predicted average DiD separation effect is -14.0ppt. This predicted resilience is clearly outside of the confidence interval of the actual DiD estimate of 0.8ppt (SE 1.0ppt) for the post-repeal differential separation rates.

5.3 Labor Demand Shocks

The absence of resilience persists even in response to negative aggregate shocks to job surplus (i.e., ε'' in our model). We construct empirical proxies in the form of negative industry and establishment employment shifts, which we interpret as primarily capturing labor demand (i.e., firm-side surplus) shifts.

Heterogeneity by Industry Growth We plot the differential post-repeal separation rates separately for the top, middle and bottom tercile of the industry employment growth distribution from 1994 to 1996 in Figure 6 Panel (a). Appendix Figure A.14 reports on the other horizons. Even in declining industries (bottom tercile), the formerly treated cohorts do not exhibit resilience compared to the control group.

Establishment-Level "Hockey Sticks" We construct establishment labor demand shocks by tracing out "hockey stick" graphs (Davis, Faberman, and Haltiwanger, 2013): separation rates sharply increase when firms shrink (largely driven by layoffs), feature a kink around zero employment growth, and grow slightly in growing firms (due to turnover associated with net hiring). We replicate the hockey stick pattern in the full population data for Austria in Figure 6 Panel (b), where we plot establishment-level annual separation rates for all male employees employed in q1, by bins of annual net employment growth. While this interpretation has not been definitively established, we interpret these shifts to reflect largely labor demand and hence firm surplus shocks (much like mass layoffs are frequently understood to reflect labor demand shocks).

Figure 6 Panel (c) plots *cohort-region-specific* separation rates through 1996 (other horizons in Appendix Figure A.14). We estimate linear slopes separately for shrinking and growing establishments and for four separate groups: by birth cohort eligibility × region. The slopes for the former control and treatment workers essentially lie on top of each other. Lastly, in Figure 6 Panel (d) we report cohort-specific slopes of separations with respect to establishment employment growth. For each birth-year cohort and region cell, we regress an indicator for a 1994-96 separation on the worker's establishment's 1994-96 growth for shrinking establishments (other horizons in Appendix Figure A.14). Both regions exhibit a downward-sloping sensitivity gradient in birth date, indicating that older workers appear shielded from separations. For the younger cohorts, the lines track each other. For the older cohorts, if anything, we see a more negative slope in the treatment region. Hence, the massive extraction of marginal jobs does not attenuate exposure to firm shocks.

6 Alternative Coasean Rationalizations of Non-Resilience

Our finding of non-resilience is inconsistent with the simple Coasean model outlined in Section 3.1. We now ask whether Coasean models with alternative assumptions can plausibly rationalize non-resilience. Whereas our baseline model featured only aggregate post-repeal shocks, Section 6.1 studies the implications of permitting idiosyncratic post-repeal shocks, which can reshuffle the surplus distribution immediately after the repeal and hence may refill the missing mass of marginal matches. Section 6.2 evaluates other explanations.

6.1 The Role of Idiosyncratic Shocks

While our Coasean model accommodated idiosyncratic shocks *during* the program period, our Coasean benchmark for the *post-repeal* period assumed them away, making the resilience arising from the missing mass particularly stark. We now relax this assumption, and study three alternative idiosyncratic shock processes.

To understand how idiosyncratic shocks affect the prediction of post-repeal resilience, we extend Equation (7) (the kinked expression underlying our Coasean benchmark) to the case of arbitrary idiosyncratic shocks post-REBP, i.e., leaving $K(.|.)$ unrestricted. The resulting extended expression gives the post-repeal separation rate of the former treatment group as that of the control group, netting out the separations from the (missing) marginal jobs (the set $M' = \{\mathbf{V}' : 0 \leq \widetilde{S}(\mathbf{V}') < \varepsilon_b^{W}{}'\}$):

$$\Delta^1 = \frac{1-\delta^0}{1-\delta^1}\left[\Delta^0 - \int_{\mathbf{V}'\in M'} \widetilde{\mathbb{D}}(\mathbf{V}', \varepsilon'') f^0_{\text{post}}(\mathbf{V}') d\mathbf{V}'\right]. \tag{10}$$

This expression draws on Equation (6) (the general expression for post-repeal separation rates before we restricted the model to no post-repeal idiosyncratic shocks). $\widetilde{\mathbb{D}}(\mathbf{V}', \varepsilon'') = \int_{\mathbf{V}''} \mathbb{1}(\widetilde{S}(\mathbf{V}'') < \varepsilon'')K(\mathbf{V}''|\mathbf{V}')d\mathbf{V}''$ denotes the post-repeal separation probability out of a job with end-of-REBP attributes $\mathbf{V}'$ and given aggregate post-repeal shock ε''. Here, Markov process $K(\mathbf{V}''|\mathbf{V}')$ guides the post-repeal shock process, which we now study. As before, differences in f^{Z}_{post} across the treatment and control group due to the REBP separations will drive differential post-repeal separation rates. Unlike in the baseline model, we now permit post-repeal idiosyncratic shocks, i.e., $K(.|.)$, to mediate the composition shift induced by REBP.

General Conditions for Absence of Resilience Which conditions must the idiosyncratic shock process $K(.|.)$ fulfill for the Coasean model to rationalize our findings—that post-repeal separation rates are identical across treatment and control groups, across cohorts and even following negative labor demand shock proxies (formally, $\Delta^1(\varepsilon'', \delta^0, \delta^1) = \Delta^0(\varepsilon'', \delta^0, \delta^1)$ for the entire range of post-repeal aggregate shocks ε'' and REBP separation rates):

$$\underbrace{\int_{\mathbf{V}' \in M'} \int_{\mathbf{V}''} \mathbf{1}\{\widetilde{S}(\mathbf{V}'') < \varepsilon''\} K(\mathbf{V}''|\mathbf{V}')d\mathbf{V}'' \tilde{f}^0_M(\mathbf{V}')d\mathbf{V}'}_{\text{Avg. sep. rate for the marginal jobs}} = \underbrace{\int_{\mathbf{V}' \in (J' \setminus M')} \int_{\mathbf{V}''} \mathbf{1}\{\widetilde{S}(\mathbf{V}'') < \varepsilon''\} K(\mathbf{V}''|\mathbf{V}')d\mathbf{V}'' \tilde{f}^0_I(\mathbf{V}')d\mathbf{V}'}_{\text{Avg. sep. rate of inframarginal jobs}}, \quad (11)$$

where $\tilde{f}^0_M = f^0_{\text{post}}(\mathbf{V}') \left[\frac{1-\delta^0}{\delta^1 - \delta^0}\right]$ is the density of the marginal jobs in the control group and $\tilde{f}^0_I = f^0_{\text{post}}(\mathbf{V}') \left[\frac{1-\delta^0}{1-\delta^1}\right]$ is the density of the inframarginal jobs in the control group.

That is, perhaps unsurprisingly, identical post-REBP behavior, $\Delta^1 = \Delta^0$, arises if and only if the average post-repeal separation rate for the jobs in the marginal group ($\mathbf{V}' \in M'$) is the same as that for the jobs in the inframarginal group ($\mathbf{V}' \in (J' \setminus M')$). In other words, resilience arises if and only if the marginal jobs destroyed by REBP would have exhibited the same post-REBP separation behavior as the inframarginal jobs that survived REBP. Below, we start with an extreme assumption about $K(.|.)$ that achieves this condition: perfect reshuffling, which washes out compositional differences right after REBP is abolished. We then move to perhaps more plausible restricted processes, none of which perfectly generate the condition in Equation 11, but let the data quantify the amount of resilience they can generate under a Coasean model.

6.1.1 Idiosyncratic Shocks I: Perfect Reshuffling

The Shock Process One specific Markov process generating equality of separation rates between marginal and inframarginal jobs is reshuffling of jobs into the same surplus distribution—which, if occurring already within a year after the repeal (in our 1995 specification), would fill in the "hole" left by REBP. Full derivations are in Appendix C.

Interestingly, for a given single surplus shock ε'' and set of marginal jobs defined by (δ^1, δ^0), reshuffling is sufficient but not necessary for identical post-repeal separation rates. However, for the condition to hold globally—for all $(\varepsilon'', \delta^1, \delta^0)$ combinations—perfect reshuffling becomes necessary. The formal proof is in Appendix C.2. While our empirical variation indeed features large heterogeneity in REBP and post-repeal separation rates across cohort/industry cells, this variation may not sufficiently approximate this "global" condition, so we additionally consider less dramatic shock processes than full reshuffling below.

Mixed Model Neither the no-idiosyncratic-shocks assumption from Section 3.1 nor the perfect-reshuffling setting likely accurately describes the whole labor market. Using a simple "mixed model," we estimate which fraction of labor market cells would need to follow each extreme model to rationalize our results in a Coasean framework. Let i index labor market (industry-occupation) cells and c cohorts. A given cohort-cell (c, i) is either of the perfect reshuffling or no-shocks type. Share κ (share $1 - \kappa$) of cells are of the full-reshuffling (no-shocks) type. Perfect reshuffling implies $\Delta^1_{ci} = \Delta^0_{ci}$ (formally shown in Appendix C) while no shocks implies that Δ^1_{ci} follows the piece-wise linear function (7). The latter depends on the policy-period separations (δ^0_c, δ^1_c), which we let vary by cohort as in Figure 4. We then estimate κ in the following econometric model:

$$\Delta^1_{ci} = \kappa \times \underbrace{\Delta^0_{ci}}_{\substack{\text{Coasean:} \\ \text{Reshuffling}}} + \underbrace{(1-\kappa) \times \sum_{c=1}^{C} \iota_c \max\left\{0, \frac{1-\delta^0_c}{1-\delta^1_c} \cdot \Delta^0_{ci} - \frac{\delta^1_c - \delta^0_c}{1-\delta^1_c}\right\}}_{\text{Coasean: No Idiosyncratic Shocks}} + \nu_{ci}, \tag{12}$$

where ι_c is a cohort indicator and the residual ν_{ci} captures cohort-cell-specific shocks and other model misspecification.

Estimation We estimate Equation (12) using weighted least squares, weighted by the number of REBP survivors in each cohort by cell (so κ gives the size-weighted share). Cells are 2-digit industry codes defined separately for blue and white collar occupations. We focus on cohorts defined using 5-year and 1-year birth year bins. As reflected in Equation (12), we allow (δ^0_c, δ^1_c) to vary at the cohort level, but assume they are constant across cells within a cohort, while measuring post-repeal separation rates $(\Delta^0_{ci}, \Delta^1_{ci})$ at the cohort by cell level. Intuitively, the model chooses the weighted average of the blue solid line (perfect reshuffling) and the yellow dashed line (no idiosyncratic shocks) in the cell-level analog of Figure 5 (and illustrated in Appendix Figure A.15 Panel (b)) that best fits the data. Weight κ is identified through the non-linearity in the relationship between Δ^1_{ci} and Δ^0_{ci} predicted by the Coasean model with no idiosyncratic shocks that arises from the extraction of marginal jobs, as illustrated in Appendix Figure A.15 Panel (b).

Results Column (2) of Table 4 Panel A reports NLS estimates of Equation (12) using the treatment and control group separation rates Δ^0_{ci} and Δ^1_{ci}. The estimated κ implies an essentially unit weight on the perfect reshuffling scenario, as $\hat{\kappa} = 1.04$ (SE 0.044). That is, in a Coasean world, we would fully reject any stability of job surplus in any labor market cells whatsoever, even in the short run. The lower limit of the 95% confidence interval for κ at the 1994-96 horizon indicates that at least 95% of separations would have to come from full reshuffling of job surplus for the data to be consistent with the Coasean setting when offering these two types. The model continues to put unit weight on perfect reshuffling even at a shorter one-year separation horizon ($\hat{\kappa} = 0.978$, SE 0.035, Column (1)), and even more so three and four years out (Columns (3) and (4)). The last four columns replicate this exercise using the finer 1-year cohort definition, yielding similar estimates, with some gain in precision. The scatter plot in Appendix Figure A.4 visualizes the underlying reduced-form relationship between post-repeal (5-year cohort) separation rates across groups.

Discussion One path through which the Coasean model can therefore rationalize the data is under no stability whatsoever in job-level surplus in almost all labor market cells. We believe that this strong assumption, and hence this Coasean rationalization, is implausible. First, such full convergence would be required already at the one-year horizon. Second, the reform was very large (it raised separations by about 27%, and was worth 71% of the average annual salary), and the idiosyncratic shocks necessary for sufficient reshuffling would need to be accordingly large to replenish the mass of marginal matches. Third, our sample contains older workers with, if anything, more stable surplus.

6.1.2 Idiosyncratic Shocks II: "Exogenous" Separations

The two extreme models considered above either assumed away post-repeal surplus innovation, or imposed perfect reshuffling. We now show robustness of our main results to permitting intermediate degrees of idiosyncratic surplus shocks following the repeal of REBP. Specifically, we will ask whether more restricted, less extreme and perhaps more plausible idiosyncratic shock processes can rationalize non-resilience in the Coasean model.

The Shock Process Our first intermediate scenario mimics the "exogenous" separations often assumed in search and matching models.[13] With a certain probability x, a job separates *irrespective of the initial surplus level*. While often called exogenous, such a separation can be rationalized as an endogenous and efficient separation if the shock is negative enough to yield a negative surplus level. This process can be nested by our Markov process $K(.|.)$ by having all jobs be hit with an idiosyncratic shock of size $y - \widetilde{S}(\mathbf{V}')$ with probability x, or keep their surplus level with probability $1 - x$. Formally, the shock process is:

$$\widetilde{S}(\mathbf{V}'') = \begin{cases} \widetilde{S}(\mathbf{V}') & \text{with probability} \quad 1 - x \\ y < 0 & \text{with probability} \quad x. \end{cases} \tag{13}$$

In this model, separations hence arise both from aggregate shocks ε'', and from idiosyncratic shocks (with probability x).

Post-Repeal Separation Rates How may this process rationalize our findings? The shock process defined in Equation (13) can fulfill condition (11) of equality of post-repeal separation rates in the special case where *all* separations are idiosyncratic, i.e., $\Delta^1 = \Delta^0 = x$. However, as we later explain, this scenario of one homogeneous x is inconsistent with observed heterogeneity in the REBP treatment effect and in post-repeal separation rates across industry cells (formally shown in Appendix C.3). Below, we present empirical evidence that permitting such idiosyncratic shocks does not change our basic conclusions, in that we find substantial resilience even when permitting such shocks and estimating their relevance in the data.

To make progress, we build on the fact that this Coasean model again predicts a kinked relationship between Δ^1 and Δ^0 as in Equation (7) for the model without idiosyncratic shocks. While both the treated and control groups exhibit a *baseline level* x of separations due to the idiosyncratic shocks, the additional, aggregate shocks ε'' induce separations only in the control group—unless the aggregate shock is sufficiently large as to induce otherwise very inframarginal jobs to separate even in the former treatment group. Hence, the augmented model still features the familiar missing-mass logic of the model without idiosyncratic shocks. Formally, in Appendix C.1, we derive the augmented kinked formula relating the predicted treatment group separations to the control group separations, which reduces to Equation (7) when $x = 0$ and otherwise swaps the origin of $(0, 0)$ to (x, x)

[13] We thank an anonymous reviewer for encouraging us to assess this specific specification.

capturing the baseline separations:

$$\Delta^1(\Delta^0(\varepsilon''), \delta^0, \delta^1) = \max\left\{x, \frac{1-\delta^0}{1-\delta^1}\left[\Delta^0(\varepsilon'') - \frac{\delta^1-\delta^0}{1-\delta^0}\right]\right\}. \tag{14}$$

Another Mixed Model Since the model again delivers a kinked formula, we can estimate a mixed model as in Section 6.1.1. The only difference is that the model now additionally allows for large shocks with probability x (the model is nested when $x = 0$). As detailed below, we will either calibrate x or estimate it jointly with κ, the weight on the reshuffling case. Our estimating equation for this model plugs Equation (14) into the previous estimating Equation (12), again letting i denote industries and c cohorts:

$$\Delta^1_{ci} = \kappa \times \underbrace{\Delta^0_{ci}}_{\substack{\text{Coasean:}\\ \text{Reshuffling}}} + (1-\kappa) \times \underbrace{\sum_{c=1}^{C} \iota_c \max\left\{x_c, \frac{1-\delta^0_c}{1-\delta^1_c}\cdot\Delta^0_{ci} - \frac{\delta^1_c-\delta^0_c}{1-\delta^1_c}\right\}}_{\text{Coasean: Stability or Large Idiosyncratic Shock}} + \nu_{ci}. \tag{15}$$

The interpretation is analogous to that in Section 6.1.1, in that we run another horse race between two Coasean models: the case of perfect reshuffling that we know can (trivially but implausibly) rationalize no resilience, and an alternative in which the extraction of low-surplus jobs due to REBP has measurable consequences in the form of post-repeal resilience. As before, estimating a large weight on the former model (i.e., a high κ) suggests that the Coasean model can only rationalize the data under strong, arguably implausible assumptions. Compared to the exercise in Section 6.1.1, however, we now permit the competing Coasean model to feature a richer idiosyncratic shock process and gauge robustness of κ to this assumption.

Identification As before, κ is identified by the non-linearity in the Coasean prediction without idiosyncratic shocks. As the subscripts indicate, we permit the large-shock probability x_c and (as before) the during-REBP separations δ^Z_c to vary by cohort. Hence, we estimate x_c on a cohort basis. Within a cohort, the variation in control group separation rates Δ^0_{ci} that identifies the aggregate shocks stems from industry-occupation variation.[14] Hence, the idiosyncratic shock probability x_c for each cohort c is identified by the kink position shift; intuitively, it captures the baseline separation rate in a cohort for industry-occupation cells with low aggregate shocks, i.e., low separation rates (cohort by cohort).

Estimation We jointly estimate the parameters $(\kappa, x_1, \ldots, x_c)$ using non-linear least

[14] Of course, letting the shock vary arbitrarily at the industry-occupation-cohort level would not permit identification of κ. Given the strong age patterns in separations and the industry shocks plausibly approximating aggregate shocks, we find the current sorting more plausible than the reverse one.

squares (NLS). The shock process in Equation (13) cannot rationalize the data if the parameters $\hat{x}_c$ greatly exceed the empirical separation rates, so we restrict $\hat{x}_c$ to be non-negative and weakly below the 10th percentile of Δ^0_{ci} for each cohort. We relax the latter restriction and provide further estimation details in Appendix E.

Results We find that under the Coasean assumption, the mixed model continues to place negligible weight on the scenario with persistent surplus, even when accounting for the possibility of large idiosyncratic shocks. Again, the model can rationalize the data only with full weight on the—implausible—perfect reshuffling benchmark, i.e., κ near one. Panel B of Table 4 reports point estimates of our parameters $(\kappa, x_1, \ldots, x_c)$ for all post-repeal horizons and both cohort definitions, mirroring Panel A. The parameter of interest is again κ, the weight on the reshuffling benchmark rather than the alternative model with persistence in idiosyncratic surplus except for aggregate shocks and the parametric large idiosyncratic shock. Column (2) again reports estimates for the 2-year post-REBP (1994-1996) separation horizon, using the 5-year cohort definition. We estimate $\hat{\kappa} = 1.045$ (SE 0.052) in this specification. Again, $\hat{\kappa}$ is statistically indistinguishable from 1. (We also report estimates of x_c, which are estimated to affect fewer than 8% of matches, and are less precise at longer horizons; we do not report standard errors for estimates on the boundary of the parameter space described above). Columns (5)-(8) corroborate these conclusions for the 1-year cohort definition, featuring additional degrees of freedom in $(x_c, \delta^0_c, \delta^1_c)$. Even this model places near-unit weight on perfect reshuffling, with $\kappa = 0.992$ (SE 0.008) in Column (6) for the 1994-96 post-repeal horizon, similarly for the other horizons. Appendix Tables A.2-A.9 report the one-year cohorts' $\hat{x}_c$, and robustness to alternative specifications of x_c: constant x_c, ranging from 0 to 0.3, or set to percentiles of Δ^0_{ci}. Hence, this alternative shock process changes our conclusions neither qualitatively nor quantitatively, as the mixed Coasean model continues to put full weight on the full-reshuffling model to rationalize the absence of resilience in the data; put differently, the absence of resilience does not appear to be driven by large idiosyncratic shocks.

Visualization Figure 7 Panel (a) visualizes the results, plotting the average separation rate predicted by Equation (14) for each cohort, using the estimates of $\hat{x}_c$ from the Column (6) specification of Table 4. This prediction closely tracks the no-idiosyncratic-shock benchmark discussed in Section 6.1.1, especially for the younger cohorts. As a result, treatment group separation rates should again have exhibited substantial resilience.[15]

Discussion Our main results appear robust to permitting large idiosyncratic ("exoge-

[15] Additionally, the figure plots the no-idiosyncratic shock benchmark from the industry-occupation level averages of the Coasean no-idiosyncratic-shock benchmark from Section 6.1.1, mirroring an annual version of Figure 5 Panel (a) with some attenuation of cohort-level averages of cell-level kinks from Jensen's inequality.

nous") shocks. Naturally, we restricted the level of idiosyncratic shocks to be cohort- but not cell-specific; trivially, with unrestricted flexibility, such shocks may rationalize any equality of separations. However, this reconciliation would require one to believe that aggregate shocks ε'' do not induce any separations, so that all separations occur idiosyncratically (shown formally in Appendix C.3), a strong assumption in light of the heterogeneity in separation rates across cells such as industries (see Figure 6). Our analysis also assumed perfect stability of surplus absent the shock (although we relax this assumption in Footnote 29 of Appendix C.3 and show robustness to shock processes preserving the relative ranking of matches).

Naturally, it is difficult to systematically assess the explanatory power of all alternatives in our mixed model. In the next section, we adopt a different strategy, and instead assess the plausibility of a shock process which preserves both the level and the rank of match surplus only imperfectly.

6.1.3 Idiosyncratic Shocks III: Continuous, Normal Shocks

Finally, we ask whether a continuously distributed idiosyncratic shock with realistic variance can rationalize the separation dynamics. Unlike in the model with only large idiosyncratic shocks, a model with a continuous idiosyncratic shock predicts resilience even without aggregate shocks—a given *idiosyncratic* shock of a certain size acts just as an *aggregate* shock of that size, and our continuous idiosyncratic shock model features a *distribution* of idiosyncratic shocks—such that some jobs separate everywhere, but still fewer jobs separate in the former treatment group. Rather than relying on another mixed model featuring aggregate shocks, we directly ask whether this type of idiosyncratic shock alone can predict resilience sizable enough to be rejected by the data.

To preview our strategy, we proceed as follows. First, we obtain an estimate of the control group surplus distribution, drawing on a custom survey. We then infer the treatment group distribution by truncating the control group distribution i.e., by dropping the bottom $\frac{\delta^1-\delta^0}{1-\delta^0}$ of jobs, corresponding to the missing mass of marginal matches. We next introduce a parametric shock process—a normally distributed shock—to both surplus distributions. We calibrate the standard deviation of the shock σ_0^s (where superscript s stands for simulated, as described below) to match the empirically observed post-REBP separation rate in the control group, i.e. $\Delta_0(\sigma_0^s) = \Delta_0$. With the calibrated shock process in hand, we compare the predicted post-repeal separation rate in the treatment group ($\Delta_1^s(\sigma_0^s)$) to the empirical rate (Δ_1). We find that this procedure still predicts substantial resilience, such that $\Delta_1^s(\sigma_0^s)$ is substantially below Δ_1. We perform each of these steps for each of our cohorts, reporting results in Figure 7 Panel (b). We detail the strategy, model and data in Appendix F, and highlight the results and key ingredients to our approach here.

The Shock Process We specify the post-repeal process to generate an additive shock:

$$\tilde{S}(\mathbf{V}'') = \tilde{S}(\mathbf{V}') + \nu, \tag{16}$$

where $\nu \sim F_\nu(\nu)$ and has density f_ν; the transition matrix is $K(\mathbf{V}''|\mathbf{V}') = f_\nu(\tilde{S}(\mathbf{V}'') - \tilde{S}(\mathbf{V}'))$.

Post-Repeal Separation Rates In Appendix C.1, we reformulate the general Equation (10) relating former treatment group post-repeal separations to control group separations to this model with idiosyncratic shocks only (without aggregate shocks, $\varepsilon'' = 0$):

$$\Delta^1 = \frac{1-\delta^0}{1-\delta^1}\left[\Delta^0 - \frac{\delta^1-\delta^0}{1-\delta^0}\underbrace{\left(F_\nu(-\varepsilon_b^{W\prime}) - \frac{1-\delta^0}{\delta^1-\delta^0}\int_{\mathbf{V}'\in M'} f_\nu(-\tilde{S}(\mathbf{V}'))F^0_{\text{post}}(\mathbf{V}')\,d\mathbf{V}'\right)}_{\equiv \Delta^M}\right], \tag{17}$$

where $\Delta^M \leq 1$ is the post-repeal separation rate of marginal jobs in the control group. The kink point $\Delta^0 = \frac{\delta^1-\delta^0}{1-\delta^0}\Delta^M$ is smaller compared to the case with no post-repeal idiosyncratic shocks and only aggregate shocks. Intuitively, if there are only small idiosyncratic shocks ($F_\nu(-S') = 0\, \forall S' > \varepsilon_b^{W\prime}$), then all separations in the control group are driven by marginal matches and there are no separations in the treatment group. If shocks are sufficiently large to lead to separations irrespective of the initial surplus or sufficiently small to lead to no separations, then $\Delta^0 = \Delta^1$. For interim cases, separations in the treatment group are attenuated, although, unlike in the case without idiosyncratic shocks, need not be zero. *Importantly, unlike in the previous case of large idiosyncratic shocks, the separations are now sensitive to the size and surplus composition of the marginal jobs.*

To assess the resilience predicted by this Coasean model, we must, first, specify the control group surplus distribution and, second, parameterize the shock process.

Specifying the Initial Surplus Distribution The premise of our paper is that measuring joint surplus is difficult. To specify the distribution of the baseline surplus in the *control* group at the end of REBP, $f^0_{\text{post}}(\tilde{S}(\mathbf{V}'))$, we draw on a novel nonparametric measure of the surplus distribution derived from a custom survey in the 2019 German Socioeconomic Panel (GSOEP) (Jäger et al., 2021). The custom survey elicits workers' beliefs about their own reservation wages and those of their employers.[16] As described in Footnote 9, these

[16]Jäger, Roth, Roussille, and Schoefer (2021) study workers' beliefs about outside options in the form of wages with other employers; they also construct worker rent distributions to study the counterfactual surplus distribution if workers had correct beliefs about outside options, for which they draw on the same question about workers' reservation wages.

reservation wages give measures of worker (S_W) and firm (S_F) surpluses, and the sum of these surpluses gives joint surplus. The sample covers 924 employed workers and is representative of German workers. Survey details and summary statistics are described in Appendix F and in Jäger, Roth, Roussille, and Schoefer (2021). Our main surplus (and shock) measure is in percent of the worker's (last month) salary; in Appendix F we show robustness to defining surplus levels and shocks in terms of Euros.

Figure 7 Panel (c) shows the empirical analog of the post-repeal control group surplus distribution from Figure 3 Panel (g) for the GSOEP sample. This distribution gives our estimate of the post-REBP density in the *control* group, $f^0_{\text{post}}(S')$. To obtain the surplus distribution of the *treatment* group, we truncate the control group surplus distribution at the percentile in the CDF that corresponds to the size of the initial REBP treatment effect, given by $F^0_{\text{post}}(\varepsilon_b^{W\prime}) = \frac{\delta_1 - \delta_0}{1-\delta_0}$, indicated by the dashed red line in the histogram. This gives the treatment group distribution as $f^1_{\text{post}}(\tilde{S}(\mathbf{V}')) = f^0_{\text{post}}(\tilde{S}(\mathbf{V}'))\frac{1-\delta^0}{1-\delta^1}$ for levels $\tilde{S}(\mathbf{V}') \geq \varepsilon_b^{W\prime}$ (or equivalently, if above the treatment percentile), and zero otherwise.

Specifying a Shock Process We assume a normally distributed idiosyncratic shock. Given our focus on separations and to achieve separation rates above 50% without aggregate shocks, we let it take only nonpositive values, i.e., $\nu \equiv -|\tilde{\nu}|$, with $\tilde{\nu} \sim \mathcal{N}(0, \sigma)$. We calibrate its standard deviation σ^s_0 (where we use superscript s to denote simulated or inferred rather than directly measured objects) to have the predicted control group separation rate match its empirical Austrian analog post-REBP, i.e., $\Delta^s_0(\sigma^s_0) = \Delta^0$ (by cohort, see below).[17]

Estimation We randomly assign the GSOEP observations into equally sized treatment and control groups, and bootstrap the predicted separation rates 20 times, reporting the means below; results with bootstrapped SEs in Appendix F are generally tightly estimated.

Results To recap, our strategy is to compare the empirical with the implied treatment group separation rate Δ^s_1. To illustrate the strategy, we start by pooling the birth cohorts by treatment and control group, reporting results in Figure 7 Panel (d), so that all cohorts have the same idiosyncratic shock variance. Feeding the dispersion implied by the control group's average post-repeal separation rate into the truncated surplus distribution of the treatment group, the implied post-repeal separation rate would be about 20% for the 1994-96 horizon. These values are far below the empirical separation rate of about 30%. Appendix Figure A.5 replicates the result for the other post-repeal horizons.[18]

This exercise aggregated cohorts into one coarse treatment and one control group.

[17]As a complement, we invert groups, calibrating dispersion σ^s_1 such that $\Delta^s_1(\sigma^s_1) = \Delta^1$.

[18]Similarly, the reverse exercise from Footnote 17 strongly overpredicts control group post-repeal separation rates, or/and implies an excess dispersion in the treatment group.

To account for cohort heterogeneity in separations, we replicate this analysis at the birth year level, calculating cohort-specific separation rates $\delta_c^1, \delta_c^0, \Delta_c^0$. On that basis, we obtain the predicted cohort-c-specific post-repeal separation rate in the treatment group, $\Delta_{1,c}^s$, by again feeding in $\sigma_{1,c}^s = \sigma_{0,c}^s$, in turn such that $\Delta_{0,c}^s(\sigma_{0,c}^s) = \Delta_c^0$ (details in Appendix F). Appendix Figure A.6 reports the implied control group dispersions σ_c^s for each cohort and year.[19]

Figure 7 Panel (b) plots the post-repeal separation rates (1994-96) together with the calibrated shock dispersion (depicted on the secondary y-axis). While these predicted separation rates for the treatment group are higher than in the Coasean benchmark without post-repeal idiosyncratic shocks, they are still significantly lower than the actual separation rates—in particular for birth cohorts 1935-1943, who have not yet entered retirement age. The DiD effect for this predicted separation rate is -8.5ppt, compared to the 0.8ppt effect we estimated in the data. The figures also recap the no idiosyncratic shock Coasean benchmark, from Figure 5 Panel (b), which yielded a predicted DiD effect of -0.140. Appendix F presents additional robustness checks, plotting results for other post-repeal horizons, as cohort differences across groups (rather than in levels), and for an alternative specification of the surplus and shocks, in Euros rather than as a multiple of a worker's salary.

Discussion This analysis has shown that a Coasean model in which idiosyncratic shocks with a realistically calibrated variance generate all separations in the control group would predict considerable resilience in the former treatment group, too. A few caveats apply: first, we have calibrated the surplus distribution to another dataset, and do not have available direct estimates of the surplus distribution in our specific sample. Second, it remains possible that alternative shock processes would predict less resilience. That said, we have assumed away aggregate shocks entirely, making our investigation an extreme alternative benchmark (that, e.g., would not be able to account for industry heterogeneity, as in Section 6.1.2).

6.1.4 Overall Assessment

Overall, the Coasean models with post-repeal idiosyncratic shocks still predict considerable resilience, in contrast to empirical comovement. An idiosyncratic shock process that can fully account for the data must feature perfect reshuffling of idiosyncratic joint surplus already the year following the REBP repeal, an implausible assumption.

[19] It also depicts the treatment group value for the reverse exercise from Footnote 17, which strongly overpredicts control group post-repeal separation rates, or/and implies an excess dispersion in the treatment group.

6.2 Alternative Coasean Explanations

We discuss alternative reconciliations with the Coasean view, beyond idiosyncratic shocks.

Market- and Firm-Level Spillovers on Control Workers REBP may have had persistent spillovers on the surplus distribution or shock process of the control cohorts in the treated regions. For instance, firms could have shifted training to younger workers, lowering their post-repeal separation rate, hence leading us to underestimate the resilience of treated cohorts when using that control group. However, such spillovers are not indicated by our second difference, between young cohorts across regions, in Figures 4 and 5; the corresponding coefficients on the REBP region indicator in Column (1) of Tables 2 and 3 are essentially zero (0.003 and -0.003). Appendix Figure A.16 confirms this zero result with a third difference for even younger control cohorts through 1958 (which are arguably less close substitutes or less prone to spillovers, as in Card and Lemieux, 2001; Lalive, Landais, and Zweimüller, 2015). Additionally, we provide an employer-level test of spillovers. We calculate industry- and firm-level treatment intensities: the share of workers in program-eligible cohorts (1933-43) pre-reform (1987). We divide our worker sample into quartiles by this measure, and plot the cross-region differences in post-repeal separations by cohort for top and bottom quartiles, in Appendix Figure A.20. We find no increased resilience among the younger control jobs in the heavily treated employers, nor among the slightly older treated cohorts, and hence no evidence for such spillovers.

REBP Period Shocks Literally interpreted, our theoretical framework features a discrete time setting with only one shock during REBP, in the form of Markov process $k(.|.)$ linking pre- and end-of-REBP surplus levels. Multiple idiosyncratic shocks during REBP could, depending on their persistence, change the mapping from separation effects during REBP to the missing mass at the onset of the post-repeal period. Rather than attempting a calibration of the persistence of REBP-period idiosyncratic shocks, we gauge the relevance of this consideration empirically. We zoom into the younger cohorts that, in the REBP regions, became eligible more shortly before the repeal, for whom the one-shock scenario may apply more accurately. Figure 4 Panel (b) provides clear visual evidence that for the younger 1941 cohort, who were eligible for only two years (between 1991 and 1993), an initial treatment effect of about 5ppt emerges, which in Figure 5 Panel (b) still predicts considerable resilience, contrary to the identical observed separation rates (although power shrinks with the smaller REBP treatment effect). This visual evidence is even clearer in the annual birth cohort aggregation in Figure 7 Panel (a), where the cohort aggregation smooths out the volatile prediction lines of the monthly birth cohorts from Figure 5.

Heterogeneous Sensitivity to REBP Our model assumes that REBP induced homogeneous shifts in outside options, causing low-surplus jobs to separate. If, instead, it had removed high-surplus workers (e.g., due to heterogeneous valuation), the Coasean model could rationalize non-resilience. We empirically assess the broadest possible version of this concern: that the incremental separators would, absent REBP, have had *lower* separation rates than surviving treated jobs. Using complier analysis methods, we characterize the marginal jobs in terms of their separation-relevant attributes. First, we estimate a model regressing realized separations on pre-separation attributes in a separate, pre-reform sample. Second, using the resulting estimated coefficients, we create predicted separation scores for our 1988 worker sample. Third, we study the predicted rates among the actual separators in both regions. Appendix Table A.17 presents the results; its note details the prediction model. Reassuringly, these compliers had a *higher* predicted separation rate (0.67, SE 0.098) compared to the treated survivors (0.33, SE 0.078). In turn, the predicted separation rate of the treated survivors is lower than that of the control survivors (0.37, SE 0.080), a small, insignificant negative difference that, if anything, points in the opposite direction of the concern. A related concern, that most of the workers who value REBP separate, is difficult to assess; but evidence from Sweden suggests that least 86% of workers value UI sufficiently to pay for it, considerably larger than the REBP treatment effect (see Figure 4 in Landais, Nekoei, Nilsson, Seim, and Spinnewijn, 2021).

Homogeneity Our test relies on surplus heterogeneity to generate a pecking order of efficient extensive margin adjustment (as in Bils, Chang, and Kim, 2012; Mui and Schoefer, 2021). Conversely, surplus homogeneity absent REBP could, in principle, rationalize our findings in a Coasean setting. Then, REBP would lower surplus of treated jobs and trigger separations by homogeneously decreasing resilience to i.i.d. surplus shocks. But post-repeal, all cohorts will effectively have homogeneous surplus again, leaving no room for relative resilience. However, surplus homogeneity within age groups appears implausible in light of heterogeneous separation rates between firm and worker types, and the above evidence that *predicted* separation rates differ between compliers and non-separators. It is also inconsistent with evidence for heterogeneous rents (Mui and Schoefer, 2021; Jäger, Roth, Roussille, and Schoefer, 2021), see also Section 6.1.3.

Large Firms and Perfect Substitutes Another Coasean rationalization is a large-firm model with homogeneous workers (e.g., by types broader than age) and decreasing marginal products, in which old and young workers are perfect substitutes. Here, separations could occur because of firm-wide shocks to, e.g., productivity, which change the firm's optimal employment level. REBP-eligible workers optimally separate first, shielding the young control group during REBP. But absent heterogeneity, the repeal of REBP restores the homogeneity of surplus, such that no post-repeal resilience emerges. However, this model essentially involves a spillover effect on the separation rates of younger workers in treated regions, for which we did not find empirical evidence above.

Severance Pay In Appendix B, we recap that severance pay is neutral in a Coasean setting, and show that the Coasean wage dynamics required to neutralize the Austrian tenure-severance pay schedule could be offset by small shifts in the wage-tenure gradient.

7 Wage Rigidity as Source of Non-Coasean Job Dynamics

We close by exploring wage rigidity as a source of the non-Coasean separation dynamics, clarifying the required theoretical conditions, and providing some empirical evidence.

7.1 Conditions for a Non-Coasean Explanation with Wage Rigidity

We discuss key ingredients that would enable a non-Coasean model with wage rigidity, as described in Section 3.2, to rationalize the evidence.

High Initial Worker Surplus With wage rigidity, post-repeal resilience arises in response to worker shocks but not to shocks to firm surplus. Hence, if firm shocks drive post-repeal separations—e.g., because baseline worker surplus is large and firm surplus is small and hence less insulated from shocks—the model can rationalize the findings. This configuration of surplus is particularly plausible for our sample of older and high-tenured workers, due to, e.g., implicit contract models with backloading of compensation and "overpayment" for older workers (Lazear, 1979, 1981). Employer competition models (Cahuc, Postel-Vinay, and Robin, 2006) also generate this joint distribution for high-tenured jobs (although they feature efficient (re-)bargaining and separations). In the Austrian institutional setting, works councils have consultation rights in layoffs, making such implicit contracts easier to enforce. Additionally, multiple months of severance payments are due in the case of layoffs or retirement, which are foregone for quitters, thus raising workers' inside value (see Appendix B for a detailed discussion).

Large Worker Surplus Shift From REBP With initially high worker surplus, boosts to workers' outside options must be large for otherwise inframarginal workers to separate. The exceptional size of the REBP UI treatment—*three additional years* of UI eligibility, hence also serving as a bridge into early retirement—plausibly achieved this. In Appendix A, we benchmark that the average cash value is 71% of annual earnings. Indeed, smaller UI shifts or those applying to younger workers do not appear to trigger separations (as shown in Jäger, Schoefer, Young, and Zweimüller, 2020, for the Austrian context).

Limited Correlation Between Baseline Firm and Worker Surpluses The final key ingredient is that the baseline correlation between firm and worker surplus is limited—such that the lower *worker* surplus jobs extracted by REBP are not necessarily marginal with respect to *firm* surplus. Wage frictions may help limit this correlation.[20] (By contrast, in the Coasean setting, the correlation of the fundamentals is irrelevant due to rebargaining.)

Another Mixed Model In fact, assuming no correlation, the non-Coasean framework interprets the mixed model in Equation (12) as putting weight κ on firm shocks (or perfect reshuffling) and $1 - \kappa$ on worker shocks driving post-repeal separations.

7.2 Empirical Evidence

In a final step, we empirically investigate the plausibility of wage rigidity as a mediator of the inefficient separation dynamics. We analyze heterogeneity across cells sorted by proxies for wage rigidity. Indeed, the rigid cells experience higher initial separation effects, and nevertheless exhibit no post-repeal resilience. Our exercise is exploratory, as the wage rigidity proxies are not randomly assigned and hence may correlate with confounders.

Empirical Strategy We sort our 1988 job holder sample into quartiles based on proxies for wage rigidity, with the bottom quartile featuring more rigid wages. Our analysis proceeds in two steps. First, we analyze initial treatment effects on separations. We predict that more rigid cells will experience larger separation effects: wage rigidity may inhibit efficient renegotiation so that matches separate when worker surplus turns negative. Second, we study post-repeal resilience. We predict that—conditional on a given initial treatment effect—the flexible-wage cells will exhibit more resilience and thus accord more closely with the Coasean model (whereas the rigid cells need not exhibit resilience).

Proxies for Wage Rigidity The type of wage friction relevant for our cohort-specific treatment differs from standard downward nominal wage rigidity insofar as it must constrain firms' *differentiation* of wages between similar workers, and as it requires rigidity *both* upward (in response to worker surplus reduction from REBP) *and* downward (in response to negative firm shocks post-repeal). With these qualities in mind, we construct four proxies for wage rigidity. First, we measure the standard deviation of log wages across male workers at the firm-year level, averaged at the firm level over the time period from 1982 to 1987. Second, to capture wage adjustment, we calculate the analogous standard deviation

[20] The non-Coasean model could even generate *higher* separations among the former treatment group in response to firm shocks, e.g., under a "random" wage triggering a negative correlation between worker and firm surplus: REBP quitters would then be particularly valuable to firms. In contrast, Figure 6 Panels (c) and (d) documents similar slopes for the treatment group compared to, e.g., older cohorts in the control region.

of wage *growth*. Third, we consider a measure of deviation of wages from collective bargaining agreements (following Jäger, Schoefer, Young, and Zweimüller, 2020), which set wage floors, e.g., at the industry, occupation, and experience level. To do so, we calculate the within-firm standard deviation of residuals from a regression of log wages on the interaction of year, industry, occupation, as well as tenure and experience cell fixed effects.[21] Finally, we calculate the standard deviation of the residuals of an analogous regression with wage growth as outcome variable. Appendix G details the variable construction.

Summary Statistics and Correlations Appendix Table A.18 presents, by quartile, ranges, means and cross correlations of the four proxies. They are positively correlated, capturing some underlying similarities of the firms. But the correlations are far from perfect, with rank correlations as low as 0.35. Appendix Table A.19 reports firm characteristics by quartile. Across all measures, higher rigidity firms tend to have workers with more experience and tenure, and employ more blue-collar workers. Perhaps surprisingly, we find no clear correlation between wage levels and the wage rigidity proxies.

Empirical Results We show heterogeneity across quartiles of our four wage rigidity proxies in Figure 8. Initial REBP separation effects are indeed larger in cells with higher proxied wage rigidity. This evidence is consistent with wage rigidity mediating the initial separations, but might also reflect confounding factors such as a correlation with baseline surplus levels. As one check for such an alternative explanation, we also plot control-group separation rates during the policy period, finding a flat relationship.

Post-repeal, *neither* high- nor low-rigidity cells exhibit meaningful differences in separation rates comparing the former treatment and control regions. For the high-rigidity cells, this finding supports the predictions of the non-Coasean model with wage rigidity discussed in the previous section. For the low-rigidity cells, which plausibly may have exhibited more resilience in line with the Coasean prediction, we also do not find evidence for resilience. However, the absence of resilience does *not* invalidate the Coasean model in this case; as the low-rigidity (or high-flexibility) cells did not see REBP-induced separations to begin with, our resilience test does not have power.[22]

Discussion While our evidence is consistent with wage rigidity as the source of non-Coasean dynamics, the proxies may partially reflect confounding factors—a challenge that motivated our paper to begin with. Our analysis also highlights that our main findings

[21] Tenure $n(i,t)$ is made up of 5 three-year categories and a category for those with more than 15 years of tenure. Experience $e(i,t)$ is made up of 5 five-year categories and a category for those with more than 25 years experience. (Importantly, neither we nor collective bargaining agreements define wage groups based on age.) Occupation refers to white- vs. blue-collar.

[22] We have also experimented with tracing wage effects of the REBP shock as in Jäger, Schoefer, Young, and Zweimüller (2020), but did not find strong patterns in any cell.

may be driven by rigid wage cells, and, more broadly, the REBP compliers. The type of wage rigidity relevant to our design is symmetric (upward and downward, mediating effects of an age-specific boost to workers' outside option and subsequent negative shocks). It also captures constraints on differentiating wage setting between similar workers perhaps within the same firm. Such frictions may reflect collective bargaining or informal institutions, such as equity concerns (Card, Mas, Moretti, and Saez, 2012; Dube, Giuliano, and Leonard, 2019; Saez, Schoefer, and Seim, 2019; Drenik, Jäger, Plotkin, and Schoefer, 2020), and are inherent to workhorse models of wage posting and monopsony.

8 Conclusion

We have provided a revealed-preference test of the widely invoked, but empirically elusive, Coasean theory of bilaterally efficient separations. The test is based on a quasi-experiment that extracted marginal matches from a treated group but preserved them in a control group. Rejecting the Coasean view, after this treatment was removed, the survivors in the former treatment group exhibited no resilience compared to the control group. Wage rigidity emerges as the friction plausibly underlying the inefficient separations.

We close by highlighting three questions our study leaves open. First, our wage rigidity proxies are not (quasi-)randomly assigned, so that we cannot definitively establish wage rigidity as the source of the inefficient separation dynamics. Second, we leave open the deeper sources of that wage rigidity. Third, our test only assesses the bilateral efficiency of bargaining in the jobs that dissolved in response to REBP (the compliers). More generally, gauging the external validity of our findings beyond our variation and sample would require replicating our design in additional samples and settings.

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Tables

Table 1: Summary Statistics

	Treatment Region		Control Region	
	Eligible Cohort	Ineligible Cohort	Eligible Cohort	Ineligible Cohort
	(1)	(2)	(3)	(4)
Birth Year	1938.636	1945.704	1938.749	1945.692
	(2.800)	(1.526)	(2.802)	(1.544)
Experience	22.138	20.536	20.988	19.150
	(5.408)	(5.509)	(6.006)	(6.100)
Tenure	11.168	9.630	10.075	8.719
	(5.885)	(6.027)	(5.941)	(5.932)
Annual Earnings (1,000 EUR)	36.332	35.747	36.466	35.908
	(10.002)	(10.103)	(10.787)	(11.025)
White Collar	0.378	0.401	0.470	0.483
	(0.485)	(0.490)	(0.499)	(0.500)
Observations	52,294	29,059	198,124	116,852

Note: This table reports summary statistics—means and standard deviations (in parentheses)—for our sample of workers employed at the onset of the reform (1988q2). Columns (1) and (2) do so for the treatment regions and Columns (3) and (4) for the control regions, described in Section 2 and outlined in Panel (b) of Figure 1. Columns (1) and (3) report on the eligible cohorts (cohorts born between 1933 and 1943 who were 50 or older at some point while REBP was active), Columns (2) and (4) on the ineligible control cohorts (cohorts born between 1943 and 1948 who did not turn 50 during the policy period). Details on the sample selection are in Section 2.3. Annual earnings (in logs) are based on 2018 EUR (in 1,000s).

Table 2: Initial Treatment Effect: Difference-in-Differences Effects on Separations (1988-93) Among Pre-Reform Job Holders

	(1) Separation	(2) Separation Into Nonemployment	(3) Nonemployment (Quarters)	(4) Unemp. (Benefits) (Quarters)	(5) Cont. Empl. (Quarters)
REBP Region × Treated Cohort	0.110***	0.121***	1.483***	0.971*	-1.056***
	(0.041)	(0.043)	(0.380)	(0.532)	(0.370)
REBP Region	0.003	-0.003	-0.235	-0.098	0.014
	(0.044)	(0.008)	(0.276)	(0.182)	(0.677)
Treated Cohort	0.030	0.111***	0.817***	0.158***	0.154
	(0.026)	(0.004)	(0.129)	(0.058)	(0.392)
Constant	0.375***	0.058***	1.500**	0.682	15.956***
	(0.100)	(0.017)	(0.664)	(0.450)	(1.846)
Observations	384,200	384,200	384,200	384,200	384,200
Adjusted R^2	0.008	0.047	0.023	0.019	0.002
No of Clusters	100	100	100	100	100

Note: This table reports results of the econometric specification in Equation (9). The coefficient of interest is that on REBP Region × Treated Cohort, which captures the effect of REBP eligibility on the outcomes listed in columns (1) through (5) on a sample of workers employed at the onset of the reform (1988q2). We exclude workers born before 1933 and after 1948. *Separation* denotes an indicator function that is 1 if a worker separated from their 1988-employer by the end of the REBP period (1988q2 to 1993q3). *Separation into Nonemployment* denotes an indicator for *Separation* from the initial employer interacted with an indicator for not taking up employment with another employer. *Nonemployment (Quarters)*, *Unemployment (Benefits) (Quarters)*, and *Continuous Employment (Quarters)* denote the quarters of nonemployment, unemployment benefits, and continuous employment with the initial employer between 1988q2 and 1993q3. Standard errors clustered at the administrative region level are reported in parentheses. Levels of significance: * 10%, ** 5%, and *** 1%.

Table 3: Resilience Test: Difference-in-Differences Effects on Post-Repeal Separations (1994-96) Among Program Survivors

	(1) Separation	(2) Separation Into Nonemployment	(3) Nonemployment (Quarters)	(4) Unemp. (Benefits) (Quarters)	(5) Cont. Empl. (Quarters)
REBP Region × Treated Cohort	0.008	0.008	0.041	-0.071	-0.079**
	(0.010)	(0.009)	(0.027)	(0.045)	(0.036)
REBP Region	-0.003	0.008	-0.007	0.006	0.108
	(0.019)	(0.011)	(0.056)	(0.042)	(0.091)
Treated Cohort	0.135***	0.160***	0.715***	0.149**	-0.581***
	(0.008)	(0.003)	(0.008)	(0.071)	(0.048)
Constant	0.152***	0.063**	0.306**	0.139	8.203***
	(0.052)	(0.030)	(0.144)	(0.108)	(0.249)
Observations	201,409	201,409	201,409	201,409	201,409
Adjusted R^2	0.025	0.046	0.038	0.006	0.016
No of Clusters	99	99	99	99	99

Note: This table reports results of the specification in Equation (9). Here, the sample is restricted to workers employed at the same establishment in May 1988 and February 1994, i.e., survivors. The coefficient of interest is REBP Region × Treated Cohort and captures the effect of REBP-eligibility on the outcomes listed in columns (1) through (5), with outcomes measured by February 1996. We exclude workers born before 1933 and after 1948. *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. *Nonemployment (Quarters)*, *Unemployment (Benefits) (Quarters)*, and *Continuous Employment (Quarters)* denote the quarters of nonemployment, unemployment benefits, 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: Mixed Model Estimates of Share of Cells With Full Post-Repeal Surplus Reshuffling

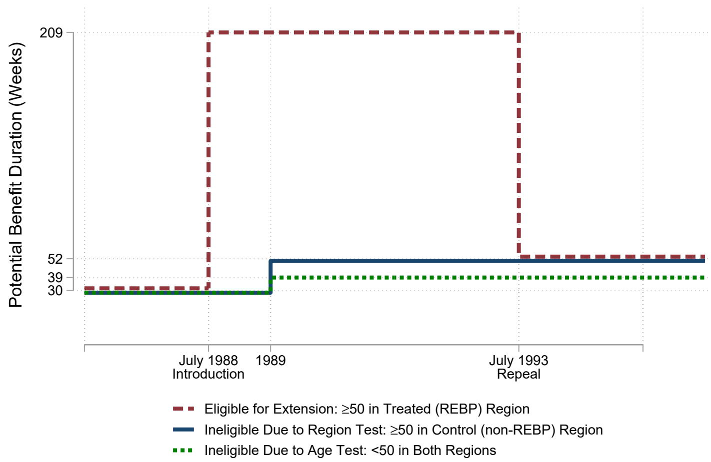
	5-year cohorts				1-year cohorts			
Separation Horizon:	1995	1996	1997	1998	1995	1996	1997	1998
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: No Post-Repeal Idiosyncratic Shocks								
κ	0.978	1.040	1.097	1.117	0.974	1.083	1.157	1.226
	(0.035)	(0.044)	(0.057)	(0.077)	(0.032)	(0.035)	(0.043)	(0.059)
$1-\kappa$	0.022	-0.040	-0.097	-0.117	0.026	-0.083	-0.157	-0.226
	(0.035)	(0.044)	(0.057)	(0.077)	(0.032)	(0.035)	(0.043)	(0.059)
R^2	0.792	0.910	0.935	0.952	0.803	0.883	0.915	0.930
N	182	182	182	182	513	513	513	513
Panel B: Idiosyncratic Shocks								
κ	0.934	1.045	1.097	0.889	0.864	0.992	1.157	0.937
	(0.053)	(0.052)	(0.057)	(0.096)	(0.051)	(0.008)	(0.043)	(0.047)
$1-\kappa$	0.066	-0.045	-0.097	0.111	0.136	0.008	-0.157	0.063
	(0.053)	(0.052)	(0.057)	(0.096)	(0.051)	(0.008)	(0.043)	(0.047)
$x_{1933-1938}$	0.080	0.265	0.001	0.233				
	(0.089)	(0.210)		(0.220)				
$x_{1938-1943}$	0.039	0.000	0.000	0.263				
R^2	0.793	0.909	0.935	0.950	0.809	0.881	0.915	0.928
N	182	182	182	182	513	513	513	513

Note: This table reports NLS estimates of Equation (12) (Panel A) and Equation (15) (Panel B), assessing what fraction of (size-weighted) labor market cells would need to exhibit full post-repeal surplus reshuffling in order to rationalize our empirical control and treatment group separation rates. Panel A estimates the simple specification described in Section 6.1.1; Panel B augments the simple specification by additionally allowing for "large" idiosyncratic shocks of the type described in Section 6.1.2. In all specifications, we collapse the data at the cohort by industry by occupation (blue/white collar) level and weight each observation by the number of workers in each cell, dropping cells with fewer than ten workers who survived REBP. Post-repeal separation rates are measured in the year specified above the column heading, ranging from one year post-REBP in Column (1) to four years post-REBP in Column (4). Columns (1)-(4) use 5-year cohort definitions, while Columns (5)-(8) use 1-year cohort definitions. Standard errors are reported in parentheses for all specifications. The first four columns of Panel B additionally report estimates of the prevalence of the large idiosyncratic shocks for each cohort, with rate x_c. We restrict these estimates to be between 0 and the 10th percentile of the control group separation rates Δ^0_{ic}, omitting standard errors when estimates are on the boundary of the parameter space. Additional NLS estimation details are provided in Appendix E.

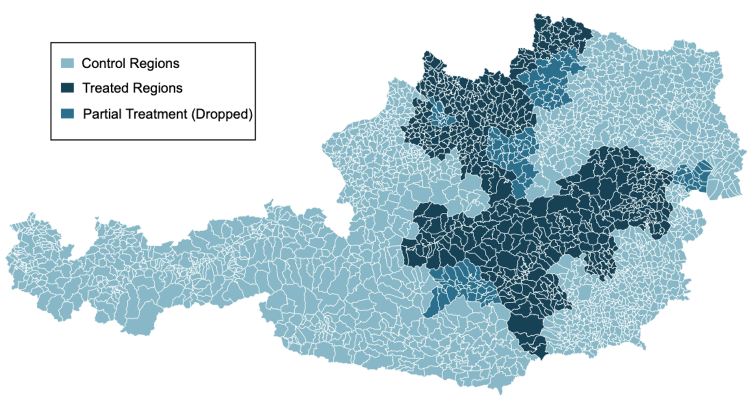
Figures

Figure 1: The Regional Extended Benefit Program (REBP)

(a) Timeline of Potential Benefit Duration During REBP

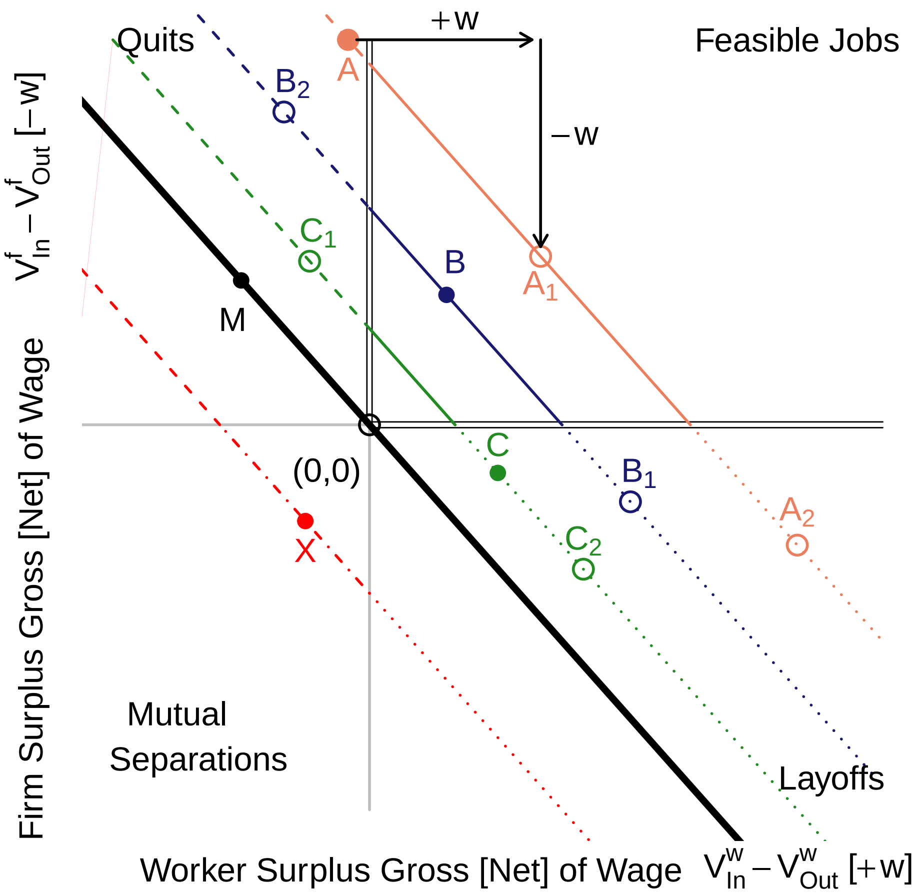


(b) Map of REBP Treatment and Control Regions



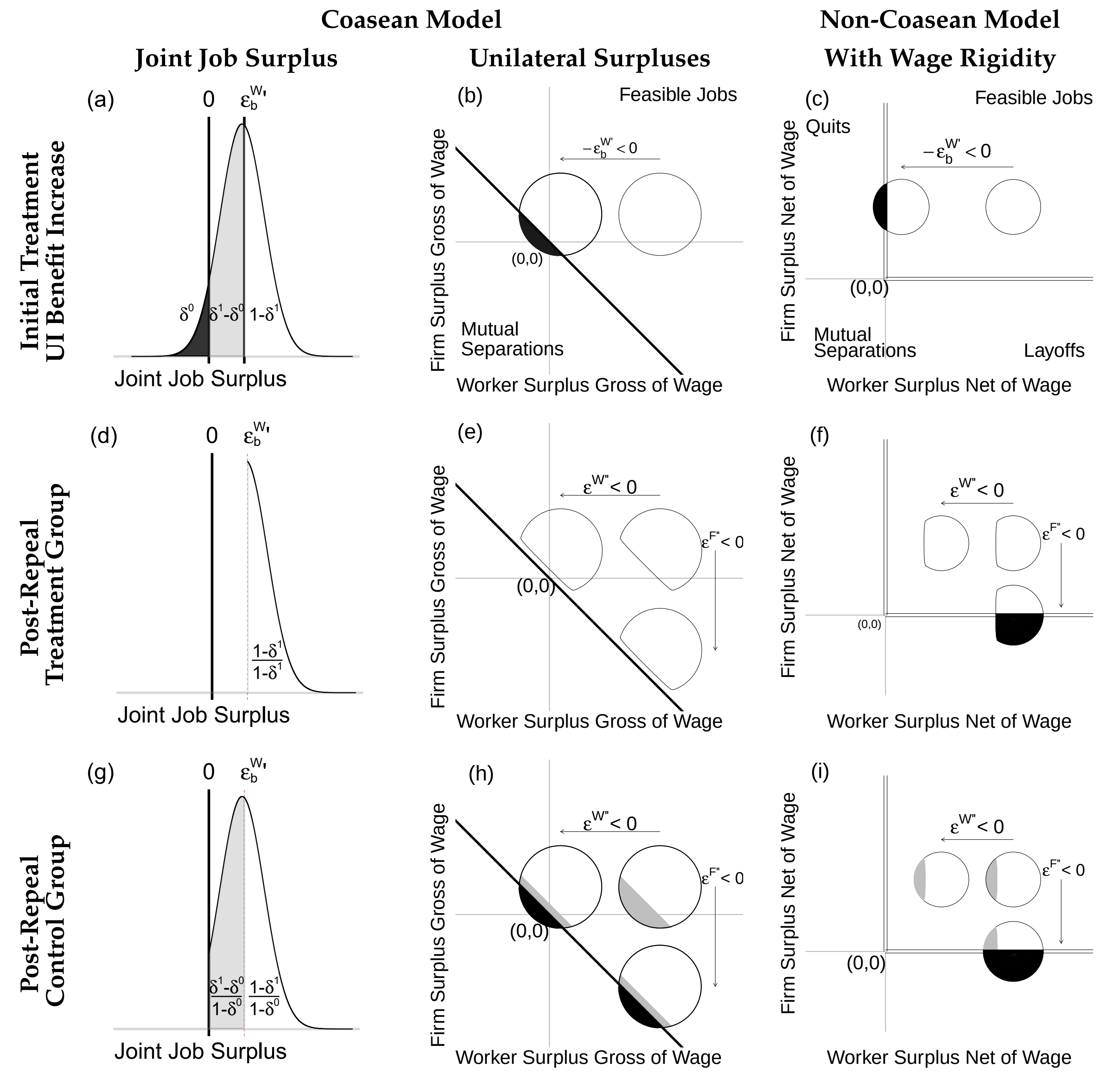
Note: Panel (a) shows the timeline of reform changes in potential benefit duration (PBD) for eligible workers in treatment (REBP) and control (non-REBP) regions. It first shows the PBD for individuals aged 50 or older in the REBP region, which increased from 30 to 209 weeks starting July 1988. Second, individuals 50 or older but in the control (non-REBP) region were ineligible. Lastly, individuals not meeting the age requirement were ineligible in either region. The figure also shows a smaller, nation-wide PDB reform in 1989, which our difference-in-differences design nets out. Section 2 summarizes further details on eligibility. Panel (b) depicts a map of Austrian municipalities categorized into REBP treatment and control regions. We drop the partially treated regions, where REBP was repealed in 1991. Source for map: Inderbitzin, Staubli, and Zweimüller (2016), Figure 1.

Figure 2: Case Studies of Jobs



Note: This figure plots job case studies in the two-dimensional space of worker and firm job surplus. The *solid* circles (•) denote *gross-of-wage surpluses*, i.e., $V^W_{\text{In}} - V^W_{\text{Out}}$ for the worker and $V^F_{\text{In}} - V^F_{\text{Out}}$ for the firm. The *empty* circles (∘) denote *net-of-wage surpluses*, i.e., $V^W_{\text{In}} + w - V^W_{\text{Out}}$ for the worker and $V^F_{\text{In}} - w - V^F_{\text{Out}}$ for the firm. The 135-degree lines are iso-joint-surplus lines, along which wages reallocate surplus between the firm and the worker. The empty lines (||) at a right angle at the origin denote the participation constraints of the worker and the firm, namely positive net-of-wage surpluses. The bold diagonal line (**|**) represents the threshold for job viability on the basis of joint job surplus (which an appropriately set wage can in principle distribute to render each parties' surplus positive). For a job to be viable net of the wage, it must be in the top right corner, providing positive surplus to both parties. Three kinds of separations are represented by the three remaining corners. Quits emerge with negative worker but positive firm surplus. Job A is "born" a quit but the positive wage transforms it into viable job A_1. The wage can also "overshoot" to job A_2, leading to a layoff due to negative firm surplus. Job B is born viable even with a zero wage, e.g., an internship or a high-amenity job. Here, too positive (negative) a wage, B_1 (B_2), leads to a layoff (quit). Job C is a layoff case with a zero wage, so viability requires a negative wage. Doomed jobs such as X are born with negative surplus for both parties. Job X provides negative joint surplus; no wage can render it viable, and both parties are better off outside this match (mutual separation). Finally, M is a marginal job, with zero joint surplus. Born a quit, a unique positive wage moves it to the origin with zero surplus for either party.

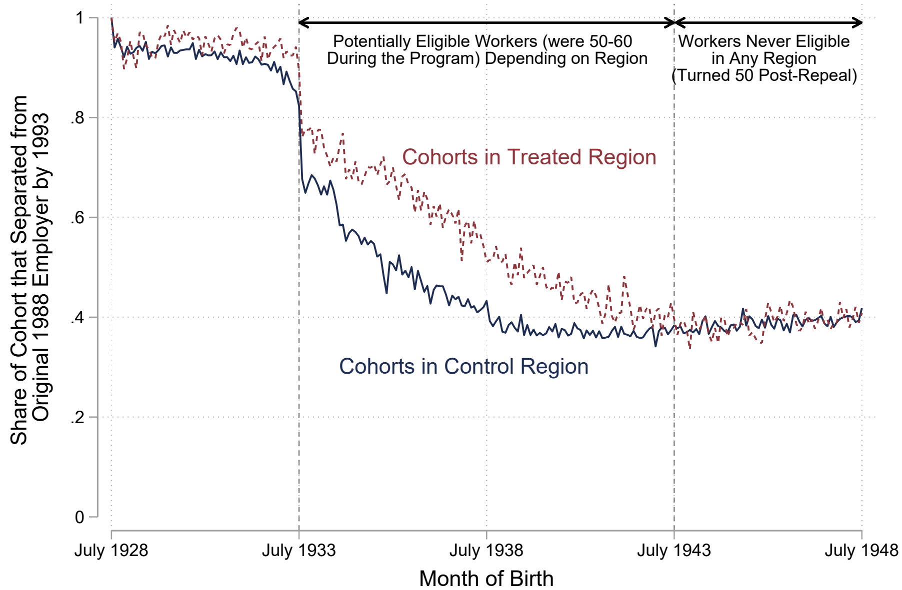
Figure 3: Separation Dynamics and Surplus Distributions: Coasean vs. Wage Rigidity Model



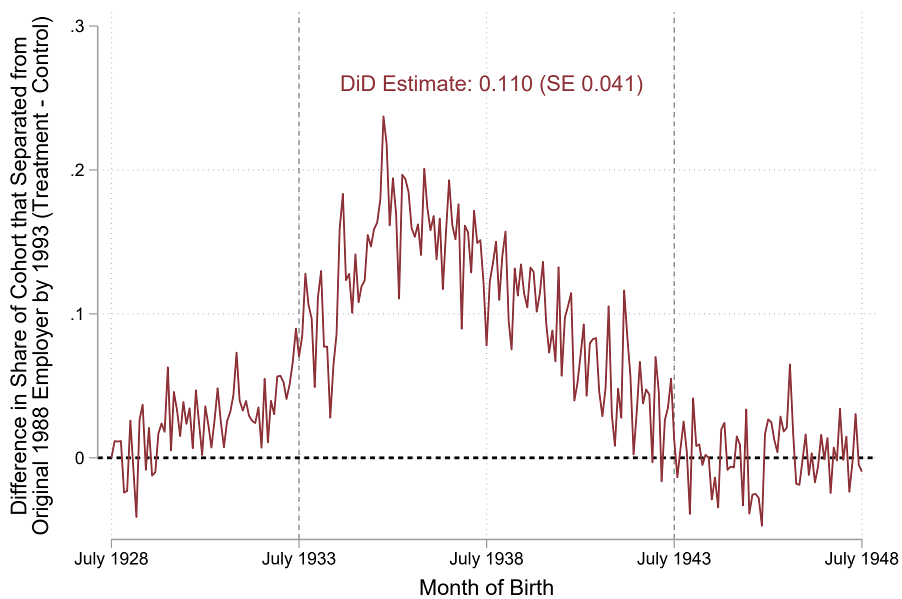
Note: This figure plots example surplus distributions underlying the separation dynamics. The distributions are illustrative and do not correspond to a specific numerical case we will treat. The left column shows a Coasean case in a joint-surplus representation; the middle column shows the model in a two-dimensional representation in terms of unilateral gross-of-wage surpluses, building on Figure 2. The right column shows net-of-wage surpluses for a rigid-wage model. There, the empty lines (||) denote separation thresholds for net-of-wage unilateral surpluses. The bold diagonal line (**|**) does so for joint surplus in the middle column. The top row shows initial effects of REBP. The middle (bottom) row shows post-repeal surplus distributions among surviving matches in the former treatment (control) group. For the middle and right column, the two last rows also show responses to shocks. Panel (a) also includes separators unrelated to REBP but due to idiosyncratic shocks, indicated by the black mass of share δ^0. Throughout, the marginal jobs are gray, making up share $\delta^1 - \delta^0$. Inframarginal jobs surviving REBP are white and share $1 - \delta^1$. At the point of repeal, among survivors in the control group, $(\delta^1 - \delta^0)/(1 - \delta^0)$ are marginal, low-surplus jobs.

Figure 4: Initial Treatment Effect: Separations (1988-93) Among Pre-Reform Job Holders

(a) Levels



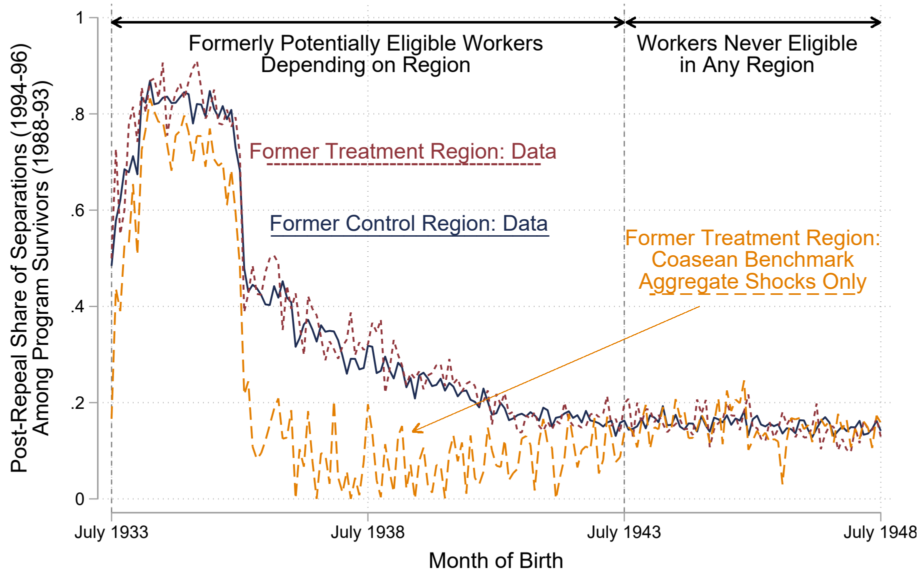
(b) Differences



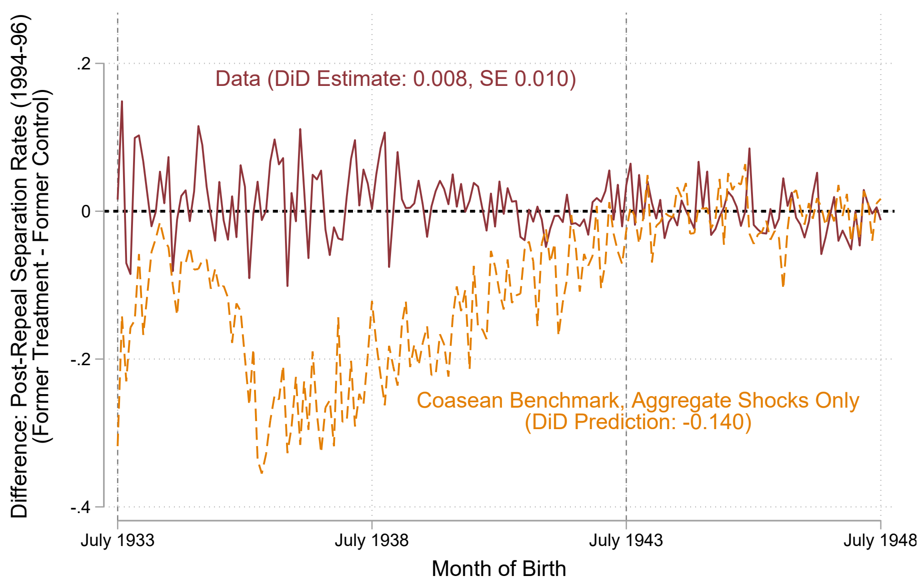
Note: Panel (a) shows the share of workers who separated from their 1988q2-employer (right before the reform) by 1993q3 (when the reform had just ended). We plot rates by month of birth and within the treated (red, short dashes) and the control (blue, solid) regions. Panel (b) shows the difference between the treated and the control region by cohort. Cohorts born after 1943 were not covered by the policy as they turned 50 after the program was repealed 1993. Cohorts born before 1933 had all reached retirement age by 1993.

Figure 5: Resilience Test: Post-Repeal Separations (1994-96) Among Program Survivors

(a) Levels

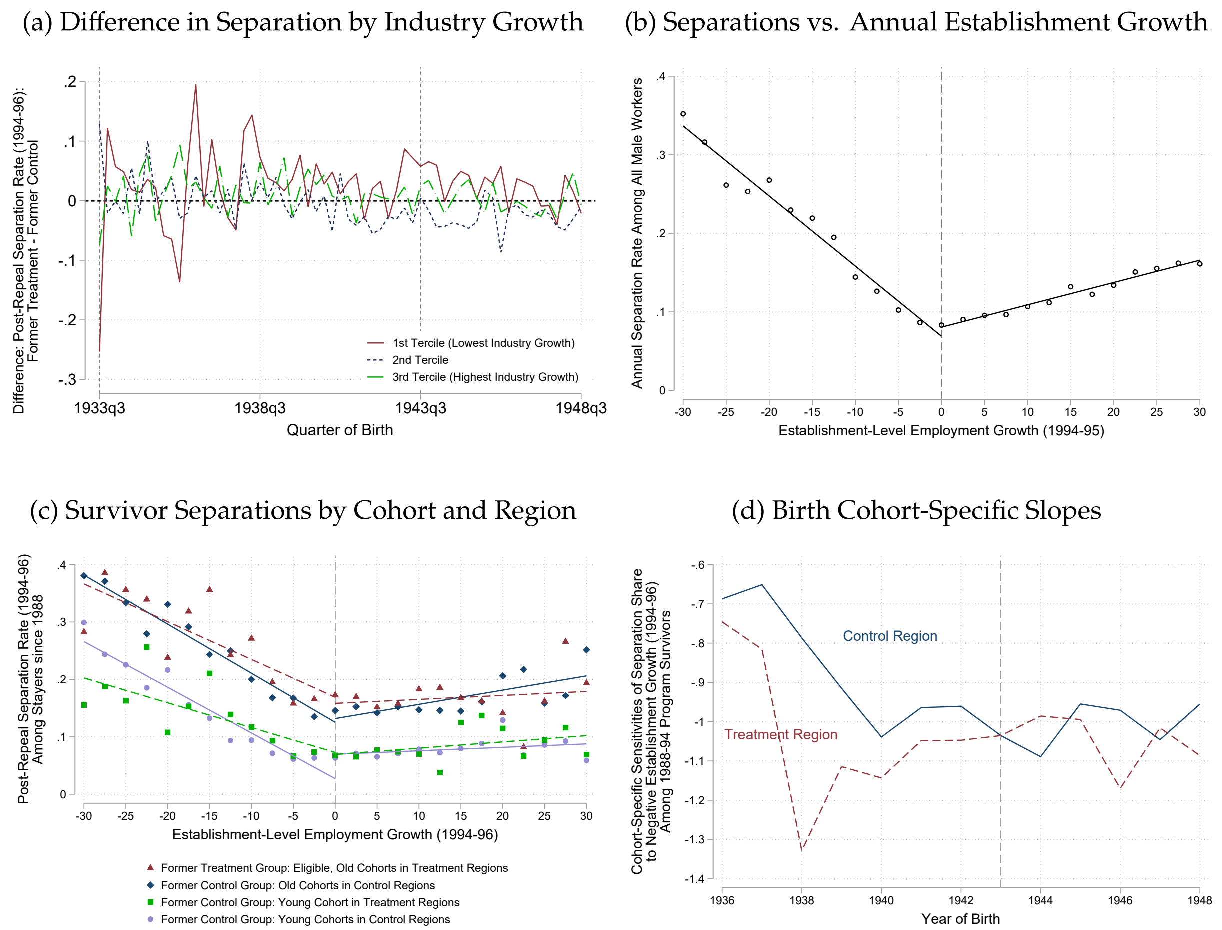


(b) Differences



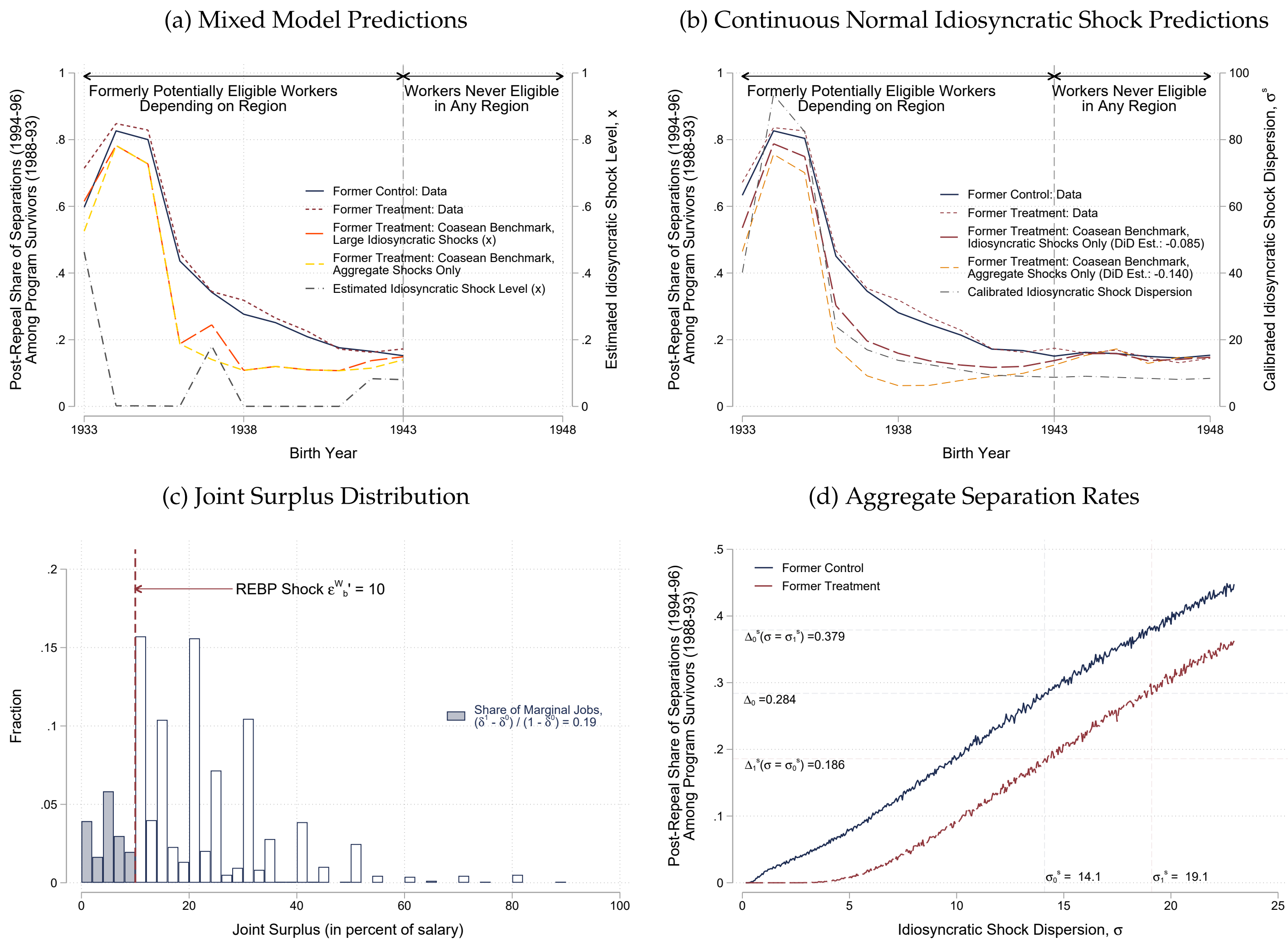
Note: Panel (a) shows, by month of birth, the share of workers observed in the same establishment between 1988q2 and 1994q1 who separate from that employer by 1996q1. The sample is split into treated (red, short dashes) and control (blue, solid) regions. The yellow dashed line plots the Coasean benchmark using Equation (7) (no post-repeal idiosyncratic shocks case). Panel (b) shows, by month of birth, the difference in separation rates from Panel (a) between the treatment and control regions (red, solid), and between separations predicted based on the Coasean benchmark in the treated region and observed separations in the control region (yellow, dashed). The retirement age for Austrian men was 60 years in this period, which explains the spike in separations among older cohorts.

Figure 6: Resilience Tests: Post-Repeal Separation Responses to Negative Industry and Establishment-Level Growth Events (1994-96)



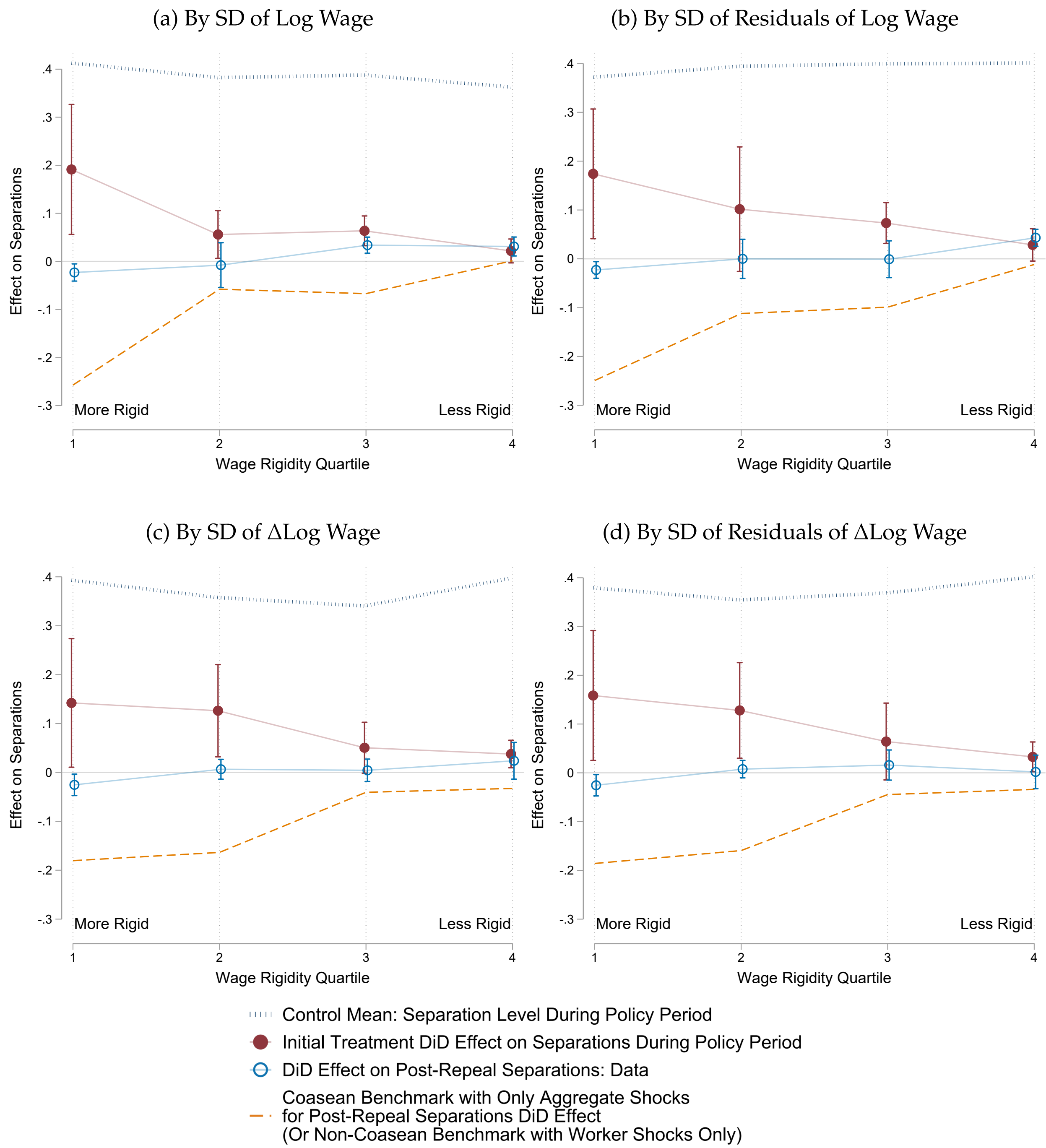
Note: Panel (a) splits the by-cohort regional difference from Figure 5 Panel (b) into terciles of industry growth, with the first tercile denoting the lowest and the third tercile denoting the highest industry growth. Specifically, we calculate employment growth between 1994q1 and 1996q1 for each industry (two-digit NACE), among all workers (not just stayers) born after 1938. Panels (b), (c) and (d) plot the results of an analysis focusing on labor demand shifts within establishments. We confirm the "hockey-stick" relationship between separations and employment growth at the establishment level (Davis, Faberman, and Haltiwanger, 2013) in Panel (b). It plots annual separation rates for male workers employed in a given year by bins of 1994q1-95q1 establishment employment growth. Panel (c) focuses on the four REBP groups (eligible and ineligible cohorts and regions), and plots their separations against *total* establishment employment growth. We ignore the cohorts born before 1936 since they have reached retirement age in 1996. Panel (d) plots the slope of the cohort-specific relationship between separations and establishment growth (1994-1996) among shrinking establishments by cohort and region. We adjust throughout for spurious layoffs due to mergers, take-overs, and administrative changes using the procedure in Fink, Kalkbrenner, Weber, and Zulehner (2010).

Figure 7: Predicted and Observed Post-Repeal Separations (1994-96) Among Program Survivors



Note: This figure reports robustness of our main results to permitting idiosyncratic shocks to surplus after the repeal of REBP, for the 1994-96 horizon. We explore two specifications, both of which yield predicted post-repeal separation rates in the former treatment group that remain substantially below the empirical one. Panel (a) shows robustness to permitting large idiosyncratic shocks that lead jobs to separate irrespective of their initial surplus, with a cohort-specific probability. It reports separation rates averaged across industry-occupation cells for 1-year birth year cohorts from 1933 to 1943. The average control group separation trend is plotted in solid blue, while the treatment group trend is plotted in dashed dark red. The yellow dashed line plots the treatment group separation rate implied by the Coasean model with no post-repeal idiosyncratic shocks according to Equation (7), again averaged over industry-occupation cells. The orange dashed line additionally accounts for the presence of large idiosyncratic shocks, predicting treatment cell separation rates using Equation (14), with x_c estimated in the Column (6) specification of Table 4. The estimates of x_c used are additionally plotted in dashed black, as well as reported in Column (8) of Appendix Table A.5. The other panels refer to the alternative specification of idiosyncratic shocks in the form of continuous, additive, normal shocks. Panel (b) shows, by year of birth, the share of workers observed in the same establishment between 1988q2 and 1994q1 who separate from that employer by 1996q1. The sample is split into treated (red) and control (blue) regions. The yellow dashed line plots the Coasean benchmark using Equation (7) (no post-repeal idiosyncratic shocks case) and the green line shows the predicted separation rate using a continuous normal idiosyncratic shock (but no aggregate shock) as described in Appendix F and in the main text. Panels (c) and (d) plot additional ingredients of this alternative specification. Panel (c) shows the joint surplus distribution based on the GSOEP survey described in Appendix F, together with the size of the REBP shock that is necessary to rationalize a fraction of marginal jobs $(\delta^1 - \delta^0)/(1 - \delta^0)$ (red, dashed). Panel (d) shows predicted post-repeal separation rates, Δ_Z^s, as a function of the idiosyncratic shock dispersion σ, separately for the treatment and control groups.

Figure 8: Separations (1994-96) by Wage Rigidity Proxies



Note: This figure plots several coefficients by quartiles of the within-firm standard deviation of log wages (Panel (a)), the within-firm standard deviation of Mincer residuals from a regression of log earnings on tenure-experience-occupation-industry-year fixed effects (Panel (b)), and analogous measures for changes in log wages over a 5-year horizon (Panels (c) and (d)). We measure wage rigidity at the firm level in the pre-reform period. Cells further to the right exhibit more between-worker dispersion and thus less rigidity. The blue vertical dashes display the control group separation rate during REBP. The red circles plot the treatment effect of REBP on separations among the sample of workers who held a job in 1988 right before the onset of the program. The blue hollow circles plot the effect on separations in the post-repeal period (separation by 1996) in the sample of those workers who were employed in 1988 and whose job survived until 1994. Finally, the yellow dashed lines plot the predicted effect based on the Coasean benchmark with aggregate shocks only, which also corresponds to the non-Coasean benchmark with worker shocks only. Appendix Figure A.21 replicates this figure for the post-repeal horizons other than 1994-96.