Wages and the Value of Nonemployment

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Abstract

Nonemployment is often posited as a worker's outside option in wage setting models such as bargaining and wage posting. The value of nonemployment is therefore a key determinant of wages. We measure the wage effect of changes in the value of nonemployment among initially employed workers. Our quasi-experimental variation in the value of nonemployment arises from four large reforms of unemployment insurance (UI) benefit levels in Austria. We document that wages are insensitive to UI benefit changes: point estimates imply a wage response of less than \$0.01 per \$1.00 UI benefit increase, and we can reject sensitivities larger than \$0.03. The insensitivity holds even among workers with low wages and high predicted unemployment duration, and among job switchers hired out of unemployment. The insensitivity of wages to the nonemployment value presents a puzzle to the widely used Nash bargaining model, which predicts a sensitivity of \$0.24–\$0.48. Our evidence supports wage-setting models that insulate wages from the value of nonemployment.

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1 Introduction

A prominent view in macroeconomics and labor economics is that workers' nonemployment outside options are a key determinant of wages. Most prominently, matching models of the aggregate labor market feature wage bargaining with nonemployment as the worker's outside option (Pissarides, 2000; Shimer, 2010; Ljungqvist and Sargent, 2017). This view helps explain aggregate wage dynamics such as the Phillips and wage curves: high unemployment weakens workers' threat point in bargaining, and thereby lowers wages (Beaudry and DiNardo, 1991; Blanchflower and Oswald, 1994; Ravenna and Walsh, 2008; Christiano, Eichenbaum, and Trabandt, 2016). It also shapes policy debates, such as whether countercyclical unemployment insurance generosity may depress hiring during recessions by pushing up wage demands (Krusell, Mukoyama, and Sahin, 2010; Hagedorn et al., 2013; Chodorow-Reich, Coglianese, and Karabarbounis, 2019). The sensitivity of wages to the nonemployment value also determines the capacity of macroeconomic models to generate realistic labor demand fluctuations (Shimer, 2005; Hagedorn and Manovskii, 2008; Hall and Milgrom, 2008; Chodorow-Reich and Karabarbounis, 2016; Hall, 2017). In wage posting models, the nonemployment value also determines reservation wages of the unemployed, forming the cornerstone of firms' wage policies and the equilibrium wage distribution (Burdett and Mortensen, 1998; Manning, 2011). Similarly, firms pay wage premia above worker's nonemployment outside option in efficiency wage models (Shapiro and Stiglitz, 1984; Akerlof and Yellen, 1986; Katz, 1986). Yet, there exists no direct empirical estimate of the sensitivity of wages to the value of nonemployment.

We estimate the dollar-for-dollar sensitivity of wages to the nonemployment value arising from changes in unemployment insurance benefit (UIB) levels, which we analyze in a quasi-experimental research design studying four large UIB reforms in Austria in 1976, 1985, 1989, and 2001. Only the 1989 reform has been studied, with a focus on unemployment spell duration (Lalive, Van Ours, and Zweimüller, 2006). The reforms raised UIBs differentially for workers based on their previous salaries by as much as 28% in 1985, for example. Our difference-in-differences design compares wage growth between workers eligible for increased UIBs (treatment group) and their unaffected peers (control group). We use administrative data on workers and firms going back to 1972. The Austrian UI context is particularly suitable: (i) most separators receive UI due to broad eligibility and high take-up, (ii) quitters are UI-eligible, (iii) there is no experience rating, (iv) and post-UI welfare benefits move nearly one-to-one with the reforms' UIB shifts.[1]

We document that wages are *insensitive* to increases in UI benefit levels. We first visually analyze each reform nonparametrically. We sort workers into bins by their reference wages that determine UIBs, and then plot wages before and after each reform. These raw data do not

[1]Quitters have full benefit duration after a brief 28-day wait period. Wait periods are considerably longer in other OECD countries, such as three months in Germany, while U.S. quitters are, de jure, permanently ineligible.

reveal any wage responses among treated workers. Second, our difference-in-differences regression reveals point estimates for wage sensitivity to a \$1.00 increase in UIBs below \$0.025 after one and two years. Our confidence intervals rule out sensitivities above \$0.03 in our preferred specification with rich controls and above \$0.07 as the maximal upper bounds for the confidence interval we estimate.

These estimates are an order of magnitude smaller than predicted by the widely used Nash bargaining model with nonemployment as the outside option. Here, wages are the weighted average of the job's inside value (e.g., productivity) – of which the worker receives a share equal to the worker bargaining power parameter – and the worker's outside option – of which the worker receives one minus her bargaining power. UIBs boost workers' outside option by increasing the payoff during nonemployment but importantly also through an endogenous feedback effect on reemployment wages. We calibrate worker bargaining power to 0.1, consistent with the micro evidence from firm-level rent sharing (i.e. inside value shifts from productivity changes). The basic Nash model then predicts that wages will increase by \$0.48 whenever UIBs increase by \$1.00. This prediction is robust to model refinements, such as equilibrium or micro responses. Additionally, incorporating institutional features, such as incomplete take-up or finite duration of benefits, results in predicted sensitivities of \$0.24. In fact, the Nash benchmark could only rationalize the insensitivity we document with *full* worker bargaining power, an assumption inconsistent with the small rent sharing elasticities in the data.

We also test a central cross-sectional prediction of the model: the pass-through of UI into outside options and wages is mediated by a worker's post-separation nonemployment duration. Yet, when we split up workers by their predicted post-separation time on UI (and other proxies for unemployment risk), both the bottom and top groups exhibit the zero wage effect. Relatedly, we find little evidence of larger sensitivity among workers with plausibly lower bargaining power (e.g., blue-collar or female workers) for whom wages should be more sensitive to outside options.

We rule out various confounders that could explain the wage insensitivity. First, standard wage stickiness is unlikely to explain the insensitivity, which extends to new hires (even those hired out of unemployment) whose wages are likely flexibly reset (and allocative for hiring in standard matching models, e.g., Pissarides, 2009). We also find no wage incidence after two years, or in firms exhibiting more flexible or volatile wage policies. Lastly, since the reforms should entail wage *increases*, standard downward wage rigidity should not bind.

Second, the sensitivity remains well below the model benchmark even for workers with frequent interaction with, and hence awareness of, the UI system (see Lemieux and MacLeod, 2000). Supplementary survey evidence indicates that Austrian employees know their own UIB levels. We additionally document wage insensitivity to an age-specific – and thus simple and salient – reform raising UIB duration.

Third, we investigate whether our findings could be explained by bargaining occurring with a firm's entire workforce rather than with individual workers (as in union bargaining models and as documented in Saez, Schoefer, and Seim, 2019). We rerun the regressions with a firm-level average of the worker-level benefit changes. These wage sensitivities remain substantially below the benchmark. While collective bargaining is prevalent in Austria, the institutional environment leaves substantial room for between-firm wage variation. Firms regularly deviate upward from the resulting industry-wide wage floors (mean wages exceed the floors by more than 20% in manufacturing, Leoni and Pollan, 2011), and between-firm wage dispersion is large (Borovičková and Shimer, 2017)).

Fourth, robustness checks reveal that the reforms did not affect separations or sickness spells, suggesting that wage effects were not masked by composition or efficiency-wage effects.

To our knowledge, our paper is the first to quantitatively assess the wage effects of UI-induced outside option shifts against calibrated wage setting models. We complement studies of UI effects on search behavior and reemployment wages of unemployed workers (see, e.g., Katz and Meyer, 1990; Schmieder, von Wachter, and Bender, 2016; Nekoei and Weber, 2017). Our focus on employed workers isolates the bargaining channel, whereas the unemployed are subject to multiple, perhaps offsetting, non-bargaining wage effects, such as skill depreciation (Dinerstein, Megalokonomou, and Yannelis, 2019), job composition (McCall, 1970; Nekoei and Weber, 2017), or stigma (Kroft, Lange, and Notowidigdo, 2013; Kroft et al., 2016). Second, much of the literature focuses on benefit *duration* reforms, which are harder to price and map back into our model, and affect only long spells.

Our evidence supports models that insulate wage setting from the nonemployment outside option. This set includes models with on-the-job search and job ladders, where competing job offers can serve as outside options in bargaining (e.g., Postel-Vinay and Robin, 2002; Cahuc, Postel-Vinay, and Robin, 2006; Altonji, Smith, and Vidangos, 2013; Bagger et al., 2014). Interestingly, wage effects for recently unemployed workers, for whom nonemployment remains the outside option in these models, also remain substantially below the model predictions. Another promising bargaining model is alternating offer (or credible) bargaining (Hall and Milgrom, 2008), in which the threat point is to extend bargaining rather than to terminate negotiations – thereby limiting the role of outside options in general. Wage posting models may be another promising route to explore, although they too can deliver large sensitivities.

Our findings also raise the question whether the short-run comovement between aggregate wages and labor market conditions, such as the Phillips curve and the wage curve (Beaudry and DiNardo, 1991; Blanchflower and Oswald, 1994; Winter-Ebmer, 1996; Blanchard and Katz, 1999), may arise from mechanisms other than fluctuations in workers' nonemployment outside option, such as compositional effects (Hagedorn and Manovskii, 2013; Gertler, Huckfeldt, and Trigari, 2020) or wage pressure from job-to-job transitions (Moscarini and Postel-Vinay, 2017).

The type of wage insensitivity we document is also a crucial theoretical ingredient for the capacity of matching models to produce realistic labor demand fluctuations (Shimer, 2005), where Nash bargained wages move procyclically with the nonemployment value and thereby provide stabilization (Shimer, 2004; Hall and Milgrom, 2008; Hall, 2017; Chodorow-Reich and Karabarbounis, 2016). Relatedly, our findings for limited short-run wage pressure from UI speak against large labor demand effects (Krusell, Mukoyama, and Sahin, 2010; Hagedorn et al., 2013), and rationalize evidence for small employment effects from UI duration extensions in Chodorow-Reich, Coglianese, and Karabarbounis (2019) and positive employment spillovers on ineligible control workers in Lalive, Landais, and Zweimüller (2015).

Section 2 derives wage-UIB sensitivity in our Nash benchmark, and discusses alternative models. Section 3 describes institutions, reforms, and data. Section 4 presents our empirical design and results. Section 5 studies subsamples and group bargaining. Section 6 concludes. All appendix materials can be found in the Online Appendix.

2 Conceptual Framework

We draw on wage bargaining to conceptualize and benchmark the wage effects of UI shifts through the outside option channel. Our point of departure, the canonical and widely used Nash bargain, predicts a wage-benefit sensitivity of 0.24–0.48: when UI benefits – or any components of the nonemployment payoff – go up by \$1.00, wages should increase by \$0.24 to \$0.48 – dramatically higher than our empirical estimate of a wage-benefit sensitivity of at most \$0.03 in our preferred specification in Section 4. We then discuss alternative wage setting models that insulate wages from the nonemployment value.

2.1 Nash Bargaining

We derive the sensitivity in our baseline model with risk-neutral agents, fixed job finding and separation rates, and UIBs as the only nonemployment payoff (with complete take-up and infinite potential duration). Going from an aggregate steady state, we study an unanticipated and permanent shift in UI benefits. In Section 2.2, we show how this baseline sensitivity extends to richer environments, such as other nonemployment payoffs (including without UI, as with incomplete take-up or finite duration), micro responses (e.g., search effort), and market-level adjustments.

2.1.1 Basic Model: Wages, the Nonemployment Value, and UI Benefits

The Nash-Bargained Wage Nash bargaining results in a wage that is the average of the inside value of a job, here productivity p, and the worker's outside option Ω, weighted by worker bargaining power ϕ (equivalently, w is outside option Ω plus share ϕ of surplus $p - \Omega$):[2]

$$w = \phi \cdot p + (1 - \phi) \cdot \Omega. \tag{1}$$

Hence, the sensitivity of the wage to the outside option is $1 - \phi$ (and ϕ with respect to the inside option, e.g., productivity). This implies, for example, that if workers' bargaining power is zero, they are paid exactly their outside option, with which wages then move one-to-one.

The Nonemployment Outside Option The canonical specification of the worker's outside option is a job separation, potentially into temporary nonemployment (e.g., in matching models Pissarides, 2000; Shimer, 2005; Chodorow-Reich and Karabarbounis, 2016; Ljungqvist and Sargent, 2017). Nonemployment carries *value* N. Its *flow value* ρN (in continuous time, with discount rate ρ) consists of (instantaneous) *payoff* b (UI benefits) and, at job finding rate f, the potential *"capital gain"* into reemployment $E(w')$ with its payoff wage w':

$$\Omega \equiv \rho N = b + f \cdot (E(w') - N) = \rho \frac{b + f \cdot E(w')}{\rho + f}. \tag{2}$$

Reemployment flow value $\rho E(w') = w' + \delta(N - E(w'))$, in turn, incorporates returning into N at separation rate δ. The nonemployment flow value ρN then consists of the amortized expected present value of the instantaneous payoffs from nonemployment, b, and reemployment, w':

$$\rho N = \underbrace{\frac{\rho + \delta}{\rho + f + \delta}}_{\substack{\equiv \tau \\ \text{Nonemployment Time} \\ \text{Post-Separation}}} b + \underbrace{\frac{f}{\rho + f + \delta}}_{\substack{\equiv 1 - \tau \\ \text{Reemployment Time} \\ \text{Post-Separation}}} w'. \tag{3}$$

Payoffs b and w' are weighted by $\tau \equiv \frac{\rho + \delta}{\rho + f + \delta}$, capturing discounting and the expected time the worker will spend in nonemployment conditional on separating. A high discount rate $\rho \to \infty$ (e.g., due to myopia or liquidity constraints), a low job finding rate $f = 0$, or a high subsequent separation rate $\delta \to \infty$ will put full weight on b such that $\tau = 1$ and $\rho N = b$ (the initial state after bargaining breaks down). A high job finding rate $f \to \infty$ implies $\rho N = w'$.

The Wage-Benefit Sensitivity Our variation in the outside option is brought about by variation in workers' payoff while nonemployed, specifically shifts in UI benefit levels. With

[2] The firm and the worker choose wage $w = \arg\max_{\tilde{w}} \left(E(\tilde{w}) - N\right)^{\phi} \left(J(\tilde{w}) - V\right)^{1-\phi}$ (with employment value E, nonemployment value N, firm's job value J and vacancy value V). Firm's job value $J = p - w + \delta[V - J(w)]$ draws on productivity p. Jobs end exogenously with probability δ. Formally, one obtains $w = \phi p + (1 - \phi)\rho N - \phi \rho V$. We ignore dV/db, either relying on canonical free entry $V = 0$, or on a control group netting out dV (Section 2.2). We assume $dp/db = 0$, but argue against room for quantitatively important dp effects in Appendix Section A.5.

$\Omega = \rho N$ and plugging in the expression for ρN derived in (3), the Nash wage becomes:

$$w = \phi \cdot p + (1-\phi) \cdot \overbrace{(\tau b + (1-\tau) w')}^{\Omega}. \tag{4}$$

The wage sensitivity to b works through outside option Ω and is therefore mediated by $1 - \phi$:

$$\frac{dw}{db} = (1-\phi) \cdot \overbrace{\left(\tau + (1-\tau)\frac{dw'}{db}\right)}^{d\Omega/db}. \tag{5}$$

The first term, τ, is the *mechanical effect* of b on N through the instantaneous payoff while nonemployed. Second, the *feedback effect*, $(1-\tau)dw'/db$, captures that reemployment wages in future jobs (thus weighted by post-separation time in reemployment $1-\tau$) also respond to b.

Nash bargaining in the next job implies $\frac{dw'}{db} = \frac{dw}{db}$. This allows us to solve for the wage-benefit sensitivity in terms of ϕ and τ as the fixed point in Equation (5):

$$\frac{dw}{db} = \frac{(1-\phi)\cdot\tau}{1-(1-\phi)\cdot(1-\tau)}. \tag{6}$$

Conversely, a given sensitivity dw/db and τ imply a bargaining power $\phi = \frac{1-dw/db}{1+dw/db\cdot(\tau^{-1}-1)}$. For intuition, Figure Ia plots a contour map of the predicted wage-benefit sensitivity as a function of worker bargaining power ϕ for various levels of τ. The lower τ, the lower the weight the outside option puts on UI benefit b, thereby insulating wages from b. By contrast, for $\tau = 1$, such that $\rho N = b$, we have $\frac{dw}{db} = 1 - \phi$. Figure Ib plots the sensitivity as a function of τ, for various levels ϕ. The higher τ, the more weight b receives. For $\phi = 1$, the wage is insulated from the outside option for any τ; for $\phi = 0$, the wage equals the outside option, and so $\frac{dw}{db} = 1$ for any $\tau > 0$.

2.1.2 Calibrating the Wage-Benefit Sensitivity

We now calibrate the sensitivity in Equation (6) as a benchmark for the empirical estimates.

Calibrating ϕ We calibrate worker bargaining power to match the empirical dollar-for-dollar pass-through of firm-specific shifts in labor productivity p (proxied for by profits and productivity shifts) into wages, $\phi = \frac{dw}{dp}$. Our source is the large body of rent sharing estimates (reviewed in, e.g., Manning, 2011; Card et al., 2018), as well as our own calculation based on Austrian data. Figure II plots the implied ϕ values in a meta study. Among these studies, we focus on worker-level specifications to net out composition effects. We calculate an average of 0.099, hence setting $\phi = 0.1$.[3] As a reference, we also list macro calibrations, which typically treat ϕ as a free

[3] This small effect is not due to wage stickiness or insurance, as studies with longer-term productivity shifts (Guiso, Pistaferri, and Schivardi (2005), Cardoso and Portela (2009), and Card et al. (2018)) imply an

parameter or set it to meet the Hosios condition of constrained efficiency in matching models.

The figure also foreshadows, *assuming* the Nash benchmark, the large, close to one, ϕ values implied by our estimated empirical wage insensitivity to the outside option. This striking inconsistency with the rent sharing estimates suggests a rejection of the baseline model.

Calibrating τ To calibrate τ, we exploit the fact that the discount rate ρ is small compared to empirical worker flow rates f and δ, such that $\tau \lessapprox \frac{\delta}{\delta+f}$ (where $\rho \approx 0$ implies a lower bound for $\frac{dw}{db}$). τ then corresponds to an individual's expected fraction of post-separation time spent on UI, mirroring the familiar steady-state expression for *aggregate* unemployment.

We can directly measure this τ concept for actual separators. We start with our full regression sample and keep all individuals who, in the next year, separate into nonemployment for at least one day. Importantly, we do not impose that a separator ever take up UI. For each separator, we calculate her realized post-separation share of time spent on unemployment insurance (UI) or unemployment assistance (UA) (*Notstandshilfe*, which is indexed nearly one-to-one to an individual's UI benefit level and inherits our policy variation db, as detailed in Footnote 16). We refer to "UI" in this paper typically as encompassing both programs.

We then assign each worker in our regression sample (whether she separates or not) her idiosyncratic predicted $\hat{\tau}_i$. We construct these predicted values because we may not see a separation for many of these workers, because the composition of these workers may differ from the separators, and because in Section 4.4 we will exploit heterogeneity in $\hat{\tau}_i$. Specifically, to construct $\hat{\tau}_i$, we plug a worker's pre-separation attributes (industry/occupation, tenure, experience, age, region, gender, separation year, and previous UI history) into the corresponding regression model estimated off the actual separators' realized τ_i's (details in Appendix B).[4]

Table I reports the results for our regression sample, along with predicted values for non-UI states. The average of $\hat{\tau}_i$ across the entire regression sample yields an average expected time spent on UI, E$[\hat{\tau}_i]$, of 10.4% for our preferred specification in column (1).[5] The columns show robustness to restrictions on the separator sample underlying the prediction. Each odd column considers employment restrictions of some (at least one day of) work within four post-separation years, thereby dropping emigrants or other permanent labor force exits. The column pairs also loop over "time restrictions": in our preferred specification, we stop including separators' labor market states at the earliest of 16 years, reaching age 70, or death (the longest horizons we can apply given the 2001 reform and the regression sample age restriction (54)), as well as

inverse-variance weighted mean of 0.094. We exclude studies that report profit-sharing elasticities, which do not directly identify bargaining power, as we discuss in Appendix I.1.

[4] The R^2 of the model is 9%, and the unexplained variation captures a combination of unobservables, model misspecification, and likely also ex-post stochastic realized spell durations unrelated to the quality of the model.

[5] Appendix Table A.5 Panel A reports the realized τ values among the actual separators, whose somewhat higher average τ, 11.6%, reflects composition differences from our full sample. Panel B reports for the full sample the naturally smaller unconditionally-realized (rather than predicted-in-case-of-separation) average τ of 4%, reflecting that our full worker sample is stably employed unless taking up the separation outside option.

either of retirement or disability, if "absorbing" (no subsequent employment or UI/UA spells in the next 16 years). The other columns additionally show robustness to stopping counting only at absorbing retirement in columns (3)-(4), or neither at disability nor retirement in columns (5)-(6).

Benchmark Wage-Benefit Sensitivity In Table I, we also report the implied wage-benefit sensitivities. For $\phi = 0.1$, suggested by the micro studies on rent sharing, and an average $\tau = 10\%$ as described above, the predicted wage–benefit sensitivity is:

$$\left.\frac{dw}{db}\right|_{(\tau=0.104,\phi=0.1)} = (1-0.1)\cdot\frac{0.104}{1-(1-0.1)\,(1-0.104)} \approx 0.48. \tag{7}$$

That is, the calibrated Nash model predicts a \$0.48 wage response to a \$1.00 increase in UIBs. Even if calibrating $\phi = 0.2$, the upper end of the rent sharing estimates, the model predicts a 0.29 sensitivity. Even for $\phi = 0.5$, the middle of the macro targets inconsistent with microempirical evidence, we would find a sizable sensitivity of 0.09. The table also reports the sensitivity respecting Jensen's inequality (as dw/db is nonlinear in τ), which is similarly sized $\mathbb{E}[dw/db(\hat{\tau}_i)] = 0.46$, so our exposition reports $dw/db(\mathbb{E}[\hat{\tau}_i])$. Finally, in Section 4.4, we also study subsamples with τ ranging from 0.02 to 0.2 yielding predicted $\frac{dw}{db}$ from 0.15 to above 0.60.

2.2 Robustness

The baseline model holds fixed all terms except for wages and UIBs, and hardwires UI and nonemployment. We now show that the baseline sensitivity extends exactly to richer nonemployment payoffs, general micro responses (e.g., search effort), and market-level adjustments. Second, a large sensitivity prevails with nonemployment without UI receipt (as with limited take-up).

2.2.1 Richer Payoffs, Micro Reoptimization, and Market Adjustment

Richer Payoff While Nonemployed The level-on-level sensitivity is invariant to the (hard-to-measure) *share* of b in a more general nonemployment payoff $z(b)$, for $z'(b) = 1$ since b simply enters the budget constraint. For example, $z(b)$ may include leisure value or employment disutility $-v$ (MRS), search effort costs $c(e)$, unemployment stigma γ (all normalized into money units by budget multiplier λ), and other nonwork-contingent income y:

$$z(b_i, \ldots) = b_i + \frac{-v_i - c_i(e_i) - \gamma_i}{\lambda_i} + y_i + \ldots \tag{8}$$

Micro Choices and Market Adjustment We now also include vector $\mathbf{c}$ of choice variables (search effort,...), of which we permit micro-reoptimization in response to the reform, as well as vector $\mathbf{x}$ of exogenous variables (e.g. market-level labor demand) taken as given by the household, yet which we now permit to adjust. Now, values N and E are:

$$\rho N(b,\mathbf{c},\mathbf{x}) = \max_{\mathbf{c}} \left\{ z(b,\mathbf{c},\mathbf{x}) + f(\mathbf{c},\mathbf{x}) \cdot \left[E(w',b,\mathbf{c},\mathbf{x}) - N(b,\mathbf{c},\mathbf{x}) \right] \right\} \tag{9}$$

$$\rho E(w,b,\mathbf{c},\mathbf{x}) = \max_{\mathbf{c}} \left\{ w + \delta(\mathbf{c},\mathbf{x}) \cdot \left[N(b,\mathbf{c},\mathbf{x}) - E(w,b,\mathbf{c},\mathbf{x}) \right] \right\}. \tag{10}$$

We group the total derivative of ρN in Equation (9) with respect to b, into four effects:

$$\frac{d\rho N}{db} = \underbrace{\overbrace{\frac{\partial \rho N}{\partial b}}^{=\tau}}_{\substack{\text{Mechanical}\\ \text{Effect}}} + \underbrace{\overbrace{\frac{\partial \rho N}{\partial w'}}^{=1-\tau} \frac{dw'}{db}}_{\substack{\text{Feedback from}\\ \text{Wage Response}}} + \underbrace{\overbrace{\nabla_{\mathbf{c}} \rho N(b,\mathbf{c}^*,\mathbf{x}) \cdot \nabla_b \mathbf{c}^*}^{=0 \text{ By Envelope Theorem}}}_{\text{Micro Re-Optimization}} + \underbrace{\overbrace{\nabla_{\mathbf{x}} \rho N \cdot \nabla_b \mathbf{x}}^{\substack{\text{Net Out with}\\ \text{Control Group}}}}_{\text{Market Adjustment}}, \tag{11}$$

where $\nabla_{\mathbf{a}} f(\mathbf{a},\mathbf{b})$ denotes the gradient of $f(.)$ over the subset of arguments given by vector $\mathbf{a}$. The first two terms are *exactly* the mechanical and feedback effects from the basic model.

Envelope Theorem: the Irrelevance of Micro Reoptimization The third term, capturing reoptimization of the agent's choices $\mathbf{c}$ in response to the shift in b, can be ignored by appeal to the *envelope theorem*, as in the neighborhood around the original optimum $\nabla_{\mathbf{c}} \rho N(b,\mathbf{c}^*,\mathbf{x}) = \mathbf{0}$, where $\nabla_{\mathbf{c}} \rho N(b,\mathbf{c}^*,\mathbf{x}) \nabla_b \mathbf{c}^* = \mathbf{0}$.[6] This result permits us to disregard rich responses in choice variables, and should carry over to unmodeled extensions with job search effort, reservation wages, liquidity effects, take-up and program substitution, or skill loss.

Netting out Market-Level Effects with a Control Group The fourth term accounts for shifts in factors $\mathbf{x}$ that the individual agent takes *as given*, e.g., shifts in job finding rate f due to labor demand or labor force participation shifts, or crowd-out of substitute transfer programs entering z. We net out such effects with a *control group*. Consider treatment and control groups T and C in the same market $m(T) = m(C)$, for whom $db^T > 0$ and $db^C = 0$. For a given individual i in market $m(i)$ and group $g(i)$, we split up $\mathbf{x}_i = (\boldsymbol{\mu}^{m(i)}, \boldsymbol{\iota}^i)$ into market-level variables $\boldsymbol{\mu}^m$, and worker/type-specific factors $\boldsymbol{\iota}^i$ perhaps differing between T and C.

Control group C is exposed to db^T only through market-level effects and own-wage spillovers. Our difference-in-differences strategy nets out market-level effects:

$$\frac{dN^g}{db^T} = \mathbb{1}_{(g=T)} \times \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \frac{dw'^g}{db^T} + \nabla_{\boldsymbol{\iota}} N \cdot \nabla_b \boldsymbol{\iota}^g + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^m \tag{12}$$

$$\Rightarrow \frac{dN^T}{db^T} - \frac{dN^C}{db^T} = \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \cdot \left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] + \left[\underbrace{\nabla_{\boldsymbol{\iota}} N \cdot \nabla_b \boldsymbol{\iota}^T - \nabla_{\boldsymbol{\iota}} N \cdot \nabla_b \boldsymbol{\iota}^C}_{\text{Assume }=0} \right]. \tag{13}$$

[6]Moreover, this benchmark underestimates the effect of non-small b *increases* (the direction of our reforms) on N (as permitting reoptimization weakly increases N), implying a conservative lower bound for $\frac{dw}{db}$.

We cannot evaluate the overall effect of potential b-sensitive $\boldsymbol{\iota}$, and thus must ignore them going forward. Examples are b-dependent transfers (e.g., $z(b) = b + x(b)$), statistical discrimination in hiring by mere treatment status, social stigma, or credit-worthiness changing with benefit level.

Difference-in-Differences Sensitivity Rearranging Equation (13) yields a difference-in-differences version of the wage-benefit sensitivity – which we will empirically estimate – that exactly mirrors the simple model in Equation (6), which held fixed non-wage variables:[7]

$$\frac{dw^T}{db^T} - \frac{dw^C}{db^T} = (1-\phi)\left(\tau + (1-\tau)\left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T}\right]\right) = \frac{(1-\phi)\tau}{1-(1-\phi)(1-\tau)}. \tag{14}$$

Imperfect Labor Market Overlap We will assess consequences of potential imperfect labor market overlap between the groups in four ways. First, our empirical analysis in Section 4 starts by plotting raw wage growth data for a continuum of worker groups sorted by income, permitting visual inspection of treatment and control observations around the cutoff. Second, in our regression framework, we add year- and group-specific fixed effects, and in one specification even firm-by-year fixed effects capturing difference-in-differences between treated and control colleagues in the same firm. Third, while our reforms are income-specific, in Appendix F we provide an additional difference-in-differences design that exploits sharp segmentation of treatment and control groups by *date of birth*, plausibly close substitutes in the same markets. Fourth, in Appendix Section D.2, we show that even if markets were perfectly segmented, the *market-level* wage-benefit sensitivity is similarly sized in calibrated equilibrium (DMP) models with Nash bargaining – with similar mathematical structure as the micro sensitivity.[8]

2.2.2 Nonemployment Without UI

Our benchmark Nash bargaining model assumes infinite UI benefit duration and universal, immediate take-up, such that b adds into $z(b)$ for all individuals. In Austria, however, benefit duration is finite (see Section 3), while take-up is high but not universal (due to the waiting period for quitters and endogenous take-up decisions). In Appendix C, we derive the wage-benefit sensitivity in a three-state model that additionally features non-UI nonemployment (the nonemployed start out with or without UI receipt, then transition back and forth) – capturing concisely a variety of such otherwise hard-to-jointly-model-and-quantify specific mechanisms.

The expected nonemployment value with this second nonemployment state mirrors the two-state baseline model, with $\rho N = \tau^U z^U(b) + \tau^O z^O + (1 - \tau^U - \tau^O)w'$, where τ^U is the

[7] Here, we use $\partial(\rho N)/\partial b = \tau$ and $\partial(\rho N)/\partial w' = 1 - \tau$ from Equation (3), $dw/db = (1-\phi) \cdot d(\rho N)/db$ from Equation (5), and $dw'^g/db^T = dw^g/db^T$, implied by Nash bargaining in subsequent jobs.

[8] The DMP equilibrium wage-benefit sensitivity we derive in Appendix Section D.2 (again for $\rho = 0$) is $\frac{dw^{\text{DMP}}}{db} \approx \eta \frac{(1-\phi)u}{1-(1-\phi)(1-u)-(1-\eta)u}$. It equals 0.32 for market-level unemployment rate $u = 0.07$, and DMP matching function parameter $\eta = 0.72$ (e.g., Shimer, 2005). For $u = 0.05$ or $\eta = 0.5$, the sensitivity would be 0.25.

share of time on UI, and $\frac{dz^U(b)}{db} = 1$. Now however, not all non-UI time $1 - \tau^U$ is spent in reemployment (where the payoff (wage) is UI-sensitive: $1 > \frac{dw'}{db} > 0$), but fraction τ^O of post-separation time occurs in a state with a UI-insensitive nonemployment payoff $\frac{dz^O}{db} = 0$. Hence, this extension clarifies that, for a given τ^U, such features attenuate the feedback effect $\frac{dw'}{db}$ in $\frac{dw}{db} = (1-\phi)\cdot(\tau^U \cdot 1 + \tau^O \cdot 0 + (1 - \tau^U - \tau^O)\cdot \frac{dw'}{db})$. This feature is thus a potentially powerful force, as the feedback accounts for 0.39/0.48=81.25% of the two-state sensitivity of $0.48 \approx (1-0.1)\cdot(0.1 + 0.9\cdot 0.48) \approx 0.09 + 0.39$.

This formulation yields an intuitive variant of the familiar two-state sensitivity:

$$\frac{dw}{db} = \frac{(1-\phi)\tau^U}{1 - (1-\phi)(1 - (\tau^U + \tau^O))}. \tag{15}$$

To assess the attenuation of the feedback effect through non-UI nonemployment, Table I presents estimates for τ^O (i.e. we additionally measure nonemployment states without UI-sensitive payoffs, and predict this value from actual separators onto our regression sample, also detailed in Appendix Section B). Most time post-separation is spent reemployed, and only a fraction $\tau^O = 0.21$ is spent in non-UI nonemployment, such that the fraction of time reemployed $\tau^E = 1 - \tau^U - \tau^O$ is not far from our baseline (two-state) assumption $\tau^E = 1 - \tau^U$. Of course, our measure of τ^U remains the same. The three-state model therefore preserves a high (though attenuated) wage-benefit sensitivity of 0.24. Since in both benchmarks we still exclude other factors that would push up the sensitivity (e.g., discounting, the fact that Austrian UIBs are not taxed,...), going forward we refer to 0.24-0.48 as the range of predicted wage-benefit sensitivities, and use the sensitivity from the simpler two-state model when including theoretical benchmarks for predicted wage growth in the empirical figures.

2.2.3 Further Robustness and Extensions

In Appendix Section D.1, we show that the predicted wage effect remains below the firm's post-reform reservation wage for our reforms, i.e. that the job has sufficiently large initial firm surplus. Away from the most basic DMP setting, incorporating firing or hiring costs or specific human capital easily suffices to accommodate the predicted wage effects.

Appendix Section D.2 contains additional robustness checks, including specific models of finite benefit duration, limited take-up, wage stickiness, liquidity constraints/myopia, treatment and control groups in segmented markets with DMP equilibrium effects, and on the job search and endogenous separations, individual households with risk aversion, and multi-worker firms.

2.3 Alternative Wage Setting Models

We briefly discuss wage sensitivities to nonemployment values in alternative wage setting models.

Sequential Auctions In sequential auction models with on-the-job search and employer competition, wages are often still set by Nash bargaining (Cahuc, Postel-Vinay, and Robin, 2006). Unemployed workers initially use nonemployment as their outside option. Yet, while on the job, workers receive outside job offers that may dominate and replace the nonemployment outside option, such that $d\Omega_i/dN_i = 0$, leaving wages insulated from shifts in N and b.[9] Yet, for workers without such offers, unemployment remains the outside option, and their wages should still exhibit the large sensitivity to benefit increases from standard Nash.[10]

Credible Bargaining Hall and Milgrom (2008) analyze wage setting by alternating offer bargaining in which threat points are to extend bargaining – rather than separating as in the Nash model, which we formally study in Appendix Section D.4. Outside options only become relevant in exogenous break-downs of bargaining – limiting their influence. Moreover, the wage can simultaneously exhibit outside-option insensitivity *and* empirically small productivity effects.

Wage Posting Survey evidence from US workers (Hall and Krueger, 2012) and German employers (Brenzel, Gartner, and Schnabel, 2014) suggests about equal relevance for bargaining and wage posting. Models of the latter (e.g., Albrecht and Axell, 1984; Burdett and Mortensen, 1998) can exhibit large wage-benefit sensitivities as well, albeit through very different mechanisms, and are more difficult to characterize and calibrate (in particular along a transition). Here, firms post jobs with predetermined wages. Workers accept jobs that dominate their outside options, which is the current job's employment value if employed, and is the value of nonemployment otherwise. Due to random search, firms set wages taking into account the entire distribution of outside options. We relegate additional formal intuitions into Appendix Section D.5. The nonemployment reservation wage R given by the nonemployment value $E(w = R) = N$, thus forms the *cornerstone* – the lower bound $\underline{w}$ – of firms' equilibrium wage policy distribution. Thus, b-induced shifts in R trigger one-for-one responses at the low end of the wage distribution. But they also entail ripple effects through the *entire* equilibrium wage policy distribution. These effects and hence the implied wage sensitivities can be small in the knife-edge case of perfect homogeneity (Burdett and Mortensen, 1998). Yet, away from perfect homogeneity, as with heterogeneity in firm productivity, not only do the least productive firms (who pay R) adjust wages, but cascading effects can raise even the highest wages nearly one-to-one. Relatedly, we expect similar rippling effects (across submarkets) with directed search (Wright et al., forthcoming).

[9] Mortensen and Nagypal (2007), Fujita and Ramey (2012), Mercan and Schoefer (2020) feature on-the-job search with the nonemployment outside option. Beaudry, Green, and Sand (2012) and Caldwell and Harmon (2019) find evidence consistent with job offers raising wages.

[10] These workers' wage is pinned down by: $E(w) = (1-\phi)\cdot N(b_i) + \phi\cdot(E(w) + J(x_f, w))$, where x_f is the match- or firm-specific productivity. An employed worker having received outside offer $x_{f'}$ dominating N yet dominated by the current job ($E(w) + J(x_f, w) - U(b) > W(w) + J(x_{f'}, w) - U(b) > U(b)$) renegotiates the current wage with that external job offer as the outside option: $E(w) = (1-\phi)\cdot[E(w) - E(w_{f'})] + \phi\cdot(E(w) + J(x_f, w))$.

Some wage posting models also preclude firms from differentiating wages between treated and control workers (as in, e.g., Bontemps, Robin, and Van den Berg, 1999; Vuuren, Van Den Berg, and Ridder, 2000; Saez, Schoefer, and Seim, 2019), motivating our additional design relating firm-level average wages to average treatment.

3 Institutional Context, Reforms, and Data

We review Austrian wage setting institutions, the UI system, our four reforms, and the data.

Wage Setting in Austria About 95% of Austrian workers are covered by a central bargaining agreement (CBA) negotiated between unions and employer associations, typically at the industry-by-occupation level. Besides working hours and conditions, CBAs also regulate wage *floors* (Bönisch, 2008)– such that in practice, additional establishment-level and bilateral negotiations regularly lead to substantially higher wages.[11] In our sample period from the 1970s through the early 2000s, actually paid average wages in manunfacturing exceeded CBA wage floors by more than 20% on average in any given year (Leoni and Pollan, 2011), suggesting substantial scope for negotiations at the firm or worker level. At a macroeconomic level, the flexible wage setting institutions are mirrored in high levels of aggregate real and nominal wage flexibility (Hofer, Pichelmann, and Schuh, 2001; Dickens et al., 2007), although our reforms entail benefit and hence potential wage *increases*. We also document direct evidence consistent with firm-specific rent-sharing and thus wage deviations in Austria even when controlling for industry-by-year and firm effects.[12] Accordingly, Austria has large wage dispersion between firms, even within the same industry (Borovičková and Shimer, 2017). As robustness checks, we additionally study wage responses to firm- or industry-by-occupation level treatment definitions, and we zoom in on firms with particularly flexible wage policies or in industries with high growth rates.

Unemployment Insurance in Austria The Austrian UI system assigns benefit levels to granular reference wages. Appendix H.2 plots these schedules along with the social security earnings maximum (above which earnings are censored in our data) by year, from 1976 to 2003. The replacement rate was 41% at the beginning of our sample period, and benefits start at a minimum level and are capped. By 2001, the replacement rate with respect to net-of-tax $(1-\tau_i)$ earnings, $\frac{b_i}{(1-\tau_i)w_i}$, had increased to 55%. Before 2001, the benefit schedule was based on gross income. UIBs are neither taxed nor means-tested, but UI recipients are required to search for

[11]For example, comparative industrial relations work concludes that "in practice local works councils often negotiate supplementary wage increase" (OECD, 1994, p. 176).

[12]We use firm panel data from Bureau van Dijk from 2004 to 2016 and regress wages per employee on value-added per employee, controlling for firm and industry-by-year effects, estimating a level-on-level coefficient of 0.046 (SE 0.009). See Figure II for a comparison to coefficients from other settings.

jobs suitable to their qualifications.[13]

Until 1989, benefit duration was only experience-dependent, with 12 (20, 30) weeks for workers with 12 (52, 156) weeks of UI contributions in the last two (two, five) years. From 1989 on, workers with sufficient experience aged 40-49 (above 50) were eligible for 39 (52) weeks.[14]

The Austrian UI system is particularly suitable for our study. First, workers that unilaterally quit are eligible for UI, ensuring that our UI variation shifts workers' outside options.[15] Second, as a consequence of broad eligibility, relatively long PBD, and mandatory registration with the UI agency (for continuity of health insurance coverage), most workers who separate will take up UI. Appendix Table A.1 reports take-up rates after separations into nonemployment. 65.2% (68.2%) of nonemployment spells longer than 14 (28) days lead to take-up of UI. Third, workers ineligible or exhausting UI can apply for means-tested unemployment assistance (UA or *Notstandshilfe*) benefits, which track a given worker's reform-induced UIB shifts nearly one-to-one.[16] So we will often denote by "UI" as encompassing both programs. Fourth, there is no experience rating. UI is financed by a payroll tax split between the worker and firm.

Four Large Reforms to the UI Benefit Schedule Figure III plots the four reforms we study as a function of nominal earnings (Panels (a)-(d)), and of contemporaneous earnings percentile (Panel (e)). These four particularly large *increases* in benefit levels differentially affected different segments of the earnings distribution. To cleanly test for pre-trends or anticipation effects, we exclude reforms in 1978 and 1982 and several small maximum-level inflation adjustments as the affected earnings regions had recently been exposed to other benefit reforms. In each panel of Figure III, we plot the new schedule and the pre-reform schedule. Benefits and earnings are in nominal Austrian shillings (ATS). We convert EUR into ATS starting 1999 at a rate of 1 to 13.76. The timing and policy process is summarized in Appendix H.1. Three reforms were parts of legislation passed in parliament; the 1985 reform followed a decree from the Ministry of Social Affairs. The 2001 reform simplified the benefit schedule by switching from a gross to a net earnings base.

[13]Income-independent UIB add-ons (e.g., in 2018 EUR 29.50 per dependent) are orthogonal to our variation.

[14]A program in place from 1988 to 1993 raised duration to 209 weeks for workers 50 or older, with 708 weeks employment in the last 25 years, residing in certain regions (Winter-Ebmer, 1998; Lalive and Zweimüller, 2004).

[15]By contrast, US quitters are de-jure ineligible for UI. Compared to most European countries, Austrian UI features a very short wait period to claim UI benefits after a quit (four weeks). By contrast, the wait period is, e.g., 12 weeks in Germany, 45 days in Sweden, and 90 days in Hungary and Finland. Quitters in many other European countries such as the Netherlands, Portugal, and Spain are fully ineligible for UI benefits. See Venn (2012) for an overview.

[16] Precisely, $\text{UAB}_i = \min\{0.92\text{UIB}_i, \max\{0, 0.95\text{UIB}_i - \text{Spousal Earnings}_i + \text{Dependent Allowances}_i\}\}$. Due to the spousal earnings means test, not all workers are eligible for UA. For 1990, Lalive, Van Ours, and Zweimüller (2006) report median UA at 70% of median UIB. Card, Chetty, and Weber (2007) gauge average 2004 UA at 38% of UI for a typical job loser.

Data Our primary dataset is the Austrian Social Security Database (ASSD) (Zweimüller et al., 2009). The underlying spell data provide day-specific labor force status, and average earnings per days worked by employer and calendar year ("wages", detailed in Appendix Section E.2), but no hours information. It covers all private (and non-tenured public) sector employees from 1972 onward, and for most of our period excludes the self-employed and farmers. Earnings are censored at annual social security contribution caps (see Zweimüller et al., 2009).[17] Across the sample years of a given reform, we harmonize the cap at the lowest censored percentile. The ASSD includes covariates such as gender, age, citizen status, a white/blue collar indicator, establishment ("firm") location, and industry. We also draw on UI registry data (AMS) to validate our prediction of actual benefits based on lagged earnings (Appendix Section E.5).

4 Estimating the Wage Effects of Four UIB Reforms

We estimate σ, a *dollar-for-dollar sensitivity* of wages to the nonemployment value by comparing reform-induced variation in UI benefits $db_{i,t}$ with wage changes $dw_{i,t} = w_{i,t} - w_{i,t-1}$:

$$dw_{i,t} = \sigma \cdot db_{i,t}. \tag{16}$$

We first plot raw data of wage against benefit changes by workers, and then implement a difference-in-differences regression analysis. We estimate a wage effect of \$0.00 to a \$1.00 benefit increase, with confidence intervals ruling out effects above \$0.03 even after two years in our preferred specification. This insensitivity extends to new hires, and workers with high predicted time on UI.

4.1 Variable Construction and Samples

Wage Responses Our main outcome of interest is the change in average daily wages from one year to the next, $dw_{i,t}$ where $dw_{i,t} = w_{i,t} - w_{i,t-1}$, whose construction we detail in Appendix Section E.2. We will further normalize $dw_{i,t}$ by lagged wages $w_{i,t-1}$, so that we study percent wage growth (but will similarly normalize benefit changes db, so that we will estimate the level-on-level sensitivity rather than an elasticity). For any job spell (lasting at most one calendar year), we divide total earnings by spell length (days). To account for job switching, we assign jobs by calendar month. Within a month, we prioritize the job with the longest spell in that year. For job switchers, we only consider post-separation earnings. Lastly, we calculate annual averages of these monthly values (excluding months without earnings) to obtain our year-t wage measure.

[17] The statutory caps reported there are for 12 months of earnings. Our earnings data also capture two bonus payments entering the UIB calculation (see Appendix E.3 for details). We have confirmed that our reforms did not affect the probability of censored $t+1$ earnings, so censoring is unlikely to mask positive wage effects.

Reform-Induced UI Benefit Level Changes Our variation in the nonemployment option arises from reform-induced shifts in UI benefit levels. Formally, a worker i with UI-relevant attributes $\mathbf{x}_{i,t}$ receives benefits $b_t(\mathbf{x}_{i,t})$ in year-t benefit schedule $b_t(.)$. Our variation is the difference between this benefit level and the worker's *counterfactual* benefit absent the reform, i.e. under $t-1$ schedule $b_{t-1}(\mathbf{x}_{i,t})$. In practice, UI benefit levels are a function of pre-separation reference wages, so that assignment variable $\mathbf{x}_{i,t} = \tilde{w}_{i,t}$ equals reference wage $\tilde{w}_{i,t}$ applicable in year t. We ignore additional factors such as the number of dependents, which largely entail lump sums orthogonal to our benefit variation. Our reform-induced variation in benefits is:

$$db_{i,t}(\tilde{w}_{i,t}) = b_t(\tilde{w}_{i,t}) - b_{t-1}(\tilde{w}_{i,t}). \tag{17}$$

Hence, $db_{i,t}$ captures benefit variation solely due to shifts in the benefit schedule. The variation is zero if the UI schedule remains unchanged between $t-1$ and t. Such years will form our *placebo years*. *Reform years* feature schedule changes for some workers $i \in T$, our *treatment group*. $db_{i,t}$ is zero for workers forming our *control group* C. Importantly, UI reference wages are *lagged* wages - hence predetermined and unaffected by the reforms.

Reference Wages $\tilde{w}_{i,t}$ We now describe our construction of reference wages and implied UIBs (with additional details in Appendix Section E.4). The earnings concept determining UIBs underwent slight changes over the decades spanned by our four reforms (administrative details in Appendix Section E.3). For the 2001 reform, the reference wage determining UIBs in year t is the wage from the *previous calendar year* $t-1$, a rule in place since 1996: $\tilde{w}_{i,t}^{t\geq 1996} = w_{i,t-1}$.[18] Hence, we directly assign the benefit variation $db_{i,t} = b_t(w_{t-1}) - b_{t-1}(w_{t-1})$ by a worker's lagged wage $w_{i,t-1}$. Before 1996, UIBs were calculated based on the wage in the last full month before unemployment (before 1988) or a moving average of wages during employment in the last six months (1988 to 1996). Because of wage growth, because we measure annual but not monthly wages, and because wages are potentially affected by the reform, we *predict* year-t nominal wage levels based on year-$t-1$ wages, $\hat{w}_{i,t} = \bar{g}_{t,t-1} \cdot w_{i,t-1}$, by inflating lagged earnings with average nominal wage growth in our sample, $\bar{g}_{t,t-1}$, between $t-1$ and t (whereby our strategy builds on simulated instruments as in, e.g., Gruber and Saez, 2002; Kleven and Schultz, 2014). In Appendix E.5, we validate that this procedure predicts wages and implied benefit levels well across most of the earnings distribution. There, we also show graphically and in a regression (based on a job loser subsample using the information on actually paid benefits from the AMS data) that actual received UIBs tightly track our predicted levels, finding coefficients very close to one.

[18]More precisely, UIBs for claims beginning before (after) June 30 of year t depend on $t-2$ ($t-1$) income.

Sample Restrictions and Summary Statistics We restrict the sample to workers aged 25-54 employed in each of the 12 months of the base year (reform and placebo).[19]

For each reform, we include treatment earnings regions by selecting earnings percentiles that experienced a sizeable increase in their replacement rates due to the reform and adjacent, equally sized control earnings regions (see Figure III for a visualization and Appendix Section E.1 for details of the sample construction)).

Table II provides summary statistics for the treatment and control workers, by reform. This table is *not* a balance check. Instead, our design relies on a conditional parallel trends assumption discussed in Section 4.3. We also construct pre-reform placebo cross sections occupying the reform-year earnings percentiles.

4.2 Non-Parametric Graphical Analysis

We start with a non-parametric analysis of each reform to illustrate how our variation identifies wage-benefit sensitivities, which we normalize, going forward, by the worker's lagged wage $w_{i,t-1}$:

$$\frac{dw_{i,t}}{w_{i,t-1}} = \sigma \cdot \frac{db_{i,t}}{w_{i,t-1}}. \tag{18}$$

We plot raw data on wage growth of workers sorted by UI reference wages and hence UIB changes.

2001: Large Benefit Increase for Lower Earners Figure IV(d) shows the results for the 2001 reform, which we describe in particular detail. The x-axis indicates gross earnings in 2000, the pre-reform base year. These reference wages determine 2001 benefits.[20] Here, we collapse the data into earnings percentiles.

The solid green line indicates the reform-induced benefit change for individuals at a given level of base year wages. The 2001 reform affected UI benefits for workers with base-year earnings below ATS 20,500 (32^{nd} percentile of the earnings distribution).

The blue lines with solid squares and hollow circles plot wage effects by base-year earnings at the one- and two-year horizon. For each percentile, we calculate the difference between wage growth from 2000 to 2001, when the reform was in place, and pre-reform wage growth from 1999 to 2000. (Analogously, two-year effects difference 2000-2 and 1998-2000 wage growth.) We normalize the wage effects to zero for the lowest percentile not receiving a benefit increase in

[19] Individuals with fewer than 52 weeks of experience in the past two years would be eligible for at most 12 weeks of UI benefits. We found similar results with a laxer restriction of employment in December of the base year. The heterogeneity analysis by recent unemployment relaxes this restriction.

[20] The benefit schedule $b_{2001}(.)$ is a function of *net* earnings while $b_{2000}(.)$ is a function of gross earnings, as with all schedules through 2000. We use an income tax calculator to translate gross earnings (which our administrative data provide) into net earnings to compute $b_{2001}(.)$. For visual consistency, we plot the 2001 reform in terms of gross earnings. We thank David Card and Andrea Weber for sharing the tax calculator.

2001. There is no excess wage growth for workers treated with higher benefits, either right below or away from the threshold, or at the one- or the two-year horizon.

To provide a visual benchmark, we also plot the wage growth *predicted* by our calibrated bargaining framework in Section 2, as the dashed yellow line. That is, we multiply benefit change with the calibrated wage-benefit sensitivity of 0.48. Our analysis of the 2001 reform thus clearly rejects this benchmark.

Our analysis rests on the identification assumption that the average wage growth among groups treated by a benefit reform compared to the control group, whose benefits remained unchanged, would have followed parallel trends absent the reform. We shed light on this assumption in two ways. First, the flat wage effects across the control percentiles provide support for the identification assumption. A second test, reported in Appendix Figure A.5, lags both the reform period and the pre-period by two years, and checks whether the earnings percentiles affected by the 2001 reform experienced higher or lower excess wage growth in periods *before* 2000. Such different trends could then have masked a non-zero treatment effect during the 2001 reform. Appendix Figure A.5 shows no such effects for a placebo reform in 1999 (thus comparing 1999-2000 to 1998-9 wage growth) for the one-year earnings changes. At the two-year horizon, there is even some evidence of a *positive* pre-trend. While such a pre-trend would actually bias our results upward, it motivates our regression-based difference-in-differences analysis in Section 4.3, where we add time-varying industry/occupation and firm-by-year fixed effects as well as parametric earnings controls to net out such potential confounders. There, we also formally test for – and do not find – pre-trends across all of the reforms.

1989: Increase in Benefits for Low Earners Figure IV(c) presents the analysis of the 1989 reform, which increased benefits for workers with base-year earnings below ATS 12,600 by up to eight percentage points. The graph suggests moderate, positive average wage effects which even at the two-year horizon remain much smaller than the benchmark prediction. In Appendix Figure A.4, for 1989 as for the other two reforms before 1995, we additionally confirm that the reform affected realized benefit levels by base-year earnings, validating our earnings inflation prediction and the benefit imputation.

1985: Increase in Benefit Maximum Figure IV(b) plots the effects of the 1985 reform, which raised the maximum benefit level by 29% (ca. 7,600 ATS to 9,800 ATS) for higher earners. We find no evidence for wage increases among these workers.

1976: Increase for Low Earners The 1976 reform, analyzed in Figure IV(a), raised benefits for workers with earnings below 4,100 ATS. If anything, their wages differentially decrease.

The Average Sensitivity of Wages to UI Benefits Figure V is a scatter plot of excess wage growth against UIB change by earnings percentile across all four reforms (symbols differentiate the reforms). Estimating the wage-benefit sensitivity in a linear regression reveals point estimates of $\hat{\sigma} = -0.01$ (SE 0.0083) at the one-year horizon and of $\hat{\sigma} = 0.026$ (SE 0.0181) at the two-year horizon. At both horizons, the confidence interval of the slope includes zero and clearly excludes the predicted benchmarks slopes of 0.24 to 0.48 (depicted).

4.3 Difference-in-Differences Design

We next investigate the regression analogue of the non-parametric analysis, to formally test for pre-trends (e.g., due to anticipation effects) and to include a rich set of controls. The estimated wage-benefit sensitivities range from *negative* 1.4 to positive 2.4 cents on the dollar after one and two years. The confidence intervals for our preferred specifications reject sensitivities above 3.3 cents on the dollar.

4.3.1 Econometric Framework

Our difference-in-differences design regresses wage changes, $dw_{i,r,t} = w_{i,r,t} - w_{i,r,t-1}$, on reform-induced – actual and placebo – benefit changes, $db_{i,r,t}$, both normalized by lagged wages:

$$\begin{aligned}\frac{dw_{i,r,t}}{w_{i,r,t-1}} = \sum_{e=-3}^{0} \sigma_e \cdot \left(\mathbb{1}_{(t=r+e)} \times \frac{db_{i,r,t}(\tilde{w}_{i,r,t})}{w_{i,r,t-1}} \right) + \tau_{r,P_{t-1}} + \\ \theta_{r,t-1} + \gamma_{r,t-1} \ln w_{i,r,t-1} + X'_{i,r,t-1}\phi_{r,t-1} + \epsilon_{i,r,t}\end{aligned} \tag{19}$$

where r denotes specific reforms (as we let control variables vary between reforms).

The coefficient of interest is σ_0. It captures the effect of the reform-induced benefit changes on wages during the reform year relative to one year prior, which we normalize to zero ($\sigma_{-1} = 0$). In addition to the one-year horizon, we also conduct the analysis using two-year wage outcomes (then normalizing σ_{-2} to zero and omitting σ_{-1}).

In all specifications, we control for earnings percentile fixed effects, $\tau_{r,P_{t-1}}$, year effects, $\theta_{r,t-1}$, time-varying controls for lagged earnings, $\ln w_{i,r,t-1}$, and an eligibility control for the regional PDB extension described in Footnote 14. The percentile fixed effects $\tau_{r,P_{t-1}}$ absorb permanent wage growth differentials across percentiles, e.g., due to mean reversion. They are reform-specific, i.e. common between reform and placebo years for a given reform, but separate between reforms. Calendar year effects, $\theta_{r,t-1}$, absorb aggregate wage growth shifts. Year-specific parametric earnings controls, $\ln(w_{i,r,t-1})$, account for time-varying shocks to different parts of the earnings distribution. We then incrementally add covariates $X_{i,r,t-1}$ with year-specific coefficients to absorb other time-varying shocks. First, we add demographic controls (sex, cubic polynomials of experience, tenure, and age). The second set contains industry-by-occupation-by-year fixed effects (four-digit industry by white/blue collar occupation). Third, our most fine-grained specification includes firm-by-year effects to isolate variation between workers in the same firm.

The core identification assumption of our difference-in-differences design requires conditional parallel trends: conditional on the controls, in particular percentile and year effects and time-varying parametric earnings controls, the average wage growth among groups treated by a benefit reform compared to those whose benefits remained unchanged would have followed parallel trends absent the reform. A potential violation of our identification assumption would occur if treated groups experienced an additional wage growth shock contemporaneous with the reform. Since we include time-varying parametric controls for lagged earnings, $\ln w_{i,r,t-1}$, these types of shocks to different parts of the earnings distribution would have to be quite sharply delineated.

We test the parallel trends assumption in the pre-period by assigning placebo reforms: in pre-reform years, $e < 0$, we assign the average db/w of workers in a given earnings percentile in the actual reform year to workers in that percentile in $e < 0$. A violation of the parallel trends assumption in the pre-period would occur if the placebo reforms were associated with excess wage growth as captured by $\sigma_e \neq 0$ for $e < 0$ (as we include percentile fixed effects).

We estimate specification (19) using the procedure in Correia (2017) and stack data for all reforms $r \in \{1976, 1985, 1989, 2001\}$. We restrict the earnings ranges for each reform to the "treatment" and "control" percentile groups of Section 4.2. For each reform, we add three pre-period years (the maximal amount to still study the 1976 reform, since our data start in 1972).[21] We report standard errors based on two-way clustering at the individual and the earnings percentile level (the level of our treatment variation). In Appendix Figure A.8, we confirm that other clustering levels (firm, percentile, individual, and reform-specific percentiles) lead to similar confidence intervals. We winsorize wage growth at the 1st/99th percentile; Appendix Figure A.9 confirms robustness to no winsorization as well as at the 5th/95th percentiles.

4.3.2 Visual Regression Results

To provide a bridge between Figure V and our difference-in-differences specification (19), we plot wage growth against reform-induced benefit changes in binned scatter plots and incrementally add year effects, earnings percentile effects, and year-specific earnings controls in Figure VI. The figure plots the slope, i.e. coefficient σ_0 in Equation (19), of a binned scatter plot of residualized earnings changes and benefit changes in the treatment year.[22] The panels also plot, in dashed yellow lines, the predicted relationship from our calibrated Nash bargaining model using the 0.48 wage-benefit sensitivity. Panel A only includes year fixed effects as controls. Panel B adds earnings percentile fixed effects and Panel C adds a year-specific log-earnings control.

[21] We have also assessed robustness to longer pre-periods ($L = 5$) while dropping that earliest reform in unreported results.

[22] We residualize the independent and dependent variables in the entire sample of reform and placebo years, so that the best fit lines match the coefficients in Table III.

Across all three specifications, we find no evidence of positive effects of benefits on earnings; instead, we find small, negative point estimates with tight standard errors. To assess our identifying assumptions, we present similar binned scatter plots in the $e = -3$ and $e = -2$ placebo years in Appendix Figure A.6. For $e = -2$, we find no evidence for placebo effects across specifications. For $e = -3$, we find some pre-trends or significant placebo estimates unless we include log earnings controls, as is customary in the simulated instruments literature. The visual analysis also allows us to assess the validity of the conditional parallel trends assumption. At shorter horizons, the parallel trends assumption holds without additional controls. At longer horizons, it holds conditional on time-varying parametric controls for lagged earnings.

4.3.3 Full Regression Results

Mirroring the non-parametric analysis, the difference-in-differences analysis reveals that wages are insensitive to benefit changes. The point estimate for the wage-benefit sensitivity is $\hat{\sigma} = -0.007$ (SE 0.012) after one year and $\hat{\sigma} = -0.014$ (SE 0.024) after two years in our preferred specifications with firm-by-year fixed effects in column (6) of Table III. Confidence intervals thus let us rule out wage increases above \$0.03 (more precisely, \$0.033) for a \$1.00 increase in UIBs both at the one- and two-year horizon in our preferred specification.

One-Year Effects Panel A in Table III presents one-year wage effects, i.e. estimates of σ_e. The regressor of interest is σ_0, capturing the wage growth associated with reform-induced benefit changes. Column (1) includes the same controls as in Figure VI(c), and the subsequent columns progressively add further controls. We normalize σ_{-1} to zero and assess pre-trends with σ_{-3} and σ_{-2}. Across all six specifications, we cannot reject that both pre-period estimates are jointly equal to zero, which supports our identification assumption.

Across columns, effects are quantitatively similar, centered at zero. Column (1), without additional control variables, reports estimate $\hat{\sigma}_0 = -0.009$ (SE 0.015), ruling out effects above 0.02. Column (2) finds a similar estimate when adding Mincerian controls. Our estimates remain small at -0.016 and -0.010 in columns (3) and (4), with industry-occupation-year fixed effects and then all controls jointly, while standard errors remain relatively unchanged.

Two-Year Effects Panel B in Table III reports the analogous longer-run effects of the reforms, at the two-year horizon. Column (4), with Mincerian controls and industry-occupation-year fixed effects, estimates $\hat{\sigma} = -0.002$ (SE 0.025). Similar estimates emerge with fewer controls in columns (1)–(3), ranging between -0.009 and 0.024. The pre-period effects of placebo reforms remain statistically insignificant.

Intrafirm Variation Next, we assess whether changes in the nonemployment outside option *between workers within the same firm* lead to wage changes, by including firm-by-year fixed effects in columns (5) and (6). At the one-year horizon (Panel A in Table III), the within-firm variation also leads to zero effects of -0.007, even more precisely estimated than those in columns (1)-(4). Similarly, at the two-year horizon (Panel B in Table III), the effects remain small, negative, and insignificant.

Parametric Earnings Controls Consistent with the simulated instruments literature (Gruber and Saez, 2002; Kleven and Schultz, 2014), our main specifications include time-varying controls for (log) earnings. (As Figure VI and Appendix Figure A.6 show, the parallel trends assumption holds in the pre-period without conditioning on parametric earnings controls. At longer horizons, it holds conditional on such controls.) We present variants of our main specification (column (4) in Table III) with alternative earnings controls in Appendix Figure A.10, namely log, linear, and linear percentiles, which all yield very similar estimates around zero.

Validation Exercise To assess the extent to which reform-induced benefit changes, assigned based on lagged earnings, shift benefits implied by realized earnings, we estimate a variant of (19) with reform-induced benefit changes implied by realized earnings $\frac{b_t(w_{i,t})-b_{t-1}(w_{i,t})}{w_{i,r,t-1}}$ as the dependent variable. The contemporaneous coefficient on $\frac{db_{i,r,t}(\tilde{w}_{i,r,t})}{w_{i,r,t-1}}$ could be close to zero if, hypothetically, an individual's earnings were independently redrawn each year, because then wage earnings in $t = r - 1$ would not predict earnings and thus benefits in $t = r$. We report results and write out the formal regression model in Appendix Table A.3. The analysis reveals a 0.813 (SE 0.014) coefficient at the one-year horizon and of 0.511 (SE 0.024) at the two-year horizon, confirming that the reforms affected benefits in the treatment percentiles.[23] The effects are also stable when we add in more detailed controls, even firm-by-year effects. Moreover, pre-period coefficients now test whether the same earners have systematically seen benefits change due to schedule changes or wage growth. In line with our selection of reforms, these placebo effects are an order of magnitude smaller than the reform effects ($t = r$).[24]

Transitory vs. Permanent Treatment Treatment status may be imperfectly persistent due to idiosyncratic wage changes. To evaluate this dynamic, we estimate "donut hole" treatment effects, whereby we drop individuals situated within varying bandwidths on both sides of the treatment/control earnings cutoff, and are hence particularly prone to switching status in future years. Coefficient estimates, reported in Appendix Figure A.11, indicate no evidence of increasing

[23]Excluding the 2001 reform from this validation exercise (because the reform occurred at a time when benefits were determined based on lagged years' wages) yields quantitatively very similar results with a 0.755 (SE 0.013) coefficient at the one-year horizon and of 0.481 (SE 0.028) at the two-year horizon. We also report instrumental variable estimates in Appendix Table A.4, formally interpreting the validation exercise as a first stage relationship. The IV specification leaves our conclusions quantitatively unchanged as standard errors increase only slightly.

[24]Their very high precision renders the pre-reform one-year coefficients statistically significantly different from zero. For the two-year validation, we cannot reject that the pre-period coefficients are jointly equal to zero.

treatment effects, even when dropping 25% of our sample, suggesting that transitory treatment is unlikely to mask an underlying larger effect. This finding also suggests that potential limited capacity of firms to differentiate wages by treatment status around the cutoffs (as in wage posting models) may not drive the absence of wage effects. Finally, a subsample analysis in Section 5 will not find larger treatment effects for very stable earners, e.g., with a low predicted separation rate.

Accounting for Non-Taxation of UIBs Austrian UIBs are not taxed. Our gross-wage-based estimates should imply even smaller sensitivities, which we confirm in Appendix Table A.7 (graphical analysis in Appendix Figure A.19). There, we replicate our main results from Table III but rescale the untaxed UIB shifts into gross UIB shifts (imputing an individual's average net-of-tax rate following a tax calculator detailed in Appendix G). As the tax imputations are tentative, our main results use raw net (untaxed) UIBs, hence likely *overestimating* sensitivities.

Separation Effects To rule out selective attrition, we also report treatment effects on separations and sickness in Appendix A.5. Across specifications and outcomes, the benefit increases were associated with quantitatively negligible and largely statistically insignificant effects.[25]

4.4 Wage Sensitivity by Post-Separation Time in Unemployment τ

As illustrated in Figure Ib, the baseline model's sensitivity of N to b – and thus that of w to b – increases in post-separation time on UI, τ, the weight on instantaneous payoff b.

Reporting results in Figure VII, we now estimate wage effects across worker subsamples sorted into quantiles by their idiosyncratic predicted post-separation time on UI, $\hat{\tau}_i$, which is the weight the wage bargain puts on UIB b in the wage equation (6). The quantiles, sorted within each reform, start from deciles; to obtain additional dispersion, we further split the top and bottom decile into two ventiles each, and then further split up the resulting very top/bottom ventiles into two. We thus study a total of 14 quantiles. On the x-axis, we plot the group-specific mean predicted $\hat{\tau}_i$. The y-axis reports two wage-benefit sensitivities. First, the dashed yellow line plots the quantile-specific *model-predicted* wage-benefit sensitivity based on Equation (6) and $\phi = 0.1$, drawing on the quantile's mean $\hat{\tau}_i$. (A negative correlation between τ_i and ϕ_i would steepen the gradient.) Second, the blue line (squares) traces out the *empirical* sensitivities, as heterogeneous treatment effects from our main regression model (19) interacting reform-induced UIB shifts with indicators for a worker's $\hat{\tau}_i$ quantile.

Figure VII reveals substantial variation in $\hat{\tau}_i$ – ranging from around 0.02 to nearly 0.20 –, and thus in the model-predicted wage-benefit sensitivity – from around 0.15 to above 0.60. By

[25]By contrast, Jäger, Schoefer, and Zweimüller (2018) document separation effects among workers from a reform that dramatically raised potential benefit duration for older workers in Austria, perhaps used as a bridge into early retirement.

contrast, the empirical gradient of wage effects is *flat at zero*, just as much among workers likely to experience long periods of UI – for whom the UIB increases should mechanically raise nonemployment values by more – as among workers whose separations rarely entail long UI receipt.

4.5 New Hires' Wage Sensitivity

Perhaps wage stickiness among incumbent workers slows down wage adjustments even after two years. We therefore estimate the treatment effects separately for job stayers and various mover types, whose wages are more likely to reset flexibly. By studying wages of new hires, this analysis also tests whether employer competition models (Cahuc, Postel-Vinay, and Robin, 2006) or contractual models with insurance (Beaudry and DiNardo, 1991; Bertrand, 2004) may play a role in the insensitivity of average (i.e. largely incumbent workers') wages.

Panel A of Table IV displays the one- and two-year treatment effects for job stayers, recalled workers, and job movers. We classify movers by their first type of transition from the original job in the base year. As described in Section 4.1 (with more details on how we construct the transition types in Appendix Section E.6), we consider only *post-separation wages* rather than average annual earnings for job movers. We interact an indicator for each transition type with the σ_e coefficients, the parametric year-specific earnings controls and the baseline earnings percentile fixed effects. For job stayers, the effects are small and insignificant and precisely estimated. For recalled workers, we see substantial increases in standard errors and point estimates of -0.069 (SE 0.118) at the one-year horizon and of 0.077 (SE 0.091) at the two-year horizon. For job movers, we see estimates of 0.109 (SE 0.132) at the one-year horizon and of 0.116 (SE 0.083) at the two-year horizon. While the point estimates are still substantially smaller in magnitude than the theoretical benchmarks, the upper ends of the confidence intervals for job movers and for recalled workers (at the two-year horizon) do intersect with our range of theoretical predictions. Importantly, this is a consequence of wide confidence intervals, which also include negative sensitivities of up to -0.15 for job movers and -0.30 for recalled workers.

We have further divided movers into "EE" movers, who directly move from one employer to another, and "EUE" movers, who first undergo an unemployment spell with UI receipt ("EUE"). Of particular theoretical interest are EUE movers. First, these workers receive UI benefits, and then rebargain with their next employer with UI on hand. Second, the wage responses of these new hires from unemployment are allocative for aggregate employment in standard matching models.[26] Third, these workers should exhibit the standard, large sensitivity of wages to UI shifts even in richer models with employer competition and external job offers as in Cahuc, Postel-Vinay, and Robin (2006), because these workers' best outside option is still nonemployment.

[26] Pissarides (2009) summarizes this paradigm. Richer models, as when firms face financial constraints, give allocative consequences to incumbents' wages, too (e.g., under financial constraints as in Schoefer, 2015).

Even the results for EUE movers, presented in Panel B of Table IV, do not reveal positive effects. In fact, the point estimates are negative, and remain insignificant with controls specific to transition type (our most fine-grained but also most demanding specification since a lot of variation is absorbed). Across specifications, the standard errors are large, between 0.14 and 0.23, so that the confidence intervals also include sizable negative and positive sensitivities.[27]

Estimates for direct EE movers are in Table IV Panel C. Standard errors again are fairly large between 0.09 and 0.12. This sample exhibits some positive effects at the one-year horizon, which however decrease to zero once we interact controls by transition type. At the two-year horizon, we find a point estimate of 0.135 (SE 0.100) in the specification with the sparsest set of controls and a negative estimate of -0.072 (SE 0.124) in the specification with the transition-specific control variables.

There are a few caveats to consider. First, worker transitions may be affected by the reforms, and since we condition on an endogenous outcome, selection may show up as wage effects. Second, for EUE movers, there are non-bargaining channels affecting re-employment wages, such as reservation wages, skill depreciation, or employers' statistical discrimination by nonemployment duration. These potential confounders among EUE movers had in part motivated our strategy of primarily studying on-the-job wage changes of incumbent workers in the first place.

5 The Missing Link Between Wages and Benefits

We now dissect the wage-benefit insensitivity along the following three-element chain:

$$\frac{dw_i}{db_i} = \underbrace{\frac{dw_i}{d\Omega_i}}_{\substack{\text{Sensitivity of wages}\\\text{to outside options}}} \times \underbrace{\frac{d\Omega_i}{dN_i}}_{\substack{\text{Sensitivity of outside option}\\\text{to nonemployment value}}} \times \underbrace{\frac{dN_i}{db_i}}_{\substack{\text{Sensitivity of nonemp. value}\\\text{to UIB shifts}}} \tag{20}$$

To assess the relative importance of these three factors for the insensitivity result, we conduct a battery of heterogeneity analyses. In particular, for each dimension of heterogeneity, we run our main specification (mirroring column (4) in Table III) and include interactions between heterogeneity group indicators with the treatment variable (and placebo treatments in pre-reform years). Table V presents a summary of these estimates. Appendix Section E.7 describes the variable construction. We find only little variation across groups. Additionally, we report below that wages remain insensitive to alternative treatment definitions (potential duration rather than benefit levels; firm- and industry-level average of the instrument).

[27] In unreported results, some EUE wage effects were closer to zero, namely with alternative earnings controls (as in Appendix Figure A.10), or when we drop very low (perhaps noisy) earners. We also have found positive but insignificant duration effects, which can rationalize the negative EUE wage effects when drawing on the very high end of UI effects on duration (Lalive, Van Ours, and Zweimüller, 2006; Card et al., 2015), and duration effects on wages (Schmieder, von Wachter, and Bender, 2016; Nekoei and Weber, 2017).

5.1 The Nonemployment Value and UI Benefits dN/db

A first reason why wages are insensitive to UI benefits may be that the nonemployment value does not move with the UI reform as predicted by our model.

Unemployment Risk We provide additional proxies for unemployment risk or for experience with the UI system, studying heterogeneity by unemployment (separation) risk, and the local unemployment rate. These unemployment risk proxies are not consistently associated with larger one-year point estimates, and hover around zero.

Experience with, and Salience and Knowledge about UI Limited knowledge or salience of UIB levels could diminish wage responses (as, e.g., in the context of complex tax incentives in Abeler and Jäger, 2015). Several pieces of evidence speak against this explanation. First, wages remain insensitive even after two years and in response to large (and plausibly more salient) shifts. Second, even recent UI recipients (and EUE switchers in Section 4.5) – plausibly more aware of the UIB schedule (Lemieux and MacLeod, 2000) – do not exhibit higher sensitivity, which we test by splitting up the sample by a worker's actual UI history (months since last UI receipt or nonemployment spell). We find some suggestive evidence of larger effects for recently reemployed workers at the two-year horizon, however, with point estimates remaining substantially below the theoretical benchmark. Third, compared to other sources of idiosyncratic variation in the nonemployment value, UIBs largely depend on recent earnings, information available to both parties. Fourth, we additionally analyze a 2006 Eurobarometer survey asking Austrian workers about beliefs about their hypothetical UI replacement rates (European Commission, 2012). The histograms in Appendix Figure A.1, of beliefs and actual rates (from the AMS/ASSD, binned into the survey intervals), align closely. The average worker's rate is 64.03% (SE 0.72) in the survey, close to the 65.29% among actual recipients.[28] Moreover, we found in unreported results that workers with more children correctly predict higher benefits. Fifth, we have found positive albeit noisily estimated effects of the reforms on UI take-up (Appendix Table A.2). Lastly, Jäger, Schoefer, and Zweimüller (2018) document that employed workers separated in response to, and hence were aware of, UI PBD extensions in Austria.

Variation in UI Generosity From an Age-Specific PBD Reform Appendix F additionally reports the effects of reforms to the potential benefit duration (PBD) of UIBs (rather than their level) on incumbents' wages, exploiting a 1989 reform for workers aged 40 and above. We do not find wage effects of this dimension of UI generosity either, even two years after the reform. This design complements our previous designs as the treatment assignment was age- rather than

[28] The replacement rate can deviate from 55% due to lump sum benefits for dependents, and the earnings base for benefits post-1996 are lagged annual earnings rather than current earnings, as in the survey.

past-income-based, the reform was permanent (rather than potentially eroded by inflation or subsequent benefit shifts) and perhaps more salient and simple. The age dimension divides workers who are almost certainly in the same market and close production substitutes.

5.2 Outside Options and the Nonemployment Value $d\Omega/dN$

A second reason why wages are insensitive to UI benefits may be that the nonemployment value (while shifting with b) may not shift the relevant outside option in wage bargaining.

External Job Offers and Job Mobility We sort workers by several measures of recent nonemployment, including months since UI receipt and months since last nonemployment spell. These measures proxy for the likelihood of not yet having received potential outside offers. Outside job offers may insulate wages from changes in the nonemployment value by ratcheting up wages as in models of employer competition and on the job search (Postel-Vinay and Robin, 2002; Cahuc, Postel-Vinay, and Robin, 2006). At the one-year horizon, we do not find that recently nonemployed workers exhibit larger wage-benefit sensitivities (Table V). We find some evidence for this prediction at the two-year horizon.

Group-Level Bargaining Rather than atomistic bargaining between one individual worker and one firm, real-world wage setting may occur with groups of workers (or employers). Here, the average worker's outside option may matter, as in union bargaining models, similarly for wage posting models in which firms are constrained to offer a single wage. First, we construct the average of the worker-level reform-induced benefit variation at the establishment level, as *firms* play an important role in Austrian wage setting (e.g., through plant-level works councils in Austria, see Section 3). Second, we do the same at the *industry-by-occupation* level, at which collective bargaining agreements between employer associations and unions are typically concluded in Austria, typically distinguishing white and blue collar workers (e.g., white-collar workers in the insurance industry). Most CBAs cover all of Austria; some are state-specific (see Knell and Stiglbauer, 2012). Our industry proxy is 3-digit NACE. We plot histograms of benefit variation averaged at the group level in Appendix Figure A.7. We then adapt our worker-level regression specification (19) with the group-level treatment. We include the main controls from specification (19) and additionally include two sets of group-level control variables: reform-specific percentile fixed effects for (i) the average treatment at the group level in a given year and (ii) the reform-sample specific share of workers in the group cell with a positive treatment. We also report firm-level specifications with industry-by-occupation-by-year effects, and industry-by-year (3-digit, our CBA proxy) and occupation-by-year effects in the industry-by-occupation-level specifications.

Table VI reports small firm-level point estimates ranging from 0.029 to 0.044 (0.069 to 0.074) at the one-year (two-year) horizon. The confidence intervals include the worker-level point estimates and zero with standard errors around 0.03 and 0.04 (0.05) in the one-year (two-year) specification. Table VI also reveals pre-trend violations for the specifications without industry-by-occupation-by-year effects. This suggests that firms with different shares of reform-affected workers were on different trends, perhaps because of industry-level shifts that were correlated with treatment intensity. When we include industry-by-occupation effects (specifications 3 and 4), comparing workers in the same industry and occupation but working at firms with different benefit shifts, we find that pre-trends are flat and point estimates for the pass-through remain between 0.044 (one-year horizon) and 0.074 (two-year horizon) in our most fine-grained specifications with confidence intervals ruling out effects larger than 0.16. Quantitatively, the evidence is thus also hard to square with a firm-level variant of the Nash benchmark, although the slightly larger point estimates could be consistent with a small effect at the firm level.

Table VII reports results for the industry-by-occupation level. Point estimates are less stable across specifications and have substantially wider confidence intervals. In specifications with the most fine-grained controls, i.e. industry-by-year and occupation-by-year effects in columns (5) and (6), we find negative point estimates between -0.094 and -0.071 (-0.157 and -0.074) at the one-year (two-year) horizon, with confidence intervals ruling out pass-through above 0.16. Specifications with fewer controls suggest larger effects, yet violate our identification assumption due to statistically significant placebo estimates in the pre-period, thus suggesting that the inclusion of control variables is important to account for, e.g., occupation-by-year-specific shocks. An additional caveat to the analysis reported in Table VII is that administrative industry proxies may only imperfectly overlap with actual CBA units and miss all regional differentiations.[29]

Lastly, in Table V, we also include an heterogeneity analysis for worker-level wage effects by the firm's share of employees recently nonemployed, whose reservation wages may shift most with UI, and do not find larger wage responses in those firms.

5.3 Wages and Outside Options $dw/d\Omega$

A final reason why wages are insensitive to UI benefits may be that while UI benefits shift the nonemployment value, real-world wage setting is insensitive to outside options more generally.

Worker Bargaining Power Workers with lower bargaining power should exhibit larger sensitivity to outside options. We start by splitting workers by *age* as well as occupation class (*blue vs. white collar*). The results show no clear effect heterogeneity. Since female workers' wages appear less sensitive to productivity shifts (Black and Strahan, 2001; Card, Cardoso, and

[29] The typical Austrian CBA mandates a wage floor ("*Kollektivvertragslohn*"), plus a percent raise for any job with above-floor prevailing wages ("*Istlöhne*"). In additional case studies of digitized CBAs (from *KVSystem - Kollektivverträge Online*, with best coverage around the 2001 reform), wage floors did not appear to shift differentially for treated vs. untreated groups (instead prescribed the usual homogeneous wage increases).

Kline, 2016), perhaps due to lower bargaining power, we then consider *sex*, finding somewhat larger effects among women at the one-year horizon (although the pattern reverses at the two-year horizon).

Firm Wage Premia We calculate firm fixed effects following the AKM methodology in (Abowd, Kramarz, and Margolis, 1999) and estimate the wage-benefit sensitivity in firms with high or low firm effects. In both groups, estimates are close to zero at the one-year horizon. At the two-year horizon, the sensitivity is around 0.09 in low-AKM firms, which is consistent with the idea that worker-sided renegotiation is more likely in firms with low wages (as in MacLeod and Malcomson, 1993). Yet, the estimated sensitivity remains below the calibrated benchmark.

Wage Adjustment Frictions and Infrequent Renegotiation Perhaps wage stickiness or downward wage rigidity in continuing jobs masks wage pass-through in the short run. Alternatively, rebargaining of a given wage may only occur if it otherwise were to leave the bargaining set, i.e. fall short of (exceed) the worker's (firm's) reservation wage (as in e.g., MacLeod and Malcomson, 1993). Several pieces of evidence speak against this explanation. First, we found no wage effects even after two years, when stickiness should bind for a smaller fraction of jobs.[30] Second, most of our reforms should induce *upward* wage pressure, making downward wage rigidity less binding. Third, our visual inspection did not suggest wage effects even for larger treatment, where menu costs could be overcome. Fourth, we have not found positive wage effects in settings less constrained by wage rigidity, e.g., in new jobs or in growing industries. Fifth, we also have not found wage increases in job types perhaps reflecting lower worker surplus.

Moreover, we find a zero effect in subsamples of firms with flexible wage policies. We stratify firms by the standard deviation of within-firm wage growth, perhaps indicating wage differentiation facilitating wage pass-through. We also consider a proxy for a firm's distance from the CBA-level wage floor, approximated as the firm-level standard deviation of the residuals from a regression of log wages on industry-occupation-tenure-experience-year fixed effects.

The Prevalence of Wage Bargaining Perhaps wage bargaining may not determine real-world wage setting in any pocket of the Austrian labor market. However, a vast body of empirical work points to patterns consistent with wage bargaining, such as rent sharing. Moreover, survey evidence on the actual presence of bilateral bargaining suggests that both workers and employers report the presence of bargaining in a substantial part of the labor market (Hall and Krueger, 2012; Brenzel, Gartner, and Schnabel, 2014). Here, we do not find wage effects (i) in subsamples that carry the correlates of prevalence of wage bargaining according to those surveys, such as

[30] More than half of wage contracts appear to reset each year (Barattieri, Basu, and Gottschalk, 2014; Sigurdsson and Sigurdardottir, 2016), and incumbents' wages are still half as sensitive to aggregate shocks as new hires' wages (Pissarides, 2009). Dickens et al. (2007) find lower downward wage rigidity in Austria than in Germany or the U.S.

tighter labor markets (lower unemployment), (ii) for workers with higher education (our proxy: white rather than blue collar), or (iii) among men. This suggests that even in pockets of the labor market where we expect bargaining to occur, nonemployment value shifts do not entail wage effects.

6 Conclusion

We have studied the effects of changes in the value of nonemployment on wages brought about by reforms to unemployment insurance (UI) benefit levels in Austria, a setting where UI enters the nonemployment scenario for most workers. Wages appeared fully insulated from these UI-induced shifts in the value of nonemployment, even after two years and in all pockets of the labor market. This limited wage pressure may carry over to other UI-like policies that boost workers' nonemployment value, at least in the short run and if group-specific.

This empirical wage insensitivity is inconsistent with the large theoretical sensitivity of the commonly used Nash bargaining model specified with nonemployment as workers' outside option. To reconcile our findings with that model, workers would need to hold nearly full bargaining power. Yet, this unitary bargaining power is, in turn, rejected by the large body of rent sharing estimates implying low worker bargaining power of around 0.1.

Our findings instead support wage setting protocols that insulate wages from nonemployment values. The kind of wage insensitivity we document also helps models of the aggregate labor market generate realistic labor demand fluctuations. Our findings also raise the possibility that the empirical comovement between wages and labor market conditions, such as the Phillips and wage curves, may be driven by mechanisms other than the procyclicality of workers' nonemployment value.

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Tables

Table I: Predicted Fraction of Post-Separation Time on UI (τ) and Wage-Benefit Sensitivity (dw/db)

Time Restriction:	Ret'nt or Disability		Retirement		No Restriction	
Reemployment Restriction:	4-Year	None	4-Year	None	4-Year	None
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Fraction of Post-Separation Time (τ) (Predicted Values)						
UI-Affected Nonemployment - $\hat{\tau}^U$	**0.104**	0.105	0.096	0.095	0.094	0.092
Unemployment Insurance - $\hat{\tau}^{U:UI}$	**0.083**	0.083	0.076	0.074	0.073	0.070
Unemployment Assistance - $\hat{\tau}^{U:UA}$	**0.021**	0.022	0.020	0.022	0.020	0.021
Employment - $\hat{\tau}^E$	**0.684**	0.588	0.648	0.564	0.633	0.555
Other Nonemployment - $\hat{\tau}^O$	**0.212**	0.307	0.255	0.341	0.274	0.353
Panel B: Predicted Wage-Benefit Sensitivity (dw/db)						
Baseline: Two-State dw/db Prediction						
$\mathrm{E}[dw/db(\hat{\tau}_i)]$	**0.462**	0.462	0.442	0.437	0.434	0.426
$dw/db(\mathrm{E}[\hat{\tau}_i])$	$\boxed{\mathbf{0.483}}$	0.486	0.464	0.461	0.457	0.453
Robustness: Three-State dw/db Prediction						
$\mathrm{E}[dw/db(\hat{\tau}_i)]$	**0.251**	0.217	0.224	0.194	0.217	0.190
$dw/db(\mathrm{E}[\hat{\tau}_i])$	$\boxed{\mathbf{0.243}}$	0.201	0.208	0.174	0.196	0.165

Note: The first five rows present estimates of the predicted amount of time a worker would spend on unemployment insurance $\hat{\tau}^{U:UI}$, on unemployment assistance $\hat{\tau}^{U:UA}$, which we also pool into a single *UI-affected state* $\hat{\tau}^U$ (the sum of $\hat{\tau}^{U:UI}$ and $\hat{\tau}^{U:UA}$, where unemployment assistance is included because it is indexed nearly one-for-one with UI, and we refer to "UI" in the text as encompassing both), employed $\hat{\tau}^E$, and in other nonemployment $\hat{\tau}^O$. The τ values for our preferred specification, column (1), are calculated as follows. Starting with our baseline regression sample, we keep individuals who separate from employment into nonemployment for at least one day in the next year and return to employment at least once within the next four years. For these actual separators, we calculate post-separation share of time spent in each of the above labor market states. We stop including labor market states in this share at the earliest of 16 years, reaching age 70, death, or absorbing retirement or disability (defined as entering retirement or disability and without any subsequent employment or UI/UA spells). Using the separators sample, we estimate a regression model predicting the time spent in each state based on individuals' pre-separation characteristics comprising of industry by white/blue collar fixed effects, tenure, experience, age, region, year and previous UI history. We then use this model to predict the specific τs for the entire regression sample (including non-separators). The reported τ estimates are the average predictions across the entire regression sample. The dw/db predictions plug in the predicted τ values into the two- and three-state model wage-benefit sensitivity expressions (see Section 2.1 and Appendix Section C respectively). The $\mathrm{E}[dw/db(\hat{\tau}_i)]$ estimates report the average sensitivity first plugging in the individual-level $\hat{\tau}_i$ sensitivities into the wage-benefit expressions and then taking the average across individuals (thus respecting Jensen's Inequality). The $dw/db(\mathrm{E}[\hat{\tau}_i])$ estimates plug in the average $\hat{\tau}_i$ values from rows (1) to (6) into the wage-benefit sensitivity expressions. Columns (3) and (4) also stop counting at absorbing retirement but not disability, and columns (5) and (6) stop counting labor market states only at the earliest of 16 years or age 70. The reemployment-restriction columns (1), (3) and (5) requires that individuals in the separator sample return to re-employment (at any job) sometime in the next four years (for at least one day). Appendix Table A.5 reports the realized τ values for the separator samples and the analysis sample unconditionally on a separation.

Table II: Summary Statistics

	1976 Reform		1985 Reform		1989 Reform		2001 Reform		Pooled Reform	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Proportion Women	.893	.902	.511	.23	.65	.876	.466	.82	.54	.61
	(0.309)	(0.297)	(0.500)	(0.421)	(0.477)	(0.329)	(0.499)	(0.384)	(0.498)	(0.488)
Age	40.4	40.5	38.9	39.6	38.5	39.6	38.7	39.6	38.8	39.7
	(8.263)	(8.172)	(8.406)	(7.996)	(8.352)	(7.932)	(8.021)	(7.651)	(8.234)	(7.871)
White Collar	.39	.312	.384	.546	.386	.418	.464	.521	.42	.5
	(0.488)	(0.463)	(0.486)	(0.498)	(0.487)	(0.493)	(0.499)	(0.500)	(0.493)	(0.500)
Experience in last 25 Years	10.3	9.91	15.9	18.4	15	13.3	15.5	13.4	15.2	15.1
	(6.339)	(6.039)	(5.968)	(6.118)	(5.820)	(5.793)	(6.258)	(5.994)	(6.212)	(6.594)
Tenure	2.83	2.81	7.22	8.67	7.3	6.36	7.74	6.26	7.2	7
	(1.119)	(1.114)	(4.115)	(4.213)	(5.060)	(4.745)	(6.546)	(5.602)	(5.489)	(5.011)
Avg. Monthly Earnings	4,171	2,885	13,455	20,696	13,777	8,784	24,268	15,683	17,629	15,544
	(294)	(503)	(1,237)	(2,103)	(1,086)	(1,920)	(2,074)	(3,060)	(6,343)	(5,824)
Observations in Base Year	49,315	50,762	268,708	338,999	175,702	170,036	370,786	328,345	864,511	888,142

Note: This table includes summary statistics for the control and treatment regions for the four reforms that make up the pooled sample on which we run our analysis: 1976, 1985, 1989, and 2001. Standard deviations are reported in parentheses beneath the means. All values are calculated from individuals employed all 12 months in the base year for the reform, which is defined as the year prior to the reform, e.g., 1975 for the 1976 reform. The pooled sample appends the four reform samples together. The actual number of observations in the base year will be slightly larger than the sum of the treatment and control groups for the 1985 reform sample and thus the pooled sample because the control region is shifted slightly down the income table to account for repeated treatment in a small section of the income distribution during the placebo period for that reform. Since we first construct the sample based on treated earnings regions and equal-sized control regions and *then* apply further sample restrictions (see Appendix E), the treatment and control regions do not have identical numbers of observations. Importantly, this table is *not* a balance check between "treatment" and "control" regions, which naturally must differ in a given cross section. Instead, our difference-in-differences design (with varying treatment intensity within the treatment group) relies on a conditional parallel trends assumption which we discuss in Section 4.3.

Table III: Estimated Wage Effects: Difference-in-Differences Regression Design

	Panel A: 1-Year Earning Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.018	0.001	0.020	0.019	0.026	0.035
	(0.015)	(0.015)	(0.017)	(0.016)	(0.015)	(0.016)
Placebo: 2 Yr Lag	0.007	-0.003	0.001	-0.0002	0.021	0.018
	(0.016)	(0.016)	(0.017)	(0.018)	(0.015)	(0.015)
Treatment Year	**-0.009**	**-0.004**	**-0.016**	**-0.010**	**-0.007**	**-0.007**
	(0.015)	**(0.015)**	**(0.016)**	**(0.017)**	**(0.012)**	**(0.012)**
Pre-p F-test p-val	0.508	0.951	0.488	0.459	0.209	0.093
R^2	0.050	0.068	0.088	0.104	0.275	0.295
N (1000s)	7142	7142	7138	7138	6302	6298
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

	Panel B: 2-Year Earning Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.010	-0.007	0.019	0.018	0.011	0.020
	(0.019)	(0.018)	(0.024)	(0.023)	(0.020)	(0.022)
Placebo: 2 Yr Lag						
Treatment Year	**0.008**	**0.024**	**-0.009**	**-0.002**	**-0.010**	**-0.014**
	(0.025)	**(0.025)**	**(0.026)**	**(0.025)**	**(0.024)**	**(0.024)**
Pre-p F-test p-val	0.594	0.683	0.445	0.434	0.599	0.349
R^2	0.112	0.136	0.153	0.173	0.321	0.348
N (1000s)	5045	5045	5042	5042	4439	4437
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification (19). Standard errors based on two-way clustering at the individual and earnings percentile level are in parentheses. The null hypothesis of the F-test is that the coefficients of interest are jointly all equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation. All specifications also include reform-specific earnings percentile fixed effects, year fixed effects, and year-specific log earnings controls.

Table IV: Wage Effects by Individual Labor Market Status Transition Types

Panel A: Effects by Transition Type

	Full Sample		Job Stayers		Recalled Workers		Job Movers	
Time Horizon	1-Year	2-Year	1-Year	2-Year	1-Year	2-Year	1-Year	2-Year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Est. Wage Effect	-0.010	-0.002	-0.011	-0.015	-0.069	0.077	0.109	0.116
	(0.017)	(0.025)	(0.021)	(0.024)	(0.118)	(0.091)	(0.132)	(0.083)
Base-Year Transition Rate			0.826	0.703	0.040	0.057	0.086	0.134
Mincer + Ind.-Occ. FEs	X	X	X	X	X	X	X	X

Panel B: Employment-Unemployment-Employment Movers

	1-Year Earnings Effects				2-Year Earnings Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Est. Wage Effect	-0.256	-0.193	-0.286	-0.225	-0.067	-0.076	0.020	-0.013
	(0.139)	(0.146)	(0.159)	(0.234)	(0.146)	(0.153)	(0.177)	(0.189)
Base-Year Transition Rate	0.019	0.019	0.019	0.019	0.030	0.030	0.030	0.030
Mincer + Ind.-Occ. FEs	X	X	X	X	X	X	X	X
Firm-Year FEs			X	X			X	X
Transition-Specific Controls		X		X		X		X

Panel C: Direct Employment-to-Employment Movers

	1-Year Earnings Effects				2-Year Earnings Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Est. Wage Effect	0.190	0.113	0.173	-0.003	0.135	0.066	0.019	-0.072
	(0.120)	(0.097)	(0.092)	(0.104)	(0.100)	(0.109)	(0.099)	(0.124)
Base-Year Transition Rate	0.058	0.058	0.058	0.058	0.091	0.091	0.091	0.091
Mincer + Ind.-Occ. FEs	X	X	X	X	X	X	X	X
Firm-Year FEs			X	X			X	X
Transition-Specific Controls		X		X		X		X

Note: The results show σ_0 coefficients from estimating Equation (19) but interacting an indicator for each transition type with the σ_0 and σ_e coefficients. All estimates follow specification (4) in Table III and also include parametric earnings controls (log earnings and earnings percentiles) interacted with transition type. The specifications indicating *Transition-Specific Controls* interact all controls (including firm-year FEs where applicable) with the transition types. *Job Stayers* refers to incumbent workers who remain employed at the same firm the entire next year or for two years in the specifications with a two-year outcome. *Recalled Workers* refers to individuals who leave their current employer for another employer or nonemployment and then return to their original employer within the next year or two (depending on the specification horizon). *Job Movers* refers to individuals who move to another employer in the following year or two years, with or without an intermediate unemployment spell. *EUE Movers* refers to the subset of job movers who are unemployed (receive UI/UA) before moving to their next employer. *Direct Employment-to-Employment Movers* refers to the subset of job movers who have no intervening months of nonemployment before starting work at another employer. See Appendix Section E.6 for more information on the definition of the transition types.

Table V: Heterogeneity of Nonemployment Effects on Wages: One- and Two-Year Effects

Horizon	1-Year Effects				2-Year Effects			
Quintile	1^{st} (Lowest)		5^{th} (Highest)		1^{st} (Lowest)		5^{th} (Highest)	
Unemployment Risk								
Industry EU Transition Rate	-0.056	(0.040)	0.037	(0.030)	-0.090	(0.044)	0.002	(0.048)
Local Unemployment Rate	0.022	(0.031)	0.000	(0.030)	0.018	(0.035)	0.007	(0.039)
Months since UI Receipt	-0.027	(0.048)	-0.015	(0.038)	0.121	(0.054)	-0.064	(0.083)
Months since Non-Emp.	-0.094	(0.045)	-0.031	(0.045)	0.042	(0.059)	0.024	(0.057)
Firm Characteristics								
Industry Growth Rate	-0.015	(0.030)	-0.050	(0.027)	-0.015	(0.040)	-0.010	(0.039)
Wage Premium (AKM FE)	-0.006	(0.030)	-0.017	(0.034)	0.092	(0.043)	-0.011	(0.045)
SD of Earnings Growth	0.023	(0.023)	-0.017	(0.030)	0.072	(0.030)	-0.031	(0.041)
Wage Distance from CBA Floor (Proxy)	-0.061	(0.021)	0.003	(0.036)	-0.045	(0.036)	0.023	(0.049)
Share Non-Emp Last 2 Yrs	0.025	(0.027)	-0.020	(0.032)	0.048	(0.028)	-0.048	(0.043)
Worker Characteristics								
Tenure	-0.011	(0.038)	-0.038	(0.023)	0.068	(0.054)	0.039	(0.026)
Age	-0.028	(0.037)	-0.011	(0.026)	-0.048	(0.053)	-0.031	(0.029)
Gender: Male (left) / Female (right)	-0.027	(0.026)	0.012	(0.021)	0.050	(0.032)	-0.030	(0.040)
Occupation: Blue (left) / White Collar (right)	-0.012	(0.022)	-0.009	(0.025)	0.000	(0.026)	-0.007	(0.047)

Note: The table shows σ_0 coefficients from estimating Equation (19) but interacting an indicator for each different heterogeneity group category with the σ_0 and σ_e coefficients in Equation (19). Standard errors are reported in parentheses. We also vary the parametric earnings controls by heterogeneity type, allowing for differential earnings growth patterns by heterogeneity type. The estimates are from specification (4) in Table III that include Mincerian and industry/occupation controls but not the firm-by-year fixed effects. See Section 5 and Appendix E.7 for more details about the construction of each heterogeneity group. For the cuts by months since most recent UI receipt/nonemployment, to pick up workers recently hired, we relax the sample restriction requiring 12 months of employment in the base year. Lower quintiles correspond to smaller values.

Table VI: Wage Effects: Difference-in-Differences Regression with **Firm-Level Variation**

	1-Year Earning Effects				2-Year Earning Effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo: 3 Yr Lag	-0.026	-0.041	-0.010	0.003	-0.074	-0.090	-0.020	-0.001
	(0.030)	(0.029)	(0.030)	(0.030)	(0.035)	(0.034)	(0.037)	(0.037)
Placebo: 2 Yr Lag	-0.075	-0.079	-0.035	-0.034				
	(0.024)	(0.024)	(0.027)	(0.027)				
Treatment Year	**0.029**	**0.031**	**0.037**	**0.044**	**0.069**	**0.073**	**0.070**	**0.074**
	(0.031)	**(0.031)**	**(0.030)**	**(0.030)**	**(0.052)**	**(0.051)**	**(0.046)**	**(0.046)**
Pre-p F-test p-val	0.005	0.003	0.402	0.319	0.035	0.008	0.594	0.977
R^2	0.062	0.080	0.093	0.109	0.120	0.144	0.154	0.176
N (1000s)	7142	7142	7138	7138	5044	5044	5041	5041
Mincerian Ctrls		X		X		X		X
4-Digit Ind.-Occ. FEs			X	X			X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification (19) with the variation in benefits aggregated at the firm-level. See Section 5.2 for more details about the construction of the firm-level instrument. Standard errors are in parentheses and two-way clustered at the firm and individual level. The null hypothesis of the F-test is that the coefficients of interest are all jointly equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation. All specifications also include the baseline controls in Table III, reform-specific firm-treatment intensity percentile fixed effects and firm share treated percentile fixed effects.

Table VII: Wage Effects: Difference-in-Differences Regression with **Industry-Occupation Level Variation**

	Panel A: 1-Year Earning Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.158	0.119	0.501	0.014	0.029	-0.011
	(0.099)	(0.098)	(0.062)	(0.109)	(0.098)	(0.098)
Placebo: 2 Yr Lag	0.095	0.072	0.290	0.007	-0.009	-0.035
	(0.077)	(0.077)	(0.047)	(0.083)	(0.072)	(0.073)
Treatment Year	**0.321**	**0.338**	**0.198**	**0.295**	**-0.094**	**-0.071**
	(0.076)	**(0.078)**	**(0.083)**	**(0.088)**	**(0.089)**	**(0.087)**
Pre-p F-test p-val	0.281	0.482	0.000	0.991	0.898	0.873
R^2	0.070	0.087	0.082	0.072	0.082	0.098
N (1000s)	7142	7142	7142	7142	7142	7142
Mincerian Ctrls		X				X
3-Digit Ind. FEs			X		X	X
Occ. FEs				X	X	X

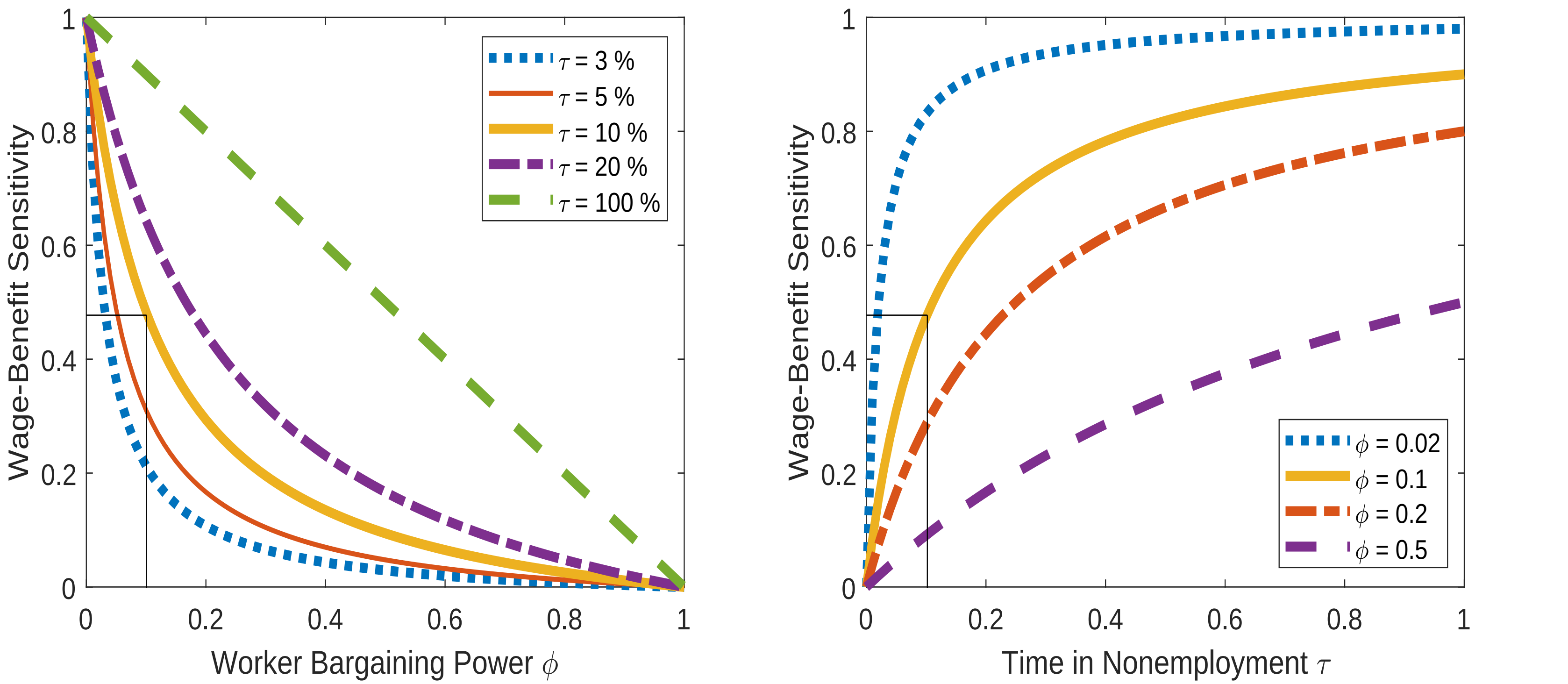
	Panel B: 2-Year Earning Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.049	0.002	0.441	-0.106	-0.019	-0.051
	(0.074)	(0.073)	(0.075)	(0.075)	(0.113)	(0.107)
Placebo: 2 Yr Lag						
Treatment Year	**0.138**	**0.197**	**-0.008**	**0.163**	**-0.157**	**-0.074**
	(0.102)	**(0.098)**	**(0.096)**	**(0.096)**	**(0.127)**	**(0.120)**
Pre-p F-test p-val	0.509	0.980	0.000	0.157	0.867	0.632
R^2	0.133	0.156	0.145	0.137	0.146	0.168
N (1000s)	5045	5045	5045	5045	5045	5045
Mincerian Ctrls		X				X
3-Digit Ind. FEs			X		X	X
Occ. FEs				X	X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification (19) with the variation in benefits aggregated at the industry-occupation level. See Section 5.2 for more details about the construction of the industry-occupation-level instrument. Standard errors are in parentheses and two-way clustered at the industry-occupation and individual level. The null hypothesis of the F-test is that the coefficients of interest are all jointly equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry controls are time-varying fixed effects for each three-digit industry, and the occupation controls are time-varying indicators for a blue vs. white-collar occupation. All specifications also include the baseline controls in Table III, reform-specific industry-treatment intensity ventile fixed effects and industry share treated ventile fixed effects.

Figures

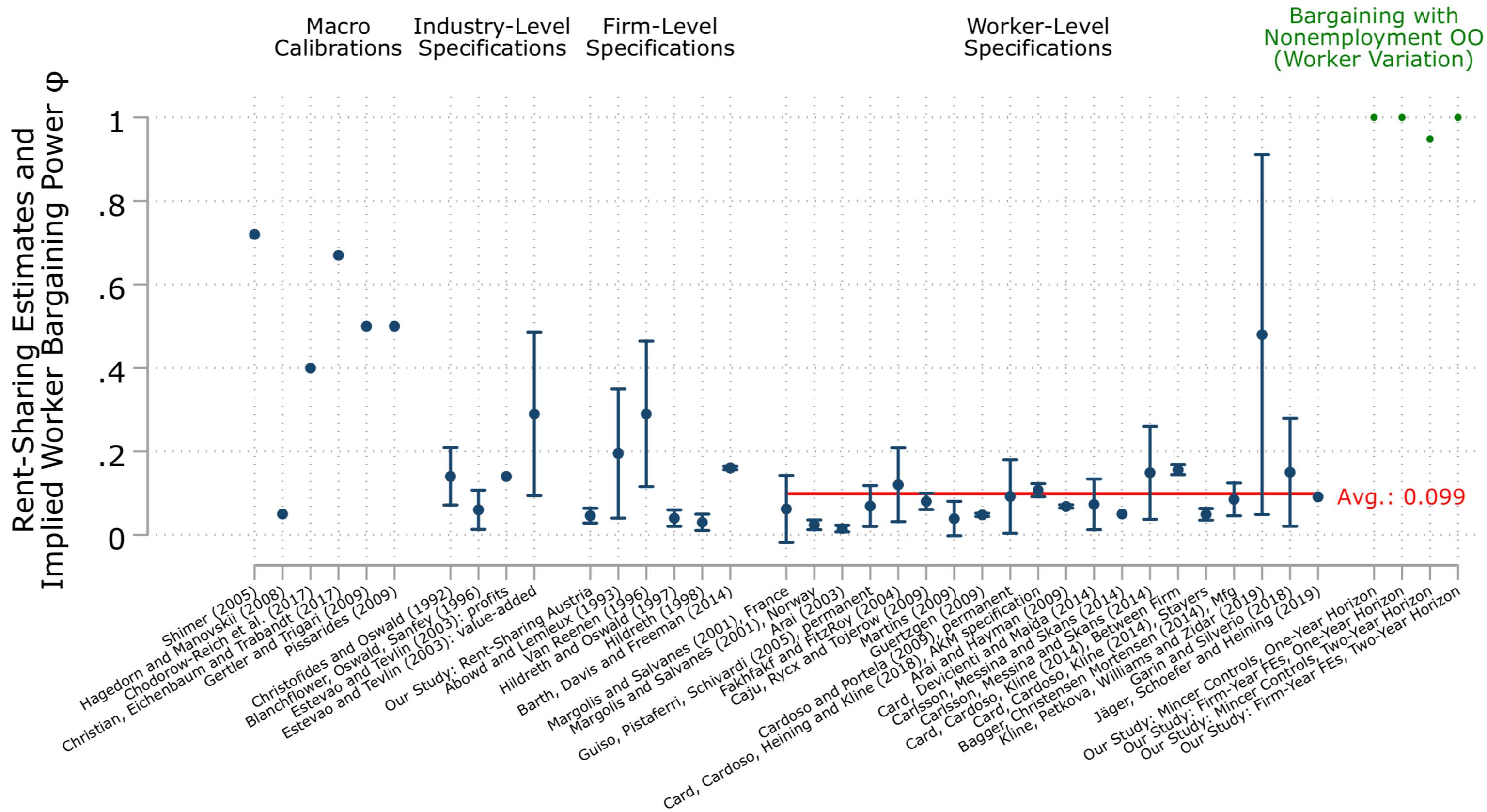
Figure I: Nash Bargaining: Relationship between Wage-Benefit Sensitivity $\frac{dw}{db}$ and Bargaining Power ϕ and Time in Nonemployment τ

(a) $\frac{dw}{db}$ and ϕ, by τ (b) $\frac{dw}{db}$ and τ, by ϕ



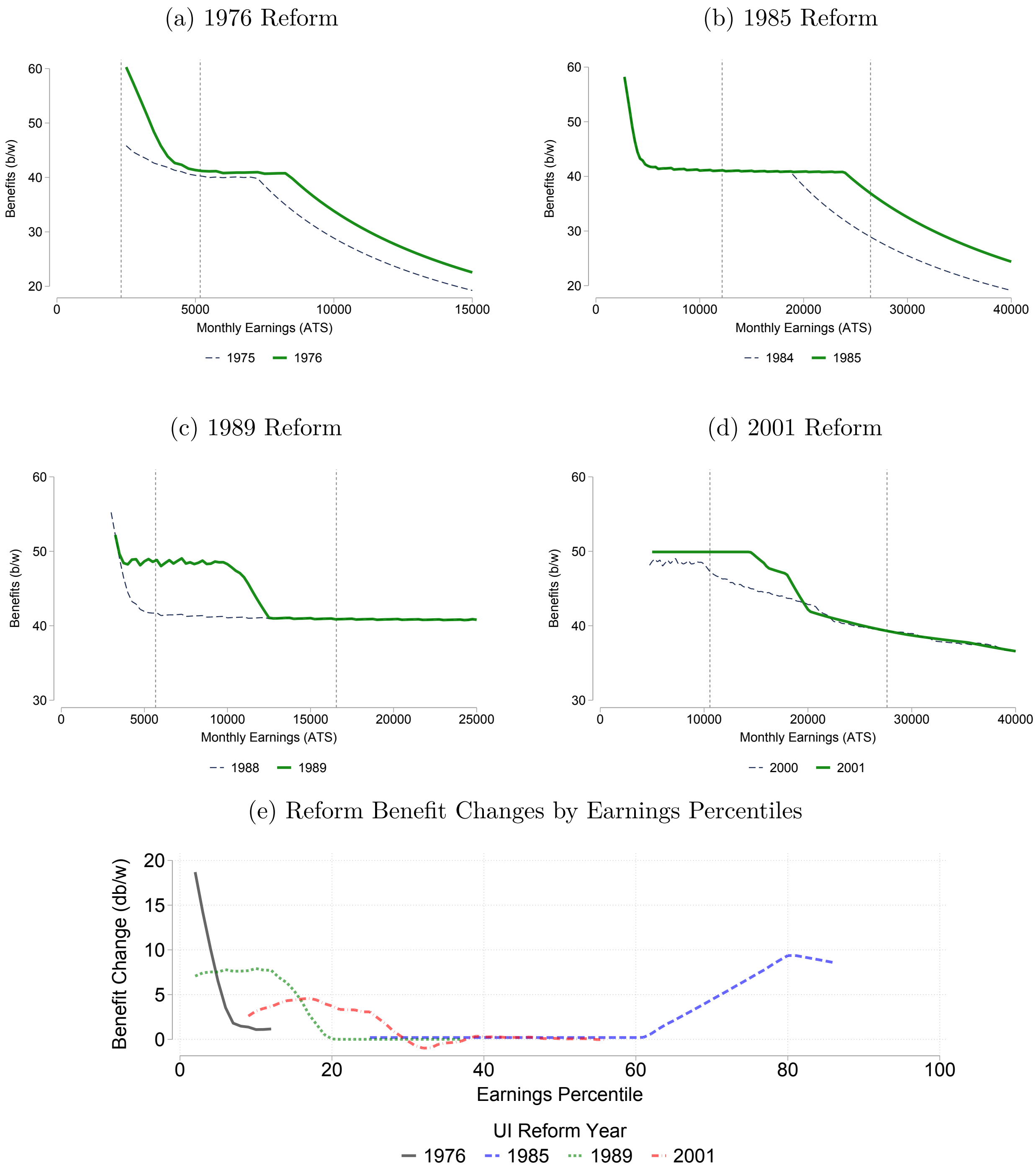
Note: The figure plots the relationship between wage-benefit sensitivity $\frac{dw}{db}$, and worker bargaining power ϕ, and time in nonemployment τ, as predicted by Equation (6). We vary τ, the post-separation time spent in nonemployment, ($\tau \in \{3\%, 5\%, 10\%, 20\%, 100\%\}$), and worker bargaining power ϕ ($\phi \in \{0.02, 0.1, 0.2, 0.5\}$). Our calibration ($\tau = 0.1$ and $\phi = 0.1$) predicts a sensitivity of 0.48, depicted in the thin line departing from $\phi = 0.1$, crossing the solid line ($\tau = 10\%$) and ending at the 0.48 sensitivity (left panel), and depicted in the thin line departing from $\tau = 0.1$, crossing the solid line ($\phi = 0.1$) in the right panel.

Figure II: Overview of Estimates and Calibrations of Worker Bargaining Power



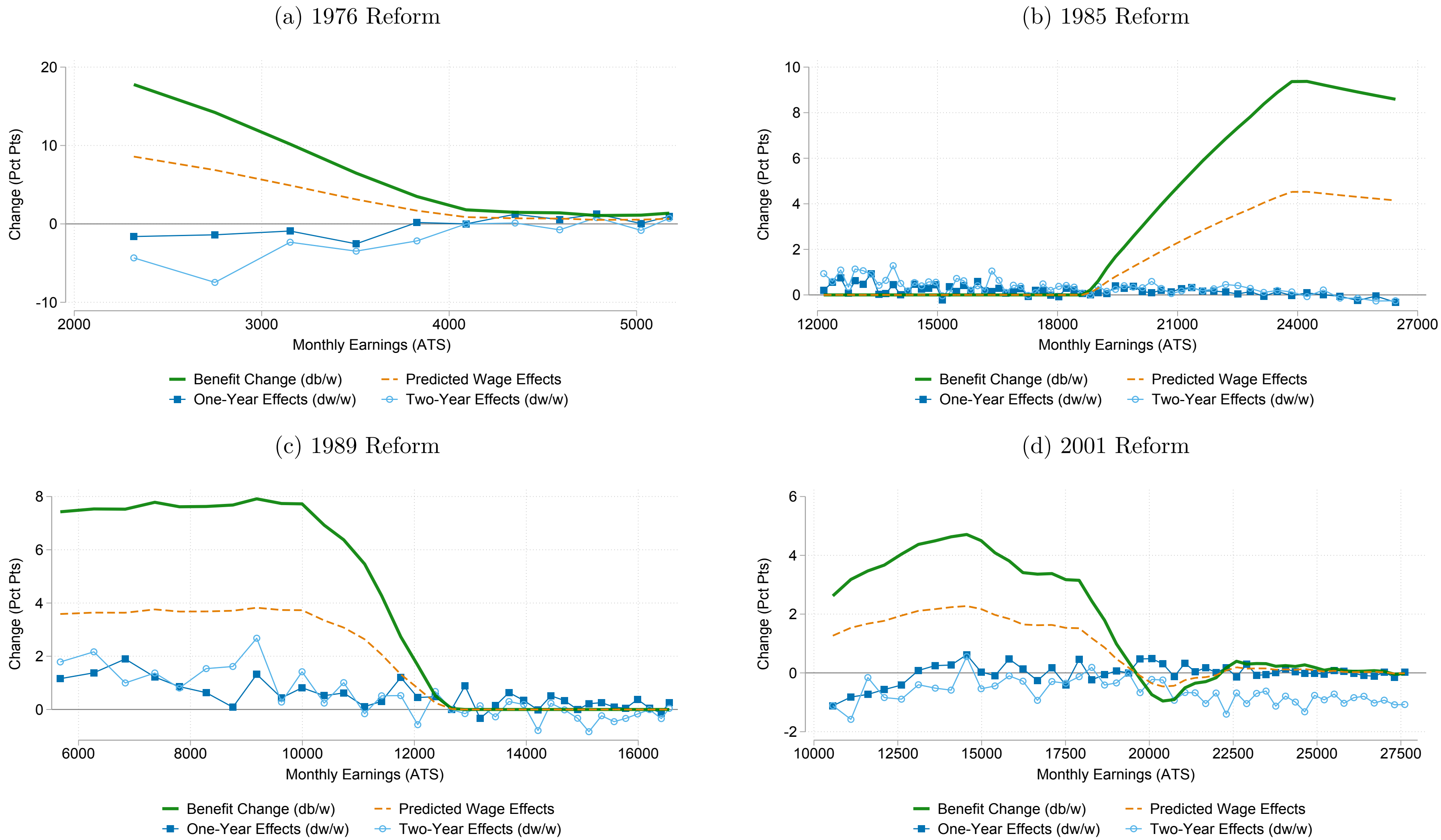
Note: The figure shows an overview of calibrations as well as implied estimates of worker bargaining power. For the calibrations, we plot the values used in the respective papers. For the estimates, we build on the meta-study in Card et al. (2018) and use level-on-level specifications from the papers included in the overview if those are reported. In addition, we report recent estimates from Kline et al. (2019) (Table 8, Panel A, column 12, avg. non-inventor stayer earnings), Garin and Silvério (2018), estimates from Card et al. (2018) and Jäger, Schoefer, and Heining (2019) relating value-added and AKM firm effects, and our own estimate for Austria. For the study of rent-sharing in Austria, we use firm panel data from Bureau van Dijk from 2004 to 2016 and regress wage costs per employee on value-added per employee, controlling for firm and industry-by-year effects in a level-on-level specification. Some of the estimates surveyed in Card et al. (2018) are cast as elasticities and are thus upper bounds for the implied worker bargaining power when rent-sharing elasticities are calculated (see Appendix Section I.1). Among the worker-level specifications, we calculate an inverse variance weighted mean of the estimates among those studies that either report level-on-level specifications or rent-sharing elasticities (we omit studies with profit-sharing elasticities since these do not provide bounds for bargaining power). For our study, we plot the implied worker bargaining power under the assumption that nonemployment is the outside option based on the results in Table III. Specifically, we plot the implied ϕ based on the estimates in columns (2) and (6) of both panels in Table III and report $\phi = 1$ if the point estimate would imply even higher values. The references for the studies included in the Figure are reported in Appendix Section I.2.

Figure III: Unemployment Benefit Schedules and Reforms



Note: Panels (a)-(d) plot the unemployment benefit schedule before and after each of the four reforms we analyze. The x-axis shows the income relevant for calculating benefits while the y-axis plots the benefits, calculated as the unemployment benefits divided by income. The vertical gray dashed lines delineate the earnings ranges included in the analysis samples, the selection of which we describe in Appendix Section E.1. Panel (e) plots the reform-induced benefit change for each reform in earnings percentile space.

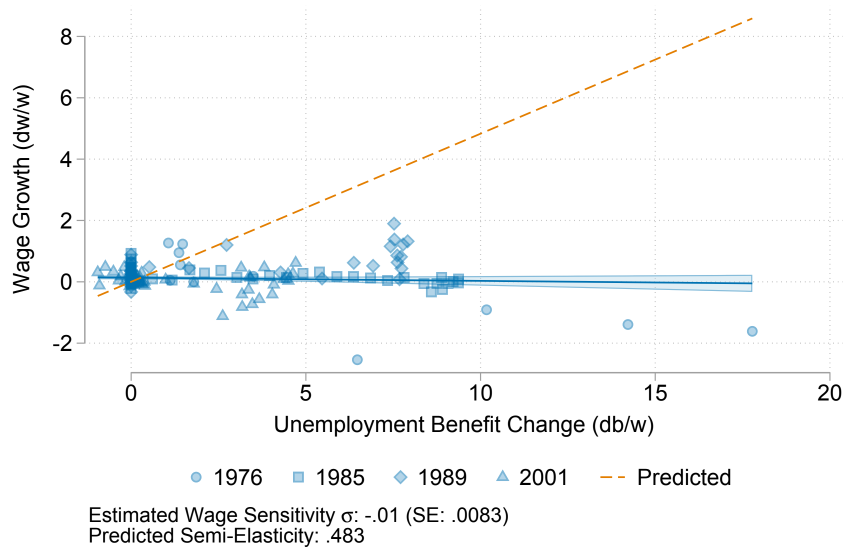
Figure IV: Nonparametric Benefit Changes and Wage Effects



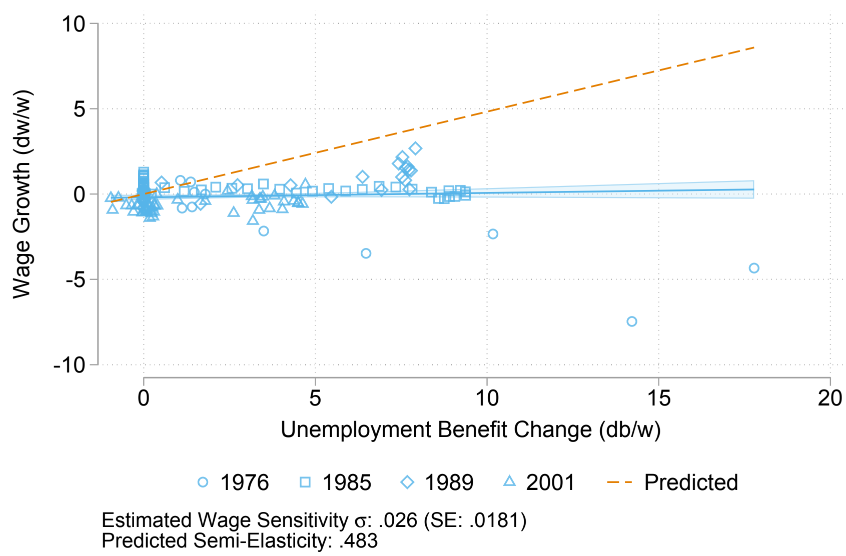
Note: The figure plots reform-induced replacement rate changes and wage effects for all four reform. Observations are binned by their base year (year before the reform was enacted) earnings percentile on the x-axis. The dashed yellow line indicates the wage growth that the reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.48. The blue squares and circles indicate the wage effects that the reform induced at the one- and two-year horizon, respectively. Section 4.2 provides more information.

Figure V: Scatter Plots of Wage Growth and Unemployment Benefit Changes

(a) One-Year Horizon



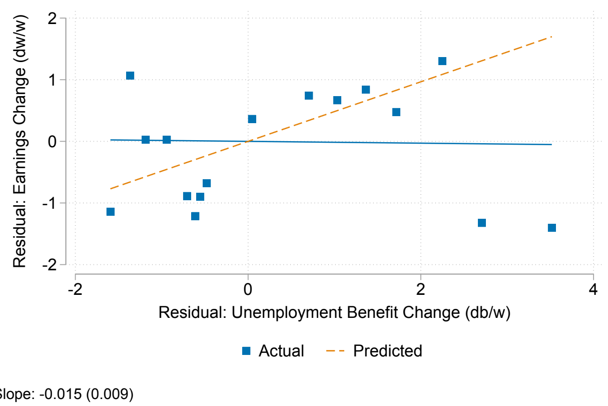
(b) Two-Year Horizon



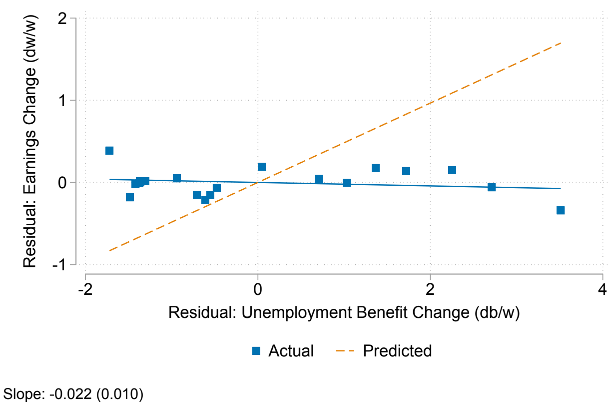
Note: The figures show scatter plots of wage growth (y-axis) and reform-induced replacement rate changes (x-axis), db/w, pooling the four reforms outlined in Figures IV(a) through IV(d). Each dot corresponds to a percentile observation from Figures IV(a) through IV(d). The upper panel shows wage effects after one year and the lower panel effects after two years. The yellow dashed line indicate the predicted wage growth that the reforms would have induced in the calibrated bargaining model with a wage-benefit sensitivity of 0.48. The remaining symbols indicate actual data points for wage growth and benefit changes. The estimated wage sensitivities $\hat{\sigma}$ are calculated as the slope of wage growth with respect to changes in the benefit level.

Figure VI: Pooled Earnings and Benefit Change Bin Scatter Plots - Treatment Year

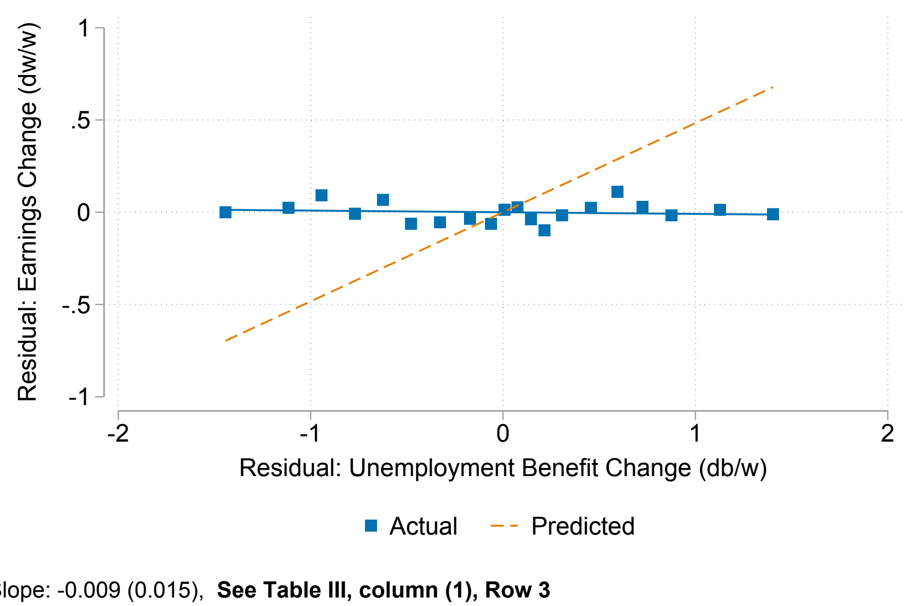
(a) Year Fixed Effects



(b) Adding Earnings Percentile Fixed Effects

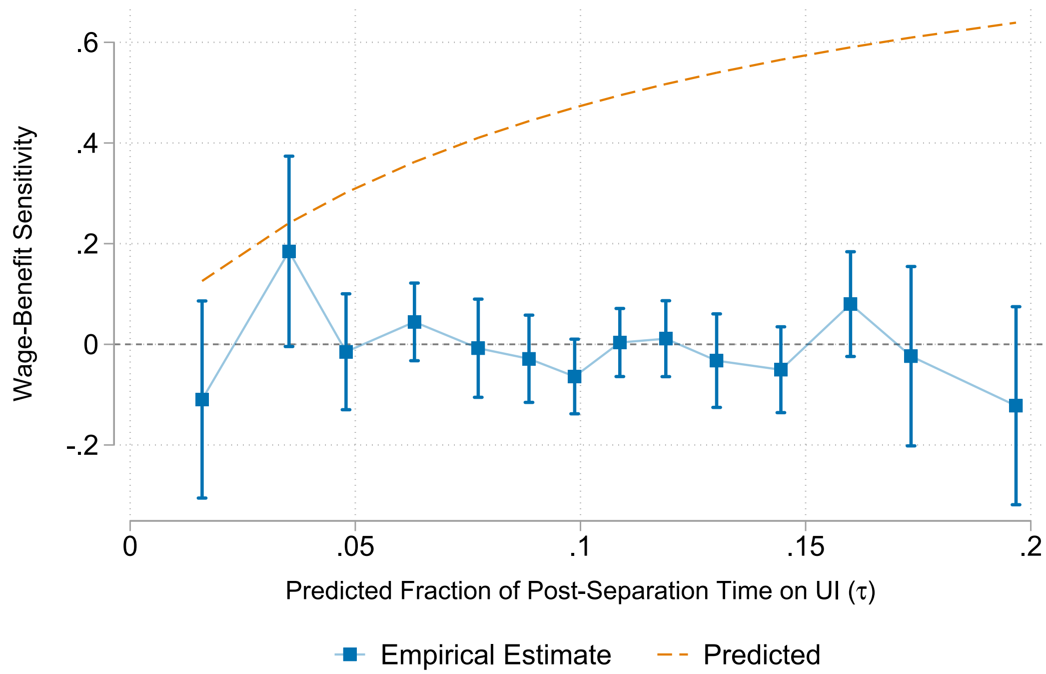


(c) Adding Year-Specific Log Earnings Controls



Note: The three panels show the best-fit lines and binned scatter plots from estimating 1-year effects in Equation 19 for the pooled sample of all four reforms. The best-fit line slope and standard errors are the coefficient and standard error on σ_0 in Equation 19. Shown in blue, the binned scatter plot is estimated on earnings changes and reform induced benefit changes both residualized by the other included controls. Panel (a) only includes year fixed effects as controls. Panel (b) adds earnings percentile fixed effects. Panel (c) adds year-specific log earnings controls. The yellow dashed line plot the predicted earnings change for each benefit change based on the calibrated Nash bargaining model.

Figure VII: Heterogeneity of Wage-Benefit Sensitivity by Predicted Post-Separation Time on UI τ: Model Prediction vs. Empirical Estimates



Note: The graph presents wage-benefit sensitivities for workers sorted by their predicted fraction of time on UI conditional on a separation, i.e. the $\hat{\tau}_i$ statistic described in Section 2.1.2, further detailed Appendix B, and summarized in Table I. Specifically, the analysis sorts the regression sample (of each reform year) into 14 quantiles: the sorting starts with deciles, and then for additional dispersion, further splits up the top and bottom decline into two equally sized groups (ventiles), and then further splits up the resulting very top/bottom ventiles into two. The x-axis denotes the quantile-specific mean $\hat{\tau}_i$ values. The graph then reports two wage-benefit sensitivities. First, the dashed yellow line plots the series of model-predicted wage-benefit sensitivity following Equation (6) and based on a Nash bargaining model with worker bargaining power $\phi = 0.1$, inputting each group's mean $\hat{\tau}_i$. The blue line (squares) presents the group-specific empirical heterogeneous treatment effects, estimated in a version of our main regression model (19) but interacting the treatment (reform-induced benefit changes) with a series of indicators for a worker's quantile membership regarding her τ value.

Online Appendix of:
Wages and the Value of Nonemployment

Simon Jäger, Benjamin Schoefer, Samuel Young and Josef Zweimüller

Contents

A Additional Empirical Results

A.1 Take-Up of UI Benefits and Knowledge of Benefit Levels

Table A.1: Take-Up of Unemployment Insurance among Nonemployment Spells

	Prop. of NE Spells	No. Spells
All Spells	0.523	3,082,638
2 Years or Shorter	0.541	2,722,998
2 Days or Longer	0.547	2,945,611
14 Days or Longer	0.652	2,391,842
28 Days or Longer	0.682	2,147,350
Between 28 Days and 2 Years	0.743	1,787,710
Men	0.523	1,571,671
Women	0.523	1,510,967
Blue Collar	0.552	1,597,019
White Collar	0.584	1,046,374
Excluding Ages 50-54	0.519	2,916,063
Employed At Least 2 Years	0.542	2,092,416
Spells between 28 Days and 2 Years		
Male	0.775	884,891
Male Under 50	0.775	830,488
Female	0.711	902,819
Female Under 50	0.712	851,411
Blue Collar	0.809	897,913
White Collar	0.779	657,425
Excluding Ages 50-54	0.743	1,681,899
Employed At Least 2 Years	0.754	1,247,794

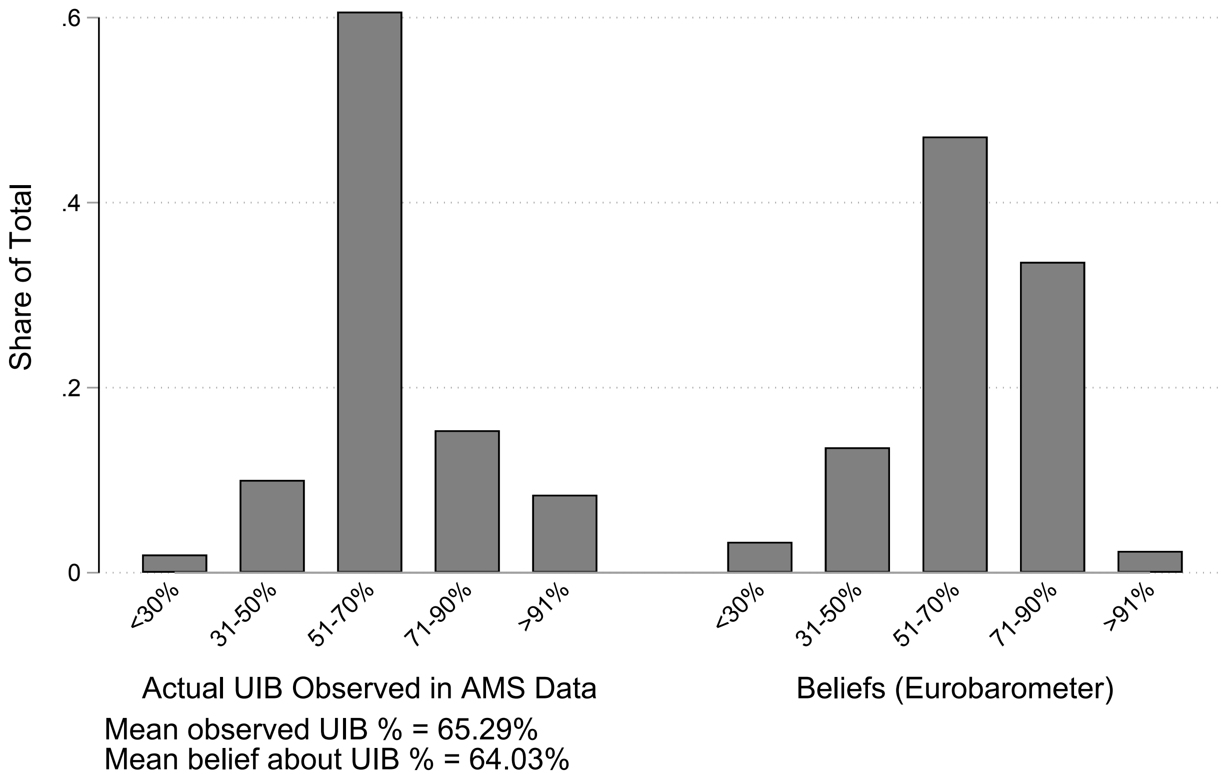
Note: This table plots the share of workers who take up unemployment insurance after the end of an employment spell. The sample is restricted to prime-age workers (25-54) whose employment spell prior to nonemployment lasted at least one year and who were not recalled by their previous employer. We also drop workers who immediately transition from employment into other types of spells, e.g., maternity leave or disability. The sample period ranges from 1972 to 2000. To illustrate, the table indicates that 65.2% of nonemployment spells of 14 days or longer led to take-up of unemployment insurance.

Table A.2: 1(Mth UI > 0| Mth NE > 0) ×100

	Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: UI Takeup, 1 Year Ahead (100 * Indicator)					
Placebo: 3 Yr Lag	-0.090	-0.055	-0.122	-0.116	0.012	0.022
	(0.110)	(0.114)	(0.110)	(0.111)	(0.129)	(0.132)
Placebo: 2 Yr Lag	-0.200	-0.106	-0.139	-0.100	-0.120	-0.077
	(0.113)	(0.111)	(0.101)	(0.097)	(0.114)	(0.114)
Treatment Year	**0.028**	**0.110**	**0.084**	**0.114**	**0.199**	**0.231**
	(0.113)	**(0.108)**	**(0.109)**	**(0.106)**	**(0.123)**	**(0.124)**
Base-Year Average	48.196	48.196	48.196	48.196	48.196	48.196
Pre-p F-test p-val	0.200	0.626	0.372	0.524	0.528	0.734
R^2	0.031	0.071	0.124	0.148	0.418	0.437
N (1000s)	910	910	908	908	609	608
	Panel B: UI Takeup, 2 Year Ahead (100 * Indicator)					
Placebo: 3 Yr Lag	-0.036	-0.033	-0.064	-0.069	0.053	0.032
	(0.078)	(0.083)	(0.081)	(0.085)	(0.098)	(0.096)
Placebo: 2 Yr Lag						
Treatment Year	**0.128**	**0.106**	**0.150**	**0.122**	**0.206**	**0.181**
	(0.082)	**(0.083)**	**(0.081)**	**(0.082)**	**(0.086)**	**(0.087)**
Base-Year Average	52.213	52.213	52.213	52.213	52.213	52.213
Pre-p F-test p-val	0.645	0.691	0.434	0.415	0.587	0.741
R^2	0.032	0.074	0.124	0.147	0.386	0.404
N (1000s)	1127	1127	1126	1126	820	819
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note. We evaluate the effect of four reforms to the Austrian unemployment insurance benefit schedule on the take-up of UI. In particular, we begin with the months on UI in year $t+1$ or $t+2$ (as a share of total months), which appears in Appendix Figure A.12, and indicate if there are nonzero months on UI and nonzero months nonemployed. This indicator is missing if the individual has no months nonemployed in year $t+1$ or $t+2$, in order to isolate take up. The outcome complements the analysis fo the effect on months on UI observed in Appendix Figure A.12 (separation effects). This indicator is multiplied by 100 for legibility. Errors are two-way clustered at the individual- and percentile-level.

Figure A.1: Beliefs About UI Benefit Levels Among Employed Workers



Note: The figure shows worker beliefs about unemployment benefits based on representative Eurobarometer 2006 data for Austria and compares it to data on actually paid out benefits among unemployed workers in 2006 based on AMS data. The Eurobarometer 2006 wave asked 568 employed respondents the following question: "Suppose you are laid off, what is your belief about the percentage of your current income that would be replaced through unemployment insurance and the Austrian social security system in the first six months?" The answer categories are 91 to 100%, 71 to 90%, 51 to 70%, 31 to 50%, less than 30%, and a category for those who do not know. 90.1% of respondents provide a quantitative answer. The figure presents the distribution of actual benefits as a percent of net earnings and individuals' beliefs about their benefits. We bin the actual benefit ratios into the same interval bins that were presented in the Eurobarometer survey. To extract the mean response, we use an interval regression and find a mean of 64.03% (SE 0.72). We also report the actual replacement rate of unemployed workers in 2006 based on AMS data and find a mean of 65.29%.

A.2 Additional Specification: IV Interpretation

Table A.3: Validation Exercise: Difference-in-Differences Regression Design

	1-Year Realized RR Effects			2-Year Realized RR Effects		
	(1)	(2)	(3)	(1)	(2)	(3)
Placebo: 3 Yr Lag	0.148	0.144	0.150	-0.006	-0.010	-0.022
	(.028)	(.027)	(.028)	(.024)	(.023)	(.025)
Placebo: 2 Yr Lag	0.095	0.093	0.103			
	(.01)	(.01)	(.009)			
Treatment Year	**0.813**	**0.805**	**0.811**	**0.511**	**0.501**	**0.513**
	(.014)	**(.014)**	**(.013)**	**(.024)**	**(.023)**	**(.021)**
Base-Year Average	2.354	2.354	2.354	3.164	3.164	3.164
Pre-p F-test p-val	0.000	0.000	0.000	0.796	0.673	0.385
R^2	0.798	0.807	0.853	0.628	0.653	0.772
N (1000s)	7202	7198	6354	5153	5150	4539
Mincerian Ctrls		X	X		X	X
4-Digit Ind.-Occ. FEs		X	X		X	X
Firm-Year FEs			X			X

Note: To assess the extent to which reform-induced benefit changes, assigned based on lagged earnings, shift benefits implied by realized earnings, we estimate a variant of (19) with benefit changes implied by actual earnings realizations as the dependent variable:

$$\frac{b_t(w_{i,t}) - b_{t-1}(w_{i,t})}{w_{i,r,t-1}} = \sum_{e=-L}^{0} \delta_e^V (\mathbb{1}_{(t-r=e)} \times \frac{db_{i,r,t}(\tilde{w}_{i,r,t})}{w_{i,r,t-1}}) + \tau^V_{r,P_{t-1}} + \theta^V_{r,t-1} + \gamma^V_{r,t-1} \ln w_{i,r,t-1} + X'_{i,r,t-1}\phi^V_{r,t-1} + \epsilon^V_{i,r,t}. \tag{A1}$$

The dependent variable is the normalized change in benefits calculated based on realized earnings while the regressors are the predicted shifts in benefits based on lagged earnings. For the 2001 reform, the relevant realized earnings concept in fact corresponds to lagged earnings. We normalize σ^V_{-1} (and σ^V_{-2}) to zero in the specification with the one-year (two-year) implied benefit change as outcome variable. Standard errors based on two-way clustering at the individual and earnings percentile level are in parentheses. The null hypothesis of the F-test is that the coefficients of interest are all equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.

Table A.4: Instrumental Variable Analysis

	Panel A: 1-Year Horizon					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	**-0.011**	**-0.0047**	**-0.020**	**-0.013**	**-0.0089**	**-0.0096**
	(0.018)	**(0.019)**	**(0.020)**	**(0.021)**	**(0.014)**	**(0.014)**
F statistic	3350.7	3258.7	3291.4	3222.2	3767.2	3805.8
N (1000s)	7141	7141	7138	7138	6301	6297

	Panel B: 2-Year Horizon					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect	**0.014**	**0.042**	**-0.015**	**-0.0039**	**-0.018**	**-0.024**
	(0.045)	**(0.045)**	**(0.048)**	**(0.046)**	**(0.042)**	**(0.042)**
F statistic	416.3	406.8	432.3	420.8	478.9	475.3
N (1000s)	5039	5039	5037	5037	4434	4432
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: We implement an instrumental variable strategy akin to the simulated instruments literature (see, e.g., Gruber and Saez, 2002; Kleven and Schultz, 2014). In the instrumental variables interpretation, specification (A1) serves as first stage and (19) is the reduced form relationship, $\frac{b_t(w_{i,t})-b_{t-1}(w_{i,t})}{w_{i,r,t-1}}$ the endogenous variable, and $\frac{db_{i,r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}$ the excluded instrument. We estimate the model with 2SLS and use two-way clustering by individual and by earnings percentile. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation. All specifications also include reform-specific earnings percentile fixed effects, year fixed effects, and year-specific log earnings controls.

A.3 Additional Graphical Evidence on Wage Effects

Description of Appendix Figures A.2-A.5 Appendix Figures A.2-A.5 present additional non-parametric results for the 1976, 1985, 1989, and 2001 replacement rate reforms. The left column in each set of figures contains results for one-year earnings changes and the right column contains results for two-year earnings changes.

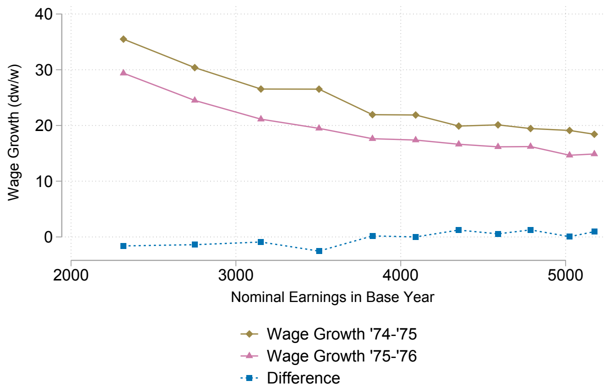
Panels (a) and (b) plot the average wage growth for the treatment year (pink triangles) and the pre-period year (olive diamonds) over the earnings distribution. Their difference (blue squares) is the same earnings growth difference that is plotted in Figures IV(a)- IV(d). The pink and olive scatter points allow us to better assess the (lack of) pre-trends in earnings growth by comparing the earnings growth gradient in the treatment and control time periods. The difference (blue scatter points) between average wage growth in the treatment and the pre-period year is normalized to be zero at the dashed vertical line.

Panels (c) and (d) plot the average of our predicted replacement rate change (the solid green line) and the average of the actual replacement rate change (the dashed blue line) over the earnings distribution. The predicted replacement rate change is calculated using the *predicted* earnings in the replacement rate reform year. See Section 4.1 for more details about this prediction process. The actual replacement rate change is the average of the replacement rate changes each individual actually experiences. In 1989, the two-year change (1988 to 1990) also captures a follow-up reform in 1990. Our interpretation of two-year wage effects in 1989 therefore largely captures delayed responses to the 1989 reform. Our two-year results are robust to excluding 1989. For 2001, since UI benefits are determined by lagged earnings, the predicted and actual replacement rate changes are identical for one-year outcomes.

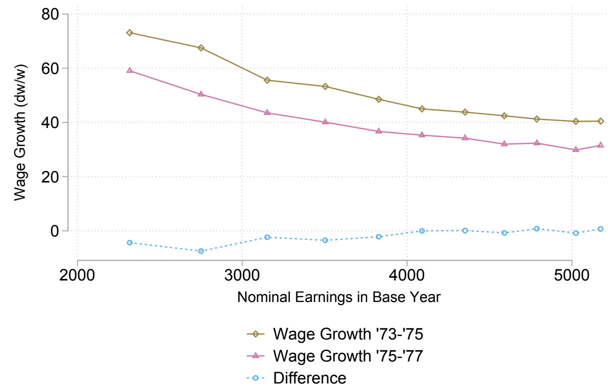
Panels (e) and (f) further assess the parallel trends assumption underlying our identification strategy. Here, we estimate the effects of placebo reforms at the same earnings percentile ranges, but we lag both the reform period and the pre-period by by two years. This placebo exercise thus assesses whether the earnings percentiles affected by the reform experienced higher or lower wage growth compared to other earnings percentiles in periods before the reform was enacted. The results presented in these panels are the same as in Panels (a) and (b) except all years are lagged by one or two to estimate the effect the placebo effects. For 1976, we cannot run the two-year placebo check because it would require calculating earnings growth from 1971-'73 and our data start in 1972. In the main regression analysis, we still report two-year earnings effect estimates including the 1976 reform because this only requires data starting in 1972.

Figure A.2: Additional Results: 1976 Reform

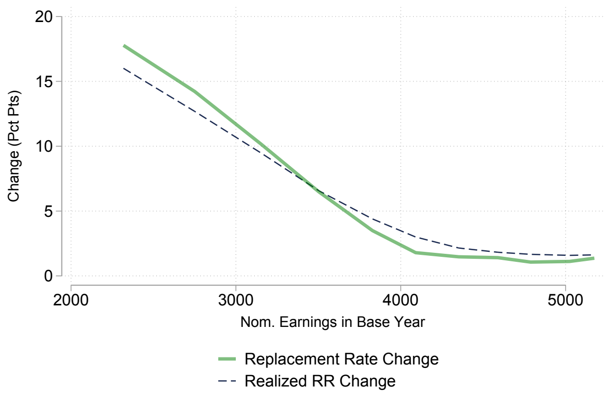
(a) Wage Growth: 1974-5 vs. 1975-6; 1 Yr



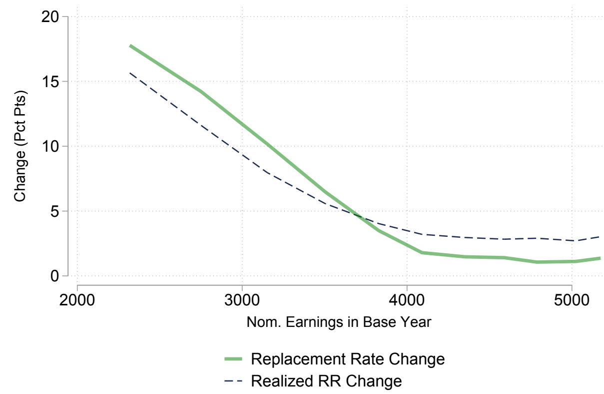
(b) Wage Growth: 1973-5 vs. 1975-7; 2 Yr



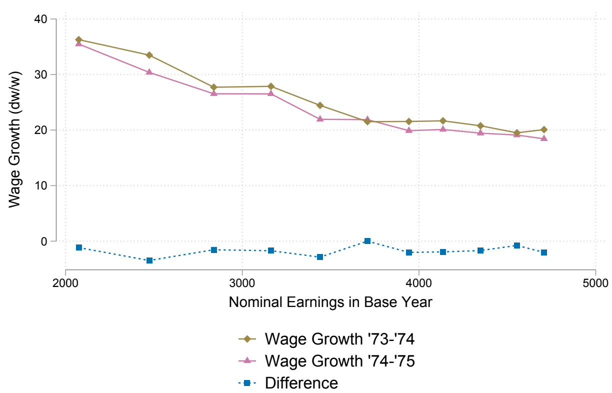
(c) Realized vs. Predicted Benefit Change; 1 Yr



(d) Realized vs. Predicted Benefit Change; 2 Yr



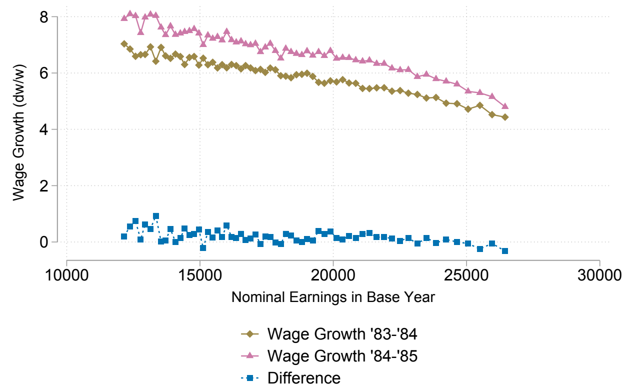
(e) Placebo 1975: 1973-4 vs. 1974-5; 1Yr



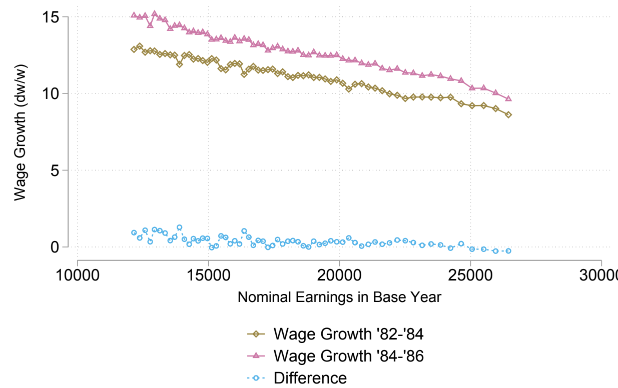
Note: The figure plots additional results related to the analysis in Figure IV(a). We provide a description at the beginning of this Appendix Section (A.3).

Figure A.3: Additional Results: 1985 Reform

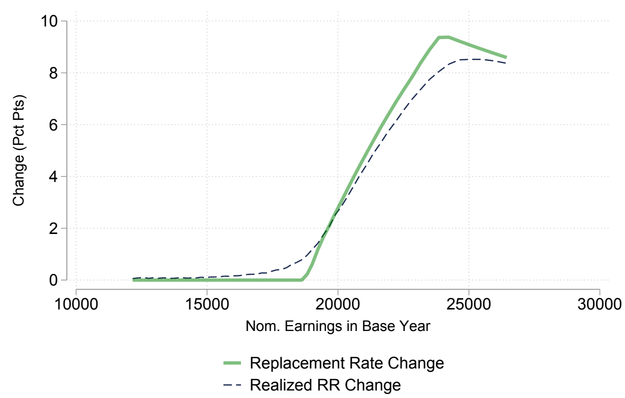
(a) Wage Growth: 1983-4 vs. 1984-5; 1 Yr



(b) Wage Growth: 1982-4 vs. 1984-6; 2 Yr

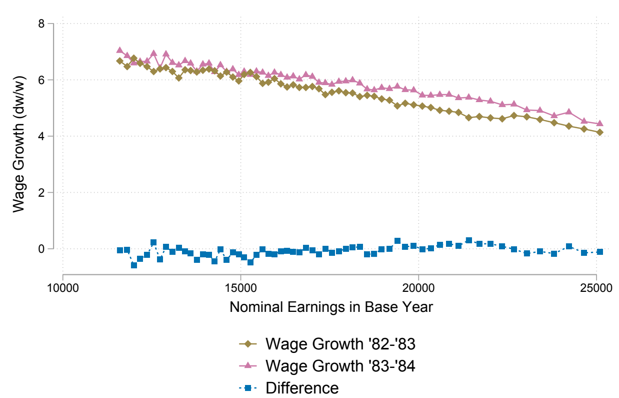


(c) Realized vs. Predicted Benefit Change; 1 Yr

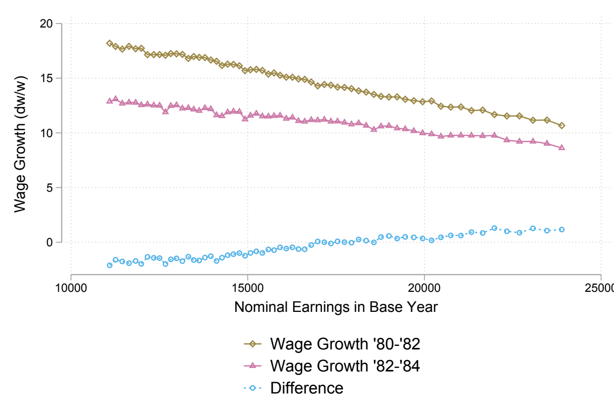


(d) Realized vs. Predicted Benefit Change; 2 Yr

(e) Placebo 1984: 1982-3 vs. 1983-4; 1Yr



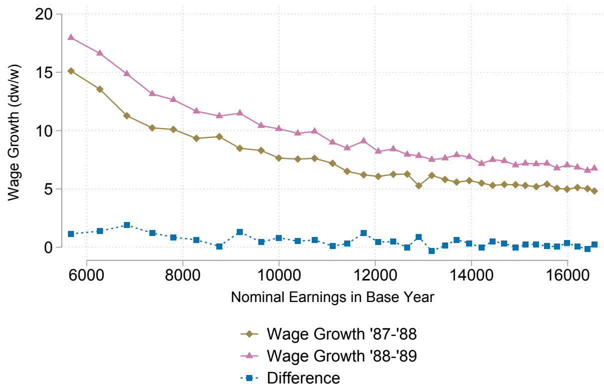
(f) Placebo 1983: 1980-2 vs. 1982-4; 2Yr



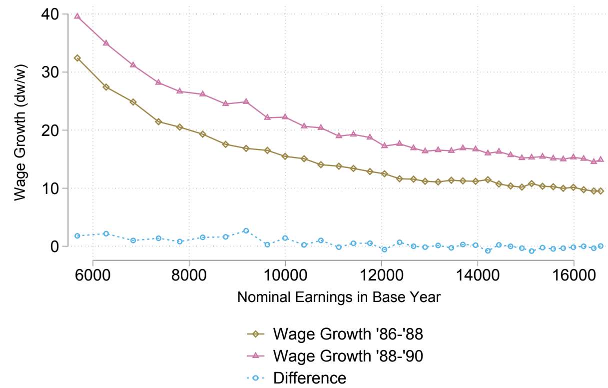
Note: The figure plots additional results related to the analysis in Figure IV(b). We provide a description at the beginning of this Appendix Section (A.3).

Figure A.4: Additional Results: 1989 Reform

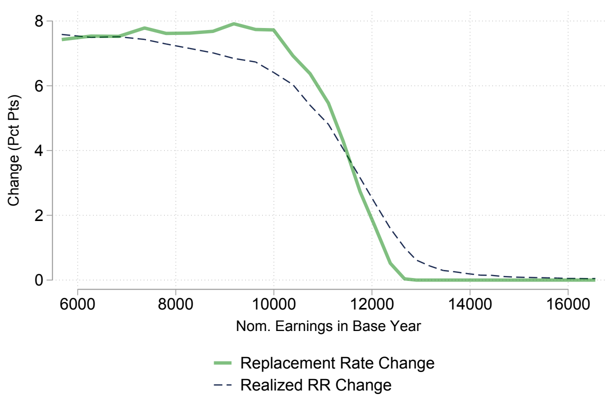
(a) Wage Growth: 1987-8 vs. 1988-9; 1 Yr



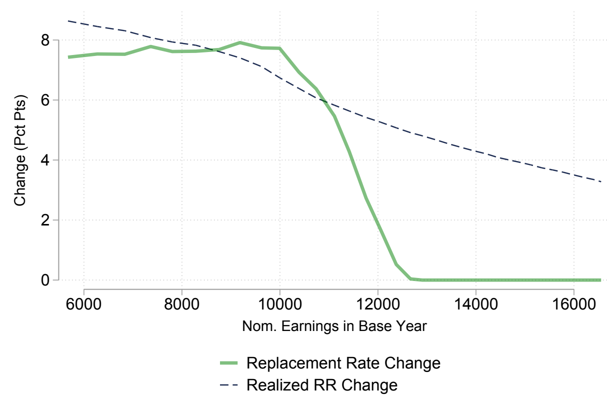
(b) Wage Growth: 1986-8 vs. 1988-90; 2 Yr



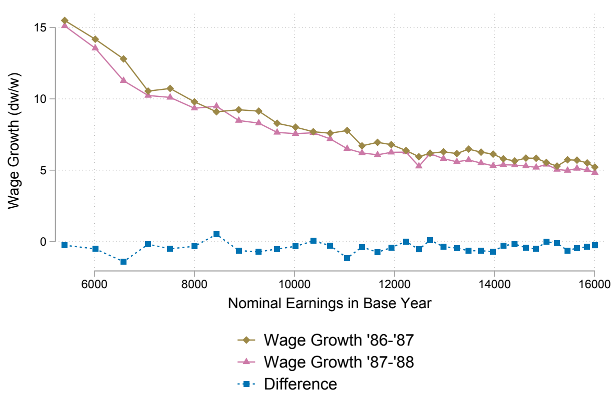
(c) Realized vs. Predicted Benefit Change; 1 Yr



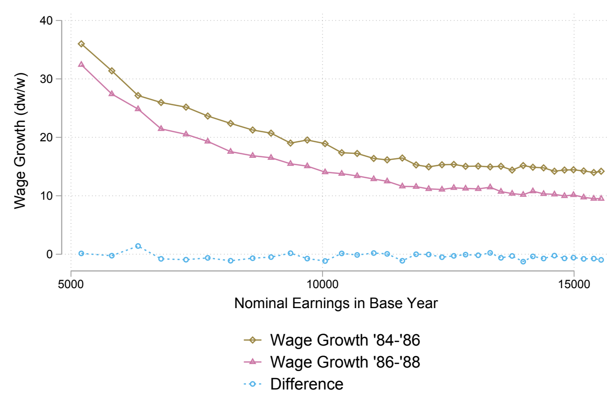
(d) Realized vs. Predicted Benefit Change; 2 Yr



(e) Placebo 1988: 1986-7 vs. 1987-8; 1Yr



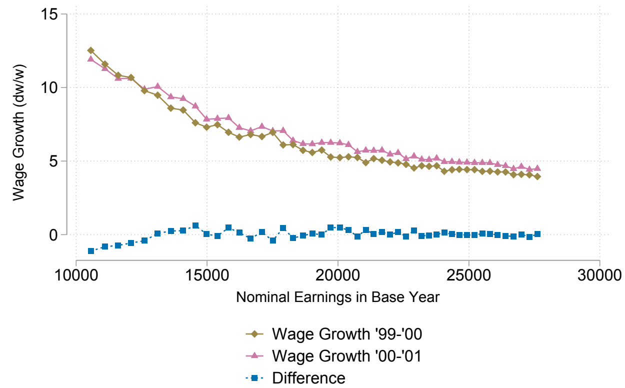
(f) Placebo 1987: 1984-6 vs. 1986-8; 2Yr



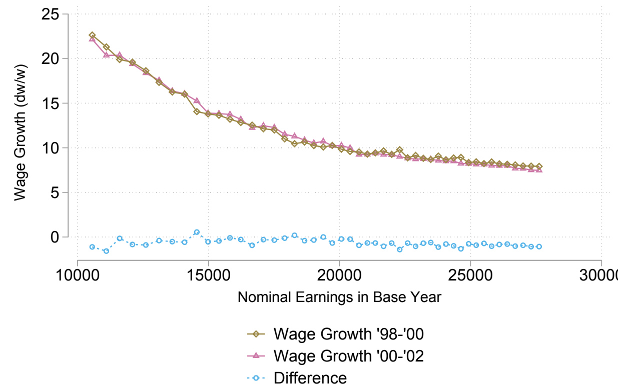
Note: The figure plots additional results related to the analysis in Figure IV(c). We provide a description at the beginning of this Appendix Section (A.3). Note that another reform shifted the schedule in 1990, broadly for the control and treatment groups, explaining the shifted line for that year. Our two-year results are robust to excluding 1989. Moreover, in our regression specifications, we will only build on one-year benefit variation as a treatment variable even when we measure longer-term wage outcomes.

Figure A.5: Additional Results: 2001 Reform

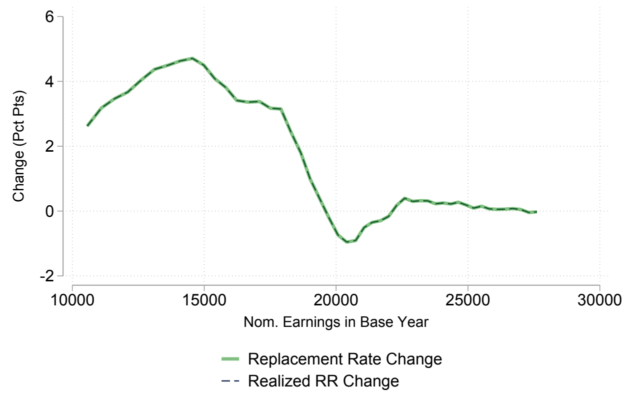
(a) Wage Growth: 2000-2001 vs. 1999-2000; 1 Yr



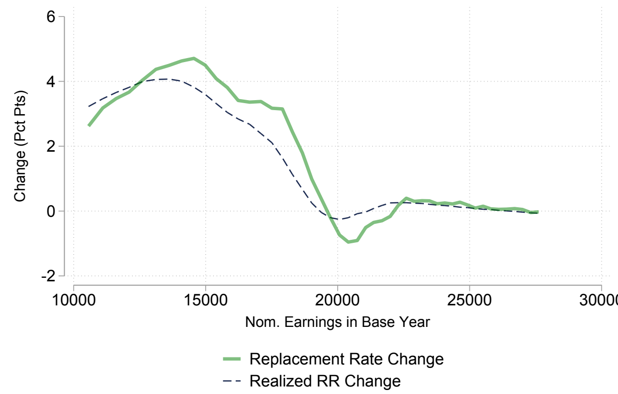
(b) Wage Growth: 2000-2 vs. 1998-2000; 2 Yr



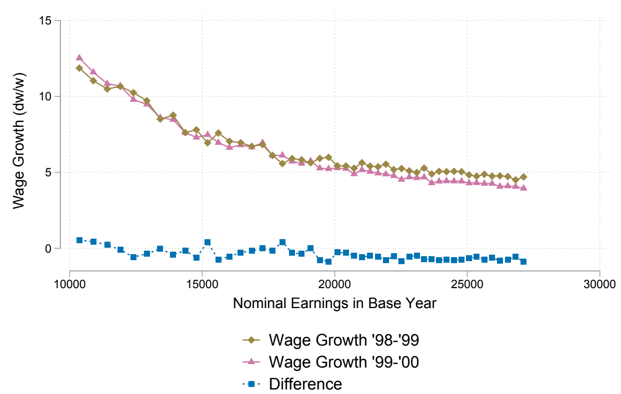
(c) Realized vs. Predicted Benefit Change; 1 Yr



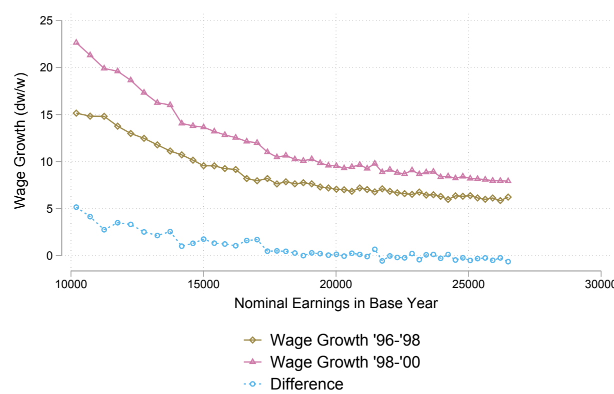
(d) Realized vs. Predicted Benefit Change; 2 Yr



(e) Placebo 2000: 1998-9 vs. 1999-2000; 1Yr



(f) Placebo 1999: 1996-8 vs. 1998-2000; 2 Yr

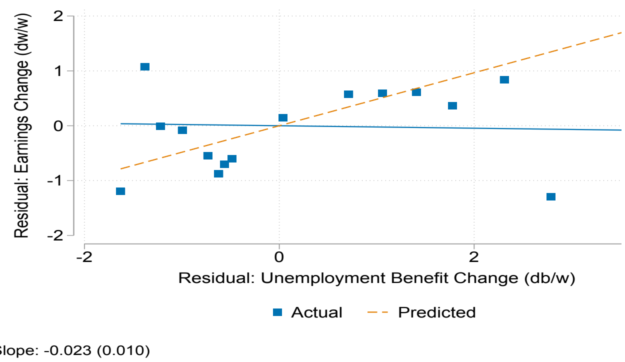


Note: The figure plots additional results related to the analysis in Figure IV(d). We provide a description at the beginning of this Appendix Section (A.3).

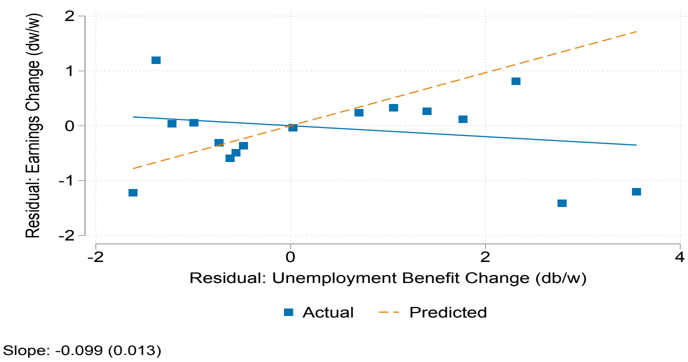
Figure A.6: Pooled Earnings and Benefit Change Bin Scatter Plots - Placebo Years

Year Fixed Effects

(a) Year $e = -2$ Placebo Treatment Effects

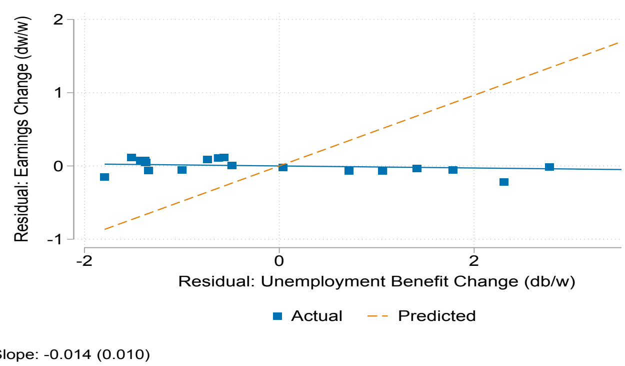


(b) Year $e = -3$ Placebo Treatment Effects

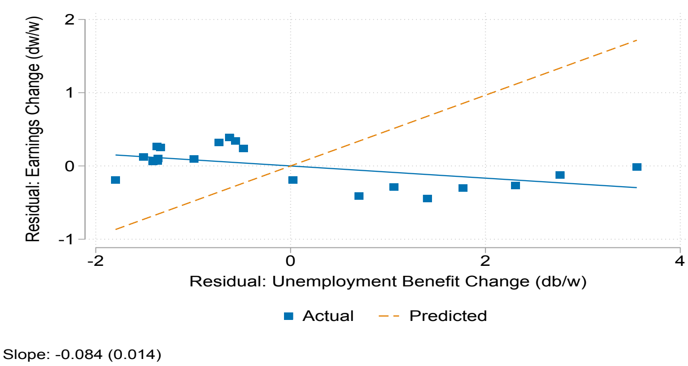


Adding Earnings Percentile Fixed Effects

(c) Year $e = -2$ Placebo Treatment Effects

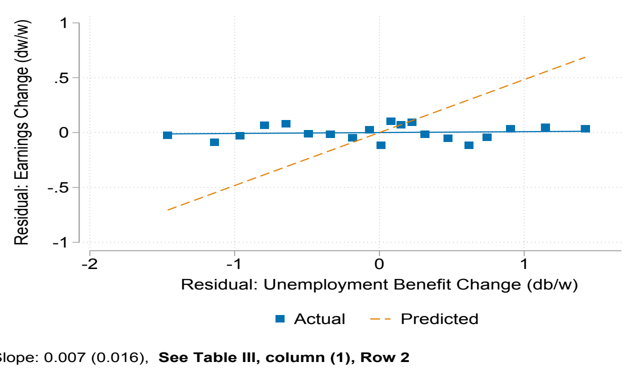


(d) Year $e = -3$ Placebo Treatment Effects

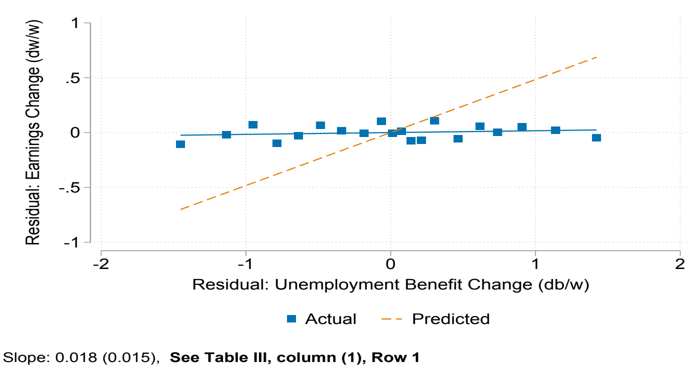


Adding Year-Specific Log Earnings Controls

(e) Year $e = -2$ Placebo Treatment Effects



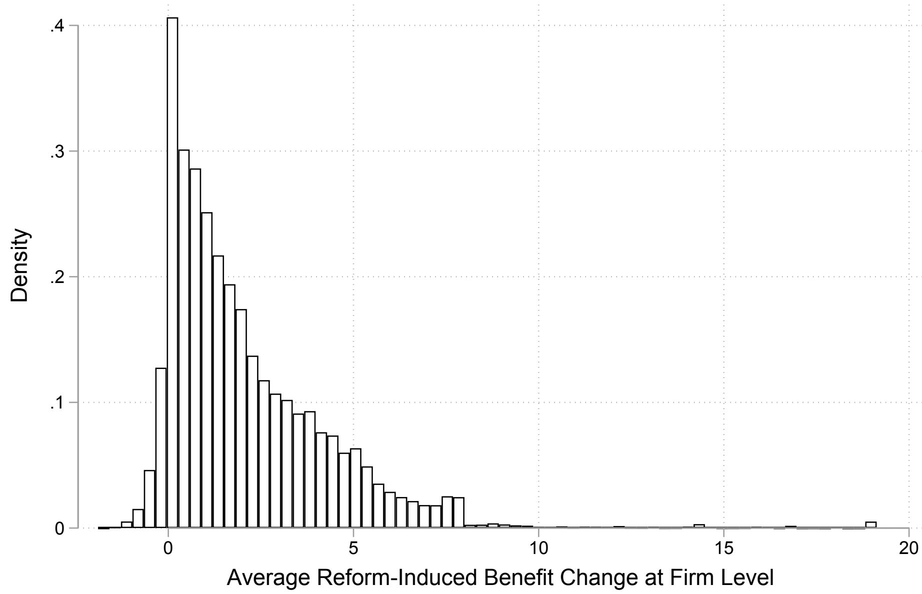
(f) Year $e = -3$ Placebo Treatment Effects



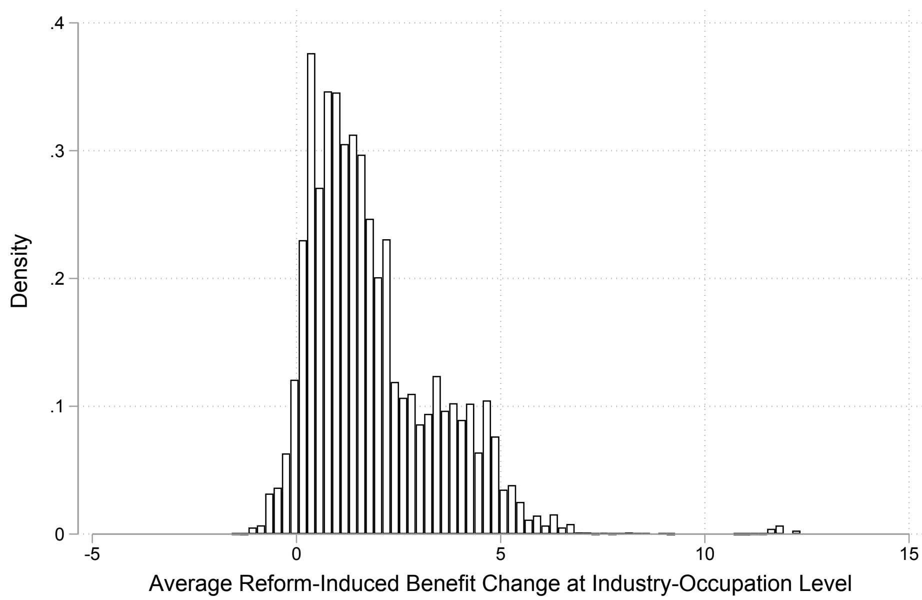
Note: The six panels show the best-fit lines and binned scatter plots from estimating Equation 19 for the pooled sample of all four reforms. The best-fit line slope and standard errors are the coefficient and standard error on σ_{-2} and σ_{-3} in Equation 19. The binned scatter plot is estimated on earnings changes and reform induced benefit changes both residualized by the other included controls.

Figure A.7: Distribution of Benefit Changes at the Firm and Industry-by-Occupation Level

(a) Firm Level



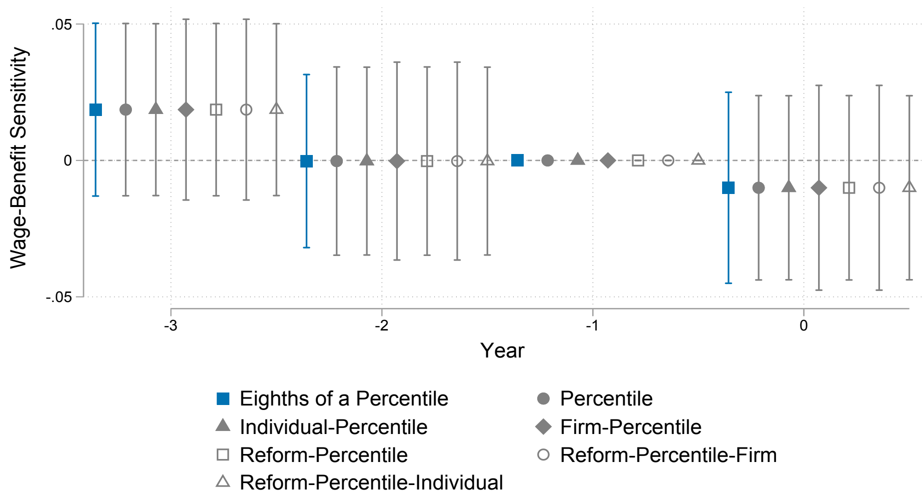
(b) Industry-by-Occupation Level



Note: The figure plots the distribution of the average reform-induced benefit change aggregated at the firm and industry-by-occupation level.

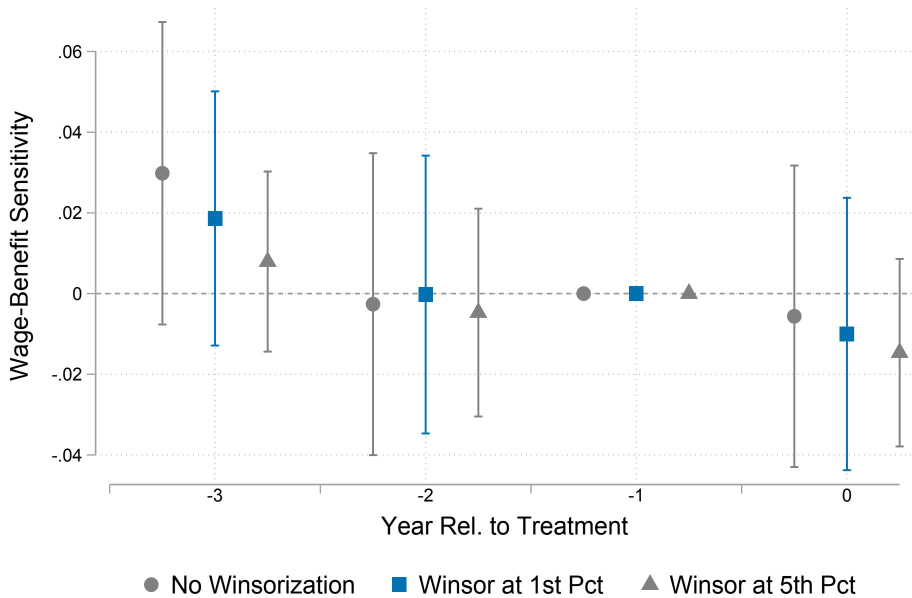
A.4 Additional Robustness Checks of Wage Effects

Figure A.8: Robustness Check: Different Levels of Clustering



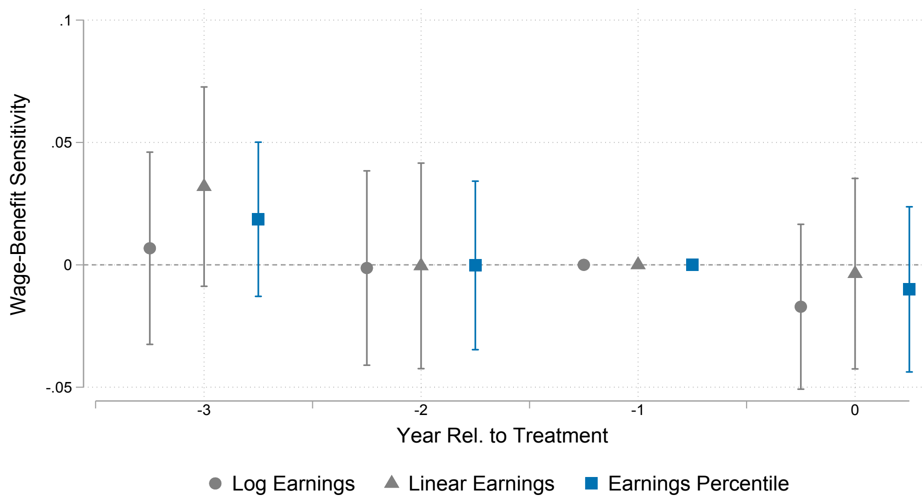
Note: The figure plots estimated δ_0 coefficients and associated confidence intervals based on the difference-in-differences specification in (19). It estimates specification (4) reported in Table III but changes the level of clustering used to calculate the standard errors. Our main specification (eighths of a percentile) is shown in blue (squares). We calculate clustering based on: (1) the eighths of a percentile level, (2) percentile level, (3) two-way clustering at the individual and percentile level, (4) two-way clustering at the firm and percentile level, (5) clustering at reform-specific percentile, (6) two-way clustering at the reform-specific percentile and firm level, and (7) two-way clustering at the reform-specific percentile and individual level. Reform-specific percentiles are calculated as percentiles separately for each reform sample.

Figure A.9: Robustness Check: Outcome Variable Winsorization



Note: The figure plots estimated δ_0 coefficients and associated confidence intervals based on the difference-in-differences specification in (19). It estimates specification (4) reported in Table III but the level of winsorization we use for the outcome variables varies across specifications. Our main specification (1% winsorization) is shown in blue (squares).

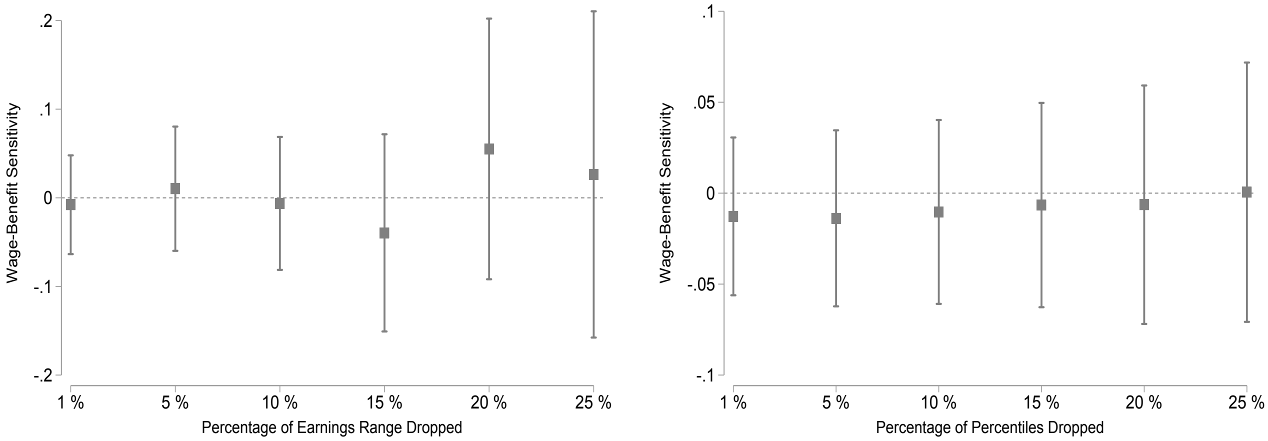
Figure A.10: Robustness Check: Different Parametric Earnings Controls



Note: The figure plots estimated δ_0 coefficients and associated confidence intervals based on the difference-in-differences specification in (19). It estimates specification (4) reported in Table III but changes the year-specific parametric earnings controls used. Our main specification (linear controls for earnings percentiles) is shown in blue (squares).

Figure A.11: Robustness of Wage-Benefit Sensitivity Estimate to Dropping Observations near Treatment/Control Cutoff ("Donut Hole" Specification)

Dropping Fixed Nominal Ranges | Dropping Fixed Percentile Ranges



Note: These figures show σ_0 estimates from estimating Equation (19) but restricting the sample to not include treated and control individuals close to the treatment cutoff. The estimates are from specification (4) in Table III. The left panel presents estimates where starting from the treatment/control cutoff earnings value, we drop a fixed percent of the *nominal earnings ranges* in the treatment and control groups. The right panel presents estimates where starting from the treatment/control cutoff earnings value, we drop a fixed percent of the *earnings percentiles* in the treatment and control groups.

A.5 Separation and Unemployment Effects

In this section, we briefly discuss effects of the reforms on alternative outcomes: separations, unemployment duration, and sickness. Across specifications and outcomes, we find that the benefit increases were associated with quantitatively negligible effects on these outcomes that are statistically indistinguishable from zero in most specifications.

Separation and Unemployment Effects The improvement in the nonemployment outside option may lead marginal workers to select into nonemployment that would have otherwise experienced higher wage growth (e.g. because they are young or have low tenure, and therefore high wage growth).[31] We therefore report treatment effects on separations and unemployment in Appendix Figure A.12 for one- and two-year horizons. The benefit change treatment is expressed in percentage points (i.e. 1ppt db/w is 1), the outcome variables are range from 0 to 1. We do not find a statistically or economically significant effect of the improved nonemployment option. [32] Appendix Figure A.12 also reports treatment effects for the probability of experiencing an employment to unemployment to employment (EUE) spell and the fraction of months spend on UI over the next one and two years. At the two-year level we see suggestive evidence that treatment increased the probability of an EUE spell and the fraction of months spend on UI, consistent with the prior literature on the effects of UI generosity on unemployment spell durations (see Lalive, Van Ours, and Zweimüller, 2006; Card et al., 2015, for evidence from Austria).

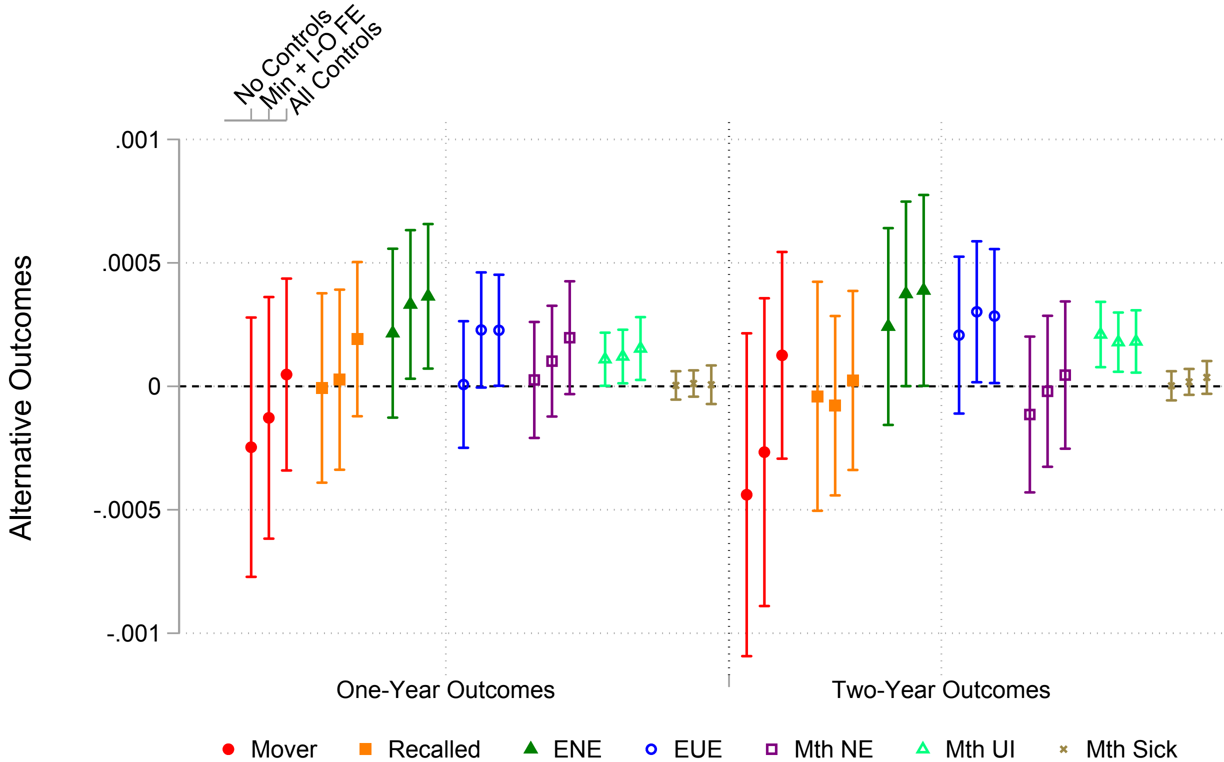
Efficiency Wage Effects: Sickness Incidence Efficiency wage mechanisms may mask bargaining-related wage effects by lowering productivity, if workers are more likely to reduce effort. Yet, we have not found retention effects in the previous robustness checks. We additionally study the treatment effect on registered sickness spells in our administrative data in Appendix Figure A.12. Sickness spells do not respond to the improved outside option.[33]

[31] See Jäger, Schoefer, and Zweimüller (2018) for evidence for older workers separating into nonemployment in response to a large increase in the potential benefit duration, along with characterization of the incremental separators.

[32] Consider the one-year mover estimate of around 0.0001. For a 10 percentage point increase in an individual's replacement rate, we can rule out an increase her mover probability by more than 0.1 percentage points. Compared to the baseline annual one-year mover rate of around 9 %, this would be an economically small increase in the mover rate. The upper end of the one-year mover confidence interval also implies that for a 10 percentage point increase in an individual's replacement rate, we can rule out an increase her mover rate by more than 0.5 percentage points.

[33] However, the productivity decrease would have had to be tremendous in order to account for the net wage effect of zero. If worker bargaining power were 0.1, then the 8ppt increase in the change in benefits (normalized by the wage) would have had to imply a $\frac{0.48 \times 8\text{ppt}}{0.1} = 38.4$ppt decline in the productivity/wage ratio to offset the bargaining channel and leave wages unchanged on net. Since we expect that $w/p \approx 1$ we would need a similar order of magnitude *percent* decline in productivity to offset the bargaining effect.

Figure A.12: Employment and Separation Effects: Difference-in-Differences Regression Design



Note: The figure plots σ_0 coefficients from estimating equation 19 but replacing the $\frac{dw}{w}$ outcome with alternative outcomes. All of the alternative outcomes range from 0-1 (either transition probabilities or shares) and the dependent variable is the percentage point change in $\frac{dw}{w}$ (ranging from around 0 to 20). *Mover*, *Recalled*, *ENE* and *EUE* refer to indicators for going through different employment transition types in the next one or two years. Specifically, *mover* refers to individuals who are observed at a new employer and do not return to their original employer within the next one or two years. *Recalled* refers to individuals who leave their current employer for another employer or nonemployment and then return to their original employer within the next year or two (depending on the specification). *ENE* refers to employer to different employer transitions with an intermediate nonemployment spell (excluding paternity leave). *EUE* refers to employer to different employer transitions with an intermediate unemployment spell (measured by any UI receipt). *Mth NE*, *Mth UI*, and *Mth Sick* are the share of months in the next one or two years spent in different labor market states (they range from 0-1). *Mth NE* refers to the share of months nonemployed. *Mth UI* refers to the share of months on UI receipt. *Mth Sick* refers to the share of months on sick leave. To construct these shares, we use a denominator of 12 months for a given year for individuals not observed for all 12 months of the particular year. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation. Firm FE indicates that time-varying firm-fixed effects were included. The base rates for the outcome variables averaged across all the pre-reform years for the 1-year horizon are: *Movers: 0.086*, *Recall: 0.040*, *ENE: 0.049*, *EUE: 0.029*, *Mth NE: 0.044*, *Mth UI: 0.017*, and *Mth Sick: 0.006*. For the 2-year horizon, the base rates are: *Movers: 0.134*, *Recall: 0.057*, *ENE: 0.078*, *EUE: 0.046*, *Mth NE: 0.064*, *Mth UI: 0.023*, and *Mth Sick: 0.006*.

B Measuring Fraction of Time on UI, and Calibrating the Wage-Benefit Sensitivity

Here we describe the measurement of the share of time spent in the various labor market states and the implied wage-benefit sensitivity.

Separator Sample We measure subsequent time spent in different labor market states conditional on an employment to nonemployment separation. We start with our regression sample of workers, described in Section 4.1. Although the sample of workers is the same as for our main analysis, for the τ calculations we use data with labor market states recorded at the *daily* rather than *monthly* level. This allows for more precise measurement of the time spent in each labor market state.

We then define separators in each year t as individuals who separate from employment into nonemployment for at least one day in the next year $t+1$. Importantly, we do not impose that the separator ever take up UI during nonemployment. In our baseline specification, we do not require the separators ever return to employment. In alternate, more realistic, specifications, we require that the separators have at least one subsequent employment spell in the next four years. This restriction drops emigrants or other permanent labor force exits.

Labor Market States For these separators, we count the fraction of time spent in the following three exhaustive labor market states:

τ^E: **Employment** - includes dependent employment (i.e. wage and salary work), minor employment, self-employment, civil servants/military/civil service, maternity/parental leave associated with a firm, and sick leave associated with a firm.

τ^U: **UI-Affected Nonemployment** *For measuring τ^U, "UI" encompasses both UA and UI.*

- $\tau^{U:UI}$: **Unemployment Insurance**
- $\tau^{U:UA}$: **Unemployment Assistance** - *Notstandshilfe*, which is indexed nearly one-to-one to individual-level unemployment insurance and hence affected by the reform-induced UIB changes we study.

τ^O: **Other Nonemployment** - includes any other recorded ASSD spell statuses and other missing labor market states not recorded in the ASSD. [34] [35]

For overlapping spells in the ASSD, we prioritize spells based on the following ordering $UI \succ UA \succ$ Employment $\succ$ Non-UI Employment. For example, if on one day an individual has a UI and an employment spell, the day would be counted towards UI.

[34]Retirement and disability cover almost half of all reported non-UI nonemployment. Around a quarter is potentially spuriously reported social security payments without employer information. Another important category is registered job search (yet without UI receipt) – which in the literature is often counted as unemployment due to its search connotation, yet which we here carefully count as non-UI nonemployment.

[35]For example, if individuals emigrate from Austria their labor market states are not recorded in the ASSD.

Time Horizons After Separation To calibrate τ, we exploit the fact that the discount rate ρ is negligible compared to the job finding and separation rates (f and δ). τ^E, τ^U, and τ^O thus correspond to the share of time separators spend in each respective labor market state. Since our model has infinitely lived agents, we approximate these measures by counting for a "long time."

In our baseline specification, we stop counting labor market states (in both the numerator and denominator of the "share of time" calculation) at the earliest of 16 years post-separation or an individual's death. We stop at 16 years because it is the longest horizon we can study for all of our reforms (the latest of which is in 2001 and our data end in 2018). This variant is most conservative with respect to the wage-benefit sensitivity because it assumes that events such as retirement occur unexpectedly and hence essentially generate a state that is non-UI nonemployment but is still accounted for in the "denominator" of the fraction. We then add more realistic variants where we "stop the clock" earlier when an individual reaches

1. **Absorbing Retirement:** earliest of 16 years post-separation, death, or absorbing retirement. Absorbing retirement is entering retirement and never subsequently becoming employed or taking up UI.

2. **Absorbing Retirement or Disability** earliest of 16 years post-separation, death, absorbing retirement, or absorbing disability (defined analogously to absorbing retirement).[36]

Actual Share of Time Spent in Labor Market States Panel A of Appendix Table A.5 presents the average fraction of post-separation time in different labor market states for the restricted sample of separators. Columns (1) and (2) stop counting after either absorbing retirement or absorbing disability. Columns (3) and (4) stop counting after absorbing retirement. Columns (5) and (6) simply stop counting at 16 years or death. The even columns include no reemployment restrictions and the odd columns add the reemployed in four year restriction. Adding more conservative restrictions reduces the share of time spent in other nonemployment, τ^O, but keeps the relevant share of time affected by UI, τ^U, relatively constant.

Panel B of Appendix Table A.5 presents the average fraction of time spent in different labor market states for the entire regression sample, *not conditioning on a separation*.[37] There are three reasons why the share of time spent on UI and UA are lower for this sample than the separator sample. First, the separation induces a nonemployment spell, whereas most of our full sample will not initially separate. Since Panel A conditions on a separation and we can only measure outcomes for a finite amount of time, the separator sample will by design have a higher share of time spent nonemployed. Second, the separation from employment may raise individuals' future separation rates (or lower job finding rates). Third, there may be compositional differences between individuals more or less likely to separate. We account for this below by adjusting the predicted τ estimates based on observables.

Predicting the Wage-Benefit Sensitivity We then assign each worker in our regression sample (whether she separates or not) a predicted $\hat{\tau}_i$. Since many of these workers will not separate, we construct these predicted values in order to account for compositional differences

[36] Over 90 % of retirement and disability spells are "absorbing."

[37] Here there is no need to impose a 4-year reemployment restriction because all individuals in the sample are initially employed, so the only worker we would drop with the restriction is one that separates into permanent nonemployment on January 2nd for the next 4 years.

between the regression sample and the separator sample in our τ calibrations. Our prediction model is an OLS regression of the separators' actual fraction of time spent in each state on the following predictors $\mathbf{x}_i$:

- 4-digit-industry by occupation (blue/white collar) FE
- Fixed effects for tenure categories in 3-year steps up to 15 years (cutoffs 2, 5, 8, 11, 14)
- Fixed effects for experience categories in 5-year steps up to 25 years (cutoffs: 5, 10, 15, 20)
- Fixed effects for age categories in 5-year steps (cutoffs 29, 34, 39, 44, 49)
- Region of establishment FE (3-digit NUTS)
- Gender FE
- 6 categories of months since UI: 1 for the censored value, then year-specific quintiles
- Reform-Year FE

For our preferred specification, column (1) in Table I and in Appendix Table A.5, the R^2 for this prediction exercise is 0.09. We then use the model coefficients on the $\mathbf{x}$s to predict individual-level τ^U, τ^E, and τ^O for each individual in our regression sample.

Average Predicted Share of Time Spent in Labor Market States The first five rows in Table I present the average predicted $\hat{\tau}_i$ values across the entire regression sample. The six columns present the same specifications as in Appendix Table A.5. Compared to the average τ_i values just for the sample of actual separators in Appendix Table A.5, the average predicted $\hat{\tau}_i$ values across the entire sample in Table I are slightly larger for UI affected nonemployment and other nonemployment and correspondingly smaller for employment. These differences are consistent with the actual separators having characteristics associated with higher future separation rates. Yet, across the two groups, the τ averages are qualitatively quite similar.

Calibrating the Wage-Benefit Sensitivity Besides reporting the underlying τ values by state, we also report the implied wage-benefit sensitivities. We report the sensitivities from our baseline two-state model (described in Section 2.1) and the extended three-state model (described in Appendix Section C). In both versions, we assume $\phi = 0.1$.

We construct the sensitivities in two different ways:

1. **Individual-level $\hat{\tau}_i$ values**, $\mathrm{E}[dw/db(\hat{\tau}_i)]$: Here we plug in the individual-level $\hat{\tau}_i$ values into the the two- and three-state wage-benefit sensitivity expressions and take averages over these individual-level wage benefit sensitivities. Since the wage-benefit sensitivity is a non-linear function of $\hat{\tau}_i$, this respects Jensen's Inequality. Additionally, for the three-state model where the wage-benefit sensitivity depends on $\hat{\tau}_i^U$ and $\hat{\tau}_i^O$, this method takes into account the individual-level correlation between the two different predicted values.
2. **Average $\hat{\tau}_i$ values**, $E[dw/db(E[\hat{\tau}_i])]$: Here we take the average $\mathrm{E}[\hat{\tau}_i]$ values presented in Table I . and plug them directly into the two- and three-state wage benefit sensitivities.

The two- and three-state calibrated wage-benefit sensitivities constructed in both ways described above are presented in the bottom four rows of Table I. Across the different specifications, the two-state model predicts a pass-through of 0.45 to 0.49. Additionally, in our preferred specification, column (1), the difference between the individual- and average-level $\hat{\tau}_i$ constructions is around 0.02 percentage points. The three-state model predicts a qualitatively lower pass-through of between 0.17 to 0.24.

Table A.5: *Actual* Fraction of Post-Separation Time on UI (τ)

Time Restriction:	Ret'nt or Disability		Retirement		No Restriction	
Reemployment Restriction:	4-Year	None	4-Year	None	4-Year	None
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Separators					
UI-Affected Nonemployment - τ^N	**0.116**	0.115	0.110	0.107	0.108	0.104
Unemployment Insurance - τ^{UI}	**0.087**	0.087	0.082	0.080	0.081	0.077
Unemployment Assistance - τ^{UA}	**0.028**	0.027	0.028	0.027	0.028	0.027
Employment - τ^E	**0.703**	0.627	0.680	0.607	0.672	0.600
Other Nonemployment - τ^O	**0.181**	0.258	0.210	0.286	0.220	0.296
	Panel B: Full Sample (Not Conditioning on Separation)					
UI-Affected Nonemployment - τ^N	**N/A**	0.041	N/A	0.040	N/A	0.039
Unemployment Insurance - τ^{UI}	**N/A**	0.032	N/A	0.031	N/A	0.030
Unemployment Assistance - τ^{UA}	**N/A**	0.009	N/A	0.009	N/A	0.009
Employment - τ^E	**N/A**	0.826	N/A	0.791	N/A	0.758
Other Nonemployment - τ^O	**N/A**	0.133	N/A	0.169	N/A	0.203

Note: The five rows in Panel A and B present estimates of the fraction of time that individuals in our sample spend on unemployment insurance $\hat{\tau}^{U:UI}$, on unemployment assistance $\hat{\tau}^{U:UA}$, which we pool into a single *UI affected state* $\hat{\tau}^U$ (the sum of $\hat{\tau}^{U:UI}$ and $\hat{\tau}^{U:UA}$, where unemployment assistance is included because it is indexed nearly one-for-one with UI, and we refer to "UI" in the text as encompassing both), employed $\hat{\tau}^E$, and in other nonemployment $\hat{\tau}^O$. The estimates here differ Table I because they are the *actual* fraction of time spent in each state rather than the *predicted* fraction of time for separators (Panel A), and for the full sample (unconditionally on a separation, Panel B). Panel A shows the fraction of time for actual separators and Panel B shows the fraction of time for the entire analysis sample. For the entire sample, we start counting future states in January of each year and do not need to add any reemployment restriction. The τ values for our preferred specification, column (1), are calculated as follows. We stop including labor market states in this share at the earliest of 16 years, reaching age 70, death, or absorbing retirement or disability (defined as entering retirement or disability and without any subsequent employment or UI/UA spells within the 16 year horizon). Columns (3) and (4) also stop counting at absorbing retirement but not disability, and columns (5) and (6) stop counting labor market states only at the earliest of 16 years or age 70. The reemployment-restriction columns (1), (3) and (5) require that individuals in the separator sample return to re-employment (at any job) sometime in the next four years (for at least one day).

C Accounting For Nonemployment Without UI Receipt: Three-State Model

Our model in the main text in Section 2 considers a two-state model of employment and UI-yielding nonemployment. We now consider a general case by which workers' nonemployment spell can consist of nonemployment with UI and without UI receipt. The setup can be interpreted to capture a series of specific institutional features such as limited take-up, finite potential benefit duration, or wait periods. We consider a series of such more specific cases in Appendix Section D.2.

The value of nonemployment continues to be denoted by value N. While nonemployed, the worker loops through states UI (value U and instaneneous payoff $z^U(b)$), and other nonemployment (value O with instantaneous payoff z^O). Transitions from state s to s' are described by Markov process $x^{s\to s'}$. We set $x^{O\to E} = x^{U\to E} = f$ and $x^{E\to N} = \delta$. When separating, due to a exogenous separation shock δ or due to the worker taking up the nonemployment outside option in bargaining, fraction υ workers start in UI, whereas fraction $1-\upsilon$ start in non-UI nonemployment.[38]

The associated value functions are defined as follows:

$$\rho E = w + \delta(N - E) \tag{A2}$$
$$N = \upsilon U + (1-\upsilon)O \tag{A3}$$
$$\rho U = z^U(b) + f(E-U) + x^{U\to O}(O-U) \tag{A4}$$
$$\rho O = z^O + f(E-O) + x^{O\to U}(U-O) \tag{A5}$$

The flow value of nonemployment can then be reformulated as a weighted average of the instantaneous payoffs in each state, analogously to our baseline two-state expression (3) yet augmented with the third state of non-UI nonemployment:

$$\rho N = \tau^U z^U(b) + \tau^O z^O + \tau^E w' \tag{A6}$$

where $\tau^E = 1 - \tau^U - \tau^O$. Concretely in terms of transition rates and the discount factor, the τ^s weights are:

$$\tau^E = \underbrace{\frac{f}{\delta+f+\rho}}_{=1-\tau^N} \tag{A7}$$

$$\tau^U = \underbrace{\frac{\delta+\rho}{\delta+f+\rho}}_{\tau^N} \cdot \underbrace{\left[\frac{(1-\nu)x^{O\to U}}{x^{O\to U}+x^{U\to O}+f+\rho} + \frac{\nu(x^{O\to U}+f+\rho)}{x^{O\to U}+x^{U\to O}+f+\rho}\right]}_{\alpha} \tag{A8}$$

$$\tau^O = \underbrace{\frac{\delta+\rho}{\delta+f+\rho}}_{\tau^N} \cdot \underbrace{\left[\frac{(1-\nu)(x^{U\to O}+f+\rho)}{x^{O\to U}+x^{U\to O}+f+\rho} + \frac{x^{U\to O}\nu}{x^{O\to U}+x^{U\to O}+f+\rho}\right]}_{1-\alpha} \tag{A9}$$

which we can separate into a series intuitive weights.

(i) $\tau^E = \frac{f}{\delta+f+\rho}$ is the (discount-rate-adjusted) time spent in employment. (ii) $\tau^N = \frac{\delta+\rho}{\delta+f+\rho} = 1-\tau^E$ is the (discount-rate-adjusted) time spent in nonemployment. Within the nonemployment

[38] This consideration eliminates the need to consider separate wages for eligible and ineligible workers.

state, (iia) workers spend fraction α of nonemployment time receiving UI, and (iib) share $1-\alpha$ of time nonemployment not receiving $z^U(b)$ but instead z^O. There are various ways by which workers end up in a given nonemployment state. They either enter the state initially (and then stay, or move out but then back in). Or, they enter the state from the other nonemployment state (and then leave, but may re-enter).

The Nash wage bargain follows the same structure as in the two-state model, yet augmented with the third state in the outside option:

$$w = \phi p + (1-\phi)\rho N \tag{A10}$$
$$= \phi p + (1-\phi)\cdot\left(\tau^U z^U(b) + \tau^O z^O + \tau^E w'\right) \tag{A11}$$

As in the full model in Section 2.2, we assume that $\frac{dz^U(b)}{db} = 1$ which implies that the wage-benefit sensitivity is:

$$\frac{dw}{db} = (1-\phi)\left(\tau^U + \tau^O \frac{dz^O}{db} + \tau^E \frac{dw'}{db}\right) \tag{A12}$$

which, if again using $\frac{dw'}{db} = \frac{dw}{db}$, solves into:

$$\frac{dw}{db} = \frac{(1-\phi)\left(\tau^U + \tau^O \frac{dz^O}{db}\right)}{1-(1-\phi)\tau^E} \tag{A13}$$

Maximal attenuation vis-à-vis the two-state model arises if payoff z^O is insensitive to b, i.e. $\frac{dz^O}{db} = 0$ (assuming away the curious case of $\frac{dz^O}{db} < 0$):

$$\frac{dw}{db} = \frac{(1-\phi)\tau^U}{1-(1-\phi)\tau^E} = \frac{(1-\phi)\tau^U}{1-(1-\phi)(1-(\tau^U+\tau^O))} \tag{A14}$$

Here, the higher τ^O, the less weight nonemployment value N puts on b-sensitive payoffs $z^U(b)$ or w', thereby attenuating either the mechanical effect or the feedback effect in the wage-benefit sensitivity, or both. In our baseline two-state model in Section 2.1, we permitted only two states – nonemployment with UI receipt and employment, and therefore $1-\tau^U = \tau^E$, effectively assuming that $\tau^O = 0$. We calibrated τ^U to the empirical share of post-separation time spent on UI – a number that carries over to the extended three-state model (i.e. $\tau^U = \tau$). In the extended three-state model, the implied time in reemployment $\tau^E = 1-\tau^U-\tau^O$ therefore is the τ^s that is attenuated by measuring and including τ^O.

Comparison to Baseline Two-State Model Our baseline two-state model assumed that $\upsilon = 1$ and $x^{U\to O} = 0$. This implies that $\tau^O = 0$, $\alpha = 1$, $\tau^N = \tau^U$, and $\tau^E = 1-\tau^U$.

Extensions Alternative setups are conceivable. A interesting setup we side-step above is one by which outside options are differentiated by eligibility status, which in turn may evolve while employed. Another setup would have workers be permanently, or expectedly, eligible or ineligible, with this status known to the bargaining parties. We cannot credibly differentiate these alternatives in the data.

Calibrating the Wage-Benefit Expression in the Presence of Non-UI Nonemployment Table I presents estimates of the wage-benefit passthrough in expression A14 calibrated based on our estimates of τ^U and τ^O. In our preferred specification, column (1), the estimated wage-benefit sensitivity from the three-state model is 0.24, compared to 0.46–0.48 in the two-state variant. See Appendix Section B for more details about the three-state model calibration.

D Additional Theoretical Material

D.1 The Size of the Bargaining Set

We thank a reviewer for encouraging us to evaluate the implicit assumption that the predicted wage effect remains below the firm's post-reform reservation wage for our reforms, i.e. that the job has sufficiently large initial firm surplus. That is, we want to make sure that the underlying model of the labor market actually admits, quantitatively, wage increases of an order of magnitude implied by the wage-benefit elasticity (0.48 or 0.24) we calibrate, and by benefit increases we exploit in form of the Austrian UIB reforms – i.e. that the predicted wage increases are within the bargaining set of the model.

We find that away from the most basic DMP setting, incorporating firing or hiring costs and specific human capital suffices to accommodate the predicted wage effects.

Concretely, we address the concern that such limitation may pose an upper bound on the potential wage effects between our treatment group and the control group. Since we typically consider wage increases, the relevant perspective is the firm's surplus and reservation wage.

Below, we argue that away from the textbook DMP setting (where jobs are only valuable due to vacancy posting costs), incorporating some realistic features should lead to sufficiently large firm surplus such that the wage effects our model predicts will be born out in realized wage responses.

First, we provide an overview of the magnitudes of wage responses we would typically expect the treatments to entail. These are typically small, on the order of 2.40% (mean) and 2.10 (median) for the 0.48 benchmark. (These values are of course half the size for the 0.24 benchmark, namely 1.20 (mean) and 1.05 (median) percent.)

Second, we provide a transparent assessment of how much attenuation we would expect under a broad class of *wage-increase caps* that the economy would support in the treatment group. Here, we conclude that the CIs from our empirical analysis (0.03) would require jobs to sustain a wage increase of less than 0.2ppt (see Appendix Figure A.13 – going from 0.03 of the y-axis to the x-axis, which gives a value below 0.2ppt).

Third, we use a DMP-like model to *calibrate* the maximum permissible wage increase, and confirm that the specific baseline model generally does not permit wage increases (before the typical job would be destroyed, for example). However, we show that plausible extensions to features likely present in real-world labor markets dramatically relax this result, accommodate our benchmark quite comfortably, and imply little attenuation.

Recapping the Predicted Wage Changes To clarify magnitudes, we describe the distribution (mean, p25, p50, p75, p90, and p95) of the benefit changes $\frac{db}{w}$ in the treatment group in Appendix Table A.6, along with the predicted unconstrained wage changes $\left(\frac{dw}{w}\right)^* = x \cdot \frac{db}{w}$, where we use $x \in \{0.24, 0.48\}$ to span the sensitivity range. Below we quote the 0.48 sensitivity because it provides the largest wage effects.

We do so for the pooled sample and separately for each reform (1976, 1985, 1989, 2001), where we also clarify the fraction of the sample contributed by a given reform. A large reform, 2001, makes up 37%, but has relatively low predicted wage effects in the treatment group, with a mean of 1.33%, a median of 1.57%, and 2.15% at the top 90th percentile. By contrast, the tiniest reform by far in terms of sample share at 5.7% is the 1976 reform, where the mean and median wage changes predicted by our Nash benchmark are 4.39 and 4.23%, respectively. When pooling the reforms (weighted by observations, reflecting our regression design and in proportion roughly to the "fraction" column), we find that the mean predicted wage change is 2.40%, and the median is 2.10%, and even the 95th percentile is at 4.47%.[39]

Table A.6: Cross-Sectional Distribution of Predicted Wage Changes $\left(\frac{dw}{w}\right)^* = x \cdot \frac{db}{w}$ with $x \in \{0.24, 0.48\}$ (*and, in Italics, Benefit Changes* $\frac{db}{w}$)

Reform	Mean	P25	Median	P75	P90	P95	Fraction
Pooled	1.20/2.40 *5.00*	0.68/1.37 *2.85*	1.05/2.10 *4.37*	1.83/3.67 *7.64*	2.15/4.29 *8.94*	2.23/4.47 *9.31*	100.0%
2001	0.66/1.33 *2.77*	0.48/0.96 *2.01*	0.78/1.57 *3.27*	0.96/1.92 *3.99*	1.08/2.15 *4.48*	1.10/2.21 *4.60*	37.0%
1989	1.46/2.91 *6.07*	1.24/2.48 *5.16*	1.78/3.57 *7.43*	1.86/3.72 *7.74*	1.89/3.79 *7.89*	1.91/3.83 *7.97*	19.1%
1985	1.44/2.88 *6.00*	0.81/1.62 *3.37*	1.60/3.20 *6.66*	2.10/4.20 *8.75*	2.21/4.41 *9.19*	2.25/4.49 *9.36*	38.2%
1976	2.19/4.39 *9.14*	0.96/1.92 *4.01*	2.11/4.23 *8.81*	3.45/6.90 *14.37*	4.04/8.09 *16.85*	4.34/8.67 *18.07*	5.7%

Note: We report a benchmark for expected wage changes in the treatment group. Specifically, we constructed moments of the reform-induced db/w among individuals in the earnings percentile range we identified as the treated group and then multiplied them by 0.24 (left) and 0.48 (right), where the left entry simply divides the 0.48 benchmark by 2 (and rounding up to the nearest digit). Below the benchmark, we report the value of the reform-induced db/w. The units are percentage points of the base-year wage.

Attenuation from Constrained Wage Increases Next, we present a transparent and general evaluation of how much attenuation we should expect in an environment in which the feasible – and hence measured – wage response $\frac{dw}{w}$ to the empirical variation $\frac{db}{w}$ is limited to be $\frac{dw}{w} = \max\left\{\overline{\frac{dw}{w}}, \left(\frac{dw}{w}\right)^*\right\}$. Here, $\left(\frac{dw}{w}\right)^*$ is the "unconstrained" predicted wage effect. For concreteness, we again use our calibrated Nash benchmark as the benchmark such that $\left(\frac{dw}{w}\right)^* = x \times \frac{db}{w}$ with $x \in \{0.24, 0.48\}$.

The object of interest is our estimate of the wage-benefit sensitivity σ for various levels of maximum wage increases $\overline{\frac{dw}{w}}$ in this constrained environment. Concretely, we pool our reforms and use the empirical cross-sectional distribution of $\frac{db}{w}$ (collapsed to the percentile level rather than individual level). For each observation, we assign a wage increase given by the truncated rule $\frac{dw}{w} = \max\left\{\overline{\frac{dw}{w}}, \left(\frac{dw}{w}\right)^*\right\}$. We generate multiple "economies" that only vary in terms of their respective maximum wage increase $\overline{\frac{dw}{w}}$. For each economy we then separately estimate the wage-benefit sensitivity σ. Naturally, when the maximum $\overline{\frac{dw}{w}}$ does not bind (i.e. it is higher than

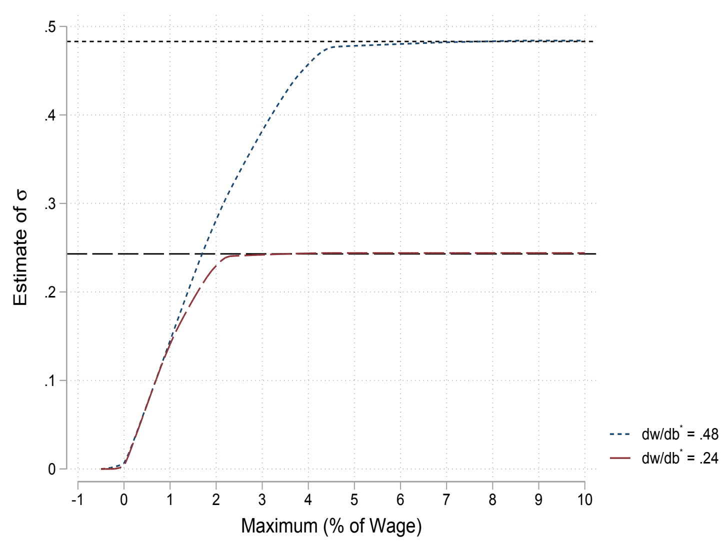
[39]We have confirmed that our results are robust to dropping the 1976 reform, therefore removing these positive "outliers".

the largest predicted wage increase in the sample), we will recover the structural sensitivity of $\sigma = 0.24$ or $\sigma = 0.48$. Whenever $\overline{\frac{dw}{w}}$ binds for some observations such that $\overline{\frac{dw}{w}} < \left(\frac{dw}{w}\right)^*$, we will estimate a lower σ – our broadest version of the type of attenuation expected from the bounded wage response.

Appendix Figure A.13 plots the sensitivity that would be estimated for a series of imposed upper limits on the permissible wage increase. The figure features two lines, one for the 0.24 benchmark and one for the 0.48 benchmark, i.e. from an economy in which wages exhibit the respective benefit sensitivity (unless hitting the corner). By construction, we estimate a zero effect when wages are not permitted to increase.[40] When wage increases are not constrained, the estimates recover our benchmark sensitivities of 0.24/0.48. For intermediate values, attenuation emerges. Yet, even in an economy where *each and every job would be destroyed if wages were to idiosyncratically increase by 1%*, we would still expect sensitivities about 0.15, which our CIs permitted us to rule out. By contrast, even just permitting a 2% wage increase constraint generates sensitivities well above 0.2. A 3% constraint generates a sensitivity essentially equal to the 0.24 benchmark (and is close to 0.4 in a world with a 0.48 structural elasticity). A 5% constraint generates sensitivities essentially equal to the 0.48 benchmark.

An interesting perspective is translating our empirical estimate into an implied maximum wage change that would rationalize the "unconstrained" benchmark with our empirical estimate. Our preferred specification had an upper CI of 0.03. To *explain* this small effect with wage-growth constraints despite a "structural" 0.24 or 0.48 sensitivity benchmark, the average job must at most support a wage increase of less than 0.2% – a result that emerges by going "in reverse": from 0.03 on the y-axis to the x-axis.

Figure A.13: Implied Sigma as a Function of the Degree of Truncation of Wage Increases



Overall, already small wage-increase "cap" values permit a fairly sizable pass-through compared to our point estimates. For example, even when wage-increase caps are very moderate, 2 percent or even somewhat less, the (constrained) benchmark model predicts a fairly sizeable pass-through, much larger than what we find empirically.

[40]Some benefit changes are negative, so the intercept is tiny but positive.

Next, we formally derive and calibrate variants of model-given maximal wage increase below.

Calibration Approach: Summary To quantify the plausible degree of attenuation of the empirical estimate for the wage-benefit sensitivity arising from a maximum limit on the job-level wage increases, $\overline{\frac{dw}{w}}$, we now calibrate this maximum limit, That is, we calibrate the upper bound for the wage effect the bilateral relationships can support. We can then *plug in* this maximal percent increase for our constraint $\overline{\frac{dw}{w}}$ into the feasible wage increase $\frac{dw}{w} = \max\left\{\overline{\frac{dw}{w}}, \left(\frac{dw}{w}\right)^*\right\}$. We can then locate for any given calibration the feasible wage-benefit sensitivity along the lines illustrated in Appendix Figure A.13.

We start with a purely idiosyncratic job-specific perspective, which provides the strictest constraint: only 1 single job in the economy is treated, with no other jobs, so the firm's reservation wage is tightest in that the firm's outside option is to hire any other – untreated and hence cheaper – worker. That single job has a given wage $\tilde{w}$. Our goal is to characterize and calibrate the maximal wage increase the firm would accept before preferring separation. We then consider richer environments with other sources of surplus (e.g. training) and when a non-zero mass of workers is treated (mirroring our empirical setting).

Calibration: Zero Mass Treated In this baseline example, we use the firm's participation constraint to derive a useful statistic: the firm's *reservation wage* $\tilde{w}^r$, i.e. the level of the job's idiosyncratic wage $\tilde{w}$ at which the firm's participation constraint holds with equality. Specifically, we reformulate this expression as the *maximum wage increase*, i.e. the difference between the reservation wage $\tilde{w}^r$ and the original wage $\tilde{w}$, to check how much of a wage increase we can expect in this environment, given an expected prevailing wage w for an untreated – and hence perhaps cheaper – replacement hire:[41]

$$\Rightarrow \tilde{w}^r - \tilde{w} = \frac{\rho+\delta}{\rho+\delta+q} \cdot [c + p - w] \tag{A15}$$

And the *maximal (percent) wage premium* is similarly (where we use $\tilde{w} = w$ as in our setting, where the treatment and control group initially have similar wage levels, since otherwise the RHS would be marked up/down by $\frac{w}{\tilde{w}}$):

$$\Leftrightarrow \frac{\tilde{w}^r - \tilde{w}}{\tilde{w}} = \frac{\rho+\delta}{\rho+\delta+q} \cdot \left[\frac{c}{w} + \frac{p-w}{w}\right] \tag{A16}$$

Next, we calibrate this maximal wage increase $\frac{\tilde{w}^r - \tilde{w}}{\tilde{w}}$ to put a number on $\overline{\frac{dw}{w}}$ and ultimately gauge the implied attenuation of the measured wage-benefit sensitivity along the lines illustrated by Appendix Figure A.13.

We will confirm that in this theoretical setting, tiny exogenous wage changes would induce

[41] The firm's participation constraint solves into $\rho J(\tilde{w}) \geq \rho V$. Then, using $\rho J(\tilde{w}) = p - \tilde{w} + \delta(V - J(\tilde{w})) = \frac{\rho}{\rho+\delta} \cdot [p - \tilde{w}] + \frac{\delta}{\rho+\delta} \cdot [\rho V]$ (where where we consider the case in which the job at hand carries wage $\tilde{w}$ not just in the current period but forever, consistent with the perspective on a persistence change in the job's fundamentals), we reformulate to: $\frac{\rho}{\rho+\delta} \cdot [p - \tilde{w}] + \frac{\delta}{\rho+\delta} \cdot [\rho V] \geq \rho V \Leftrightarrow [p - \tilde{w}] \geq \rho V$. Now using $\rho V = -c + q(J(w) - V) = \frac{\rho+\delta}{\rho+\delta+q} \cdot [-c] + \frac{q}{\rho+\delta+q} \cdot [p - w]$, without having to impose free entry ($V = 0$), we obtain: $[p - \tilde{w}] \geq \frac{\rho+\delta}{\rho+\delta+q} \cdot [-c] + \frac{q}{\rho+\delta+q} \cdot [p - w]$. The reservation wage is the wage for which the condition holds with equality.

separations (which would not occur in the first place if bargained over). However, this "small firm surplus" view crucially relies on DMP-like recruitment costs as the only source of ex-post job surplus, and strongly relies on perfect homogeneity, i.e. a specific version of the model, and on only *one* worker being treated.

At the monthly frequency, the "multiplier" $\frac{\rho+\delta}{\rho+\delta+q} \approx \frac{0+0.02}{0.02+0.5} \approx 0.04$, where we ballpark $q = 0.5$ and $\delta = 0.02$ is the total separation rate of a job (including both nonemployment and other jobs, which is the appropriate concept here).

The two terms in the brackets denote the opportunity cost of separating and rehiring: recruitment cost c, and and cash flow $p - w$.

c/w is the ratio of the recruitment cost to the per-period wage, which we reformulate to $q \cdot (c/q)/w$ in order to calibrate. c/q represents the recruitment cost per hire. Manning (2011) and and Silva and Toledo (2009) suggest hiring costs as a percentage of monthly pay to be 12.9%, such that $(c/q)/w = 0.129$. We thus ballpark $c/w = q \cdot [(c/q)/w] = 0.5 \cdot 0.129 = 0.0645$. This term, when multiplied by 0.02, will be tiny. This result reflects the property that pure recruitment costs may be small when amortized.

$(p-w)/w$ is an important term *notoriously hard to price* unless one makes specific assumptions – which may be unrealistic and model-specific. For example, a naïve calibration to the labor share $w/p = 2/3$, implying $(p - w)/w = (p - w)/p \cdot p/w = 1/3 \cdot 3/2 = 1/2$, yields feasible wage increases of around $0.02 \cdot 0.5 = 0.01$. In Appendix Figure A.13, this small value would already imply an empirical wage-benefit sensitivity estimate above 0.1. (However, we note that w/p is not the labor share, but closer to the gap between the MPL and the wage (rather than ALP as in the labor share). Moreover, this term would still ignore a variety of important sources of job surplus that we list below.)

Clear – though perhaps not empirically useful – guidance is given by specific models like the textbook equilibrium DMP model. Here, (i) the zero profit condition holds, (ii) jobs and workers are homogeneous, (iii) no idiosyncratic match-quality shocks hit on the job, (iv) no human capital acquisition takes place, and (v) exactly zero separation costs (e.g. layoff taxes, severance payments) exist, so that $(p - w)/w$ can be calibrated to be tiny for new hires. Specifically, in equilibrium the present-value benefit of hiring (a filled job) is given by $(p - w)/(\rho + \delta)$, which is set equal to hiring cost c/q, implying that $p-w = (\rho+\delta)c/q$, and so $(p-w)/w = (\rho+\delta)(c/q)/w$. Noting that we had previously ballparked $(c/q)/w = 0.129$, and $\rho \approx 0$ and $\delta \approx 0.02$, this term would be very close to zero, around $0.02 \cdot 0.0129$. Analytically, with zero profit conditions and standard DMP, the previous overall expression therefore simplifies to:[42]

$$\Leftrightarrow \frac{\tilde{w}^r - w}{w} = q \cdot \frac{c/q}{w} \cdot (\rho + \delta) \tag{A17}$$

which is around $0.5 \cdot 0.129 \cdot 0.02 \approx 0.00129$. Indeed, in this textbook DMP setting, jobs' wages are so tightly pushed against the firm's reservation wage that firms would lay off workers even if wages were just to increase by 0.13%! To us, this range of admissible wage increases feels unrealistically low. For example, Saez, Schoefer, and Seim (2019) document no decline in employment (no increase in separations) when jobs experience a 15ppt increase in the employer-born payroll tax rate for young workers (perhaps occupying low-surplus jobs) in Sweden, when workers age out of an age-dependent payroll subsidy or when the policy overall is sharply abolished.

[42]The derivation is: $\frac{\tilde{w}^r-w}{w} = \frac{\rho+\delta}{\rho+\delta+q} \cdot \left[\frac{c}{w} + \frac{p-w}{w}\right] = \frac{1}{w}\frac{\rho+\delta}{\rho+\delta+q} \cdot [c + p - w] = \frac{1}{w}\frac{\rho+\delta}{\rho+\delta+q} \cdot \left[c + \frac{c}{q}(\rho+\delta)\right] = \frac{1}{w}\frac{\rho+\delta}{\rho+\delta+q} \cdot \frac{c}{q} \cdot [q + \rho + \delta] = q \cdot \frac{c/q}{w} \cdot (\rho + \delta)$.

Below we present and price four main extensions, all of which expand the scope for wage responses.

Firing Costs First, the aforementioned models applies purely to new hires, whereas we also study incumbent workers. The firm's participation constraint for an incumbent worker contains, for example, a layoff cost, which we denote by k:

$$J(\tilde{w}) \geq V - k \tag{A18}$$

augmenting the baseline expression as follows:

$$\Leftrightarrow \frac{\tilde{w}^r - w}{w} = \frac{\rho + \delta}{\rho + \delta + q} \cdot \left[\frac{c}{w} + \frac{p - w}{w}\right] + (\rho + \delta) \cdot \frac{k}{w} \tag{A19}$$

As discussed in our institutional section, one formal component of k in Austria during our study period are severance payments that are only due upon layoffs, and a step function of tenure. Hence, k/w will be 4 for some of our workers, which multiplied by $\delta = 0.02$ would dramatically expand the wage increase by 8ppt on its own – above the level required to accommodate *all* of our reforms to exhibit the theoretical sensitivity.

We expect similar expressions with upfront hiring costs along the lines of Pissarides (2009).

Human Capital Second, the model has treated new hires and incumbent workers as perfect substitutes in, for example, productivity even though empirical evidence suggests otherwise Jäger and Heining (2019). Specific human capital built over the job further widens the gap between the incumbent worker and new hire.[43] Consider the case in which a typical incumbent worker is more productive – in present value terms – by two monthly salaries, which is a small number when amortized over the course of the job spell. This feature would provide an additional 4ppt boost in the wage-growth range, and hence again comfortably accommodate our wage increases.[44] Appendix Figure A.13 clarifies that we would already recover our benchmark 0.24 sensitivity with a 4ppt cap (but also closely approximating 0.48).

Note that we did not find larger effects for higher-tenured workers, for whom either of the aforementioned extensions would predict a wider bargaining set. This additional evidence further suggests that the mechanism is not present even in cells in which the small firm surplus is less likely to constrain wage increases.

Heterogeneity Third, the model example is one of perfect homogeneity. While used in macroeconomic analysis, the real world is likely characterized by heterogeneity in canonical match-specific or worker productivity. In this setting, there will be some workers with small firm surplus (and hence little room to bargain up their wage), and some workers with large firm surplus due to e.g. higher productivity, who could exhibit the full pass-through (e.g., again high-tenure workers (selection)). Alternatively, younger workers (perhaps underpaid due to

[43]With small bargaining power, the worker only appropriates some of this productivity gain in the form of higher wages.

[44]For example, in the previously cited sources for hiring costs c/q we strictly set to refer to recruitment costs, whereas the reviews by Manning (2011) and Silva and Toledo (2009) would suggest even larger investments – and hence ex post surplus – when including explicit training costs. In addition, learning on the job and other post-hire training investments will further increase productivity.

implicit contracts stories) or female workers (with potentially lower bargaining power) did not exhibit larger effects either. Relatedly, firms with more heterogeneity in wage growth did not exhibit larger pass-through either. Workers with low fixed effects in AKM wage regressions did not exhibit a larger effects. Even though we use EU transitions, we also do not find different effects in jobs with larger separation rates (δ in the expression).

Positive Mass of Workers Treated Fourth, we have considered the extreme case of only one worker being treated, rather than a group. With more than a single worker treated, the firm's layoff margin for the purpose of hiring a cheaper worker is reduced, thereby widening the wage growth range. In the data, our reforms of course do affect a lot of workers. We now study how this mechanism plays out.

Now consider a variant where fraction α of the labor force (both among the employed and unemployed) has some wage $\overline{w}$ (*will be "treated"*), and the remainder $1-\alpha$ has wage $\underline{w}$ (*will be the "control group"*). Initially, their wages are the same.

Due to random search, the value of the vacancy now considers the possibility of running into high-wage and low-wage workers:

$$\rho V = \frac{\rho+\delta}{q+\rho+\delta}\cdot[-c] + \frac{q}{q+\rho+\delta}\cdot[p - [\overbrace{\alpha\overline{w} + (1-\alpha)\underline{w}}^{\mathbb{E}w}]] \tag{A20}$$

Consider now the interval for wage increases in a given job of type $\tilde{w} \in \{\underline{w}, \overline{w}\}$. We again construct the "maximal wage premium" expression but generalized to $0 \leq \alpha \leq 1$ rather than $\alpha = 0$ mass of treated workers. We start with the premium of a given job above the expected wage:[45]

$$\Rightarrow \tilde{w}^r - \mathbb{E}w = \frac{\rho+\delta}{\rho+\delta+q}\cdot[c+p-\mathbb{E}w] \tag{A21}$$

At this point, we once again solve for the statistic of interest: the maximal reservation wage in a scenario where the "high" wage $\underline{w}$ is equal to that reservation wage – here the maximal gap between the treatment group and the control group. Therefore, we define $\overline{w}^r = \overline{w}$ and solve:

$$\Rightarrow \overline{w}^r - [\alpha\overline{w}^r - (1-\alpha)\underline{w}] = \frac{\rho+\delta}{\rho+\delta+q}\cdot[c+p-[\alpha\overline{w}^r + (1-\alpha)\underline{w}]] \tag{A22}$$

$$\Leftrightarrow \frac{\tilde{w}^r - \underline{w}}{\underline{w}} = \frac{\frac{\rho+\delta}{\rho+\delta+q}}{(1-\alpha) + \alpha\frac{\rho+\delta}{\rho+\delta+q}}\cdot\frac{c+p-\underline{w}}{\underline{w}} \tag{A23}$$

The expression has intuitive properties. For $\alpha = 0$, we nest the previous case, where the treated workers have mass zero, and the multiplier was $\frac{\rho+\delta}{\rho+\delta+q} \approx \frac{0.02}{0.02+0.5} \approx 0.04$. Note that the fraction of transition rates and the discount rate results in a small number, so the denominator is approximately $1-\alpha$ for purposes of evaluating the fraction. For intermediate values of $\alpha = 0.5$ for example, we still double the reservation wage set. For $\alpha \to 1$ yet $\alpha < 1$, we have maximal

[45] We again start with the participation constraint: $\overbrace{\frac{\rho}{\rho+\delta}\cdot[p-\tilde{w}] + \frac{\delta}{\rho+\delta}\cdot[\rho V]}^{J(\tilde{w})} \geq \rho V \Leftrightarrow [p-\tilde{w}] \geq \rho V \Leftrightarrow [p-\tilde{w}] \geq \frac{\rho+\delta}{\rho+\delta+q}\cdot[-c] + \frac{q}{\rho+\delta+q}\cdot[p - \overbrace{\alpha\overline{w} + (1-\alpha)\underline{w}}^{\mathbb{E}w}]$.

rebargaining opportunities, with the multiplier going to 1.00 – expanding by two orders of magnitude. To see this, we rearrange to:

$$= \frac{1}{1 + (1-\alpha)\frac{q}{\rho+\delta}} \cdot [c + p - \underline{w}] \tag{A24}$$

which goes to 1.00 (from previous 0.04) in terms of the multiplier for $\alpha \to 1$. Then, even small $c + p - \underline{w}$ terms would already sustain our predicted wage effects even with DMP calibrations in which the ZPC holds.

The ballpark for α would be a useful ingredient. That is, if jobs are usually filled by workers that come from the treatment region, then the pass-through is least likely to be constrained. We could, in principle, generate this index. We have indirect evidence that this channel is unlikely to explain our results. Most importantly, our firm-level heterogeneity in terms of "share of workforce treated" was not associated with larger wage effects. Similarly, a "donut hole" specification (Appendix Figure A.11) did not yield larger wage effects: when dropping larger and larger masses around the T/C cutoff, it is likely that firms hire from increasing polarized groups, such that we approximate a design with large α.

D.2 Additional Nash Bargaining Model Variants

Next, we show that the key prediction from the benchmark model carries over to a wide variety of richer models considered in the literature. In Section 2.3 we additionally discuss alternative models that insulate wages from the nonemployment value, and which may therefore rationalize the zero effect of b on w that we document in the empirical Section 4.

I. Equilibrium Adjustment: DMP Model Together, our difference-in-differences design and theoretical framework aim to isolate the *micro effects* of an idiosyncratic shift in the outside option on wages, holding constant (or netting out with a control group) market-level adjustment. Yet, we cannot definitely empirically rule out the concern that experimental groups populate segmented – rather than roughly the same – labor markets. Our treatment effect would then capture "macro" effects. Next, we derive this macro wage-benefit sensitivity explicitly with equilibrium adjustment in the context of a calibrated DMP model. We show that the magnitude and structure of the micro and macro sensitivities are strikingly similar quantitatively and structurally. We conclude that market-level spillovers cannot explain small zero wage-benefit sensitivities.

The canonical DMP Nash wage replaces the continuation term of the worker with an equilibrium value related to labor market tightness $\theta = v/u$, the ratio of vacancies v to unemployment u):[46]

$$w^{\text{DMP}} = \phi p + \overbrace{(1-\phi)b + \phi\theta k}^{=(1-\phi)\rho N} \tag{A25}$$

[46]In DMP models, the reemployment capital-gains term in the worker's outside option $\rho N = b + f[E(w'^{\text{DMP}}) - N]$ is replaced with the firm's value of a filled job (recognizing the Nash sharing rule such that $(1-\phi)f[E(w') - N] = \phi f[J(w') - V]$). Free entry has firms post vacancies until the value of vacancies is pushed to zero $V = 0 \Leftrightarrow \frac{k}{q} = J$, implying that $\phi f[J(w') - V] = \phi k f/q = \phi k\theta$, due to the standard constant-returns matching function, by which $f(\theta)/q(\theta) = \theta$, such that $\phi k\theta$ now captures the worker's capital gain from reemployement $(1-\phi)f(E(w') - N)$.

With a market-wide increase in benefits, the capital gain continuation term of ρN is pinned down by firm's free entry, such that the wage comovement is described by:

$$dw^{\text{DMP}} = (1-\phi)db + \phi k d\theta \tag{A26}$$

Next we solve the free entry condition $\frac{k}{q(\theta)} = J = \frac{p-w'}{\rho+\delta}$ for $kd\theta = -dw' \cdot \frac{1}{\eta}\frac{f(\theta)}{\rho+\delta}$ to move into the wage equation (noting that θ is only affected by b through w and denoting by η the elasticity of the matching function respect to unemployment):

$$dw^{\text{DMP}} = (1-\phi)db + \phi\left[-dw'^{\text{DMP}} \cdot \frac{1}{\eta}\frac{f(\theta)}{\rho+\delta}\right] \tag{A27}$$

$$\Leftrightarrow \frac{dw^{\text{DMP}}}{db} = \frac{1-\phi}{1+\phi\frac{1}{\eta}\frac{f(\theta)}{\rho+\delta}} \tag{A28}$$

$$\approx \frac{1-\phi}{1+\phi\cdot\frac{1}{\eta}\cdot(u^{-1}-1)} \tag{A29}$$

where step 2 uses $dw = dw'$, and step 3 uses $\frac{f}{\rho+\delta} \approx \frac{f}{\delta} \approx \frac{1-u}{u} = u^{-1} - 1$, where u denotes the market-level unemployment rate (since ρ is small compared to worker flow rates). Strikingly, this expression mirrors our structural *micro* sensitivity except for two differences. First, the ϕ factor in the denominator is divided by $\eta < 1$, attenuating the sensitivity slightly. Second, the relevant unemployment rate u refers to the market-level average rather than the worker's idiosyncratic time in nonemployment post-separation τ. In both limits, we have $dw/db|_{\tau=1} = dw^{\text{DMP}}/db\Big|_{u=1} = 1-\phi$. For $\phi = 0.1$ (micro estimates from rent sharing), $u \approx 7\%$ and $\eta = 0.72$ (e.g., Shimer, 2005), we obtained a calibrated benchmark for the wage-benefit sensitivity of $\frac{1-0.1}{1+0.1\frac{1}{0.72}\cdot\frac{0.93}{0.07}} \approx 0.32$. Note that the u here need not coincide with the τ (or the τ^U we provide in the extended model with nonemployment without UI receipt), as the rate here is cross-sectional rather than tracking a worker after a separation (and, respectively, as some of that non-UI nonemployment state is spent out of the labor force while the model at hand only considers those workers actively searching).[47] Moreover, higher unemployment u increases the macro sensitivity almost exactly as a higher τ increases the micro sensitivity, which generalizes the implications of whether the sensitivity differs in the local unemployment rate, a prediction we test in Section 5.1. Therefore, our quantitative and structural benchmark for the wage-benefit sensitivity carries over to a macro context with equilibrium adjustment and perfectly segmented labor markets for the treatment group and the control group.

II. Stole and Zwiebel (1996) Bargaining with Multi-Worker Firms Extensions to multi-worker contexts highlight the complications that the splitting of the *inside* option entails with multi-worker firms and diminishing returns (see Jäger and Heining, 2019, for empirical evidence). We build on the derivation of the Nash wage with firm level production function $Y = n^\alpha$ in Acemoglu and Hawkins (2014) augmented with our worker-specific outside option Ω_i:[48]

[47] With $\eta = 0.5$ instead of 0.72, the sensitivity is 0.25. With $\tau = 0.05$ instead of 0.07, we have 0.25.

[48] Cahuc, Marque, and Wasmer (2008) also derive a dynamic search model with Stole and Zwiebel (1996) bargaining and heterogeneous worker groups i that may differ in their outside options b_i and derive the wage for group i as $w_i(n) = (1-\alpha)\rho N_i + \int_0^1 a^{\frac{1-\alpha}{\alpha}} F_i(na)\, da$.

$$w^{\text{MultiWorker}} = \frac{\alpha\phi}{1-\phi+\alpha\phi} \cdot x_f \cdot n_f^{\alpha-1} + (1-\phi)\Omega_i \tag{A30}$$

That is, multi-worker firm bargaining preserves the sensitivity of wages to outside options Ω.[49]

III. Representative vs. Individual Households Implementations of matching-frictional labor markets are largely either in terms of individual households with linear utility or with large households that send off households into employment with full insurance in the spirit of indivisible labor (Rogerson, 1988; Hansen, 1985), for example Merz (1995), Andolfatto (1996), or Shimer (2010). In Appendix Section D.3 we extend this setting to an individual household with nonlinear utility. Our individual household bridges these setups with the assumption of perfect capital markets (and negligibly long unemployment spells).

IV. Endogenous Separations The Nash wage is the same in models with endogenous separations among existing jobs due to idiosyncratic productivity shocks, where p is replaced with p_{it}. Inframarginal surviving matches, i.e. those that we track in the data, exhibit the same pass-through of Ω_i into wages.[50]

V. On-the-Job Search On its own, on-the-job search with a job ladder (e.g., due to heterogeneous firms or match-specific quality) need not change the wage bargaining process as long as the worker is required to give notice to the firm before engaging in bargaining with the next employer. Nonemployment then remains the outside option in wage bargaining. This tractable route is taken by for example Mortensen and Nagypal (2007) and Fujita and Ramey (2012). We discuss alternative models with competing job offers as outside options in Section 2.3. In this class of models however, new hires from nonemployment still use nonemployment as their outside option in their initial bargain, where wages thus follow our baseline model.

VI. Finite Potential Benefit Duration While a common approach is to model benefits as having infinite potential duration, its duration is finite in Austria, as we describe in Section 3. Yet, in the Austrian setting, infinite benefit duration is a particularly good approximation for initially incumbent workers because only around 20% of unemployment spells end up in benefit exhaustion (Card, Chetty, and Weber, 2007). Moreover, after UI exhaustion, eligible Austrian workers collect a follow-up UI substitute $s(b) < b$ (*Notstandshilfe*, i.e. unemployment assistance (UA)). Importantly, $s(b)$ is explicitly indexed to a worker's pre-exhaustion UIB levels and – while in many cases lower – its level shifts almost one-to-one with changes in b. This feature leaves post-UI benefits sensitive to our reforms even for UI exhausters.[51]

[49]These models also imply that rent sharing estimates from firm-specific TFP shifts x_f transferred to predict wage sensitivity to b would require an additional scaling up if $\alpha < 1$.

[50]In these models, b_i will also shift the reservation quality at which matches are formed and destroyed. Jäger, Schoefer, and Zweimüller (2018) study a large extension of potential duration of UI for older workers and document substantial separation responses of that policy, which perhaps served as a bridge into early retirement in particular for workers in declining industries. In this paper, we do not detect significant separation effects to increases in benefit *leves*, perhaps because we study younger workers.

[51]UA benefits are capped at 92% of the worker's UI benefits. Importantly, for uncapped workers, UA benefits shift 0.95 to one with the worker's UIB level. The precise formulate is $\text{UAB}_i = \min\{0.92 b_i, \max\{0, 0.95 b_i -$

Here, we extend the model to a two-tier system of finite-duration UIBs b, after which fraction α of still-jobless workers move into post-UI substitute $s(b) < b$. Denote by ζ the fraction of the unemployment spell a separator spends on UA (vs. UI). We treat ζ as the probability that a given separator moves into s (UA) or b (UB) post-separation. An initially employed worker's expected outside option is therefore $\Omega = \rho\mathbb{E}[N] = (1-\zeta)\cdot\rho N_b + \zeta\cdot\rho N_s = \zeta\left(\tau_s \alpha s + (1-\tau_s)w_s\right) + (1-\zeta)\left(\tau_b b + (1-\tau_b)w_b\right)$. With permanent types and wages $w_s < w_b$, Nash still implies identical *sensitivities* $dw_s/ds = dw_b/db$. Moreover, due to $f_s = f_b$, we have that once in a type, $\tau_s = \tau_b$.

In consequence, the wage sensitivity to benefits for the finite benefit duration is:

$$\frac{dw^{\text{finite}}}{db} = \frac{(1-\phi)\tau}{\frac{1}{1-\zeta\left(1-\alpha\frac{ds}{db}\right)} - (1-\phi)(1-\tau)} \tag{A31}$$

Using the fact that only 20% of workers exhaust their benefits and the fraction of the unemployment spell a separator spends on UA (vs. UI), we calibrate $\zeta = \frac{0.8\cdot 0+0.2\cdot 1/f}{1/f} = 0.2$, where $1/f$ denotes both expected duration remaining in nonemployment after benefit exhaustion as well as the average time at separation. A fraction $\alpha \approx 0.6$ of those workers move on to the post-UI substitute unemployment assistance. We calculate the fraction α as the share of workers who take up post-UI benefits within a 60 day window of exhausting their UI benefits; for this analysis, the sample is restricted to workers who do not take up employment in the same time window. Among those who receive them, the post-UI benefits are almost one-to-one indexed to the household's previous, actually received UI benefit level, and thus move in lock-step with benefit *changes*.[52]

As a result, the term $[1-\zeta\left(1-\alpha\frac{ds}{db}\right)] = 0.91$ provides negligible attenuation of the wage-benefit sensitivity: the wage benefit-sensitivity remains at 0.32. This is an underestimate if the workers exhausting UI have a lower job finding rate and thus a larger τ, which for that subset of workers would greatly amplify the sensitivity: setting $\tau = 0.15$ rather than 0.104 will restore $dw/db = 0.4$ for those workers.

In other words, since an initially employed Austrian worker has a low probability of benefit exhaustion and, moreover, post-UI benefits are indexed to UI benefits our design is robust to finite benefit duration. Perhaps this fact also explains why we also do not find wage effects from potential benefit duration extensions in Section 5.1 and Appendix F. We have also not found evidence that workers with particularly high potential benefit durations exhibit different wage sensitivity to the unemployment benefit level.

VII. UI Wait Periods for Unilateral Quitters Austria has broad UI eligibility that encompasses even quitters. There is however a 28-day wait period, after which UI recipients enjoy full potential benefit duration (i.e. for 28 more calendar days than their peers receiving UI immediately). We evaluate this consideration in two steps. First, we define a probability $1-\upsilon$

Spousal Earnings$_i$ + Dependent Allowances$_i$}}. Due to the spousal earnings means test, not all workers are eligible for UA. For 1990, Lalive, Van Ours, and Zweimüller (2006) report that median UA was about 70 % of the median UIB. Based on data from 2004, Card, Chetty, and Weber (2007) gauge the average UA at 38 % of UIB for the typical job loser.

[52] The law stipulates that post-UI benefits move with a slope of 0.92 along with previous UI benefits. There are additional additive components, e.g., benefits for dependents and reductions for other income, and the post-UI benefit level is capped at 0.95 times previous UI benefits. For the calibration, we pick the middle point between 0.95 and 0.92 and assume $ds/db \approx 0.935$.

that a bargaining progress breakdown leaves the worker eligible for UI whereas at probability υ leaves the worker ineligible (for any social insurance program). Ineligible workers wait 28 days until they receive UI, implying that $z^{\text{ineligigble}} = z^{\text{eligigible}} - b$ for initial period of nonemployment. In discrete time, $N^{in} = z^{in} + f^m \beta E^{in} + (1 - f^m)\beta N^{el}$, such that:

$$\mathbb{E}[N] = (1-\upsilon)N^{el} + \upsilon N^{in} \tag{A32}$$

$$= (1-\upsilon)N^{el} + \upsilon \left[z^{in} + (1-f^m)\beta N^{el} + f^m \beta E^{in}\right] \tag{A33}$$

$$= N^{el}\left(1 - \upsilon\left[1-(1-f^m)\beta\right]\right) + \upsilon\left[z^{in} + f^m\beta E^{in}\right] \tag{A34}$$

The effect of b on the expected outside option is bounded from below by an attenuation factor times our previously derived sensitivity of N to b, due to $dE^{in}/db \geq 0$ and $dz^{in}/db \geq 0$:

$$\frac{d\mathbb{E}[N]}{db} \geq \frac{dN^{el}}{db} \underbrace{\left[1 - \upsilon + \upsilon(1-f^m)\beta\right]}_{\approx 1-\upsilon f^m} \tag{A35}$$

where $\beta = 0.9965 \approx 1$ at monthly frequencies. Therefore, the wage-benefit sensitivity is at least:

$$\frac{dw^{\text{Limited Elig.}}}{db} \geq (1-\phi) \cdot \frac{1-\upsilon f_m}{1+\phi\left(\tau^{-1}-1\right)} \tag{A36}$$

Calibrating the bracketed attenuation factor with $f = 0.12$ (incorporating a monthly $\beta = 0.9965$ will not change the result) implies that the attenuation is by 0.88 *even if all separations were to go into nonemployment with initial ineligibility* (i.e. $\nu = 1$). That is, since so many nonemployment spells go beyond one month, this institutional feature has limited effects on the predicted wage-benefit sensitivity.[53] This benchmark thereby also evaluates also delayed take-up for any reason even among the immediately eligible. In reality, most separations into nonemployment in Austria entail UI eligibility such that ν is closer to zero than to one, greatly limiting attenuation.

VIII. Wage Stickiness Rather than Period-by-Period Bargaining Real-world wage renegotiations may occur infrequently on the job, e.g. arrive at rate γ. Then, the measured wage response to a (permanent) shift in db is increasing in time-since-reform dt, and on average:

$$\mathbb{E}\left[\frac{dw^{\text{sticky,dt}}}{db}\right] = \left(1-e^{-\gamma dt}\right) \cdot \frac{1-\phi}{1+\phi\left(\tau^{-1}-1\right)} + e^{-\gamma dt} \cdot 0 \tag{A37}$$

Empirically, we approach this aspect from three angles. First, we start with observing average wage earnings in the first full calendar year after the reform takes effect.[54] We then additionally investigate earnings in the calendar year in the subsequent year, allowing two years for wage pass-through, whereas existing evidence on wage stickiness suggests half of wages to get reset within one year.[55] Second, we consider wage effects in *new jobs*, for workers switching jobs with

[53]This attenuation is further slightly reduced with finite PBD because the one-month delay does not reduce subsequent PDB, such that at probability $(1-f^m)^{\text{PBD Months}}$, the worker "buys back" the first month (valued as $b - \alpha s$, i.e. the premium over UI substitute s adjusted for eligibility probability α.

[54]An exception is the 1989 reform, which takes effect mid-year.

[55]See, e.g., Barattieri, Basu, and Gottschalk (2014) for the United States, and Sigurdsson and Sigurdardottir (2016) for Iceland. Finally, the evidence on inside-option rent sharing documents same-year wage effects for incumbent workers.

or without unemployment spells in between, where we follow the standard assumption that new jobs get to set wages initially in a flexible way. Third, we sort jobs (firms) by the usual degree of wage volatility, essentially by an empirical proxy for γ, and investigate heterogenous wage effects.

IX. Taxation Our bargaining setup so far sidesteps the tax system, but the results would carry over to a model in which both the firm and the worker face a (linear) income tax, and bargain over net surpluses by means of setting a gross wage. Taking into account taxation, however, *would increase* the effect of our UIB variation on wages. In Austria, benefits are not taxed, whereas wages and profits are. If the employer's and the worker's income taxes are approximately taxed by the same τ, then changes in *net* benefits b enter the worker's outside option relatively as $\frac{b}{1-\tau}$. For $\tau \approx 0.3$, accounting for the tax system would therefore *amplify* the predicted sensitivity of wages to b by $\frac{1}{1-0.3} \approx 1.43$ for any given ϕ. Analogously, a given wage response will, structurally interpreted in a model of Nash bargaining with nonemployment as the outside option, would for example imply 1.43 as large a worker bargaining power parameter. In an empirical robustness check in Appendix Table A.7 (graphical analysis in Appendix Figure A.19), we further report specifications in which we scale up benefits (and benefit changes) to correspond to (hypothetical) gross benefit changes so that all calculations occur in terms of gross units. The results of the robustness check lead to the same conclusion as our main results and also reveal an insensitivity of wages to (gross) benefit changes.

X. Bounded Rationality: Myopia Our framework assumes that all workers and firms are rational in particular about their expectations about the nonemployment state. However, myopic agents may discount the future by more than the social planner would on their behalf. In our model, this consideration would most simply be nested with a larger ρ. Since the initial post-separation state is unemployment, $\frac{\partial \tau}{\partial \rho} > 0$, implying that the agents put more weight on b, amplifying the effect on the wage-benefit sensitivity.

XI. Bounded rationality: bounded rationality and k-level thinking Other deviations from the fully rational benchmark may however attenuate the effect. The wage sensitivity consists of the direct effect as well as expectations about wage responses in subsequent jobs. The latter feedback effect is a strong ingredient into the theoretical sensitivity of wages to benefits and hard-wired into the model. A promising theory to attenuate the effect will therefore attenuate the feedback effect of re-employment wages into the wage bargain at hand. Perhaps $k = 1$-level thinking may provide such a rationalization: agents act while ignoring equilibrium effects because they only consider one iteration of the equilibrium adjustment, but not the reemployment wage adjustment. The resulting wage-benefit sensitivity would then be limited to the direct effect:

$$\frac{dw^{(k\,=\,1)}}{db} = (1-\phi)\tau \tag{A38}$$

Calibrating the ($k = 1$) sensitivity to $\tau = 0.1$ and $\phi = 0.1$ would return a smaller sensitivity of 0.09 on average. Larger effects would emerge with $k > 1$. However, the sensitivity is still increasing in τ, linearly so now. In Section 4.4, we test whether workers with larger τ (predicted time on UI post-separation) have larger pass-through, and do not find evidence for a slope, in contrast to the prediction from even ($k = 1$)-level thinking.

D.3 Alternative Wage Setting Model: Bilateral Nash Bargaining Between an Individual Household with a Potentially Multi-Worker Firm

The model presented here forms the basis for the additional model variants presented in Section D.2. Here we generalize the structural wage equation by a variety of dimensions, starting with a bilateral bargaining between a worker and a multi-worker firm, long-term jobs and non-linear utility.

Hiring Costs and Ex-Post Job Surplus Employment relationships carry strictly positive joint job surplus because of hiring costs, $c'(H) > 0$, $c(0) = 0$, which are sunk before bargaining. In consequence, both the worker and the firm would strictly prefer to form the match (for an efficiently set wage) than part ways.

Household Labor is indivisible and hours are normalized to one. In a given period s, the household is either employed or unemployed ($e_s \in \{0, 1\}$). There is no direct labor supply channel; workers accept job opportunities when they emerge. When employed, the worker earns wage w_s. The employed household incurs labor disutility γ. When unemployed, the worker collects unemployment insurance benefits b. With probability f, the worker finds a job and moves into employment (and wage bargaining) next period. With probability $1 - \delta$, employed job seekers lose their jobs and become unemployed. Households can borrow and save at interest rate r, fulfilling a lifetime budget constraint.[56] Households own firms and collect capital income in the form of dividends d_t.

$$V^H(e_t) = \max_{c_t} \mathbb{E}_t \sum_{s=t}^{\infty} \beta^{s-t} u(c_s) - \gamma \cdot \mathbb{I}(e_s = 1) \tag{A39}$$

$$\text{s.t.} \quad \mathbb{E}_t \sum_{s=t}^{\infty} \frac{c_s}{(1+r)^{s-t}} \leq \mathbb{E}_t \sum_{s=t}^{\infty} \frac{\mathbb{I}(e_s = 1) \cdot w_s + \mathbb{I}(e_s = 0) \cdot b + d_s}{(1+r)^{s-t}} + a_t \tag{A40}$$

$$\mathbb{E}_t[e_{s+1}|e_s = 1] = 1 - \delta \ \ \forall s \tag{A41}$$

$$\mathbb{E}_t[e_{s+1}|e_s = 0] = f \ \ \forall s \tag{A42}$$

The household's problem can be cast in dynamic programming in familiar form associated with search and matching models:

$$U_t = \max_{c_t} u(c_t|e_t = 0) + (1 - f)\beta\mathbb{E}_t U_{t+1} + f\beta\mathbb{E}_t \widetilde{W}_{t+1} \tag{A43}$$

$$W_t = \max_{c_t} u(c_t|e_t = 1) - \gamma + (1 - \delta)\beta\mathbb{E}_t W_{t+1} + \delta\beta\mathbb{E}_t U_{t+1} \tag{A44}$$

where U_t denotes the value function of a worker that is currently unemployed ($e_t = 0$) and W_t for the employed worker ($e_t = 1$). $\widetilde{W}_{t+1}$ denotes a potential subsequent job. The household's benefit from employment, at a given wage w, is pinned down by the difference in income, net of the

[56] Due to the absence of moral hazard in job search and due to the law of large numbers on the part of the unmodelled lenders, the expected lifetime earnings do not complicate the borrowing potential of households. Since average unemployment spells are short in nature (on the order of 45% at the monthly rate in the US), we abstract from shifts in lifetime earnings in shifting lifetime wealth and therefore the multiplier on the budget constraint. Therefore, we assume that the budget constraint multiplier is approximately independent of the employment status, $\lambda(e = 0) \approx \lambda(e = 1)$.

disutility of labor, plus the shift in the continuation value (where we here simplify the setting to a lifecycle budget constraint that features a λ unaffected by the stochasticity of separations and reemployment; a similar setting would emerge with complete markets or through a representative household):

$$W_t(w) - U_t = \lambda(w-b) - \gamma + (1-\delta)\cdot\beta\mathbb{E}_t(W_{t+1} - U_{t+1}) - f\cdot\beta\mathbb{E}_t(\widetilde{W}_{t+1} - U_{t+1}) \tag{A45}$$

Firm The multi-worker firm, facing a competitive product and capital market, employs N_t workers in long-term jobs and rents capital K_t at rate R_t. Capital rentals are made given wages after bargaining.[57] Production follows constant returns with all labor being of the same type and thus perfect substitutes, which together with rented capital implies linear production in labor, avoiding multi-worker bargaining complications. Each period, a fraction $1-\delta$ workers separate into unemployment exogenously, whereas the firm hires H_t workers at cost $c(H_t)$. Employment follows a law of motion as a constraint in the firm's problem. The firm maximizes the present value of payouts to the households (stockholders):

$$V_t^F(N_t) = \lambda\mathbb{E}_t \max_{H_t,K_t} \sum_{s=t}^{\infty} \beta^{s-t}[F(K_t,N_t) - w_tN_t - R_tK_t - c(H_t)] \tag{A46}$$

$$\text{s.t.} \quad N_{t+1} = (1-\delta)N_t + H_t \tag{A47}$$

The firm's problem can be cast in dynamic programming in familiar form associated with search and matching models; where the firm's state variable is the employment level:

$$V_t^F(N_t) = \max_{H_t,K_t} \left\{\lambda[F(K_t,N_t) - w_tN_t - R_tK_t - c(H_t)] + \beta V_{t+1}^F(N_{t+1})\right\} \tag{A48}$$

$$\text{s.t.} \quad N_{t+1} = (1-\delta)N_t + H_t \tag{A49}$$

The firm's input demand (capital rentals and hiring) is described by the following first-order conditions and the envelope condition for μ_t, the shadow value on the law of motion for employment, pinned down by the envelope condition:

$$F_K(N_t,K_t) = R_t \tag{A50}$$

$$c'(H_t) = \beta\mathbb{E}_t\frac{\partial V_{t+1}(N_{t+1})}{\partial N_{t+1}} \tag{A51}$$

$$\frac{\partial V_t^F(N_t)}{\partial N_t} = \lambda[f_{\tilde{N}}(K_t,N_t) - w_t] + (1-\delta)\beta\mathbb{E}_t\frac{\partial V_{t+1}^F(N_{t+1})}{\partial N_{t+1}} \tag{A52}$$

$$\Rightarrow c'(H_t) = \beta\mathbb{E}_t\left[f_{\tilde{N}}(K_{t+1},N_{t+1}) - w_{t+1} + (1-\delta)c'(H_{t+1})\right] \tag{A53}$$

These conditions describe input demand *given* the wages firms expect to pay at the bargaining stage. The firm's value of employing an incremental individual worker (hired last period and becoming productive, and thus bargaining, in period t) is:

$$\Delta V_t^F(N_t,w) = \lambda[f_{\tilde{N}}(K_t,N_t) - w] + (1-\delta)\beta {V_{t+1}^F}'(N_{t+1}) \tag{A54}$$

[57] Rental of capital inputs and this timing conventions precludes the complication of potential investment holdup associated with bargaining.

Nash Wage Bargaining Nash bargaining solves the following joint maximization problem, by which the worker and the firm pick a Nash wage w^N that maximizes the geometric sum of net-of-wage surplus of the match to the worker $W(w)-U$ and of the firm $\Delta V_t(N_{t-1}, w)$, weighted by exponents ϕ and $1-\phi$:

$$w^N = \arg\max_w \,(W(w)-U)^\phi \times \left(\Delta V^F(N_t, w)\right)^{1-\phi} \tag{A55}$$

$$\Rightarrow W(w^N) = U + \phi \underbrace{(\Delta V^F(N_t, w) + W(w^N) - U)}_{\text{Job surplus}} \tag{A56}$$

That is, the employed worker receives her outside option U plus share ϕ of the job surplus: the sum of the parties' inside options net of their outside options. Worker bargaining power parameter ϕ guides the share of the surplus that the employed worker receives, on top of her outside option. Next, we solve for the Nash wage w^N that implements this surplus split.

The model recognizes the long-term nature of jobs.[58] Wages then not only reflect current conditions but also expectations about future inside and outside values, through the continuation values. An important implication of Nash bargaining to apply also in subsequent period, renders the Nash wage identical to the myopic thought experiment except for a continuation term:[59]

$$w^N = \phi f_{\tilde{N}}(K_t, N_t) + (1-\phi)(1-\beta)\frac{U}{\lambda} \tag{A57}$$

The condition mirrors the continuous-time conditions in the main text, where $1-\beta \approx \rho$ and U/λ corresponds to N.

D.4 Alternative Bargaining Model: A Simple Version of Credible Bargaining (Hall and Milgrom, 2008)

Summary Consider the firm's optimal strategy: to offer the worker her reservation wage $\underline{w}$, given by indifference between accepting and rejecting with her optimal counteroffer – equal to the firm's reservation wage $\overline{w}$ (with discount factor β, exogenous break-down probability s, and firm's cost of delay γ): $\frac{\underline{w}+\beta\delta N}{1-\beta(1-\delta)} = z + (1-s)\beta\frac{\overline{w}+\beta\delta N}{1-\beta(1-\delta)} + s\beta N$. The firm's offer $\underline{w} = \frac{(1-\beta(1-\delta))z+(1-s)\beta\left[(1-\beta(1-\delta))\gamma+p(1-\beta(1-s))\right]}{1-\beta^2(1-s)^2} + \frac{\beta(s-\delta)\times(1-\beta)N}{1-\beta^2(1-s)^2}$ is insensitive to N if $s=\delta$.

Derivation We describe a simple version of the credible bargaining protocol proposed by Hall and Milgrom (2008) that relies on alternating offers. The model remains empirically untested but has been favored for its macroeconomic upside: it generates endogenous rigidity to shocks and therefore amplifies employment fluctuations (see, e.g., Christiano, Eichenbaum, and Trabandt, 2016; Hall, 2017). Specifically, "the credible bargaining equilibrium is less sensitive to conditions in the outside market" (Hall, 2017, p. 310).

[58] We consider period-by-period bargaining in the main part of the this exposition.

[59] The derivation recognizes that $\phi\beta\mathbb{E}_t(W_{t+1}-U_{t+1}) = (1-\phi)\beta\mathbb{E}_t {V_{t+1}^F}'(N_t)$ by Nash bargaining in $t+1$ *in the job at hand*. In consequence, the $(1-\delta)$-weighted continuation terms cancel out:

$$(1-\phi)\left[\lambda(w^N-b)-\gamma+(1-\delta)\cdot\beta\mathbb{E}_t(W_{t+1}-U_{t+1})+f\cdot\beta\mathbb{E}_t(\widetilde{W}_{t+1}-U_{t+1})\right] = \phi\left[\lambda[f_{\tilde{N}}-w^N]+(1-\delta)\beta\mathbb{E}_t {V_{t+1}^F}'(N_t)\right]$$

The firm and the worker make alternating wage offers. In between bargaining rounds, the firm incurs a delay cost γ. Importantly, in our discussion here we allow the worker's flow utility z to differ from the flow unemployment benefits b, unlike in the existing treatments in macroeconomic applications of this bargaining protocol. After all, for an employed worker z may capture leisure, disutility from bargaining, the old, still-prevailing wage, and so forth. Moreover z may accordingly differ between an unemployed negotiator entering a new job, and an already-employed job seeker potentially seeking to renegotiate.

In between rebargaining rounds, the match may dissolve. The probability of this bargaining-stage separation is s, which may be different from the probability of standard exogenous job destruction during production, δ. N will therefore enter the problem either through s or δ, with importantly opposite effects on the worker's reservation wage, as we show below.

Inside Values Preserving unemployment value N for the worker and a zero for the firm's vacancy value due to free entry, we define the inside value of the worker $W(w)$ and the firm $J(w)$ (where we have set vacancy value $V = 0$ due to free entry):

$$E(w) = \frac{w + \beta\delta N}{1 - \beta(1-\delta)} \tag{A58}$$

$$J(w) = \frac{p - w}{1 - \beta(1-\delta)} \tag{A59}$$

Strategies for Wage Offers The optimal strategies are described by reservation wages. The worker's reservation wage is $\underline{w}$, and the firm's reservation wage is $\overline{w} > \underline{w}$, which we have yet to derive. When it is the worker's (firm's) turn to make an offer, she (it) will offer $\overline{w}$ ($\underline{w}$), leaving the firm (worker) indifferent between rejecting and rebargaining.

Worker's Strategy: Offer Firm's Reservation Wage The firm's indifference condition defines the worker's strategy, to offer the firm its reservation wage $\overline{w}$:

$$\frac{p - \overline{w}}{1 - \beta(1-\delta)} = -\gamma + \beta(1-s)\frac{p - \underline{w}}{1 - \beta(1-\delta)} \tag{A60}$$

$$p - \overline{w} = -(1 - \beta(1-\delta))\gamma + \beta(1-s))(p - \underline{w}) \tag{A61}$$

$$\overline{w} = (1 - \beta(1-\delta))\gamma + \beta(1-s))\underline{w} - p(1 - \beta(1-s)) \tag{A62}$$

Firm's Strategy: Offer Worker's Reservation Wage Analogously, the firm offers the worker her reservation wage. The definition of the reservation wage is such that the worker is rendered indifferent between $\underline{w}$ and waiting a period to make her own offer to the firm – which in turn will optimally equal the firm's reservation wage $\overline{w}$:

$$\frac{\underline{w} + \beta\delta N}{1 - \beta(1-\delta)} = z + (1-s)\beta\frac{\overline{w} + \beta\delta N}{1 - \beta(1-\delta)} + s\beta N \tag{A63}$$

For $s = 1$, i.e. rejection by the worker results in unemployment, the reservation wage is equal to z, the payoff while bargaining, plus an "amortized" (hence: flow) value of unemployment U:

$$\Leftrightarrow \underline{w} = (1 - \beta(1-\delta))z + \beta(1 - \beta(1-\delta))N \tag{A64}$$

The worker's reservation wage is maximally sensitive to N if a rejected offer indeed results in unemployment, i.e. for $s = 1$. In fact, if the time period is short, the reservation wage is the payoff of not accepting the offer (and thus forgoing z this period), and the excess of that going forward compared to unemployment.

More generally, we can rearrange the terms to isolate the present value of wages promised by the firm to leave the worker indifferent:

$$\frac{\underline{w}}{1-\beta(1-\delta)} = \overbrace{z}^{\text{payoff while barg.}} + (1-s)\,\beta \overbrace{\frac{\overline{w}}{1-\beta(1-\delta)}}^{\text{Follow-up offer}} + \beta \overbrace{(s-\delta)\frac{1-\beta}{1-\beta(1-\delta)}N}^{\text{Rel. unemp. risk: bargaining vs. producing}} \tag{A65}$$

$$\Leftrightarrow \underline{w} = (1-\beta(1-\delta))z + (1-s)\beta\overline{w} + \beta(s-\delta)(1-\beta)N \tag{A66}$$

Given N, we can solve for worker and firm reservation wages. The worker's reservation wage (and the optimal wage the firm would offer the worker) is:

$$\underline{w} = \frac{(1-\beta(1-\delta))z + (1-s)\beta\left[(1-\beta(1-\delta))\gamma + p(1-\beta(1-s))\right]}{1-\beta^2(1-s)^2} + \frac{\beta(s-\delta)}{1-\beta^2(1-s)^2} \times (1-\beta)N \tag{A67}$$

The wage insensitivity to the nonemployment value $(1-\beta)N$ (ρN in our continuous time setting) is:

$$\frac{d\underline{w}}{d(1-\beta)N} = \frac{\beta(s-\delta)}{1-\beta^2(1-s)^2} \tag{A68}$$

Therefore, for $s = \delta$, the wage is insensitive to the nonemployment value. And still, the model can still accommodate small rent sharing coefficients:

$$\frac{d\underline{w}}{dp} = \frac{(1-s)\beta(1-\beta(1-s))}{1-\beta^2(1-s)^2} \tag{A69}$$

For $s = \delta \approx 0$, this becomes very close to zero:

$$\left.\frac{d\underline{w}}{dp}\right|_{s=\delta\approx 0} \approx \beta\frac{1-\beta}{1+\beta^2} \tag{A70}$$

Therefore, the protocol can accommodate wages that are, in the same calibration, insensitive to outside options including the nonemployment value, and have small wage responses to inside option shifts such as rent sharing (e.g., for small s).

The Role of s vs. δ in Mediating the Effect of N on Worker Reservation Wages As in the standard Nash model, N denotes both the outside option of the worker in case of bargaining breakdown during the bargaining process (weighted by s) as well as the value of an exogenous job destruction (arriving with probability δ). The net effect of U on the worker's reservation wage $\underline{w}$ depends on the relative size of s and δ in the alternating offer bargaining protocol.

A useful benchmark is $s = \delta$. Here, the worker is exposed to N with the same probability –

whether she decides to reject the firm's offer to get a chance to make her counteroffer (where with probability s bargaining breaks down and she becomes unemployed), or whether she accepts the current offer – when therefore production begins a period earlier (which exposes her job destruction probability δ, and thus she puts a δ weight on N one period earlier). In this knife-edge case, the worker's reservation wage $\underline{w}$ turns *completely insensitive to* N *– and thus* b, and is only driven by the while-bargaining flow utility z (which need not contain b) and the (present value of the) wage gain resulting from getting the chance to make the (in subgame perfect equilibrium expected to be accepted) counteroffer, $\overline{w}$.

Calibrating AOB to $\delta = s$ *could in principle* generate wage insensitivity to N (and thus b, assuming that $z \neq b$ for an incumbent worker). However, for cases where δ is small relative to s, AOB may feature high sensitivity of $\underline{w}$ to shifts in N and thus b. For bilateral negotiations, perhaps $s \approx 1$ with $\delta < 5\%$ may not be a poor approximation of the real world, for example.

Whether $s \approx \delta$ is empirically realistic as such is difficult to assess because independently calibrating s directly to empirical evidence is not straightforward.[60] For example, Hall (2017) calibrates $s = 0.013$ and $\delta = 0.0345$, which here would lead worker reservation wages to *fall* when N were to increase ceteris paribus. Conversely, Hall and Milgrom (2008) sets $\delta = 0.0014$ and $s = 0.0055$ at the daily frequency, which in our version of the AOB model leads increases in N to *increase* wages (reservation wages of the worker) ceteris paribus.

The Role of z vs. b While we intentionally define z (the flow utility of the worker while bargaining, perhaps not containing b for, e.g., an incumbent worker) separately from b (the nonemployment payoff, contained in N), the original authors and the follow-up literature (see, e.g., Hall and Milgrom, 2008; Christiano, Eichenbaum, and Trabandt, 2016; Hall, 2017) set both to be the same, and thus explicitly include unemployment benefits in $z = b$. But these authors are interested in new hires and their wage responses; our setting also studied incumbent workers, whose z is unlikely to contain b but rather reflect a default, previous wage. Somewhat in tension to the model however, we do not find evidence for new hires' out of unemployment to exhibit large wage sensitivity.

D.5 Alternative Wage Setting Model: Wage Posting Models and UI

We here add additional formal intuitions for how a UIB increase in a wage posting model will play out.

Reservation Wage of a Worker The McCall (1970) reservation wage forms the cornerstone of the equilibrium wage distribution in the following sense. It is $R = b + (\lambda_U - \lambda_E)\int_R^{\overline{w}}(w - R)f(w)dw$, and so $\frac{dR}{db} = [1 + (\lambda_U - \lambda_E)\int_R^{\overline{w}} f(w)dw - (\lambda_U - \lambda_E)\int_R^{\overline{w}}(w - R)\frac{df(w)}{dR}dw]^{-1}$, where λ_E ((λ_U) is the job offer arrival rate for the employed (unemployed) workers. A useful benchmark is $\lambda_E = \lambda_U$, for which $dR/db = 1$, mechanically shifting the nonemployment payoff. Away from $\lambda_E = \lambda_U$, R responses also affect the opportunity cost of continuing search by accepting a job, as well as through equilibrium adjustments in $F(w)$, feeding back into the opportunity cost.

[60]For example, in a situation with multiple applicants, s from the perspective of the worker should capture also the risk of losing out to the next applicant, with higher probability s than the incumbent worker would worry about being displaced by a colleague or get high with a job destruction shock δ. This would suggest that $s >> \delta$.

Ripple Effects Throughout the Wage Distribution We now describe the ripple effects originating from the UIB effects in some more formality. In the model of homogeneity, wage policies are an equilibrium of mixed (iso-expected-profit) strategies (with no mass points), characterized by $\underline{w} = R$, $\overline{w} = [1 - \left(\frac{\delta}{\delta+\lambda_E}\right)^2]\tilde{p} + (\frac{\delta}{\delta+\lambda_E})^2 R$, and $F(w) = (\frac{\delta+\lambda_E}{\lambda_E})(1 - \sqrt{\frac{\tilde{p}-w}{\tilde{p}-R}})$. While the bottom wage exhibits the one-to-one effect, the top wage sensitivity is $\frac{d\overline{w}}{dR} = (\frac{\delta}{\lambda_E+\delta})^2$, i.e. the unemployment rate, squared, for $\lambda_E = \lambda_U$. In the model with heterogeneity, firms differ in productivity $p \in [\underline{p}, \overline{p}]$. Type p's wage policy $w(p)$ is (see Postel-Vinay and Robin, 2006): $w(p) = p - \int_R^p \frac{\lambda_E+\delta(1-\Gamma(p))}{\lambda_E+\delta(1-\Gamma(x))} dx \Rightarrow \frac{dw(p)}{dR} = 1 - \frac{\delta}{\lambda_E+\delta}\Gamma(p)$, where $\Gamma(p) = F(w(p))$ is the (offer-weighted) CDF, which depends on recruitment costs and is not generally characterized in closed form. The lower bound of the integral ($\Gamma(\underline{p}) = F(w(\underline{p})) = 0$) makes clear that the least productive active firm pays R and earns zero profit. To see the ripple effects, consider the top wage ($\Gamma(\overline{p}) = 1$), which responds the *least*. Ballparking to $\lambda_E = \lambda_U$, implies that $\frac{\delta}{\lambda_E+\delta} = \frac{\delta}{\lambda_U+\delta}$ is the unemployment rate. Then, the *lowest* wage-UIB sensitivity (in the top) is one minus the unemployment rate. In the full model, $\Gamma(p)$ adjusts due to worker reallocation and p-specific recruitment responses. So unless $\lambda_E = \lambda_U$, feedback effects between $\Gamma(p)$ and R emerge, amplifying or attenuating the effects.

E Sample and Variable Construction

E.1 Construction of Sample

Begin with all individuals aged 25-54 with non-missing earnings. To isolate the part of the income distribution we include for each reform sample, we use the idea of treatment and control groups. The distinctions play no further role in our main empirical analysis, however. Once we have defined the treatment and control regions, we restrict the sample to workers who are employed in each of the 12 months of the base year (reform and placebo).

E.1.1 Treatment

The treatment region is defined as the percentile earnings range in the base year where the predicted benefit change is large and positive.

- Identify the average predicted UIB change for each percentile. See Section E.4.
- For the 1976 reform, the lower bound of treatment is one percentile above the first percentile (the ASSD minimum). We drop the first 1 1/8th percentile because earnings growth is very volatile here. The upper bound is the highest percentile under the 12th percentile that experiences a predicted benefit change greater than 1% of earnings.[61]
- For the 1985 reform, the lower bound of treatment is the percentile at which the average predicted benefit change is more than 0.5% of earnings. The upper bound of treatment is 3 percentiles below the reform-specific adjusted ASSD cap (above which earnings earnings are censored, so we could not accurately measure wage effects for these individuals; our results are quantitatively unchanged if with lower values for this upper limit).
- For the 1989 reform, the lower bound of treatment is the lowest percentile at which the predicted benefit change is greater than 2% of earnings. This is because at the lowest end of the income distribution, there was not a very large change. We then drop an additional 1 1/8th percentile because the earnings growth was very volatile. The upper bound is the highest percentile at which the predicted RR change was greater than 0.5% of earnings.
- For the 2001 reform, the lower bound of treatment is the percentile at which 10,000 ATS falls in 2000. The upper bound is the percentile at which 20,000 ATS falls in 2000.

E.1.2 Control

The control region is chosen as the percentile range in the base year closest to the treatment earnings range but received no change/a very small change in predicted benefits.

- Calculate the range of the treatment region, i.e. the number of percentiles included in the treatment region.
- For the 1976, 1989, and 2001 reforms, the lower bound of the control region corresponds to the upper bound of the treatment region. Then we add the treatment range, net of one eighth of a percentile, to the lower bound of the control region to indicate the upper bound of the control region.

[61] It must be the highest under the 12th percentile because there is also a cap extension at the top of the income distribution, and choosing the 12th percentile avoids the cap comfortably.

- For the 1985 reform, take the difference between the averaged predicted UIB change in the base year $r-1$ and three years before, $r-4$, for each percentile. Call this "excess db/w." This is an effort to make sure there is a region in the reform sample that has not been treated recently in the base year. It is 11 percentiles in 1984. Find the highest percentile for which this difference is 0. This is the upper bound of the control region. Then subtract the treatment region range from the upper bound of control to get the lower bound of control. In our main specification, we also include the "intermediate region" between the treatment and control region in our estimation sample for transparency; and we also check that our results are quantitatively robust to excluding this region.
- For the 2001 reform, the lower bound of control is the upper bound of treatment. Then add the treatment range to the lower bound of control to get the upper bound of control.

E.1.3 Percentile Ranges of Reform Samples

While we describe sample restrictions and our empirical framework in percentiles, we operationalize the benefit aggregation (for placebo assignment) as well as the sample construction using *eighths of percentiles*, to create fine-grained benefit levels. This is especially useful in the small reform samples. We report the cutoffs at the level of eighths of percentile in parentheses:

- **1976 Reform**
  - Treatment: 1st (1.75) to 7th percentile (7.00)
  - Control: 7th (7.00) to 13th percentile (12.125)
- **1985 Reform**
  - Treatment: 61st (61.125) to 86th percentile (86.75)
  - Control: 24th (24.25) to 49th percentile (49.875)
  - Intermediate previously treated region (included for transparency but robust to excluding): 50th (50.00) to 61st percentile (61.00)
- **1989 Reform**
  - Treatment: 1st (2.5) to 19th percentile (19.75)
  - Control: 20th (19.75) to 38th percentile (36.875)
- **2001 Reform**
  - Treatment: 8th (7.875) to 32nd percentile (32.25)
  - Control: 32nd (32.25) to 56th percentile (56.5)

E.2 Average Daily Earnings Construction

We construct our average daily earnings measure as follows:

1. For each individual-firm-year observation (even across multiple spells like recall), we calculate the total earnings the individual received from that firm during the year divided by the total number of days worked at that firm. These earnings include supplementary payments such as 13th or 14th month wage payments and extra vacation payments.

2. For each month where the individual is employed by at least one firm, we assign the individual the "daily earnings" from the firm at which the individual is employed for the longest during that year and employed at that month.

3. We calculate the average daily earnings as the average of these monthly earnings measures across all months the individual is employed by at least one firm.

E.3 Earnings Base for Unemployment Benefit Determination Throughout our Sample Period

From 1977 until 1987, the earnings base for calculating unemployment benefits are generally the earnings in the last full month of employment before the beginning of an unemployment spell (§ 21 (1) *Arbeitslosenversicherungsgesetz* 1977). Importantly, Austrian wage contracts are structured to pay out 14 instead of 12 monthly salaries, with the two additional ones typically paid out at the beginning of the summer and at the end of the year, respectively. These additional payments are proportionally factored into and added to the earnings in the last four weeks before the beginning of an unemployment spell to calculate unemployment benefits (§ 21 (2) Arbeitslosenversicherungsgesetz 1977). To illustrate, someone with constant monthly earnings of ATS 10,000 would be paid an annual salary of ATS 140,000. Unemployment benefits would be calculated based on monthly earnings of ATS 11,667 based on the monthly earnings of ATS 10,000 plus 1/12 of the two additional bonus payments (ATS 10,000 * 2 / 12 = ATS 1,667).

A reform in 1987 changed the calculation period from the last month before unemployment to the last six months before unemployment, while still factoring in the 13th and 14th monthly salary proportionally.

A 1996 reform then changed the calculation more substantially by using last year's earnings for unemployment spells beginning after June 30 of a given year and the earnings in the second to last year for spells beginning before June 30. The 1996 reform left the treatment of the 13th and 14th salaries unchanged.

An additional important feature of the Austrian unemployment insurance system is that times of nonemployment (*Beschäftigungslosigkeit*) are exempt from the calculation of average earnings (Art. 2 §21 *Arbeitslosenversicherungsgesetz*). As a consequence, average earnings for calculation of UI benefits of those who experienced a nonemployment spell in the relevant calculation time period are based on a division of total earnings by the actual days of employment in the relevant time window (multiplied by 30 to arrive at a monthly number) rather than the total calendar time of the time window.[62]

Sources The laws are contained in the respectively updated versions of § 21 of the Unemployment Insurance Act (*Arbeitslosenversicherungsgesetz*, ASVG).

[62] This is in contrast to the US setting where spells of unemployment potentially lower earnings and thus subsequent unemployment benefits. In Massachusetts, for example, UI benefits are calculated based on the average weekly earnings in the two out of the last four quarters with the highest earnings. The earnings in those two quarters get divided by 26 to arrive at a weekly average regardless of actual time in employment. Holding wages while employed constant, nonemployment periods can thus lower average earnings and thus UI benefits–unlike in the Austrian setting. Source: `https://www.mass.gov/info-details/how-your-unemployment-benefits-are-determined`.

E.4 Calculation of Predicted Benefits

The crucial ingredient for our strategy to use shifts in the benefit schedule is the correct measurement of the income concept used by the UI system to assign *employed* workers the benefit they would receive conditional on a separation leading to nonemployment.

This step requires a review of the relevant earnings concept for UIB determination. Two of our four reforms we study occurred before 1987, when the earnings in the last month of full employment were the earnings concept. In 1989, the earnings concept referred the average earnings in the last six months. In our identification strategy for these reforms, we assign an employed worker her *predicted* contemporaneous earnings to assign her a benefit level.

To calculate predicted benefits, we rely on a purpose-built calculator of unemployment benefits in Austria. Through 2000, UI benefits were calculated using a table (*Lohnklassentabelle*) based on the earnings concepts outlined in Section E.3. The benefit table and the formula that replaced it in 2001 can be found in § 21, Section 3, of the Unemployment Insurance Act and reports the earnings concepts at the daily (later monthly) level.

We collect all changes to the benefit table from 1972 through 2000 and the 2001 benefit formula by investigating all legal changes to the Unemployment Insurance Act as referenced in the Legal Information Database (*Rechtsinformationssystem*, RIS). The RIS is the Austrian government's online archive of the Austrian Federal Law Gazette (*Bundesgesetzblatt*), where all legislation passed by the Austrian Parliament and decrees by cabinet ministries are published.[63] The UI benefit schedule for each year as a function of monthly gross earnings can be found in Appendix H.2.

Prior to 1994, the earnings base was a measure of earnings right before unemployment (see Section E.3 for details). For these years (i.e. the 1976, 1985, and 1989 reform samples), we undertake the following steps to calculate the predicted benefit change:

1. Begin with the daily gross earnings concept described in Section E.2.
2. Within each sample (across the whole ASSD, not just the percentile ranges used in our regressions), calculate the *average annual growth rate* from the year into the next year.
3. Multiply the daily earnings in the year by the average earnings growth rate. Call this the *inflated earnings*.
4. Calculate the UI benefits corresponding to the *inflated earnings* value using the benefit calculator. This is b.

From 1994 onward, the earnings base was based on lagged earnings (see Section E.3 for details). From 1994 through 2000, we undertake the following steps to calculate the predicted benefit change:

1. Begin with the daily gross earnings concept described in Section E.2 *but from the previous year*.

[63]The RIS page with all references to the Unemployment Insurance Act in the Federal Law Gazette can be found here: `https://www.ris.bka.gv.at/GeltendeFassung.wxe?Abfrage=Bundesnormen&Gesetzesnummer=10008407`.

2. Calculate the UI benefit corresponding to the *lagged earnings value* using the benefit calculator in each year. This is b.

For 2001, the earnings base is based on lagged net earnings:

1. Begin with the daily gross earnings concept described in Section E.2 *but from the previous year.*

2. Calculate the *daily net earnings* using the tax calculator described in Section G.

3. Calculate the UI benefit corresponding to the *lagged net earnings value* using the benefit calculator for 2001. This is b.

Before 1994, we also calculate *realized benefits changes* for our validation exercise:

1. Begin with the daily gross earnings concept described in Section E.2.

2. Calculate the UI benefits corresponding to the *daiy gross earnings* value using the benefit calculator. That is, we do not inflate by average annual earnings growth into the next year. This is b.

Note that "realized" and predicted benefits correspond exactly after 1994, i.e. for the 2001 reform.

E.5 Validation of Benefit Calculation

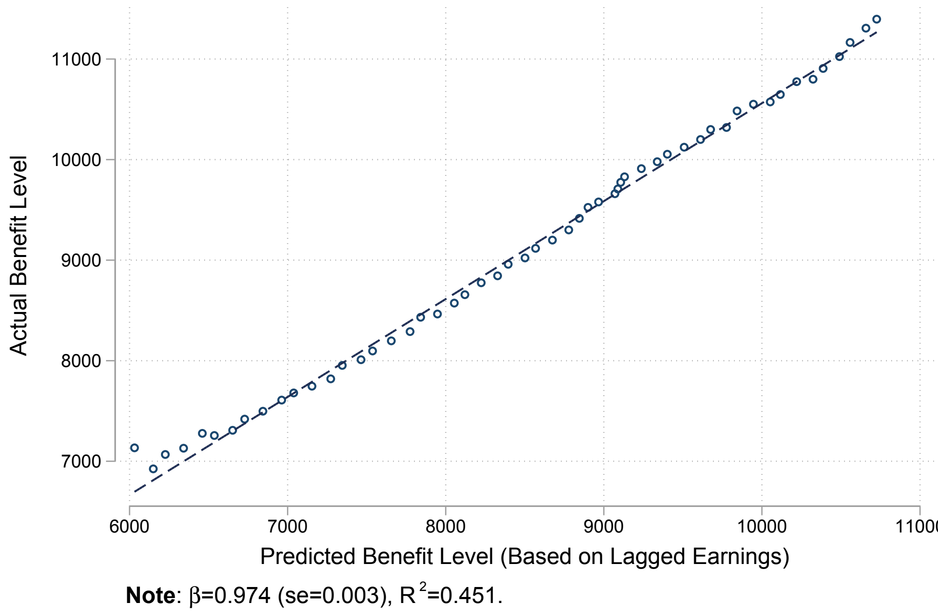
We assess the quality of our prediction of benefits based on the ASSD (see previous Section E.4) by comparing predicted unemployment insurance benefits for *actual separators* to actually received UIBs. To this end, we merge the observed unemployment benefit data (AMS) with predicted benefits in our regression sample from 1996 to 2000. All measures are nominal and not inflation-adjusted.

Appendix Figure A.14 plots the relationship between actual and predicted UI benefit levels for Austrian workers with positive months in unemployment drawing UI benefits. The relationship traces out a slope that is on average 0.974.[64] We therefore conclude that our approach accurately assigns employed workers by their ASSD-based earnings into the UI benefit levels.

In addition, we also validate that our earnings prediction works well across the earnings distribution with coefficients on predicted and actual benefits close to 1 throughout (Appendix Figure A.15).

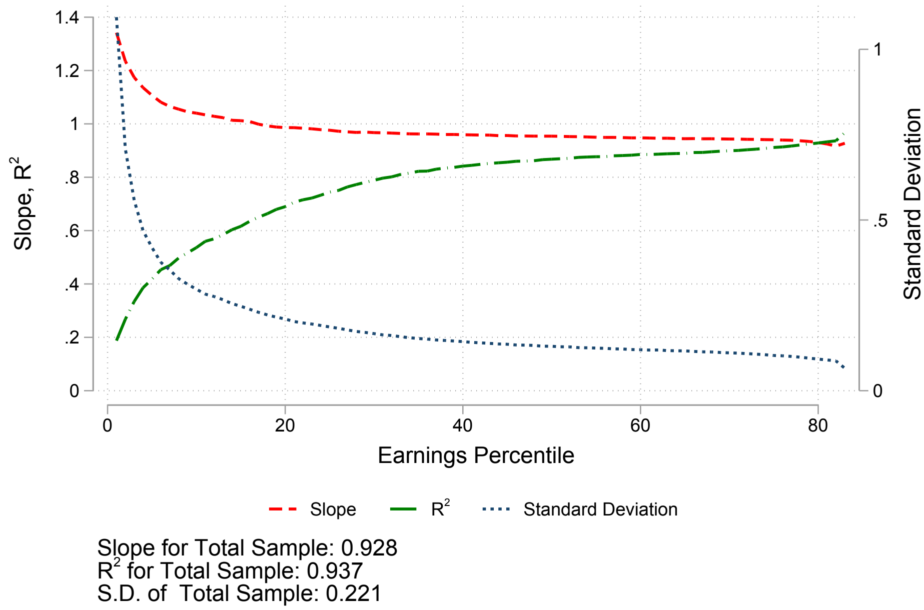
[64]The R^2 is 0.45. We would not expect $R^2 = 1$ even if we accurately predicted income for each individual since UIBs also include supplemental benefits based on the number of dependents (e.g., EUR 29.50 per month in 2018). These are not dependent on income and thus orthogonal to our variation.

Figure A.14: Actual Benefit Receipts vs. Predicted Receipts from Measured Pre-Separation Average Earnings Among Sample of Separators



Note: The figure draws on earnings data from the ASSD and benefit data from the AMS. The x-axis shows predicted benefit levels based on earnings data from the ASSD. The y-axis shows actually paid-out benefits based on data from the AMS. The figure is a binned scatter plot based on individual-level observations.

Figure A.15: Quality of Wage Prediction Procedure



Note: The figure reports several statistics by earnings percentile for the income prediction procedure. In particular, the figure reports the slope of actual to predicted wages, as well as the standard deviation of the residual and the R^2.

E.6 Construction of Job Transition Types

In Panel A of Table IV, we report treatment effects for three groups of workers: i) job stayers, ii) recalled worker, iii) job switchers. We define those based on observed employment status and employers separately at the one- and two-year horizon. Job stayers are defined as workers who are employed every month at the one- or two-year horizon at the same firm that they were employed at in December of the base year. Recalled workers are defined as workers who at the given horizon have an employment spell at the same firm as in December of the base year but have an intermittent labor market status not corresponding to employment at the same firm before returning to the initial firm. Recalled workers' one-year earnings are pre- and post-separation calendar year averages. Finally, movers are defined as workers who have an employment spell with a firm different from the initial one and do not return to the initial firm for a subsequent employment spell. For job switchers, we use our spell data to consider only *post-separation wages* rather than average annual earnings.

In Panel B of Table IV, we report effects for EUE movers, i.e. the subset of job movers who are unemployed (receive UI/UA) before moving into the next job. These are workers who switched employers after experiencing an intermittent unemployment spell (since we aggregate daily employment spell data to the monthly level by taking each individual's labor market status on the 15th of the month, this classification will misclassify some short (up to one month) EUE transitions as EE transitions.. As for job switchers overall, we study only post-separation wages as outcomes for EUE movers.

Finally, Panel C of Table IV reports results of direct EE movers, another subset of job switchers. These are workers who switched employers without an intermittent nonemployment spell at the monthly level of aggregation. Again, as for job switchers overall, we study only post-separation wages as outcomes for EE movers.

E.7 Construction of Variables for Heterogeneity Analysis

This section describes the construction of the variables we use for the analysis of treatment effect heterogeneity. Below, we describe how we divide the heterogeneity groups into quintiles (unless otherwise stated), which we calculate separately for each year and reform. For all variables split into quintiles, the smallest values correspond to the smallest quintiles. Throughout, we draw on the sample of all workers, regardless of whether they are employed all year, unless stated otherwise. Prime-age below refers to the ages 25 to 54. We exclude workers with missing birth month. The variable `status` refers to workers' employment status in the ASSD status. The following variables are merged using 2-year lagged values: Industry Months UE, Industry EU Transition Rate, Industry Growth Rate, Standard Deviation of Earnings Growth within the Firm, and a proxy for the Wage Distance from CBA Floor.

E.7.1 Unemployment Risk

1. **Industry Months UE**. This is the average number of months of unemployment in the next period, conditional on being employed in the current period.

    - Begin with the universe of prime-age workers.
    - Calculate how many months the worker is unemployed in the following year. Keep only employed workers (i.e. `status = 3`) in the current year.
    - Regress the number of months on industry-occupation-year fixed effects. Save these fixed effects.

2. **Industry EU Transition Rate**. This is the probability of being unemployed in the next period in a given industry-occupation, given that one is employed in the current period.

    - Begin with the universe of prime-age workers.
    - Create an indicator for whether the individual is unemployed (`status`= 1) in the next year. Keep only employed workers (i.e. `status = 3`) in the current year.
    - Regress this indicator on industry-occupation-year fixed effects for that year. Save these fixed effects.

3. **Local Unemployment Rate**

    - Begin with the universe of prime-age workers between 1972 and 2003.
    - Worker residence is not well populated for some years, yet firm location is. We assume nonemployed workers' location is identical to their *previous* firm's location (`gkz`). When this is not possible, we asume workers' location is identical to their *future* firm's location. We thus carry firm location forward first and then backward.
    - A: Count the number of workers who are unemployed (`status`= 1) by the `gkz` variable and year.
    - B: Count all the workers in the area of residence who are unemployed, sick, employed, self-employed, on parental leave, or in minor employment.

- Divide A by B.
- Here, we separate the sample into quartiles, not quintiles, because the sample bunches (in areas with large populations).

4. **Two measures of the time since nonemployment**. For left-censored observations, we set the time since the respective nonemployment event equal to one month in the first month of observation. We begin with sample of prime-age workers, count the number of months for each of the four designations for each worker, and split into quintiles. We merge this to the analysis sample at time t.

   - **Months since UI Receipt** (i.e. `status` = 1). Note that the employment spell length keeps counting if the worker becomes sick, goes on disability, or takes a parental leave.
   - **Months since Nonemployment** (i.e. `status` $\neq 3$) Note that if employment spells are separated by only a single month of illness, then the month of illness and the two spells are counted as a single employment spell.

E.7.2 Firm Characteristics

1. **Industry Growth Rate**

   - Begin with the universe of prime-age workers in a given year. Measure the leave-out mean industry growth rate. That is, for worker i in firm j and industry k, the growth between t and $t' = t + 1$ is

$$\triangle S_{ijk} = \frac{\sum_{j' \in J-j} \mathbb{1}(\text{Industry}_{j'} = k) \cdot (\text{Employment}_{j't'} - \text{Employment}_{j't})}{\sum_{j' \in J-j} \mathbb{1}(\text{Industry}_{j'} = k) \cdot \text{Employment}_{j't}}$$

   - Count the number of workers in the firm (`benr`), not necessarily employed the whole year.
   - Count the number of workers in the industry (`nace08`), not necessarily employed the whole year.
   - Subtract, for each firm, its population from the number of workers in the industry.
   - Find the same number for the next year $t + 1$ (i.e. two years pre-reform).
   - Calculate the percent difference between the leave-out employment in the industry between year $t + 1$ and year t.
   - Winsorize this last variable at the first and 99th percentiles by year.

2. **Wage Premium (AKM Firm Effects)**

   - For each year in the reform sample t (i.e. the four years pre-reform), take the universe of prime-age workers from year $t - 10$ to t. Before 1982, take 1972 as the earliest year. Do not use 1972 or 1973.
   - Regress winsorized log-earnings on year fixed effects, a third-order polynomial in age, and an exhaustive set of worker and firm fixed effects (Abowd, Kramarz, and Margolis, 1999). We use the procedure in (Correia, 2017) for estimation.

- Extract the largest connected set.
- Save the firm fixed effects for year t and merge to firm-years in the regression sample.
- Divide the sample into quintiles based on the firm effects.

3. **Standard Deviation of Earnings Growth within the Firm**.
   - Focus on a sample of workers who stay with their firm from one year to the next.
   - Drop workers at the ASSD cap and with missing earnings.
   - Calculate the individual earnings growth relative to last year. Winsorize to the first and 99th percentiles.
   - Calculate the standard deviation of the earnings growth by firm-year among workers who were in the same firm across the two years.

4. **Wage Distance from CBA Floor (Proxy)**. We base this measure on the residuals from a regression of log earnings on tenure-experience-occupation-industry-year fixed effects, with standard deviations calculated at the firm-year level. 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. Occupation refers to white- vs. blue-collar, for which there are often separate collective bargaining agreements. Drop workers at the earnings cap and winsorize earnings at the first and 99th percentiles by year. Regress earnings on industry-occupation-tenure-experience-year fixed effects. Calculate residuals from this regression, and take the standard deviation of the residual by firm-year. Split the sample into quintiles.

5. **Share Non-Employed within the Last 2 Years**.
   - Begin with the universe of prime-age workers.
   - A: Count the number of workers at the firm whose current employment spell is less than 24 months and who was unemployed in the month before their current employment spell (`status` = 1).
   - B: Count the total number of workers at the firm.
   - Divide A by B.
   - Split this last variable into quintiles.

E.7.3 Worker Characteristics

1. **Tenure**. We split the tenure variable into quintiles in the sample of workers in our analysis.
2. **Age**. We split the age variable into quintiles in the sample of workers in our analysis.
3. **Male/Female**. We use the dummy sex variable for workers in our sample. 1 denotes females.
4. **Blue/White Collar**. We use the dummy white collar variable for workers in our sample. 1 denotes white collar workers.

F Alternative Variation in UI Generosity: Measuring Wage Effects from An Age-Specific Reform of Potential Benefit Duration

In this section, we analyze the effect of changes in potential benefit duration (PBD) of UIBs, rather than UIB levels, on incumbent wages. We do so by exploiting a reform in 1989 that changes PBD for workers aged 40 and above. Appendix Figure A.16 shows how the PBD schedule changed for individuals age 30-49. Before 1989, the PBD was only experience and not age-dependent.[65] In 1989, these eligibility rules were changed so that individuals age 40-49 with at least five years of experience in the past 10 years were eligible for 39 weeks while individuals below age 40 were still only eligible for 30 weeks. For the analysis below, we focus on the PBD reform for individuals age 40-49 and compare their earnings growth to individuals age 30-39.[66] We apply the same sample restrictions as in our main result for the full sample but drop all individuals present in particular Austrian regions where workers aged 50 and above were eligible for even larger PBD reform since 1988.[67]

The two panels in Appendix Figure A.17 plot the average earnings percent changes (one and two years) by age groups in the treated and control years. The left-panel plots the average wage growth from 1987-1988 (the control year) and from 1988-1989 (the treatment year) as well as their difference. If the PBD extension for older workers passed through to their wages, we would expect an increase in wage growth for older workers. The right panel plots the same for two-year wage growth. Neither figure shows an increase in wage growth for treated individuals.

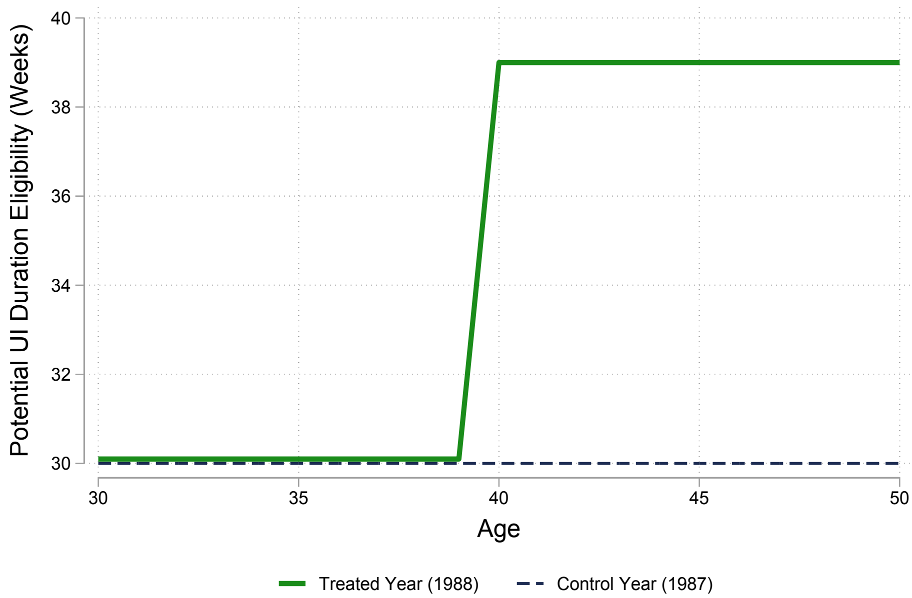
In Appendix Figure A.18, we report results from estimating a specification similar to Equation (19) but replacing the replacement rate reform indicators with an indicator for being ages 39-42 and adding age-specific fixed effects. We also include the same controls included in specification (4) in Table III. The figures show no significant treatment effects when the reform was enacted as well as a lack of pre-trends, validating our identifying assumptions. In conclusion, we do not find wage effects of PBD on incumbent wages either, thereby mirroring the insensitivity we document for UI level shifts.

[65] Individuals with less than 12 weeks of UI contributions in the last two years were eligible for 12 weeks, individuals with 52 weeks in the last two years were eligible for 20 weeks, and individuals with 156 weeks (3 years) and the last five years were eligible for 30 weeks.

[66] These rules applied to workers with at least 6 years of experience in the past 10 years, which is our sample restriction for this part of the analysis. See Nekoei and Weber (2017) for an evaluation of this reform on *unemployed* job seekers' spell duration and reemployment wages.

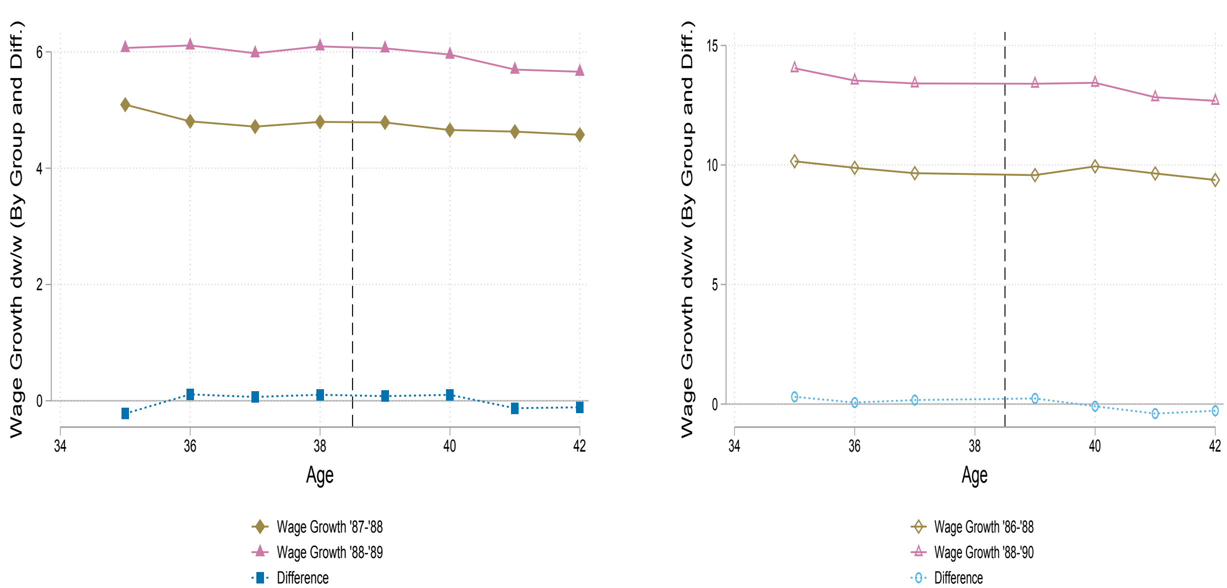
[67] We do not study the latter reform because of a regional reform that further increased PBD for workers older than 50 and led to separations (and thus attrition) among those older workers (Jäger, Schoefer, and Zweimüller, 2018), that would not allow for a measurement of wage effects.

Figure A.16: PBD Schedule - Treated and Control Years



Note: The figure plots potential benefit duration (PBD) schedule by age for individuals in 1988 and 1989. Before 1988, all individuals with at least five years of work experience in the past ten years were eligible for 30 weeks of PBD. In 1989, individuals age 40-49 with the same experience were eligible for 39 weeks.

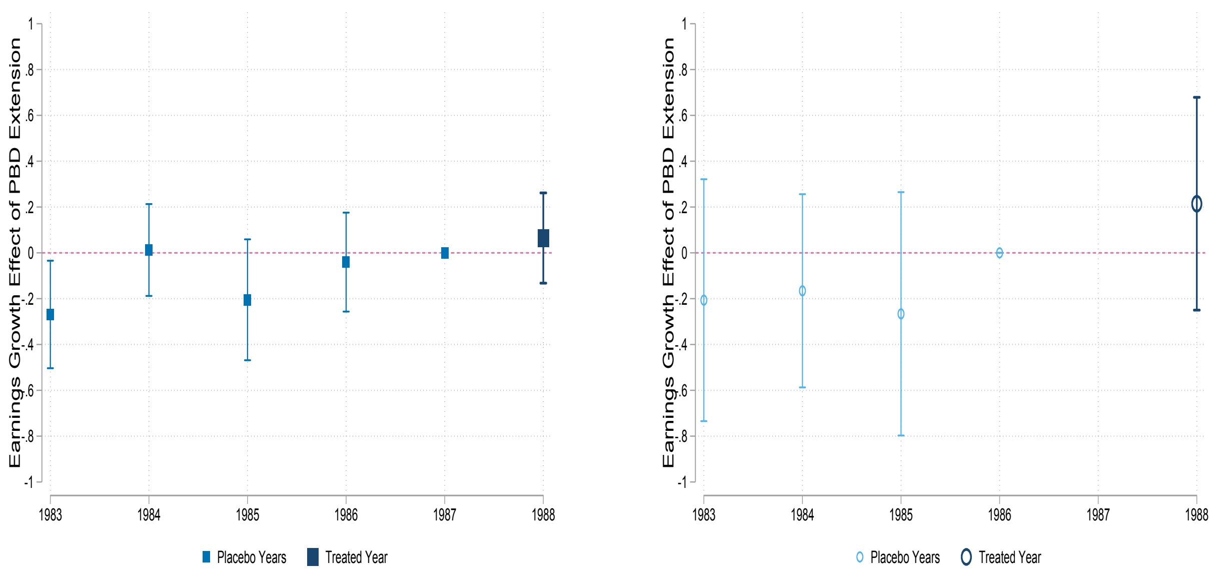
Figure A.17: Non-Parametric PBD Figures - One- and Two-Year Earnings Growth



Note: These figures plot average earnings growth by age from 1987-1988 and 1989-1989 (the year the PBD extension went into effect). Consequently, they mirror the non-parametric analysis for the replacement rate reforms presented in the first two panels of Appendix Figures A.5-A.2.

Figure A.18: Difference-in-Difference Coefficient Estimates

One-Year Earnings Growth　　　　Two-Year Earnings Growth



Note: These figures report results from estimating a specification similar to Equation (19) but replacing the reform-induced benefit changes with an indicator for being ages 39-42 in 1988 (treated by the PBD reform) and adding age-specific fixed effects. We include the same controls included in specification (4) in Table III.

G Robustness of Wage-Benefit Sensitivity to Tax Treatment

To take into account that unemployment insurance benefits are not taxed in Austria, we verify that our results are robust to changes in gross benefits. Appendix Table A.7 reports those results. Appendix Figure A.19 presents the graphical analysis. Since the benefit changes are now larger, the implied gross-wage/gross-benefit sensitivities shrink compared to our main results using net benefits. Since our tax calculator may only imperfectly approximate individuals' actual tax situation in particular in the early years of our sample, our main results use the raw net (untaxed) benefit variation. We detail the tax calculator below.

Features of the Austrian Tax System Based on the 2001 tax regime, we construct a tax calculator that incorporates the key elements of the Austrian income tax system:

1. **Base salary (*Bemessungsgrundlage*)**. Austrian salaries are paid in 14 installments, usually of equal size. Twelve are paid monthly as a base salary, which is subject to a more elaborate tax schedule and eligible for more credits and deductions. We observe the annual pre-tax amount paid as base salary from each establishment to every worker in the ASSD.

2. **Holiday bonuses (*Sonderzahlungen*)**. Austrian salaries are paid in 14 installments, usually of equal size. The 13th installment is usually paid during the summer (*Urlaubszuschuss*) and the 14th before Christmas (*Weihnachtsremuneration*). They are subject to a simpler tax schedule and social security contribution policies. We observe the annual pre-tax amount paid as holiday bonuses from each establishment to every worker in the ASSD.

3. **Social security contribution.** Each of three social welfare programs is partly financed by contributions as a proportion of workers' gross income: the unemployment insurance system (*Arbeitslosenversicherung*), health insurance system (*Krankenversicherung*), and old-age pension system (*Pensionsversicherung*). The proportions have changed over time. The salary contributions to unemployment insurance can be found in § 61, section 1, of the Unemployment Insurance Act (*Arbeitslosenversicherungsgesetz*, AlVG) through 1994 and in § 2, section 1, of the Labor Market Policy Financing Act (*Arbeitsmarktpolitik-Finanzierungsgesetz*, AMPFG) from 1995 onward. The salary contributions for health insurance and old-age pensions can be found in § 51, section 1, of the Social Security Act (*Allgemeines Sozialversicherungsgsetz*, ASVG). There are also payroll contributions from employers to these programs that are not counted. Social security contributions are not taxed.

4. **Base salary-only social security contributions**. Workers make additional contributions as a proportion of their base salary to a national housing subsidy program (*Wohnbauförderungsbeitrag*) and the Austrian Chamber of Labour (*Arbeitkammerumlage*), an organization that represents workers and consumers in Austria and is independent of the trade unions and their federation (*Österreichischer Gewerkschaftsbund*, ÖGB). Unlike for trade unions, membership in the Chamber and the associated base salary contribution are compulsory for all Austrian workers. Together these contributions add 1 percentage point to the proportion of the base salary taken for social security programs, and the contributions are not taxed.

5. **Work-related expenses (*Werbungskosten*).** There is a tax deduction for unavoidable expenses during work. We use the amounts available to workers who are not self-employed, which can be found in § 16, section 3, of the Income Tax Act (*Einkommensteuergesetz*, EstG).

6. **Special expenses (*Sonderausgaben*).** There is a small tax deduction for various "special expenses" such as charitable donations and church donations. The amounts can be found in § 4, section 3, of the Income Tax Act.

7. **Income tax schedules**. Tax schedules for the base salary can be found in § 33, section 1, of the Income Tax Act and those for the holiday bonuses in § 67, section 1, of the Act. We use the 1972 and 1988 Acts as well as intermediate reforms.

8. **General tax credit (*Allgemeiner Absetzbetrag*).** During our period of study, a tax credit was provided to all Austrian taxpayers. The amount—or the earnings schedule on which it is calculated in later years—can be found in § 33, section 5, of the Income Tax Act of 1972 and subsequent amendments and in § 33, section 3, of the Income Tax Act of 1988 and subsequent amendments.

9. **Commuting tax credit (*Verkehrsabsetzbetrag*).** The Income Tax Act of 1988 introduced a tax credit for expenses related to commutes, and the amount can be found in § 33, section 5.

10. **Wage earner's tax credit (*Arbeitnehmerabsetzbetrag*).** Those who are not self-employed can claim a small additional tax credit, the amount of which could be found in § 33, section 8, of the Income Tax Act of 1972 and subsequent reforms and in § 33, section 5, of the Income Tax Act of 1988 and subsequent reforms.

Other Elements of the Austrian Tax System We have highlighted the key features of the Austrian tax system. However, there are many other tax credits and deductions, such as for households with children, pensioners, and those on disability and parental leave. Thus, our calculations are for an individual who is not self-employed and ignore exemptions for specific groups. In addition to focusing on observable characteristics of individuals in the ASSD, this also is in line with how the pre-separation income base is meant to be calculated for UI benefits after 2000, when benefits were determined using net incomes rather than gross incomes.

Calculating Net-of-Tax Rates We calculate the net income in the following steps, using values for each feature of the tax system.

- For the **base salary**:

  1. Calculate **taxable income**
     = Gross base salary
     − Social security contribution (as a proportion of gross earnings)
     − Base salary-only social security contributions (as a proportion of gross earnings)
     − Tax deduction for work-related expenses
     − Tax deduction for special expenses
  2. Calculate the **tax burden** using the tax schedules and the taxable income.

3. Calculate the **tax owed**, which is then set to 0 if negative
   = Tax burden
   − General tax credit
   − Commuting tax credit
   − Wage earner's tax credit
4. Calculate **net base salary**
   = Gross base salary
   − Social security contribution (as a proportion of gross earnings)
   − Base salary-only social security contributions (as a proportion of gross earnings)
   − Tax owed

- For the **holiday bonuses**:

  1. Calculate **taxable income**
     = Gross holiday bonuses
     − Social security contribution (as a proportion of gross earnings)
  2. Calculate the **tax burden** using the (simpler) tax schedules and the taxable income. There are no tax credits on the holiday bonuses, so this is also the **tax owed**. This is set to 0 if negative.
  3. Calculate **net holiday bonuses**
     = Gross holiday bonuses
     − Social security contributions (as a proportion of gross earnings)
     − Tax owed

- Calculate **total net earnings** as the sum of the net base salary and the net holiday bonuses.

- Calculate the **net-of-tax rate** by dividing the total net earnings by total gross earnings.

Converting to Gross Units To make a direct gross-gross comparison, we convert each individual's UI benefit shift $db_{i,r,t}(w_{i,r,t-1})$ in specification (19) into gross terms by dividing by the individual's net-of-tax rate in the base year $t-1$. This inflates the change in the nonemployment value.

Placebo Years We treat benefit changes in placebo years just as we do with our standard benefit change (see Section 4.3.1), assigning individuals in pre-reform years the average $db/(1-\tau)w$ of the eighth of a percentile in the reform year, where τ is the average tax rate.

Table A.7: Estimated Wage Effects with Shifts in Gross UI Benefits

Panel A: 1-Year Earning Effects

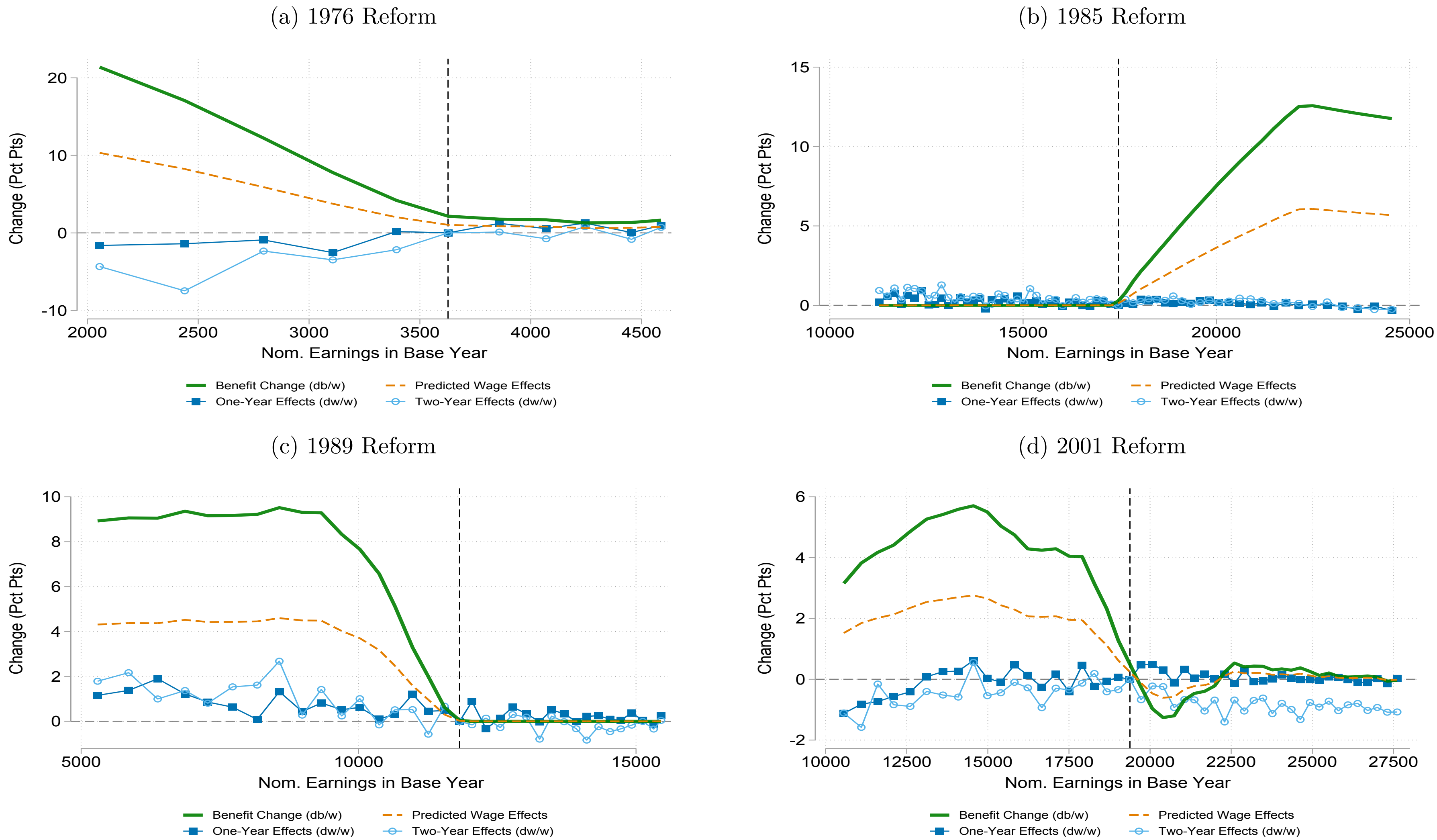
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.014	0.000	0.014	0.013	0.018	0.024
	(0.011)	(0.011)	(0.013)	(0.012)	(0.011)	(0.012)
Placebo: 2 Yr Lag	0.006	-0.003	-0.000	-0.001	0.015	0.012
	(0.011)	(0.012)	(0.013)	(0.013)	(0.011)	(0.011)
Treatment Year	**-0.007**	**-0.004**	**-0.013**	**-0.009**	**-0.006**	**-0.007**
	(0.011)	**(0.011)**	**(0.012)**	**(0.013)**	**(0.009)**	**(0.009)**
Pre-p F-test p-val	0.482	0.943	0.511	0.487	0.254	0.122
R^2	0.050	0.068	0.088	0.104	0.275	0.295
N (1000s)	7142	7142	7138	7138	6302	6298
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Panel B: 2-Year Earning Effects

	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.010	-0.004	0.014	0.014	0.008	0.015
	(0.014)	(0.013)	(0.018)	(0.017)	(0.015)	(0.016)
Placebo: 2 Yr Lag						
Treatment Year	**0.007**	**0.019**	**-0.006**	**-0.002**	**-0.007**	**-0.010**
	(0.018)	**(0.018)**	**(0.019)**	**(0.018)**	**(0.017)**	**(0.017)**
Pre-p F-test p-val	0.488	0.743	0.430	0.417	0.583	0.353
R^2	0.112	0.136	0.153	0.173	0.321	0.348
N (1000s)	5045	5045	5042	5042	4439	4437
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: The table reports results of a robustness check for the specifications reported in Table III. The specifications reported here take into account that UI benefits are untaxed in Austria. To take non-taxation into account, we translate the UI benefit shift, *db* from specification (19), into a change in (hypothetical) gross benefits by scaling up the actual benefit shift by an individual's average net-of-tax rate so that both the benefit and the wage change are in gross units. For further information on the specification see notes for Table III. To calculate individuals' net-of-tax rate, we rely on a purpose-built tax calculator for Austria. See Appendix Section G for details on the Austrian income tax system and constructing gross benefits.

Figure A.19: Overview of Non-Parametric Results with Gross UI Benefit Changes



Note: The figure plots robustness checks for the results reported in Figures IV(a) through IV(d). The specifications reported here take into account that UI benefits are untaxed in Austria. To take non-taxation into account, we translate the UI benefit shift, db/w reported in the solid green line above, into a change in (hypothetical) gross benefits, db_{Gross}/w, by scaling up the actual benefit shift by an individual's average net-of-tax rate so that both the benefit and the wage change are in gross units. See Appendix G for additional information on the tax calculation.

H Reform Timelines and Unemployment Benefit Schedules Over Time

H.1 Timeline for Reforms to Unemployment Benefits

We report on the procedural timelines for each of the four reforms to the Austrian unemployment insurance system.

2001 Reform. The Budget Act of 2001 (*Budgetbegleitgesetz 2001*) was introduced in the Austrian Parliament on October 17, 2000, and passed on November 23 (Item 142 in the 2000 Federal Law Register). It went into effect on January 1, 2001 and included reforms to other laws apart from Unemployment Insurance Law of 1977. On November 17, the Budget Committee produced its report and requested adoption of the legislative proposal. It was discussed by parliament on November 23. Martin Bartenstein, the labor minister, argued during this session that the change to a 55% net income replacement rate was intended a means of improving transparency in the determination of unemployment benefits. The law was approved with 93 votes in favor and 70 votes against, and published in the official gazette on December 29. Changes to unemployment benefits went into effect January 1, 2001.

1989 Reform. The Amendment of the Unemployment Insurance Act 1977 and the General Social Insurance Act (*AIVG-Novelle 1989*) was introduced in Parliament on June 7, 1989, and passed 20 days later (Item 364 in the 1989 Federal Law Register). It went into effect on August 1. The *AIVG-Novelle 1989* was approved unanimously with no modifications to the original draft, barring a proposed amendment from the Social Affairs Committee produced amending the salary contribution to the UI system. The unemployment benefit schedule in the final version was identical to the one in the original proposal.

1985 Reform. On October 16, 1984, the Austrian Ministry of Social Affairs published a decree (*Verordnung*) extending the unemployment benefit schedule which went into effect on January 1, 1985 (Item 416 in the 1984 Federal Law Register). The decree cited a requirement in the Unemployment Insurance Act that the Ministry extend the cap on the benefit schedule when the Parliament increases the maximum contribution from salaries to the UI system (*Höchstbeitragsgrundlage*).[68] These increases are meant to adjust for nominal wage changes, and the requirement existed throughout the period of study in this paper. The 1985 adjustment was much larger than previous adjustments, however, because a year earlier, the Parliament had switched the maximum contribution to the UI system from that used for the national health insurance system to that used for the old-age pension system.[69]

1976 Reform. The Austrian government first introduced a bill increasing unemployment benefits

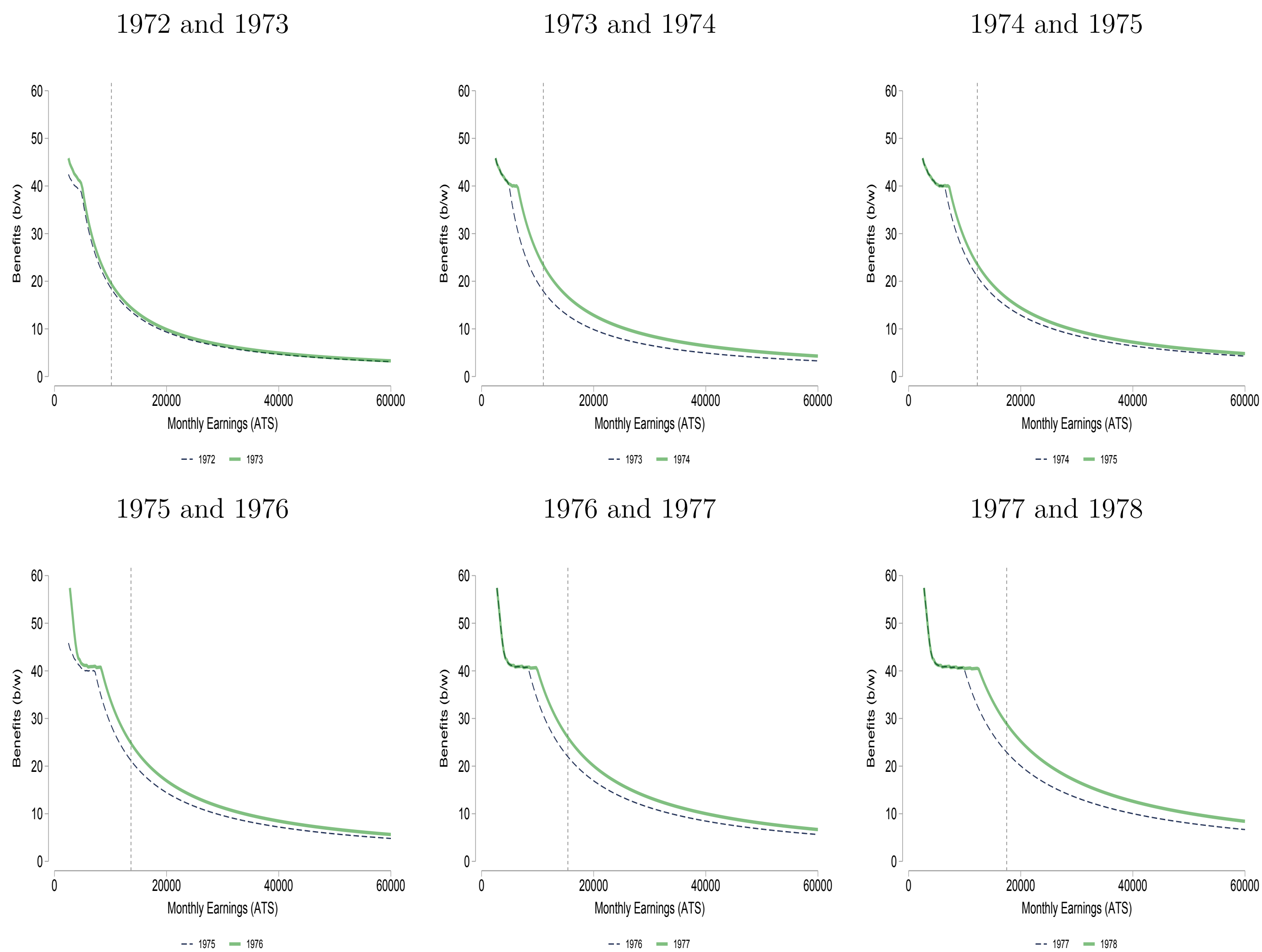
[68]See § 21, section 4, of the Unemployment Insurance Act (*Arbeitslosenversicherungsgesetz*, AlVG). Specifically, the law as of 1984 stipulates a deadline for these unemployment insurance cap extensions—i.e. within a year that the increased contribution cap takes effect—the size of the additional earnings brackets, and the benefit level at each bracket.

[69]See § 61, section 1, of the Unemployment Insurance Act for the definition of the maximum salary contribution to the UI system and § 45, section 1, of the Social Security Act (*Allgemeines Sozialversicherungsgesetz*, ASVG) for the maximum contributions to the health insurance and old-age pension systems. The change in the maximum contribution originated as a bill proposed by a MP from the right-wing Freedom Party of Austria (*FPÖ*) that was in a governing coalition with the Social Democrats (*SPÖ*). The bill was introduced in Parliament on October 21, 1983; passed in a subsequent session on November 29; and went into effect on January 1, 1984. Aside from tying the cap to a different maximum contribution, the law was also the first to stipulate a specific deadline of a year for the Ministry of Social Affairs to issue an appropriate cap extension.

(*AlVG-Novelle 1976*) at a session of Parliament on March 17, 1976, which was approved in a subsequent session on May 6 (Item 289 of the 1976 Federal Law Register). It went into effect on July 1, with the benefit increases exactly as proposed in the government's original bill. An amendment from a member of an opposition party increasing benefits for a slightly higher range of gross earnings was considered in a committee meeting but rejected during the May 6 parliamentary session.

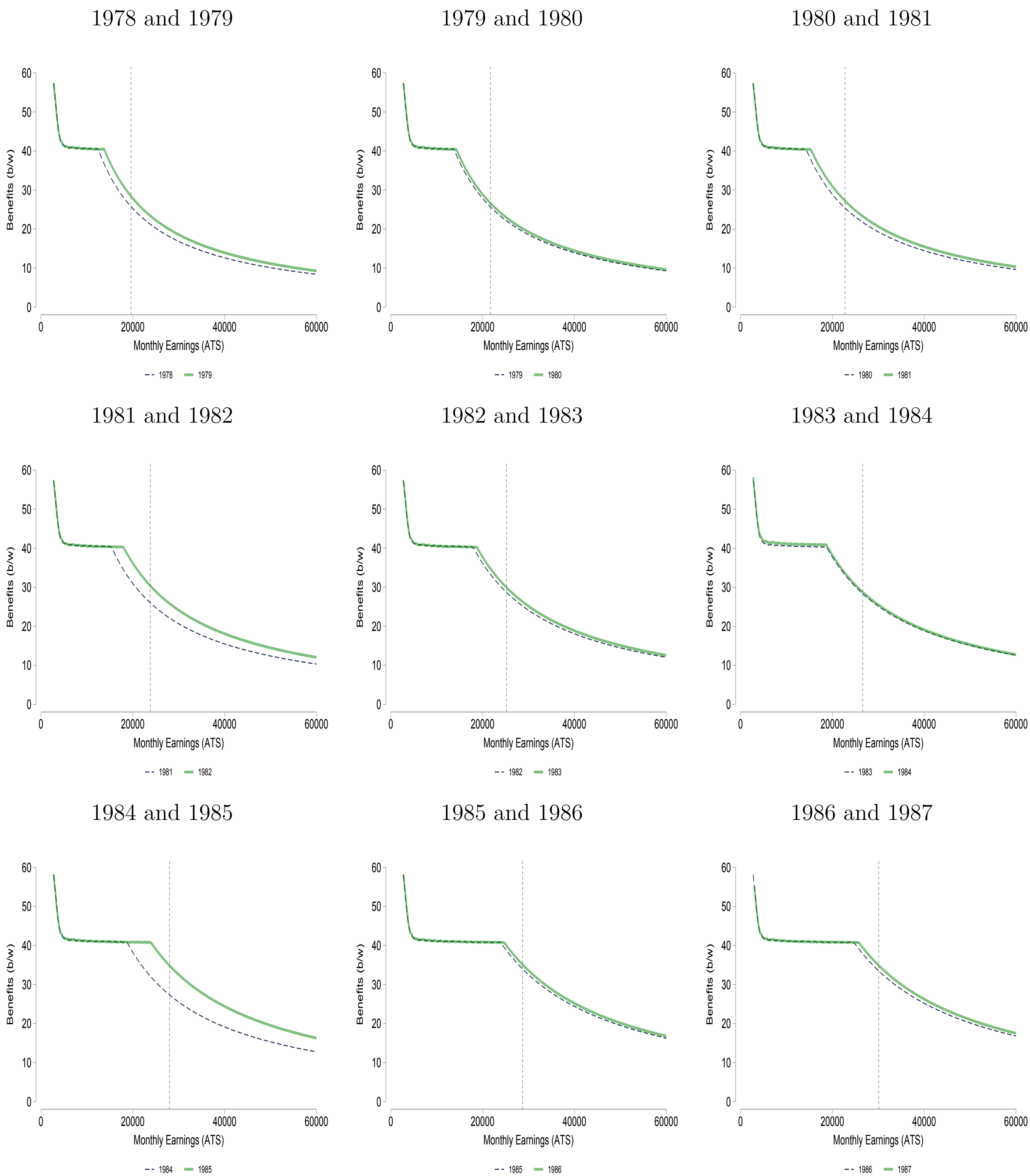
H.2 UI Benefit Schedules in Austria: 1972–2003

Figure B.1: UI Benefit Schedules 1972-1978



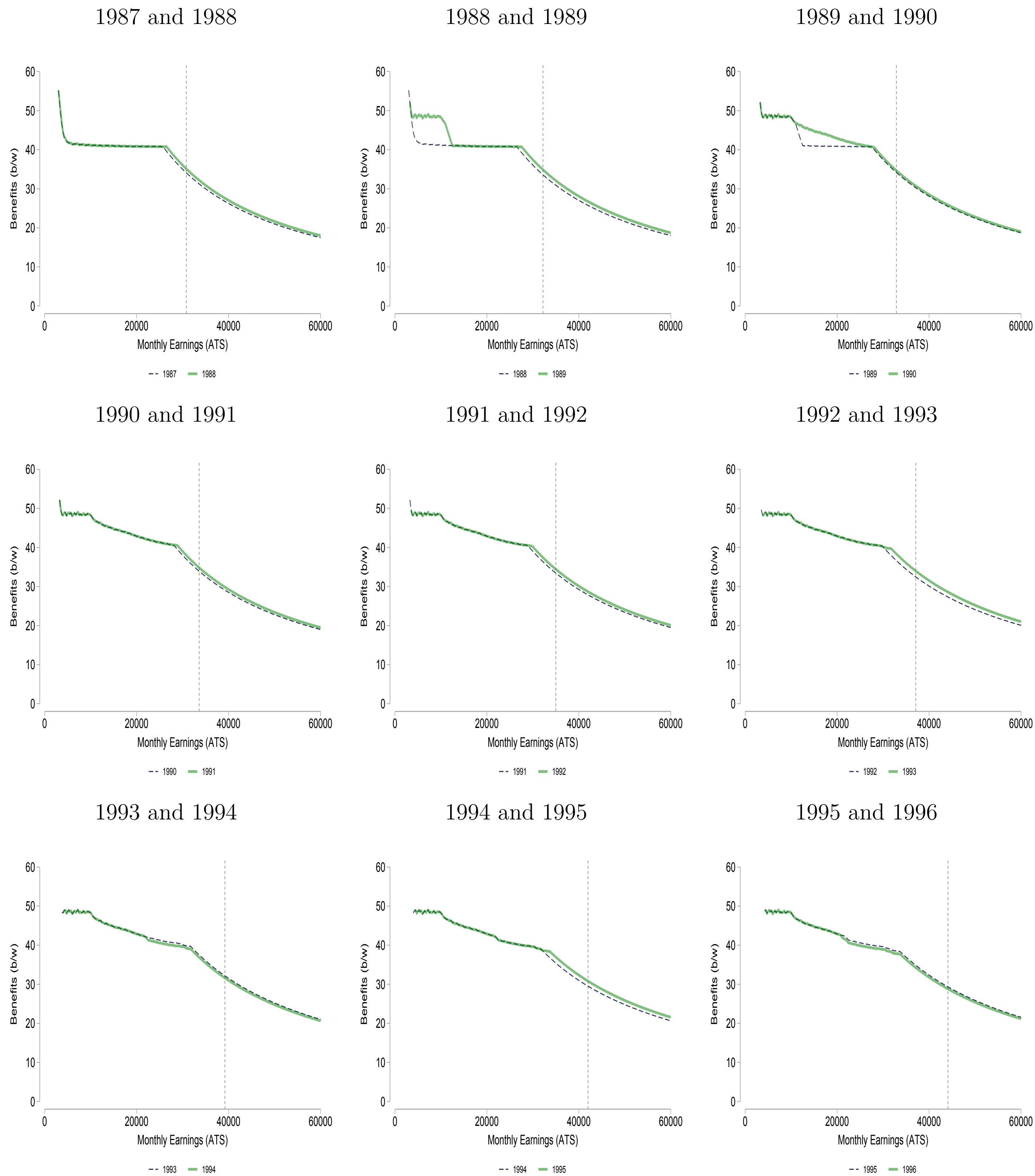
Note: The dashed vertical lines in gray correspond to the social security earnings maximum.

Figure B.2: UI Benefit Schedules 1978-1987



Note: The dashed vertical lines in gray correspond to the social security earnings maximum.

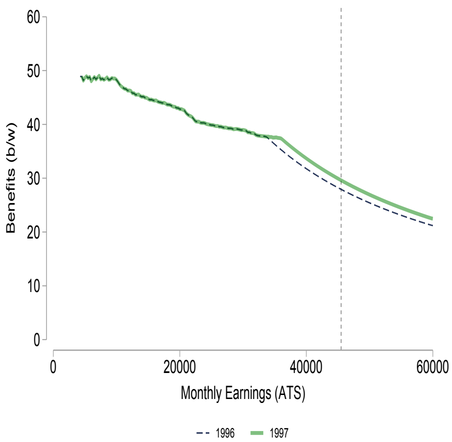
Figure B.3: UI Benefit Schedules 1987-1996



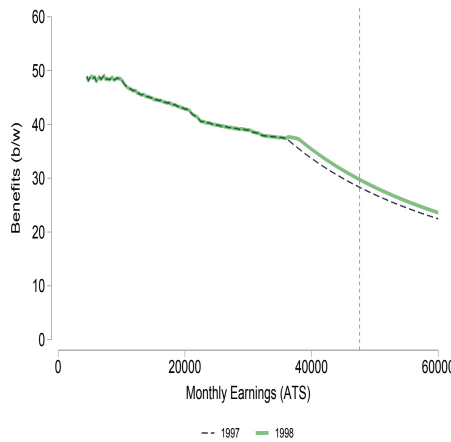
Note: The dashed vertical lines in gray correspond to the social security earnings maximum.

Figure B.4: UI Benefit Schedules 1996-2003

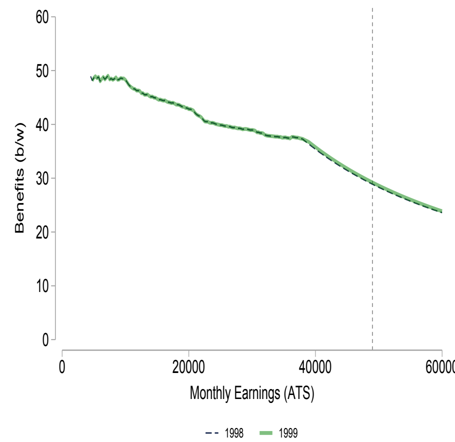
1996 and 1997



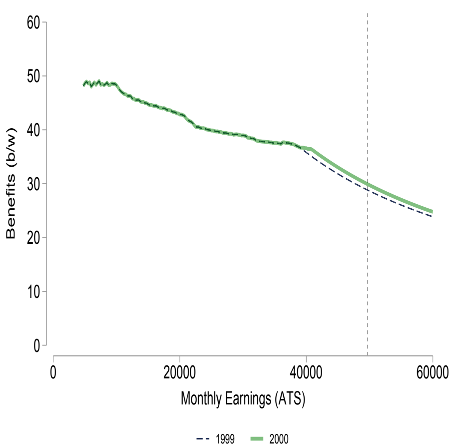
1997 and 1998



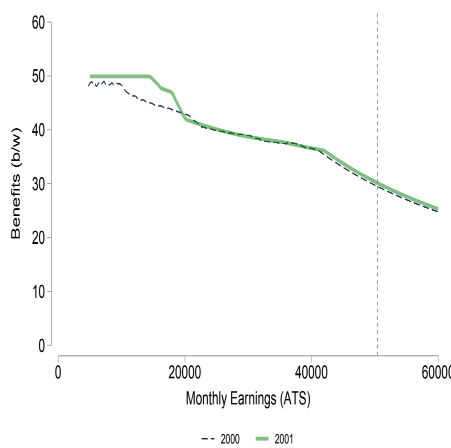
1998 and 1999



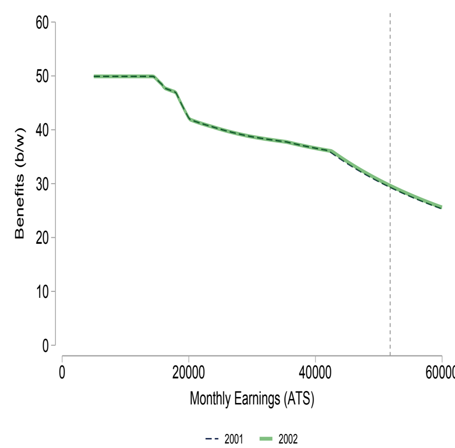
1999 and 2000



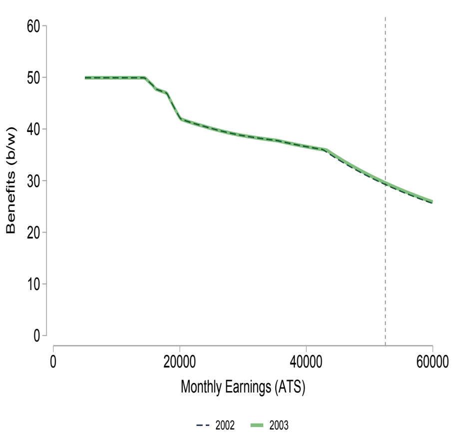
2000 and 2001



2001 and 2002



2002 and 2003



Note: The dashed vertical lines in gray correspond to the social security earnings maximum.

I Additional Details on Rent Sharing Meta Analysis

I.1 Interpreting Firm- and Industry-Level Rent Sharing Estimates in a Bargaining Setting

A larger body of evidence examines the effect of idiosyncratic inside values of jobs on wages: rent sharing of firm- and industry-specific productivity and profit shifts, which is consistent with rent sharing. Card et al. (2018) review that literature. A leading interpretation is that shifts in surplus arise from TFP shifters. A structural interpretation of a shift in the inside value of the employment relationship in Nash bargaining is:

$$w^N = \phi \times p + (1-\phi) \times \Omega \tag{A71}$$

$$\Rightarrow dw^N = \phi \times \underbrace{dp}_{\text{Rent sharing variation}} \tag{A72}$$

Below, we proceed under the assumption that p shifts are well-measured. If so, the rent-sharing result can be readily interpreted in a bargaining framework.

Elasticity Specifications A common empirical estimate comes in an *elasticity* of wages with respect to value added per worker, measured at the firm or industry level:[70]

$$\xi = \frac{dw/w}{dp/p} \tag{A73}$$

Structurally interpreted in the Nash bargaining setup, this elasticity turns out to capture a product of two distinct terms: the ratio of the marginal product over the wage, times bargaining power ϕ:

$$\frac{dw^N/w^N}{dp/p} = \phi \times \frac{p}{w^N} \tag{A74}$$

Rent sharing elasticities ξ therefore provide *upper bounds* for ϕ:

$$\phi = \frac{w}{p} \cdot \xi \leq \xi \tag{A75}$$

Of course, if the ratio of w to p, the marginal product of the worker, were known, ϕ can be immediately backed out. However, the very motivation of models of imperfectly competitive labor markets, which give rise to bargaining, rent sharing and wage posting, is that these two values can diverge dramatically and in heterogeneous ways.

This bound is tight if $\phi \approx 1$ or if $b \approx p$ since then, by Nash, $w \approx p$. However, this bound is less useful in case the elasticity is small. In that case, ϕ is implied to be small, and w may deviate from MPL greatly unless b is close to p. In the data, x is indeed estimated to be small, implying a small bargaining power parameter and also permitting a small wage–MPL ratio absent high b. In this case, information on the *level* of b is required again to make progress. Formally, one can plug in the Nash expression for w to obtain a correspondence between ϕ and p, b and the measured

[70] Some studies consider profit elasticities rather than value added shifts; rescaling into value added elasticities that rely on strong assumptions about homogeneity and the comovement of variable and fixed factors with productivity shifts.

wage–productivity elasticity ξ as follows:

$$\phi = \frac{b\xi}{p(1-\xi)+b\xi} = \frac{1}{\frac{p}{b}\cdot\frac{1-\xi}{\xi}+1} \tag{A76}$$

We caution that it may therefore be impossible to translate the elasticity estimates into bargaining power parameters without strong quantitative assumptions about the bargaining structure, chiefly because the observable variables, w and perhaps p, do not uniquely map into b and ϕ.

An interesting example is Card, Cardoso, and Kline (2016), who among many verification tests also estimate the heterogeneity in ξ for women and men. The elasticity for women is below the elasticity for men. However, even with measured productivity shifts being homogeneous, two distinct factors may cause the elasticity differences within a bargaining framework. First, either men and women wield differential bargaining power ϕ^g where $g \in \{w, m\}$. Second, $\phi^w = \phi^m$ yet $p^f/w^f < p^m/w^m$ or $p^f/b^f < p^m/b^m$. That is, the latter scenario could arise if the opportunity cost of working of women $b^f > b^m$, as would also be in line with their larger labor supply elasticities, higher unemployment, and lower participation overall.

The information needed to translate a given value added rent sharing elasticity into the point estimate for ϕ therefore requires strong assumptions or empirical knowledge about b. Measuring the level of the worker's flow valuation of nonemployment b (and thus surplus $b = MPL - b$) is difficult even for an average household (see, e.g., Chodorow-Reich and Karabarbounis, 2016). z includes unemployment benefits but also any utility differences between the employed and unemployed state, or other income. $\frac{z}{MPL-z}$ is similarly elusive and related to the fundamental surplus in Ljungqvist and Sargent (2017), which is $\frac{MPL}{MPL-z}$).

Identifying ϕ off *level* shifts in p rather than percentage shifts eliminates the complications arising from elasticities.

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