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WAGES AND THE VALUE OF NONEMPLOYMENT

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ABSTRACT

Nonemployment is often posited as a worker's outside option in wage setting models such as bargaining and wage posting. The value of this state is therefore a fundamental determinant of wages and, in turn, labor supply and job creation. We measure the effect of changes in the value of nonemployment on wages in existing jobs and among job switchers. Our quasi-experimental variation in nonemployment values arises from four large reforms of unemployment insurance (UI) benefit levels in Austria. We document that wages are insensitive to UI benefit levels: 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. In contrast, a calibrated Nash bargaining model predicts a sensitivity of 0.39 – more than ten times larger. The empirical insensitivity holds even among workers with a priori low bargaining power, with low labor force attachment, with high predicted unemployment duration, among job switchers and recently unemployed workers, in areas of high unemployment, in firms with flexible pay policies, and when considering firm-level bargaining. The insensitivity of wages to the nonemployment value we document presents a puzzle to widely used wage setting protocols, and implies that nonemployment may not constitute workers' relevant threat point. Our evidence supports wage-setting mechanisms that insulate wages from the value of nonemployment.

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1 Introduction

How does the worker's value of nonemployment affect wage setting? A prominent view in macroeconomics and labor economics is that the nonemployment value, which includes unemployment insurance benefits, stigma, potential utility from leisure, home production, and the opportunity to move into another job, constitutes workers' outside option in wage bargaining. The value of nonemployment is thus a core determinant of equilibrium employment levels.[1] The view that nonemployment values affect wages also helps explain aggregate wage fluctuations such as the aggregate Phillips curve and cross-sectional wage dispersion such as the wage curve. The basic argument underlying these explanations is that high unemployment weakens workers' threat point: nonemployment.[2] This framework has also shaped policy debates such as whether countercyclical unemployment insurance generosity may depress labor demand by pushing up wages during recessions.[3] Nonemployment values also help determine wages in important non-bargaining wage determination models: in wage posting models, for instance, it scales the equilibrium wage distribution.[4] The theoretical degree of wage sensitivity to fluctuations in the value of nonemployment also determines the capacity of macroeconomic models to generate realistic employment fluctuations.[5] Yet, there exists no direct empirical estimate of the sensitivity of wages to the value of nonemployment, and hence no measure for the relevance of the nonemployment scenario as workers' threat point in real-world bargaining.

We estimate the dollar-for-dollar sensitivity of wages to the nonemployment value, and benchmark our estimates against predictions from calibrated wage setting models. To obtain money-metric variation in the nonemployment value, we exploit quasi-experimental reforms in unemployment insurance benefit (UIB) levels. Our benchmark is canonical Nash bargaining, where wages are the average of the job's inside value (e.g., productivity) and the worker's outside option, weighted by worker bargaining power ϕ:

$$\text{Wage} = \phi \times \text{Productivity} + (1-\phi) \times \overbrace{\text{Nonemployment Value}}^{\text{Workers' Outside Option}} .$$

Shifts in the worker outside option – such as from UIBs – should pass through into wages by one minus worker's bargaining power. We show that the calibrated bargaining model predicts

[1] Wage bargaining with nonemployment outside options is, e.g., featured in Pissarides (2000), Shimer (2010), Hagedorn and Manovskii (2008), Chodorow-Reich and Karabarbounis (2016), and Ljungqvist and Sargent (2017).

[2] Ravenna and Walsh (2008) and Christiano et al. (2016) integrate wage bargaining with nonemployment outside options into New Keynesian models. Empirical work linking wages with aggregate or local unemployment or nonemployment values includes Beaudry and DiNardo (1991), Blanchflower and Oswald (1994), Hagedorn and Manovskii (2013), or Chodorow-Reich and Karabarbounis (2016).

[3] Pissarides (2000), Krusell et al. (2010), Hagedorn et al. (2013) and Chodorow-Reich et al. (2018) examine the wage pressure channel of UI.

[4] Burdett and Mortensen (1998) and Manning (2011) are references for wage posting models.

[5] Shimer (2005), Hall and Milgrom (2008), Chodorow-Reich and Karabarbounis (2016) and Hall (2017) discuss the role of the sensitivity of the Nash wage to outside options for employment fluctuations in matching models.

that wages increase by \$0.39 whenever UIBs increase by \$1.00. This effect comes from two margins: first, while the worker is unemployed, she mechanically receives a higher UIB payment. Second, her re-employment wages also respond to the outside option boost, generating a built-in feedback effect. This logic and thus the high theoretical sensitivity hold across a broad set of model refinements of bargaining models, and are robust to incorporating various features such as finite benefit duration, to micro re-optimization such as job search choices (due to the envelope theorem) and even to market-level adjustment (netted out by a control group in the same market).[6]

We document that empirical are *insensitive* to increases in workers' UI benefit levels. Point estimates for wage-benefit sensitivity are less than 0.01 after one and two years. Our confidence intervals reject that a \$1.00 increase in UI benefits increases wages by more \$0.03 after two years. The insensitivity holds across various subsets of workers, even those with higher unemployment risk and new hires. These estimates are an order of magnitude smaller than the calibrated model predicts and thus present a puzzle to the Nash bargaining model with nonemployment as the outside option, unless one is willing to believe that workers have close to *full* bargaining power. Such an interpretation is however empirically rejected by existing small estimates of firm-level rent sharing (inside value shifts from productivity), which we argue imply worker bargaining power of around 0.1 and that we use to calibrate our bargaining model.[7] We juxtapose our implied bargaining power estimates with these values in Figure 1. Our findings imply that nonemployment is not the relevant outside option in wage bargaining, providing causally identified micro-empirical support for models that insulate wage setting from the nonemployment value or perhaps outside options more generally.

Our empirical setting is a unique set of four reforms that generated large, quasi-experimental variation in UI benefit levels in Austria, in 1976, 1985, 1989 and 2001.[8] Reforms to UI benefit levels are rarer than potential benefit duration reforms, whose effects on the nonemployment value would be harder to theoretically price. The reforms raised benefits differentially for workers based on their *previous salaries* (the UI reference wage that determines benefits in the event of a UI claim), for example by as much as 28% in 1985. Our difference-in-differences design compares wage growth between those treated workers and their unaffected peers (control group). We use administrative data on workers and firms covering 1972 through today, with daily information on UI claims and benefit receipts, as well as wage earnings and other labor force statuses.

Additional features of the Austrian UI system permit particularly clean variation in nonemployment values. First, Austrian UI affects the nonemployment value for most workers. Conditional on a separation, most workers receive UI due to broad eligibility and high take-up, due to

[6]We also show that the model makes the same predictions for the sensitivity if treatment and control workers occupy segmented markets.

[7]Manning (2011) and Card et al. (2018) review rent sharing estimates. In Appendix D, we translate the reduced-form elasticities into an upper bound for worker bargaining power.

[8]Only the 1989 reform has been studied, with a focus on unemployment spell duration (Lalive et al., 2006).

mandatory registration and because of long unemployment spells. Second, Austrian workers who *quit* are eligible for benefits – crucial for UI to indeed shift workers' threat points.[9] Third, Austrian UI does not feature experience rating. Fourth, even after UI benefit exhaustion, a significant share of Austrian workers receives means-tested benefits that, while lower in levels, move nearly one-to-one with a worker's previous UIB levels and thus our variation.

We first visually analyze each reform nonparametrically by plotting raw data. We sort workers by their UI reference wage, and then plot their wage growth before and after each reform, along with the between-worker reform-induced variation in UI benefit levels. The raw data do not reveal any wage responses even after two years – in stark contrast to the large predicted wage effects from our calibrated bargaining model that we also plot.

Next, we estimate the wage-benefit sensitivity in a regression-based difference-in-differences analysis. The point estimates are below 0.01, with confidence intervals that rule out even a $0.03 wage increase to a $1.00 benefit increase. We also formally test our identification assumptions with placebo tests and can include rich controls. By including firm-by-year effects, for instance, we leverage variation in nonemployment value shifts between workers within the same firm.

We also test a central cross-sectional prediction of the model: UI benefits raise outside options, and thus wages, by more for workers whose separation will entail longer unemployment spells. Yet, when we split up workers by the predicted time on UI post-separation, even workers in the top quintile of UI risk – whose model-predicted sensitivity would have reached nearly 0.6 – exhibit the zero wage effect. Relatedly, we also find find little evidence for larger sensitivity among workers with plausibly lower bargaining power, e.g., blue-collar or female workers, for whom a given outside option shift should have triggered larger wage effects.

We rule out a variety of confounders that could generate the wage insensitivity. First, while wage stickiness may mask wage effects in existing matches, the insensitivity extends to new hires (even those hired out of unemployment), where wages are likely set more flexibly (and allocative for hiring in standard matching models (Pissarides, 2009)). We also find no wage incidence after two years, or in firms with more flexible wage policies (with greater dispersion in wage levels and growth). Lastly, since the reforms should entail wage *increases*, downward wage rigidity should not bind.

Second, perhaps actors do not perceive UI to be a part of the nonemployment value, due to limited knowledge, attention or salience. However, our result extends even to workers with frequent interaction with the UI system, thus plausibly more aware of UI (see Lemieux et al., 1995; Lemieux and MacLeod, 2000). We also present survey evidence indicating that Austrian workers' beliefs line up with actual UI benefit levels. We additionally study sharply age-differentiated and thus arguably less complex and perhaps more salient reforms increasing potential *duration*

[9] Quits only differ from layoffs in a brief 28-day wait period with subsequently full benefit duration. In other OECD countries wait periods are considerably longer, such as three months in Germany. Quitters in the United States are de-jure ineligible for UI benefits. Hagedorn et al. (2013) discuss de-facto UI eligibility of U.S. quitters.

of UIBs, but confirm that employed workers' wages remain insensitive.

Third, we investigate whether our findings could be explained by bargaining occurring at the level of a firm's entire workforce rather than for each individual worker.[10] To test this possibility, we rerun the regressions with a firm-level average of the worker-level treatment variable. The point estimates for the wage sensitivities remain small and insignificant. Moreover, while collective bargaining is prevalent in Austria, the institutional environment leaves substantial room for idiosyncratic deviation from the collectively bargained wage floors. For example, employers regularly pay wage premia above the collective bargaining agreement wage floors and actual wages are more than a third higher than these wage floors (Leoni and Pollan, 2011). The scope for flexible wage setting is also mirrored in recent findings of large wage dispersion between firms even within the same industry in the Austrian labor market (Borovičková and Shimer, 2017).

Fourth, our evidence may imply that bargaining plays no role in wage setting. Several existing pieces of evidence stand in contrast to that interpretation. In particular, survey evidence in Hall and Krueger (2012) and Brenzel et al. (2014) as well as rent-sharing evidence is consistent with scope for bargaining in real-world wage setting. Yet, we do not find larger wage effects even in pockets of the labor market where bargaining may be more prevalent according to these surveys.

Fifth, our robustness checks also reveal that the particular UI-induced boosts in the nonemployment value we study here did not lead to larger turnover (making composition effects unlikely) or sickness spells (perhaps ruling out efficiency wage mechanisms).

Our evidence may be consistent with models with on-the-job search and job ladders, where the outside options employed workers bargain with are competing job offers rather than nonemployment (e.g., Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Altonji et al., 2013; Bagger et al., 2014). However, more nuanced predictions of those models are not borne out in our data. Specifically, unemployed entrants or recently reemployed workers – for lack of other job offers – still resort to nonemployment as outside option and therefore negotiate wages upwards when UI benefits increase, just as in our benchmark model. In the data, however, we find no evidence for positive wage effects for new hires undergoing unemployment after the reform, and no or only small responses among job stayers with recent unemployment right before the reform.

Another promising model to account for our finding is the alternating offer bargaining game by Hall and Milgrom (2008), in which the threat point is to extend bargaining rather than to terminate negotiations. In the appendix, we show that this model can be parameterized to render wages insensitive both to outside options and to inside job values (e.g., firm-level productivity), jointly consistent with the small but positive rent-sharing estimates and our findings.

Beyond bargaining models, wage posting models with pure wage dispersion (Burdett and

[10] Saez et al. (2018) find that group-specific payroll tax cuts pass through into wages as broad-based firm-level rent sharing. Worker-level wages *can* reflect idiosyncratic factors, such as group-specific marginal product shifts due to worker exits (Jäger, 2016). A long literature has documented wage differentiation between similar workers even within the same firm (Abowd et al., 1999), including in Austria (Borovičková and Shimer (2017)).

Mortensen (1998)) can yield smaller wage sensitivity to UI. But larger sensitivities can emerge in the extended models required for realistic wage distributions (e.g., with firm heterogeneity). Perhaps a frictionless labor market with market-clearing wages could rationalize our findings (but would not be a useful point of departure for other phenomenon consistent with labor market frictions that have motivated, e.g., bargaining and wage posting theories to begin with). Finally, compensating differentials may somewhat offset wage pressure and help explain wage insensitivity, exactly in jobs prone to unemployment risk.

To our knowledge, ours is the first study quantitatively assessing the effects of UI-induced outside option shifts on wage setting vis-a-vis a calibrated wage setting (bargaining) model. Our analysis complements studies of UIB duration on the job search behavior of already unemployed workers (Katz and Meyer, 1990). Besides spell duration, some of these studies also investigate potential composition shifts in the jobs unemployed job seekers accept; any shifts in the wage distribution however may combine multiple offsetting mechanisms other than bargaining, such as skill depreciation, changing job composition, stigma or statistical discrimination (Feldstein and Poterba, 1984; Kroft et al., 2013, 2016; Krueger and Mueller, 2016; Schmieder et al., 2016; Nekoei and Weber, 2017; Le Barbanchon et al., 2017). Additionally, those studies rely on variation from potential benefit duration that changes job search incentives, which would be harder to price theoretically and map back into a bargaining model.

The first implication of the empirical insensitivity of wages to UI-induced shifts in the nonemployment value is to help adjudicate between models: our evidence favors models of wage setting insulated from the nonemployment value, such as the ones described above. Second, if wages are insensitive to the nonemployment value, empirical regularities of comovement between aggregate wages and labor market conditions, such as the aggregate Phillips curve and the cross-regional wage curve, may arise from mechanisms other than the nonemployment option in bargaining, perhaps from employer competition or selection. Third, our micro-empirical evidence for the insensitivity of wages to the nonemployment value is good news for some macroeconomic debates: the *theoretical* insensitivity of wages to the nonemployment value has been recognized as a crucial ingredient in successful models of aggregate employment fluctuations. Fourth, the insensitivity of wages to UI also suggests that the wage pressure channel of UI on labor demand may be limited in the short run, although our evidence does not necessarily speak to long-run effects of policies, or to aggregate rather than group-specific shifts.

In Section 2, we derive the wage-benefit sensitivity in a calibrated bargaining model. We describe the institutions, reforms, and data in Section 3. Section 4 presents our empirical design and main results. Section 5 adds subsample analyses and tests for firm-level bargaining. In Section 6 we discuss models that may be consistent with our evidence. We conclude in Section 7.

2 Conceptual Framework

We draw on wage bargaining to conceptualize the effect of outside options on wages and to derive a theoretical benchmark to guide our empirical design. Our point of departure is Nash bargaining with the outside option that may involve a brief spell of nonemployment before reemployment. In that model, we derive the theoretical sensitivity of the wage to UI benefits. We calibrate it by arguing that existing micro estimates of firm-level rent sharing imply low bargaining power – and thus high wage sensitivity to outside options. Our calibrated wage-benefit sensitivity is 0.39: when UI benefits – or any component of nonemployment payoff – go up by \$1.00, wages should increase by \$0.39. The first mechanism by which benefits affect wages is by mechanically increasing the payoff while actually nonemployed. Second, when reemployed, wages in the next job will have shifted, too, generating a feedback effect built into the bargaining model by further increasing worker's outside option. In Section 2.2 and Appendix Section C.1, we show that the predictions are robust to rich micro choice variables (e.g. job search effort), equilibrium market adjustment, and richer specifications of the nonemployment payoff, and to incorporating institutional features such as finite benefit duration. A natural question is which kinds of bargaining models break the link between nonemployment values and wages, which we discuss in Section 6.

2.1 Wage Bargaining, the Nonemployment Value and UI Benefits

Most jobs carry strictly positive joint surplus, due to relationship-specific investments, or hiring or firing costs. Here, a variety of wages would implement the bilaterally efficient allocation of employment: the worker would accept any wage of at least her outside option, and the firm would accept any wage up to the productivity of the worker. Bargaining models pin down a wage bargain w within the interval of the worker's and firm's reservation wages (the bargaining set).

Nash wage. The Nash wage, derived in detail in Section 2.2, is the weighted average of the inside value of a job, here productivity p, and the worker's outside option Ω, weighted by worker bargaining power ϕ (alternatively w will equal outside option Ω plus share ϕ of surplus $p - \Omega$):

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

Hence the wage exhibits a ϕ-sensitivity to p, and a $(1 - \phi)$-sensitivity to outside option Ω. If workers wield full bargaining power, wages are insulated from the outside option (as long as job surplus remains positive). If workers' bargaining power is zero, they are paid exactly their outside option, such that wages move one to one with the outside option. In principle, wage responses to suitable variation in the correctly specified outside option would therefore identify one minus worker bargaining power. In this paper, we test for a prediction using quasi-experimental, micro-level shifts in workers' outside options.

The nonemployment outside option. Our variation in the outside option is brought about by variation in the workers' payoff while nonemployed, specifically shifts in UI benefit levels. This specification of the outside option is consistent with the canonical specification of Nash bargaining in, e.g., matching models, where job surplus is the difference between worker productivity and nonemployment value N (e.g., Pissarides, 2000; Shimer, 2005; Chodorow-Reich and Karabarbounis, 2016; Ljungqvist and Sargent, 2017). That is, if the worker were to take the outside option, she may incur at least a brief spell of nonemployment (value N) during which she collects instantaneous flow payoff b, the unemployment insurance benefit. The duration of this nonemployment spell – and thus the relevance of b in the outside option – depends on job finding rate f. The flow value of nonemployment is therefore (in continuous time, with discount rate ρ):

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

When finding reemployment at wage w', the worker obtains value $E(w')$, which incorporates the movement back into nonemployment due to the separation rate δ: $\rho E(w') = w' + \delta(N - E(w'))$. Workers that quickly find jobs, with high f, have low time in nonemployment (and on UI) and quickly move back into reemployment E, such that $\lim_{f\to\infty} N = E(w')$. The flow value of nonemployment is accordingly the amortized expected value of the instantaneous payoffs from nonemployment b and reemployment w':

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

Time spent in nonemployment τ. Payoffs b and w' are weighted by the expected time the worker will spend in the nonemployment state $\tau \equiv \frac{\rho+\delta}{\rho+f+\delta}$, conditional on a separation from the current employer, discounted at rate ρ. τ has an intuitive role and properties. A worker with a high discount rate $\rho \to \infty$ (e.g., due to myopia or liquidity constraints), or a worker with a low job finding rate $f = 0$ or with a high subsequent separation rate $\delta \to \infty$ will put full weight on b such that $\tau = 1$ and $\rho N = b$ (her initial state after bargaining breaks down). A worker with a high job finding rate $f \to \infty$ will have $\rho N = w'$.

A convenient approximation exploits the fact that the discount rate ρ is very small compared to empirical worker flow rates f and δ, suggesting a convenient approximation of $\rho = 0$, and thus

$\tau \approx \frac{\delta}{\delta+f}$.[11] This ratio corresponds to the steady-state expression for *aggregate* unemployment rate with long-term jobs and unemployment spells. In our empirical analysis we find that $\tau \approx 7\%$ is the median employed workers' predicted time on UI receipt conditional on a separation; we present details of this calculation in Section 4.4. This calculation takes into account the fact that unemployment spells may affect the separation rate in future jobs. Thus, 7% will be our benchmark calibration for Austria.

The wage-benefit sensitivity holding fixed all non-wage terms. With $\Omega = \rho N$ and plugging in the expression for ρN derived in (3), the Nash wage becomes a function of the discounted time in nonemployment τ vs. in re-employment $1-\tau$, instantaneous payoffs b during nonemployment and that during re-employment w', and worker bargaining power ϕ:

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

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

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

Decomposing the wage response. First, b has a *mechanical* effect on the instantaneous payoff while nonemployed and hence is weighted by τ, the time nonemployed after taking up the outside option and separating. On its own, this effect is modest for the median worker ($\tau = 0.07$), though higher for workers with high unemployment risk.

Second, weighted by $1-\tau$, the *feedback effect* $\frac{dw'}{db}$ captures the fact that reemployment wages in subsequent jobs will also respond to the shift in the outside option, therefore affecting the payoff in the $1-\tau$ fraction of post-separation time the worker will be re-employed. Our benchmark model has Nash bargaining naturally also determine these re-employment wages, such that $dw' = dw \Rightarrow \frac{dw'}{db} = \frac{dw}{db}$, implying a simple fixed point of the wage sensitivity. Our structural wage-benefit sensitivity characterizing the outside option channel is therefore:

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

[11]For example, in the United States, monthly separation rates are around 3% and monthly job finding rates are around 45%. The wage-benefit sensitivity is *increasing* in ρ, implying that our calibrated model *understates* the predicted benefit-wage sensitivity if calibrated τ is to be interpreted as time in nonemployment. This is because the worker is initially unemployed and puts, for any $\rho > 0$, more weight on that first period than the future.

Vice versa, Equation (6) implies a worker bargaining power ϕ for any wage-benefit sensitivity:

$$\phi = \frac{1 - \frac{dw}{db}}{1 + \frac{dw}{db} \cdot (\tau^{-1} - 1)}. \tag{7}$$

Intuitions from contour maps. We provide intuitions for the relationship between τ, ϕ, and $\frac{dw}{db}$ in two contour maps. Figure 2a traces out the relationship between worker bargaining power ϕ and the predicted wage-benefit sensitivity, for various levels of τ. The higher job finding rate f and thus the lower τ, the lower a weight the outside option puts on the UI benefit b. As a consequence, wages become insulated from b, reducing $\frac{dw}{db}$ to zero for $\tau = 0$ for any ϕ. By contrast, for $\tau = 1$, e.g., because $f = 0$ or $\delta \to \infty$, the outside option becomes $\Omega = \rho N = b$. The wage sensitivity then becomes $\frac{dw}{db} = 1 - \phi$. Therefore, for a given ϕ, $\frac{dw}{db} \in [0, 1 - \phi]$.

Figure 2b plots the wage-benefit sensitivity as a function of post-separation time in nonemployment τ, for various levels of worker bargaining power ϕ. The higher τ, the more weight the instantaneous payoff while nonemployed, b, receives. Naturally, for $\tau = 0$, the sensitivity is zero no matter the bargaining power. For $\tau = 100\%$, the sensitivity is equal to $1 - \phi$. The lower the bargaining power of the worker, the steeper the effect of τ on the wage sensitivity. That is, for a given τ, $\frac{dw}{db} \in [0, 1]$ depending on ϕ: if $\phi = 1$, the wage is always insulated from the outside option and thus $\frac{dw}{db} = 0$ independently of τ; if $\phi = 0$, the wage is always equal to the outside option, and thus $\frac{dw}{db} = 1$ for any $\tau > 0$.

Calibrating ϕ to micro-estimates of rent sharing. We now calibrate ϕ (having already set $\tau \approx 0.07$ for the median Austrian worker). We plot potential calibration targets for worker bargaining power ϕ in Figure 1. First, macroeconomic calibrations often treat ϕ as a free parameter and provide little empirical guidance, being set to fulfill the Hosios condition of constrained efficiency in matching models.[12]

Second, we argue that micro-empirical rent-sharing estimates do provide direct calibration targets for ϕ, since Nash bargaining also prescribes a tight link between *inside* value (proxies in the data: profits and productivity) and wages, guided by ϕ. In Figure 1, we plot the implied worker bargaining power parameters from a meta study of the pass-through of firm-specific shifts in productivity of labor p into worker's wages, stemming from a large body of "rent sharing" estimates in labor economics (e.g., Manning (2011) and Card et al. (2018) review the empirical literature) as well as our own calculation based on Austrian data. In Appendix D, we derive and discuss the structural interpretation of the wage-labor productivity elasticity in the context of Nash bargaining models. We show that a given rent sharing (elasticity) estimate is an *upper*

[12] In their textbook, Cahuc and Zylberberg (2004) summarize macroeconomic calibrations of worker bargaining power as follows: "We do not have at our disposal a reliable order of magnitude representing the bargaining power of workers γ. [...] The usual procedure is to assume that [bargaining power parameter] γ is equal to the elasticity of the matching function with respect to the unemployment rate."

bound for ϕ. Specifically, the average of the studies with the most fine-grained variation and outcome data, i.e. those with firm-level rent variation and worker-level measures of wages, is 0.104, hence our calibration target. Under the assumption of Nash bargaining by which $\frac{dw}{d\Omega} = 1 - \frac{dw}{dp}$, these small rent sharing estimates therefore directly imply large sensitivity of wages to outside option shifts.

Benchmark for the wage-benefit sensitivity. For $\phi = 0.1$, suggested by the micro studies on rent sharing, and a 7% post-separation "unemployment rate" as in our data, the predicted wage–benefit sensitivity is:

$$\left.\frac{dw}{db}\right|_{(\tau=0.07,\phi=0.1)} = (1 - 0.1) \cdot \frac{1}{1 + 0.1\left(0.07^{-1} - 1\right)} \approx 0.39. \tag{8}$$

That is, a \$1.00 increase in UI benefits should entail a \$0.39 increase in wages according to the calibrated Nash bargaining model. This value will form our theoretical benchmark. Even for larger bargaining power parameters from the upper end of the rent sharing estimate such as $\phi = 0.2$, the prediction would imply a 0.22 sensitivity. Even for $\phi = 0.5$ – far above the micro-empirically plausible estimates though in the middle of the macro calibration targets that are not disciplined by empirical estimates – the model would predict a sizable sensitivity of 0.07. Finally, we will also study worker subgroups with larger τ and predicted $\frac{dw}{db}$ of almost 0.6.

2.2 Full Model: Equilibrium Adjustment and Micro Reoptimization

Wage-benefit sensitivity (6) explicitly holds fixed all elements of the values except for wages and UI benefits. Next, we present a fuller model to show that the structural benefit-wage sensitivity is robust to richer components of the payoff during nonemployment, to micro-reoptimzation of choice variables such as endogenous search effort, as well as to equilibrium market adjustment. First, we apply the envelope theorem argument, clarifying that micro-level reoptimization is irrelevant for dN/db. Second, we clarify how a control group in the same market (yet not receiving the change in benefits itself) will net out market level responses such as those in f, δ or productivity p, and any other market-wide responses not explicitly showing up in the simple model. We relegate additional robustness checks to Appendix Section C.1.

2.2.1 Wage Nash Bargaining and Outside Options

Defining the outside option. First we define the household's outside option, i.e. leaving the current job. The flow value from this outside option consists of the (now more general) instantaneous payoff z of nonemployment, as well as the capital gain associated from moving into re-employment in another job at rate f, which provides value E:

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

$\mathbf{c}$ denotes a vector of choice variables of the household such as search effort, consumption and asset allocation choices. w' is the re-employment wage the worker would obtain if separating and moving into another job. $\mathbf{x}$ denotes exogenous variables the household takes as parametric, such as market-level outcomes.

Richer payoff from nonemployment $z(b)$. The instantaneous payoff from nonemployment consists of a variety of terms: the UI benefit, the extensive margin analogue of the marginal rate of substitution (the value of leisure vs. employment at counterfactual hours h, normalized by budget multiplier λ), potential search effort costs $c(e)$, stigma from nonemployment γ, and other nonemployment-contingent income sources y:

$$z(b_i, ...) = b_i + \frac{v_i(h = 0) - v_i(h > 0) - c_i(e_i) - \gamma_i}{\lambda_i} + y_i + \dots . \tag{10}$$

While a shift in any component of this flow value z would shift the value of nonemployment and thus the worker's outside option in bargaining, our strategy is to use directly quantifiable, money-metric variation in b_i. Specifically, we will derive a structural wage-benefit sensitivity that will be in levels (rather than an elasticity) and directly constitute our treatment effect estimating equation. As a result, we do not need to know the *share* $\frac{b}{z}$ and thus our strategy is robust to a variety of additional components in z besides b.[13] Broad eligibility implies $\frac{\partial z}{\partial b} \approx 1$; we evaluate robustness to limited take-up in Appendix C.1.

Employment. When employed at wage w, the household's flow value is the wage plus the capital gain (loss) of moving into nonemployment at rate δ:

$$\rho E(w, b, \mathbf{c}, \mathbf{x}) = \max_{\mathbf{c}} \{ w + \delta(\mathbf{c}, \mathbf{x}) \cdot [N(b, \mathbf{c}, \mathbf{x}) - E(w, b, \mathbf{c}, \mathbf{x})] \}. \tag{11}$$

The employing firm receives value J from the labor productivity p and pays wage w:

$$\rho J(w) = p - w + \delta \cdot [V - J(w)]. \tag{12}$$

where we take p is parametric and fixed; moreover we suppress choice variables for the firm. The value of a vacant job V (or one fewer worker in the firm) need not be specified as long as treated

[13] While such additional components of z may needed to rationalize reservation wage behavior of unemployment job seekers by means of *low* $z < b$ (Hornstein et al., 2011) or fluctuations in aggregate employment by means of *high* $z > b$ (Ljungqvist and Sargent, 2017), our design is robust to the baseline *level* of z. Chodorow-Reich and Karabarbounis (2016) discuss empirical measurement of average z over the business cycle.

and control worker groups (which we discuss subsequently) are in the same market or as long as free entry pushes V to zero.

Nash bargaining. The firm and the worker maximize the geometric average of worker surplus $E - N$ and firm surplus $J - V$, weighted by worker bargaining power $\phi \in (0,1)$, using wage w to transfer utility. The Nash wage w is the solution to this maximization problem:

$$w = \arg\max_{w^N} \left(E(w^N) - N\right)^{\phi} \left(J(w^N) - V\right)^{1-\phi}. \tag{13}$$

$$\Rightarrow (1-\phi)(E(w) - N) = \phi(J(w) - V). \tag{14}$$

Plugging in worker employment value (11) and firm job value (12):

$$\Leftrightarrow (1-\phi)\left[\frac{w + \delta(N - E(w))}{\rho} - N\right] = \phi\left[\frac{p - w + \delta(V - J(w))}{\rho} - V\right]. \tag{15}$$

Using the fact that $(1-\phi)\delta(E(w) - N) = \phi\delta(J - V)$ due to bargaining next period eliminates continuation terms and returns the Nash wage as weighted average of the inside value of the job p and the flow value of the outside options:

$$w = \phi p + (1-\phi)\rho N - \phi\rho V. \tag{16}$$

Again, the wage bargain exhibits a $\frac{dw}{dp} = \phi$ sensitivity to shifts in the inside option, and a $\frac{dw}{d(\rho N)} = 1 - \phi$ sensitivity to shifts in the worker outside option.[14]

2.2.2 The Effect of UI Benefits on the Outside Option

Next we derive the effect of b on the value of nonemployment as the outside option in bargaining. The flow value of nonemployment is an equilibrium term that can be affected by b through various channels. Specifically, we group the total derivative of Equation (9) with respect to b, defining N, into four effects:

$$\frac{dN}{db} = \overbrace{\underbrace{\overbrace{\frac{\partial N}{\partial b}}^{=\tau}}_{\substack{\text{Mechanical}\\ \text{Effect}}} + \underbrace{\overbrace{\frac{\partial N}{\partial w'}}^{=1-\tau} \frac{dw'}{db}}_{\substack{\text{Feedback of}\\ \text{Wage Response}}}}^{\text{Calibration}} + \overbrace{\underbrace{\nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_b \mathbf{c}^*}_{\text{Micro Re-optimization}}}^{=0 \text{ By Envelope Theorem}} + \overbrace{\underbrace{\nabla_{\mathbf{x}} N \cdot \nabla_b \mathbf{x}}_{\text{Market Adjustment}}}^{\substack{\text{Net Out with}\\ \text{Control Group}}}. \tag{17}$$

where $\nabla_{\mathbf{a}} f(\mathbf{a}, \mathbf{b})$ denotes the gradient of $f()$ over the subset of arguments vector $\mathbf{a}$.

The first two terms mirror the ones in the simple model presented in the previous section:

[14] Below, we either rely on canonical free entry ($V = 0$), or that a control group will net out shifts in V.

the first partial effect captures the mechanical change in the instantaneous payoff while nonemployed.The second partial effect captures the feedback effect from re-employment wages also responding to the outside option change, weighted by time re-employed post-separation, $1-\tau$.

Next, we show that, despite the two additional terms, the basic wage sensitivity expression (6) continues to *precisely* hold for this richer context.

Envelope theorem and the irrelevance of micro reoptimization. The third, new term captures the potential reoptimization of the agent's choices $\mathbf{x}$ following the benefit changes. We apply the *envelope theorem* to clarify that those micro behavioral responses can be ignored by the Nash bargain. To see this, note that value of the outside option takes into account that in subgame perfect equilibrium, the agent will maximize N if entering that state, such that for any original level of b, the following vector of first-order conditions holds for a nonemployed worker:

$$\nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) = \mathbf{0}. \tag{18}$$

Any reoptimization of choices in response to shifts in the environment occur in the neighborhood of this optimum. In consequence, *re-optimization* in response to a small shift in b will not trigger first-order effects on the nonemployment value:

$$\Rightarrow \nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_b \mathbf{c}^* = \mathbf{0}. \tag{19}$$

This application of the envelope theorem implies that, from a value perspective, a calculation of the effect of the benefit changes on the value of the outside option can ignore the effect of behavioral responses to the benefit change at the worker level. This result allows us to sidestep a wide and rich class of complex responses to UI shifts documented in the existing literature, such as in job search effort, reservation wage behavior, liquidity effects or human capital depreciation during unemployment, and even switching between program substitutes and UI take-up of UI. The appeal to the envelope theorem does *not* require that behavior remain unchanged. The result is stronger: even if adjustment occurs, the adjustment does not incur first-order changes in value N.[15]

Netting out market-level effects with a control group. The fourth, new term captures shifts in factors $\mathbf{x}$ that the individual agent takes *as given*. These factors include labor demand responses affecting the job finding rate f, shifts in labor market tightness due to some workers entering the labor force or changing job search effort, aggregate wage shifts, or responses in social insurance programs due to, e.g., congestion. Thanks to the linearity of the wage bargain,

[15] Additionally, if the change in b is not marginal so the envelope theorem does not apply, the effect of b on N will be even larger (because reoptimization can only increase the value of nonemployment). In this case the envelope theorem assumption again leads to a conservative lower bound for $\frac{dw}{db}$.

we net out such effects with a suitable *control group* in the same market, and thus exposed to the treatment exclusively through market effects. Consider two types of households defined by group $g(i) \in \{T, C\}$, the treatment group for whom $db^T > 0$ and the control group for whom $db^C = 0$. T and C operate in the same market $m(T) = m(C)$. Market-level variables (which the household takes as parametric) are $\boldsymbol{\mu}^m$, such as the job finding rate, or other mechanisms that would affect all agents in a given market. $\boldsymbol{\iota}^i$ are worker- or type-specific factors (and thus differ between T and C) that the worker takes as parametric but that may be affected by the exposure to the treatment. In fact, $\frac{dw^i}{db^g}$ is one particular element of ι^i that we have previously separated from $\mathbf{x}$:

$$N^i(b^{g(i)}, \mathbf{c}^i, \underbrace{\boldsymbol{\iota}^i, \boldsymbol{\mu}^{m(g(i))}}_{\mathbf{x}^i}). \tag{20}$$

The treatment group is exposed to *all* channels of treatment db^T (where we have excluded micro reoptimization terms $\nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_b \mathbf{c}^*$ due to the previous envelope theorem argument):

$$\frac{dN^T}{db^T} = \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \frac{dw'^T}{db^T} + \nabla_{\boldsymbol{\iota}} N \cdot \nabla_b \boldsymbol{\iota}^T + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^{m(T)}. \tag{21}$$

The control group is exposed to db^T only through market-level effects and own-wage spillovers:

$$\frac{dN^C}{db^T} = \frac{\partial N}{\partial w'} \frac{dw'^C}{db^T} + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^{m(C)}. \tag{22}$$

Our difference-in-differences strategy nets out market-level effects and thereby isolates micro effects by comparing the treatment and control group (exposed to the treatment db^T only indirectly through market-level effects):

$$\frac{dN^T}{db^T} - \frac{dN^C}{db^T} = \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \cdot \left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] + \nabla_{\boldsymbol{\iota}} N \cdot \nabla_b \boldsymbol{\iota}^T. \tag{23}$$

Potential confound: group-specific effects beyond wages. Bias in our empirical strategy would arise from micro, or group-specific elements of elements of $\boldsymbol{\iota}$ that adjust to b. An example is $z = b + x(b)$ with $x'(b) \neq 0$, as would arise from the crowd-out of transfers by UI benefit increases (e.g., means-tested UI program substitutes), or from statistical discrimination in hiring by treatment status, or from changes in social stigma changing with benefit levels. Or, workers' higher benefit level may increase the credit worthiness of treated workers and thereby help smooth consumption. We cannot evaluate the sign or size of such effects, and thus must ignore them going forward.

Potential confound: treatment and control group in different markets. Another potential source of bias is that our control group may not be in the exact same labor market as the treatment group. We tackle this concern in four ways. First, in our empirical analysis in Section 4, we first plot raw data of wage growth for a continuum of worker groups sorted by income. This allows the reader to visually inspect whether more closely related control groups exhibit wage differentials reflecting potential spillovers. Second, in our regression framework, we will add rich year- and group-specific fixed effects such that, in our most granular specification, including firm-by-year fixed effects, we conduct *within-firm* difference-in-differences analyses by comparing two colleagues in the same firm making up the treatment and control groups. Third, while our benefit level reforms are income-specific, in Appendix H we provide an additional difference-in-differences design that exploits sharp segmentation of treatment and control groups by *date of birth*. There, workers below and above the sharp threshold are plausibly perfect substitutes in a market. Fourth, in Appendix Section C.1, we show that even if markets are perfectly segmented, *market-level* wage-benefit sensitivities turn out to be similarly large in calibrated equilibrium (DMP) models with Nash bargaining – and follows almost exactly the same model structure as the micro sensitivity.[16]

The difference-in-differences effect of benefits on the nonemployment value. Thanks to the envelope theorem declaring micro reoptimization irrelevant and a control group netting out market-level adjustment, we have now reduced the differential effect of benefit changes on the outside option to two partial derivatives capturing solely shifts in the instantaneous payoffs triggered by the benefit shifts and the feedback effect on reemployment wages:

$$\overbrace{\frac{d(\rho N^T)}{db^T} - \frac{d(\rho N^C)}{db^T}}^{= \frac{d\Omega^T}{db^T} - \frac{d\Omega^C}{db^T}} = \frac{\partial(\rho N)}{\partial b} + \frac{\partial(\rho N)}{\partial w'} \cdot \left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T}\right]. \tag{25}$$

Using the definition of N in terms of τ from Equation (3), we can express the partial derivatives in terms of τ (using $\partial z^T/\partial b^T - \partial z^C/\partial b^T = 1$) and $(1-\tau)$ times the wage-response differential:

$$= \tau + (1-\tau) \cdot \left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T}\right]. \tag{26}$$

[16]The DMP equilibrium expression for the wage-benefit sensitivity we derive in Appendix Section C.1 is:

$$\frac{dw^{\text{DMP}}}{db} \approx \frac{1-\phi}{1+\phi \cdot \frac{1}{\eta} \cdot (u^{-1}-1)} \approx 0.32, \tag{24}$$

where $u \approx 0.07$ (for consistency with our micro calibration of τ) now denotes the market-level unemployment rate (since ρ is small compared to worker flow rates) and η is a parameter in the DMP matching function, which is around 0.72 (e.g. Shimer (2005)).

where again $\tau = \frac{\rho+\delta}{\rho+\delta+f}$.

2.2.3 Difference-in-Differences Version: The Effect of Benefit Changes on Wages

We have now derived the effect of benefit changes db^T on the nonemployment value of T and C. Since the wage bargain exhibits a $(1-\phi)$-sensitivity to shifts in the worker outside option, $\frac{dw}{db} = (1-\phi)\frac{d(\rho N)}{db}$, the following estimating equation relating our difference-in-differences treatment effect to the general bargaining equation:

$$\frac{dw^T}{db^T} - \frac{dw^C}{db^T} = (1-\phi)\rho\left[\frac{dN^T}{db^T} - \frac{dN^C}{db^T}\right], \tag{27}$$

$$= (1-\phi)\left(\tau + (1-\tau)\left[\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T}\right]\right). \tag{28}$$

Using that $\frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} = \frac{dw^T}{db^T} - \frac{dw^C}{db^T}$ due to Nash bargaining in the next job too for both groups, we again obtain the fixed point pinning down the wage sensitivity we take to the data:

$$= (1-\phi)\frac{\tau}{1-(1-\phi)(1-\tau)} = (1-\phi)\cdot\frac{1}{1+\phi\left(\tau^{-1}-1\right)}. \tag{29}$$

The difference-in-difference wage-benefit sensitivity *exactly* mirrors the one simple model in Equation (6) that held fixed non-wage variables. Hence the calibration, to 0.39, carries over.

2.3 Further Theoretical Robustness

In Appendix Section C.1, we present a series of robustness checks that confirm that the large theoretical sensitivity of wages to UI-induced shifts in the nonemployment value are robust when we relax assumptions of our benchmark model. We allow for finite benefit duration, limited take-up, wage stickiness, liquidity constraints/myopia, segmented markets between treatment and control groups with equilibrium effects, the non-taxation of benefits, and discuss on the job search and endogenous separations, individual households with risk aversion, and bargaining with multi-worker firms. In Section 6, we discuss wage setting models with lower wage-benefit sensitivities, which may be consistent with our findings.

3 Institutional Context, Reforms, and Data

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

3.1 Wage Setting in Austria

Even in the presence of central bargaining, wage setting institutions in Austria leave substantial scope for flexible wage setting at the establishment level and bargaining between individual workers and firms. About 95% of Austrian workers are covered by a central bargaining agreements (CBA) regulating working hours, working conditions, and wage floors (Bönisch, 2008). Importantly, the CBAs – which are negotiated between unions and employer associations at the industry level – only set wage *floors*, and additional negotiations at the establishment level as well as bilateral negotiations between workers and firms regularly lead to substantially higher wages within specific firms.[17] Even in the early 1980s, actually paid wages were, on average, 34% higher than the wage floors negotiated in the industry-level CBAs (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 wage flexibility (Hofer et al., 2001). We also find direct evidence consistent with rent-sharing at the firm level in Austria, as higher value-added per worker is associated with higher wage costs per worker even when controlling for firm and industry-by-year effects.[18] These findings also mirror recent work by Borovičková and Shimer (2017), who document large wage dispersion between firms even within the same industry. Finally, comparative work has found lower prevalence of downward nominal wage rigidity in Austria compared to, e.g., the United States (Dickens et al., 2007). Importantly, the variation we exploit primarily consists of benefit increases that would trigger wage *increases*, which downward nominal wage rigidity should not mute. Finally, as robustness checks, we additionally study wage responses to firm-level treatment definitions and we zoom in on firms with particularly flexible wage policies or from industries with high growth rates.

3.2 Unemployment Insurance in Austria

UI benefit schedules. The Austrian UI system assigns benefit levels to granular income bins, which we refer to as reference wages.[19] We describe the reference wages and discuss our calculation in Section 4.1, and in Appendix E verify that our measured income concept from the administrative data accurately predicts benefit level receipts. We provide an overview of UIB schedules from 1976 to 2003 in Appendix Figure B.1, plotting benefits as a function of gross income. At the beginning of our sample period, the replacement rate was 41% for individuals

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

[18] 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. We find a statistically significant, positive coefficient of 0.046 (se 0.009) in a level on level specification. See Figure 1 for a comparison of the magnitude to coefficients from other settings.

[19] There are supplemental UI benefits based on the number of dependents (e.g., EUR 29.50 per month in 2018). These are not dependent on income and thus orthogonal to our variation.

above a minimum benefit level and below a maximum benefit level. By 2001, the net replacement rate, $\frac{b_i}{(1-\tau_i)w_i}$, had increased to 55 %. Before 2001, the benefit schedule was based on gross income. UIBs are not taxed, and not means-tested, but benefit recipients are required to search for employment relevant to their qualifications.

Potential benefit duration. The PBD determines the maximum number of weeks someone can receive UI benefits.[20] Individuals whose UI benefits have expired or who were initially ineligible can apply for means-tested transfer payments (*Notstandshilfe*, i.e. unemployment assistance (UA)). Importantly, these UA benefit levels are indexed to a worker's pre-exhaustion UIB levels almost one to one, leaving post-UI benefits sensitive to UIB reforms.[21]

Quitters are eligible for UI. An important feature of the Austrian UI system is that workers that unilaterally quit their job are eligible for UI benefits. By contrast, in the United States quitters are de-jure ineligible for UI. Compared to other European countries, the Austrian UI system features the shortest wait period of four weeks.[22] Quitters' UI eligibility is crucial for the mapping between model and empirics: it ensures that our particular variation indeed shifts most Austrian incumbent workers' nonemployment outside options.

Take-up is high. As a consequence of broad eligibility, relatively long benefit durations, and mandatory registration with the UI agency (for continuity of health insurance coverage), take-up of UI benefits is high in Austria. Most workers who separate will take up UI. Table A.1 reports take-up rates after transitions from employment into nonemployment. We find that 63.8% of nonemployment spells longer than 14 days lead to take-up of UI, the corresponding fraction is 67.4% when we focus on nonemployment spells longer than 28 days. Hence the Austrian setting contrasts with, e.g., the United States, where low eligibility and take-up potentially attenuate the role of UI benefits in the nonemployment value (Chodorow-Reich and Karabarbounis, 2016).

[20] Before 1989, PBD was experience- and not age-dependent. Individuals with less than 12 (52, 156) weeks of UI contributions in the last two (two, five) years were eligible for 12 (20, 30) weeks. Starting in 1989, individuals with sufficient experience aged 40-49 (above 50) were eligible for 39 (52) weeks. An additional regional reform, the Regional Extended Benefit Program, in place from 1988 to 1993, extended benefit duration to 209 weeks for workers that met three criteria: (i) age 50 or older, (ii) 780 employment weeks during the last 25 years prior to a UI claim, and (iii) residence in one of 28 labor market districts (see Jäger et al. (2018) for more details).

[21] UA benefits are capped at 0.92 of the worker's UI benefits. Importantly, for uncapped workers, UA benefits shift 0.95 to one with the worker's UIB level. The precise formulate is $\text{UAB}_i = \min\{0.92b_i, \max\{0, 0.95b_i - \text{Spousal Earnings}_i + \text{Dependent Allowances}_i\}\}$. Due to the spousal earnings means test, not all workers are eligible for UA. For 1990, Lalive et al. (2006) report that median UA was about 70 % of the median UIB. Based on data from 2004, Card et al. (2007) gauge the average UA at 38 % of UIB for the typical job loser.

[22] For instance, the wait period to claim UI benefits after a quit without cause 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.

Financing of benefits. There is no experience rating in Austria. UI benefits are financed by a payroll tax roughly split between the employer and the employee.

3.3 Four Large Reforms to the UI Benefit Schedule

A key motivation to study the Austrian setting is the unique variation from quasi-experimental reforms to UI benefit levels that directly map into the calibrated model. For our empirical analysis, we focus on four particularly large *increases* in benefit levels in sharply defined segments of the earnings distribution. Figure 3 provides an overview of the schedule changes, and we describe each reform below. Throughout, we report benefits and earnings in nominal Austrian shillings (ATS), the currency until 1999, when it was replaced by the euro at a rate of 13.76 to 1.

In each panel of Figure 3, we plot the new schedule induced by the reform and compare it to the most recent pre-reform schedule in the previous year. Panel (e) in Figure 3 plots the four reforms together in contemporaneous earnings percentile space. It shows that the reforms affected a wide range of earnings percentiles. In June **1976**, a reform was enacted that increased the replacement rate in the lower part of the earnings distribution. The maximum increase was 21.7ppt, among the lowest earners (Figure 3 (a)). The reform primarily raised unemployment benefits below earnings of 3,700 ATS (7th percentile). The reform left replacement rates largely unchanged for workers with wages above the 13th percentile.[23] In January **1985**, the maximum monthly UI benefit increased by 29% from around 7,600 ATS to around 9,800 ATS. Figure 3 (b) shows that this increase in the cap caused an increase in the replacement for individuals above the 61st percentile. This resulted in a replacement rate increase for these individuals of up to 8 ppt.[24] On August 1st, **1989**, reforms were enacted that increased benefits for low earners, depicted in Figure 3.(c).[25] Specifically, for individuals with previous monthly earnings between 5,000 and 10,000 ATS, i.e. the 19th percentile, the replacement rate increased by up to 7.4 percentage points. This increase then phased out for individuals earning between 10,000 and 12,610 ATS. For individuals with monthly earnings between 5,000-10,000 ATS, this reform corresponded to around a 15% increase the monthly UI benefit.[26] In January **2001**, a benefit

[23] Another reform in January 1976 raising the maximum benefit level, alas in the higher parts of the wage distribution that had previously experienced a benefit reform, so we cannot study it and restrict our sample, to the first reform. That reform also entailed a small 2 percentage point increase in benefits in both our treatment and control group, which will be netted out by our difference-in-differences design.

[24] Such shifts in the nominal maximum benefit level frequently occurred. The typical reform is not suitable for identification because these reforms occurred closely to each other, were small (inflation catch-up) and affected similar earnings percentiles, preventing a clean difference-in-difference design. The 1985 reform was particularly large and was not preceded by substantial extensions in the previous year.

[25] A 1989 reform additionally increased the PBD for older employees with sufficient work experience. Additionally, in June 1988, Austria enacted a Regional Extension Benefit Program (REBP) with a large extension of potential benefit duration for certain older workers who lived in regions affected by a declining steel industry (Lalive et al., 2015). Since these reforms concerned other dimensions of the system and were age- but not income-specific, we can account for them by with appropriate controls.

[26] Subsequently in June 1990, an additional reform raised benefits and gradually phased out between 10,000

reform switched the UI reference wages to *net* wages. Between a minimum and maximum benefit level, base benefits were 55% of net earnings. Before 2001, benefits were based on *gross* wage earnings. The income tax schedule generated tremendous variation in benefits, in particular for lower earners below the 32nd percentile of the earnings distribution. Figure 3 (d), cast in terms of gross earnings, illustrates this variation at the lower part of the earnings distribution.

We selected large reforms that occurred in parts of the wage distribution that did not experience prior reforms in the years before so that we can cleanly test for pre-trends.[27] We confirmed that the timing as well as the underlying reasoning for the reforms seemed to be largely unrelated to labor market conditions. For example, the 2001 reform was meant to simplify the benefit schedule by implementing a uniform 55% net income replacement rate (while previous benefits where based on gross income). The 1985 reform was enacted to catch up with past inflation, which had eroded the real maximum benefit level.

3.4 Administrative Data

Our primary data source is the Austrian Social Security Database (ASSD), described in Zweimüller et al. (2009). It provides day-specific labor force status and average daily earnings ("wages") for all private-sector and non-tenured public sector employees from 1972 onward. It excludes tenured public sector workers, the self-employed, and farmers. Earnings are censored at the annual social security contribution caps (see Zweimüller et al., 2009).[28] We harmonize the cap at the lowest prevailing earnings percentile in the set of years of each reform. The ASSD includes covariates such as gender, age, citizen status, and a white/blue collar indicator, and establishment ("firm") location and detailed industry. We also draw on UI registry data (AMS), with which we validate our prediction of actual benefits based on lagged earnings (Appendix Section E.2).

4 Quasi-Experimental Evidence on Wage Effects of UI Benefits From Four Large UI Reforms in Austria

In this section, we analyze the wage effects of the four UI benefit level reforms by first transparently plotting raw data of wage and benefit changes by workers sorted by their UI reference wages. We then implement a difference-in-differences analysis with various robustness checks. We

and 26,400 ATS. We interpret our two-year results on wage effects largely as a response to the 1989 reform; but our estimates of two-year wage effects when pooling all reforms are robust to excluding 1989.

[27] Hence we exclude several large reforms, such as 1978 and 1982, that affected segments of the earnings distribution that had recently experienced benefit level reforms.

[28] The statutory caps listed in that reference and elsewhere are for 12 months of earnings. Since our data includes the 13th and 14th bonus payments the observed earnings maximums are higher than listed there. Our earnings include two additional bonus payments received in May or June and December that are included in the calculation of unemployment benefits (see Appendix Section E.1 for a detailed description).

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

$$dw_{i,t} = \sigma \cdot db_{i,t}. \tag{30}$$

Throughout, we estimate very small effects of individual-level benefit changes on wages. A $1.00 increase in benefits leads to a wage effect of $0.00 in our preferred specification, with confidence intervals allowing us to rule out effects above $0.03 even after two years. These results are robust to the inclusion of a variety of controls, including time-varying industry-occupation fixed effects and firm-by-year fixed effects. This insensitivity extends to new hires, and even holds among workers with the highest predicted time in unemployment, for whom the shift in UI benefits would have increased outside options the most. Interpreted through the standard Nash bargaining framework with nonemployment as the outside option, this wage insensitivity implies bargaining power parameters close to one. We interpret this to be a quantitative rejection of the assumption that nonemployment is the relevant outside option in bargaining, as estimates of rent-sharing would imply a $0.39 increase in wages in response to a $1.00 increase in benefits (see overview of implied bargaining power estimates in Figure 1). In Section 5, we dissect this result in a series of theory-driven analyses of treatment effect heterogeneity to assess the robustness of our findings, our interpretation and the implication for alternative models of wage setting.

4.1 Variable Construction and Samples

Wage responses. Our main outcome variable is the relative change in the average daily wage from one year to the next, $dw_{i,t} = w_{i,t} - w_{i,t-1}$. For any job spell (lasting at most one calendar year), we divide total compensation by spell length in days.[29] If there are multiple concurrent employment spells, we select the one with the longest duration.

Reform-induced UI benefit level changes. Our variation in the nonemployment option arises from reform-induced shifts in UI benefit levels. Formally, a worker i with UI-relevant attributes $\mathbf{x}_{i,t}$ receives benefits $b_t(\mathbf{x}_{i,t})$ in year-t benefit schedule $b_t(.)$. Our variation is the difference between this benefit level and the *counterfactual* benefit the worker would collect absent the reform, i.e. under the $t-1$ schedule $b_{t-1}(\mathbf{x}_{i,t})$. In practice, UI benefit levels are a function of pre-separation reference wages, i.e. the assignment variable equals a reference wage $\tilde{w}_{i,t}$ applicable in year t such that $\mathbf{x}_{i,t} = \tilde{w}_{i,t}$.[30] The variation we explore is:

$$db_{i,t}(\tilde{w}_{i,t}) = b_t(\tilde{w}_{i,t}) - b_{t-1}(\tilde{w}_{i,t}). \tag{31}$$

[29] We do not measure hours and hence cannot assess whether hours reductions may mask some wage increases.

[30] We ignore additional factors that in principle enter $\mathbf{x}$ besides $\tilde{w}$, such as the count of dependents, which largely occur as lump-sum payments shifting the intercept and are thus orthogonal to the variation that we study.

Hence, $db_{i,t}$ captures variation in the benefit level solely due to shifts in the benefit schedule. The variation is zero if the benefit schedule does not change between $t-1$ and t, i.e. $b_{i,t} = b_{i,t-1}$, $\forall i$. Such years will form our *placebo years*. *Reform years* feature benefit 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 references wages are *lagged* wages; being predetermined they are by construction unaffected by the reform. In consequence, workers' pre-reform reference wages assign them treatment $db_{i,t}$.[31]

Reference wages $\tilde{w}_{i,t}$. The earnings concept used for calculation of UI benefits underwent slight changes over the multiple decades that span our reform sample. For the 2001 reform, the reference wage legally determining benefits in year t is the worker's actual wage from the *previous calendar year* $t-1$, a rule in place since 1996: $\tilde{w}_{i,t}^{t \geq 1996} = w_{i,t-1}$.[32] Hence we directly assign worker's reform-induced benefit variation $db_{i,t} = b_t(w_{t-1}) - b_{t-1}(w_{t-1})$ by directly sorting them by their lagged wage $w_{i,t-1}$. During the 1970s and 1980s, the reference wage was the previous *month's* wage.[33] Because of nominal and real wage growth and because we do not measure monthly but only annual wages, and due to the fact that wages are potentially affected by the reform, we *predict* year-t nominal wage levels based on year-$t-1$ wages, $\hat{w}_{i,t} = \bar{g}_{t,t-1} \cdot w_{i,t-1}$, i.e. by inflating their earnings with aggregate nominal wage growth, $\bar{g}_{t,t-1}$, between $t-1$ and t. We calculate aggregate nominal wage growth $\bar{g}_{t,t-1}$ by taking the average of individual nominal wage growth $g_{i,t,t-1} = w_{i,t}/w_{i,t-1}$. Appendix E.2 verifies that this wage inflation procedure almost perfectly predicts wages and benefit levels.

Benefit schedules and predicting benefit levels. Our identification design tracks incumbent workers and matches them with the UI benefits these workers were to receive in an unemployment scenario. To verify our imputation of the benefit receipt, we obtained data of actual UI benefit receipt for the sample of unemployed job seekers (the AMS data from the agency processing unemployment claims). Appendix Section E.2 describes a validation exercise by which we compare actual receipts with imputed receipts. Even for the samples of the 1976 and 1985 reforms, when benefits were a function of brief lags of income, we find coefficients close to one for the relationship between predicted and actual benefits.

Sample restrictions and summary statistics. We restrict the sample to workers aged 25-54 with non-zero monthly earnings each year, employment 12 months out of the base year

[31] Our empirical strategy therefore builds on simulated instruments (see, e.g., Cutler and Gruber, 1996; Gruber and Saez, 2002; Kopczuk, 2005; Kleven and Schultz, 2014; Weber, 2014). Our design additionally includes multiple pre-periods to test the common trends assumption underlying our identification strategy.

[32] More precisely, UI claims for unemployment spells beginning before June 30 of year t depend on labor income in $t-2$, whereas and earnings in $t-1$ pin down UIBs for spells beginning after June 30 of t.

[33] Strictly speaking, between 1988 and 1995, the reference wage as the moving average of the six previous full months of employment; the pre-1988 had the last full month of earnings as the reference wage.

relative to which we calculate wage growth.[34] For each reform, we include treatment earnings regions and adjacent, equally sized control earnings regions that did not experience benefit changes.[35] We construct placebo cross-sections of workers from pre-reform years from the same earnings percentiles as in the reform year for our difference-in-differences analyses. Table 1 provides summary statistics for the individuals affected by each reform (the "treated" columns) and a "control" group of individuals for each reform. Importantly, this table is *not* a balance check between "treatment" and "control" regions. Instead, our difference-in-differences design relies on the identification assumption that treated and control earnings regions do not face differential shocks to earnings growth in the same year and do not exhibit differential trends in the subsequent analyses, which we confirm in the subsequent analyses.

4.2 Non-Parametric Graphical Analysis

We start with a non-parametric analysis of each reform to transparently illustrate how our variation identifies the sensitivity of wages to benefit shifts, and ultimately to the nonemployment value. We cast the wage-benefit sensitivity normalized by the worker's lagged wage $w_{i,t-1}$:

$$\frac{dw_{i,t}}{w_{i,t-1}} = \sigma \cdot \frac{db_{i,t}}{w_{i,t-1}}. \tag{32}$$

We plot raw data on wage growth of workers sorted by UI reference wages, and thus exposure to benefit changes. We start with a particularly detailed description of the 2001 reform.

2001 reform: large benefit increase for lower earners. Figure 4 shows the results for the 2001 reform. The x-axis indicates gross earnings in the pre-reform base year, i.e. 2000. These reference wages determine 2001 benefits.[36] We group our data set into percentile bins; one data

[34]We have relaxed this restriction to only require employment in December of the base year, and found similar results, with point estimates again tightly centered around zero. This sample restriction ensures that the individuals have at least 52 weeks of experience in the past two years. Individuals without this experience requirement are only eligible for at most 12 weeks of UI benefits. For some of the heterogeneity analysis where we are interested in individuals who very recently experienced unemployment, we relax this restriction.

[35]The treatment (T) and control (C) regions for the four reforms that we analyze are:
1976: T: 1150 to 3650 ATS, 405,937 person-years, C: 3650 to 4850 ATS, 398,576 person-years
1985: T: 17500 to 25000 ATS, 2,455,649 person-years, C: 11100 to 15500 ATS, 2,456,159 person-years
1989: T: 4000 to 11800 ATS, 1,826,892 person-years, C: 11800 to 15700 ATS, 1,815,046 person-years
2001: T: 9950 to 20500 ATS, 3,212,131 person-years, C: 20500 to 27800 ATS, 3,195,125 person-years
For the 1985 reform that changed the maximum benefit level, we exclude data right below the maximum earnings level above which earnings are censored. Specifically, we only include observations three percentiles below the respective maximum earnings level. We also probe the robustness of our findings to lower values for the upper limit for the sample and find that our results remain quantitatively unchanged. We further also measure whether the reforms affected the probability of being above the censoring limit and find tightly estimated zero effects.

[36]The benefit schedule $b_{2001}(.)$ is a function of *net* earnings while $b_{2000}(.)$ is a function of gross earnings, as with all schedules through 2000. We use an income tax calculator to translate gross earnings (which our administrative data provide) into net earnings to compute $b_{2001}(.)$. To keep our wage concept plotted on the x-axis consistent between pre-2001 reforms we study (when reference wages were gross), we then plot the 2001 reform in terms of

point represents one percentile of the earners in the full sample.

The solid green line indicates the reform-induced benefit change for individuals at a given level of base year wages. The 2001 reform affected UI benefits for workers with base-year earnings below about ATS 19,300 (32nd percentile of the earnings distribution). By construction, the variation in benefits below and above ATS 19,300 is driven by reform-induced benefit changes.

We then asses whether the reform-induced benefit changes affected wages. The red lines with solid and hollow circles plot wage effects by base-year earnings at the one- and two-year horizon and shows no excess wage growth for workers treated with higher benefits. For each percentile, we calculate these wage effects as the simple difference between wage growth from 2000 to 2001, when the reform was in place, to pre-reform wage growth from 1999 to 2000.[37] We normalize the wage effects to zero for the lowest percentile not receiving a benefit increase in 2001 (the horizontal dashed line).

There is no visible increase or slope change in wages whatsoever around the threshold below which the reform increases workers' nonemployment outside options, suggesting that the benefit variation did not affect wage growth. This insensitivity holds both at the one- and at the two-year horizon. To provide a visual benchmark, we also plot the wage growth *predicted* by our calibrated bargaining framework in Section 2, as the dashed orange line. That is, we multiply benefit change with the calibrated wage-benefit sensitivity of 0.39. Our analysis of the 2001 reform thus clearly rejects bargaining with nonemployment as the outside option — unless one is willing to believe that workers hold all bargaining power.

Our analysis rests on an identification assumption that in the reform year, wage growth would have been parallel to the pre-reform year even in the absence of 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. Additionally, a second test, reported in Appendix Figure A.1, further assesses the parallel trends assumption. Here, we lag both the reform period and the pre-period by two years, simply checking whether the earnings percentiles affected by the 2001 reform experienced higher or lower excess wage growth compared to other earnings percentiles in periods *before* 2000. This could occur if different earnings regions were on different trends -- such that our zero result could have been a coincidence and masked a treatment effect. Figure A.1 shows no such effects for a placebo reform in 1999 (thus comparing 1999-2000 to 1998-9 wage growth) for the one-year earnings changes. At the two-year horizon, there is even some evidence of a *positive* pre-trend. While such a pre-trend would actually bias our results upward, it motivates our difference-in-differences analysis in Section 4.3, where we add time-varying industry/occupation and firm by year fixed effects to net out such potential confounders. We also formally test for, and do not find, pre-trends across all of the reforms in

gross earnings. We thank David Card and Andrea Weber for sharing the tax calculator.

[37] Analogously, we calculate two-year wage effects as the percentile-level difference between wage growth from 2000 to 2002 vs. from 1998 to 2000.

the difference-in-differences analysis.

1989 reform: increase in benefits for low earners. We conduct an analogous analysis for the 1989 reform and present results in Figure 5. The 1989 reform increased benefits for workers with base-year earnings below ATS 12,000 by up to eight percentage points. For that reform, we detect moderate, positive wage effects. Nonetheless, even the two-year wage effect is on average smaller than the effect that would be predicted by the calibrated bargaining model with nonemployment outside options, as indicated by the discrepancy between the orange and red lines.[38] In our difference-in-differences analysis we revisit the 1989 reform again and find effects close to zero, thereby indicating that the difference-in-differences design absorbs time-varying shocks to different parts of the earnings distribution that may have contributed to the potentially spurious small wage increases that the graphical analysis here may suggest.

1985 reform: increase in benefit maximum. Figures 6 plots the results of our analysis for the reform in 1985 that increased the maximum benefit amount by 29% from around 7,600 ATS to around 9,800 ATS for workers in higher parts of the earnings distribution. In Appendix Figure A.3, we document that the assignment variable (green line) and the actual benefit level based on contemporaneous earnings (red line) line up very closely. Nonetheless, we find no evidence for tantamount wage increases among workers treated by the reforms.

1976 reform: increase for low earners. We conduct an analogous analysis for the 1976 reform and present results in Figure 7. The 1976 reform affected benefits for workers with base year earnings below ATS 3,700 ATS. Our analysis of wages reveals, if anything, wage decreases among those workers that are associated with the benefit increase. The evidence thus does not point towards positive wage effects of the 1976 benefit increase. In our difference-in-differences analysis we revisit the ostensibly negative effects for the 1976 and find effects closer to zero, thus suggesting that our richer difference-in-differences analysis can account for some time-varying shocks to different parts of the earnings distribution that the nonparametric analysis does not.

[38]For 1989 as for the other two reforms before 1995, we additionally confirm that the reform affected actual benefit levels by base-year earnings as predicted by our reform-induced variation and the homogeneous earnings inflation procedure. In Appendix Figure A.2, the assignment variable—based on inflated lagged earnings—is again plotted with the green line and the *actual* benefit level with the red line, for the one-year and two-year horizons. If, counterfactually, earnings were randomly redrawn each year, then workers in different parts of the base-year earnings distribution would not actually experience differential benefit changes. Realized benefit changes closely track our reform-induced variation at the one-year horizon. (This exercise complements our validation exercise in Appendix Section E.2, where we compare actual receipts with imputed receipts and find a relationship between predicted and actual benefits of close to one.) Note that for the 1989 reform, another reform shifted the schedule in 1990, broadly for the control and treatment groups, explaining the shifted line for that year. Our two-year results are robust to excluding 1989. Moreover, in our regression specifications, we will only build on one-year benefit variation as a treatment variable even when we measure longer-term wage outcomes.

The average sensitivity of wages to benefit changes. We provide a quantitative estimate of the sensitivity of wages to the nonemployment value. Figure 8 plots the excess wage growth and unemployment benefit change for each earnings percentile across all four reforms (using different colors/symbols to differentiate between reforms). We can estimate the sensitivity with a linear regression on the data points in the scatter plots. This is equivalent to simply calculating the sensitivity by averaging the percentile-specific ratios of excess wage growth to the reform-induced benefit changes (assuming that errors around the common sensitivity are mean zero). Aggregating across reforms, we find point estimates of $\hat{\sigma} = -0.01$ (with a standard error 0.02) at the one-year horizon and of $\hat{\sigma} = 0.03$ (s.e. 0.04) at the two-year horizon. At both horizons, the confidence interval of the slope includes zero and clearly excludes the predicted slope of 0.39. This sensitivity of wages to the nonemployment value is smaller than expected in all the Nash bargaining models with nonemployment as the outside option described in Section 2.

4.3 Difference-in-Differences Design

We next investigate the regression analogue of the non-parametric analysis, to formally test for pre-trends and to include a rich set of controls. The estimated wage-benefit sensitivities range from *negative* 3 to positive 0.7 cents on the dollar after one and two years. The confidence intervals for our preferred specifications reject sensitivities above 3 cents on the dollar.

4.3.1 Econometric Framework

The variation we use for identification of the wage-benefit sensitivity is reform-induced benefit changes occurring across the earnings distribution, comparing percentiles that experience a benefit reform to those that do not in a given year. Additionally, we also compare the earnings growth in treated percentiles during the reform year to the wage growth these percentiles experienced in placebo pre-reform years.

Formally, 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, $db_{i,r,t}$:

$$\begin{aligned}\frac{dw_{i,r,t}}{w_{i,r,t-1}} =& \sigma_0 \cdot \left(\mathbb{1}_{(t=r)} \times \frac{db_{i,r,t}(w_{i,r,t-1})}{w_{i,r,t-1}} \right) + \sum_{e=-L}^{-2} \sigma_e \cdot \left(\mathbb{1}_{(t=r+e)} \times \frac{db_{i,r,t}(w_{i,r,t-1})}{w_{i,r,t-1}} \right) \\ &+ \tau_{r,P_t} + \theta_{r,t} + \gamma_{r,t} \ln w_{i,r,t-1} + X'_{i,r,t}\phi_{r,t} + \epsilon_{i,r,t}.\end{aligned} \tag{33}$$

The coefficient of interest is σ_0, capturing the effect of actual, reform-induced benefit changes on wages.[39] It captures a difference-in-differences analogue of the relationship between excess wage changes and reform-induced benefit changes plotted in Figure 8. We test for effects of placebo

[39]Following Equation (31), $db_{i,r,t}$ is the difference between the benefits $b_r(.)$ in the reform year r and the counterfactual benefits received in reform year r if the pre-reform year schedule $b_{r-1}(.)$ were still active.

reforms in the pre-period and capture these placebo effects with the σ_e coefficients, letting $e = t - r$ denote event time.[40] We control for earnings percentile fixed effects and year effects, as well as additional control variables. Due to the earnings percentile fixed effects, we normalize one placebo coefficient, σ_{-1}, to zero.[41] The remaining variation in $db_{i,r,t}$ that identifies the wage-benefit sensitivity σ_0 compares wage growth across earnings percentiles with and without reform-induced benefit changes in a year, and within a percentile over time, comparing actual to placebo reforms. We again normalize both the wage and the benefit change by i's wage level in $t-1$, $w_{i,r,t-1}$.

Testing for pretrends with placebo reforms. In pre-reform years $t < r$, $db_{i,r,t}$ denotes placebo benefit changes copied from the actual reform year into earnings percentiles. σ_e $(e < -1)$ estimates wage effects of such placebo reforms, thus testing our core identification assumption: that treatment group would not be on different wage growth trends even absent the reform.

Controls. Percentile fixed effects τ_{r,P_t} absorb permanent differences in wage growth across percentiles, e.g., due to mean reversion. They are reform-specific, i.e. common between reform and placebo years for a given reform, but separate between reforms. Calendar year effects absorb aggregate wage growth shifts. Year-specific parametric earnings controls $(\ln(w_{i,r,t-1}))$ account for effects of time-varying shocks to different parts of the earnings distribution. We then incrementally add additional covariates $X'_{i,r,t}$ with year-specific coefficients to absorb other time-varying shocks. Here the first set is demographics (sex, cubic polynomials of experience, tenure, and age). The second set contains industry-by-occupation-by-year fixed effects $\gamma_{o(i,t),k(f(i,t)),t}$.[42] Third, in our most fine-grained specification, we leverage variation between workers within the same firm by including firm-by-year effects $\psi_{f(i,t),t}$.

Estimation. We estimate the specification in (33) by stacking data for all reforms $r \in \{1976, 1985, 1989, 2001\}$. We restrict the earnings ranges included for each reform to the "treatment" and "control" regions included in Section 4.2.[43] For each reform, we add $L = 3$ pre-period years.[44] We report standard errors based on two-way clustering at the individual and

[40] Formally, we define the db/w for placebo reforms in $e < 0$ as follows: First, we calculate the average actual, reform-induced shift db/w for each percentile of the earnings distribution. We then assign these db/w backwards in time by earnings percentile. To illustrate our approach, the 1989 reform increased benefits between the 5th and 10th percentiles and left the benefit schedule unchanged in other parts of the earnings distribution. Our design will then assign placebo reforms of equal economic magnitude to individuals between the 5th and 10th percentile of the earnings distribution in pre-reform years and thus capture placebo effects of a fictitious reform in the pre-reform period.

[41] In addition to the one-year horizon, we also conduct the analysis using two-year wage outcomes. We normalize σ_{-2} to zero and omit σ_{-1} for specifications in which we consider two-year outcomes.

[42] Specifically, we included fixed effects for each year by four-digit occupation by white/blue collar interaction.

[43] For estimation, we leverage the procedure in Correia (2017).

[44] $L = 3$ is the maximal amount of pre-periods we can include to be able to study the 1976 reform, since our data start in 1972. We have also assessed the robustness of our findings to longer pre-periods ($L = 5$); this

the earnings percentile level as our treatment variation is at the earnings percentile level. In Appendix Figure A.6, we confirm that other clustering levels (firm, percentile, individual, and reform-specific percentiles) lead to similar confidence intervals. We winsorize wage growth at the 1st/99th percentile; Appendix Figure A.7 confirms robustness to no winsorization as well as winsorization at the 5th/95th percentiles.

4.3.2 Regression Results

Mirroring the non-parametric analysis, the difference-in-differences analysis reveals that wages are insensitive to benefit changes. The point estimate for the wage-benefit sensitivity is $\hat{\sigma} = 0$ (se 0.013) after one year and $\hat{\sigma} = -0.027$ (se 0.026) after two years in our preferred specifications with firm by year fixed effects. Given our confidence intervals, we can reject that a \$1.00 increase in the nonemployment payoff due to UIBs increases wages by more \$0.03 after two years.

One-year effects. Table 2 presents the results for wage effects, i.e. estimates of σ_e, the interaction of actual (reform-induced) and placebo benefit changes with event time. The regressor of interest is σ_0, capturing the wage growth associated with reform-induced benefit changes. The treatment effects are also plotted in the left part of Figure 9. The different columns progressively include richer individual and firm-level controls. We have normalized σ_{-1} to zero and assess pre-trends with the σ_{-3} and σ_{-2}. Across all six specifications in Table 2, we cannot reject that both pre-period estimates are jointly equal to zero, which supports our identification assumption.

Throughout all specifications in Table 2, we find quantitatively similar effect sizes that are centered at zero. Specifically, we find effects of $\hat{\sigma} = -.004$ in a specification without control variables (column 1) and a similar estimate when adding Mincerian controls (column 2). Our coefficient estimates are even smaller at -0.014 and -0.019 (columns 3 and 4) when including industry-occupation-year fixed effects and including all controls jointly.

Two-year effects. Table 3 and the right half of Figure 9 report longer-term effects of the benefit reforms, at the two-year horizon. In a specification with all control variables (column 4 of Table 3), we find an effect of $\hat{\sigma} = -.022$ (se 0.03). In specifications with fewer control variables (columns 1 through 3), we again find effect sizes of similar magnitude ranging between -0.027 and 0.007. The effects of placebo reforms in the pre-period are statistically insignificant, providing additional support for the common trends assumption underlying our research design.

Intrafirm variation. Next we assess whether changes in the nonemployment outside option *between workers within in the same firm* lead to wage changes, by including firm-by-year fixed

extension requires us to exclude the 1976 reform.

effects in columns 5 and 6, which absorb any between-firm variation in wage growth. At the one-year horizon (Table 2), we find that the within-firm variation leads to identical, zero effects, even more precisely estimated than the effects in columns (1)-(4). Similarly, at the two-year horizon (Table 3), the effects remain small in magnitude and insignificant.

Parametric earnings controls. Consistent with the simulated instruments literature that relies on parametric earnings controls to control for heterogeneous trends (Kopczuk, 2005; Kleven and Schultz, 2014), our main specifications controls for a time-varying trend in log earnings. We therefore present additional estimates of our main specification (column (4) in Table 2) with different earnings controls in Appendix Figure A.8: log-earnings, linear earnings, and linear earnings percentiles. The alternative earnings controls yield very similar estimates around zero.

Validation exercise. We supplement the reduced form analysis in Equation (33) with a validation exercise to assess whether reform-induced benefit changes led to realized benefit changes. Formally, we estimate the following specification, letting superscripts V denote coefficients for the validation exercise:

$$\frac{b_{i,r,t}-b_{i,r,t-1}}{w_{i,r,t-1}} = \sum_{e=-L}^{0} \delta_e^V (\mathbb{1}_{(t-r=e)} \times \frac{db_{r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}) + \tau_{r,P_t}^V + \theta_{r,t}^V + \gamma_{r,t}^V \ln w_{i,r,t-1} + X'_{i,r,t}\phi_{r,t}^V + \epsilon_{i,r,t}^V. \quad (34)$$

Intuitively, the coefficient δ_0^V captures the extent to which reform-induced benefit changes lead to actual benefit changes, with coefficients close to one indicating a strong relationship. Intuitively, δ_0^V could be close to zero if, hypothetically, an individual's earnings were independently redrawn each year, because wage earnings in $t = r-1$ would not be indicative of earnings and thus of benefit levels in $t = r$. Analogous to specification (33), we normalize δ_{-1}^V to zero. In years $t < r$, the coefficients δ_e^V indicate the extent to which earnings percentiles that experienced benefit reforms in year r were affected by potential previous schedule changes or endogenously experienced benefit changes, e.g., due to wage growth related or unrelated to the treatment effect.

We report results in Appendix Table A.2. The analysis reveals a 0.807 (se 0.013) coefficient at the one-year horizon and of 0.529 (0.021) at the two-year horizon and thus indicates that the reforms we study meaningfully affected benefits among those that we predict to be affected.[45] The effects are also stable when we add in more detailed controls, indicating that we have sufficient power even in the specifications controlling for firm-by-year effects. The pre-period coefficients test whether the same earners have been recently exposed to schedule changes. In line with our selection of reforms, these placebo coefficients are an order of magnitude smaller

[45]Note that for the 2001 reform, the validation exercise is successful by design because the reform occurred at a time when benefits were determined based on lagged years' wages. We also run a validation specification excluding the 2001 reform and find overall similar results.

than the coefficient for the actual reform year.[46] We conduct an additional validation exercise in Appendix Section E.2, comparing actual with imputed UI receipts.

Accounting for non-taxation of UI benefits. Our estimate represents the gross-wage sensitivity to UI benefits, which are actually slight overestimates because Austrian UI is not taxed. We account for non-taxation of benefits and report results based on scaled-up changes of benefits in Appendix Tables A.3 and A.4. We translate the UI benefit shift, db from specification (33), into a change in (hypothetical) gross benefits by scaling up the actual benefit shift by an individual's average net-of-tax rate so that both the benefit and the wage change are in gross units.[47] The results of the specifications in Tables A.3 and A.4 are quantitatively similar to the ones in Tables 2 and 3 and also indicate an insensitivity of wages to nonemployment value shifts, with even tighter confidence intervals.[48]

Separation effects and other outcomes. To rule out selective attrition, we also report treatment effects on separations and sickness in Appendix F. Across specifications and outcomes, we find that the benefit increases were associated with quantitatively negligible effects on these outcomes that are statistically indistinguishable from zero in most specifications.

4.4 Wage Sensitivity by Post-Separation Time in Unemployment τ

In the model in Section 2 and illustrated in Figure 2b, the sensitivity of N to b – and thus the sensitivity of w to b – is mediated by post-separation time in nonemployment τ, which puts more weight on the instantaneous payoff while unemployed b. Next, we leverage *empirical* heterogeneity in τ to investigate whether even in high-τ samples, wages remain unresponsive. We assign each employed worker an idiosyncratic predicted post-separation time in unemployment τ, using a regression model with pre-separation attributes fit to actual separators.[49] We consider only *UI receipt* in our measurement of τ, so as to tightly connect to the model and avoiding complications

[46] For the one-year estimates, the coefficients are very precisely estimated in the pre-reform periods and thus the point estimates are statistically significantly different from zero. For the two-year validation exercise, we cannot reject that the pre-period coefficients are jointly equal to zero.

[47] We rely on a tax calculator for Austria provided by Andrea Weber and David Card for 2000 onwards. We extrapolate it into previous years by assigning each earnings percentile before 2000 the same net of tax rate as in the 2000 distribution. For the 2001 reform, the results are exact.

[48] We also complement our graphical nonparametric analysis with an analysis accounting for non-taxation of UI benefits and report results in Figure A.5.

[49] We take the sample of all E-N transitions in a given year and count the full months during which the just-separated worker will receive UI over the course of the next *16* years, the maximum horizon our data allow while including the 2000 cross-section of workers. We then run a basic prediction model using the separator's pre-separation attributes, such as industry, occupation dummy, gender, age as well as a nonparametric control for time since last UI receipt. We use the estimated coefficients to assign all employed workers their idiosyncratic predicted τ_i, feeding that worker's pre-separation attributes into the model. The detailed variable construction is in Appendix Section G.

from takeup or finite benefit duration.[50] We then back out the predicted wage-benefit sensitivity based on the structural wage-benefit sensitivity expression (6), maintaining $\phi = 0.1$ for all workers.[51] In each reform, we group our sample of workers into quintiles of τ. We then estimate heterogenous treatment effects in our data, for the five τ quintiles, which we plot in the blue solid line (empty circles) in Figure 10. On the x-axis, the figure plots the corresponding mean of each worker group's τ. The figure also plots the *predicted* wage-benefit sensitivity based on expression (6), in the yellow line (solid squares).[52]

Figure 10 reveals three insights. First, the median separator's τ is around 7% (consistent with our baseline calibration). Second, there is substantial variation in τ, and thus in the predicted wage-benefit sensitivity, reflected in the slope of the predicted effects, which is almost 0.6 for the top quintile group. Third, inspecting the empirical treatment effects, the graph is *completely flat*. That is, we do not see large wage effects even for worker groups that in the data clearly experience long and frequent unemployment spells – for whom the UIB increases should plausibly – mechanically – entail larger shifts in the nonemployment value.

4.5 New Hires' Wage Sensitivity

Perhaps wage stickiness among incumbent workers slows down wage adjustments even after two years. We therefore estimate the treatment effects separately for job stayers and various mover types, which are more likely to reset wages flexibility.

Figure 11 displays the estimated one- and two-year treatment effects for job stayers, and recalled workers and job switchers. We classify workers by their first type of transition from the original job in the base year. Importantly, when considering one and two-year earnings, we use our spell data to consider *post-separation wages* rather than average annual earnings.[53] We interact an indicator for each transition type with the σ_e coefficients in regression model (33). The parametric year-specific earnings controls and the baseline earnings percentile fixed effects vary by transition type. Across all three transition types, we estimate small and insignificant effects. Even with the much smaller sample sizes, the confidence intervals do not include our theoretical benchmark of 0.39. Even workers we classify as recalled workers – who return to the same firm and presumably same job after an unemployment spell – do not appear to bargain a higher wage when outside options improve (although this sample is likely selected). Job-specific wage stickiness is therefore unlikely to explain the insensitivity of wages to the nonemployment

[50]We ignore *Notstandshilfe* (post-UI unemployment assistance), thereby *underestimating* the overall level of τ. When we considered all nonemployment time (which may include disability or retirement), we have naturally obtained even larger τ measures.

[51]Permitting a negative correlation between τ and ϕ will generate even more dispersion in the predicted sensitivity.

[52]The predicted sensitivities are the within-quintile mean of the predicted micro-sensitivities from the worker-specific τ, thus respecting the nonlinearity of the sensitivity–τ relationship in the aggregation.

[53]At the one-year horizon, recalled workers' daily earnings cover the entire calendar year and are thus averaged.

value.

We have also divided movers into (a) movers that directly move from one employer to another and ("EE") (b) workers who move to another employer after going through an unemployment spell with UI receipt ("EUE").[54] 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 matching models (Pissarides (2009)). Third, these workers should exhibit standard, large sensitivity of wages to UI shifts even in richer models with employer competition and external job offers as in Cahuc et al. (2006), simply because these workers' sole outside option is still nonemployment.

We plot the estimated wage-benefit sensitivities for EUE movers in Figure 12. Even the results for EUE movers do not reveal positive effects. In fact, the point estimates are negative, although seven out of the twelve estimates are insignificantly different from zero (due to larger confidence intervals). None of the upper confidence intervals includes the predicted value.[55] We have also experimented with much longer horizons (e.g. five years) defining the average annual wages over the entire job duration for new hires for the first job, and even permitting subsequent job transitions and thus complicated dynamic compensation considerations, and have found no positive effects for EUE movers.[56]

Estimates for EE movers are in Figure 13. Confidence intervals again widen. Interestingly, this sample contains some positive effects at the one-year horizon, although this effect moves to zero once we interact controls by transition type, and fully converge to zero with firm-by-year effects. At the two-year horizon, estimates are very close to zero no matter the control specification, suggesting that post-transition earnings in year one appear to be noisier than year-two wages.

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 effects may play a role in the wage effects in either direction. Second, for EUE movers, there are non-bargaining channels through which our reforms may have affected re-employment wages, such as reservation wage channel, a skill depreciation channel (where longer UI induced unemployment spells lead to skill depreciation), or one of statistical discrimination (where longer spell duration may entail wage

[54] We maintain our monthly panel data set, by which a given monthly spell gives priority to employment, and then unemployment with UI receipt, and then other states. As a result, EUE movers contain movers with at least a full calendar month of unemployment; EE movers also contain movers with adjacent calendar months with at least one day employment (in different firms).

[55] In unreported results, we have further investigated the sensitivity of these results to including alternative earnings controls following the robustness check of our main results presented in Appendix Figure A.8. Here, some point estimates for EUE movers were closer to zero. Moreover, we have found that EUE and EE estimates were very stable around zero when we drop very low (albeit perhaps noisy) earners. We thank Giuseppe Moscarini for pointing out that in principle the prospect of faster EE wage growth could have offset the positive entry wage effect among EUE separators.

[56] We do not report those results because we would either need to drop five years of pre-reform data or make the strong assumption that pre-reform new-hires cohorts' continuation wages are not affected by the reforms.

penalties from re-employers due to asymmetric information). These potential confounds among EUE movers had in part motivated our approach of primarily studying on-the-job wage changes of incumbent workers in the first place.

5 The Missing Link Between Wages and Benefits

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

$$\frac{dw_i}{db_i} = \underbrace{\frac{dw_i}{d\Omega_i}}_{\substack{\text{Sensitivity of wages}\\ \text{to outside options}}} \times \underbrace{\frac{d\Omega_i}{dN_i}}_{\substack{\text{Sensitivity of outside option}\\ \text{to nonemployment value}}} \times \underbrace{\frac{dN_i}{db_i}}_{\substack{\text{Sensitivity of nonemp. value}\\ \text{to UIB shifts}}} \tag{35}$$

We conduct heterogeneity analyses by estimating our main specification (column (4) in Tables 2 and 3) with interactions between the treatment variable (and placebo treatments in pre-reform years) and an exhaustive set of heterogeneity groups indicators. We estimate a separate specification for each dimension of heterogeneity. Figure 14 presents these estimates for a large number of heterogeneity groups at the one-year horizon; Appendix Figure A.9 reports two-year effects, and Appendix Section G describes the variable constructions in detail. Except for the categorical variables, the top red estimate is for individuals with the lowest values of the respective heterogeneity variable and the bottom blue estimate is for individuals with the highest values (e.g., lowest and highest tenure quintile). We find very little variation across groups. We also find wages to be insensitive to alternative treatment definitions (potential duration rather than level of benefits; firm-level average of the instrument).

5.1 The Nonemployment Value and UI Benefits dN/db

Exposure to unemployment risk and experience with the UI system. We provide additional proxies for exposure to unemployment risk or for experience with the UI system. The heterogeneity dimensions we consider are unemployment risk, the local unemployment rate, and a direct prediction of time on UI unconditionally of a separation, along with long-term unemployment (above 6 months). This analysis complements our heterogeneity analysis by τ proxies in Section 4.4, which mapped directly into our structural wage bargaining equation. We additionally consider the worker's actual UI history: months since the last UI receipt or nonemployment spell (excluding or including recalled workers). These additional proxies for unemployment risk are not consistently associated with larger one-year point estimates, and hover around zero. We find some suggestive evidence of larger effects for recently reemployed workers at the two-year horizon, which however remain insignificant (and do not square with our even negative results in Section 4.5 for EUE movers during the reform).

Salience and knowledge about UI. Limited salience of benefit changes could diminish wage responses simply because the bargaining parties are not aware of the perhaps complex institutional intricacies of the UI system.[57] For our study, the specific statistic of interest is whether employed workers are aware of their *own applicable* benefit level and changes therein. If workers are not aware of changes in N or b, then one may not expect these shifts to affect wages.

To directly evaluate the plausibility of the salience requirement, we leverage results from a unique Eurobarometer survey conducted among a representative sample of Austrian employees in 2006 that asks about beliefs about benefits were they to become unemployed (European Commission, 2012). We display results of our analysis based on the Eurobarometer survey data and compare it to actually paid out benefits in Figure 15. The figure presents the distribution of actual benefits as a percent of net earnings and individuals' beliefs about their benefits. We bin the actual benefit ratios into the same interval bins that were presented in the Eurobarometer survey. We also use an interval regression to estimate the mean benefit ratio in the survey data and compare it to average ratio for actual UI recipients. Strikingly, the two histograms look fairly similar and the average worker's belief about their benefit replacement rate is 64.03% (SE 0.72) compared to an actual replacement rate of 65.29% among unemployed workers in the AMS data.[58] Moreover, we also found in unreported results that workers with more children correctly predict that they would receiver higher benefits.

Several additional pieces of our evidence are hard to square with a salience-based explanation of our findings. First, even over multiple years, workers would have to not learn about the shift in the system. Second, we have found that even large shifts do not entail wage responses, which are arguably more salient and could also overcome adjustment costs (see, e.g., Chetty, 2012). Third, even recently unemployed and UI-receiving workers, who are plausibly more aware of the UIB schedule (see Lemieux et al., 1995; Lemieux and MacLeod, 2000), do not exhibit higher wage sensitivity. Fourth, even workers with higher risk of future unemployment events for whom the UI system is likely more salient, do not respond.[59] Fifth, Jäger et al. (2018) document that existing jobs with low surplus are sensitive on the separation margin to UI generosity in Austria, suggesting that at least older Austrian workers and/or employers appear to take the nonemployment value into account in separation decisions. Finally, compared to other types of perhaps idiosyncratic variation in the nonemployment value (e.g. an idiosyncratic shift in a worker's taste for leisure, the cost of work, or reemployed probabilities), an advantage of the institutional variation we use is that the benefit schedule is in principle verifiable and perhaps even common knowledge. In fact, the benefit level is a function of previous wages, a piece of information that should be readily accessible particularly for the employer that paid that wage

[57] For instance, Abeler and Jäger (2015) find evidence consistent with lower responses to incentives in more complex systems.

[58] The replacement rate can differ from 55% due to lump sum benefits for dependents, and the earnings base for benefits post-1996 are annual earnings lagged by one or two years rather than contemporaneous earnings.

[59] Hendren (2017) finds that employed workers can predict separations.

to the incumbent worker in the previous year.

Additional analysis: variation in UI generosity arising from potential benefit duration, and treatment assignment by age. In Appendix Section H, we additional investigate the effect of changes in the potential benefit duration (PBD) of UIBs (rather than the UIB level) on incumbent wages, exploiting a reform in 1989 for workers aged 40 and above. This design complements our benefit variation as the treatment assignment was age- rather than past-income-based, the reform was permanent (rather than potentially eroded by inflation or subsequent benefit schedule shifts), and perhaps more salient and non-complex (a simple cutoff in age). Lastly, as we note in the derivation of the model, although our predictions are quantitatively robust to allowing for treatment and control groups being in separate markets (and thus wage changes capturing equilibrium adjustment rather than micro effects), the age eligibility cutoff provides sharp discontinuous identification of workers almost certainly in the same market and close substitutes in production. We do not find wage effects of this dimension of UI generosity either, even two years after the reform.

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

Perhaps our variation does induce shifts in the nonemployment value N, but that shift does not affect the outside option Ω that enters real-world wage bargaining.

External job offers and job mobility. We also 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, which, in models of employer competition and on-the-job search, may shield 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 et al., 2006). At the one-year horizon, we do not find that recently nonemployed workers exhibit larger wage-benefit sensitivities (Figure 14). We find some, but statistically insignificant and quantitatively small, evidence for this prediction at the two-year horizon (Figure A.9).

Group-level bargaining: firm-level treatment. Rather than atomistic bargaining, between one individual worker and one firm, real-world wage setting may follow bargaining at the firm level between the employer and their entire workforce, for instance represented by plant-level works councils in the Austrian case. Then, the average workers' outside option may matter for wages, such as in union bargaining models.[60]

[60] Saez et al. (2018) document that the rent sharing of a firm-specific windfall from a youth payroll tax cut occurred with all workers in the firm, rather than the directly treated young workers. Yet, a large body of evidence on capacity of worker-level wage growth to reflect idiosyncratic shifts include subgroup productivity

We study the role of firm-level bargaining by averaging the worker-level reform-induced benefit variation at the *firm* level. Our design now investigates whether wages grow more at firms if the benefit change of the *average* employee is larger – rather than the worker-level idiosyncratic benefit changes. We plot the variation in this firm-level average treatment in the histogram in Appendix Figure A.10. We associate workers with the firm they work at in the pre-reform year. Our regression specification otherwise mirrors the worker-level specification (33).[61]

Figure 16 and Table 4 report firm-level point estimates that remain small, ranging from 0.013 to 0.035 at the one-year horizon and from 0.023 to 0.035 at the two-year horizon. The confidence intervals include the point estimates for the worker-level specifications. Table 4 also reveals pre-trend violations for the specification without industry-by-occupation effects. This suggests that firms with different shares of workers affected by the benefit reforms were on different trends, perhaps because of industry-level shifts that were correlated with treatment intensity. When we include industry and occupation effects (specifications 3 and 4), thereby comparing workers in the same industry and occupation but working at firms with different benefit shifts, we find that pre-trends are flat and point estimates for the pass-through remain between 0.033 and 0.035. Quantitatively, the evidence is hard to square with bargaining at the firm-level in which the value of nonemployment determines workers' outside options, although the slightly larger pass-through may be consistent with some degree of wage compression within the firm.

Collective bargaining wage floors. Despite substantial scope of worker-firm wage bargaining, Austria is a unionized economy where wage *floors* are set at the industry level. As we discuss in Section 3.1, these wage floors often do not bind as firms are free to, and often do, pay a premium. We have additionally reviewed whether the wage floors specified collective bargaining agreements (CBAs) appear to differentiate wages for treated worker groups around the reform years we study.[62] While a thorough digitalization of Austrian CBA wage floors is beyond the scope of the paper, our case studies suggest that these negotiated wage floors do not appear to respond either to the shift in the nonemployment value, in line with our analysis of micro earnings data from social security records.

(Jäger, 2016; Kline et al., 2017). Carneiro et al. (2012) documents cyclical within-firm wage growth differentiation between new and incumbent workers in the same jobs. Similarly, Borovičková and Shimer (2017) show that workers' wage premia are carried across employers in Austria, implying that Austrian firms can differentiate wages within the firm according to idiosyncratic factors.

[61] Before averaging, individual workers' benefit changes at winsorized at the 1st and 99th percentiles. The firm-level specification includes two controls mirroring the worker-level ones: reform-sample (but not year) specific percentile fixed effects for the average treatment at the firm and also for the share of workers with positive idiosyncratic treatment.

[62] The typical Austrian CBA specifies a binding wage floor ("Kollektivvertragslohn") for all workers covered by the CBA and the percentage wage raise for all workers in existing employment relationships ("Istlöhne"), that the firm has to implement even for workers with a wage above the wage floor.

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

Perhaps our variation does induce shifts in the nonemployment value N, and indeed those would shift the outside option Ω as in the model, yet real-world wage setting is insensitive to outside options. Below we explore this direction.

Proxies for 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 the type of occupation (**blue vs. white collar**). The results show no clear pattern of effect heterogeneity in these dimensions. We then consider effect heterogeneity by **sex**, motivated by findings by Black and Strahan (2001) and Card et al. (2015) that female workers' wages are less exposed to productivity shifts, consistent with lower bargaining power. We find some evidence consistent with somewhat larger effects among women at the one-year horizon (although the pattern reverses at the two-year horizon).

Firm pay premia. We calculate firm fixed effects following the methodology in (Abowd et al., 1999, AKM in the following) and estimate the wage-benefit sensitivity in firms with high or low firm effects. In both high and low AKM firm effect firms, i.e. in those with a particularly positive or negative pay premium, we find estimates close to zero at the one-year horizon. At the two-year horizon, we estimate a wage-benefit sensitivity of around 0.1 in low-AKM firms. This finding could be broadly consistent with models of bargaining where wages are only renegotiated when one of the parties' outside options becomes binding (MacLeod and Malcomson, 1993) and a given shift in the nonemployment value is more likely to bind when the pay premium is lower to begin with. Yet, even in those firms the size of the wage-benefit sensitivity is substantially smaller than the one predicted based on a calibrated bargaining model.

Wage adjustment frictions. Perhaps wage stickiness in continuing jobs masks wage pass-through in the short run. We have found several pieces of evidence that reject wage stickiness as an explanation for our overall findings. First, wages remain insensitive even after two years. Given the small fraction of still-constrained wages and given that downward wage rigidity would not bind in our scenario of *upward* wage pressure, general wage stickiness is thus an unlikely explanation for the small wage effects.[63] Second, we found no evidence for nonlinear effects such that even large shocks in the nonemployment value did not entail noticeable wage effects, implying that menu costs may not explain wage insensitivity. Third, we have not found

[63] Estimates of wage stickiness imply that more than half of wage contracts should reset each year (Barattieri et al. (2014) for the United States, and Sigurdsson and Sigurdardottir (2016) for Iceland) or that incumbent worker's wages are half as sensitive to aggregate shocks as new hires' wage contracts (Pissarides, 2009). Dickens et al. (2007) find that wages exhibit lower downward nominal wage rigidity in Austria than in Germany or the United States.

positive wage effects in *new* jobs, presumably less constrained by wage rigidity. Fourth, our institutional review in Section 3.1, confirms that real-world wage setting leaves substantial room for idiosyncratic deviation from collective wage floors.

To further study whether wage frictions may be be driving our results, we estimate heterogeneous treatment effects across a number of measures of firm-level wage flexibility. First we estimate the treatment effect by **industry growth rates**. Wage rigidity might prevent shrinking industries from increasing wages and workers in low-growth industries may be less likely to have other job offers, so that the nonemployment outside option might be more relevant. Second we stratify firms by various measures of their flexibility in wage setting. The first measure we consider is the **standard deviation of wage growth within the firm**. If wage growth is more dispersed in a particular firm, then there are some firm-level mechanisms that allow earnings growth to be more individualized and that may allow for a larger pass-through of outside option shifts into wages. Next, we consider an alternative measure of the same underlying concepts as the **difference between the 75th and 25th percentile of within-firm wage growth**. Third, we calculate a measure of **residualized wage dispersion** by regressing log-wages on industry-occupation-tenure-experience-year fixed effects and calculating the residuals from this regression and take the standard deviation at the firm level. Fourth, we calculate a proxy for **distance from CBA-level wage averages**. Specifically, we regress log wages on tenure-experience-occupation-industry-year fixed effects, reflecting that CBAs frequently set wage floors at those levels. We then calculate the mean squared residuals of this regression at the firm level to proxy for how far away from the average wage in those cells a firms wages are. Finally, we also split **firms by size** (employment count), since survey data show that wage bargaining is more prevalent in smaller firms. The point estimates even for firms with more flexible wage setting are well around zero and insignificant.

The prevalence of wage bargaining. One potential rationalization of the insensitivity of wages to the nonemployment outside option is that 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 ex-post rent sharing with incumbent workers. Moreover, direct worker and employer survey evidence on the actual presence of bilateral bargaining suggests that both sides of the labor market perceive much of wage setting to occur through bargaining (vs. wage posting). Hall and Krueger (2012) survey workers in the United States, and Brenzel et al. (2014) survey employers in Germany. It is natural to ask whether correlates of the prevalence of wage bargaining in those surveys are associated with larger sensitivity of wages to our variation.[64] However, we do not find larger pass-through

[64] According to those surveys, wage bargaining is more likely for the following job characteristics (for which we have constructed empirical proxies): small firm size (establishment employment count), higher worker age, higher education (white collar), more specialized jobs (experience and tenure; white collar), more time since

for those characteristics, such as tighter labor markets (lower unemployment), workers with higher education (our proxy: white rather than blue collar), in smaller firms, among males, or in industries with more dispersed productivity (our proxy: wage dispersion in the firm). This suggests that even in pockets of the labor market where we expect bargaining to occur, nonemployment value shifts do not entail wage effects.

6 Implications for Models of Wage Determination

The insensitivity of wages to the nonemployment value presents a puzzle to the predictions from a Nash bargaining model with nonemployment as the assumed outside option, including in extensions of the basic model that we review in Section C.1. Here, we discuss alternative bargaining and non-bargaining models of wage setting that may account for our findings.

Credible bargaining (Hall and Milgrom, 2008). Hall and Milgrom (2008) build on results in Rubinstein (1982), Rubinstein and Wolinsky (1985) and Binmore et al. (1986) and replace Nash bargaining with an alternating offer bargaining game in which both firms' and workers' threat point is to extend bargaining rather than to terminate negotiations. In their model, outside options only become relevant in exogenous break-downs of the bargaining process. In Appendix Section C.2.2, we derive the wage and discuss the role of b in wage setting. In theory, a knife-edge case of the wage bargain expression is complete insulation from the outside option under certain parameter restrictions about the probability of negotiation breakdown vs. exogenous job destruction in formed matches, while permitting limited comovement with idiosyncratic productivity, consistent with rent sharing estimates.[65] However, the theoretical wage bargain does remain very sensitive to the flow payoff while bargaining, which for the unemployed is b, but for incumbent workers may be the old, default wage. The model would therefore predict large wage sensitivities to UIBs among new hires out of unemployment (whose bargaining-stage payoff is still b), a prediction for which we find little evidence.

Rebargaining in corner cases. Alternative models (e.g., MacLeod and Malcomson, 1993) *only* reset in case either the worker's or the firm's surplus from the job would turn negative absent wage resetting (but joint surplus remains positive, i.e. a wage can be found to fulfill both parties' participation constraint). This scenario requires the wage to fall beneath the worker's reservation wage or above the firm's reservation wage. These models can rationalize an attenuated effect

unemployment (in months), tight labor market (local unemployment rate; individual-level predicted unemployment spell duration; industry-occupation unemployment risk), and dispersed productivity (firm-level standard deviation of employees' residualized log earnings and their growth)), and female gender.

[65] In fact, our findings are complementary to evidence from laboratory experiments that find outside options to be irrelevant in shaping bargaining options unless the threat to take the outside option is credible (see, e.g., Binmore et al., 1989).

of nonemployment shifts on wages even in a bargaining setting. The model is thus consistent with our main empirical finding. Yet, we find only mixed evidence for additional predictions from such models. For example, we find no evidence that larger, reform-induced nonemployment value increases lead to large wage increases. We also found only mixed evidence for larger wage effects among workers for whom the model would predict initial wages to be close to the worker reservation wage. While we found a slightly positive wage-benefit sensitivity in low-AKM firms at the two-year horizon, we did not find that worker with proxies for low bargaining power or marginally attached workers with high unemployment risk experienced larger wage increases in response to benefit shifts. We also showed that the wages for initially employed workers transitioning through unemployment spells, i.e. for whom wages are newly set in the new match, are not more sensitive to nonemployment shifts. Finally, the empirical literature on rent sharing has managed to detect even small wage effects from idiosyncratic productivity shocks, consistent with rebargaining occurring in many employment relationships.

On-the-job search, employer competition, and negotiation capital (e.g., Cahuc et al., 2006; Altonji et al., 2013; Bagger et al., 2014). In models with on-the-job search, downward sticky wages and employer competition, *employed* workers that search on the job to move up a job ladder of firms with heterogeneous productivity. Once they receive such an outside option that dominates nonemployment, they ratchet up the wage (which subsequently stays elevated) – whether actually they switch jobs or not.[66] Wages are subsequently insulated from the nonemployment value. Beaudry et al. (2012), Caldwell and Danieli (2018), Caldwell and Harmon (2018), and Conlon et al. (2018), provide evidence in support of the idea that job opportunities at other employers raise earnings, in line with this channel. This view may reconcile a zero or small effect of nonemployment outside options on wages, while not implying full bargaining power.

However, we found less support for nuanced predictions for workers with recent unemployment. Absent alternative external offers, unemployment remains the threat point of unemployed workers – and even for an employed worker *until* she receives an outside offer more attractive than unemployment. For these workers, wages follow the standard Nash bargain and should still exhibit the large wage-benefit sensitivity.[67] While we found slightly higher sensitivity for workers with shorter time since nonemployment at the two-year horizon (but not for workers undergoing

[66] An exception are models with employer competition and on-the-job search in which nonemployment remains the outside option when bargaining with the next employer (see, e.g., Fujita and Ramey, 2012; Beaudry et al., 2012); these on-the-job search models thus feature nonemployment as outside options and cannot be reconciled with our main result of wage insensitivity.

[67] Unemployed job seekers and incumbent job seekers without suitable outside offers receive the standard Nash wage with unemployment as the outside option: $E(w) = (1-\phi) \cdot N(b_i) + \phi \cdot (E(w) + J(x_f, w))$. where x_f is the match- or firm-specific productivity. An employed worker having received outside offer $x_{f'}$ dominating N yet dominated by the current job $(E(w)+J(x_f,w)-U(b) > W(w)+J(x_{f'},w)-U(b) > U(b))$ renegotiates the current wage with that external job offer as the outside option: $E(w) = (1-\phi) \cdot [E(w) - E(w_{f'})] + \phi \cdot (E(w) + J(x_f, w))$.

unemployment during the reform), the effect sizes remained small. This insensitivity would require that workers freshly hired out of unemployment receive alternative wage offers very fast.

Wage posting and monopsony. Besides wage bargaining, wage posting models are the second leading alternative to the Walrasian market-clearing model. In such models, firms post wages with full commitment. The nonemployment value remains a cornerstone of the wage distribution by factoring into workers' reservation wages (and thus into firms' wage strategies). The wage posting model in Burdett and Mortensen (1998) generates wage dispersion as a mixed equilibrium strategy homogenous firms play when recruiting homogeneous workers with random search. Wage policies are sensitive to the nonemployment value because firms may meet unemployed job seekers. Shifts in b therefore shift the entire distribution of wages to the right, a prediction that carries over to richer wage posting models with worker or firm heterogeneity. In sum, wage posting models without heterogeneity are broadly consistent with our findings. However, the larger sensitivities that emerge in more realistic wage posting models (e.g., with firm heterogeneity) generate large wage-benefit sensitivities and are thus harder to square with our evidence.

Walrasian labor markets. Frictional labor market models have been the point of departure of our our study of wages and outside options. The Walrasian, frictionless labor market model with market-clearing wages may perhaps rationalize the absence of wage effects from UI. The analysis would in essence appeal to an incidence framework of labor demand and labor supply, where UI generosity acts as a leisure subsidy, and labor supply would need to be relatively elastic (in contrast to our findings of limited quantity effects in our context). A richer perspective may consider different labor markets, where compensating differentials may offset wage pressure and help explain wage insensitivity, exactly in jobs prone to unemployment risk. However, several other features of labor markets in general, and the Austrian more specifically, are harder to square with a Walrasian model (e.g., idiosyncratic rent-sharing).

7 Conclusion

We have studied the effects of the value of nonemployment on wages brought about by a unique set of quasi-experimental variation in unemployment insurance benefit levels in Austria, a setting where UI enters the nonemployment scenario for most workers. We have found that wages appear nearly perfectly insulated from the value of nonemployment. While we exploit particular features of the Austrian context, our results are informative for a variety of contexts and debates in macroeconomics and labor economics.

The first implication of our main empirical fact – that at the micro level wages are insensitive to nonemployment scenarios – is that the short-run comovement between *market-level* 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), may arise from economic mechanisms other than fluctuations in workers' outside option in bargaining, such as compositional effects (Hagedorn and Manovskii, 2013; Gertler et al., 2016) or wage pressure from job to job transitions (Moscarini and Postel-Vinay, 2017). Importantly, while we do not find evidence even at two years, our difference-in-differences design cannot provide definite evidence for whether pass-through may occur in the longer run.

Second, the empirical insensitivity of wages to the nonemployment value is inconsistent with the large theoretical sensitivity of commonly used wage setting models such as Nash bargaining specified with nonemployment as workers' outside option. Either the specification to nonemployment scenarios as the relevant outside option is at fault, or even deeper structural assumptions of Nash bargaining. Our research design therefore supports alternative wage setting protocols that insulate wages from nonemployment values. Promising candidates include alternating offer bargaining (Hall and Milgrom, 2008) and models with employer competition and on-the-job search (Cahuc et al., 2006), and perhaps wage posting and monopsony models.

Third, the view that nonemployment values constitute workers' outside options in bargaining according to the Nash protocol, also underlie the active policy debate about the distortion of labor demand from policies that boost workers' nonemployment values, such as unemployment insurance. We complement existing research measuring the reduced-form effects of potential benefit duration on unemployment rates (Hagedorn et al., 2013; Chodorow-Reich et al., 2018), by estimating the wage responses to UI generosity. Consistent with the limited wage effects, we found limited quantity effects on separations and time in unemployment in response to the reforms we study. Our findings suggest that a potential wage pressure channel of UI on job destruction and creation may be small at least in Austria, although again the most narrow reading of our results concerns short-run, micro effects.[68]

Fourth, the empirical insensitivity of wages to the nonemployment value, for which we provide identified microeconometric evidence, is good news for some macroeconomic debates: the *theoretical* insensitivity of wages to the nonemployment value is a crucial ingredient to successful models of aggregate employment fluctuations. In contrast, Nash bargaining, exactly due to its sensitivity to the nonemployment value, stabilizes labor demand reductions as incipient unemployment increases entail wage decreases, largely offsetting the initial labor demand shock, leading models with Nash bargaining to underpredict aggregate employment fluctuations.[69]

[68]This evidence contrasts with an evaluation of the job destruction effects of a large PBD reform for older workers (perhaps at the margin of retirement and thus with higher Frisch elasticities) in Austria in 1989 documented in Jäger et al. (2018).

[69]For example, Chodorow-Reich and Karabarbounis (2016) discuss the business cycle consequences of procyclical instantaneous payoff from nonemployment z for labor market models with the Nash wage bargaining.

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Tables

Table 1: Summary Statistics

	1976 Reform		1985 Reform		1989 Reform		2001 Reform		Pooled Reform	
	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment
Proportion Women	.891	.896	.511	.23	.64	.876	.466	.82	.54	.62
	(0.312)	(0.305)	(0.500)	(0.421)	(0.480)	(0.329)	(0.499)	(0.384)	(0.498)	(0.486)
Age	40.3	40.6	38.9	39.6	38.5	39.7	38.7	39.6	38.8	39.7
	(8.282)	(8.208)	(8.406)	(7.996)	(8.358)	(7.932)	(8.021)	(7.651)	(8.240)	(7.879)
White Collar	.382	.289	.384	.546	.386	.412	.464	.521	.42	.49
	(0.486)	(0.453)	(0.486)	(0.498)	(0.487)	(0.492)	(0.499)	(0.500)	(0.493)	(0.500)
Experience in last 25 Years	10.3	9.8	15.9	18.4	15.1	13.3	15.5	13.4	15.2	15
	(6.375)	(6.055)	(5.968)	(6.118)	(5.821)	(5.798)	(6.258)	(5.994)	(6.230)	(6.616)
Tenure	2.83	2.83	7.22	8.67	7.34	6.32	7.74	6.26	7.2	6.9
	(1.122)	(1.113)	(4.115)	(4.213)	(5.069)	(4.729)	(6.546)	(5.602)	(5.476)	(4.998)
Avg. Monthly Earnings	4269	2655	13455	20696	13900	8529	24268	15683	17456	15253
	(347.579)	(688.626)	(1237.210)	(2103.023)	(1144.354)	(2120.226)	(2074.368)	(3059.542)	(6409.774)	(6060.462)
Observations in Base Year	59222	61149	268708	338999	188362	180839	370786	328345	887078	909332

Note: This table includes summary statistics for the control and treatment regions for the four reforms that make up the pooled sample on which we run our analysis: 1976, 1985, 1989, and 2001. Standard deviations are reported in parentheses beneath the means. All values are calculated from individuals employed all 12 months in the base year for the reform, which is defined as the year prior to the reform, e.g., 1975 for the 1976 reform. The pooled sample appends the four reform samples together. The actual number of observations in the base year will be slightly larger than the sum of the treatment and control groups for the 1985 reform sample and thus the pooled sample because the control region is shifted slightly down the income table to account for repeated treatment in a small section of the income distribution during the placebo period for that reform. 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 the identification assumption that earnings regions do not face differential shocks to earnings growth in the same year after conditioning on earnings percentiles, rich individual-level demographic and industry information, and time-varying firm effects. We confirm the lack of differential trends through nonparametric and parametric placebo checks (see ex. the (lack of) pretends Tables 2 and 3 and nonparametric analysis in Figures A.1 to A.4).

Table 2: Wage Effects at **One-Year Horizon**: Difference-in-Differences Regression Design

	1-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	0.016	-0.002	0.016	0.014	0.022	0.028
	(.017)	(.016)	(.018)	(.016)	(.013)	(.014)
Placebo: 2 Yr Lag	-0.001	-0.014	-0.007	-0.009	0.018	0.014
	(.014)	(.015)	(.015)	(.015)	(.014)	(.014)
Treatment Year	**-0.004**	**-0.001**	**-0.019**	**-0.014**	**0.002**	**-0.000**
	(.016)	**(.017)**	**(.015)**	**(.016)**	**(.013)**	**(.013)**
Base-Year Average	7.304	7.304	7.304	7.304	7.304	7.304
Pre-p F-test p-val	0.532	0.581	0.413	0.381	0.245	0.119
R^2	.048	.067	.076	.094	.257	.281
N (1000s)	7139	7139	7138	7138	6299	6298
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification (33). Standard errors based on two-way clustering at the individual and earnings percentile level are in parentheses. The null hypothesis of the F-test is that the coefficients of interest are jointly all equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.

Table 3: Wage Effects at **Two-Year Horizon**: Difference-in-Differences Regression Design

	2-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
Placebo: 3 Yr Lag	-0.007	-0.025	-0.001	-0.001	-0.003	0.007
	(.021)	(.021)	(.025)	(.024)	(.022)	(.022)
Treatment Year	**-0.007**	**0.007**	**-0.027**	**-0.022**	**-0.022**	**-0.027**
	(.031)	**(.031)**	**(.032)**	**(.03)**	**(.026)**	**(.026)**
Base-Year Average	14.364	14.364	14.364	14.364	14.364	14.364
Pre-p F-test p-val	0.752	0.241	0.966	0.962	0.878	0.742
R^2	.103	.125	.14	.16	.305	.332
N (1000s)	5039	5039	5038	5038	4434	4433
Mincerian Ctrls		X		X		X
4-Digit Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: See Table 2 table note.

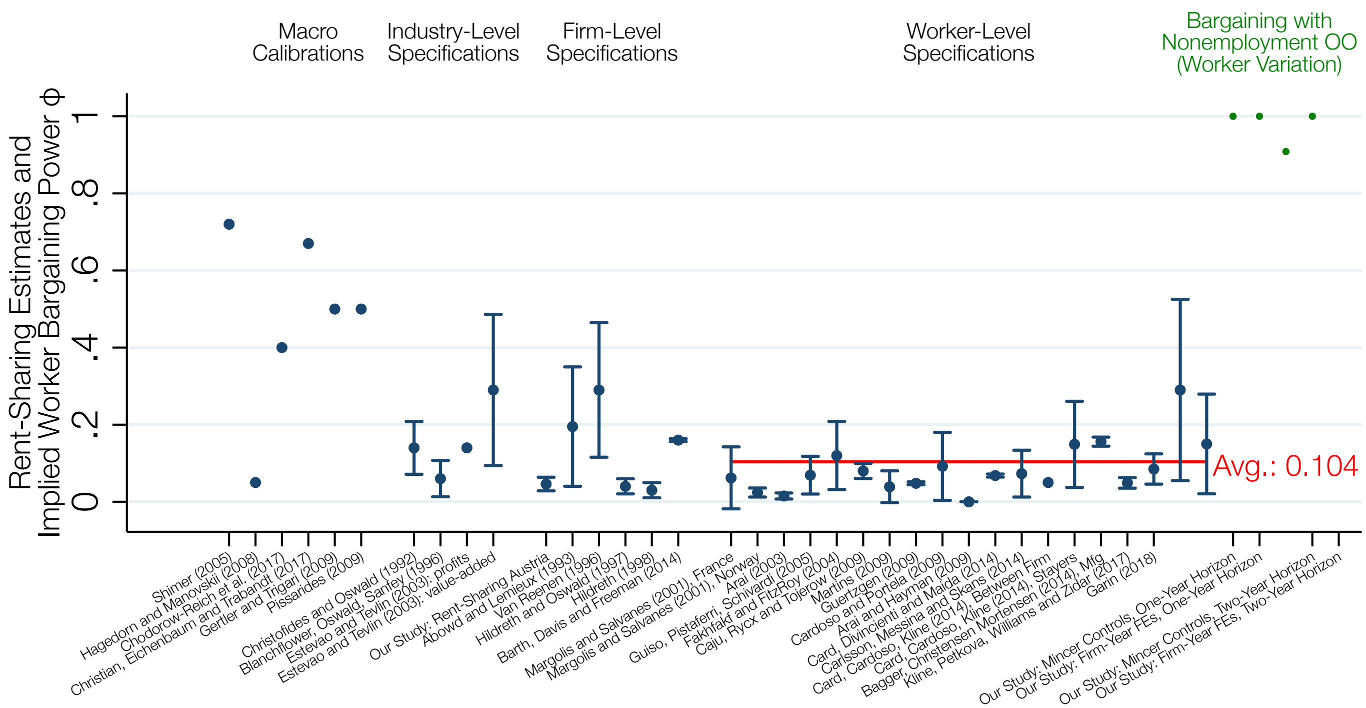
Table 4: Wage Effects: Difference-in-Differences Regression with **Firm-Level Variation**

	1-Year Earnings Effects				2-Year Earnings Effects			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Placebo: 3 Yr Lag	-0.090	-0.081	-0.021	-0.005	-0.094	-0.079	-0.005	0.022
	(.026)	(.025)	(.027)	(.027)	(.034)	(.033)	(.037)	(.037)
Placebo: 2 Yr Lag	-0.056	-0.059	-0.010	-0.008				
	(.023)	(.023)	(.026)	(.026)				
Treatment Year	**0.016**	**0.013**	**0.033**	**0.035**	**0.030**	**0.023**	**0.035**	**0.033**
	(.027)	**(.027)**	**(.027)**	**(.027)**	**(.043)**	**(.043)**	**(.042)**	**(.042)**
Base-Year Average	7.304	7.304	7.304	7.304	14.364	14.364	14.364	14.364
Pre-p F-test p-val	0.002	0.004	0.732	0.952	0.005	0.017	0.888	0.551
R^2	.055	.074	.079	.097	.111	.133	.142	.163
N (1000s)	7139	7139	7138	7138	5038	5038	5038	5038
Mincerian Ctrls		X		X		X		X
4-Digit Ind.-Occ. FEs			X	X			X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification (33) with the variation in benefits aggregated at the firm-level. See Section 5.2 for more details about the construction of the firm-level instrument. Standard errors are in parentheses and clustered at the firm level. The null hypothesis of the F-test is that the coefficients of interest are all jointly equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.

Figures

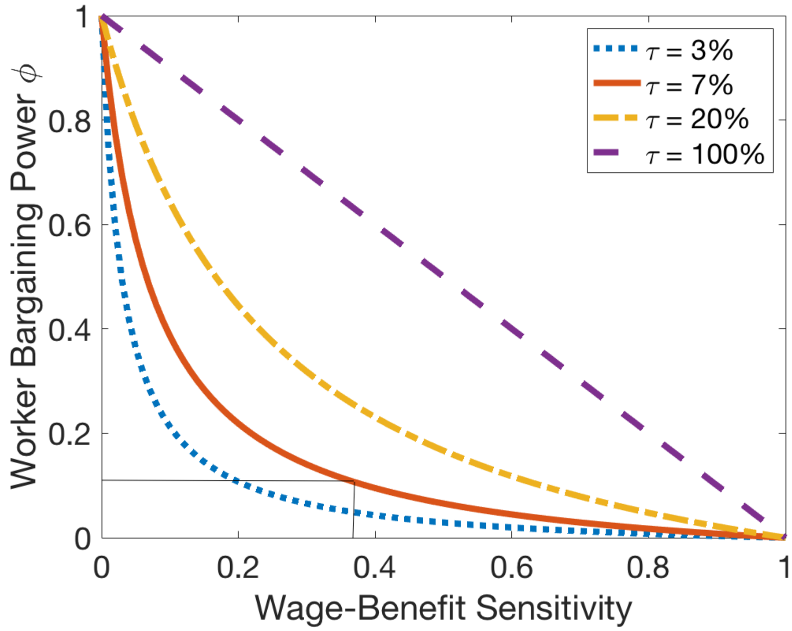
Figure 1: Overview of Estimates and Calibrations of Worker Bargaining Power



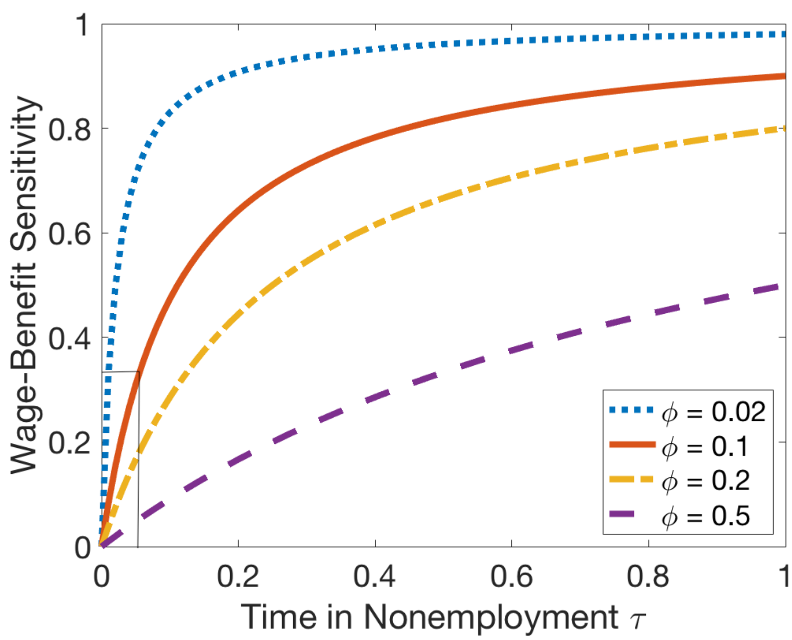
Note: The figure shows an overview of calibrations as well as implied estimates of worker bargaining power. For the calibrations, we plot the values used in the respective papers. For the estimates, we build on the meta-study in Card et al. (2018) and use level-on-level specifications from the papers included in the overview if those are reported. In addition, we add recent estimates from Kline et al. (2017) (Table 9, Panel A, column 1b, for all workers), Garin (2018), 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 Section D). Among the worker-level specifications, we calculate an inverse variance weighted mean of the estimates among those studies that either report level-on-level specifications or rent-sharing elasticities (we omit studies with profit-sharing elasticities since these do not provide bounds for bargaining power). For our study, we plot the implied worker bargaining power under the assumption that nonemployment is the outside option based on the results in Figure 9. Specifically, we plot the implied ϕ based on the estimates in columns (2) and (6) of Tables 2 and 3 and report $\phi = 1$ if the point estimate would imply even higher values.

Figure 2: Model: Worker Bargaining Power ϕ, Time in Nonemp. τ, Wage-Benefit Sensitivity $\frac{dw}{db}$

(a) ϕ and $\frac{dw}{db}$ by τ



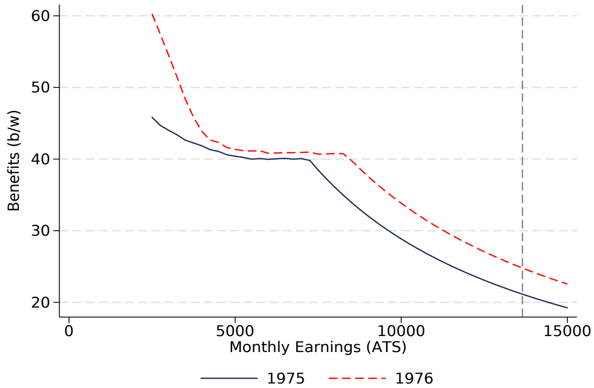
(b) $\frac{dw}{db}$ and τ by ϕ



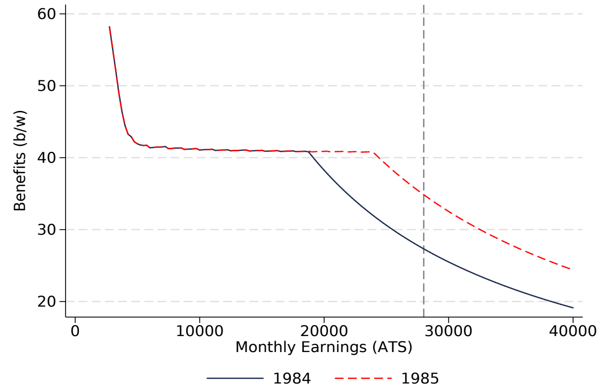
Note: The figure plots the relationship between wage-benefit sensitivity $\frac{dw}{db}$ and worker bargaining power ϕ as predicted by Equations (7) and (6). We vary τ, the post-separation time spent in nonemployment, ($\tau \in \{3\%, 7\%, 20\%, 100\%\}$), and worker bargaining power ϕ ($\phi \in \{0.02, 0.1, 0.2, 0.5\}$). Our calibration ($\tau = 0.07$ and $\phi = 0.1$) predicts a sensitivity of 0.39, depicted in the thin line departing from $\phi = 0.1$, crossing the solid line ($\tau = 7\%$) and ending at the 0.39 sensitivity (top panel), and depicted in the thin line departing from $\tau = 0.07$, crossing the solid line ($\phi = 0.1$) in the bottom panel.

Figure 3: Unemployment Benefit Schedules and Reforms

(a) 1976 Reform

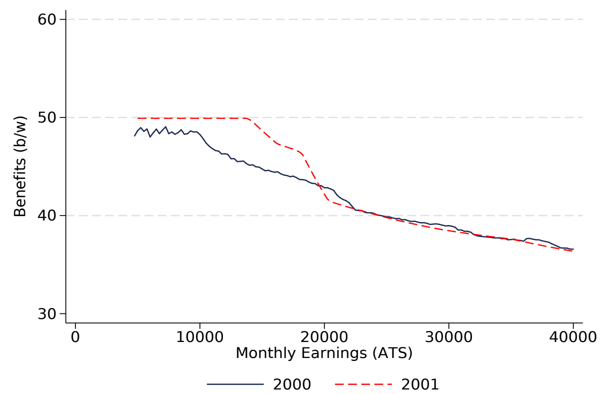


(b) 1985 Reform

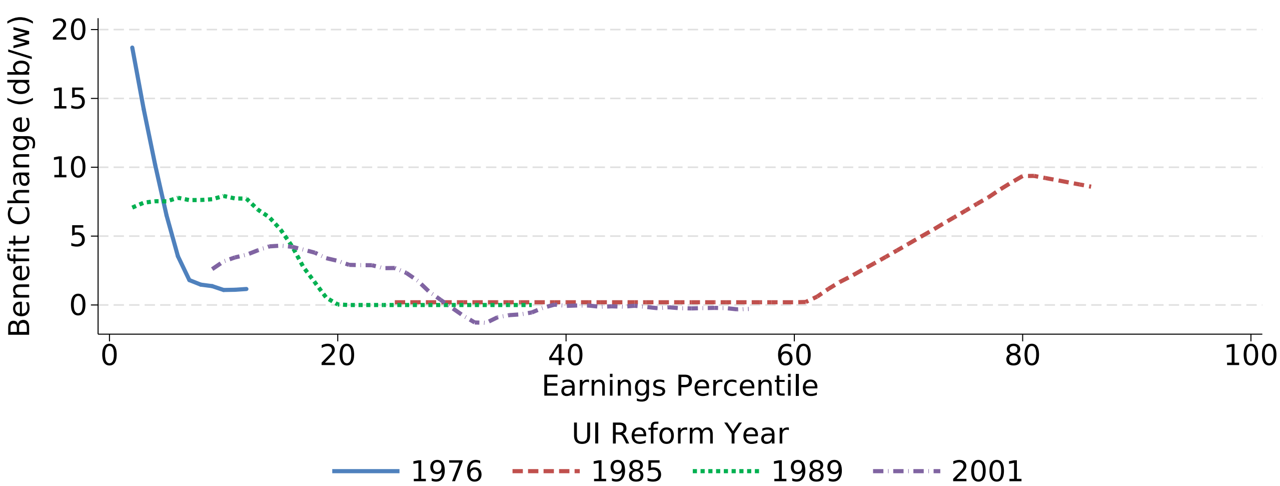


(c) 1989 Reform

(d) 2001 Reform

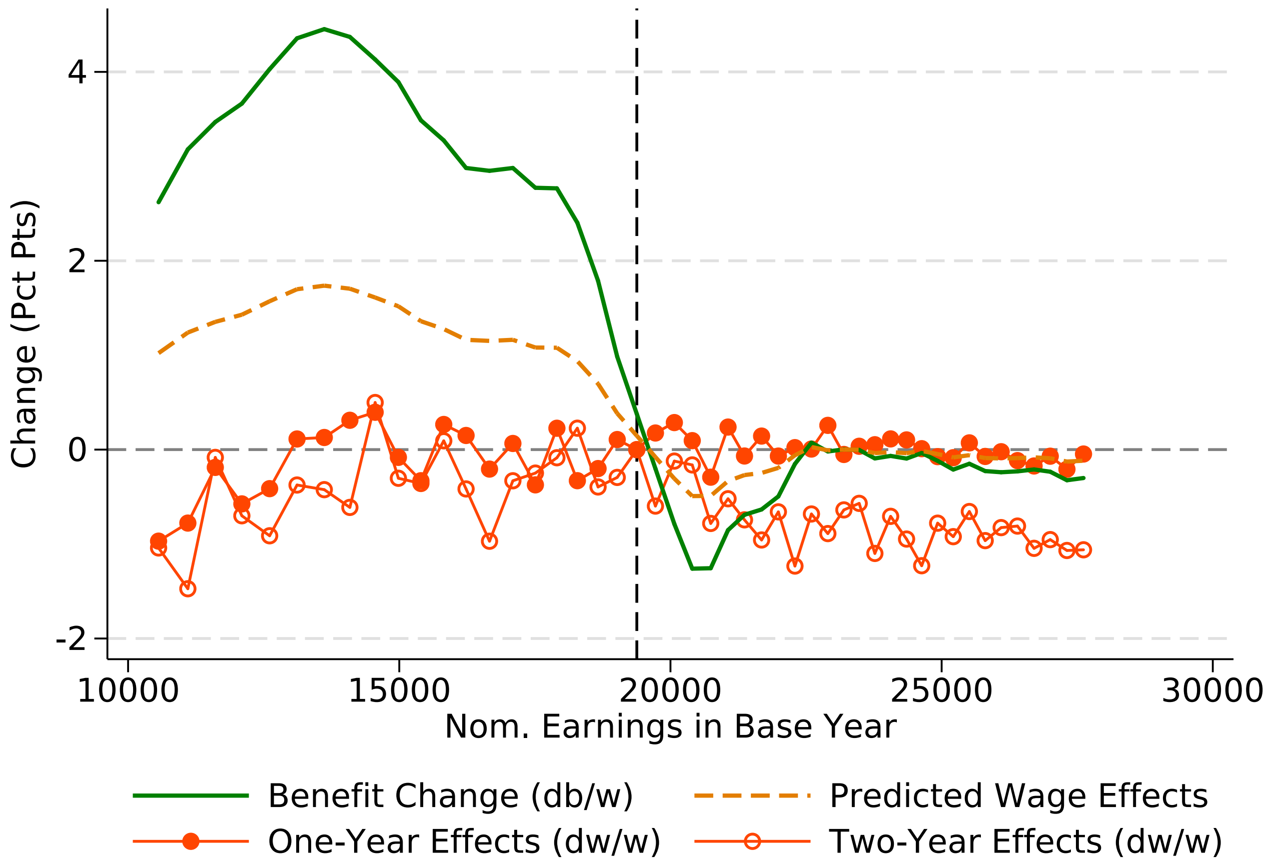


(e) Reform Benefit Changes by Earnings Percentiles



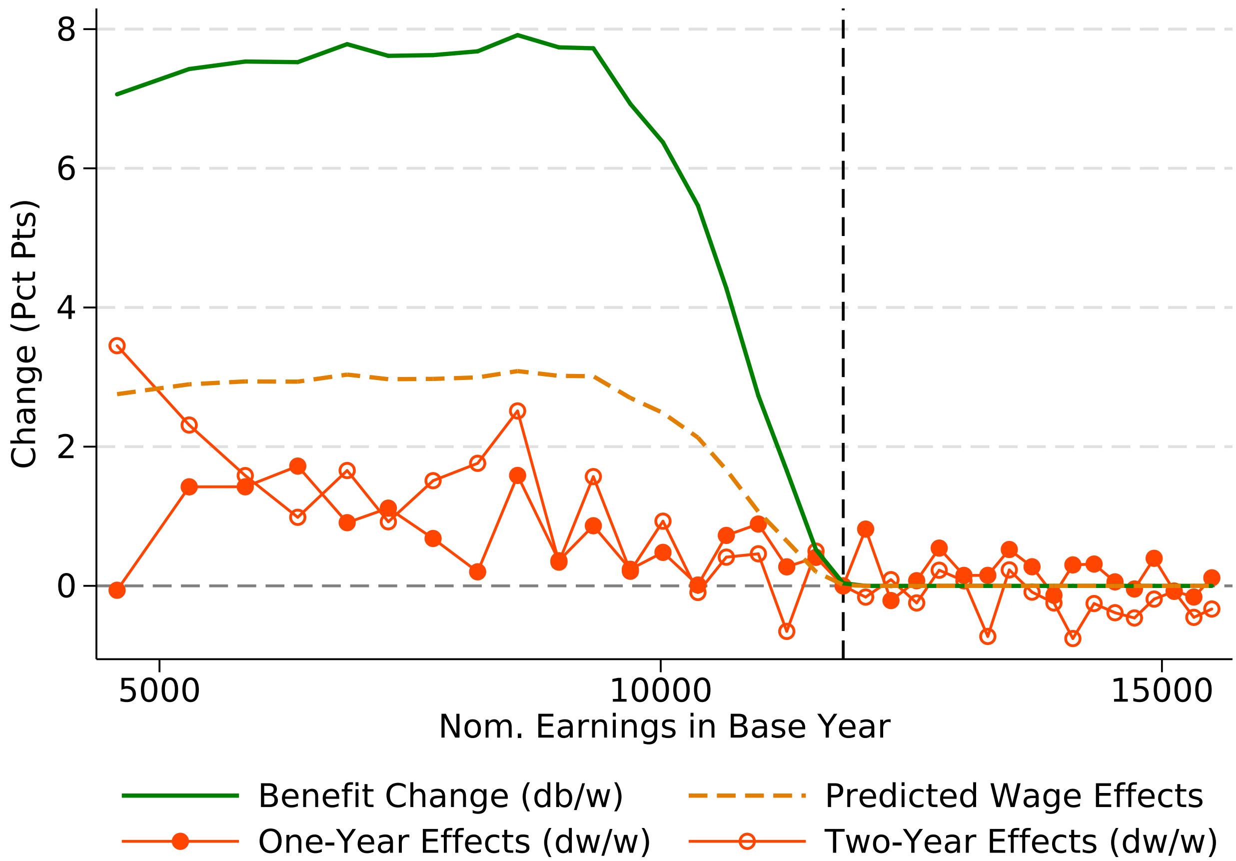
Note: Figures (a)-(d) plot the unemployment benefit schedule before and after each of the four reforms we analyze. The x-axis shows the income relevant for calculating benefits while the y-axis plots the benefits, calculated as the unemployment benefits divided by income. The dashed vertical line shows the social security earnings maximum, which caps our earnings data, if it appears in the gross earnings range. Figure (e) plots the reform induced benefit change for each reform in earnings percentile space.

Figure 4: 2001 Reform: Benefit Changes and Wage Effects



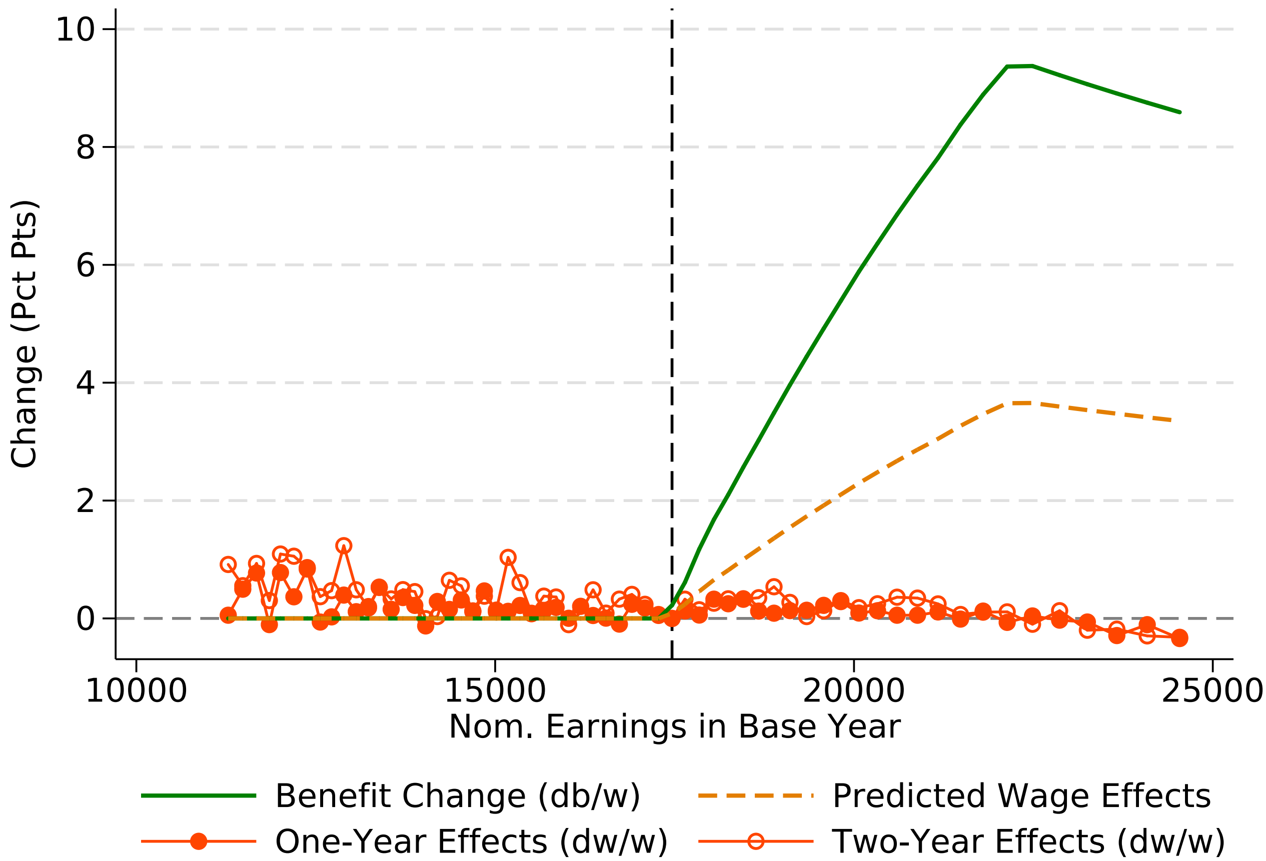
Note: The figure plots reform-induced replacement rate changes and wage effects for the 2001 reform. Observations are binned by their base year (2000) earnings percentile on the x-axis. The 2001 reform increased replacement rates below the 32nd percentile as indicated by the green line. The dashed orange line indicates the wage growth that the 2001 reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.39 The red circles indicate the wage effects that the reform induced at the one- and two-year horizon. We calculate wage effects at the percentile level by calculating wage growth experienced by individuals in that percentile and subtracting the wage effect that a given percentile experienced in a placebo pre-period two years or three years before the reform for the one- and two-year wage effects, respectively. We then normalize the wage effect to zero in the lowest percentile that did not experience a reform-induced replacement rate change in 2001, denoted by the vertical dashed line. Section 4.2 provides more information.

Figure 5: 1989 Reform: Benefit Changes and Wage Effects



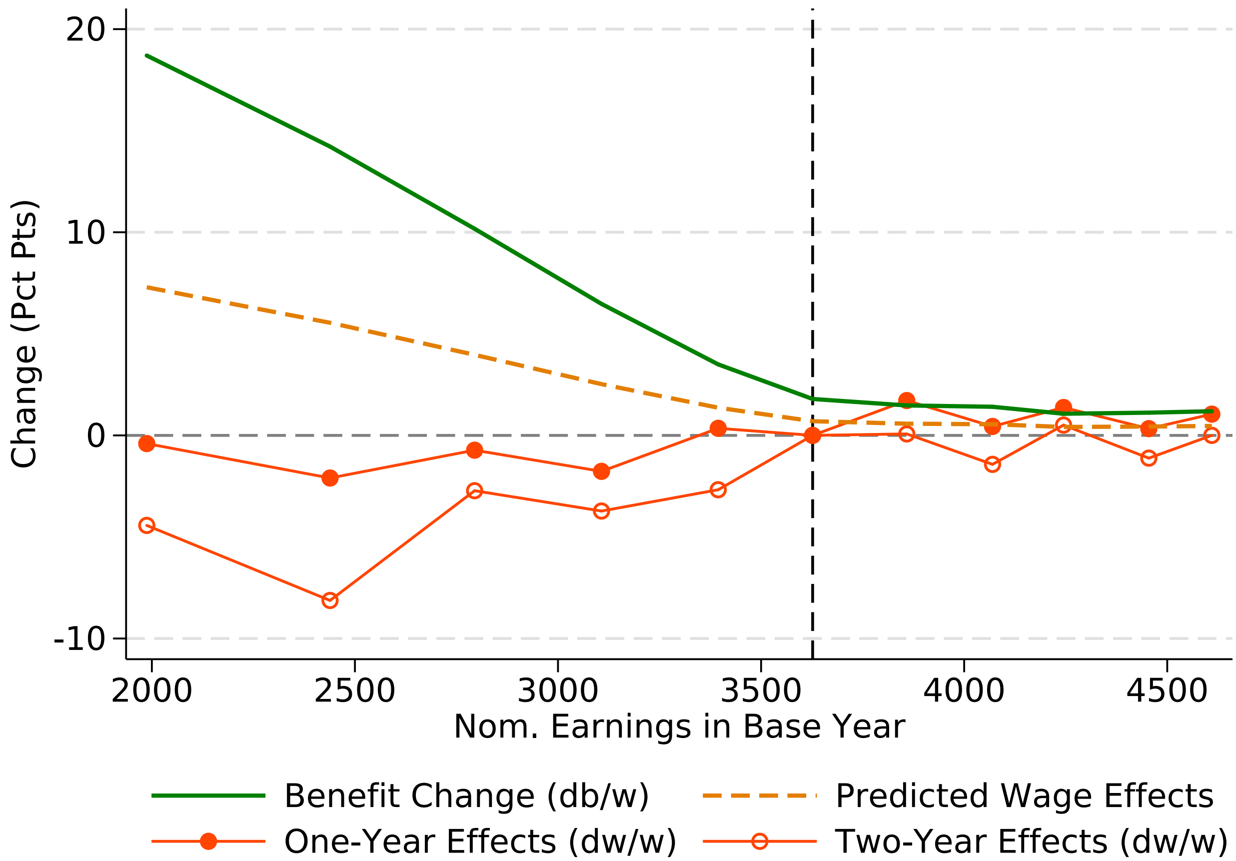
Note: The figure plots reform-induced replacement rate changes and wage effects for the 1989 reform. Observations are binned by their base year (1988) earnings percentile on the x-axis. The 1989 reform increased replacement rates below the 19th percentile as indicated by the green line. The dashed orange line indicates the wage growth that the 1989 reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.39. The red circles indicate the wage effects that the reform induced at the one- and two-year horizon. The reform was implemented in June 1989 and the one- and two-year horizon effects refer to wage growth from 1988 to 1989 and 1990, respectively. We calculate wage effects at the percentile level by calculating wage growth experienced by individuals in that percentile and subtracting the wage effect that a given percentile experienced in a placebo pre-period two years or three years before the reform for the one- and two-year wage effects, respectively. We then normalize the wage effect to zero in the lowest percentile that did not experience a reform-induced replacement rate change in 1989, denoted by the vertical dashed line. Section 4.2 provides more information.

Figure 6: 1985 Reform: Benefit Changes and Wage Effects



Note: The figure plots reform-induced replacement rate changes and wage effects for the 1985 reform. Observations are binned by their base (1984) year earnings percentile on the x-axis. The 1985 reform increased replacement rates above the 61st percentile as indicated by the green line. The dashed orange line indicates the wage growth that the 1985 reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.39. The red circles indicate the wage effects that the reform induced at the one- and two-year horizon. We calculate wage effects at the percentile level by calculating wage growth experienced by individuals in that percentile and subtracting the wage effect that a given percentile experienced in a placebo pre-period two years or three years before the reform for the one- and two-year wage effects, respectively. We then normalize the wage effect to zero in the highest percentile that did not experience a reform-induced replacement rate change in 1985, denoted by the vertical dashed line. Section 4.2 provides more information.

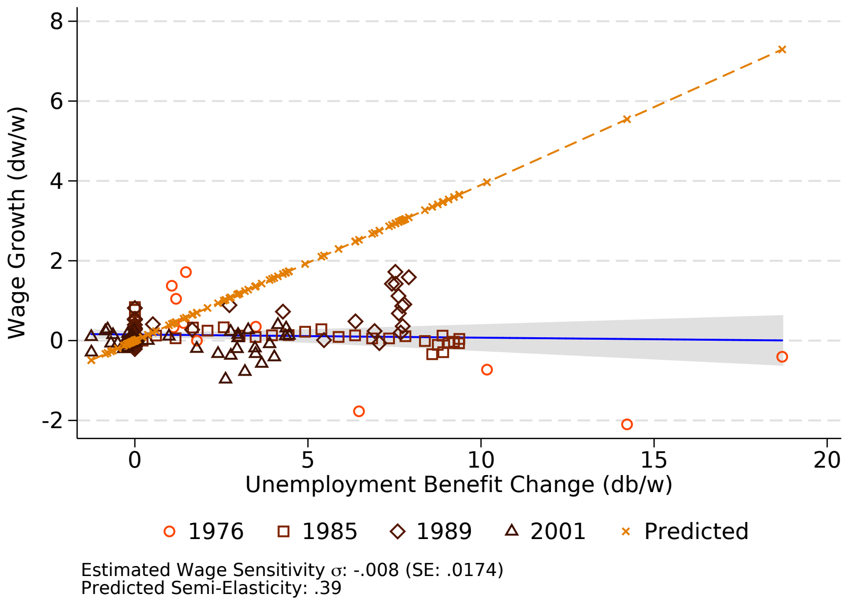
Figure 7: 1976 Reform: Benefit Changes and Wage Effects



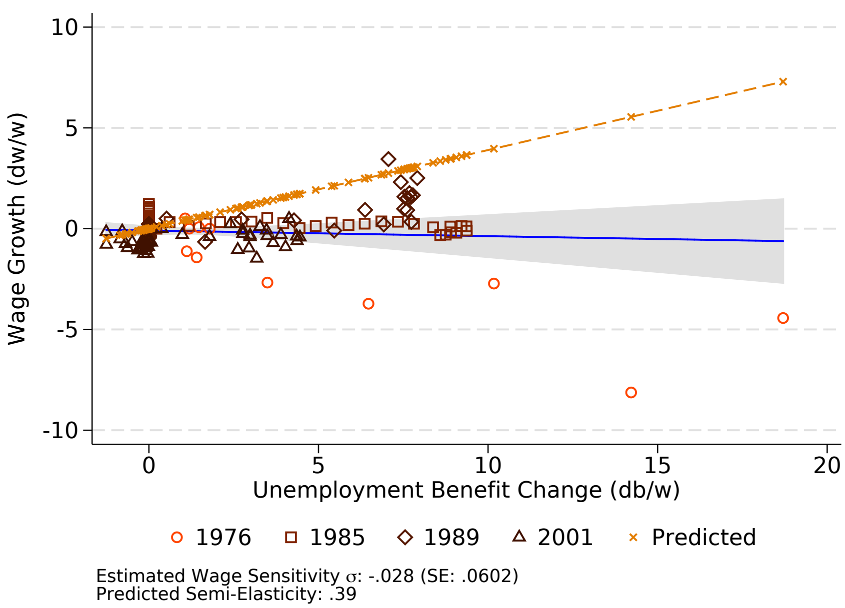
Note: The figure plots reform-induced replacement rate changes and wage effects for the 1976 reform. Observations are binned by their base year (1975) earnings percentile on the x-axis. The 1976 reform increased replacement rates below the 6th percentile as indicated by the green line. The dashed orange line indicates the wage growth that the 1976 reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.39. The red circles indicate the wage effects that the reform induced at the one- and two-year horizon. The reform was implemented in June 1976 and the one- and two-year horizon effects refer to wage growth from 1975 to 1976 and 1977, respectively. We calculate wage effects at the percentile level by calculating wage growth experienced by individuals in that percentile and subtracting the wage effect that a given percentile experienced in a placebo pre-period two years or three years before the reform for the one- and two-year wage effects, respectively. We then normalize the wage effect to zero in the lowest percentile that did not experience a reform-induced replacement rate change in 1976, denoted by the vertical dashed line. Section 4.2 provides more information.

Figure 8: Scatter Plots of Wage Growth and Unemployment Benefit Changes

(a) One-Year Horizon

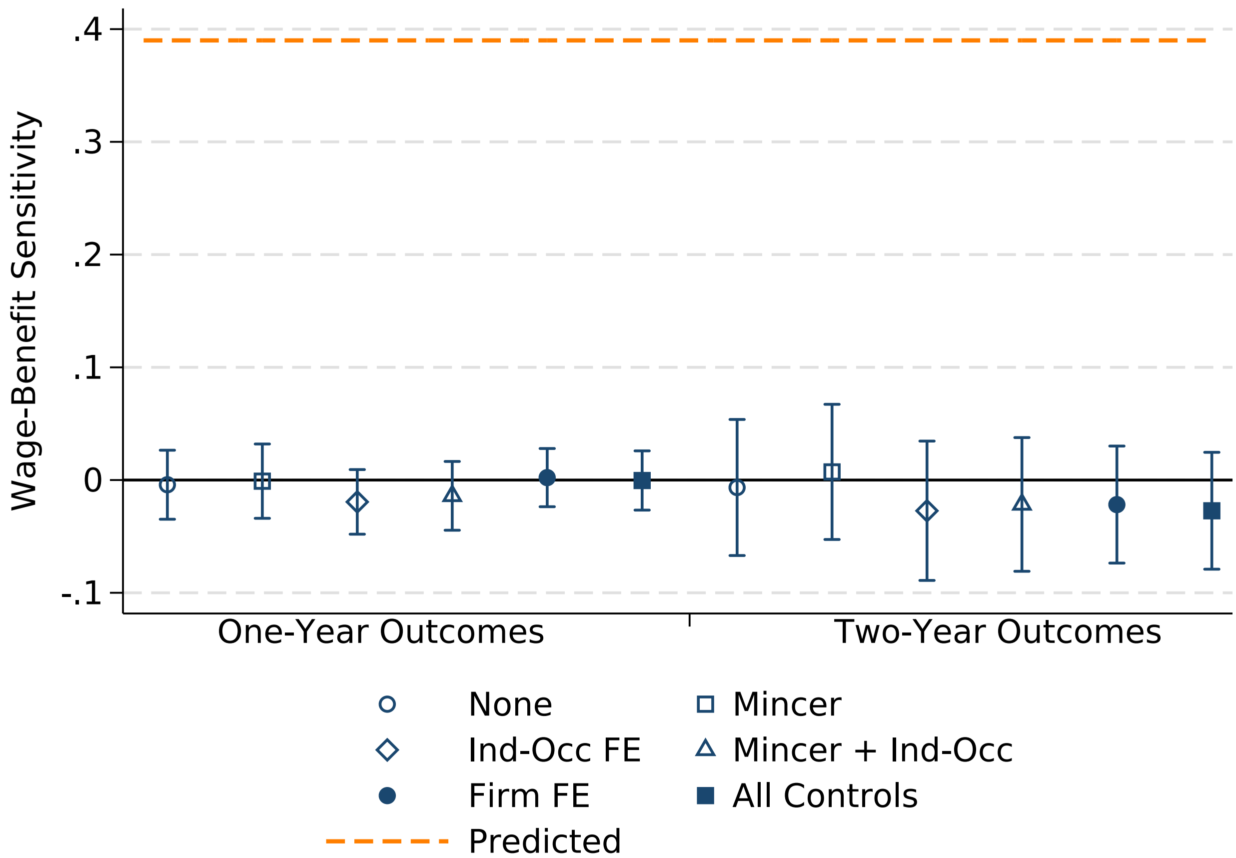


(b) Two-Year Horizon



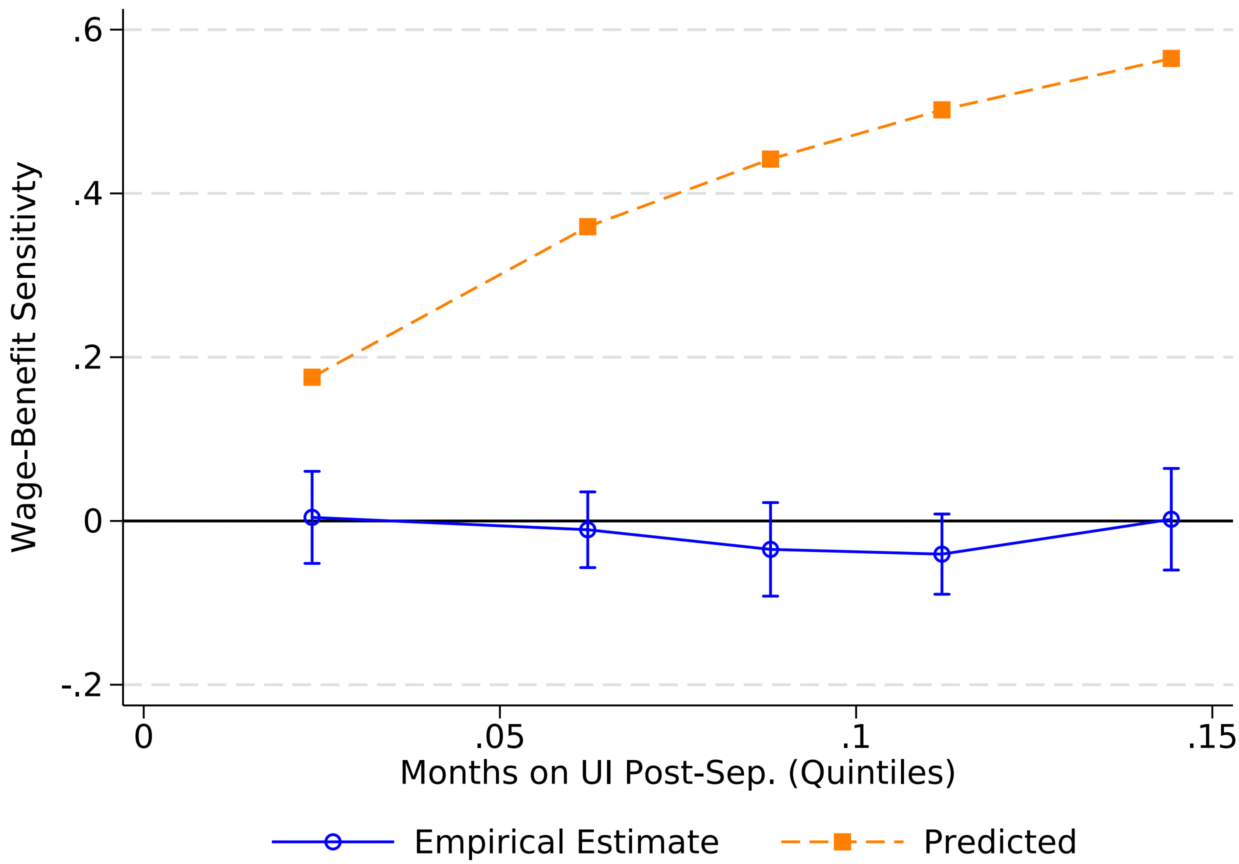
Note: The figures show scatter plots of wage growth (y-axis) and reform-induced replacement rate changes (x-axis), db/w, pooling the four reforms outlined in Figures 4 through 7. Each dot corresponds to a percentile observation from one of the 4 through 7. The upper panel shows wage effects after one year and the lower panel effects after two years. The orange cross marks indicate the predicted wage growth that the reforms would have induced in the calibrated bargaining model with a wage-benefit sensitivity of 0.39. The remaining symbols indicate actual datapoints for wage growth and benefit changes. The estimated wage sensitivities $\hat{\sigma}$ are calculated as the slope of wage growth with respect to changes in the benefit level.

Figure 9: Wage Effects: Difference-in-Differences Regression Design



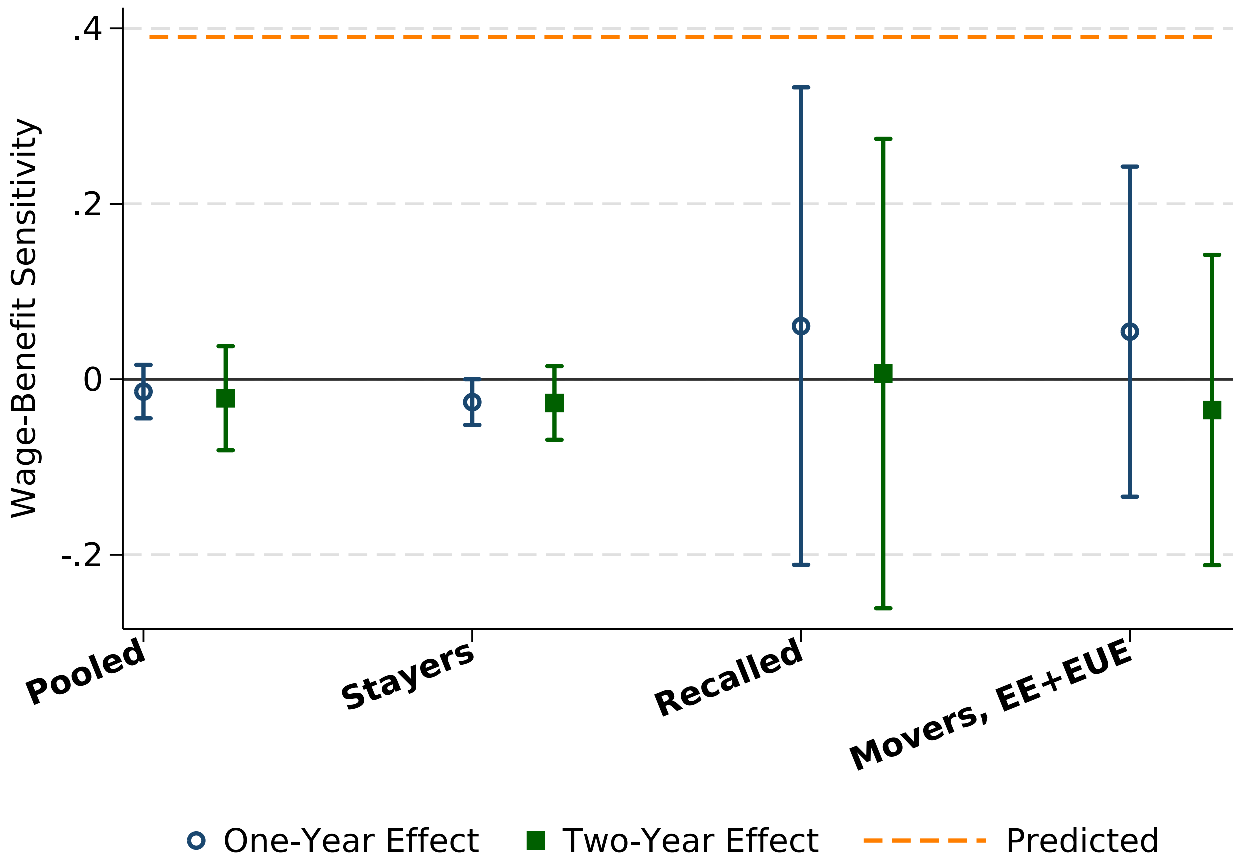
Note: The figure shows the effects of nonemployment value shifts, db/w, on wages based on the difference-in-differences specification in (33). It plots the estimated σ_0 coefficients and associated confidence intervals as reported in Tables 2 and 3. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The Mincerian controls include time-varying polynomials of experience, tenure, and age; and a time-varying gender indicators. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation. Firm FE indicates that time-varying firm-fixed effects were included. A validation analysis relating predicted and realized benefit changes is reported in Appendix Table A.2. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level.

Figure 10: Heterogeneity of Wage-Benefit Sensitivity by Predicted Time in Unemployment τ_i: Theoretical Prediction from Calibrated Bargaining Model vs. Empirical Estimate



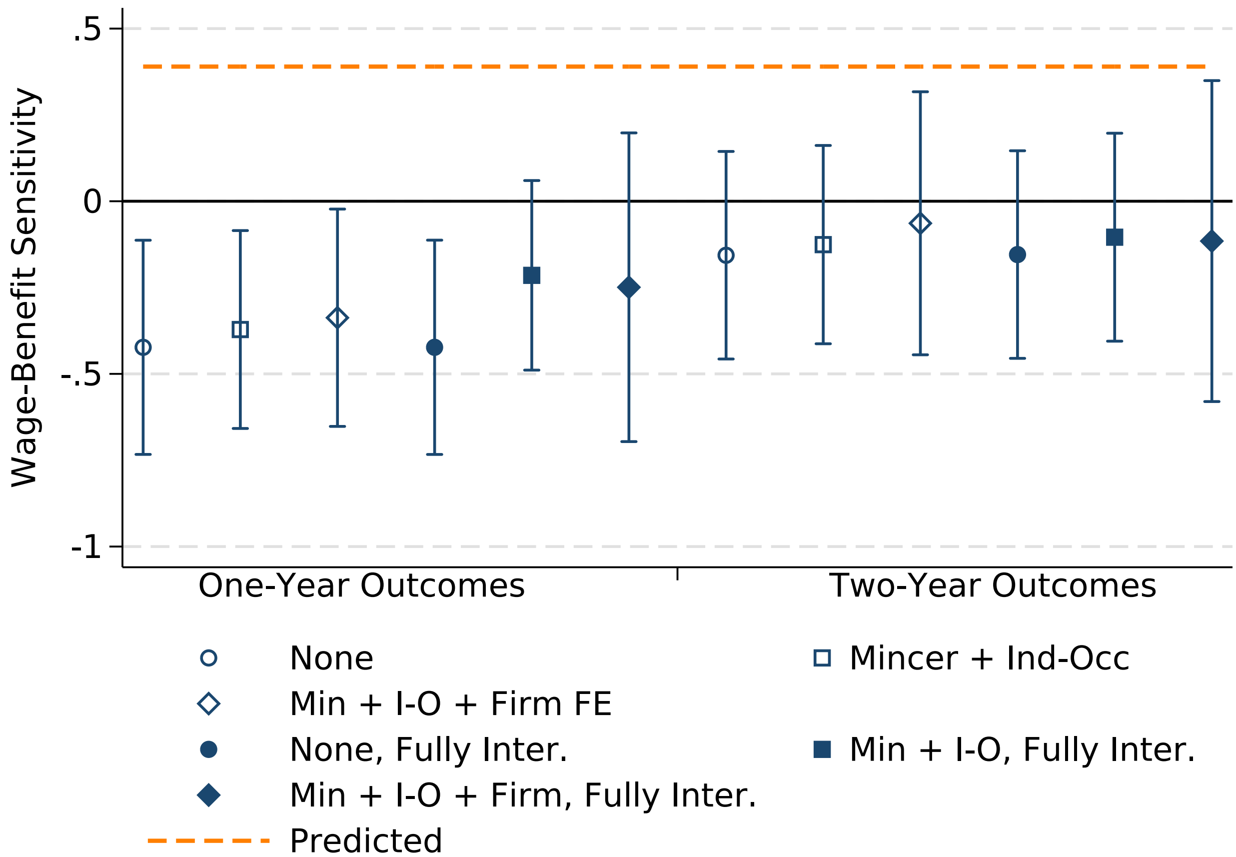
Note: The graph presents wage-benefit sensitivities for workers sorted by their predicted fraction of time in unemployment conditional on a separation ("τ") over the subsequent 16 years (the longest horizon we can track workers over while including the 2001 reform). The x-axis sorts these workers into five quintiles and traces out the median value per quintile. For example, "0.1" indicates that the median worker is expected to spend 19.2 out of $12 \cdot 16 = 192$ months in unemployment. "Unemployment" means receipt of unemployment insurance benefits for the calendar month. The detailed variable construction is in Appendix Section G. The yellow (top) line plots predicted wage-benefit sensitivity on the basis of each worker's idiosyncratic predicted τ_i. Predictions are from a regression model using pre-separation attributes over the sample of actual separators; the model is described in the main text. The wage sensitivity is estimated following structural Equation (6) and based on a Nash bargaining model with worker bargaining power $\phi = 0.1$.

Figure 11: Wage Effects: DiD Regression Design by Transition Type



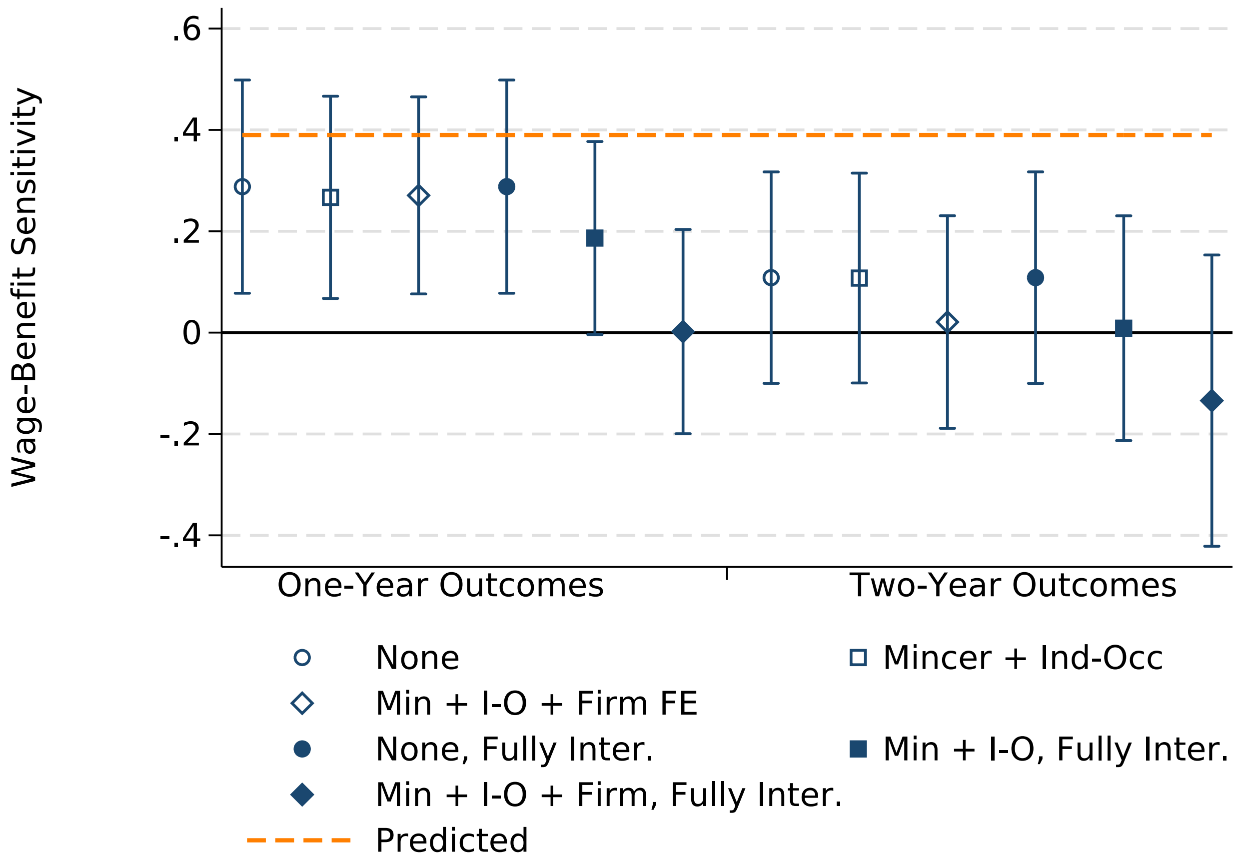
Note: The figure shows σ_0 coefficients from estimating Equation (33) but interacting an indicator for each transition type with the σ_0 and σ_e coefficients in Equation (33). We also vary the parametric earnings controls by transition type, allowing for differential earnings growth patterns by transitions type. The estimates are from specification (4) in Tables 2 and 3 that include Mincerian and industry/occupation controls but not the firm-by-year fixed effects. *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* 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). *Movers, EE+EUE* refers to individuals who move to another employer either with or without and intermediate unemployment spell.

Figure 12: Wage Effects: Employment-Unemployment-Employment Movers



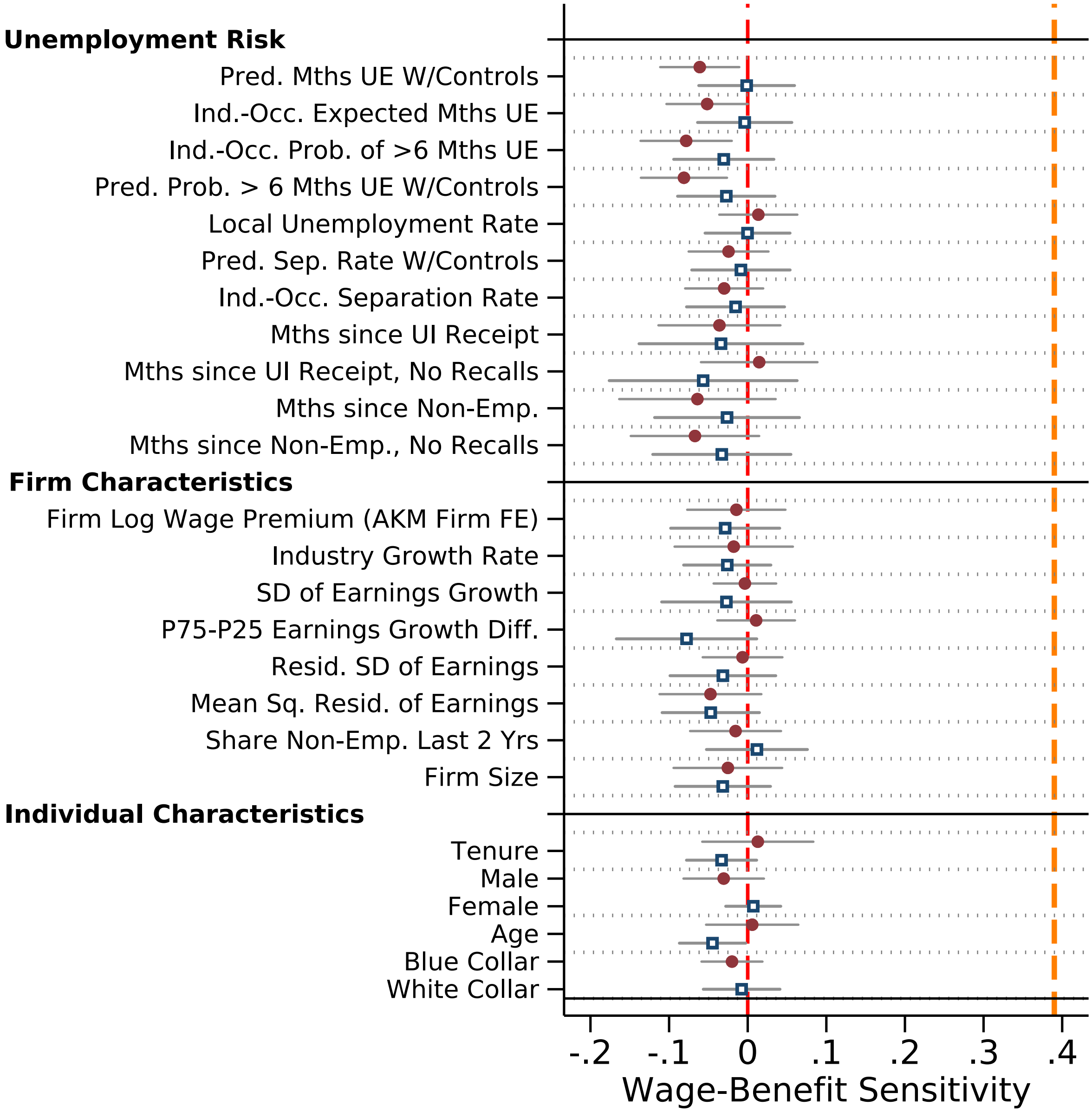
Note: The figure shows σ_0 coefficients from estimating Equation (33) but interacting an indicator for each transition type with the σ_0 and σ_e coefficients in Equation (33). We also vary the parametric earnings controls by transition type, allowing for differential earnings growth patterns by transitions type. From each such regression, the figure reports the coefficients for EUE movers specifically. The estimates show robustness for a variety of specifications: year-specific Mincerian controls, year-specific industry/occupation controls, firm-by-year fixed effects. "Fully int." means that we fully interact all controls (except for firm-by-year fixed effects) with the transition type. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level, indicating that 8 out of 12 estimates are not statistically different from zero.

Figure 13: Wage Effects: Direct Job-to-Job (Employment–Employment) Movers



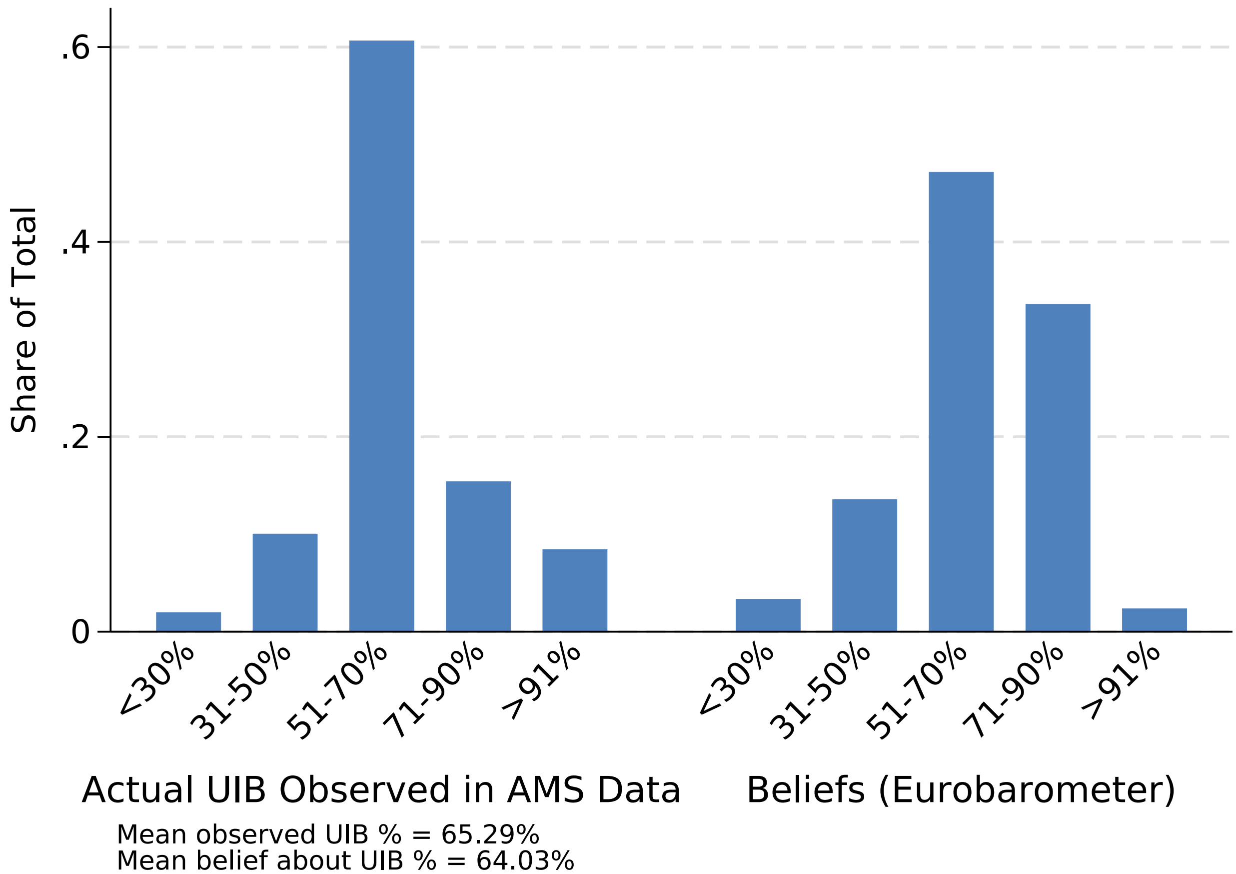
Note: The figure shows σ_0 coefficients from estimating Equation (33) but interacting an indicator for each transition type with the σ_0 and σ_e coefficients in Equation (33). We also vary the parametric earnings controls by transition type, allowing for differential earnings growth patterns by transitions type. From each such regression, the figure reports the coefficients for EE movers specifically. The estimates show robustness for a variety of specifications: year-specific Mincerian controls, year-specific industry/occupation controls, firm-by-year fixed effects. "Fully int." means that we fully interact all controls (except for firm-by-year fixed effects) with the transition type. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level, indicating that 7 out of 12 estimates are not statistically different from zero.

Figure 14: Heterogeneity of Nonemployment Effects on Wages: One-Year Effects



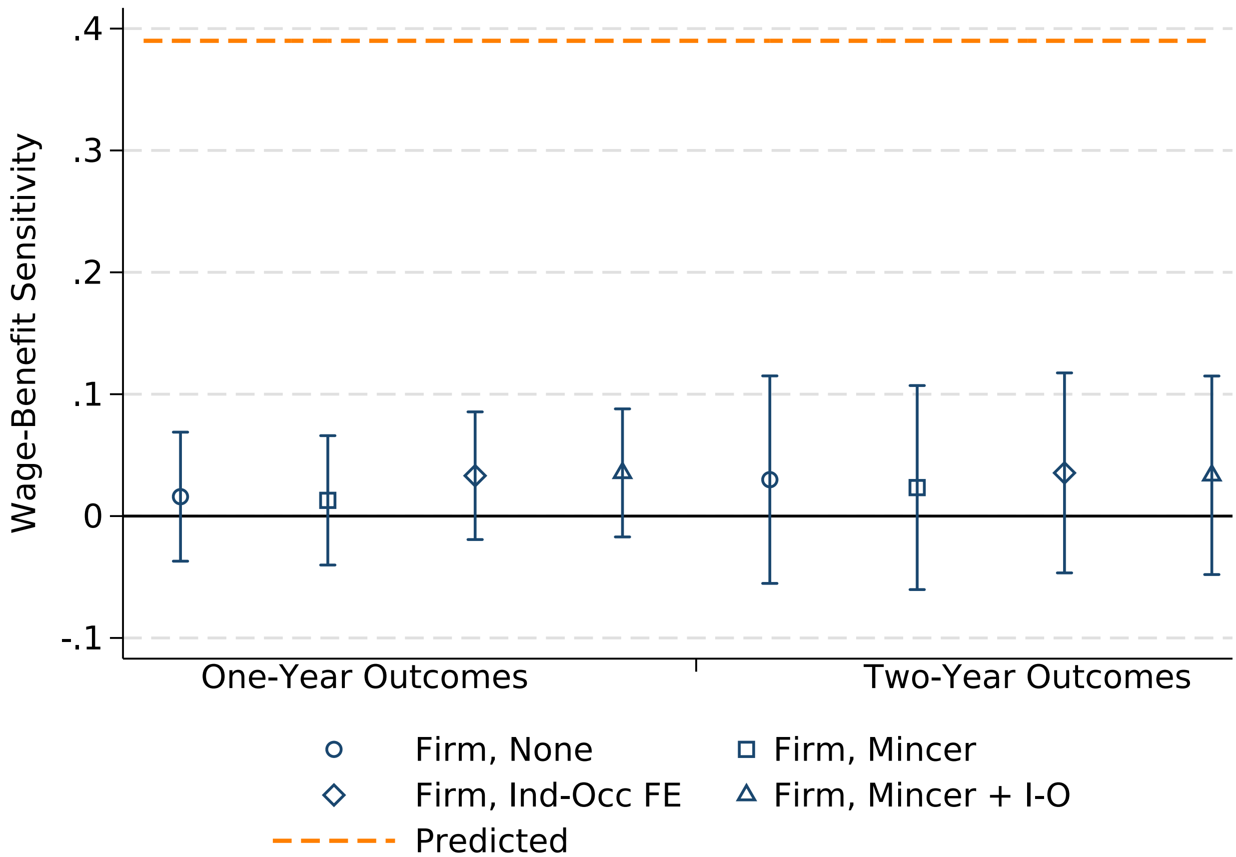
Note: The figure shows σ_0 coefficients from estimating Equation (33) but interacting an indicator for each different heterogeneity group category with the σ_0 and σ_e coefficients in Equation (33). We also vary the parametric earnings controls by heterogeneity type, allowing for differential earnings growth patterns by heterogeneity type. The estimates are from specification (4) in Tables 2 and 3 that include Mincerian and industry/occupation controls but not the firm-by-year fixed effects. See Section 5 and Appendix G for more details about the construction of each heterogeneity group. For all the categories except for sex and occupation, the top red estimate is for individuals with the lowest values of that heterogeneity group and the bottom blue estimate is for individuals with the highest values. For the investigations regarding months since most recent UI receipt/nonemployment, we also relax the sample restriction requiring 12 months of employment in the base year to pick up workers recently hired specifically .

Figure 15: Beliefs About UI Benefit Levels Among Employed Workers



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

Figure 16: Wage Effects: DiD Regression Design with Firm-Level Treatment



Note: The figure shows the effects of nonemployment value shifts, db/w, on wages based on the difference-in-differences specification in (33) with the variation in benefits aggregated at the firm- and industry-level. See Section 5.2 for more details about the construction of the firm-level instrument. The figure plots the estimated wage sensitivity to benefit changes aggregated at the firm level as well as the associated confidence intervals as reported in Table 4. The navy symbols are the estimated treatment effects. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.