

WAGES AND THE VALUE OF NONEMPLOYMENT*

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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

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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. *JEL* Codes: E24, J3, J63, J65.

I. 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 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 across all specifications.

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. In addition,

1. Quitters have full benefit duration after a 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.

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 postseparation nonemployment duration. Yet when we split up workers by their predicted postseparation time on UI (and other proxies for unemployment risk), 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. Last, 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 also 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 article 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](#)). Our focus on employed workers isolates the bargaining channel, whereas the unemployed are subject to multiple, perhaps offsetting, nonbargaining 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](#)). Moreover, 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 raise the question of 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 forthcoming) 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, Cogleanese, and Karabarbounis (2019) and positive employment spillovers on ineligible control workers in Lalive, Landais, and Zweimüller (2015).

Section II derives wage-UIB sensitivity in our Nash benchmark and discusses alternative models. Section III describes institutions, reforms, and data. Section IV presents our empirical design and results. Section V studies subsamples and group bargaining. Section VI concludes. All appendix materials can be found in the Online Appendix.

II. 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 UIBs—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 IV. We then discuss alternative wage-setting models that insulate wages from the nonemployment value.

II.A. 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 II.B](#), 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.

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

$$(1) \quad w = \phi \cdot p + (1 - \phi) \cdot \Omega.$$

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' :

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

2. The firm and the worker choose wage $w = \arg \max_{\tilde{w}} (E(\tilde{w}) - N)^\phi (J(\tilde{w}) - V)^{1-\phi}$ (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 $\frac{dV}{dw}$, either relying on canonical free entry $V = 0$, or on a control group netting out dV ([Section II.B](#)). We assume $\frac{dp}{dw} = 0$, but argue against room for quantitatively important dp effects in [Online Appendix A.5](#).

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' :

$$(3) \quad \rho N = \underbrace{\frac{\rho + \delta}{\rho + f + \delta}}_{\equiv \tau} b + \underbrace{\frac{f}{\rho + f + \delta}}_{\equiv 1 - \tau} w'.$$

Nonemployment Time Reemployment Time
Postseparation Postseparation

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 $\rho \rightarrow \infty$ (e.g., due to myopia or liquidity constraints), a low job-finding rate $f = 0$, or a high subsequent separation rate $\delta \rightarrow \infty$ 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 $f \rightarrow \infty$ 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 $\Omega = \rho N$ and plugging in the expression for ρN derived in [equation \(3\)](#), the Nash wage becomes:

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

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

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

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

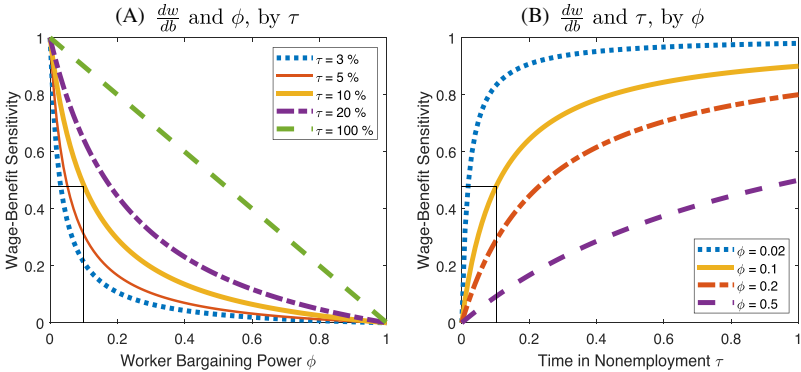


FIGURE I

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

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 postseparation 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 (Panel A), and depicted in the thin line departing from $\tau = 0.1$, crossing the solid line ($\phi = 0.1$) in Panel 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):

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

Conversely, a given sensitivity $\frac{dw}{db}$ and τ imply a bargaining power $\phi = \frac{1 - \frac{dw}{db}}{1 + \frac{dw}{db} \cdot (\tau - 1)}$. For intuition, Figure I, Panel A 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 I, Panel B 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. *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 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 ϕ values, close to one, 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 $\tau \approx \frac{\delta}{\delta + f}$ (where $\rho \approx 0$ implies a lower bound for $\frac{dw}{db}$). τ then corresponds to an individual's expected fraction of postseparation 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 postseparation share of time spent on 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 ,

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 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 [Online Appendix I.1](#).

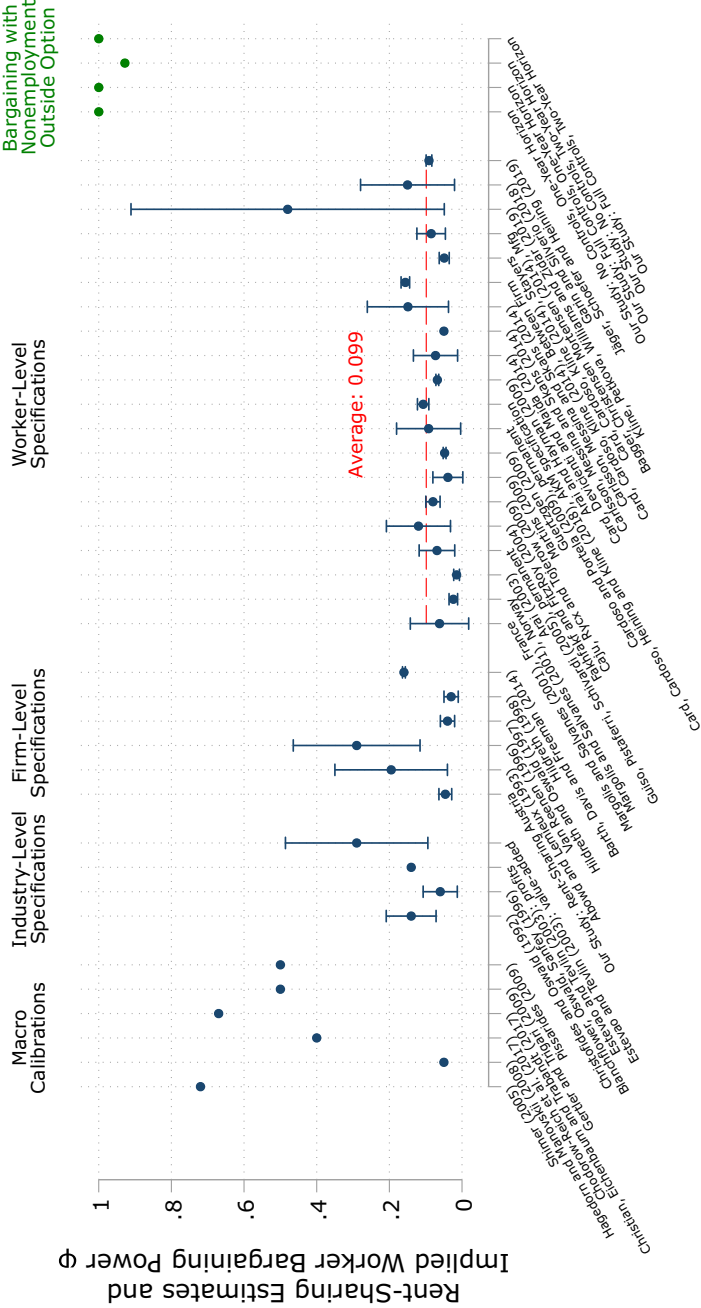


FIGURE II
Overview of Estimates and Calibrations of Worker Bargaining Power

Figure II (*Continued*). 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, column 12, average 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 [Online Appendix I.1](#)). Among the worker-level specifications or rent-sharing elasticities 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 [Online Appendix I.2](#).

as detailed in note 16). We refer to UI in this article 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 IV.D](#) we exploit heterogeneity in $\hat{\tau}_i$. Specifically, to construct $\hat{\tau}_i$, we plug a worker's preseparation 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 [Online Appendix B](#)).⁴

[Table I](#), Panel A 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 Since the table lists time in other states as well, it denotes τ by τ^U . 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 postseparation 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 either of retirement or disability, if "absorbing" (no subsequent employment or UI/UA spells in the next 16 years). The other columns also show robustness to stopping counting only at absorbing retirement in columns (3) and (4), or neither at disability nor retirement in columns (5) and (6).

Benchmark Wage-Benefit Sensitivity. In [Table I](#), Panel B, we also report the implied wage-benefit sensitivities. For $\phi = 0.1$,

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. [Online 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 average τ_i of 4%, reflecting that our full worker sample is stably employed unless taking up the separation outside option.

TABLE I
PREDICTED FRACTION OF POSTSEPARATION TIME ON UI (τ) AND WAGE-BENEFIT
SENSITIVITY ($\frac{dw}{db}$)

Time restriction:	Retirement or disability		Retirement		No restriction	
	4-year (1)	None (2)	4-year (3)	None (4)	4-year (5)	None (6)
Panel A: Fraction of postseparation time (average predicted values)						
UI-affected nonemployment $\hat{\tau}^U$	0.104	0.105	0.096	0.095	0.094	0.092
Unemp. insurance, $\hat{\tau}^{U:UI}$	0.083	0.083	0.076	0.074	0.073	0.070
Unemp. 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 ($\frac{dw}{db}$)						
Baseline: two-state $\frac{dw}{db}$ prediction						
$E[\frac{dw}{db}(\hat{\tau}_i)]$	0.462	0.462	0.442	0.437	0.434	0.426
$\frac{dw}{db}(E[\hat{\tau}_i])$	0.483	0.486	0.464	0.461	0.457	0.453
Robustness: three-state $\frac{dw}{db}$ prediction						
$E[\frac{dw}{db}(\hat{\tau}_i)]$	0.251	0.217	0.224	0.194	0.217	0.190
$\frac{dw}{db}(E[\hat{\tau}_i])$	0.243	0.201	0.208	0.174	0.196	0.165

Notes. 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 (likewise for the other odd columns; by contrast, column (2) and the other even columns do not impose this reemployment restriction). For these actual separators, we calculate the postseparation 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' preseparation characteristics comprising industry by white/blue-collar fixed effects, tenure, experience, age, region, year, and previous UI history. We use this model to predict the specific τ 's for the entire regression sample (including nonseparators). The reported τ estimates are the average predictions across the entire regression sample. The $\frac{dw}{db}$ predictions plug in the predicted τ values into the two- and three-state model wage-benefit sensitivity expressions (see Section II.A and Online Appendix C, respectively). The $E[\frac{dw}{db}(\hat{\tau}_i)]$ 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 $\frac{dw}{db}(E[\hat{\tau}_i])$ estimates plug in the average $\hat{\tau}_i$ values from the first six rows 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 reemployment (at any job) sometime in the next four years (for at least one day). Online Appendix Table A.5 reports the realized τ values for the separator samples and the analysis sample unconditionally on a separation. Numbers in bold indicate our preferred specifications.

suggested by the micro studies on rent sharing, and an average $\tau = 10\%$ as described above, the predicted wage–benefit sensitivity is:

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

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 micro-empirical evidence, we would find a sizable sensitivity of 0.09. The table also reports the sensitivity respecting Jensen’s inequality (as $\frac{dw}{db}$ is nonlinear in τ), which is similarly sized $\mathbb{E}[\frac{dw}{db}(\hat{\tau}_i)] = 0.46$, so our exposition reports $\frac{dw}{db}(\mathbb{E}[\hat{\tau}_i])$. Finally, in Section IV.D, 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.

II.B. 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).

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 $-v$, search effort costs $c(e)$, unemployment stigma γ (all normalized into money units by budget multiplier λ), and other nonemployment-contingent income y :

$$(8) \quad z(b, \dots) = b + \frac{-v - c(e) - \gamma}{\lambda} + y + \dots$$

Micro Choices and Market Adjustment. We now 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 allow to adjust. Now, values N and E are:

(9)

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

(10)

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

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

$$\begin{aligned} \frac{d\rho N}{db} &= \underbrace{\frac{\partial \rho N}{\partial b}}_{\text{Mechanical Effect}} + \underbrace{\frac{\partial \rho N}{\partial w'} \frac{dw'}{db}}_{\text{Feedback from Wage Response}} + \underbrace{\frac{\partial \rho N}{\partial \mathbf{c}} \nabla_{\mathbf{c}} \rho N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_{\mathbf{b}} \mathbf{c}^*}_{\substack{= 0 \text{ By Envelope Theorem} \\ \text{Micro Reoptimization}}} \\ (11) \quad &\quad \underbrace{\quad}_{\text{Net Out with Control Group}} \\ &+ \underbrace{\nabla_{\mathbf{x}} \rho N \cdot \nabla_{\mathbf{b}} \mathbf{x}}_{\text{Market Adjustment}}, \end{aligned}$$

where $\nabla_{\mathbf{a}} f(\mathbf{a}, \mathbf{b})$ denotes the gradient of $f(\cdot)$ 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 $\nabla_{\mathbf{c}} \rho N(b, \mathbf{c}^*, \mathbf{x}) = \mathbf{0}$, such that $\nabla_{\mathbf{c}} \rho N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_{\mathbf{b}} \mathbf{c}^* = \mathbf{0}$.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

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

agent takes as given, for example, shifts in the 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 $\mathbf{x}_i = (\boldsymbol{\mu}^{m(i)}, t^{g(i)})$ into market-level variables $\boldsymbol{\mu}^m$, and group-specific factors t^g 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:

$$\begin{aligned}
 (12) \quad \frac{dN^g}{db^T} &= \mathbb{1}_{(g=T)} \times \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \frac{dw'^g}{db^T} + \nabla_t N \cdot \nabla_b t^g + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^m, \\
 (13) \quad \frac{dN^T}{db^T} - \frac{dN^C}{db^T} &= \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \cdot \left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] \\
 &\quad + \underbrace{\left[\nabla_t N \cdot \nabla_b t^T - \nabla_t N \cdot \nabla_b t^C \right]}_{\text{Assume } =0}.
 \end{aligned}$$

We cannot evaluate the overall effect of potential b -sensitive t , 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 nonwage variables:⁷

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

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

Imperfect Labor Market Overlap. We assess consequences of potential imperfect labor market overlap between the groups in four ways. First, our empirical analysis in [Section IV](#) 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 [Online 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 [Online Appendix D.2](#), we show that even if markets were perfectly segmented, the market-level wage-benefit sensitivity is similarly sized in calibrated equilibrium Diamond-Mortensen-Pissarides (DMP) models with Nash bargaining—with similar mathematical structure as the micro-sensitivity.⁸

2. *Nonemployment without UI.* Our benchmark Nash bargaining model assumes infinite UIB 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 III](#)), while take-up is high but not universal (due to the waiting period for quitters and endogenous take-up decisions). In [Online Appendix C](#), we derive the wage-benefit sensitivity in a three-state model that also features non-UI nonemployment (the nonemployed start out with or without UI receipt, then transition back and forth)—capturing concisely a variety of such specific mechanisms that are otherwise hard to jointly model and quantify.

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 share of time on UI, and $\frac{\partial z^U(b)}{\partial b} = 1$. Now however, not all non-UI time $1 - \tau^U$ is spent in reemployment (where the payoff (the

8. The DMP equilibrium wage-benefit sensitivity we derive in [Online Appendix 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.

wage) is UI-sensitive: $1 > \frac{dw'}{db} > 0$), but fraction τ^O of postseparation time occurs in a state with a UI-insensitive nonemployment payoff $\frac{\partial z^O}{\partial b} = 0$. 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 $\frac{0.39}{0.48} = 81.25\%$ 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 of the three-state model yields an intuitive variant of the familiar two-state sensitivity:

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

To assess the attenuation of the feedback effect through non-UI nonemployment, [Table I](#) presents estimates for τ^O (i.e., we also measure nonemployment states without UI-sensitive payoffs, and predict this value from actual separators onto our regression sample, also detailed in [Online Appendix B](#)). Most time postseparation 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. Because 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 (0.48) when including theoretical benchmarks for predicted wage growth in the empirical figures.

3. Further Robustness and Extensions. In [Online Appendix D.1](#), we show that the predicted wage level remains below the firm's postreform reservation wage for our reforms, that is, 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.

[Online Appendix 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 multiworker firms.

II.C. Alternative Wage-Setting Models

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

1. 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 $\frac{d\Omega}{dN} = 0$, 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

2. 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 Online Appendix D.4. Outside options only become relevant in exogenous breakdowns of bargaining, limiting their influence. Moreover, the wage can simultaneously exhibit outside-option insensitivity and empirically small productivity effects.

3. Wage Posting. Survey evidence from U.S. 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

9. Mortensen and Nagypal (2007), Fujita and Ramey (2012), and 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 $E(w) = (1 - \phi) \cdot N(b) + \phi \cdot (E(w) + J(w))$. An employed worker having received an outside offer dominating N yet dominated by the current job renegotiates the current wage with that external job offer as the new outside option, which thereby replaces the nonemployment value as the outside option relevant for wage setting going forward.

1984; Burdett and Mortensen 1998) can exhibit large wage-benefit sensitivities as well, although 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 to [Online Appendix D.5](#). The nonemployment reservation wage R given by the nonemployment value $E(w = R) = N$, thus forms the cornerstone—the lower bound 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).

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.

III. INSTITUTIONAL CONTEXT, REFORMS, AND DATA

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

III.A. 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 manufacturing 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 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.

III.B. Unemployment Insurance in Austria

The Austrian UI system assigns benefit levels to granular reference wages. Online 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 earnings 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 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,

11. For example, comparative industrial relations work concludes that “in practice local works councils often negotiate supplementary wage increase” (OECD 1994, 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 (std. err. 0.009). See Figure II for a comparison to coefficients from other settings.

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

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 who 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 potential benefit duration (PBD), and mandatory registration with the UI agency (for continuity of health insurance coverage), most workers who separate will take up UI. [Online 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 UA benefits, which track a given worker's reform-induced UIB shifts nearly one to one.¹⁶ We often denote UI as encompassing both programs. Fourth, there is no experience rating. UI is financed by a payroll tax split between the worker and firm.

III.C. 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 pretrends 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 prereform schedule. Benefits and earnings are in nominal Austrian shillings

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 past 25 years, residing in certain regions ([Winter-Ebmer 1998](#); [Lalive and Zweimüller 2004](#)).

15. By contrast, U.S. quitters are de jure ineligible for UI. Compared with 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 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, $UAB_i = \min \{0.92UIB_i, \max \{0, 0.95UIB_i - SpousalEarnings_i + DependentAllowances_i\}\}$. 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.

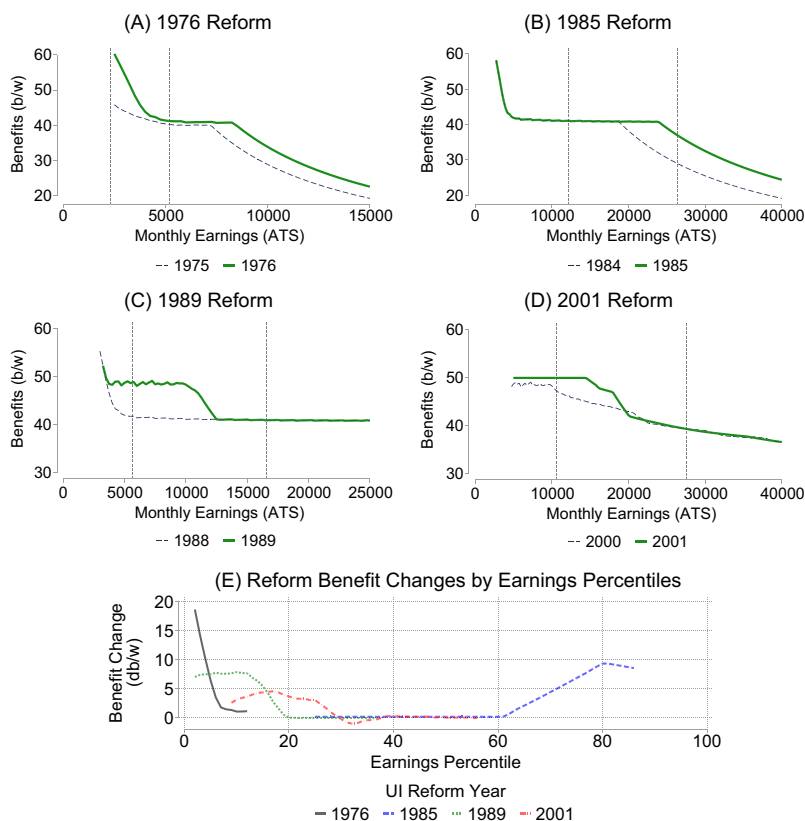


FIGURE III

Unemployment Benefit Schedules and Reforms

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 and 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 [Online Appendix E.1](#). Panel E plots the reform-induced benefit change for each reform in earnings percentile space.

(ATS). We convert EUR into ATS starting in 1999 at a rate of 1 to 13.76. The timing and policy process is summarized in [Online 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.

III.D. Data

Our primary data set 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 Online Appendix E.2), but no hours information. It covers all private-sector (and nontenured 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 (Online Appendix E.5).

IV. 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 $dw_{i,t} = w_{i,t} - w_{i,t-1}$:

$$(16) \quad dw_{i,t} = \sigma \cdot db_{i,t}.$$

We first plot raw data of wage against benefit changes by workers, and then implement a difference-in-differences regression analysis. Our 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 even after two years in our preferred specification. This insensitivity extends to new hires and workers with high predicted time on UI.

IV.A. Variable Construction and Samples

1. Wage Responses. Our main outcome of interest is the change in average daily wages from one year to the next, $dw_{i,t}$

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 Online 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.

where $dw_{i,t} = w_{i,t} - w_{i,t-1}$, whose construction we detail in [Online Appendix 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 $db_{i,t}$, so that we will estimate the level-on-level sensitivity). 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. For overlapping spells within a month, we prioritize the job with the longest spell in that year. For job switchers, we only consider post-separation earnings. Last, we calculate annual averages of these monthly values (excluding months without earnings) to obtain our year- t wage measure.

2. 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 $b_t(\cdot)$. Our variation is the difference between this benefit level and the worker's counterfactual benefit absent the reform, that is, under $t - 1$ schedule $b_{t-1}(\mathbf{x}_{i,t})$. In practice, UI benefit levels are a function of preseparation 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:

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

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 and hence predetermined and unaffected by the reforms.

3. Reference Wages $\tilde{w}_{i,t}$. We now describe our construction of reference wages and implied UIBs (with additional details in [Online Appendix E.4](#)). The earnings concept determining UIBs underwent slight changes over the decades spanned by our four reforms (administrative details are in [Online Appendix 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: $\tilde{w}_{i,t}^{\geq 1996} = w_{i,t-1}$.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 past 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 Gruber and Saez 2002; Kleven and Schultz 2014). In Online 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.

4. 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 Online Appendix 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 IV.C. We also construct prereform placebo cross sections occupying the reform-year earnings percentiles.

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

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.

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	0.89 (0.31)	0.90 (0.30)	0.51 (0.50)	0.23 (0.42)	0.65 (0.48)	0.88 (0.33)	0.47 (0.50)	0.82 (0.38)	0.54 (0.50)	0.61 (0.49)
Age	40.37 (8.26)	40.45 (8.17)	38.86 (8.41)	39.62 (8.00)	38.46 (8.35)	39.65 (7.93)	38.71 (8.02)	39.58 (7.65)	38.80 (8.23)	39.66 (7.87)
White-collar	0.39 (0.49)	0.31 (0.46)	0.38 (0.49)	0.55 (0.50)	0.39 (0.49)	0.42 (0.49)	0.46 (0.50)	0.52 (0.50)	0.42 (0.49)	0.50 (0.50)
Experience in last 25 years	10.31 (6.34)	9.91 (6.04)	15.87 (5.97)	18.42 (6.12)	15.00 (5.82)	13.33 (5.79)	15.45 (6.26)	13.41 (5.99)	15.20 (6.21)	15.11 (6.59)
Tenure	2.83 (1.12)	2.81 (1.11)	7.22 (4.12)	8.67 (4.21)	7.30 (5.06)	6.36 (4.74)	7.74 (6.55)	6.26 (5.60)	7.21 (5.49)	7.00 (5.01)
Avg. monthly earnings	4,171 (294)	2,885 (503)	13,455 (1,237)	20,696 (2,103)	13,777 (1,086)	8,784 (1,920)	24,268 (2,074)	15,683 (3,060)	17,629 (6,343)	15,544 (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

Notes. 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, for example, 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. Because we first construct the sample based on treated earnings regions and equal-sized control regions and then apply further sample restrictions (see [Online 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 IV C](#).

IV.B. Nonparametric Graphical Analysis

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

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

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

1. 2001: Large Benefit Increase for Lower Earners. [Figure IV, Panel D](#) shows the results for the 2001 reform, which we describe in particular detail. The x -axis indicates gross earnings in 2000, the prereform base year. These reference wages determine 2001 benefits.²⁰ Here, we collapse the data into earnings percentiles.

The solid green line (color version of figure available online) 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 (32nd 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 horizons. 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 2000–2002 and 1998–2000 wage growth.) We normalize the wage effects to zero for the lowest percentile not receiving a benefit increase in 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 horizons.

To provide a visual benchmark, we also plot the wage growth predicted by our calibrated bargaining framework in [Section II](#), as the dashed yellow line. That is, we multiply the benefit change

20. The benefit schedule $b_{2001}(\cdot)$ is a function of net earnings while $b_{2000}(\cdot)$ 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 $b_{2001}(\cdot)$. 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.

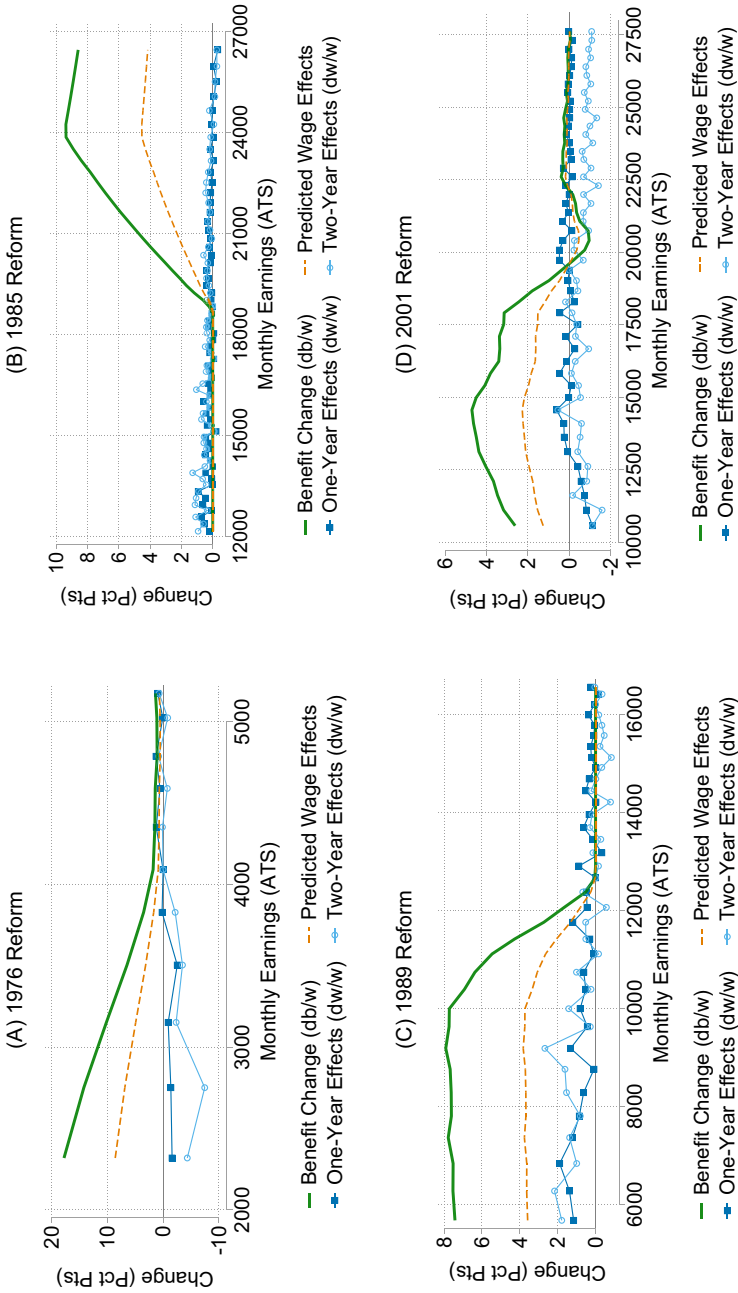


FIGURE IV

Nonparametric Benefit Changes and Wage Effects

The figure plots reform-induced replacement rate changes and wage effects for all four reforms. 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 horizons, respectively. [Section IV.B](#) provides more information.

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 [Online Appendix Figure A.5](#), lags the reform period and the preperiod 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 nonzero treatment effect during the 2001 reform. [Online Appendix Figure A.5](#) shows no such effects for a placebo reform in 1999 (thus comparing 1999–2000 to 1998–1999 wage growth) for the one-year earnings changes. At the two-year horizon, there is even some evidence of a positive pretrend. While such a pretrend would actually bias our results upward, it motivates our regression-based difference-in-differences analysis in [Section IV.C](#), 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—pretrends across all of the reforms.

2. *1989: Increase in Benefits for Low Earners.* [Figure IV](#), Panel C presents the analysis of the 1989 reform, which increased benefits for workers with base-year earnings below ATS 12,600 by up to 8 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 [Online Appendix Figure A.4](#), for 1989 as for the other two reforms before 1995, we also confirm that the reform affected realized benefit levels, validating our earnings inflation prediction and the benefit imputation.

3. *1985: Increase in Benefit Maximum.* [Figure IV](#), Panel B plots the effects of the 1985 reform, which raised the maximum benefit level by 29% (about 7,600–9,800 ATS) for higher earners. We find no evidence for wage increases among these workers.

4. *1976: Increase for Low Earners.* The 1976 reform, analyzed in [Figure IV](#), Panel A, raised benefits for workers with

earnings below 4,100 ATS. If anything, their wages differentially decrease.

5. *The Average Sensitivity of Wages to UI Benefits.* Figure V is a scatterplot 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 $\hat{\sigma} = -0.01$ (std. err. 0.0083) at the one-year horizon and of $\hat{\sigma} = 0.026$ (std. err. 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).

IV.C. Difference-in-Differences Design

We investigate the regression analogue of the nonparametric analysis, to formally test for pretrends (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.

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:

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

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 0 ($\sigma_{-1} = 0$). In addition to the one-year horizon, we also conduct the analysis using two-year wage outcomes (then normalizing σ_{-2} to 0 and omitting all $t = r - 1$ observations because these observations are partially treated).

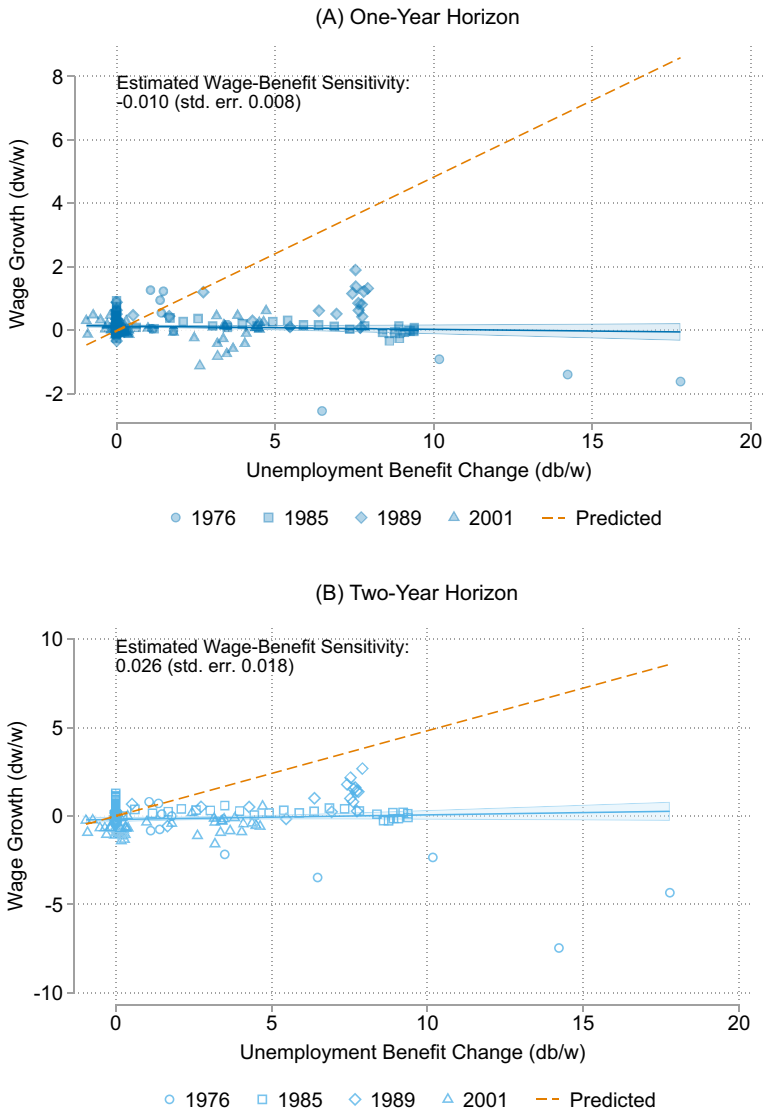


FIGURE V

Scatter Plots of Wage Growth and Unemployment Benefit Changes

Figure V (*Continued*). The figures show scatter plots of wage growth (y-axis) and reform-induced replacement rate changes (x-axis), $\frac{db}{w}$, pooling the four reforms outlined in Figures IV, Panels A–D. Each dot corresponds to a percentile observation from Figures IV, Panels A–D. The upper panel shows wage effects after one year and the lower panel effects after two years. The yellow dashed line indicates 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.

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 note 14. The percentile fixed effects $\tau_{r,P_{t-1}}$ absorb permanent wage growth differentials across percentiles, for example, due to mean reversion. They are reform-specific, that is, 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 NACE industry classification codes 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 preperiod by assigning placebo reforms: in prereform years, $e < 0$, we assign the average $\frac{db}{w}$ 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 preperiod 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 IV.B](#). For each reform, we add three preperiod 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 [Online 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; [Online Appendix Figure A.9](#) confirms robustness to no winsorization as well as at the 5th/95th percentiles.

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, that is, 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 preperiods ($L = 5$) 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](#).

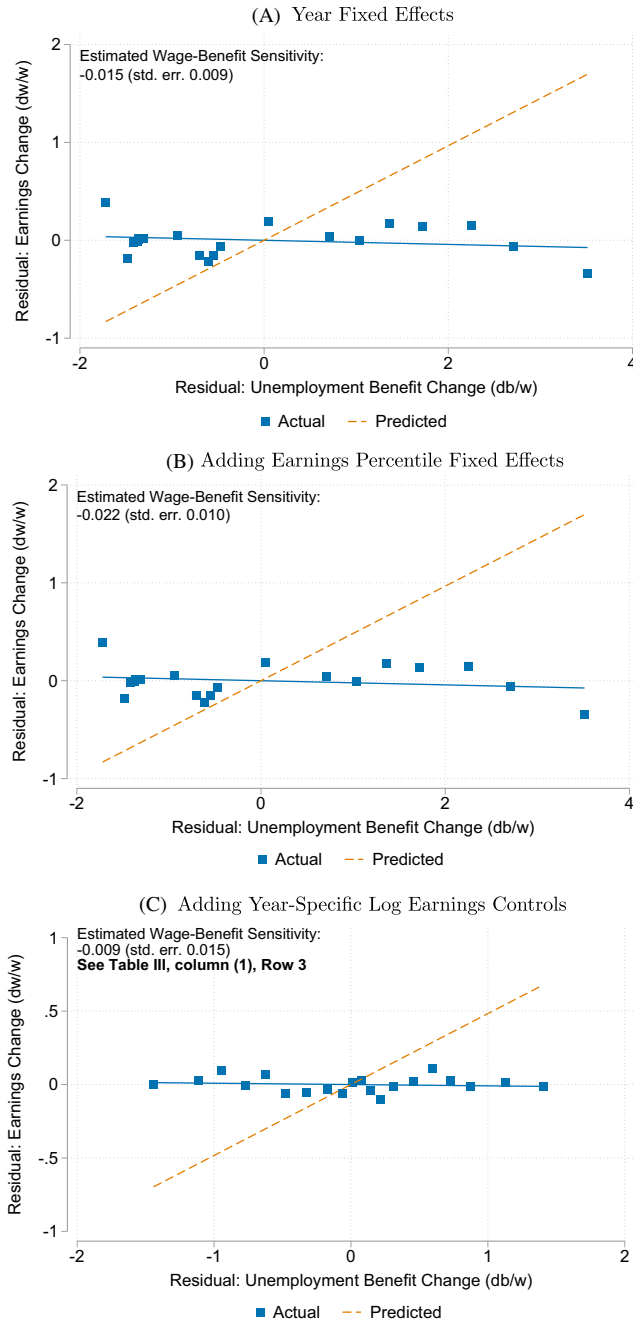


FIGURE VI

Pooled Earnings and Benefit Change Binned Scatter Plots—Treatment Year

Figure VI (*Continued*). The three panels show the best-fit lines and binned scatter plots from estimating one-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 squares, 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 plots the predicted earnings change for each benefit change based on the calibrated Nash bargaining model.

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 [Online Appendix Figure A.6](#). For $e = -2$, we find no evidence for placebo effects across specifications. For $e = -3$, we find some pretrends 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.

3. Full Regression Results. Mirroring the nonparametric analysis, the difference-in-differences analysis reveals that wages are insensitive to benefit changes. The point estimate for the wage-benefit sensitivity is $\hat{\sigma}_0 = -0.007$ (std. err. 0.012) after one year and $\hat{\sigma}_0 = -0.014$ (std. err. 0.024) after two years in our preferred specifications with firm-by-year fixed effects in [Table III](#), column (6). 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. [Table III](#), Panel A presents one-year wage effects, that is, estimates of σ_e . The coefficient of interest is σ_0 , capturing the wage growth associated with reform-induced benefit changes in the treatment year. Column (1) includes the same controls as in [Figure VI](#), Panel C, and the subsequent columns progressively add further controls. We normalize σ_{-1} to 0 and

TABLE III
ESTIMATED WAGE EFFECTS: DIFFERENCE-IN-DIFFERENCES REGRESSION DESIGN

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: One-year earnings effects						
Placebo: 3-yr lag	0.018 (0.015)	0.001 (0.015)	0.020 (0.017)	0.019 (0.016)	0.026 (0.015)	0.035 (0.016)
Placebo: 2-yr lag	0.007 (0.016)	−0.003 (0.016)	0.001 (0.017)	−0.0002 (0.018)	0.021 (0.015)	0.018 (0.015)
Treatment year	−0.009 (0.015)	−0.004 (0.015)	−0.016 (0.016)	−0.010 (0.017)	−0.007 (0.012)	−0.007 (0.012)
Preperiod <i>F</i> -test <i>p</i> -val	.508	.951	.488	.459	.209	.093
<i>R</i> ²	0.050	0.068	0.088	0.104	0.275	0.295
<i>N</i> (1,000s)	7,142	7,142	7,138	7,138	6,302	6,298
Mincerian controls		X		X		X
4-digit ind.-occ. FEs			X	X		X
Firm-year FEs					X	X
Panel B: Two-year earnings effects						
Placebo: 3-yr lag	0.010 (0.019)	−0.007 (0.018)	0.019 (0.024)	0.018 (0.023)	0.011 (0.020)	0.020 (0.022)
Treatment year	0.008 (0.025)	0.024 (0.025)	−0.009 (0.026)	−0.002 (0.025)	−0.010 (0.024)	−0.014 (0.024)
Preperiod <i>F</i> -test <i>p</i> -val	.594	.683	.445	.434	.599	.349
<i>R</i> ²	0.112	0.136	0.153	0.173	0.321	0.348
<i>N</i> (1,000s)	5,045	5,045	5,042	5,042	4,439	4,437
Mincerian controls		X		X		X
4-digit ind.-occ. FEs			X	X		X
Firm-year FEs					X	X

Notes. 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 zero in the preperiod. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being eligible for a regional PBD extension described in note 14. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue- versus white-collar occupation. All specifications also include reform-specific earnings percentile fixed effects, year fixed effects, and year-specific log earnings controls.

assess pretrends with two- and three-year lagged placebos, with coefficients σ_{-2} and σ_{-3} . Across all six specifications, we cannot reject that both preperiod 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 $\hat{\sigma}_0 = -0.009$ (std. err. 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. Table III, Panel B 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 $\hat{\sigma}_0 = -0.002$ (std. err. 0.025). Similar estimates emerge with fewer controls in columns (1)–(3), ranging between -0.009 and 0.024 . The preperiod 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 (Table III, Panel A), 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 (Table III, Panel B), 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 Online Appendix Figure A.6 show, the parallel trends assumption holds in the preperiod without conditioning on parametric earnings controls. At longer horizons, it holds conditional on such controls.) We present variants of our main specification (Table III, column (4)) with alternative earnings controls in Online 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 specification (19) with reform-induced benefit changes implied by realized earnings $\frac{b_t(w_{i,t}) - b_{t-1}(w_{i,t})}{w_{i,t,t-1}}$ as the dependent variable. The contemporaneous coefficient on $\frac{db_{i,r,t}(\tilde{w}_{i,r,t})}{w_{i,r,t,t-1}}$ could be close to 0 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 [Online Appendix Table A.3](#). The analysis reveals a 0.813 (std. err. 0.014) coefficient at the one-year horizon and a coefficient of 0.511 (std. err. 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, preperiod coefficients now test whether the same earners have systematically seen benefits change because of 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 versus 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 [Online Appendix Figure A.11](#), indicate no evidence of increasing 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 V](#) will not find larger treatment effects for very stable earners, for example, with a low predicted separation rate.

Accounting for Nontaxation of UIBs. Austrian UIBs are not taxed. Our gross-wage-based estimates should imply even

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 (std. err. 0.013) coefficient at the one-year horizon and a coefficient of 0.481 (std. err. 0.028) at the two-year horizon. We also report instrumental variable estimates in [Online 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 prereform one-year coefficients statistically significantly different from zero. For the two-year validation, we cannot reject that the preperiod coefficients are jointly equal to zero.

smaller sensitivities, which we confirm in [Online Appendix Table A.7](#) (graphical analysis in [Online 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 [Online 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 [Online Appendix A.5](#). Across specifications and outcomes, the benefit increases were associated with quantitatively negligible and largely statistically insignificant effects.²⁵

IV.D. Wage Sensitivity by Postseparation Time in Unemployment τ

As illustrated in [Figure I](#), Panel B, the baseline model's sensitivity of N to b —and thus that of w to b —increases in postseparation 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 postseparation 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 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

25. By contrast, [Jäger et al. \(2018\)](#) 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.

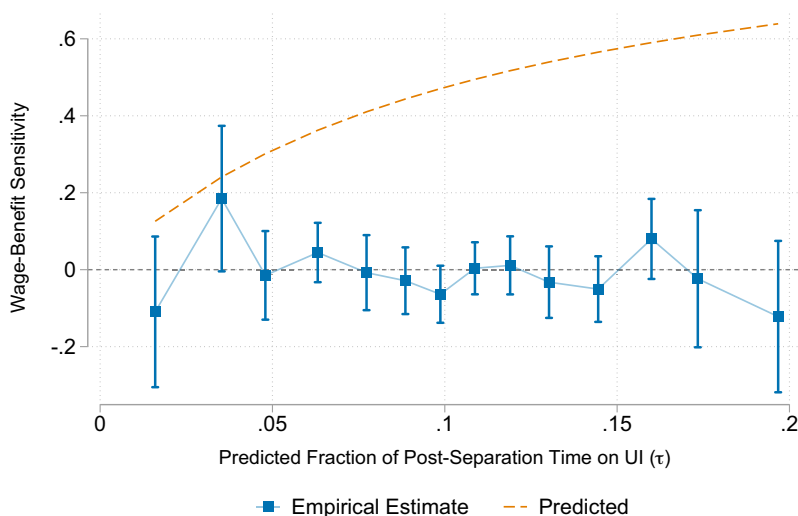


FIGURE VII

Heterogeneity of Wage-Benefit Sensitivity by Predicted Postseparation Time on UI, τ : Model Prediction versus Empirical Estimates

The graph presents wage-benefit sensitivities for workers sorted by their predicted fraction of time on UI conditional on a separation, that is, the $\hat{\tau}_i$ statistic described in Section II.A, further detailed in Online 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 decile into two equally sized groups (ventiles), and 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.

wage-benefit sensitivity (from around 0.15 to above 0.60). By 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.

IV.E. 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.

Table IV, Panel A 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 IV.A (with more details on how we construct the transition types in Online Appendix E.6), we consider only postseparation 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 (std. err. 0.118) at the one-year horizon and of 0.077 (std. err. 0.091) at the two-year horizon. For job movers, we see estimates of 0.109 (std. err. 0.132) at the one-year horizon and of 0.116 (std. err. 0.083) at the two-year horizon. Although 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. 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

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).

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	
	1-Year (1)	2-Year (2)	1-Year (3)	2-Year (4)	1-Year (5)	2-Year (6)	1-Year (7)	2-Year (8)
Treatment effect	-0.010 (0.017)	-0.002 (0.025)	-0.011 (0.021)	-0.015 (0.024)	-0.069 (0.118)	0.077 (0.091)	0.109 (0.132)	0.116 (0.083)
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								
	One-year earnings effects				Two-year earnings effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect	-0.256 (0.139)	-0.193 (0.146)	-0.286 (0.159)	-0.225 (0.234)	-0.067 (0.146)	-0.076 (0.153)	0.020 (0.177)	-0.013 (0.189)
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								
	One-year earnings effects				Two-year earnings effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment effect	0.190 (0.120)	0.113 (0.097)	0.173 (0.092)	-0.003 (0.104)	0.135 (0.100)	0.066 (0.109)	0.019 (0.099)	-0.072 (0.124)
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

Notes. The results show σ_0 coefficients from estimating equation (19) but interacting an indicator for each transition type with the 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 Online Appendix E.6 for more information on the definition of the transition types.

Cahuc, Postel-Vinay, and Robin (2006), because these workers' best outside option is still nonemployment.

Even the results for EUE movers, presented in Table IV, Panel B, 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 because 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 0 once we interact controls by transition type. At the two-year horizon, we find a point estimate of 0.135 (std. err. 0.100) in the specification with the sparsest set of controls and a negative estimate of -0.072 (std. err. 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 nonbargaining channels affecting reemployment 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.

V. THE MISSING LINK BETWEEN WAGES AND BENEFITS

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

$$(20) \quad \frac{dw}{db} = \underbrace{\frac{dw}{d\Omega}}_{\text{Sensitivity of Wages to Outside Options}} \times \underbrace{\frac{d\Omega}{dN}}_{\text{Sensitivity of Outside Option to Nonemployment Value}} \times \underbrace{\frac{dN}{db}}_{\text{Sensitivity of Nonemp. Value to UIB Shifts}}.$$

To assess the relative importance of these three factors for the insensitivity result, we conduct a battery of heterogeneity analy-

27. In unreported results, some EUE wage effects were closer to zero, namely, with alternative earnings controls (as in Online 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).

ses. In particular, for each dimension of heterogeneity, we run our main specification (mirroring [Table III](#), column (4)) and include interactions between heterogeneity group indicators with the treatment variable (and placebo treatments in prereform years). [Table V](#) presents a summary of these estimates. [Online Appendix 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).

V.A. *The Nonemployment Value and UI Benefits $\frac{dN}{db}$*

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

1. *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.

2. *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 IV.E](#))—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 with other sources of idiosyncratic variation in the nonemployment value, UIBs largely depend on recent earnings, information available to both parties. Fourth, we analyze a 2006 Eurobarometer survey asking Austrian workers

TABLE V
HETEROGENEITY OF NONEMPLOYMENT EFFECTS ON WAGES: ONE- AND TWO-YEAR EFFECTS

Quintile	One-year earnings effects		Two-year earnings effects	
	1st (lowest)	5th (highest)	1st (lowest)	5th (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 nonemp.	-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)
Std. dev. of earnings growth	0.023 (0.023)	-0.017 (0.030)	0.072 (0.030)	-0.031 (0.041)
Std. dev. of wage distance from CBA cell mean	-0.061 (0.021)	0.003 (0.036)	-0.045 (0.036)	0.023 (0.049)
Share nonemp. last two 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)

Notes. 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 V and Online 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.

about beliefs about their hypothetical UI replacement rates (European Commission 2012). The histograms in Online 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% (std. err. 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 (Online Appendix Table A.2). Last, Jäger, Schoefer, and Zweimüller (2018) document that employed workers separated in response to, and hence were aware of, UI PBD extensions in Austria.

3. *Variation in UI Generosity from an Age-Specific PBD Reform.* Online Appendix F reports the effects of reforms to the 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 based on age rather than past income, the reform was permanent (instead of 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.

V.B. *Outside Options and the Nonemployment Value* $\frac{d\Omega}{dN}$

A second reason 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.

1. *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

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

nonemployed workers exhibit larger wage-benefit sensitivities (Table V). We find some evidence for this prediction at the two-year horizon.

2. 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 firm level (in practice, in the ASSD workplace IDs often refer to establishments), as firms play an important role in Austrian wage setting (e.g., through works councils in Austria, see Section III). 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 three-digit NACE. We plot histograms of benefit variation averaged at the group level in Online Appendix Figure A.7. We adapt our worker-level regression specification (19) with the group-level treatment. We include the main controls from specification (19) and 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 report firm-level specifications with industry-by-occupation-by-year effects, and industry-by-occupation-level specifications with industry-by-year and occupation-by-year effects.

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 0 with standard errors around 0.03 and 0.04 (0.05) in the one-year (two-year) specification. Table VI also reveals pretrend 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

TABLE VI
WAGE EFFECTS: DIFFERENCE-IN-DIFFERENCES REGRESSION WITH FIRM-LEVEL VARIATION

	One-year earnings effects				Two-year earnings effects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo: 3-yr lag	-0.026 (0.030)	-0.041 (0.029)	-0.010 (0.030)	0.003 (0.030)	-0.074 (0.035)	-0.090 (0.034)	-0.020 (0.037)	-0.001 (0.037)
Placebo: 2-yr lag	-0.075 (0.024)	-0.079 (0.024)	-0.035 (0.027)	-0.034 (0.027)				
Treatment year	0.029 (0.031)	0.031 (0.031)	0.037 (0.030)	0.044 (0.030)	0.069 (0.052)	0.073 (0.051)	0.070 (0.046)	0.074 (0.046)
Preperiod <i>F</i> -test <i>p</i> -val	.005	.003	.402	.319	.035	.008	.594	.977
<i>R</i> ²	0.062	0.080	0.093	0.109	0.120	0.144	0.154	0.176
<i>N</i> (1,000s)	7,142	7,142	7,138	7,138	5,044	5,044	5,041	5,041
Mincerian controls		X		X		X		X
4-digit ind.-occ. FEs			X	X			X	X

Notes. 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 VB 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 zero in the preperiod. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being eligible for a regional PBD extension described in note 14. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue- versus 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.

industry and occupation but working at firms with different benefit shifts, we find that pretrends 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 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, that is, 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 0.10 (0.16). Specifications with fewer controls suggest larger effects, yet violate our identification assumption due to statistically significant placebo estimates in the preperiod, thus suggesting that the inclusion of control variables is important to account for, say, 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

Last, in Table V, we include a 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.

V.C. Wages and Outside Options $\frac{dw}{d\Omega}$

A final reason 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.

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 KVSsystem - Kollektivverträge Online, with best coverage around the 2001 reform), wage floors did not appear to shift differentially for treated versus untreated groups (which were instead prescribed the usual homogeneous wage increases).

TABLE VII
WAGE EFFECTS: DIFFERENCE-IN-DIFFERENCES REGRESSION WITH
INDUSTRY-OCCUPATION LEVEL VARIATION

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: One-year earnings effects						
Placebo: 3-yr lag	0.158 (0.099)	0.119 (0.098)	0.501 (0.062)	0.014 (0.109)	0.029 (0.098)	−0.011 (0.098)
Placebo: 2-yr lag	0.095 (0.077)	0.072 (0.077)	0.290 (0.047)	0.007 (0.083)	−0.009 (0.072)	−0.035 (0.073)
Treatment year	0.321 (0.076)	0.338 (0.078)	0.198 (0.083)	0.295 (0.088)	−0.094 (0.089)	−0.071 (0.087)
Preperiod <i>F</i> -test <i>p</i> -val	.281	.482	.000	.991	.898	.873
<i>R</i> ²	0.070	0.087	0.082	0.072	0.082	0.098
<i>N</i> (1,000s)	7,142	7,142	7,142	7,142	7,142	7,142
Mincerian controls		X				X
3-digit ind. FEs			X		X	X
Occ. FEs				X	X	X
Panel B: Two-year earnings effects						
Placebo: 3-yr lag	0.049 (0.074)	0.002 (0.073)	0.441 (0.075)	−0.106 (0.075)	−0.019 (0.113)	−0.051 (0.107)
Treatment year	0.138 (0.102)	0.197 (0.098)	−0.008 (0.096)	0.163 (0.096)	−0.157 (0.127)	−0.074 (0.120)
Preperiod <i>F</i> -test <i>p</i> -val	.509	.980	.000	.157	.867	.632
<i>R</i> ²	0.133	0.156	0.145	0.137	0.146	0.168
<i>N</i> (1,000s)	5,045	5,045	5,045	5,045	5,045	5,045
Mincerian controls		X				X
3-digit ind. FEs			X		X	X
Occ. FEs				X	X	X

Notes. 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 V.B 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 zero in the preperiod. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being eligible for a regional PBD extension described in note 14. The industry controls are time-varying fixed effects for each three-digit industry, and the occupation controls are time-varying indicators for a blue-versus 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.

1. 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 versus white collar). The results show no clear effect heterogeneity. Because female workers’ wages appear less sensitive to

productivity shifts (Black and Strahan 2001; Card, Cardoso, and 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).

2. *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 0 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.

3. *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, re-bargaining 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, for example, in new jobs or in growing industries. Fifth, we 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

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 United States.

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.

4. 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 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.

VI. CONCLUSION

We have studied the effects of changes in the value of nonemployment on wages brought about by reforms to 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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SUPPLEMENTARY MATERIAL

An [Online Appendix](#) for this article can be found at *The Quarterly Journal of Economics* online.

DATA AVAILABILITY

Code replicating tables and figures in this article can be found in [Jäger et al. \(2020\)](#), in the Harvard Dataverse, doi: 10.7910/DVN/GBRHTC.

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