

# 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 or monopsony. 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 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. Our worker-level analysis reveals an insensitivity of wages 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 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. This insensitivity of wages to the nonemployment value presents a puzzle to widely used wage setting protocols and implies that nonemployment may not constitute a relevant threat point in bargaining. 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.<sup>1</sup> 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.<sup>2</sup> This framework has shaped policy debates such as the one pertaining the wage pressure channel of unemployment insurance.<sup>3</sup> Nonemployment values also determine wages in important non-bargaining wage determination models: in wage posting models, for instance, the value of nonemployment is a cornerstone scaling the equilibrium wage distribution by factoring into workers’ reservation wages.<sup>4</sup> The theoretical degree of wage sensitivity to fluctuations in the value of unemployment also determines the capacity of macroeconomic models to generate realistic employment fluctuations.<sup>5</sup> Yet, there exists no direct empirical estimate of the sensitivity of wages to the value of nonemployment.

We estimate the dollar-for-dollar sensitivity of wages to 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, by which wages are the average of the job’s inside value (e.g., productivity) and the worker’s outside option, weighted by worker bargaining power  $\phi$ :

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

Shifts in the worker outside option – such as from UIBs – should pass through into wages by one minus worker’s bargaining power. Specifically, our calibrated bargaining model predicts that

<sup>1</sup>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).

<sup>2</sup>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).

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

<sup>4</sup>See, e.g., Manning (2011) or Burdett and Mortensen (1998) for the effect of the nonemployment flow value on wages in wage posting models.

<sup>5</sup>See, e.g., Shimer (2005), Hall and Milgrom (2008), Chodorow-Reich and Karabarbounis (2016) and Hall (2017).

wages increase by \$0.39 whenever UIBs increase by \$1.00. This effect comes from two margins: first, while the worker is unemployed, her instantaneous payoff is shifted. Second, re-employed wages also respond to the outside option boost, generating a built-in feedback effect. We show that this prediction holds across a broad set of model refinements of bargaining models and is robust to incorporating various features such as finite benefit duration, to micro re-optimization such as endogenous job search (due to the envelope theorem) and to market-level adjustment (if netted out by a control group not affected by the shift in benefits).

We document that real-world wages appear *insensitive* to sharp increases in workers' UI benefit levels. Point estimates for the effect of benefit changes on wages are less than \$0.01 after one and two years. Our confidence intervals reject that a \$1.00 increase UI benefits value increases wages by more \$0.03 after two years. The insensitivity holds across various subsets of workers such as those with higher unemployment risk and even new hires. These estimates are one to two orders of magnitude smaller than the calibrated model predicts. This insensitivity presents 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. This interpretation is however empirically rejected by existing small estimates of firm-level rent sharing (inside option shifts from productivity), which imply a worker bargaining power of around 0.1.<sup>6</sup> 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.

The setting of our study is a unique set of four large reforms that generated quasi-experimental variation in UI benefit levels in Austria, in 1976, 1985, 1989 and 2001.<sup>7</sup> Reforms to benefit levels are rarer than changes in potential UI benefit duration, whose effects on the nonemployment value are harder to price. The reforms raised benefits for subsets of workers, for example by as much as 28% in 1985. These reforms differentially affected different groups of workers based on workers' *previous salaries* (the UI reference wage that determines benefits in the event of a UI claim). Exploiting this feature, we implement a difference-in-differences design comparing wage growth of workers affected by the reform (treatment group) to those of 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 wages and other labor force statuses.

Besides the unique set of UIB reforms, the Austrian UI system provides particularly clean variation in nonemployment values for institutional reasons. First, Austrian unemployment insurance affects the nonemployment value for most workers. Conditional on a separation, most

<sup>6</sup>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.

<sup>7</sup>Only the 1989 reform has been studied, with a focus on unemployment duration (Lalive et al., 2006).

workers receive UI due to broad eligibility and high take-up, due to 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.<sup>8</sup> Third, Austrian UI does not feature experience rating. Fourth, after UI benefit exhaustion, a significant share of Austrian workers receives means-tested unemployment assistance that, while lower in levels, moves nearly one-to-one with UIB levels.

We first analyze each reform nonparametrically in scatter plots of raw data. On the x-axis, we sort annual cross sections of workers by their UI reference wage, a pre-determined lagged wage. On the y-axis, we plot the *predicted* wage growth effect from our calibrated bargaining model, tracing out 0.39 times the benefit variation, as clearly visible wage effects of multiple percentage points. We also plot *realized* wage growth before and after the reform, as well as the difference, to detect a treatment effect. A visual inspection of the raw data does not exhibit any such wage increases, in any of the reforms we examine. Even after two years, the wage gradients remain parallel to pre-reform years and clearly show no response of wages to the benefit shift, in stark contrast to the predicted large wage effects.

Next, we estimate the wage-UI sensitivity in a regression-based difference-in-differences analysis. The point estimates of the sensitivity are smaller than 0.01, with tight confidence intervals allowing us to rule out a \$0.03 wage-benefit sensitivity in response to a \$1.00 benefit shift. Here, we also formally test our identification assumptions with placebo tests and can additionally include a rich set of control variables. By including firm-by-year effects, for instance, we can leverage sharp 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 more time in unemployment. Yet, when we split up workers by the predicted time on UI post-separation, *all* groups exhibit a zero wage sensitivity to UI. The model would have predicted a steep gradient, ending in a wage sensitivity of .60 for the the top quintile. Relatedly, we also 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 larger wage effects.

We rule out a variety of confounders generating the wage insensitivity. First, while wage stickiness may mask wage effects in existing matches, we document that the insensitivity extends to new hires entering new jobs (incl. with intervening UI receipt), where wages are presumably set more flexibly.<sup>9</sup> We also find no incidence after two years, or in firms with more flexible

<sup>8</sup>The only differentiation between quits and layoffs in the UI system is a 28-day wait period with subsequently *full* benefit duration and levels. 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. Rothstein (2011) exploits the ineligibility of U.S. quitters for unemployment insurance to estimate the effect of potential benefit duration extensions on unemployment spell durations. Hagedorn et al. (2013) discuss de-facto UI eligibility of U.S. quitters.

<sup>9</sup>Moreover, wages in new jobs are allocative for the hiring margin.

wage policies (with greater pre-existing dispersion in wage levels and growth). Lastly, since the reforms increased generosity and should entail wage *increases*, downward wage rigidity would not bind.

Second, perhaps nonemployment is workers' threat point in bargaining, but actors do not perceive UI to be as part of that value, due to limited knowledge, attention or salience. However, our result extends even to workers with particularly frequent interaction with the UI system, who are plausibly more aware of the UIB schedule (see Lemieux et al., 1995; Lemieux and MacLeod, 2000). Moreover, we 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* of UIBs, but find that these reforms do not lead to wage increases among incumbent workers either.

Third, we also investigate whether our findings could be explained by bargaining occurring at the firm rather than at the individual level.<sup>10</sup> 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 Austria is heavily unionized, it leaves substantial room for idiosyncratic deviation from the collectively bargained wage floors: actual wages are more than a third higher than these wage floors (Leoni and Pollan, 2011), suggesting substantial scope for firm-level or idiosyncratic negotiations, in line with the institutional fact that establishments are free to deviate upwards (as would be predicted in the reforms we study).

Fourth, our evidence may be consistent with wage setting mechanisms featuring no bargaining. 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 suggests significant scope for bargaining in real-world wage setting. Yet, we do not find evidence for larger wage effects in pockets of the labor market with more scope for bargaining suggested by such surveys. In fact, our long battery of heterogeneity analyses reveals that the insensitivity extends across diverse worker and firm subgroups, states of the aggregate or local business cycles.

Fifth, our robustness checks also reveal that boosting the nonemployment value does not seem to lead larger turnover (making composition effects unlikely) or sickness spells (perhaps ruling out efficiency wage mechanisms in our context).

Alternative bargaining protocols in which most employed workers wield other job offers as outside options, rather than nonemployment, are consistent with our main result. However, more nuanced predictions of those models are not borne out in the data. Specifically, even in models with on-the-job search and job ladders (e.g., Postel-Vinay and Robin, 2002; Cahuc et al., 2006; Altonji et al., 2013; Bagger et al., 2014), *recently reemployed* workers – for lack of other

<sup>10</sup>Saez et al. (2017) find that incidence of group-specific payroll tax cuts pass through 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), incl. in Austria (Borovičková and Shimer (2017)). Carneiro et al. (2012) documents cyclical within-firm wage differentiation between new and incumbent workers in the same jobs.

job offers – resort to nonemployment as outside option – and therefore negotiate wages upwards when UI benefits increase, just as in our benchmark model. In the data, we however do not find larger wage effects among recently unemployed workers, or workers transitioning through unemployment during the reform periods.

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 have wages be 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 Mortensen (1998)) 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 the frictionless labor market model with market-clearing wages may rationalize the absence of wage incidence of the leisure subsidy from UI. Finally, compensating differentials may offset wage pressure and help explain wage insensitivity, exactly in jobs prone to unemployment risk.

The first implication of our core fact – insensitivity of wages to UI-induced shifts in the nonemployment value – is to help adjudicate between models: our evidence favors models of wage setting that is insulated from the nonemployment value, such as the ones described above. The nonemployment scenario does not appear to act as the relevant outside option in wage bargaining in any pocket of the Austrian labor market.

Second, if wages are insensitive to the nonemployment value more generally, the empirical regularity of positive comovement between wages and labor market conditions, such as the aggregate Phillips curve and the cross-sectional wage curve, may arise from mechanisms other than nonemployment option affect bargaining, perhaps from employer competition or selection.

Third, the empirical insensitivity of wages to the nonemployment value, for which we provide identified, microeconomic evidence, is good news for some macroeconomic debates: the *theoretical* insensitivity of wages to the nonemployment value has been discussed 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.

**Outline.** In Section 2, we derive the wage-benefit sensitivity in calibrated bargaining models. We describe institutional details, the reforms, and the data in Section 3. Section 4 presents our empirical design and main results. Section 5 adds a series of extensions such as subsample analyses and tests for group-level bargaining. In Section 6 we interpret and discuss implications of our findings. We conclude in Section 7.

## 2 Conceptual Framework and Empirical Strategy

In this Section, we draw on wage bargaining as a conceptual framework to understand the effect of outside options on wages, and then proceed to calibrate the comparative static to guide our empirical analysis. We first derive the Nash bargaining problem and the resulting wage, where the outside option canonically may involve a brief spell of nonemployment before reemployment. We then derive the effect of UI benefit changes in workers' outside option in bargaining. We calibrate the expression by interpreting existing micro estimates of firm-level rent sharing through the lens of bargaining models, which imply low bargaining power and thus high wage sensitivity to outside options.

Our calibrated bargaining model predicts a wage-benefit sensitivity of 0.39: whenever UI benefits – or any component of the payoff during nonemployment – 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.

While we start with a simple model that seemingly holds various parameters fixed and reduces the instantaneous payoff while nonemployed to UI only, we then also present a fuller model to derive the identical wage-benefit sensitivity, despite rich micro choice variables (e.g. job search effort), equilibrium market adjustment, and various components of that nonemployment payoff. Finally we also show that the prediction of high wage sensitivity to nonemployment shifts is robust in a variety of model refinements and alternatives and in different institutional environments.

A natural question is which kinds of bargaining models break the link between nonemployment values and wages. To interpret our empirical findings, we review and relate our evidence to alternative wage setting models that deviate from Nash and the nonemployment outside option specification in Section 6.

### 2.1 Basic Model: Wage Bargaining, the Nonemployment Value and UI Benefits

Most jobs carry strictly positive joint match surplus, for example due to relationship-specific investments or search, hiring, or firing costs. In this situation, 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. The difference between these two reservation wages is the joint surplus. Bargaining models provide an allocation rule and thereby make clear predictions for wage setting, in form of a wage bargain  $w$  within the bargaining set, the interval of the worker's and firm's reservation wages.

**Nash wage.** The Nash wage, derived in detail in Section 2.2, allocates the worker her outside option  $\Omega$ , plus a share of the surplus  $p - \Omega$  equal to her bargaining power  $\phi$ . Stated alternatively, the Nash wage is the weighted average of the inside value of a job, here productivity  $p$ , and the outside option of the worker  $\Omega$ , weighted by worker bargaining power  $\phi$ :

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

$$= \phi \cdot p + (1 - \phi) \cdot \Omega \tag{2}$$

Therefore, the wage bargain exhibits a  $\phi$ -sensitivity to shifts in the inside option  $p$ , and a  $1 - \phi$ -sensitivity to shifts in the worker outside option  $\Omega$ :

$$\frac{dw}{dp} = \phi \tag{3}$$

$$\frac{dw}{d\Omega} = 1 - \phi \tag{4}$$

If workers wield full bargaining power, their wages are insulated from shifts in 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 increase or fall by \$1 whenever the outside option shifts by \$1. A correctly specified outside option and suitable variation would therefore in principle identify one minus worker bargaining power.

In this paper, we empirically test for this prediction by exploiting quasi-experimental, micro-level shifts in workers' outside options and measure the wage effects of these outside option shifts. Specifically, our variation in the outside option is brought about by variation in the workers' payoff while nonemployed in form of shifts in unemployment insurance benefit levels. The sensitivity of outside options to UI therefore depends on the worker's time in nonemployment before finding a new job. This framework 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 (e.g., Pissarides, 2000; Shimer, 2005; Chodorow-Reich and Karabarbounis, 2016; Ljungqvist and Sargent, 2017). We discuss alternative wage setting models, for instance with different outside options, in Section 6.

**The outside option and the role of the value of nonemployment.** If the worker were to take the outside option, she would incur a brief spell of nonemployment 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 the job finding rate  $f$ . The flow value of nonemployment is its value  $N$  times discount rate  $\rho$ :

$$\Omega \equiv \rho N = b + f \cdot (E(w') - N) = \rho \frac{b + f \cdot E(w')}{\rho + f} \tag{5}$$



When finding reemployment at wage  $w'$ , the worker obtains value  $E(w')$ , which incorporates the movement back into nonemployment due to the separation rate  $\delta$ :  $\rho E(w') = w' + \delta(N - E(w')) \Leftrightarrow E(w') = \frac{w' + \delta N}{\rho + \delta}$ . A tight labor market can eliminate search frictions from the perspective of the household by having  $f$  go to infinity and thus not spending any time in nonemployment but directly moving into reemployment of value  $E(w')$ , since  $b$  naturally requires time nonemployed since  $\lim_{f \rightarrow \infty} N = E(w')$ . A worker that faces poor re-employment prospects will have a low job finding rate, spending more time in nonemployment. The nonemployment value can accordingly be solved for as the average of reemployment of the instantaneous payoffs  $w'$  and  $b$ , weighted by the post-separation time in nonemployment  $\tau$ :

$$\rho N = \underbrace{\frac{\rho + \delta}{\rho + f + \delta}}_{\equiv \tau} b + \underbrace{\frac{f}{\rho + f + \delta}}_{\equiv 1 - \tau} w' \quad (6)$$

Post-Separation Time  
in Nonemployment
Post-Separation Time  
in Re-employment

**Time spent in nonemployment  $\tau$ .** The flow value of nonemployment is the amortized expected value of the instantaneous payoffs from nonemployment  $b$  and reemployment  $w$ . These two values 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  $\rho$ . A worker with a high discount rate  $\rho \rightarrow \infty$  (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 and thus low job duration  $\delta \rightarrow \infty$  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  $f \rightarrow \infty$  will have  $\rho N = w'$ .

A convenient approximation exploits the fact that the discount rate  $\rho$  is very small compared to empirical worker flow rates  $f$  and  $\delta$ , suggesting a convenient approximation of  $\rho = 0$ , and thus  $\tau \approx \frac{\delta}{\delta + f}$ .<sup>11</sup> This ratio corresponds to the steady-state expression for *aggregate* unemployment rate with long-term jobs and unemployment spells. Here it refers to the (discounted) time the *individual* worker will spend in the nonemployment state; the transition rates are worker-specific and refer those *conditional on a separation*. Still, on average the unemployment rate is a good approximation, such that  $\tau \approx 7\%$  will be our benchmark calibration for Austria.  $\tau$  refers to outcomes *conditional on a separation*, which usually is associated with increased unemployment risk in future jobs.

**The sensitivity of wages to UI benefits holding fixed all non-wage terms.** With  $\Omega = \rho N$  and plugging in the expression for  $\rho N$  derived in (6), the Nash wage becomes a function

<sup>11</sup>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  $\rho$ , implying that our calibrated model *understates* the predicted benefit-wage sensitivity if calibrated  $\tau$  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.

of the discounted time in nonemployment  $\tau$  vs. in re-employment  $1 - \tau$  (each a function of transition rates  $f$  and  $\delta$ , and discount rate  $\rho$ ), instantaneous payoffs  $b$  during nonemployment, and  $w'$  during re-employment, and worker bargaining power  $\phi$ :

$$w = \phi \cdot p + (1 - \phi) \cdot \overbrace{(\tau b + (1 - \tau)w')}^{\Omega} \quad (7)$$

The *total* wage sensitivity to UI benefit  $b$  works solely through affecting worker outside option  $\Omega$  and is therefore mediated by  $1 - \phi$ :

$$\frac{dw}{db} = (1 - \phi) \cdot \overbrace{\left( \tau + (1 - \tau) \frac{dw'}{db} \right)}^{d\Omega/db} \quad (8)$$

**Decomposing the wage response.**  $b$  affects the wage through two channels. First, the *mechanical* effect of  $b$  increases the instantaneous payoff from nonemployment and is therefore weighted by  $\tau$ , the time in nonemployment after taking up the outside option and separating. On its own, this effect is then relatively small, although linear in  $\tau$  and perhaps quite large for actual separators due to repeated unemployment spells.

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  $1 - \tau$  of the 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 according to the outside option channel is therefore:

$$\frac{dw}{db} = (1 - \phi) \cdot \frac{\tau}{1 - (1 - \phi)(1 - \tau)} = (1 - \phi) \cdot \frac{1}{1 + \phi(\tau^{-1} - 1)} \quad (9)$$

Vice versa, Equation (9) implies a worker bargaining power  $\phi$  for any wage-benefit sensitivity:

$$\phi = \frac{1 - \frac{dw}{db}}{1 + \frac{dw}{db} \cdot (\tau^{-1} - 1)} \quad (10)$$

Below we calibrate  $\phi$  and  $\tau$  to predict a wage-benefit sensitivity; the rest of the paper is concerned with estimating that sensitivity in the data.

**Intuitions from three contour maps.** We provide intuitions for the relationship between  $\tau$ ,  $\phi$ , and  $\frac{dw}{db}$  in three contour maps. Figure 2 traces out iso- $\tau$  curves relating a given worker bargaining power  $\phi$  with a predicted wage-benefit sensitivity, for various levels of  $\tau$ . The higher job finding rate  $f$  and thus the lower  $\tau$ , the lower a weight the outside option puts on the UI

benefit  $b$ . As a consequence, wages are insulated from *changes* in  $b$  too, reducing  $\frac{dw}{db}$  to zero for  $\tau = 0$ . By contrast, for  $\tau = 1$  – e.g., because  $f = 0$  or  $\delta \rightarrow \infty$  or because  $\rho \rightarrow \infty$  –, the outside option becomes  $\Omega = b$  and the wage sensitivity then is  $\frac{dw}{db} = 1 - \phi$ . Therefore, for a given  $\phi$ ,  $\frac{dw}{db} \in [0, 1 - \phi]$ . Conversely, for a given  $\tau$ ,  $\frac{dw}{db} \in [0, 1]$  depending on  $\phi$ : if  $\phi = 1$ , the wage is always insulated from the outside option and thus  $\frac{dw}{db} = 0$  independently of  $\tau$ ; if  $\phi = 0$ , the wage is always equal to the outside option, and thus  $\frac{dw}{db} = 1$  for any  $\tau > 0$ .

Figure 3 plots the wage-benefit sensitivity as a function of post-separation time in nonemployment  $\tau$ , for various levels of worker bargaining power  $\phi$ . The higher  $\tau$ , 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  $\tau$  on the wage sensitivity.

Figure 4 shows the role of discount rate  $\rho$  in shaping the sensitivity: we set  $\rho = 0$  in the previous discussion, interpreting  $\tau$  merely as the time the worker spends in nonemployment, akin to a lifetime, post-separation unemployment rate at the individual level. The Figure traces out the sensitivity of wages to benefits as a function of  $\rho$  (for various  $f$  and  $\delta$  combinations that yield different unemployment rates): the *lowest* wage sensitivity arises with  $\rho = 0$ , whereas discounting puts more and more weight on the initial nonemployment state while discounting reemployment opportunities. An interesting interpretation here is that liquidity constraints or myopia would strengthen the effect of the nonemployment value in bargaining (from the perspective of the worker’s outside option).

**Calibrating  $\phi$ .** We plot potential calibration targets for worker bargaining power  $\phi$  in Figure 1. First, macroeconomic calibrations often treat  $\phi$  as a free parameter and provide little empirical guidance, being set to fulfill the Hosios condition of constrained efficiency in matching models.<sup>12</sup>

Second, we argue that micro-empirical rent-sharing estimates do provide direct calibration targets for  $\phi$ , since Nash bargaining also prescribes a tight link between *inside* value (proxies in the data: profits and productivity) and wages, guided by  $\phi$  (rather than  $(1 - \phi)$  for outside options, our focus). 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). 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 bound* for  $\phi$ . Under the assumption of Nash bargaining by which  $\frac{dw}{d\Omega} = 1 - \frac{dw}{dp}$ , these small rent

<sup>12</sup>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  $\gamma$ . [...] The usual procedure is to assume that [bargaining power parameter]  $\gamma$  is equal to the elasticity of the matching function with respect to the unemployment rate.”

sharing estimates therefore directly imply large sensitivity of wages to outside option shifts. 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, have an average of 0.104, constituting an upper bound for  $\phi$ . For our empirical predictions, we set  $\phi = 0.1$ , i.e. the upper end of what the empirical rent-sharing estimates imply.

**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,” the predicted wage-benefit sensitivity is:

$$\left. \frac{dw}{db} \right|_{(\tau=0.07, \phi=0.1)} = (1 - 0.1) \cdot \frac{1}{1 + 0.1(0.07^{-1} - 1)} \approx 0.39 \quad (11)$$

That is, a \$1.00 increase in UI benefits should entail a \$0.39 increase in wages according to the canonical Nash bargaining model calibrated to match empirical rent sharing estimates. 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, relaxing our conservative calibration of  $\tau = 0.07$  (time nonemployed *conditional* on a separation) to a higher  $\tau$  will substantially raise these estimates.

**Empirical strategy and preview of results.** Our empirical strategy, described in Section 4, exploits quasi-experimental and directly quantifiable variation in  $b$ . In practice, our estimation comes in form of a straightforward treatment effect estimator of the wage effect of between-worker, reform-induced variation in UI benefit levels. Our particular setting will be the Austrian UI system, which approximates the ideal empirical setting due to the eligibility to receive unemployment benefits even if the worker unilaterally quit her job, the absence of experience rating, high take-up rates, post-UI-exhaustion being explicitly indexed to previous UIB levels, along with rich quasi-experimental variation in benefit levels brought about by UI reforms.

Our point estimates of the empirical wage-benefit sensitivity  $\frac{dw}{db}$  are close to zero. For this sensitivity to be consistent with *any* value  $\tau$ , the estimated insensitivity would require (or structurally interpreted, directly imply) a worker bargaining power  $\phi$  of unity. In Figure 1, we therefore preview our key result by plotting our implied worker bargaining power parameter of essentially 1.0, as we find wages to be insensitive to variations in the nonemployment flow value  $b$ . Our point estimates exceed even the highest calibration targets assumed in the macro literature. Our preferred interpretation is that we reject Nash bargaining with nonemployment as a relevant

feature in the outside option, instead suggesting that real-world wage setting follows protocols that largely insulate wages from the nonemployment value, as we discuss in Section 6.

**Heterogeneity by post-separation time in nonemployment  $\tau$ .** The model clearly predicts heterogeneity by the expected time in nonemployment,  $\tau$ : for somewhat marginally attached workers with  $\tau = 0.1$ , the wage sensitivity to benefits is 0.48 (for  $\phi = 0.1$ ).  $\tau = 0.2$ , such as may be expected from unemployment spells triggered increased unemployment risk in future jobs, pushes the sensitivity to 0.65. Even workers that are very insulated from unemployment such as those with 1% discounted remaining labor market time after a separation will exhibit more than a 0.08 pass-through of benefits into wages. In Section 5 we directly exploit heterogeneity by various proxies for workers’ idiosyncratic unemployment risk and investigate whether these cross-sectional predictions are borne out in the data, finding no evidence for the predicted heterogeneity that is inherent in the  $\tau$ -dependent transmission from  $b$  to  $N$ .

**Heterogeneity by worker bargaining power  $\phi$ .** In Section 5, we additionally explore heterogeneity in the wage-benefit sensitivity by tentative proxies for worker bargaining power  $\phi$ , such as gender, blue/white collar, or income. For a given  $\tau$ , the model will predict accordingly larger wage-benefit sensitivities for lower is working bargaining power  $\phi$ ; at the extreme, for  $\phi = 0$ , the sensitivity is one independently of  $\tau$ ; for  $\phi = 1$ , the sensitivity is zero for any  $\tau$ . We do not find that groups that plausibly wield lower worker bargaining power exhibit larger effects.

## 2.2 Full Model: Equilibrium Adjustment and Micro Reoptimization

Wage-benefit sensitivity (9) explicitly holds fixed all elements of the values except for wages and UI benefits. Next, we present a full model to show that the structural benefit-wage sensitivity is robust to a richer notion of the components of the payoff during nonemployment, to rich micro reoptimization of choice variables such as endogenous search effort, as well as to equilibrium market adjustment. These terms thereby permit responses in  $f$  and  $\delta$ , but also any other micro choices or market-level variables. 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 received the change in benefits itself) will net out market level responses such as those in  $f$ ,  $\delta$  or productivity  $p$ , and any other market-wide responses not explicitly showing up in the simple model. In the subsequent Section, we then discuss a set of model extensions and alternatives and show quantitative robustness of our results.

### 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  $E$ :

$$\rho N(b, \mathbf{c}, \mathbf{x}) = \max_{\mathbf{c}} \left\{ z(b, \mathbf{c}, \mathbf{x}) + f(\mathbf{c}, \mathbf{x}) \cdot [E(w', b, \mathbf{c}, \mathbf{x}) - N(b, \mathbf{c}, \mathbf{x})] \right\} \quad (12)$$

$\mathbf{c}$  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.

**The instantaneous payoff from nonemployment  $z$  vs. UI benefit  $b$ .** 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  $h_i$ , normalized by budget multiplier  $\lambda_i$ ), potential search effort costs  $c(e)$ , stigma from nonemployment  $\gamma$ , and other nonemployment-contingent income sources  $y$ :

$$z_i = b_i + \frac{v_i(h = 0) - v_i(h > 0) - c_i(e_i) - \gamma_i}{\lambda_i} + y_i + \dots \quad (13)$$

**Unemployment benefits vs. other  $z$  components.** 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 variety of additional components in  $z$  besides  $b$ .<sup>13</sup> Here we continue to assume full take up, such that  $\frac{\partial z}{\partial b} = 1$ , the benchmark for this section; we will evaluate robustness to the full-take-up assumption in the subsequent Section.

**Inside option: employment at wage  $w$  and production.** When employed at wage  $w$ , the household's flow value is the wage rate plus the capital gain, which is moving into nonemployment at rate  $\delta$ :

$$\rho E(w, b, \mathbf{c}, \mathbf{x}) = \max_{\mathbf{c}} \{ w + \delta(\mathbf{c}, \mathbf{x}) \cdot [N(b, \mathbf{c}, \mathbf{x}) - E(w, b, \mathbf{c}, \mathbf{x})] \} \quad (14)$$

<sup>13</sup>While such additional components of  $z$  may be 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 cycle.

The employing firm receives value from the output of the worker (productivity  $p$ ) and pays wage  $w$ , where the match may separate at rate  $\delta$ :

$$\rho J(w) = p - w + \delta \cdot [V - J(w)] \quad (15)$$

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 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:

$$w = \arg \max_{w^N} \left( E(w^N) - N \right)^\phi \left( J(w^N) - V \right)^{1-\phi} \quad (16)$$

$$\Rightarrow (1 - \phi)(E(w) - N) = \phi(J(w) - V) \quad (17)$$

Plugging in worker employment value (14) and firm job value (15):

$$\Leftrightarrow (1 - \phi) \left[ \frac{w + \delta(N - E(w))}{\rho} - N \right] = \phi \left[ \frac{p - w + \delta(V - J(w))}{\rho} - V \right] \quad (18)$$

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:

$$w = \phi p + (1 - \phi)\rho N - \phi\rho V \quad (19)$$

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.

### 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 (12) with respect to  $b$ ,

defining  $N$ , into four effects:

$$\begin{aligned}
 \frac{\cdot dN}{db} = & \overbrace{\underbrace{\frac{\partial N}{\partial b}}_{\text{Mechanical Effect}} + \underbrace{\frac{\partial N}{\partial w'} \frac{dw'}{db}}_{\text{Feedback of Wage Response}}}}^{\text{Calibration}} + \underbrace{\frac{\nabla_{\mathbf{x}} N \cdot \nabla_b \mathbf{x}}}_{\text{Market Adjustment}} + \underbrace{\frac{\nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_b \mathbf{c}^*}_{\text{Micro Re-optimization}}}}_{\text{Net Out with Control Group}} \\
 & \text{= } \tau \quad \text{= } 1 - \tau \quad \text{=0 By Envelope Theorem}
 \end{aligned} \tag{20}$$

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: the first partial effect captures the mechanical change in the instantaneous payoff while nonemployment, holding constant the labor market path i.e. post-separation time in nonemployment  $\tau$ . 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,  $1 - \tau$ .

The third, new term captures shifts in the economic environment  $\mathbf{x}$  that the individual agent takes as given. We will present a difference-in-difference strategy that allows us to net out market-level adjustment with the use of the control group. These factors include labor demand that may move around job finding rate  $f$ , or shifts labor market tightness due to some workers entering the labor force or changing job search effort. It also include shifts in wage levels common to all workers, or shifts in social insurance programs including congestion.

The fourth, new term captures the potential reoptimization of the agent's choice variable following the shift in incentives brought about by the shift in benefits. Choices include job search effort, reservation wage strategies, intensive-margin labor supply effort choices, and switching between program substitutes and even, for marginal participants, shifts in take-up of UI. We apply the envelope theorem to clarify that those micro behavioral responses will be ignored by the Nash bargain.

As a result, we will isolate the *micro effects* of the benefit level, resulting from simple and transparent mechanical effects of the benefit change on the flow payoffs during the nonemployment and employment states. These partial effects simply require us to construct a weighted average of the time the worker will expect to spend in nonemployment as well as in employment after taking up the outside option, resulting in a difference-in-differences version of the basic wage sensitivity expression (9).

**Envelope theorem and the irrelevance of micro reoptimization.** The value of the outside option takes into the account that in subgame perfect equilibrium, the agent will maximize  $N$  once (if) entering that state, such that for any original level of  $b$ , the following



vector of first-order conditions holds for a nonemployed worker:

$$\nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) = \mathbf{0} \quad (21)$$

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:

$$\Rightarrow \nabla_{\mathbf{c}} N(b, \mathbf{c}^*, \mathbf{x}) \cdot \nabla_b \mathbf{c}^* = \mathbf{0} \quad (22)$$

This application of the envelope theorem implies that from value perspective, 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.

The appeal to the envelope theorem appears to mirror the assumption that behavior remains unchanged, as would, e.g., emerge if incentives do not actually change or if adjustment costs would curb any such adjustment. The result is stronger: even if adjustment occurs, the adjustment does not incur first-order changes in the value due to the point of departure being at the optimum, at which arbitrary shifts in behavior do not entail first-order value effects. Moreover, when bargaining the worker must simply expect to be maximizing any subjective value  $N$  that forms the outside option, e.g., even if the worker eventually were to search myopically.

**Netting out market-level effects with a control group.** Remaining responses arise from adjustment in the variables  $\mathbf{x}$  that the worker takes as given, such as market-level adjustment or productivity effects from shifts in quantities. We net out such effects with a suitable control group that is in the same market and ideally a production substitute, and thus exclusively exposed to the treatment through market level spillover effects. The effects we net out include shifts in market-level wages that are common to all workers rather than those whose outside option has been specifically shifted by the idiosyncratic exposure to  $db$  (treatment group).

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  $db^C = 0$ :

$$N^i(b^{g(i)}, \mathbf{c}^i, \underbrace{\boldsymbol{\nu}^i, \boldsymbol{\mu}^{m(g(i))}}_{\mathbf{x}^i}) \quad (23)$$

The two types are yet 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.  $\boldsymbol{\nu}^i$  are worker- or type-specific factors (and thus differ

between  $T$  and  $C$ ) that the worker takes as parametric but that may yet be affected by the exposure to the treatment. In fact,  $\frac{dw^i}{db^g}$  is one particular element of  $\iota^i$ , which we have pulled out.

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):

$$\frac{dN^T}{db^T} = \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \frac{dw'^T}{db^T} + \nabla_{\iota} N \cdot \nabla_b \boldsymbol{\iota}^T + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^{m(T)} \quad (24)$$

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

$$\frac{dN^C}{db^T} = \frac{\partial N}{\partial w'} \frac{dw'^C}{db^T} + \nabla_{\boldsymbol{\mu}} N \cdot \nabla_b \boldsymbol{\mu}^{m(C)} \quad (25)$$

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):

$$\frac{dN^T}{db^T} - \frac{dN^C}{db^T} = \frac{\partial N}{\partial b} + \frac{\partial N}{\partial w'} \cdot \left[ \frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] + \nabla_{\iota} N \cdot \nabla_b \boldsymbol{\iota}^T \quad (26)$$

**Potential confound: group-specific effects beyond wages.** The first remaining confound comprises micro, or group-specific effects that households take as parametric, i.e. those that are not arising from optimizing behavior of the individual agents. Bias in our empirical strategy towards zero would arise from elements of  $\boldsymbol{\iota}$  that adjust in a way that would attenuate the effect of  $b$  on  $N$ . Suppose for example that  $z = b + x(b)$  with  $x'(b) < 0$ , as would arise from crowd-out of transfers that are crowded out by UI benefit increases (e.g., means-tested UI program substitutes), or because employers may observe treatment status and differentiate hiring or statistical discrimination by treatment status. Or, workers' higher benefit level may increase credit worthiness of workers specifically treated by the reform and thereby help smooth consumption. Or, perhaps UI generosity may affect the social stigma of being unemployed. Our design is unable to evaluate or rule out the sign or size of these effects, and thus must ignore them going forward.

**Potential confound: market spillovers.** The second caveat will be the correct specification of the control group as being in the same labor market as the treatment group, such that market spillovers do occur. 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 rather than distant control groups may exhibit wage differentials reflecting potential spillovers. Second, in our regression framework, we will add rich year- and group-specific fixed effects such as, in

our most granular specification, firm-by-year fixed effects, essentially conducting *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 Section 5.1.3 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 the subsequent Section 2.3, 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.

**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 mechanical shifts in the instantaneous payoffs triggered by the benefit shifts:

$$\begin{aligned} &= \frac{d\Omega^T}{db^T} - \frac{d\Omega^C}{db^T} \\ &\underbrace{\frac{d(\rho N^T)}{db^T} - \frac{d(\rho N^C)}{db^T}} = \frac{\partial(\rho N)}{\partial b} + \frac{\partial(\rho N)}{\partial w'} \cdot \left[ \frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] \end{aligned} \quad (27)$$

Using the definition of  $N$  in terms of  $\tau$  from Equation (6), we can express the partial derivatives in terms of  $\tau$  (using  $\partial z^T/\partial b^T - \partial z^C/\partial b^T = 1$ ) and  $(1 - \tau)$  times the wage-response differential:

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

where again  $\tau = \frac{\rho + \delta}{\rho + \delta + f}$ , such that these two partial effects are simply the present value of the average time the worker taking advantage of the outside option (i.e. separating) would spend in either the nonemployment state and the re-employment state.

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

We have now derived the effect of benefit changes on the nonemployment value, where the benefit shift is our instrument shifting the nonemployment value. Next, we clarify how the benefit-induced shift in the outside option is predicted to mechanically pass through into wages in the canonical Nash bargaining framework.

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:

$$\frac{dw^T}{db^T} - \frac{dw^C}{db^T} = (1 - \phi)\rho \left[ \frac{dN^T}{db^T} - \frac{dN^C}{db^T} \right] \quad (29)$$

$$= (1 - \phi) \left( \tau + (1 - \tau) \left[ \frac{dw'^T}{db^T} - \frac{dw'^C}{db^T} \right] \right) \quad (30)$$

Using that  $dw' = dw$  due to Nash bargaining in the next job too for both groups, we again obtain the feedback of wage effects into the reemployment states:

$$= (1 - \phi) \frac{\tau}{1 - (1 - \phi)(1 - \tau)} = (1 - \phi) \cdot \frac{1}{1 + \phi(\tau^{-1} - 1)} \quad (31)$$

The difference-in-difference version mirrors the structural Equation (9) that held fixed non-wage variables, again generating unique mapping between  $\phi$  and  $\frac{dw}{db}$ , containing the *same parameters* that will be calibrated as in the basic model that explicitly held fixed various market-level and micro choice variables.

### 2.3 Robustness of the Wage-Benefit Sensitivity Across Model Variants

Next, we show that the key prediction from the benchmark model carries over to a wide variety of richer models considered in the literature. In Section 6 we additionally discuss alternative models that insulate wages from the nonemployment value, and which may therefore rationalize the zero effect of  $b$  on  $w$  that we document in the empirical Section 4.

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

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

unemployment  $u$ ):<sup>14</sup>

$$w^{\text{DMP}} = \phi p + \overbrace{(1 - \phi)b + \phi k}^{=(1-\phi)\rho N} \quad (32)$$

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

$$dw^{\text{DMP}} = (1 - \phi)db + \phi k d\theta \quad (33)$$

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

$$dw^{\text{DMP}} = (1 - \phi)db + \phi \left[ -dw'^{\text{DMP}} \cdot \frac{1}{\eta} \frac{f(\theta)}{\rho + \delta} \right] \quad (34)$$

$$\Leftrightarrow \frac{dw^{\text{DMP}}}{db} = \frac{1 - \phi}{1 + \phi \frac{1}{\eta} \frac{f(\theta)}{\rho + \delta}} \quad (35)$$

$$\approx \frac{1 - \phi}{1 + \phi \cdot \frac{1}{\eta} \cdot (u^{-1} - 1)} \quad (36)$$

where step 2 uses  $dw = dw'$ , and step 3 uses  $\frac{f}{\rho+\delta} \approx \frac{f}{\delta} \approx \frac{1-u}{u} = u^{-1} - 1$ , where  $u$  denotes the market-level unemployment rate (since  $\rho$  is small compared to worker flow rates). Strikingly, this expression mirrors our structural *micro* sensitivity except for two differences. First, the  $\phi$  factor in the denominator is divided by  $\eta < 1$ , attenuating the sensitivity slightly. Second, the relevant unemployment rate  $u$  refers to the market-level average rather than the worker's idiosyncratic time in nonemployment post-separation  $\tau$ . In both limits, we have  $dw/db|_{\tau=1} = dw^{\text{DMP}}/db|_{u=1} = 1 - \phi$ . For  $\phi = 0.1$  (micro estimates from rent sharing),  $u \approx 7\%$  (consistent with the calibration post-separation) and  $\eta = 0.72$  (e.g., Shimer, 2005), we obtained a calibrated benchmark for the wage-benefit sensitivity of  $\frac{1-0.1}{1+0.1 \frac{1}{0.72} \cdot \frac{0.93}{0.07}} \approx 0.32$ .<sup>15</sup> Moreover, higher unemployment  $u$  increases the macro sensitivity almost exactly as a higher  $\tau$  increases the micro sensitivity, which generalizes the implications of whether the sensitivity differs in the local unemployment rate, a prediction we test in Section 5. Therefore, our quantitative and structural benchmark for the wage-benefit sensitivity carries over to a macro context with equilibrium adjustment and perfectly segmented

<sup>14</sup>In DMP models, the reemployment capital-gains term in the worker's outside option  $\rho N = b + f[E(w^{\text{DMP}}) - N]$  is replaced with the firm's value of a filled job (recognizing the Nash sharing rule such that  $(1 - \phi)f[E(w') - N] = \phi f[J(w') - V]$ ). Free entry has firms post vacancies until the value of vacancies is pushed to zero  $V = 0 \Leftrightarrow \frac{k}{q} = J$ , implying that  $\phi f[J(w') - V] = \phi k f/q = \phi k \theta$ , due to the standard constant-returns matching function, by which  $f(\theta)/q(\theta) = \theta$ , such that  $\phi k \theta$  now captures the worker's capital gain from reemployment  $(1 - \phi)f[E(w') - N]$ .

<sup>15</sup>With  $\eta = 0.5$  instead of 0.72, the sensitivity is 0.25. With  $\tau = 0.05$  instead of 0.07 (plausible because we no longer consider post-separation time in unemployment but the aggregate unemployment rate), we have 0.25.

labor markets for the treatment group and the control group.

**II. Stole and Zwiebel (1996) bargaining with multi-worker firms.** Extensions to multi-worker contexts highlight the complications that the splitting of the *inside* option entails with multi-worker firms and diminishing returns. We build on the derivation of the Nash wage with firm level production function  $Y = n^\alpha$  in Acemoglu and Hawkins (2014) augmented with our worker-specific outside option  $\Omega_i$ :<sup>16</sup>

$$w^{\text{MultiWorker}} = \frac{\alpha\phi}{1 - \phi + \alpha\phi} \cdot x_f \cdot n_f^{\alpha-1} + (1 - \phi)\Omega_i \quad (37)$$

That is, multi-worker firm bargaining preserves the sensitivity of wages to outside options  $\Omega$ .<sup>17</sup>

**III. Representative vs. individual households.** Implementations of matching-frictional labor markets are largely either in terms of individual households with linear utility or with large households that send off households into employment with full insurance in the spirit of indivisible labor (Rogerson, 1988; Hansen, 1985), for example Merz (1995), Andolfatto (1996), or Shimer (2010). In Appendix Section C.1 we extend this setting to an individual household with nonlinear utility. Our individual household bridges these setups with the assumption of perfect capital markets (and negligibly long unemployment spells).

**IV. Endogenous separations.** The Nash wage is the same in models with endogenous separations among existing jobs due to idiosyncratic productivity shocks, where  $p$  is replaced with  $p_{it}$ . Inframarginal surviving matches, i.e. those that we track in the data, exhibit the same pass-through of  $\Omega_i$  into wages.<sup>18</sup>

**V. On-the-job search.** On its own, on-the-job search with a job ladder (e.g., due to heterogeneous firms or match-specific quality) need not change the wage bargaining process as long as the worker is required to give notice to the firm before engaging in bargaining with the next employer. Nonemployment then remains the outside option in wage bargaining. This tractable route is taken by for example Mortensen and Nagypal (2007) and Fujita and Ramey (2012).

<sup>16</sup>Cahuc et al. (2008) also derive a dynamic search model with Stole and Zwiebel (1996) bargaining and heterogeneous worker groups  $i$  that may differ in their outside options  $b_i$  and derive the wage for group  $i$  as  $w_i(n) = (1 - \alpha)\rho N_i + \int_0^1 a^{\frac{1-\alpha}{\alpha}} F_i(na) da$ .

<sup>17</sup>These models also imply that rent sharing estimates from firm-specific TFP shifts  $x_f$  transferred to predict wage sensitivity to  $b$  would require an additional scaling up if  $\alpha < 1$ .

<sup>18</sup>In these models,  $b_i$  will also shift the reservation quality at which matches are formed and destroyed. Jäger et al. (2018) study a large extension of potential duration of UI for older workers and document substantial separation responses of that policy, which perhaps served as a bridge into early retirement in particular for workers in declining industries. In this paper, we do not detect significant separation effects to increases in benefit *leves*, perhaps because we study younger workers.

We discuss alternative models with competing job offers as outside options in Section 6.1. In this class of models however, new hires from nonemployment still use nonemployment as their outside option in their initial bargain, where wages thus follow our baseline model.

**VI. Finite potential benefit duration.** While a common approach is to model benefits as having infinite potential duration, its duration is finite in Austria, as we describe in Section 3. Yet, in the Austrian setting, infinite benefit duration is a particularly good approximation for initially incumbent workers because only around 20% of unemployment spells end up in benefit exhaustion (Card et al., 2007). Moreover, after UI exhaustion, eligible Austrian workers collect a follow-up UI substitute  $s(b) < b$  (*Notstandshilfe*, i.e. unemployment assistance (UA)). Importantly,  $s(b)$  is explicitly indexed to a worker’s pre-exhaustion UIB levels and – while in many cases lower – its level shifts almost one to one with changes in  $b$ . This feature leaves post-UI benefits sensitive to our reforms even for UI exhausters.<sup>19</sup>

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

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

$$\frac{dw^{\text{finite}}}{db} = \frac{(1 - \phi)\tau}{\frac{1}{1 - \zeta(1 - \alpha \frac{ds}{db})} - (1 - \phi)(1 - \tau)} \quad (38)$$

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

<sup>19</sup>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  $UAB_i = \min\{0.92b_i, \max\{0, 0.95b_i - \text{Spousal Earnings}_i + \text{Dependent Allowances}_i\}\}$ . 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.

the household's previous, actually received UI benefit level, and thus move in lock-step with benefit *changes*.<sup>20</sup>

As a result, the term  $[1 - \zeta (1 - \alpha \frac{ds}{db})] = 0.91$  provides negligible attenuation of the wage-benefit sensitivity: the wage benefit-sensitivity remains at 0.24. This is an underestimate if the workers exhausting UI have a lower job finding rate and thus a larger  $\tau$ , which for that subset of workers would greatly amplify the sensitivity: setting  $\tau = 0.12$  rather than 0.07 will restore  $dw/db = 0.36$  for those workers.

In other words, since an initially employed Austrian worker has a low probability of benefit exhaustion and, moreover, post-UI benefits are indexed to UI benefits our design is robust to finite benefit duration. Perhaps this fact also explains why we also do not find wage effects from potential benefit duration extensions in Section 5.1.3. We have also not found evidence that workers with particularly high potential benefit durations exhibit different wage sensitivity to the unemployment benefit level.

**VII. Limited take-up and UI wait periods for unilateral quitters.** Austria has broad UI eligibility that encompasses even quitters. There is however a 28-day wait period, after which UI recipients enjoy full potential benefit duration (i.e. for 28 more calendar days than their peers receiving UI immediately). We evaluate this consideration in two steps. First, we define a probability  $1 - v$  that a bargaining progress breakdown leaves the worker eligible for UI whereas at probability  $v$  leaves the worker ineligible (for any social insurance program). Ineligible workers wait 28 days until they receive UI, implying that  $z^{\text{ineligible}} = z^{\text{eligible}} - b$  for initial period of nonemployment. In discrete time,  $N^{\text{in}} = z^{\text{in}} + f^m \beta E^{\text{in}} + (1 - f^m) \beta N^{\text{el}}$ , such that:

$$\mathbb{E}[N] = (1 - v)N^{\text{el}} + vN^{\text{in}} \tag{39}$$

$$= (1 - v)N^{\text{el}} + v [z^{\text{in}} + (1 - f^m) \beta N^{\text{el}} + f^m \beta E^{\text{in}}] \tag{40}$$

$$= N^{\text{el}} (1 - v [1 - (1 - f^m) \beta]) + v [z^{\text{in}} + f^m \beta E^{\text{in}}] \tag{41}$$

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

$$\frac{d\mathbb{E}[N]}{db} \geq \frac{dN^{\text{el}}}{db} \underbrace{[1 - v + v(1 - f^m) \beta]}_{\approx 1 - v f^m} \tag{42}$$

<sup>20</sup>The law stipulates that post-UI benefits move with a slope of 0.92 along with previous UI benefits. There are additional additive components, e.g., benefits for dependents and reductions for other income, and the post-UI benefit level is capped at 0.95 times previous UI benefits. For the calibration, we pick the middle point between 0.95 and 0.92 and assume  $ds/db \approx 0.935$ .



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

$$\frac{dw^{\text{Limited Elig.}}}{db} \geq (1 - \phi) \cdot \frac{1 - \nu f_m}{1 + \phi(\tau^{-1} - 1)} \quad (43)$$

Calibrating the bracketed attenuation factor with  $f = 0.12$  (incorporating a monthly  $\beta = 0.9965$  will not change the result) implies that the attenuation is by 0.88 *even if all separations were to go into nonemployment with initial ineligibility* (i.e.  $\nu = 1$ ). That is, since so many nonemployment spells go beyond one month, this institutional feature has limited effects on the predicted wage-benefit sensitivity.<sup>21</sup> This benchmark thereby also evaluates also delayed take-up for any reason even among the immediately eligible. In reality, most separations into nonemployment in Austria entail UI eligibility such that  $\nu$  is closer to zero than to one, greatly limiting attenuation.

**VIII. Wage stickiness rather than period-by-period bargaining.** Real-world wage renegotiations may occur infrequently on the job, e.g. arrive at rate  $\gamma$ . Then, the measured wage response to a (permanent) shift in  $db$  is increasing in time-since-reform  $dt$ , and on average:

$$\mathbb{E} \left[ \frac{dw^{\text{sticky,dt}}}{db} \right] = (1 - e^{-\gamma dt}) \cdot \frac{1 - \phi}{1 + \phi(\tau^{-1} - 1)} + e^{-\gamma dt} \cdot 0 \quad (44)$$

Empirically, we approach this aspect from three angles. First, we start with observing average wage earnings in the first full calendar year after the reform takes effect.<sup>22</sup> We then additionally investigate earnings in the calendar year in the subsequent year, allowing two years for wage pass-through, whereas existing evidence on wage stickiness suggests half of wages to get reset within one year.<sup>23</sup> Second, we consider wage effects in *new jobs*, for workers switching jobs with or without unemployment spells in between, where we follow the standard assumption that new jobs get to set wages initially in a flexible way. Third, we sort jobs (firms) by the usual degree of wage volatility, essentially by an empirical proxy for  $\gamma$ , and investigate heterogenous wage effects.

**IX. Taxation.** Our bargaining setup so far sidesteps the tax system. In Austria, benefits are not taxed, whereas wages and profits are. If the employer’s and the worker’s income taxes are approximately taxed by the same  $\tau$ , then changes in *net* benefits  $b$  enter the worker’s outside option relatively as  $\frac{b}{1-\tau}$ . For  $\tau \approx 0.3$ , accounting for the tax system would therefore *amplify* the predicted sensitivity of wages to  $b$  by  $\frac{1}{1-0.3} \approx 1.41$  for any given  $\phi$ . (Analogously, a given wage response will, structurally interpreted in a model of Nash bargaining with nonemployment as the

<sup>21</sup>This attenuation is further slightly reduced with finite PBD because the one-month delay does not reduce subsequent PBD, such that at probability  $(1 - f^m)^{\text{PBD Months}}$ , the worker “buys back” the first month (valued as  $b - \alpha s$ , i.e. the premium over UI substitute  $s$  adjusted for eligibility probability  $\alpha$ ).

<sup>22</sup>An exception is the 1989 reform, which takes effect mid-year.

<sup>23</sup>See, e.g., Barattieri et al. (2014) for the United States, and Sigurdsson and Sigurdardottir (2016) for Iceland. Finally, the evidence on inside-option rent sharing documents same-year wage effects for incumbent workers.

outside option, imply 1.41 as large a worker bargaining power parameter.) To our knowledge, the empirical relevance of tax wedges in wage bargaining is an open question; we therefore side-step this channel when quantitatively evaluating our effects, noting that taking taxation into account will likely increase the predicted wage effects and thus amplify any given shortfall of empirical compared to theoretical wage sensitivities.

### 3 Institutional Context and Data

Here we review the institutional features of unemployment insurance in Austria, the four reforms we study, the data we use, and relevant wage setting institutions.

#### 3.1 Unemployment Insurance in Austria

This section provides an overview of relevant features of the unemployment insurance (UI) system, including the rules on benefit duration, levels, and eligibility.

**Potential benefit duration.** The PBD determines the maximum number of weeks someone can receive UI benefits and the benefit schedule determines the size of payments as a function of pre-unemployment earnings.<sup>24</sup> 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, UAB levels are explicitly indexed to a worker’s pre-exhaustion UIB levels almost one to one, leaving post-UI benefits sensitive to UIB reforms even for UI exhausters.<sup>25</sup>

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

**UI benefit schedules.** The Austrian UI system assigns benefit levels to granular income bins, which we refer to as reference wages. UI benefit payments in Austria are not means-tested but

<sup>24</sup>Before 1989, the PBD was generally only experience- and not age-dependent. Individuals with less than 12 weeks of UI contributions in the last two years were eligible for 12 weeks, individuals with 52 weeks in the last two years were eligible for 20 weeks, and individuals with 156 weeks (3 years) in the last five years were eligible for 30 weeks. In 1989, these eligibility rules were changed so that individuals age 40-49 in the past 10 years were eligible for 39 weeks and individuals above 50 eligible for 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).

<sup>25</sup>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  $UAB_i = \min\{0.92b_i, \max\{0, 0.95b_i - \text{Spousal Earnings}_i + \text{Dependent Allowances}_i\}\}$ . 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.

benefit recipients are required to search for employment relevant to their qualifications. We provide an overview of UIB schedule changes from 1976 to 2001 in Appendix Figures A.15. We discuss the calculation of the income base in Section E.2 and additionally verify that our measured income concept from the administrative data accurately predicts benefit level receipts. At the beginning of the time period we consider, the replacement rate was 41% for individuals above a minimum benefit level and below a maximum benefit level. Our analysis builds on a series of reforms to replacement rates and the 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 is based on gross income. UIBs are not taxed.

During the time period we consider, the Austrian UI benefit schedule was reformed substantially. We describe the reforms in detail below in Section 3.2.

**UIB reference wages over time.** Through 1987, the reference wage relevant for calculation of UIBs was the last full month’s wage. Between 1988 and 1995, the reference wage as the moving average of the six previous full months of employment. After 1996, the reference wages was last year’s earnings for unemployment spells beginning after June 30 of a given calendar year, and the earnings in the second to last year for spells beginning before June 30. In Section 4.2, we describe in detail how we construct these reference wages in our data for each reform.

**Quitters are eligible for UI in Austria.** 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.<sup>26</sup> The fact that quitters are eligible for UI in Austria is crucial for our design: it ensures that our particular variation in the nonemployment outside option indeed shifts most Austrian incumbent workers’ outside options in the hypothetical scenario in which they were to take up the threat point of nonemployment in wage bargaining.

**Take-up is high in Austria.** As a consequence of broad eligibility, relatively long benefit durations, as well as mandatory registration with the UI agency (for continuity of health insurance coverage), take-up of UI benefits is high in Austria (in contrast to, e.g., the United States, where low eligibility and take-up potentially attenuate the role of UI benefits in the nonemployment value, see, e.g., Chodorow-Reich and Karabarbounis (2016)). Most workers separating will take up UI – unconditionally on the particular reason of separation in our analyses tracking workers separating from a job and entering employment. Table A.1 reports take-up rates after

<sup>26</sup>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 not eligible for UI benefits. See Venn (2012) for an overview.

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.

## 3.2 Four Large Reforms to the UI Benefit Schedule

A key motivation to study the Austrian setting is the unique amount of variation from quasi-experimental reforms to unemployment benefit levels, leading to readily measurable, money-metric nonemployment value changes. For our empirical analysis, we focus on four particularly large shifts in benefit levels. These reforms *increased* benefits in sharply defined segments of the earnings distribution. One of the reforms we analyze (1985) increased the maximum benefit level. Three reforms (1976, 1989, and 2001) sharply increased benefits in the lower part of the earnings distribution. The magnitude of the shifts in the replacement rate schedule was large with, e.g., a change of 21.7ppt for some workers in the 1976 reform or an increase in the maximum benefit level by 28% in 1985.

Figure 5 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 5, 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 5 plots the four reforms together in contemporaneous earnings percentile space. It shows that the reforms affected a wide range of earnings percentiles.

**Selection of reforms.** We choose the four large reforms in 1976, 1985, 1989 and 2001 as they 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. Consequently, we exclude several large reforms from our analysis, e.g., 1978 and 1982, that affected segments of the earnings distribution that had experienced other benefit level reforms in the last two years before the respective reform. Below we describe each of the four reforms.

**1976 reform.** 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 5 (a)). The reform primarily raised unemployment benefits below earnings of 3,700 ATS (6th percentile). The reform left replacement rates largely unchanged for workers with wages above the 12th percentile.<sup>27</sup>

<sup>27</sup>Another reform, enacted in January 1976, affected unemployment benefits in the higher parts of the earnings distribution, by raising the maximum benefit level, alas in parts of the wage distribution that had previously experienced a benefit reform. We thus restrict our attention, and our sample, to the first experiment.

**1985 reform.** In January 1985, the maximum monthly UI benefit increased by 29% from around 7,600 ATS to around 9,800 ATS. Figure 5 (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.<sup>28</sup>

**1989 reform.** On August 1st, 1989, reforms were enacted that increased benefits for low earners, depicted in Figure 5.(c).<sup>29</sup> Specifically, for individuals with previous monthly earnings between 5,000 and 10,000 ATS, i.e. the 16th 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.<sup>30</sup>

**2001 reform.** In January 2001, a benefit reform took place that 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 26th percentile of the earnings distribution. Figure 5 (d), cast in terms of gross earnings, illustrates this variation at the lower part of the earnings distribution.

**Reform Sample Summary Statistics.** Table 1 provides summary statistics for the individuals affected by each reform (the “treated” columns) and a “control” group of individuals for each reform (see section 4.1 for more details). 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 firm-by-year effects. We confirm the lack of differential trends through nonparametric and parametric placebo checks (see ex. the lack of pretends in Tables 2 and 3 and nonparametric analysis in Figures A.1 to A.4). Perhaps

<sup>28</sup>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.

<sup>29</sup>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.

<sup>30</sup>One year later, in June 1990, an additional replacement rate change was enacted. The replacement rate now gradually phased out between 10,000 and 26,400 ATS.

more compellingly, across most of our specifications, the parallel trends assumption holds during the pre-periods even without conditioning on control variables.

### 3.3 Data Description

Our primary data source is the Austrian Social Security Database (ASSD), described in Zweimüller et al. (2009). It provides monthly employment and annual earnings for all private-sector and non-tenured public sector employees in Austria from 1972 onward. Consequently, it excludes tenured public sector workers, the self-employed, and farmers. 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). In the data, annual earnings are censored at the social security contribution caps (see Zweimüller et al., 2009).<sup>31</sup> To account for the fact that the earnings cap changes over time, we adjust the earnings cap each year so that it falls in the same percentile of the earnings distribution. The ASSD data also include individual level covariates including gender, age, citizen status, and a white/blue collar indicator and firm side covariates including the firms location and detailed industry information.

In addition to the ASSD, we also draw on the universe of the Austrian unemployment register (AMS). The AMS allows us to measure the actual benefits paid to unemployed workers and thereby to assess the extent to which we can predict actual benefits based on lagged earnings. We report on this validation exercise in Appendix Section E.2. The employer identifiers denote establishments; we will often refer to employers as ‘firms’ in this paper.

### 3.4 Wage Setting in Austria

About 95% of Austrian workers are covered by a central bargaining agreement (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 that regularly lead to substantially higher wages within specific firms. At the beginning of our sampling period 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. Austrian collective bargaining agreements often feature clauses that require actually paid wages to rise in lockstep with the wage growth of the wage floors, although some some clauses specify lower wage growth. Importantly, the variation we exploit consists of benefit increases that should entail wage *increases* (rather than decreases) for specific treated workers, such that

<sup>31</sup>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.

even mandates for growth of actually paid wages would not be constrained upward. In addition, we can zoom in on firms with particularly flexible wage policies (see Section 5.3.2).

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

In this section, we analyze four of the largest UI benefit level reforms that occurred in Austria from the 1970s until today: 1976, 1985, 1989, and 2001. We first define the core of our empirical strategy that allows us to transparently analyze the effect of these reforms on wages by plotting raw data. As a complement, we implement a difference-in-differences analysis that compares wage growth in treated and untreated regions of the earnings distribution in a reform year to wage growth differences between those regions in previous years. Finally, we subject our findings to several robustness checks.

**Summary of results.** Throughout, we estimate very small effects of individual-level benefit changes on wages. Pooling the four reforms, we find tight estimated \$0.00 wage effects of a \$1.00 increase in benefits after one and two years, 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 industry-occupation and firm-by-year fixed effects. This insensitivity extends to new hires switching jobs and out of unemployment.

Interpreted through the standard Nash bargaining framework with nonemployment as the outside option, this result implies very small bargaining power parameters close to one. We interpret this to be a clear, quantitative rejection of the assumption that nonemployment is the relevant outside option in bargaining as existing, cleanly identified estimates of rent-sharing would imply a \$.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 Research Design: Wage Effects of Reform-Induced Benefit Changes

The spirit of our research design is a transparent difference-in-differences design that exploits reform-induced changes in UI benefits for treated workers and compares their wages to those of workers not treated by the reforms.

**Empirical relationship of interest: the effect of benefit levels on wages.** We estimate  $\sigma$ , a *dollar-for-dollar sensitivity* of wages to the nonemployment value by comparing variation in

UI benefits  $db_{i,t}$  with wage changes  $dw_{i,t} = w_{i,t} - w_{i,t-1}$ :

$$dw_{i,t} = \sigma \cdot db_{i,t}. \quad (45)$$

Our initial restriction will be that  $\sigma$  is homogeneous, although our subsample analysis will later allow for heterogeneous sensitivities for subgroups. As a normalization and to potentially include controls in regressions, we also normalize both sides by the worker’s lagged wage  $w_{i,t-1}$ , rendering the wage outcome into worker-level wage growth:

$$\Leftrightarrow \frac{dw_{i,t}}{w_{i,t-1}} = \sigma \cdot \frac{db_{i,t}}{w_{i,t-1}}. \quad (46)$$

The normalization implies that the benefit change can also be interpreted as the difference in the replacement rate (holding the lagged wage constant). An alternative interpretation of  $\sigma$  is then the semi-elasticity of wages with respect to replacement rates, since we divide by a lagged wage.

The analysis is built on an identification assumption that different earnings percentiles in our sample did not experience differential wage growth trends, which we test with placebo exercises.

**Theoretical benchmark: wage bargaining.** Our calibrated bargaining framework with nonemployment as outside option predicts a wage-benefit sensitivity of  $\sigma^{\text{Nash}} = 0.39$ , derived in Section 2.

**Constructing reform-induced UI benefit level changes.** The variation in the nonemployment option that we analyze arises from reform-induced shifts in UI benefit levels: the *difference* between the worker’s actual benefit level and her *counterfactual benefit absent the reform*. Formally, in year  $t$ , a worker  $i$  with benefit-relevant attributes  $\mathbf{x}_{i,t}$  is assigned benefit level  $b_t(\mathbf{x}_{i,t})$  by year- $t$  benefit schedule  $b_t(\cdot)$ . Our variation is the difference between this benefit level and the *counterfactual* benefit that the worker would collect in the same period  $t$  had the reform not been implemented and the  $t - 1$  schedule remained unchanged:  $b_{t-1}(\mathbf{x}_{i,t})$ :

$$db_{i,t} = b_t(\mathbf{x}_{i,t}) - b_{t-1}(\mathbf{x}_{i,t}). \quad (47)$$

Hence,  $db_{i,t}$  captures solely variation in the benefit level that is due to shifts in the benefit schedule. This is because we hold  $\mathbf{x}_{i,t}$  fixed and  $\mathbf{x}_{i,t}$  will by construction be unaffected by the reform-induced variation: in practice, it reflects pre-determined (lagged) wages.

The variation is zero if the benefit schedule does not change between  $t - 1$  and  $t$ , i.e.  $b_t(\mathbf{x}_{i,t}) = b_{t-1}(\mathbf{x}_{i,t}) \forall \mathbf{x}_{i,t}$ . Such years will form our *placebo years*. *Reform years* feature benefit schedule changes, such that  $b_t(\mathbf{x}_{i,t}) \neq b_{t-1}(\mathbf{x}_{i,t})$  for some workers  $i \in T$ , our *treatment group*. The value of  $db_{i,t}$  is zero for workers forming our *control group*  $C$ , i.e. workers for whom any



change in the benefit schedule leaves the benefit level unchanged even in the reform year.

**UI benefit reference wages.** In Austria, UI benefit levels are a function of pre-separation reference wages for UI claims in year  $t$ ,  $\tilde{w}_{i,t}$ , the precise construction of which we describe below in Section 4.2.<sup>32</sup> That is,  $\mathbf{x}_{i,t} = \tilde{w}_{i,t}$ , i.e. the assignment variable equals a reference wage  $\tilde{w}_{i,t}$  applicable in year  $t$ :

$$db_{i,t}(\tilde{w}_{i,t}) = b_t(\tilde{w}_{i,t}) - b_{t-1}(\tilde{w}_{i,t}). \quad (48)$$

Importantly, we rely on *lagged* wages for assigning reform-induced benefit changes. Since these wages are predetermined they are by construction unaffected by the reform. In consequence, our analysis sorts workers solely on the basis of their pre-reform reference wages that assigns treatment  $db_{i,t}$ . In Section 4.3, we conduct this exercise nonparametrically by sorting workers by their reference wages on the x-axis, and plot simple binned scatter plots of implied  $db_{i,t}$  treatments along with wage growth on the y-axis. In our regression analysis in Section 4.4, we additionally assign placebo treatments to the workers occupying the *earnings percentiles* in years before the actual reform.

The particular definition of the base-year earnings changed over the multiple decades that span our reform sample, so we review these concepts and validate our empirical earnings measure in Section 4.2. In the end, our strategy maps benefit levels to last-year earnings throughout all reform cases.

## 4.2 Outcome Variable and Sample Description

**Outcome variable.** The core outcome variable we consider is the change in the average daily wage  $w_{i,t}$ .<sup>33</sup> Since the data report employment spell length at daily frequencies, we can measure variation in compensation not driven by time in employment. For any employment spell, we calculate the average daily wage as the total compensation for that spell divided by spell length. If there are multiple concurrent employment spells, we select the one with the longest duration. Our main outcome variable is the relative change in the average daily wage from one year to the next, i.e.  $dw_{i,t+1}/w_{i,t}$ . Our calculations are based on nominal wages; since our econometric specifications include year effects, our results would remain unchanged if we considered real wages instead. We winsorize  $dw_{i,t+1}/w_{i,t}$  at the 1st and 99th percentile in our main specifications and also probe the robustness of our findings to different winsorization levels.

**Sample restrictions.** For our analysis, we impose a number of sample restrictions on the ASSD data. First, we restrict the sample to workers aged 25-54 with non-zero monthly earnings

<sup>32</sup>We ignore additional factors that in principle enter  $\mathbf{x}$  besides  $\tilde{w}$ , such as count of dependents, which largely occur as lump-sum payments shifting the intercept and are thus orthogonal to the variation that we study.

<sup>33</sup>The data are wage earnings, implying that we do not see hours changes. It is therefore possible that hours reductions may offset some wage effects.

each year. Second, we require that individuals are employed for 12 months out of the base year relative to which we calculate wage growth.<sup>34</sup> We impose this sample restriction because we are interested in actual *wage growth* among workers with high labor force attachment.<sup>35</sup> We separately examine potential effects on job mobility and employment in robustness checks.

**Reform samples.** For each reform, we define a treatment region as the nominal earnings interval in the reform year that was affected by the reform, recording the minimum and maximum percentile.<sup>36</sup> We then include observations from the same percentile interval in the non-reform years. We also include control earnings regions that are close to the treatment region but do not experience benefit changes in the reform year. The control region has the same percentile range as the treatment region.<sup>37</sup>

**Lagged base-year earning concept over the years.** UI benefits are a function of lagged wages, a fact we exploited to sort workers into treatment and control groups based on pre-determined wages, which are by construction not themselves affected by the reform-induced benefit shifts. The particular definition of the base-year earnings changed over the multiple decades that span our reform sample, so we now review these concepts. Ultimately, our strategy is to use last-year earnings for all those slight redefinitions, and validate that they accurately predict benefit receipt.

For the 2001 reform, the reference wage determining benefits in year  $t$  is the worker’s actual wage from the *previous calendar year*  $t - 1$ :  $\tilde{w}_{i,t}^{t > 1996} = w_{i,t-1}$ .<sup>38</sup> We observed this wage and therefore directly assign worker’s reform-induced benefit variation  $db_{i,t} = b_t(w_{t-1}) - b_{t-1}(w_{t-1})$  by sorting them by their lagged wage  $w_{i,t-1}$ . That is,  $db_{i,t}$  is solely a function of  $w_{t-1}$ . This concept has prevailed since 1996. During the 1970s and 1980s, the reference wage was the previous

<sup>34</sup>This sample restriction also 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 so we want to exclude them from the treatment and control regions. For some of the heterogeneity analysis where we are interested in individuals who very recently experienced unemployment, we relax this restriction.

<sup>35</sup>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.

<sup>36</sup>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.

<sup>37</sup>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

<sup>38</sup>More precisely, UI claims for unemployment spells beginning before June 30 of year  $t$  depend on labor income in  $t - 2$ , whereas earnings in  $t - 1$  pin down UIBs for spells beginning after June 30 of  $t$ .

*month's* wage.<sup>39</sup> 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  $t$ . That is, we simply multiply actual lagged wages  $w_{i,t-1}$  by a common factor: the average growth of nominal wages between  $t - 1$  and  $t$ ,  $\bar{g}_{t,t-1}$ . We calculate aggregate nominal wage growth  $\bar{g}_{t,t-1}$  by taking the average of individual nominal wage growth  $g_{i,t,t-1} = w_{i,t}/w_{i,t-1}$ .<sup>40</sup> This simple wage inflation procedure almost perfectly predicts wages and thus benefit levels.<sup>41</sup>

**Benefit schedules and predicting benefit levels.** Our identification design tracks incumbent workers and matches them with 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.

### 4.3 Non-Parametric Analysis

We start with a non-parametric analysis of each of the four reforms separately. We plot worker wage growth sorted by pre-reform UI reference earnings, which determine whether and how intensely workers are treated by the reforms. We illustrate our approach with a detailed description of our methodology for one particular reform, in 2001. Across reforms, our analysis reveals at most a low sensitivity of wages to benefit changes.

<sup>39</sup>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.

<sup>40</sup>For the reforms before 1996, our empirical strategy has analogues in the simulated instruments literature (see, e.g., Cutler and Gruber, 1996; Gruber and Saez, 2002; Kopczuk, 2005; Kleven and Schultz, 2014; Weber, 2014). For those reforms, we use lagged income to predict the assignment variable so that our identifying variation remains unaffected by the reforms. In contrast, for the 2001 reform, the assignment variable that guides benefit changes in our setting is determined in the past and thus already unaffected by the reform. As an additional conceptual difference to parts of the simulated instruments literature aimed at estimating taxable income elasticities, the variation we use is the benefit change that an individual experiences relative to the counterfactual had the reform not been implemented rather than the predicted, reform-induced benefit change over time. Finally, as an additional conceptual difference, we cast our analysis as a difference-in-differences design with multiple pre-periods so that we can directly assess the common trends assumption underlying our identification strategy.

<sup>41</sup>We verify the fit of the wage prediction procedure. The  $R^2$  for this regression is 0.93 in a worker-level regression pooling our sample years 1972 through 2003 (the slope of the coefficient is trivially 1); on average 0.87 for our reform years. For our context, where benefit changes affect workers differentially between earnings percentiles, we are particularly interested in whether the fit between the predicted nominal wage level and the actual wage is similarly good between different earnings percentiles. We therefore split up the sample into year-specific percentiles, and repeat the regression analysis in separate, percentile-specific regressions.

### 4.3.1 Graphical Analysis

**2001 reform: large benefit increase for lower earners.** Figure 6 shows the main results for the non-parametric analysis 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.<sup>42</sup> We group our data set into percentile bins; one data point represents one percentile of the earners in the full sample. We zoom into the wage range around the extension, covering all percentiles that experience the benefit increase, and extend the sample to an equal-sized control group of earners that did not see a reform-induced benefit change.

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 (23rd 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 assess whether the reform-induced benefit changes affected wages. The orange 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. We calculate the one-year wage effects at each percentile by calculating the difference between wage growth from 2000 to 2001, when the reform was in place, to wage growth during a pre-period from 1999 to 2000 when the reform was not in place.<sup>43</sup> We normalize this variable to zero for the lowest percentile not treated in 2001.

Wage effects of the reform would be captured by excess wage growth in treated parts of the wage distribution. As the figure shows, 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. Quantitatively, the average one- and two-year wage effects, calculated as the average excess wage growth below the dashed vertical line, compared to the average in the control region above the line are -0.37 and 0.07 percentage points, respectively. That is, the 2001 reform which increased benefits relative to previous earnings by about 4 percentage points was associated with a 0.07 percentage point increase in wages after two years.

To provide a visual benchmark for these effects, we also plot the wage growth *predicted* by our calibrated bargaining framework in section 2. Values of  $\phi < 0.1$  imply wage effects even above the

<sup>42</sup>The benefit schedule  $b_{2001}(\cdot)$  is a function of *net* earnings (while  $b_{2000}(\cdot)$  is a function of gross earnings, as with all schedules through 2000). We use a tax calculator to translate gross earnings (which our administrative data provide) into net earnings to compute  $b_{2001}(\cdot)$ . 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 gross earnings. We thank David Card and Andrea Weber for sharing an income tax calculation program for Austria.

<sup>43</sup>Analogously, we calculate two-year wage effects as the percentile-level difference between wage growth from 2000 to 2002 vs. from 1998 to 2000.

orange line. 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, which is in contrast to the rent-sharing estimates we reviewed in our calibration of  $\phi$  in Section 2. We discuss model alternatives to Nash and associated alternative empirical predictions in Section 6.

Our analysis rests on an underlying identification assumption that in the reform year, wage growth would have been parallel to the pre-reform year (up to an intercept shift) even in the absence of the reform. We test this assumption in two ways. First, the flat wage effects across the control percentiles strongly support this assumption. Additionally, a second test, reported in Appendix Figure A.1, further assesses the parallel trends assumption underlying our identification strategy. Here, we estimate the effects of placebo reforms at the same earnings percentile ranges, but we lag both the reform period and the pre-period by two years. This placebo exercise thus assesses whether the earnings percentiles affected by the 2001 reform experienced higher or lower wage growth compared to other earnings percentiles in periods before 2000. This could occur if the gradient were tilting over time -- such that our zero result could have been a coincidence and mask a treatment effect, whereas for example the 1999 cohort had a negative excess wage growth among the treated workers compared to the 1998 cohort.

In Figure A.1 can discern no such effects for a placebo reform in 1999 for the one-year earnings changes. At the two-year level there is some evidence of a *positive* pre-trend. While such a pre-trend would bias our results upward, it motivates our parametric analysis in Section 4.4 where we add industry/occupation and time-varying firm fixed effects to net out any such pre-trends. In this framework, we formally test for pre-trends and find no significant trends across all of the reforms.

**1989 reform: increase in benefits for low earners.** We conduct an analogous analysis for the 1989 reform and present results in Figure 7. The 1989 reform increased benefits for workers with base-year earnings below ATS 12,000 by up to eight percentage points.<sup>44</sup> For that reform, we detect moderate, positive wage effects: we find a 0.71 percentage point effect on wages at the one-year horizon and of 1.69 percentage points at the two-year horizon. Nonetheless, even the two-year wage effect is substantially smaller than the effect that would be predicted based on bargaining with nonemployment as the outside option, as indicated by the discrepancy between the orange and the brown lines.

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

<sup>44</sup>Appendix Figure A.2 documents that the assignment variable (green line) and the actual benefit level based on contemporaneous earnings (red line) line up very closely for the 1989 reform at the one-year horizon. At the two-year horizon, an additional reform affected benefits in the control region and our analysis of longer-run effects of the 1989 reform hinges on assumptions about whether the 1990 reform had short-run effects on wages.

and the homogeneous earnings inflation procedure. In Appendix Figure A.2 in the Appendix, the assignment variable—based on inflated lagged earnings—is again plotted with the green line and the actual benefit level based on contemporaneous earnings with the red line. 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.<sup>45</sup> The analysis reveals that realized benefit changes closely track our reform-induced variation: workers with base-year earnings above ATS 12,000 experience almost no benefit change while workers with lower base-year earnings experience marked increases in benefit levels that closely track the reform-induced schedule changes, increasing benefits by up to 15 percent.

**1985 reform: increase in maximum benefit levels** Figures 8 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. 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. The 1985 reforms thus led to a significant increase in benefits for workers in higher parts of the earnings distribution. Nonetheless, we find no evidence for tantamount wage increases among workers treated by the reforms. Instead, our results indicate wage effects in treated earnings regions of about -0.26 percentage points at the one-year horizon and of -0.57 percentage points at the two-year horizon.

**1976 reform: increase for low earners.** We conduct an analogous analysis for the 1976 reform and present results in Figure 9. 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 point estimate for the relative wage decrease is -2.06 over one year and -3.5 over two years. The negative point estimates do 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.

#### 4.3.2 The Average Sensitivity of Wages to Benefit Changes Across Reforms

Finally, we provide an illustration for our research design to provide a quantitative estimate of the sensitivity of wages to the nonemployment value. Figure 10 plots the excess wage growth and unemployment benefit change for each earnings percentile across all four reforms (using different

<sup>45</sup>Similarly, had we found a large treatment effect on wages, this relationship would be weaker for the treated workers.

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  $\hat{\sigma} = -0.001$  (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 includes zero and we can rule out effects larger than 0.03 and 0.11 at the one- and two-year horizon, respectively. 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.

## 4.4 Difference-in-Differences Design

We next investigate the regression analogue of the non-parametric analysis above in Section 4.3 to a difference-in-differences framework. This approach provides estimates and confidence intervals for the effects of benefits on wages, allows us to formally test for pre-trends and thus the identification assumption of our design, and accommodates a rich set of controls. By pooling the reforms, we increase the statistical power of our analysis and can precisely measure even potentially small effects. The analysis reveals wage sensitivities to nonemployment that range from *negative* 3 to positive 0.7 cents on the dollar after one and two years. Additionally, the confidence intervals for our preferred specifications allow us to reject an estimated wage benefit-sensitivity of more than 3 cents on the dollar.

### 4.4.1 Econometric Framework

The variation we use for identification are reform-induced benefit changes occurring across percentiles of the earnings distribution, comparing percentiles that experience a benefit reform to those that do not in a given year, and within an earnings percentile over time comparing actual to placebo reforms in the pre-period. Additionally, our difference-in-differences design compares the effects of benefit changes in actual reform years to those of placebo reforms in pre-reform periods, testing for parallel pretrends.

**Regressor of interest.** Our regressor of interest is the reform-induced change in unemployment benefits,  $db_{i,t}$ . Following Equation (46), this variation is the difference between the predicted benefits  $b$  in the reform year  $r$  and the counterfactual benefits worker  $i$  would receive in reform year  $r$  if the pre-reform year schedule  $b_{r-1}(\cdot)$  were still active.  $db_{i,t}$  varies across individuals in different regions of the earnings distribution and is zero for the control group.

**Reduced-form specification.** The reduced-form specification of our difference-in-differences design regresses wage changes,  $dw_{i,r,t} = w_{i,r,t} - w_{i,r,t-1}$ , on actual and placebo benefit changes,  $db_{i,r,t}$ . We again normalize both the wage and the benefit change by  $i$ 's wage level in  $t - 1$ ,  $w_{i,r,t-1}$ .<sup>46</sup> For identification, we control for percentile fixed effects and year effects as well as additional control variables. We estimate the following difference-in-differences specification using the procedure in Correia (2017):

$$\frac{dw_{i,r,t}}{w_{i,r,t-1}} = \sum_{e=-L}^0 \delta_e (\mathbb{1}_{(t-r=e)} \times \frac{db_{i,r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}) + \tau_{r,P_t} + \theta_{r,t} + \gamma_{r,t} \ln w_{i,r,t-1} + X'_{i,r,t} \phi_{r,t} + \epsilon_{i,r,t}. \quad (49)$$

**Treatment effect.** We let  $e = t - r$  denote event time and the  $\delta_e$  are the coefficients of interest. In  $e = 0$ ,  $db_{i,r,t}$  corresponds to the actual reform-induced change in the benefit level,  $db_{i,r-1}(w_{i,r-1})$ , from  $r - 1$  to  $r$  so that  $\delta_0$  captures the effect of the reform-induced benefit change on wages from the pre-reform year,  $r - 1$ , to  $r$ . Intuitively, the coefficient  $\delta_0$  therefore estimates the linear slope between the benefit change and excess wage changes traced out by the observations underlying the binned data points in Figure 10 – except that we can now include multiple pre-period years, add control variables, and can directly test the parallel trends assumption.

**Testing for parallel trends with placebo reforms.** The core identification assumption is that percentiles that received higher benefits due to the reforms would not experience differential wage growth if, counterfactually, the reforms had not been implemented. We can assess the plausibility of this parallel trends assumptions by analyzing the  $\delta_e$  in the pre-period ( $e < -1$ ), the coefficient on placebo reforms in pre-reform years. We assign placebo benefit changes to earnings percentiles based on the benefit change that an earnings percentile experienced in a reform year.<sup>47</sup> In years  $t < r$ , the  $db_{i,r,t}$  capture placebo benefit changes and we normalize  $\delta_{-1}$  to

<sup>46</sup>In addition to the one-year horizon exposted here, we also conduct our analysis with wage changes  $dw_{i,r,t}$  at the two-year horizons.

<sup>47</sup>Formally, we define the following regressor for the difference-in-differences design:

$$db_{i,r,t}(w_{i,t-1}) = \begin{cases} db_{i,r}^{\text{Reform}}(w_{i,t-1}), & \text{if } t = r \\ db_{i,t}^{\text{Placebo}}(w_{i,t-1}), & \text{if } t < r \end{cases}$$

As before, we define  $db_{i,r}^{\text{Reform}}(w_{i,r-1}) = b_r(w_{i,r-1}) - b_{r-1}(w_{i,r-1})$  for the 2001 reform and  $db_{i,r}^{\text{Reform}}(w_{i,r-1}) = b_r(\bar{g}_{r,r-1} \cdot w_{i,r-1}) - b_{r-1}(\bar{g}_{r,r-1} \cdot w_{i,r-1})$  for the reforms before 1995. To obtain  $db_{i,t}^{\text{Placebo}}(w_{i,t-1})$ , we calculate the average of  $db_{i,r}^{\text{Reform}}$  in each percentile  $P_{r-1}$  of the earnings distribution in the pre-reform year  $r - 1$ , i.e.  $\bar{db}_{i,r}^{\text{Reform}}(P_{r-1}(w_{i,r-1}))$ . We then assign each individual the  $db_{i,r}^{\text{Reform}}$  corresponding to their earnings percentile in a given year  $t$  in the pre-reform period. Moreover, to keep the economic magnitude of the placebo reform similar across reform and placebo years, we deflate  $\bar{db}_{i,r}^{\text{Reform}}$  by the average nominal wage growth rate  $\bar{g}_{r,t}$  between a placebo year  $t$  and the reform year  $r$ :

$$db_{i,t}^{\text{Placebo}}(w_{i,t-1}) = \frac{\bar{db}_{i,r}^{\text{Reform}}(P_{r-1}(w_{i,t-1}))}{\bar{g}_{r,t}}.$$



zero (since we omit one coefficient due to earnings percentile fixed effects).<sup>48</sup> If differentially affected earnings percentiles were on different trends, then the  $\delta_e$  in the pre-period would be systematically different from zero.

**Controls.** The model includes reform-specific percentile fixed effects  $\tau_{r,P_t}$  which absorb any permanent differences in wage growth across percentiles, e.g., due to mean reversion.<sup>49</sup> The model also includes year effects and thereby absorbs differential wage growth across years. In addition, we control for  $\ln(w_{i,r,t-1})$  in our main specifications, allowing coefficients to vary by year. Doing so allows us to control parametrically for, e.g., effects of time-varying shocks to different parts of the earnings distribution.

In addition, the setup also allows us to control for a rich set of covariates  $X'_{i,r,t}$  with year-specific coefficients that can absorb potential additional shocks. As a first additional set of control variables, we include demographic characteristics in  $X'_{i,r,t}$  and control for gender as well as cubic polynomials of experience, tenure, and age. Second, we can also control for industry-by-occupation-by-year fixed effects  $\gamma_{o(i,t),k(f(i,t)),t}$ , which absorb time-varying shocks at the industry level that may have differential effects on different parts of the earnings distribution. In our most fine-grained specification, we leverage variation between workers within the same firm by including firm-by-year effects  $\psi_{f(i,t),t}$ .

**Samples.** We estimate the difference-in-differences specification in Equation (49) jointly by stacking data for each reform. In the main specifications, we draw data from three years before a reform to the post-reform period, restricting the sample to be local to the part of the income distribution affected by the reform and in line with the nonparametric analysis the previous Section 4.3, and described in detail in Section 4.2. We also assess the robustness of this methodological choice by estimating specifications with varying control percentile ranges.

**Standard errors.** In our main specifications, we report standard errors based on two-way clustering at the individual and the earnings percentile level as we also assign benefit changes at the earnings percentile level. We also run specifications with standard errors clustered at other levels, leading to quantitatively similar results (Appendix Figure A.6).

**Validation exercise.** We supplement the reduced form analysis in Equation (49) with a validation exercise to assess whether reform-induced benefit changes led to realized benefit changes.

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.

<sup>48</sup>We normalize  $\delta_{-2}$  to zero and omit  $\delta_{-1}$  for specifications in which we consider outcomes over  $n = 2$  periods.

<sup>49</sup>By reform-specific, we mean that the percentiles are added separately for each of the four reform samples that we stack.

Formally, we estimate the following specification, letting superscripts  $V$  denote coefficients for the validation exercise:

$$\frac{b_{i,r,t} - b_{i,r,t-1}}{w_{i,r,t-1}} = \sum_{e=-L}^0 \delta_e^V (\mathbb{1}_{(t-r=e)} \times \frac{db_{r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}) + \tau_{r,P_t}^V + \theta_{r,t}^V + \gamma_{r,t}^V \ln w_{i,r,t-1} + X'_{i,r,t} \phi_{r,t}^V + \epsilon_{i,r,t}^V \quad (50)$$

Intuitively, the coefficient  $\delta_0^V$  captures the extent to which reform-induced benefit changes lead to actual benefit changes, with coefficients close to one indicating a strong relationship. Intuitively,  $\delta_0^V$  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  $t = r$ . As before, we normalize  $\delta_{-1}^V$  to zero. In years  $t < r$ , the coefficients  $\delta_e^V$  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.

#### 4.4.2 Results

Mirroring the non-parametric analysis, the difference-in-differences analysis reveals that wages are close to insensitive to benefit changes. At the one- and two-year horizons we find insignificant and generally negative effects, wages remain economically close to insensitive to changes in the nonemployment option. Across all specifications, we do not find point estimates larger than  $\hat{\sigma} = 0.007$ . Specifically, the point estimate for the effect of benefit changes on wages 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. Stated alternatively, given our confidence intervals, we can reject that a \$1.00 increase in the nonemployment payoff due to UIB increases wages by more \$0.03 after two years. Interpreted through the lens of a model of bargaining with nonemployment as outside options, our point estimates would thus indicate bargaining power parameters close to one even for the two-year specification.

Table 2 presents the results for the difference-in-differences specification for wage effects after one year. The table presents estimates of  $\delta_e$ , i.e. the interaction of actual (reform-induced) and placebo benefit changes with event time. The regressor of interest is  $\delta_0$ , capturing the wage growth associated with reform-induced benefit changes. The different columns progressively include richer individual and firm-level controls. We have normalized  $\delta_{-1}$  to zero and assess pre-trends with the  $\delta_{-3}$  and  $\delta_{-2}$  as well as testing that the pre-period coefficients are jointly equal to zero. Across all six specifications in Table 2, we cannot reject that both pre-period estimates are equal to zero. As a complement, we also run specifications with different controls for base-year earnings and find quantitatively similar results (Appendix Figure A.5).

Throughout all specifications in Table 2, we find quantitatively similar effect sizes centered at zero. The treatment effects are also plotted in the left part of Figure 11. 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  $-.014$  (columns 3 and 4) when including industry-occupation-year fixed effects and including all controls jointly.

In a next step, we analyze longer-term effects of the benefit reforms and study effects at the two-year horizon. These results are reported in in Table 3 and similarly plotted in the right half of Figure 11. 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  $-0.07$  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.** Our research design also allows us to assess whether changes in the nonemployment outside option *between workers within in the same firm* lead to wage changes. This is a core difference to the literature estimating rent-sharing elasticities which relies on variation at the firm level. Our research design allows us to do so by including firm-by-year fixed effects as control variables so that the fixed effects absorb any between-firm variation in wage growth. We report the results of these specifications in columns 5 and 6 of Tables 2 and 3. 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.

## 4.5 Robustness Checks

We report the results of several additional specifications that probe the robustness of our findings and address potential identification concerns by (i) addressing wage stickiness by looking at estimated effects by job transition type, (ii) estimating effects on employment and retention, (iii) unemployment durations, and (iv) sickness, (v) assessing whether reform-induced variation based on lagged wages led to actual benefit variation, (vi) assessing robustness to parametric earnings controls (vii) probing different levels of clustering, and (viii) winsorization.

**Wage stickiness: estimated effects for movers vs. stayers.** One potential explanation for small wage benefit pass-through is that wage stickiness among incumbent workers slows down wage adjustments and masks the pass-through. To assess whether this may be driving our results, we estimate the treatment effects separately for job stayers, recalled workers, job-to-job movers, and job-to-job movers who experience an intervening unemployment spell.

Figure 13 displays the estimated one- and two-year treatment effects for each of the transition types. In particular, we consider job-stayers, workers who were recalled and workers who moved

to another employer. In the reported specifications, we interact an indicator for each transition type with the  $\delta_e$  coefficients in equation 49. We also vary the parametric earnings controls by transition type, allowing for differential earnings growth patterns by transitions type. Across all three transition types, we estimate small and insignificant effects and, even with the much smaller sample sizes, the confidence intervals do not include our theoretical benchmark of 0.39. Since job movers are likely to renegotiate their wages (even if wages remain fixed on the job afterwards), wage stickiness could not explain the lack of a wage-benefit pass-through for the movers. Consequently, it seems unlikely that it is also driving our main result.<sup>50</sup>

There are a few caveats to consider about these results. First, job-to-job transitions may also be affected by the reforms. Consequently, since we condition on an endogenous outcome, selection effects may be offsetting a positive treatment effect. In Figure 12, however, we find no evidence that the reforms affected individuals' probability to experience one of the transition types described above, likely limiting potential selection concerns here. An additional caveat to interpreting the movers results is that for those movers that went through an unemployment spell, there are other non-bargaining channels through which our reforms may have affected the re-employment wages of unemployed individuals. For example, this wage effect could combine the bargaining channel with the McCall model reservation wage channel and a skill depreciation channel (where longer UI induced unemployment spells lead to skill depreciation). These potential confounds from looking at individuals who experienced an unemployment spell had in part motivated our approach of looking at the on-the-job wage changes of incumbent workers in the first place.

**Employment outcomes: composition and productivity effects.** Our wage analysis tracks a panel of workers and investigates their wage changes before and after the improvement in the treatment group's outside option. One concern is that the improvement in the nonemployment outside option may lead marginal workers to select into nonemployment that would have otherwise experienced higher wage growth (e.g. because they are young or have low tenure, and therefore high wage growth).<sup>51</sup> To assess whether there is selective attrition by wage growth, we also study whether benefit level reforms affected employment and retention. Figure 12 reports treatment effects for the probability of separating from the current firm and the probability of being recalled over one- and two-year horizons. We do not find a statistically or economically significant effect of the improved nonemployment option on these outcome variables across a wide number of

<sup>50</sup>We have also probed further into dividing movers into (a) movers that directly move from one employer to another and (b) workers who move to another employer after going through an unemployment spell. In those specifications, we find negative estimates for movers that go through an unemployment spell and positive effects for movers who move to another employer without an intervening unemployment spell. However, we cannot reject that the effect on wages in this group is zero after two years.

<sup>51</sup>See Jäger et al. (2018) for evidence for older workers separating into nonemployment in response to a large increase in the potential benefit duration, along with characterization of the incremental separators.

control. Another concern is that the marginal product of the treated workers may be boosted if this skill group becomes relatively scarce. The small wage effects and no evidence for separations implies that this channel is unlikely to be active.

**Unemployment duration and incidence effects.** Figure 12 also reports treatment effects for the probability of experiencing an employment to unemployment to employment (EUE) spell and the number of months spend on UI over the next one and two years. At the two-year level we see suggestive evidence that treatment increased the probability of an EUE spell and significant effects on the number of months spend on UI. This result is consistent with the prior literature on the effects of UI generosity on unemployment spell durations (some of which comes from analyzing a subset of the reforms we consider (Lalive et al., 2006)). These effects are also not concerning for our empirical strategy because we focus on the wage changes of incumbent workers and not the re-employment wages of unemployed workers. Finding an effect on UI durations, however, is reassuring as it shows that, as expected, these reforms were economically meaningful and substantially altered incentives for unemployed workers.

**Efficiency wage effects: sickness incidence.** Efficiency wage mechanisms may mask bargaining-related wage effects by lowering productivity, if workers are more likely to reduce effort. We do not find evidence for this (difficult to document) channel in our data. First, we have not found retention effects in the previous robustness checks. Second, we additionally exploit the registration of sickness spells in our administrative data, and report the treatment effect on this outcome in Figure 12. Sickness spells do not respond to the improved outside option, suggesting that workers do not appear to engage in additional shirking as potentially recorded by this proxy.<sup>52</sup>

**Validation exercise.** We implement a validation exercise as outlined in Equation 50 and report results in Appendix Table A.2 to assess whether reform-induced variation based on lagged wages led to actual benefit variation. For the effect of predicted, reform-induced benefit changes on realized benefit changes, the analysis reveals a 0.754 (se 0.018) coefficient at the one-year horizon and of 0.455 (0.024) at the two-year horizon and thus indicates that the reforms we study meaningfully affected benefits among those that we predict to be affected. 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.<sup>53</sup> In the pre-period, the estimated coefficients are an order of magnitude smaller than the coefficient in the reform year. For the

<sup>52</sup>However, the productivity decrease would have had to be tremendous in order to account for the net wage effect of zero. If worker bargaining power were 0.2, then the 8ppt increase in the change in benefits (normalized by the wage) would have had to imply a  $\frac{1-0.1}{0.1} \times 8\text{ppt} = 7.2\text{ppt}$  decline in the productivity/wage ratio to offset the bargaining channel and leave wages unchanged on net.

<sup>53</sup>We also run a validation specification excluding the 2001 reform and find overall similar results.

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. The effects associated with the actual reforms,  $\hat{\delta}_0^V$ , are an order of magnitude larger than the placebo effects in the pre-period. For the two-year validation exercise, we cannot reject that the pre-period coefficients are jointly equal to zero.

**Parametric Earnings Controls.** In our main specifications we control for a time-varying trend in log earnings. A large literature makes use of the simulated instruments approach that is similar to our empirical strategy. Heterogeneous income trends are a major threat to identification in these settings, since it is not possible to discern directly whether wage changes are due to the reforms if there are substantial systematic differences in income growth across the earnings distribution. Most studies using simulated instruments rely on parametric earnings controls to control for heterogeneous trends (Kopczuk, 2005; Kleven and Schultz, 2014). As is common in this literature, we test the sensitivity of our results to different parametric earnings controls, namely earnings percentiles and in linear earnings.

To assess whether this choice of parametric earnings control affects our estimated results, we present estimates of our main specification (column (4) in Table 2) with different earnings controls in Appendix Figure A.5. Specifically, we include estimates controlling for log-earnings, linear earnings, and linear earnings percentiles. The change in earnings controls does not change our qualitative conclusion of the small wage-benefit pass-through and the alternative earnings controls actually result in more negative estimated treatment effects.

**Levels of clustering.** In Appendix Figure A.6, we assess the robustness of our empirical conclusions to the level of clustering. We estimate the specifications reported in column (4) of Table 2 that reported confidence intervals based on two-way clustering at the individual and percentile level and also include confidence intervals based on finer percentile levels and two-way clustering with various firm, percentile, individual, and reform-specific levels.<sup>54</sup> As Appendix Figure A.6 reveals, standard errors and the resulting confidence intervals are of similar size across the different clustering levels that we consider at both the one- and two-year levels.

**Winsorizing the wage growth variable.** In addition, we also assess whether winsorizing the outcome variable affects the results we find. Appendix Figure A.7 reports results with no winsorization as well as winsorization at the 1st and 5th percentile (all of our winsorization is symmetric so winsorizing at the 1st percentile means the 1st and 99th). The estimates are quantitatively robust across specifications and do not differ significantly from the ones in our main specification that winsorized at the 1st and 99th percentile.

<sup>54</sup>To illustrate, the reform-specific clustering would lead us to treat observations in the 5th percentile for the 1976 and the 2001 reform as part of different clusters.

## 5 Finding the Missing Link: Dissecting the Insensitivity of Wages

In this Section, we dissect the wage insensitivity guided by the following three-element chain linking  $b$  and  $w$  in the model:

$$\frac{dw_i}{db_i} = \underbrace{\frac{dw_i}{d\Omega_i}}_{\text{Sensitivity of wages to outside options}} \times \underbrace{\frac{d\Omega_i}{dN_i}}_{\text{Sensitivity of outside option to nonemployment value}} \times \underbrace{\frac{dN_i}{db_i}}_{\text{Sensitivity of nonemp. value to UIB shifts}} \quad (51)$$

Consequently, we conduct theory-informed 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. Besides heterogeneity in the worker-level wage-benefit sensitivity, we also test for alternative treatments shifting outside options, such as potential benefit duration rather than benefit level reforms and group-level aggregates of the instrument at the firm level.

Figures 14 and 15 present these estimates for a large number of heterogeneity groups at the one- and two-year horizon. For all categories except for sex and occupation, the top red estimate is for individuals with the lowest of the respective heterogeneity variable and the bottom blue estimate is for individuals with the highest (e.g., lowest and highest tenure quintile). Across the larger number of heterogeneity groups we consider, we find very little variation across groups, and discuss the results and implications in detail below.

### 5.1 Sensitivity of the Nonemployment Value to UI Benefits $dN/db$

Our model provides a tight link between the shift in the payoff while nonemployment  $db$ , and the nonemployment value  $N$ . Here we assess the empirical validity of that link.

#### 5.1.1 Exposure to Unemployment Risk and the Unemployment Insurance System

We directly test for the theoretical prediction that wages are more sensitive to UI-induced shifts in the nonemployment value for workers more exposed to unemployment risk. In the model in Section 2, the sensitivity of  $N$  to  $b$  is mediated by post-separation time in nonemployment  $\tau$ , which puts more weight on the instantaneous payoff while unemployed  $b$ . The heterogeneity dimensions we consider are unemployment risk, the local unemployment rate, and a direct prediction for post-separation time in nonemployment.

**Heterogeneity by time in nonemployment  $\tau$ .** The blue line in Figure 16 presents coefficient estimates of heterogenous treatment effects of the benefit change on wages, for workers differing in *predicted* post-separation time in nonemployment  $\tau$  (sorted into quintiles). On the x-axis, the figure plots the corresponding mean of each worker group’s  $\tau$ . The figure also plots the predicted wage-benefit sensitivity based on expression (9), in the yellow line. This wage sensitivity– $\tau$  gradient therefore traces out an empirical analogue of the theoretical gradient in Figure 3. This analysis therefore provides another, cross-sectional test of the bargaining model and the role of outside options in wage setting, complementing our average treatment effect in the previous section.

Specifically, we assign each employed worker an idiosyncratic predicted post-separation time in unemployment  $\hat{\tau}_i$ , using a regression model with pre-separation attributes fit to actual separators.<sup>55</sup> Specifically, we consider only UI receipt in our measurement of  $\tau$ , tightly connected to the model and avoiding takeup or finite benefit duration complications.<sup>56</sup> We then back out her predicted wage-benefit sensitivity based on the structural wage-benefit sensitivity expression (9), maintaining  $\phi = 0.1$  for all workers (permitting a negative correlation between  $\tau$  and  $\phi$  will generate even more dispersion in the predicted sensitivity). In each reform, we group our sample of workers into quintiles of  $\tau_i$ . We then estimate heterogenous treatment effects in our data, for the five  $\tau$  quintiles.

Figure 16 reveals three insights. First, the median separator’s  $\tau$  is around 7% (consistent with our baseline calibration) – there is substantial variation in  $\tau$  between workers. Importantly, Figure 3 revealed that for low values of  $\tau$ , the predicted wage-benefit sensitivity is more sensitive to shifts in  $\tau$  than for high values. Therefore, the empirical  $\tau$  quintiles of workers have large variation in the predicted wage-benefit sensitivity, reflected in the slope of the yellow line, which almost reaches 0.60 for the high group. Second, inspecting the blue coefficients by treatment quintile confirms the zero average effect of benefits on wages. Third, and perhaps more strikingly, 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.

**Other unemployment risk proxies.** Additional dimensions of unemployment risk are presented in the *unemployment risk* panel in Figure 14 and Figure 15. These figures present the estimated treatment effects for the top and bottom quintiles of these heterogeneity categories. While three of the four measures of unemployment risk are associated with larger point estimates

<sup>55</sup>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. 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  $\tau_i$ , feeding that worker’s pre-separation attributes (lagged by three years) into the model.

<sup>56</sup>We ignore *Notstandshilfe* (post-UI unemployment assistance), thereby underestimating the overall level of  $\tau$ .



(although still close to zero), we find that the estimates in the lowest and highest category of unemployment risk are not statistically significantly different from zero for any of the four categories.

In conclusion, we do not find evidence for wage effects even among subgroups for whom the UIB increases would have plausibly – and mechanically – entailed larger shifts in  $N$ .

### 5.1.2 Salience and Knowledge about Unemployment Insurance or the Nonemployment Scenario More Generally

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.<sup>57</sup> 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.

**Direct evidence on salience of and knowledge about UI benefit levels.** 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 18. 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.<sup>58</sup> Moreover, we also run several additional tests and find that workers with more children accurately predict that they would receive higher benefits.

**Reviewing additional evidence.** 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

<sup>57</sup>For instance, Abeler and Jäger (2015) find evidence consistent with lower responses to incentives in more complex systems.

<sup>58</sup>The replacement rate can differ from 55% as there are additive 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.

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.<sup>59</sup> 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. in 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 to the incumbent worker in the previous year.

### 5.1.3 Variation in UI Generosity From Potential Benefit Duration

Next, we 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 also complements our benefit variation as it the 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 wages 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.

The particular reform we study occurred in 1989. Figure A.8 shows how the PBD schedule changed for individuals age 30-49 in 1989. Before 1989, the PBD was only experience and not age-dependent.<sup>60</sup> In 1989, these eligibility rules were changed so that individuals age 40-49 with at least five years of experience in the past 10 years were eligible for 39 weeks while individuals below age 40 were still only eligible for 30 weeks. For the analysis below, we focus on the PBD reform for individuals age 40-49 and compare their earnings growth to individuals age 30-39.<sup>61</sup> We apply the same sample restrictions as in our main result for the full sample but drop all

<sup>59</sup>Hendren (2017) finds that employed workers can predict separations.

<sup>60</sup>Individuals with less than 12 weeks of UI contributions in the last two years were eligible for 12 weeks, individuals with 52 weeks in the last two years were eligible for 20 weeks, and individuals with 156 weeks (3 years) and the last five years were eligible for 30 weeks.

<sup>61</sup>These rules applied to workers with at least 6 years of experience in the past 10 years, which is our sample restriction for this part of the analysis. See Nekoei and Weber (2017) for an evaluation of this reform on *unemployed* job seekers’ spell duration and reemployment wages.

individuals present in particular Austrian regions where workers aged 50 and above were eligible for even larger PBD reform since 1988.<sup>62</sup>

The two panels in A.9 plot the average earnings log differences (one and two years) by age groups in the treated and control years. The left-panel plots the average wage growth from 1987-1988 (the control year) and from 1988-1989 (the treatment year) as well as their difference. If the PBD extension for older workers passed through to their wages, we would expect an increase in wage growth for older workers. The right panel plots the same for two-year wage growth. Neither Figures show an increase in wage growth for treated individuals.

In Figure A.10, we report results from estimating a specification similar to equation 49 but replacing the replacement rate reform indicators with an indicator for being ages 39-42 and adding age-specific fixed effects. We also include the same controls included in specification (4) in Table 2. The figures show no significant treatment effects when the reform was enacted as well as a lack of pre-trends, validating our identifying assumptions.

We draw two conclusions. First, PBD reforms do not appear to affect wages among incumbent workers through a bargaining channel even two years after the reform. Second, the findings from our benefit-level-based design, which uses base-year income as the assignment variable, carry over to reforms that change other dimensions of UI generosity but assign treatment based on age.

## 5.2 Sensitivity of Outside Options to the Nonemployment Value $d\Omega/dN$

Perhaps our variation does induce shifts in the nonemployment value  $N$ , but the resulting variation does not actually shift the worker's relevant outside option  $\Omega$  that enters wage bargaining in real-world wage negotiations.

### 5.2.1 Alternative Worker-Level Outside Options

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 (see Postel-Vinay and Robin, 2002; Cahuc et al., 2006), as wages have been ratcheted up. Consistent with the investigation of proxies for unemployment risk in the previous investigations, we find some, but statistically insignificant and quantitatively small, evidence that recently nonemployed workers

<sup>62</sup>We do not study the latter reform because of a regional reform that further increased PBD for workers older than 50 and led to separations (and thus attrition) among those older workers (Jäger et al., 2018), that would not allow for a measurement of wage effects.

exhibit larger wage-benefit sensitivities at the two-year horizon (Figure 15), but not at the one-year horizon (Figure 14).

### 5.2.2 Firm-Level Instruments and Group-Level Bargaining

While the model assumes atomistic bargaining – between one individual worker and one firm – perhaps real-world wage setting does follow bargaining with the nonemployment outside option but negotiations occur at the firm level.<sup>63</sup> In such an alternative model of bargaining, employers do not negotiate with individual workers but instead bargain with their workers as a collective, for instance with plant-level works councils in the Austrian case.<sup>64</sup> The entity negotiating on the workers’ behalf may take into account an average of workers’ outside options in bargaining with the firm, such as in union bargaining models.<sup>65</sup>

We study the role of firm-level bargaining by aggregating the reform-induced benefit variation at the firm level. Intuitively, we compare firms where many workers receive benefit changes due to a particular reform to those firms where only few or no workers are affected. Our analysis tests whether nonemployment outside options matter for wage bargaining at the firm-level, such that increases in the firm’s employees’ average nonemployment value affect wage growth. Our regression specification remains structurally identical to the specification in 49. However, rather than the worker-level treatment variable, the regression now features its group-level *average* as the regressor of interest.<sup>66</sup> We associate workers with the firm they work at in the pre-reform year. We plot the variation used in our firm-level specifications in Appendix Figure A.11, which shows the distribution of the average, reform-induced benefit change across firms. The Figure reveals that a significant share of firms experience no reform-induced changes in workers’ average benefits as well as a wide dispersion in average benefit changes among the remaining firms. As before, we also assess pre-trends based on placebo reforms, similarly aggregating the individual-level placebo treatments assigned at the earnings group to the firm level. Our specification now includes two additional firm-level control variables: we control non-parametrically for the average exposure of the firm to reform-induced benefit changes and for the share of workers in the firm experiencing

<sup>63</sup>So far, the worker-level variation already in part reflected group-level treatment in specifications that do not control for granular firm-by-year or industry-by-year fixed effects. A large body of evidence on capacity of worker-level wage growth to reflect idiosyncratic shifts include subgroup productivity (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. (Abowd et al., 1999), the patterns of which also hold Austria (Borovičková and Shimer (2017)), show that workers’ wage premia are carried across employers, implying that Austrian firms can differentiate wages within the firm according to idiosyncratic factors.

<sup>64</sup>In Section 2 we have shown that the mere presence of multi-worker firms on their own (Stole and Zwiebel (1996) and Brügemann et al. (2018)) does not change the pass-through of outside options into wages.

<sup>65</sup>Chetty et al. (2011) documents intensive margin adjustment of workers to tax incentives to be mediated by the fraction of affected workers. Saez et al. (2017) 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.

<sup>66</sup>Before calculating firm-level average benefit changes, we winsorize individual workers’ benefit changes at the 1st and 99th percentiles.

a reform-induced non-zero benefit change. Specifically, we include fixed effects for percentiles of each of these variables in the pre-reform year and let these vary between reforms but not across years within a given reform.

Figure 17 and Table 4 report estimates based on firm-level variation in outside options and reveal an only slightly larger benefit sensitivity of wages at the firm level. The point estimates for the pass-through of average benefit changes at the firm level into wages ranges from 0.013 to 0.035 at the one-year horizon and from 0.023 to 0.035 at the two-year horizon. Throughout, we find that standard errors are slightly larger than in the individual-level specifications and the confidence interval for the firm-level treatment effect includes the point estimate for the individual-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 reform-induced benefit shifts, we find that pre-trends are flat and point estimates for the pass-through of average benefit changes at the firm level into wages remain between 0.033 and 0.035. Overall, the evidence at the firm-level is hard to square quantitatively with bargaining at the firm-level in which the value of nonemployment determines workers' outside options, although our results suggest that slightly larger pass-through may occur at the firm level, consistent with some degree of wage compression within the firm.

**Collective bargaining.** Despite substantial scope of worker-firm wage bargaining, Austria is a heavily unionized country that includes wage *floors* being set at the industry level. As we discuss in Section 3.4, these wage floors often do not bind as firms are free to 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.<sup>67</sup> 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 actually received wages using the micro data from the social security records.

### 5.3 Sensitivity of the Wage to Outside Option $dw/d\Omega$

Perhaps our variation does induce shifts in the nonemployment value  $N$ , and indeed those would shift the outside option  $\Omega$  as in the model, yet real-world wage setting is insensitive to outside

<sup>67</sup>Austrian CBAs typically either specify across-the-board nominal growth of the wage floors, or specify explicitly group-specific wage floors that ultimately grow in lockstep. These mandates for typically homogeneous growth in the aggregate wage floors often require these mandates to be extended to actually paid wages (where firms are free to raise wages idiosyncratically above these floors).

options. Below we explore this direction.

### 5.3.1 Demographic Heterogeneity Proxying for Bargaining Power

In the bargaining framework in Section 2, the pass-through of nonemployment shifts into wages is guided by the factor  $1 - \phi$ , one minus the worker bargaining power. 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**, since we are particularly interested in demographics associated with *lower* bargaining power, which should correlate with *larger* pass-through. We allow for different effects for male and female workers, 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. If nonemployment is the relevant outside option in bargaining, then female workers' wages should exhibit stronger wage comovement with unemployment benefit shifts. Figures 14 and 15 show that 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.

### 5.3.2 Wage Adjustment Frictions

Perhaps Nash bargaining with nonemployment as the outside option is an empirically relevant wage setting protocol, yet our empirical design fails to detect wage responses due to frictions in wage adjustments. Wage stickiness in continuing employment relationships would then slow down adjustment and mask pass-through. While this could account for an insensitivity of wages to nonemployment value shifts, we have already found several pieces of evidence that reject wage stickiness as an explanation for our overall findings. First, we estimate wage effects over longer horizons and find only small wage effects 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.<sup>68</sup> Second, we have also found only small wage effects for workers in firms with more flexible wage policies as proxied for by dispersion in wage levels relative to the CBA level or intrafirm differentiation in wage growth. Third, we found no evidence for nonlinear effects such that even large shocks in the nonemployment value did not entail noticeable wage effects.<sup>69</sup>

<sup>68</sup>In any case, existing estimates of wage stickiness imply that more than half of wage contracts should reset each year (see, e.g., Barattieri et al. (2014) for the United States, and Sigurdsson and Sigurdardottir (2016)) or that incumbent worker's wages are half as sensitive to aggregate shocks as new hires' wage contracts (see, e.g., Pissarides, 2009). Assuming that half of the wage contracts remain perfectly rigid but half of the wages adjust fully, would only moderately widen the lower bound of bargaining power. To add, in a cross-country study Dickens et al. (2007) find that Austrian wages exhibit lower downward nominal wage rigidity compared to Germany or the United States.

<sup>69</sup>This analysis is motivated by the idea that adjustment cost constitute a friction that may censor small wage changes, while permitting pass-through of larger shocks. See Chetty (2012) for a discussion of this mechanism in the context of labor supply elasticities.

Finally, we have additionally measured wage effects in *new* jobs in Section 4.4 and found no evidence for higher wage pass-through among workers that switched jobs, i.e. where stickiness would not constrain wage setting in the new job. We thus conclude that wage stickiness cannot account for our finding of wage insensitivity to nonemployment value changes.

While Austria is indeed heavily unionized, it leaves substantial room for idiosyncratic deviation from the collective wage floors: actual wages are about a third higher than the wage floors set by central bargaining agreements (Leoni and Pollan, 2011). And, our latest reform, 2001, occurred in a time period with substantially more scope for firm-level or idiosyncratic negotiations. To assess whether wage adjustment frictions may be driving our results, we estimate the treatment effects across a number of measures of firm-level wage flexibility and other firm measures.

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. Finally, we calculate a measure to proxy for **distance from CBA-level wage setting**. Firms will be scored higher on this metric if they pay wages that differ from the CBA-level average.

Finally, we also split **firms by size** (employment count), since survey data show that wage bargaining is more prevalent in smaller firms. We separate workers into four groups based on their firms' size: (i) less than 10 workers, (ii) 11 to 100 workers, (iii) 101 and to 1,000 workers, and (iv) larger than 1,000 workers.

Across all of these measures, we do not find any consistent trends that the treatment effects are larger at firms with more flexible wage setting. All of the point estimates are well below the 0.39 theoretical benchmark and generally insignificant. These results indicate that firm-level wage rigidity does not seem to be driving our main result of small wage-benefit pass-through.

### 5.3.3 The Prevalence of Wage Bargaining

One potential rationalization of the insensitivity of wages to the nonemployment value is that wage bargaining may not determine real-world wage setting for the typical worker in any pocket

of the Austrian labor market – while nonemployment would be the relevant outside option in principle, *were* bargaining to occur. 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 prevalence of wage bargaining in those surveys are associated with larger sensitivity of wages to our variation.<sup>70</sup> However, we do not find larger pass-through for firms and workers where survey evidence suggests a high bargaining prevalence, 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. In fact, reading our long battery of heterogeneity analyses as a whole reveals that the insensitivity extends across diverse worker and firm subgroups, states of the aggregate or local business cycles, making it unlikely that any pocket of the market that does bargain does so with nonemployment as the worker outside option.

## 6 Implications for Models of Wage Determination

The insensitivity of wages to the nonemployment value we document 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 2.3. Here, we discuss alternative bargaining and non-bargaining models of wage setting that may account for our findings. We then discuss how to reconcile the insensitivity of wages to UI with existing evidence on wage sensitivity to labor market conditions and conclude with a discussion of external validity and limitations of our findings.

### 6.1 Candidates Among Alternative Bargaining Models

The inability of Nash bargaining with nonemployment outside options to account for the evidence we present leads to the natural question of whether alternative bargaining models are consistent

<sup>70</sup>According to those surveys, wage bargaining is more likely for the following job characteristics (for which we have constructed empirical proxies listed in brackets): small firm size (establishment employment count), higher worker age (time since birth date), higher education (white collar), more specialized jobs (experience and tenure; white collar), more time since 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 demographics (females are less likely to state they bargain).



with our evidence. Here, we discuss several plausible alternative bargaining protocols that fare better in light of the evidence we present, including models of credible bargaining (Hall and Milgrom, 2008) and employer competition and on-the-job search (see, e.g., Cahuc et al., 2006). These models are consistent with the broad insensitivity of wages to nonemployment value shifts; but we also discuss that we find less support for more nuanced predictions of some of these models.

**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 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.<sup>71</sup> However, the 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 (see, e.g., MacLeod and Malcomson, 1993) have wages set entirely in advance of the first match, and then *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 at all times, i.e. a wage can be found to fulfill both parties' participation constraint). This is equivalent to the wage falling beneath the worker's reservation wage or above the firm's reservation wage. These models would be consistent with an attenuated effect of nonemployment shifts on wages. In contrast to predictions from this class of models, however, we do not find evidence that larger, reform-induced nonemployment value increases lead to large wage increases. Moreover, we also did not find large wage effects among workers for whom the model would predict initial wages to be close to the worker reservation wage, such as worker with proxies for low bargaining power or marginally attached workers with high unemployment risk. We also show 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

<sup>71</sup>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).

more sensitive to nonemployment shifts. Finally, the empirical literature on rent sharing has managed to detect even small wage effects from idiosyncratic productivity shocks, implying that complete absence of rebargaining does not provide a full description of the typical employment relationship.

**On-the-job search, employer competition, and negotiation capital (see, e.g., Cahuc et al., 2006).** An alternative class of bargaining models features *employed* workers that search on the job to move up a job ladder of firms with heterogeneous productivity. Several models in this class are consistent with our main result of insensitivity of wages to nonemployment value shifts. 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.

In models with on-the-job search and employer competition that can account for our main finding, already-employed workers can use their current employer’s wage as the (dominated) outside option when bargaining with the new potential employer, or dominated external offers as outside options when negotiating with their current employer, as, e.g., in Cahuc et al. (2006), Altonji et al. (2013), or Bagger et al. (2014). External offers (whether they lead to job transitions or not) can thereby insulate incumbent workers from the noneemployment value by swapping it with better outside options, as well as boost wages. Conlon et al. (2018) and Caldwell and Harmon (2018) provide evidence in support of the idea that information about job opportunities at other employers raises earnings and job mobility, 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 find less support for more nuanced predictions from models of employer competition and on-the-job search that predict larger effects from workers with recent unemployment. Absent alternative external offers, unemployment remains the threat point of unemployed workers – and even an employed worker *until* she receives an outside option that is more attractive than unemployment. For these workers, wages follow the standard Nash wage, and inherit the sensitivity of wages to the nonemployment option of our baseline model (where all workers’ outside option is nonemployment).<sup>72</sup> While we found some evidence consistent with a slightly higher sensitivity for workers with shorter time since nonemployment, effect sizes remain small.

<sup>72</sup>Unemployed job seekers and incumbent job seekers still without suitable outside options receive the standard Nash wage with unemployment as the outside option, generating the standard Nash wage:

$$E(w) = (1 - \phi) \cdot N(b_i) + \phi \cdot (E(w) + J(x_f, w)) \quad (52)$$

where  $x_f$  is the match- or firm-specific productivity. An employed worker having received outside offer  $x_{f'}$  dominating unemployment yet dominated by the current job ( $E(w) + J(x_{f'}, w) - U(b) > W(w) + J(x_f, w) - U(b) >$

This insensitivity is harder to square with models of employer competition and on-the-job search unless one is willing to assume that workers freshly hired out of unemployment receive alternative wage offers fast that will insulate them from nonemployment value shifts.

## 6.2 Wage Setting Models Beyond Bargaining

**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. In a particular specification (when employed and unemployed workers have the same job offer arrival rate), the sensitivity of the mean posted wage to UI benefits is small (roughly equal to the unemployment rate). The prospect of meeting unemployed job seekers whom they meet with probability equal to the unemployment rate and whose reservation value of a job is the nonemployment value, makes wage policies sensitive to components such as  $b$ . The equilibrium distribution of wages follows a mixed strategy of firms' wage policies that spans the interval between the nonemployment value and labor productivity. Shifts in  $b$  therefore shift the entire distribution of wages to the right.<sup>73</sup> However, the real-world earnings dispersion our designs exploit are likely driven by some persistent differences between workers or firms.<sup>74</sup>

Our heterogeneity results in Section 5.1.1 have not found clear support for the prediction that posted wages should respond more to  $b$  if the pool of searchers' contains more unemployed workers, for example among firms usually hiring from the unemployment pool, in areas with high unemployment, or for individual workers more prone to unemployment risk.

**Frictionless labor market adjustment.** We have focused on wage setting protocols in frictional labor markets, in part because those models motivated our investigation about the role of outside options and because they are promising candidates capable of explaining phenomena  $U(b)$  renegotiates the current wage with that external job offer as the outside option:

$$E(w) = (1 - \phi) \cdot [E(w) - E(w_{f'})] + \phi \cdot (E(w) + J(x_f, w)) \quad (53)$$

<sup>73</sup>The distribution of posted wages collapses to the nonemployment value when arrival rate of jobs falls; it moves towards labor productivity (while still having positive support at the nonemployment value) as job arrivals accelerate, i.e. search frictions fall.

<sup>74</sup>Richer wage posting models feature heterogeneity in firm-specific productivity of ex-ante identical workers, or workers heterogeneous in their idiosyncratic valuation of nonemployment, or a combination of both. In those richer wage posting models, wage dispersion partially tracks firm productivity, for example. Yet, the entire wage distribution still scales with the nonemployment value, which therefore remains the cornerstone of the wage distribution even if the lowest firm's productivity value strictly exceeds the nonemployment value.

such as firm-level rent sharing and cross-sectional wage dispersion among similarly skilled workers. 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 is 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.

### 6.3 Related Evidence on Wages and Outside Options

**Reconciling the insensitivity of wages to UI with existing evidence on wage responses to labor market conditions.** As a reduced-form fact, our finding of an insensitivity of wages to the nonemployment outside option appears to contrast with a substantial body of evidence that the external (local) labor market – typically proxied for by the unemployment rate – appears to have contemporaneous as well as long-lasting effects on workers’ wage levels. A leading interpretation of the empirical regularity of the “wage curve (Blanchflower and Oswald, 1994) is the bargaining channel, since high unemployment weakens workers’ threat point by making nonemployment less valuable. Winter-Ebmer (1996) confirms the wage curve relationship for the case of Austria. However, the reduced-form pattern may reflect a variety of mechanisms (see Card, 1995, for a discussion). Similarly, an influential finding in Beaudry and DiNardo (1991) is that wages in existing jobs evolve with the history of the external labor market, where high initial unemployment entails persistently lower wages due to the weak outside option of the worker; tight labor markets over the course of the job allow the worker to renegotiate upwards due to the improved outside option. However, an ongoing debate studies whether these patterns may instead be explained by compositional effect such as match quality selection due to procyclical job ladders (Hagedorn and Manovskii, 2013; Gertler et al., 2016).

**Empirical findings on wage effects of UI.** To our knowledge, ours is the first study focused on evaluating wage effects of nonemployment value shifts in a calibrated bargaining model. Our variation in the nonemployment value arises from UI benefits. Other dimensions of UI generosity have received attention (yet not been studied in a wage bargaining framework). Hagedorn et al. (2013) study the effect of potential benefit duration (PBD) extensions in the United States on unemployment, focusing on hiring effects of the wage pressure channel. They also include a reduced-form investigation of equilibrium (macro) wage effects among stayers.<sup>75</sup> By contrast, we

<sup>75</sup>Pricing PBD shifts is naturally challenging (compared to the variation in dollar-valued benefit level changes, which are the focus of our paper). Additionally, PBD may very heterogeneous effects such as only for workers expecting to exhaust the previous limit. This effect heterogeneity is documented in Nekoei and Weber (2017) for unemployed job seekers’ unemployment duration and in Jäger et al. (2018) for employed workers’ separation

focus on micro effects and provide a calibrated benchmark to quantitatively evaluate our effects, thanks to variation in benefit levels (which is easier to price). We also intentionally select a particular setting where take-up is very high and quitters are eligible for UI, whereas, e.g., in the United States quitters are de-jure ineligible for UI which would further reduce wage effects.<sup>76</sup>

A sizable literature has studied moral hazard effects of UI on unemployment duration and re-employment wages, chiefly through a reservation wage channel, which would predict wage increases through job selection channels. For potential benefit duration rather than benefit levels, Schmieder et al. (2016) and Nekoei and Weber (2017) find slightly negative or small positive wage effects among already unemployed workers, in contrast to the canonical reservation wage and selectivity channel. In these studies focusing on the search behavior of the unemployed, wage effects however could be masked by skill depreciation or statistical discrimination (see, e.g., Kroft et al., 2013; Nekoei and Weber, 2017; Kroft et al., 2016). At unemployment entry, workers' stated reservation wages appear unresponsive to potential benefit duration among unemployed job seekers in France (Le Barbanchon et al., 2017). By contrast, Feldstein and Poterba (1984) present survey evidence of unemployed job seekers, and find that stated reservation wages increase by 40 cents on the dollar in UI benefits for job losers (and less for other separators such as quitters), while Krueger and Mueller (2016) find estimates close to zero.

## 6.4 External Validity and Policy Implications

We close with a discussion of potential limitations arising from our UI-based variation as well as with implications for policy debates.

**External validity to UI in other contexts.** We next highlight some specific features of our specific context and the source of variation in UI benefits. First, the variation we exploit arises from UI benefit levels. We chose Austria for our research design because UI appears to be a component of the nonemployment scenario for most workers, due to long unemployment durations and institutional incentives to immediately register with the UI agency for health insurance continuity. Moreover, eligibility is broad for separators, even voluntary quitters are eligible in Austria; due to absence of experience rating, firms furthermore face no incentives to limit separators' UI take-up. Austria additionally features a post-UI program substitute the level of which is almost one to one indexed to eligible worker's pre-exhaustion UI levels (*Notstandshilfe*). This contrasts with, e.g., the US context, where a smaller fraction of unemployed job seekers decisions.

<sup>76</sup>The degree to which UI in the United States affects bargaining depends greatly on the bargaining game and whether the outside option would result from a quit or a layoff, since quitters are de-jure ineligible for UI in the United States (but not in Austria). In an appendix, Hagedorn et al. (2013) show that quitters can enjoy de-facto eligibility for UI even in the United States. See also Chodorow-Reich and Karabarbounis (2016) and Chodorow-Reich et al. (2018) for a discussion of UI eligibility and take-up in the US context. In our setting, we find that the majority of nonemployment spells longer than 28 days lead to take-up of UI.

collect UI (see, e.g., Chodorow-Reich et al., 2018); although some features such as the absence of experience rating are prevalent in most OECD countries.

**Implications for the distortionary effects of UI on employment through the wage pressure channel.** An additional implication of our findings is that the wage pressure channel of UI is likely small and has limited job destruction and potentially creation effects. Our design therefore also provides new evidence for the mechanism assumed in recent work that focused on the U.S. UI extensions during the Great Recession (see, e.g., Hagedorn et al., 2013; Chodorow-Reich et al., 2018), which measure the reduced-form effects of potential benefit duration on unemployment rates. While we also document insensitivity of wages among workers hired from nonemployment or undergoing E-N-E transitions during the reform, our main design documents the lack of wage effects in existing jobs. Consistent with the limited wage effects, we found limited quantity effects on separations and time in unemployment in response to the reforms we study.<sup>77</sup> However, our difference-in-differences design has several limitations pertaining to macro effects, which we discuss below.

**Long-run and aggregate implications.** Our identification strategy uses panel variation in the nonemployment value brought about by UI reforms, comparing treatment and control groups. While we allow for two (and even three) year delays in the effect and thus medium-run horizons which may be suggestive for longer-run effects, our design cannot directly speak to whether longer-run pass-through on wages may occur, i.e. the effect relevant for comparing steady states differences between countries or worker groups. A similar caveat applies to inferring from our difference-in-differences design to aggregate shifts rather than cross-sectional variation in the nonemployment value.

## 7 Conclusion

We have studied the effects of the value of nonemployment on wages brought about by quasi-experimental variation in unemployment insurance generosity in Austria. Our evidence shows that wages appear nearly perfectly insulated from the value of nonemployment. Our findings have direct implications for several debates in macroeconomics and labor economics.

The first implication is that the empirical regularity of positive comovements between wages and labor market conditions, such as the Phillips curve and the wage curve, may arise from

<sup>77</sup>This evidence contrasts with an evaluation of the job destruction effects of a large PBD reform in Austria in 1989 documented in Jäger et al. (2018). This reform is excluded from our wage analysis because of potential for attrition and composition bias. We note that the reforms examined in the paper at hand treat all workers, whereas Jäger et al. (2018) focus on an UI reform that targeted older workers at the margin of retirement, with potentially larger Frisch elasticities.

economic mechanisms other than outside options in bargaining in form of nonemployment value variation.

Second, the insensitivity of wages to the nonemployment value contradicts the large sensitivity predicted by popular macroeconomic models of wage setting. The most prominent example is Nash bargaining specified with nonemployment as workers' outside option. Here, our findings imply that either the specification to nonemployment scenarios as the relevant outside option is at fault, or even deeper structural assumptions of Nash bargaining.

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. Our findings suggest that this wage pressure channel of UI on hiring may remain small, although our research design may not carry over to economy-wide increases in workers' outside options and moreover has taken a shorter-run view.

Fourth, the empirical insensitivity of wages to the nonemployment value, for which we provide identified microeconomic 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.<sup>78</sup> Our research design therefore supports alternative bargaining protocols that insulate wages from nonemployment values.<sup>79</sup>

<sup>78</sup>For example, Chodorow-Reich and Karabarbounis (2016) measure a procyclical instantaneous payoff from nonemployment for the U.S. economy, and discuss the consequences for labor market models with wage bargaining.

<sup>79</sup>Examples of such wage setting models are ad-hoc sticky wages (e.g., Shimer, 2004). Micro-founded alternative models include credible bargaining (Hall and Milgrom, 2008), which attenuates the role of the outside option in bargaining, or models with employer competition and on-the-job search (see, e.g., Cahuc et al., 2006), which may rationalize small sensitivity of wages for high-tenure incumbent workers. Our evidence on the wage insensitivity of even workers going through unemployment is however inconsistent with these models.

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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	.84 (.35)	.87 (.33)	.45 (.49)	.22 (.41)	.56 (.49)	.81 (.38)	.41 (.49)	.74 (.43)	.49 (.5)	.62 (.48)
Age	38.8 (8.6)	39.1 (8.5)	37.5 (8.7)	38.8 (8.3)	36.9 (8.6)	37.7 (8.3)	37.7 (8.2)	38.1 (8.0)	37.5 (8.5)	38.3 (8.2)
White Collar	.35 (.47)	.29 (.45)	.35 (.47)	.52 (.49)	.35 (.47)	.39 (.48)	.42 (.49)	.49 (.5)	.38 (.48)	.46 (.49)
Experience in last 25 Years	9.3 (6.2)	8.9 (5.9)	14.5 (6.2)	17.5 (6.5)	13.4 (6.1)	11.6 (6.0)	14.1 (6.5)	11.8 (6.2)	13.8 (6.4)	13.3 (6.9)
Tenure	2.3 (1.3)	2.3 (1.3)	6.3 (4.3)	8.1 (4.4)	6.0 (5.1)	5.0 (4.7)	6.5 (6.3)	4.9 (5.4)	6.1 (5.4)	5.8 (5.0)
Avg. Monthly Earnings	4,277 (349)	2,648 (691)	13,383 (1,243)	20,617 (2,100)	13,837 (1,142)	8,565 (2,135)	24,176 (2,082)	15,684 (3,049)	17,168 (6,407)	14,553 (6,007)
Observations in Base Year	100,328	102,089	412,055	411,817	318,229	320,121	558,295	561,278	1,388,907	1,395,305

*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 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	.016 (.017)	-.002 (.016)	.016 (.018)	.014 (.016)	.022 (.013)	.028 (.014)
Placebo: 2 Yr Lag	-.001 (.014)	-.014 (.015)	-.007 (.015)	-.009 (.015)	.018 (.014)	.014 (.014)
<b>Treatment Year</b>	-.004 (.016)	-.001 (.017)	-.019 (.015)	-.014 (.016)	.002 (.013)	0 (.013)
Base-Year Average	7.304	7.304	7.304	7.304	7.304	7.304
Pre-p F-test p-val	.532	.581	.413	.381	.245	.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 49. Standard errors based on two-way clustering at the individual and earnings percentile level are in parentheses. The null hypothesis of the F-test is that the coefficients of interest are all equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.

Table 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	-.007 (.021)	-.025 (.021)	-.001 (.025)	-.001 (.024)	-.003 (.022)	.007 (.022)
<b>Treatment Year</b>	-.007 (.031)	.007 (.031)	-.027 (.032)	-.022 (.03)	-.022 (.026)	-.027 (.026)
Base-Year Average	14.364	14.364	14.364	14.364	14.364	14.364
Pre-p F-test p-val	.752	.241	.966	.962	.878	.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**

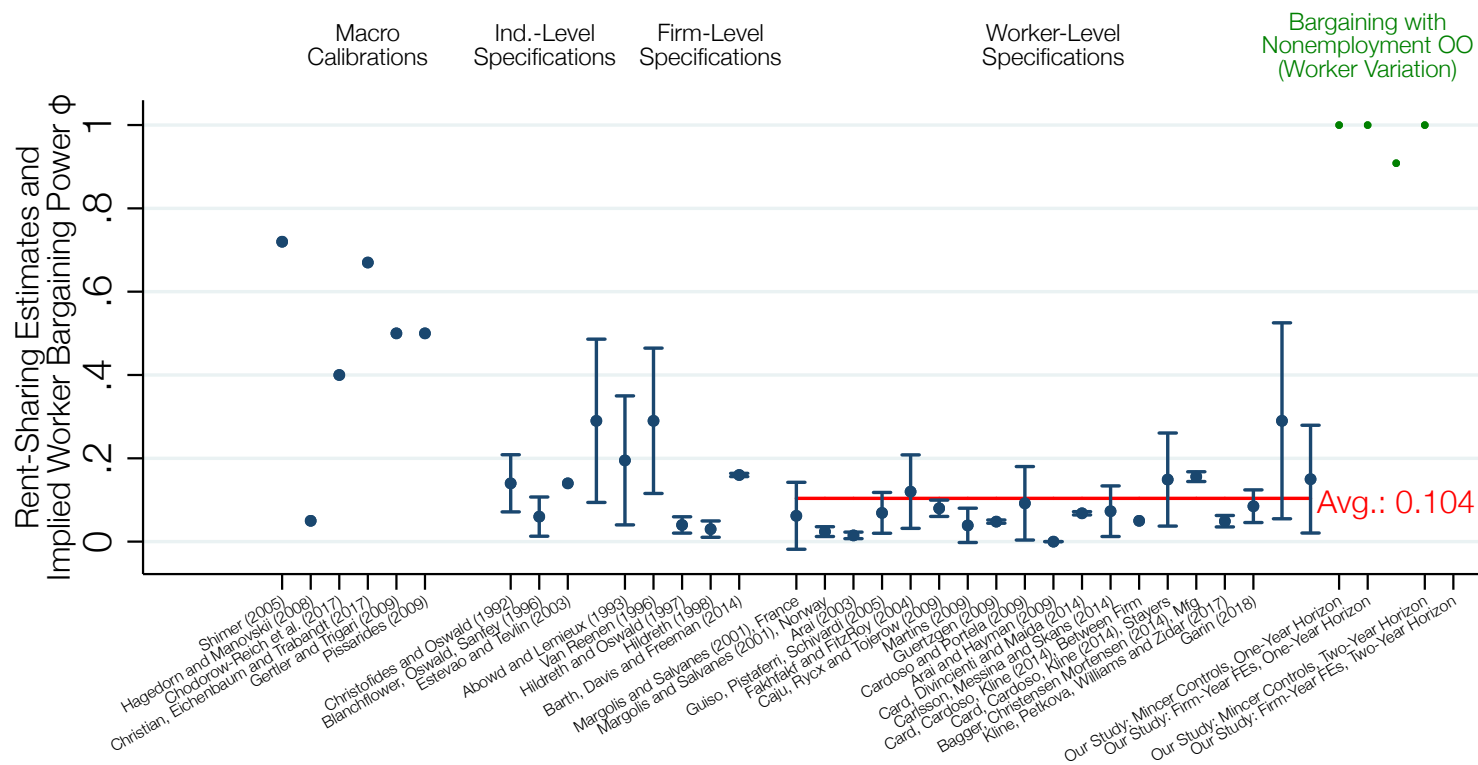
	1-Year Earnings Effects				2-Year Earnings Effects			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Placebo: 3 Yr Lag	-.09 (.026)	-.081 (.025)	-.021 (.027)	-.005 (.027)	-.094 (.034)	-.079 (.033)	-.005 (.037)	.022 (.037)
Placebo: 2 Yr Lag	-.056 (.023)	-.059 (.023)	-.01 (.026)	-.008 (.026)				
<b>Treatment Year</b>	.016 (.027)	.013 (.027)	.033 (.027)	.035 (.027)	.03 (.043)	.023 (.043)	.035 (.042)	.033 (.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	.002	.004	.732	.952	.005	.017	.888	.551
$R^2$	.055	.074	.079	.097	.111	.133	.142	.163
$N$ (1000s)	7139	7139	7138	7138	5038	5038	5038	5038
Mincerian Ctrl		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 49 with the variation in benefits aggregated at the firm-level. See Section 5.2.2 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 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

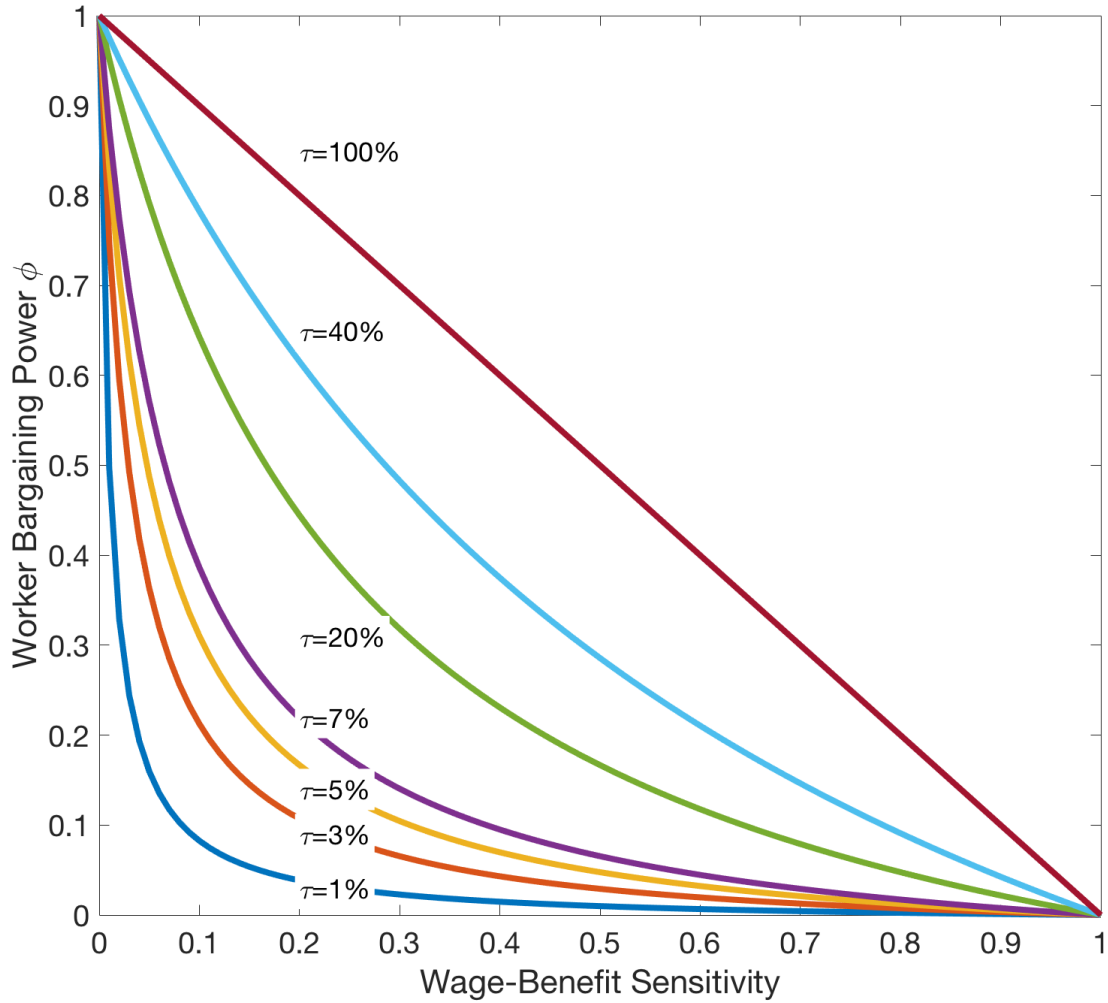


Figure 1: Overview of Estimates and Calibrations of Worker Bargaining Power



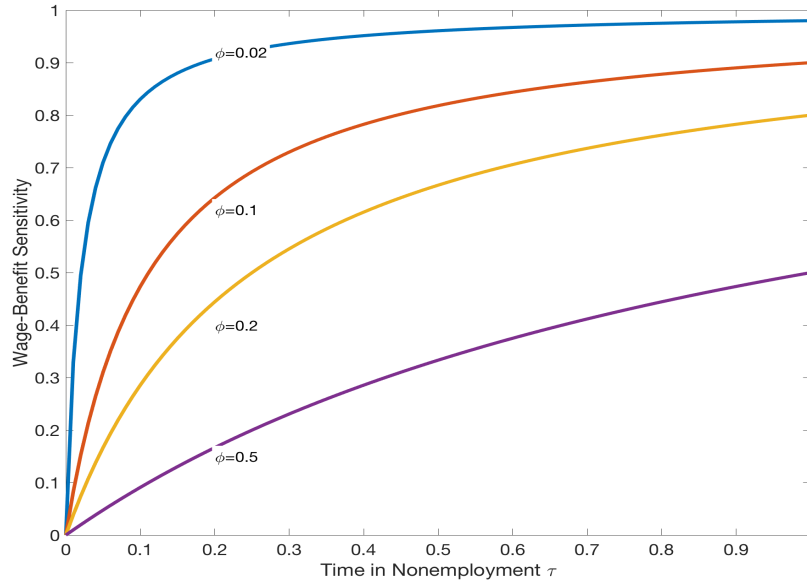
*Note:* The figure shows an overview of calibrations as well as implied estimates of worker bargaining power. For the calibrations, we plot the values used in the respective papers. For the estimates, we build on the meta-study in Card et al. (2018) and use level-on-level specifications from the papers included in the overview if those are reported. In addition, we add recent estimates from Kline et al. (2017) and Garin (2018). Some of the estimates surveyed in Card et al. (2018) and Garin (2018) are cast as elasticities and are thus upper bounds for the implied worker bargaining power when rent-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 11. Specifically, we plot the implied  $\phi$  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: Relationship between  $\phi$  and  $\frac{dw}{db}$  by Discounted Time in Nonemployment  $\tau$



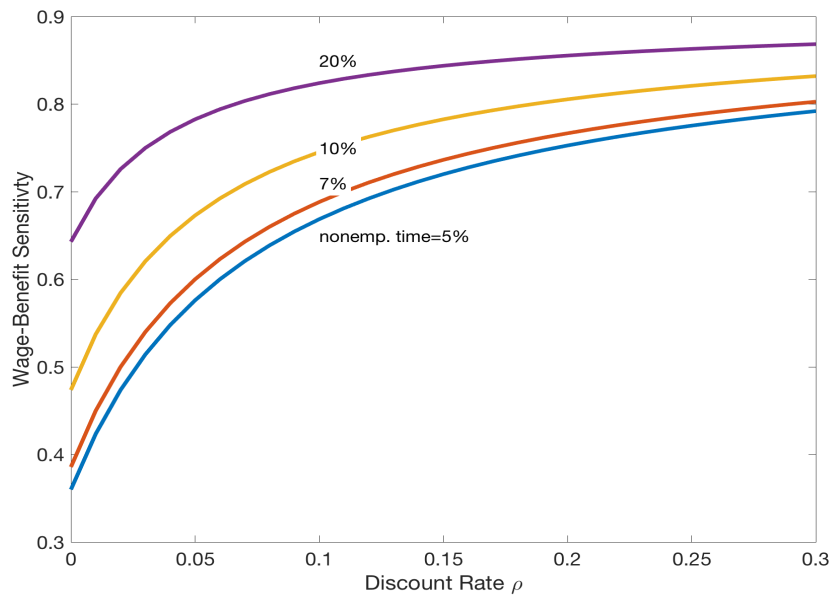
*Note:* The figure plots the relationship between the wage-benefit sensitivity  $\frac{dw}{db}$  and workers' bargaining power  $\phi$  as predicted by equation 10. We vary  $\tau$ , the discounted share of post-separation time spent in nonemployment conditional on a separation ( $\tau \in \{1\%, 3\%, 5\%, 7\%, 20\%, 40\%, 100\%\}$ ). For large values of  $\tau$ , the wage-benefit sensitivity is larger for a given value of  $\phi$  as nonemployment is a more important part of a worker's outside option. Even for our preferred estimate of  $\tau = 0.07$ , the small estimates of  $\phi$  around 0.1 implied by the micro rent-sharing estimates imply a wage-benefit sensitivity of 0.39.

Figure 3: Relationship between Wage-Benefit Sensitivity  $\frac{dw}{db}$  and  $\tau$  by Work Bargaining Power  $\phi$



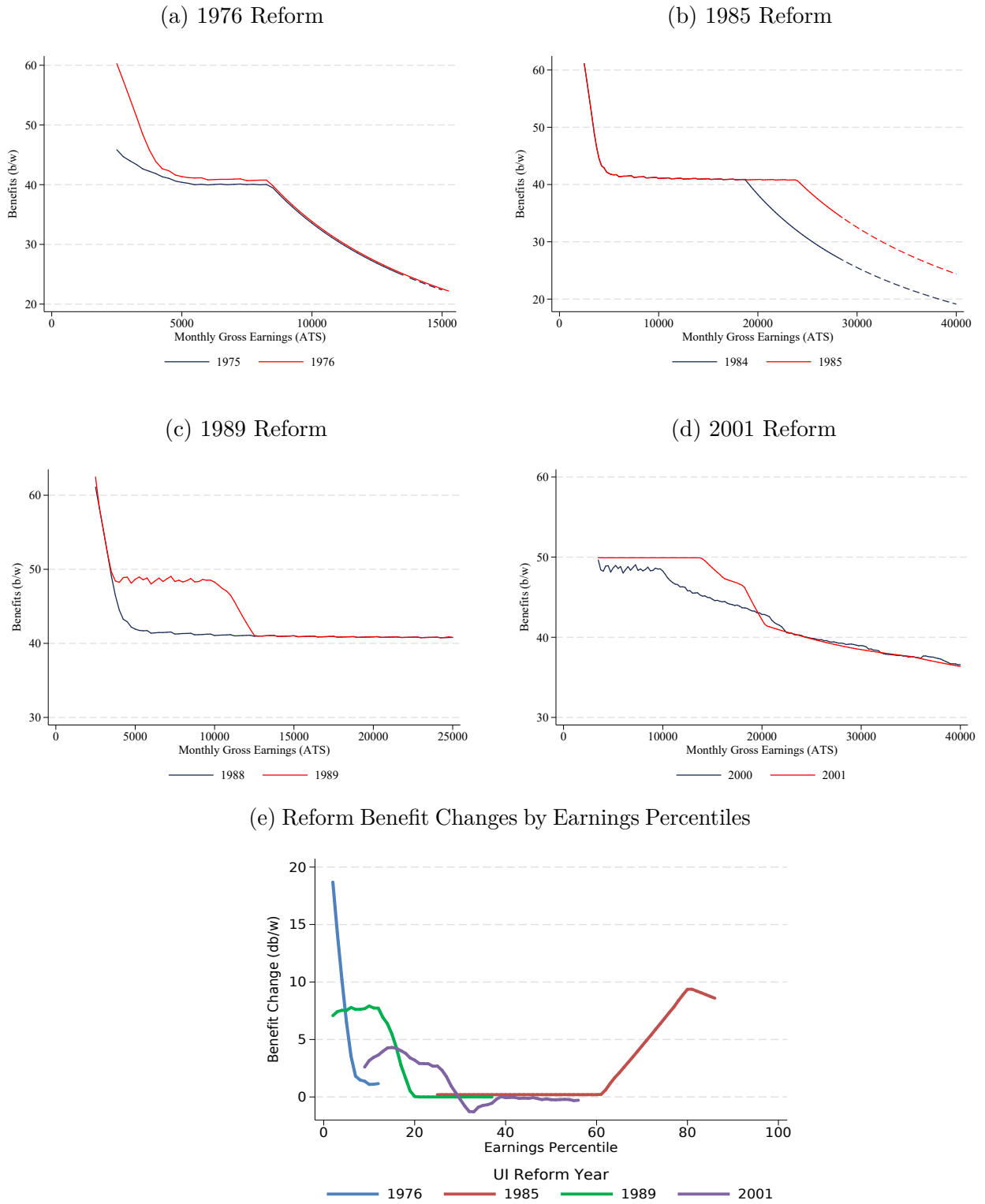
*Note:* The figure plots the relationship between the wage-benefit sensitivity  $\frac{dw}{db}$  and  $\tau$ , the discounted share of post-separation time spent in nonemployment conditional on a separation, as predicted by equation 9. We vary worker bargaining power  $\phi$  ( $\phi \in \{0.02, 0.1, 0.2, 0.5\}$ ).

Figure 4: Discount Rate and Wage-Benefit Sensitivity Relationship by Time Nonemployed



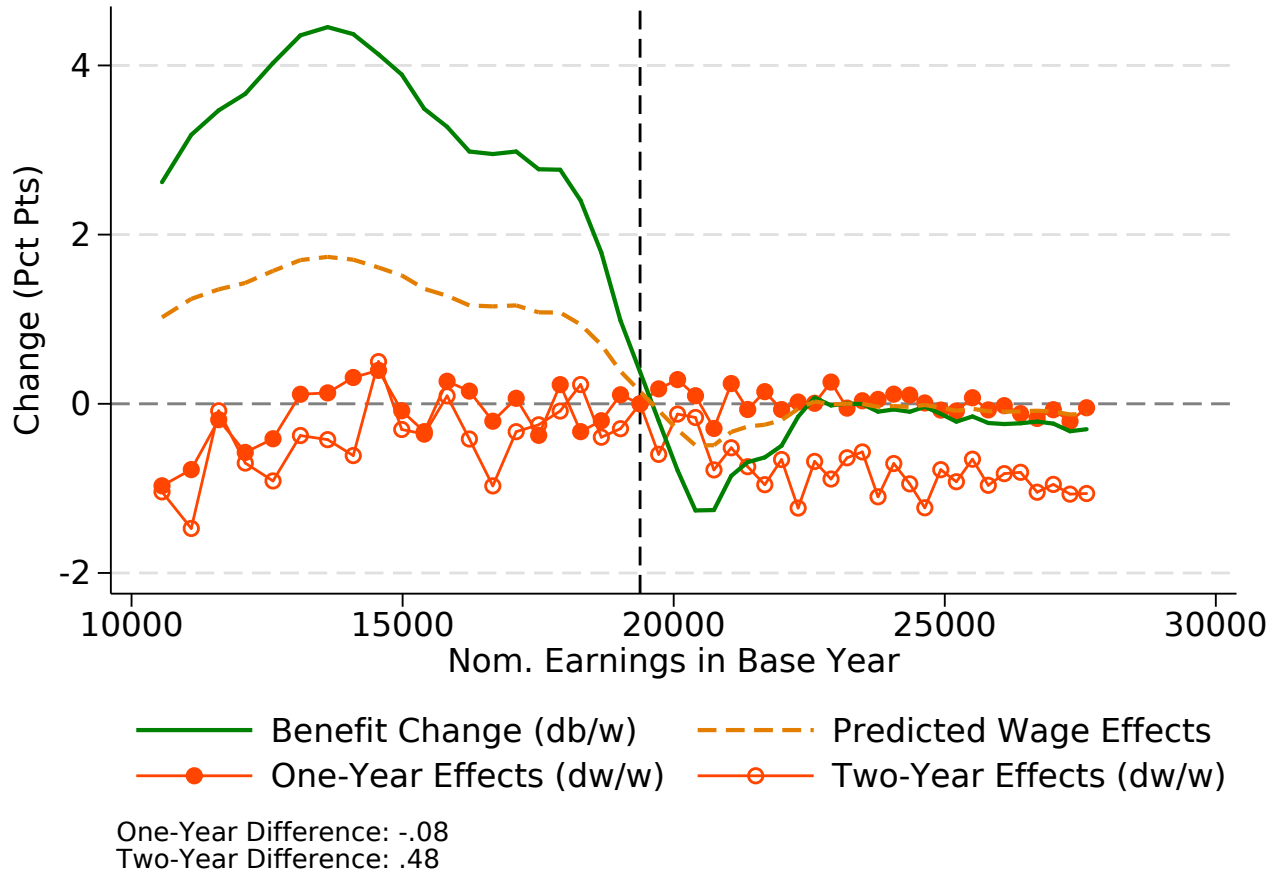
*Note:* The figure plots the relationship between the discount rate  $\rho$  and wage-benefit sensitivity. For a given  $\rho$  we calculate  $\tau$  by assuming a  $f$  and  $\delta$  combination that yields a *non-discounted* time spent in nonemployment of 5 %, 7 %, 10 %, and 20 %. We then calculate  $\tau$  from  $\rho$ ,  $f$ , and  $\delta$  and plug this into equation 9.

Figure 5: Unemployment Benefit Schedules and Reforms



*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 fraction of unemployment benefits divided by income. The dashed segments of the lines indicate incomes above the social security earnings maximum. Figure (e) plots the reform induced benefit change for each reform in earnings percentile space.

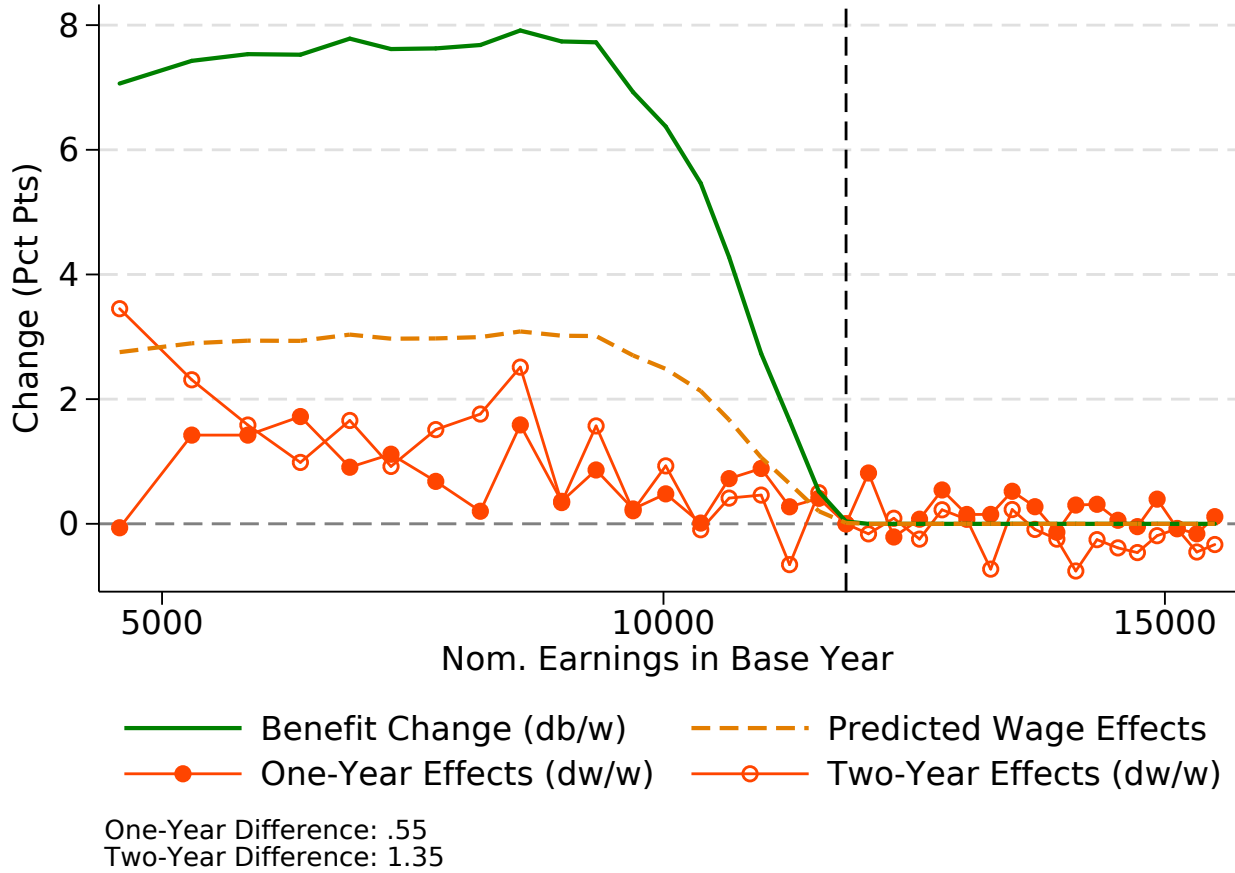
Figure 6: 2001 Reform: Benefit Changes and Wage Effects



*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 26th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 2001 reform would induce if nonemployment was the outside option and worker bargaining power was  $\phi = 0.2$ . The red squares and diamonds 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. As a summary measure, we calculate average treatment effects as the difference in wage effects between the treated and untreated segments of the earnings distribution, i.e. above and below the 26th percentile. Section 4.3 provides more information.

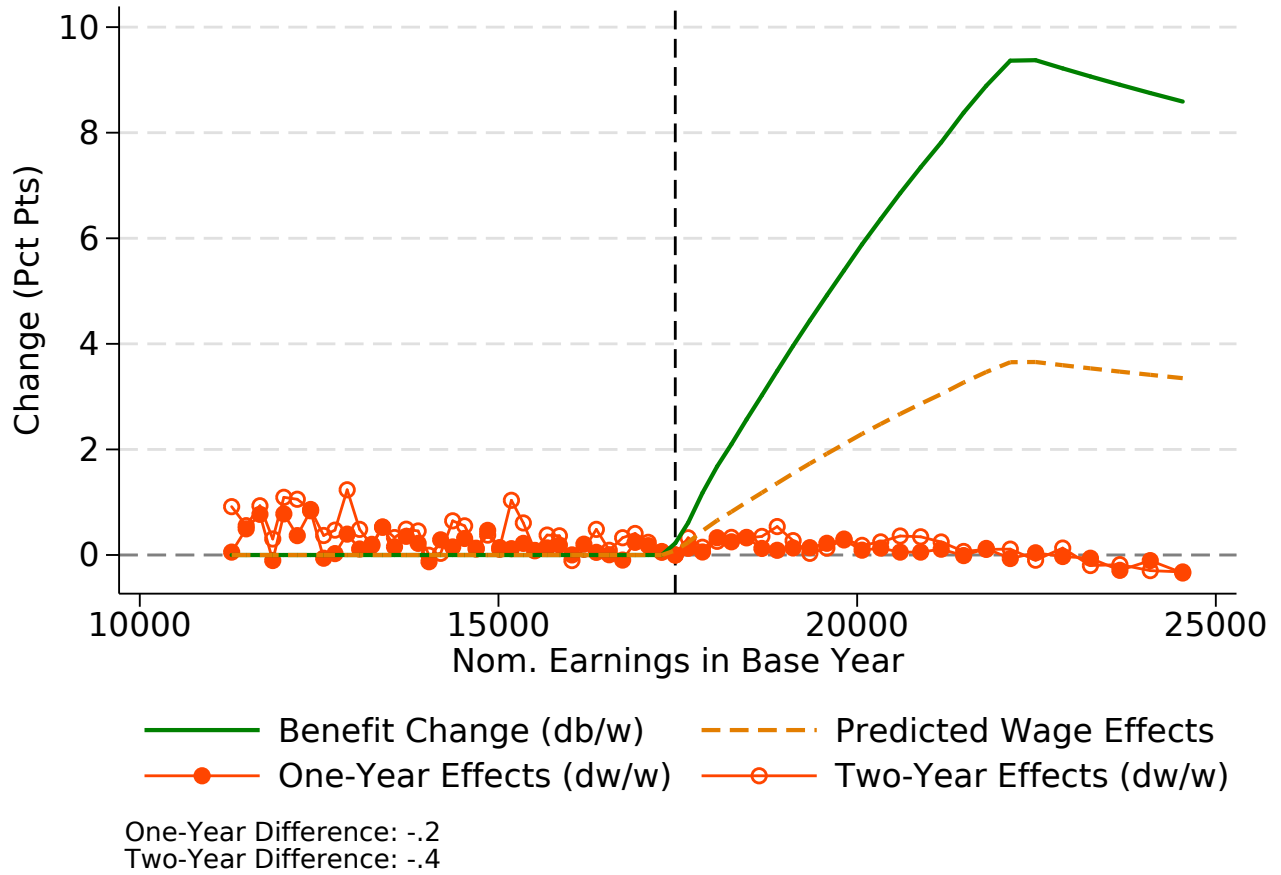


Figure 7: 1989 Reform: Benefit Changes and Wage Effects



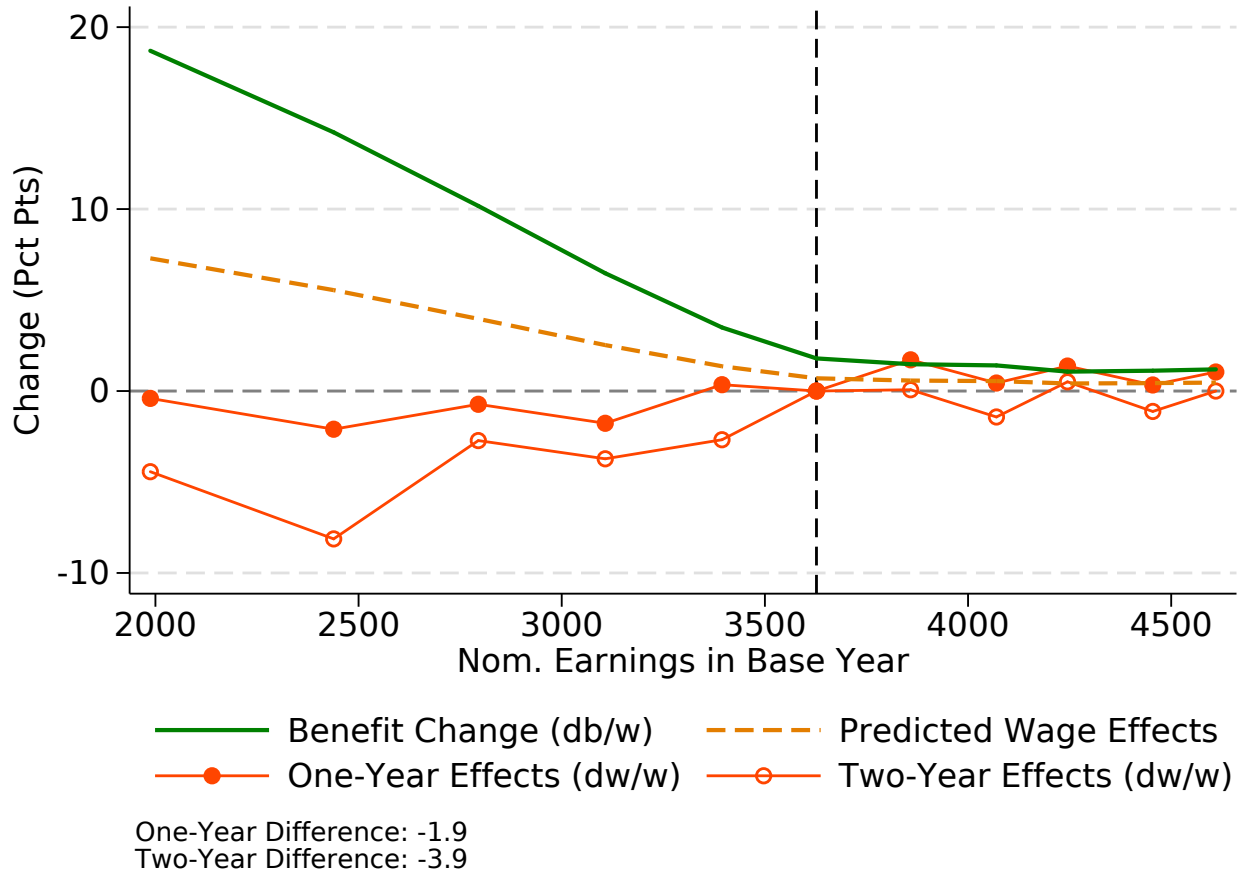
*Note:* The figure plots reform-induced replacement rate changes and wage effects for the 1989 reform. Observations are binned by their base year (1988) earnings percentile on the x-axis. The 1989 reform increased replacement rates below the 16th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1989 reform would induce if nonemployment was the outside option and worker bargaining power was  $\phi = 0.2$ . The red squares and diamonds 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. As a summary measure, we calculate average treatment effects as the difference in wage effects between the treated and untreated segments of the earnings distribution, i.e. above and below the 16th percentile. Section 4.3 provides more information.

Figure 8: 1985 Reform: Benefit Changes and Wage Effects



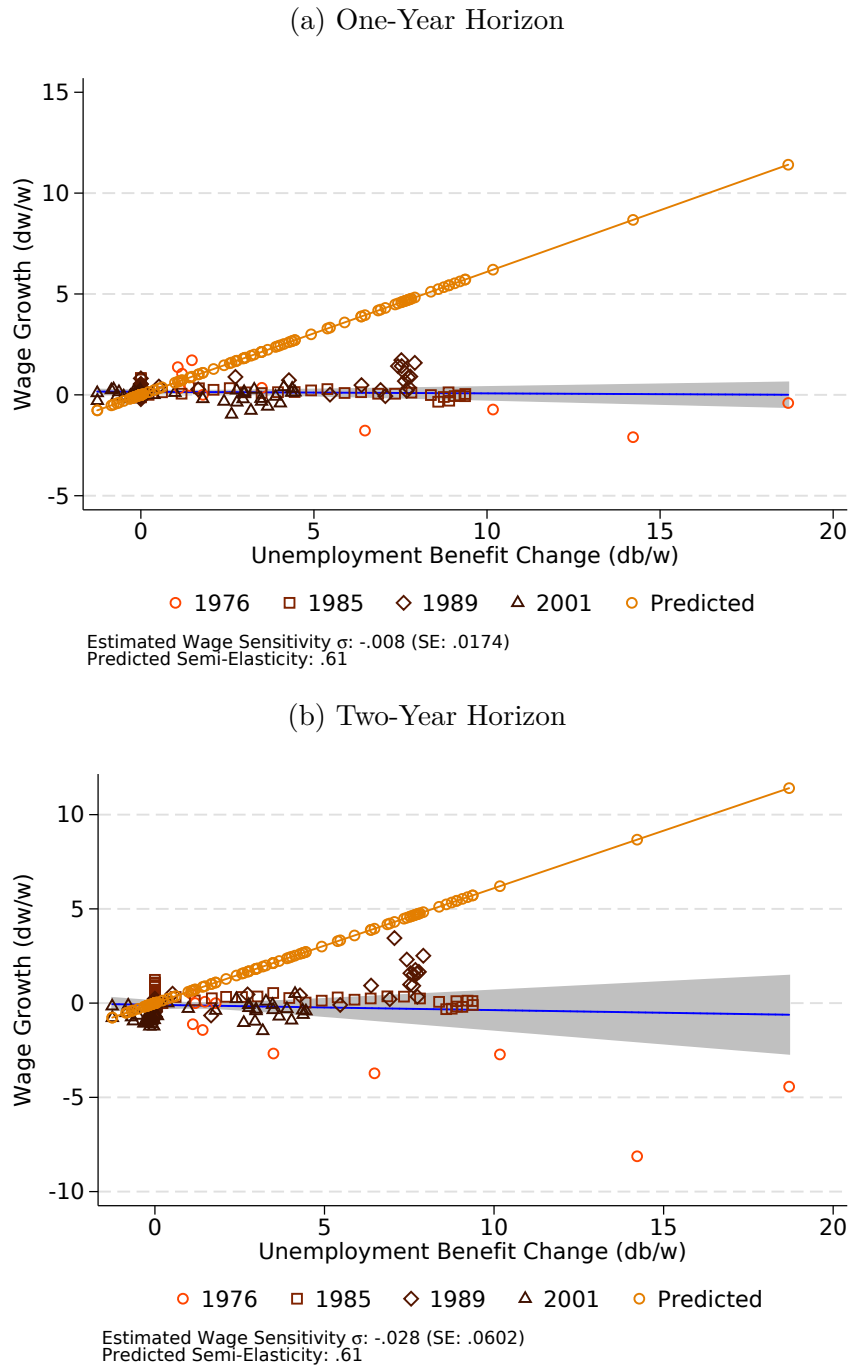
*Note:* The figure plots reform-induced replacement rate changes and wage effects for the 1985 reform. Observations are binned by their base (1984) year earnings percentile on the x-axis. The 1985 reform increased replacement rates above the 61st percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1985 reform would induce if nonemployment was the outside option and worker bargaining power was  $\phi = 0.2$ . The red squares and diamonds 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. As a summary measure, we calculate average treatment effects as the difference in wage effects between the treated and untreated segments of the earnings distribution, i.e. above and below the 61st percentile. Section 4.3 provides more information.

Figure 9: 1976 Reform: Benefit Changes and Wage Effects



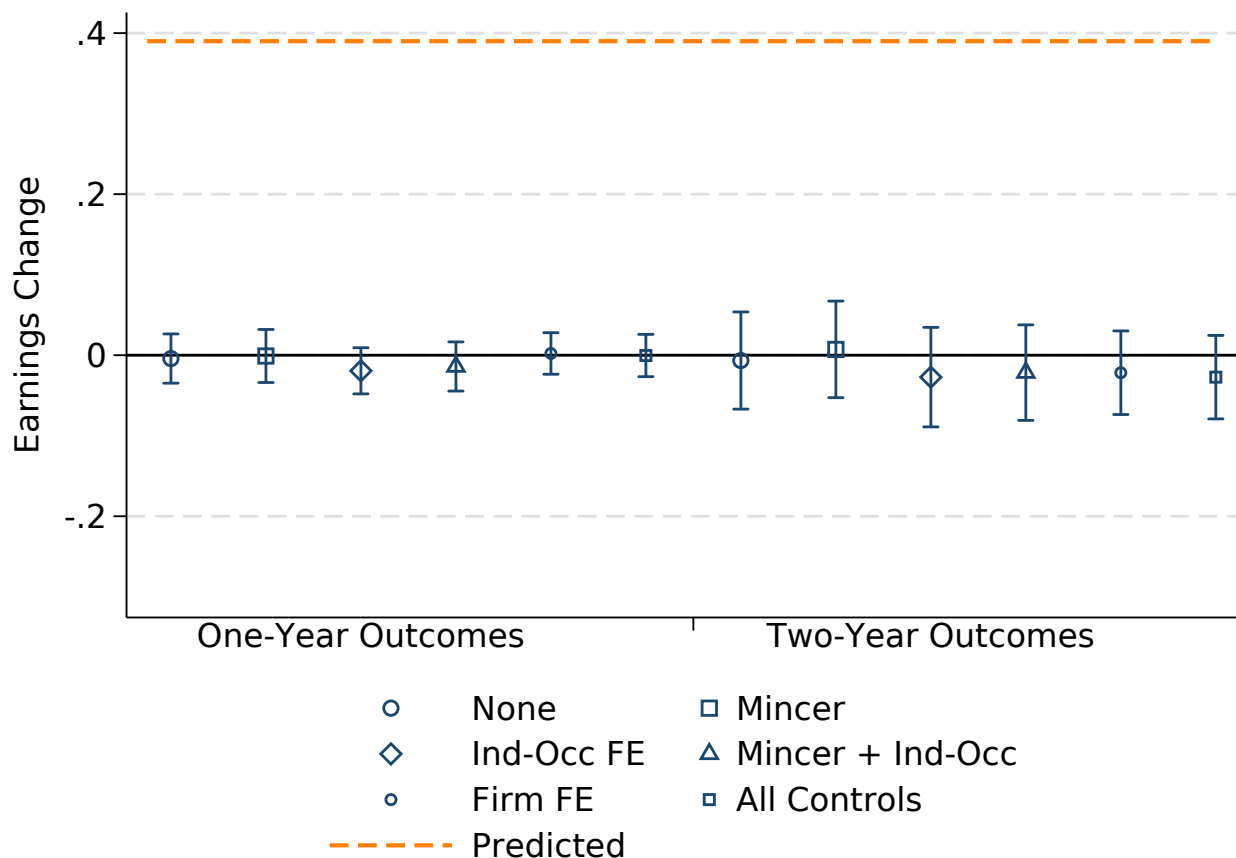
*Note:* The figure plots reform-induced replacement rate changes and wage effects for the 1976 reform. Observations are binned by their base year (1975) earnings percentile on the x-axis. The 1976 reform increased replacement rates below the 6th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1976 reform would induce if nonemployment was the outside option and worker bargaining power was  $\phi = 0.2$ . The red squares and diamonds 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. As a summary measure, we calculate average treatment effects as the difference in wage effects between the treated and untreated segments of the earnings distribution, i.e. above and below the 6th percentile. Section 4.3 provides more information.

Figure 10: Scatter Plots of Wage Growth and Unemployment Benefit Changes



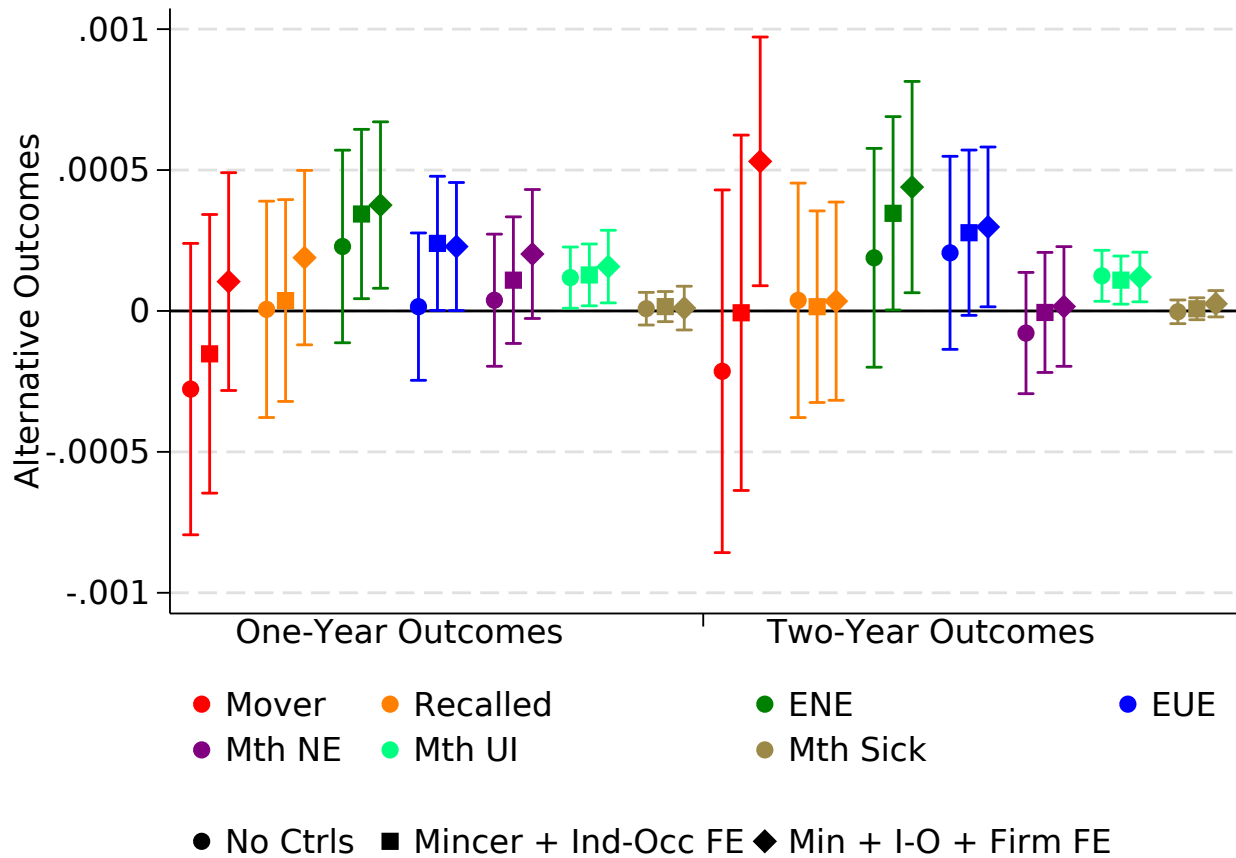
*Note:* The figures show scatter plots of wage growth (y-axis) and reform-induced replacement rate changes (x-axis),  $db/w$ , pooling the four reforms outlined in Figures 6 through 9. Each dot corresponds to a percentile observation from one of the 6 through 9. The upper panel shows wage effects after one year and the lower panel effects after two years. The orange triangles indicate the wage growth that the reforms would have induced if nonemployment was the outside option and worker bargaining power was  $\phi = 0.1$ . The red circles indicate the wage effects that the reforms actually induced. The estimated wage sensitivities  $\hat{\sigma}$  are calculated as the slope of wage growth with respect to changes in the nonemployment option.

Figure 11: Wage Effects: Difference-in-Differences Regression Design



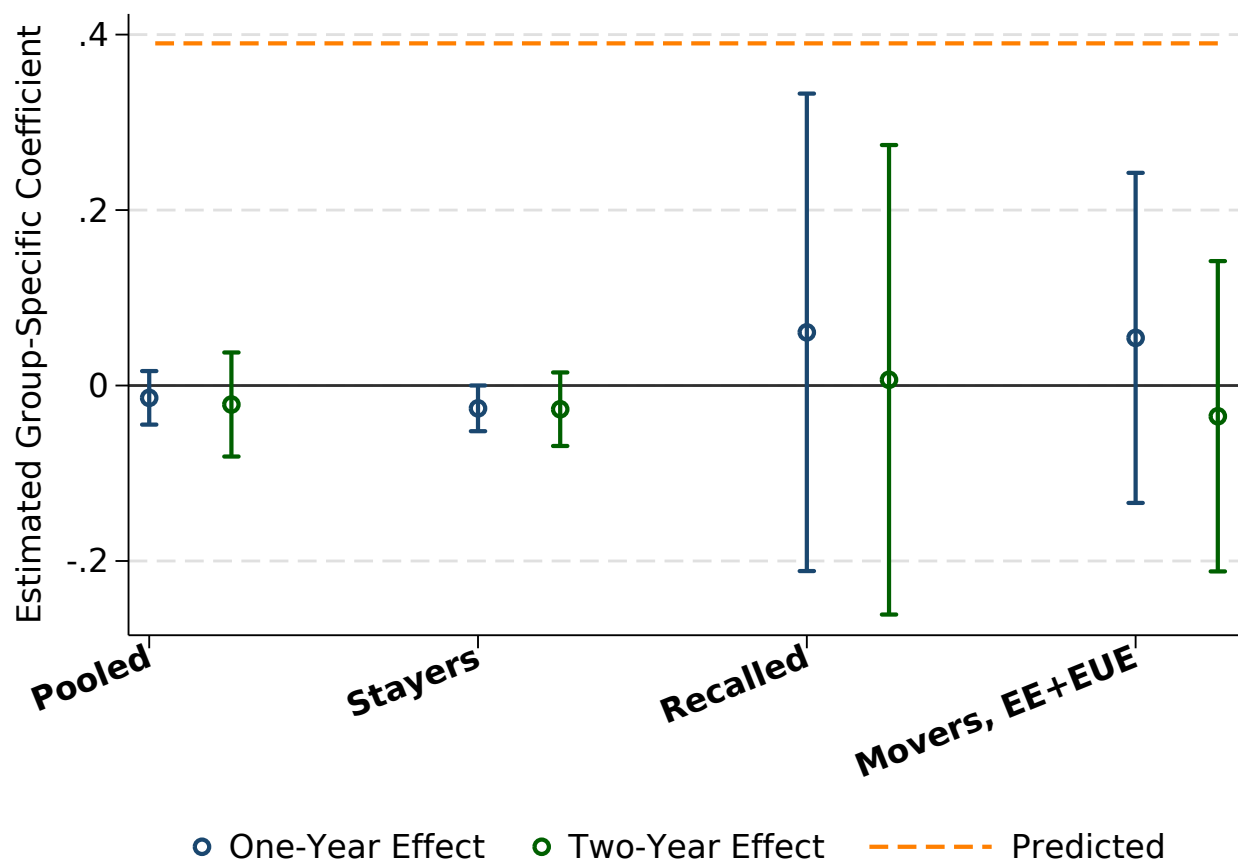
*Note:* The figure shows the effects of nonemployment value shifts,  $db/w$ , on wages based on the difference-in-differences specification in 49. It plots the estimated  $\delta_0$  coefficients and associated confidence intervals as reported in Tables 2 and 3. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The Mincerian controls include time-varying polynomials of experience, tenure, and age; 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. 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 12: Employment and Separation Effects: Difference-in-Differences Regression Design



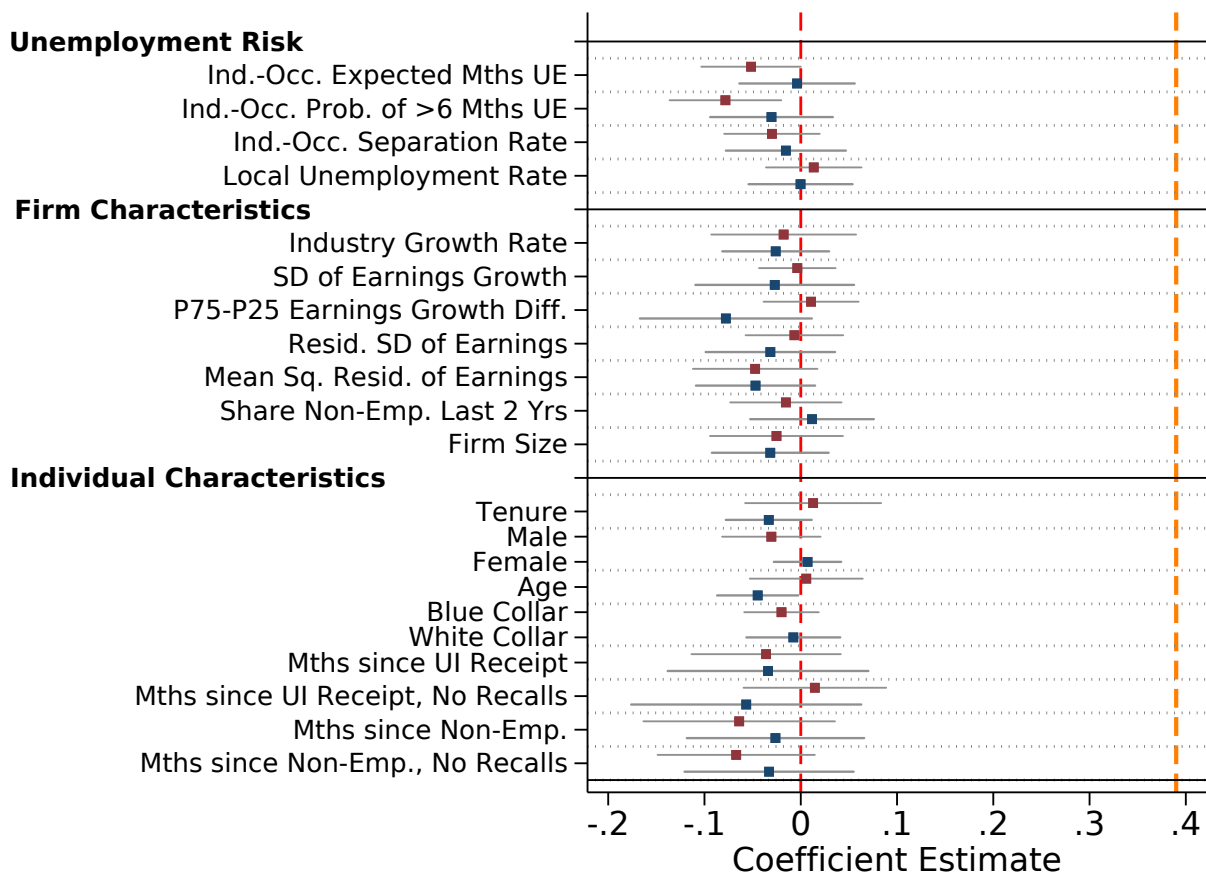
*Note:* The figure shows  $\delta_0$  coefficients from estimating equation 49 but replacing the  $\frac{dw}{w}$  outcome with alternative outcomes. Specifically, *mover* refers to individuals who go through an employer to employer transition without an intermediate nonemployment spell. *Recalled* refers to individuals who leave their current employer for another employer or nonemployment and then return to their original employer within the next year or two (depending on the specification). *ENE* refers to employer to different employer transitions with an intermediate nonemployment spell (excluding paternity leave). *EUE* refers to employer to different employer transitions with an intermediate unemployment spell (measured by any UI receipt). *Mth UI* refers to the number of months in the next year or two with UI receipt. *Mth Sick* refers to the number of months in the next year or two on sick leave. 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.

Figure 13: Wage Effects: DiD Regression Design by Transition Type



*Note:* The figure shows  $\delta_0$  coefficients from estimating equation 49 but interacting an indicator for each transition type with the  $\delta_e$  coefficients in equation 49. 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. For this specification we also relax the sample restriction requiring 12 months of employment in the base year to pick up individuals more likely to experience employment transitions. *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.

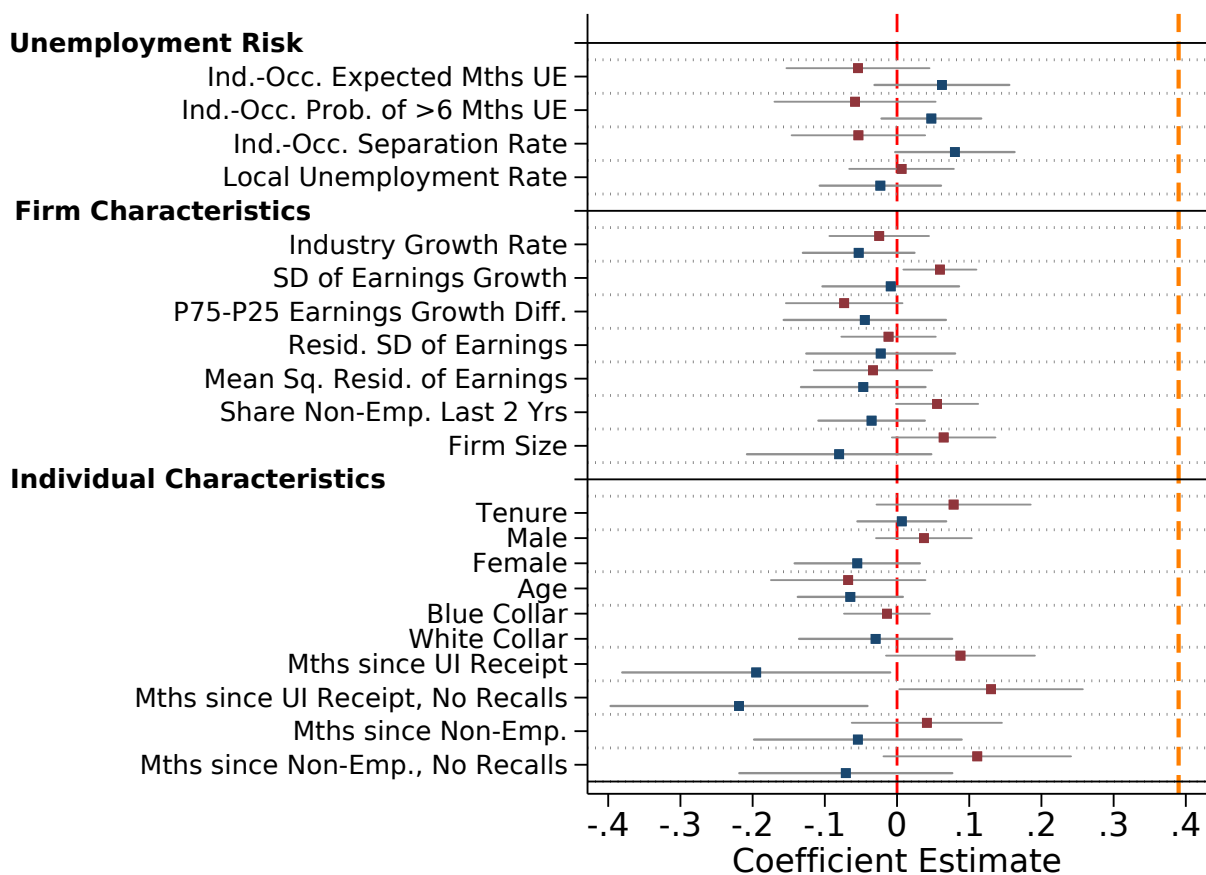
Figure 14: Heterogeneity of Nonemployment Effects on Wages: One-Year Effects



*Note:* The figure shows  $\delta_0$  coefficients from estimating equation 49 but interacting an indicator for each different heterogeneity group category with the  $\delta_e$  coefficients in equation 49. 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 F 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.

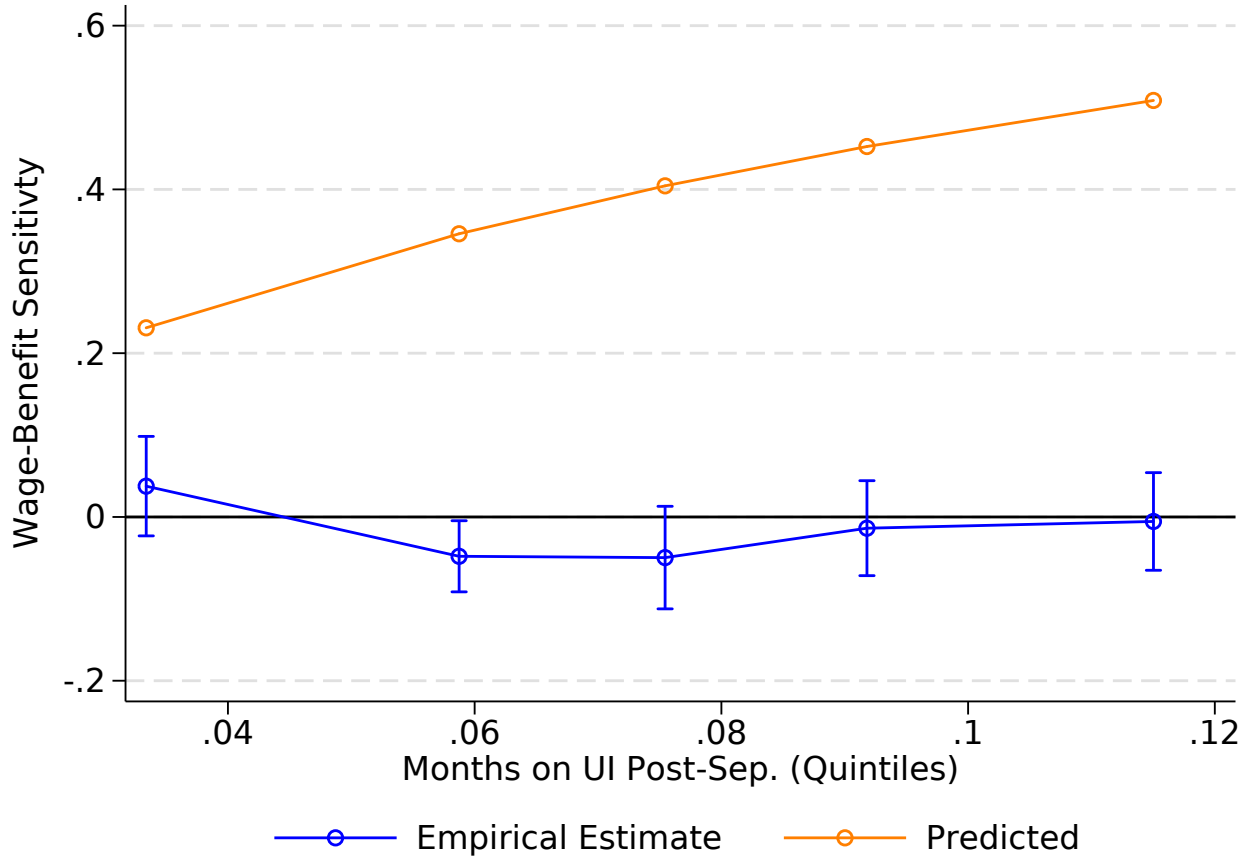


Figure 15: Heterogeneity of Nonemployment Effects on Wages: Two-Year Effects



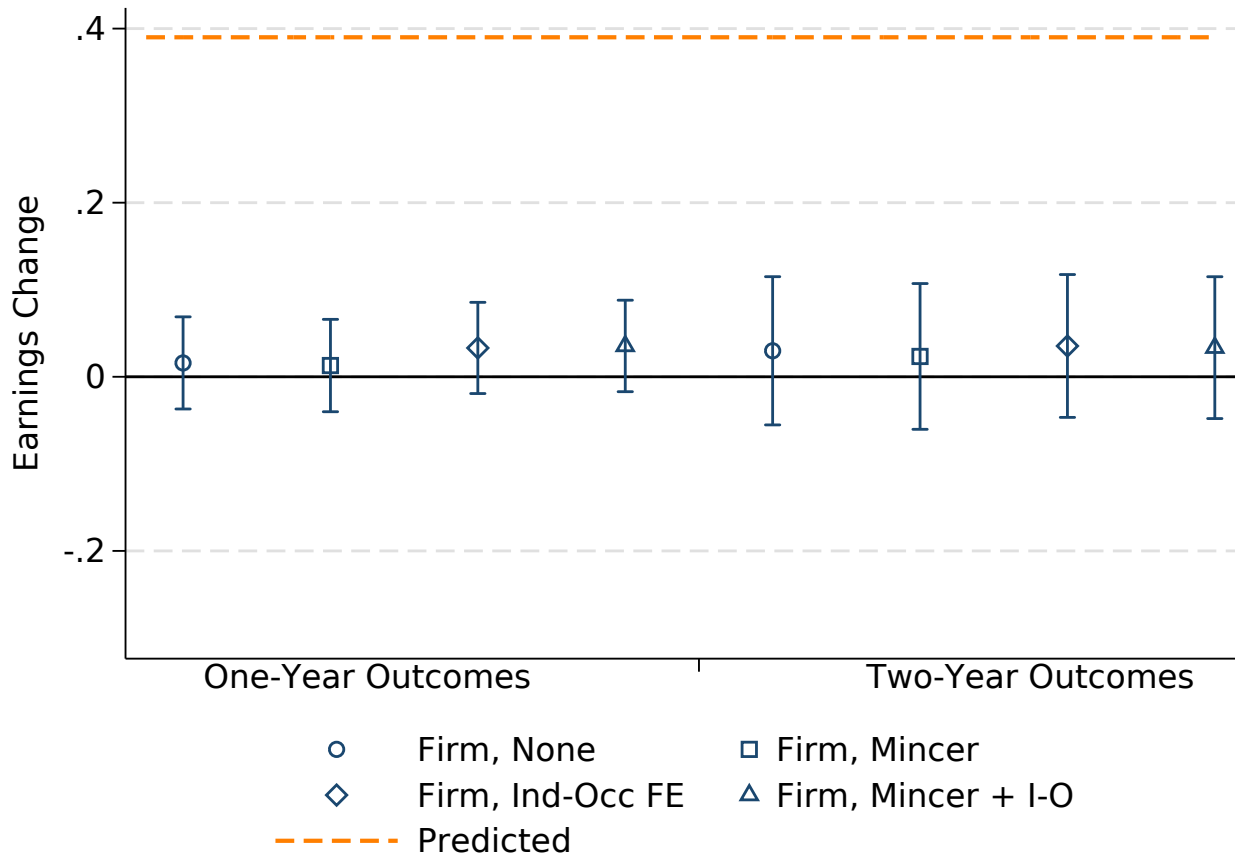
*Note:* The figure shows  $\delta_0$  coefficients from estimating equation 49 but interacting an indicator for each different heterogeneity group category with the  $\delta_e$  coefficients in equation 49. We also vary the parametric earnings controls by heterogeneity type, allowing for differential 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 F 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.

Figure 16: Heterogeneity of Wage-Benefit Sensitivity by Predicted Time in Unemployment  $\tau_i$ : Theoretical Prediction from Calibrated Bargaining Model vs. Empirical Estimate



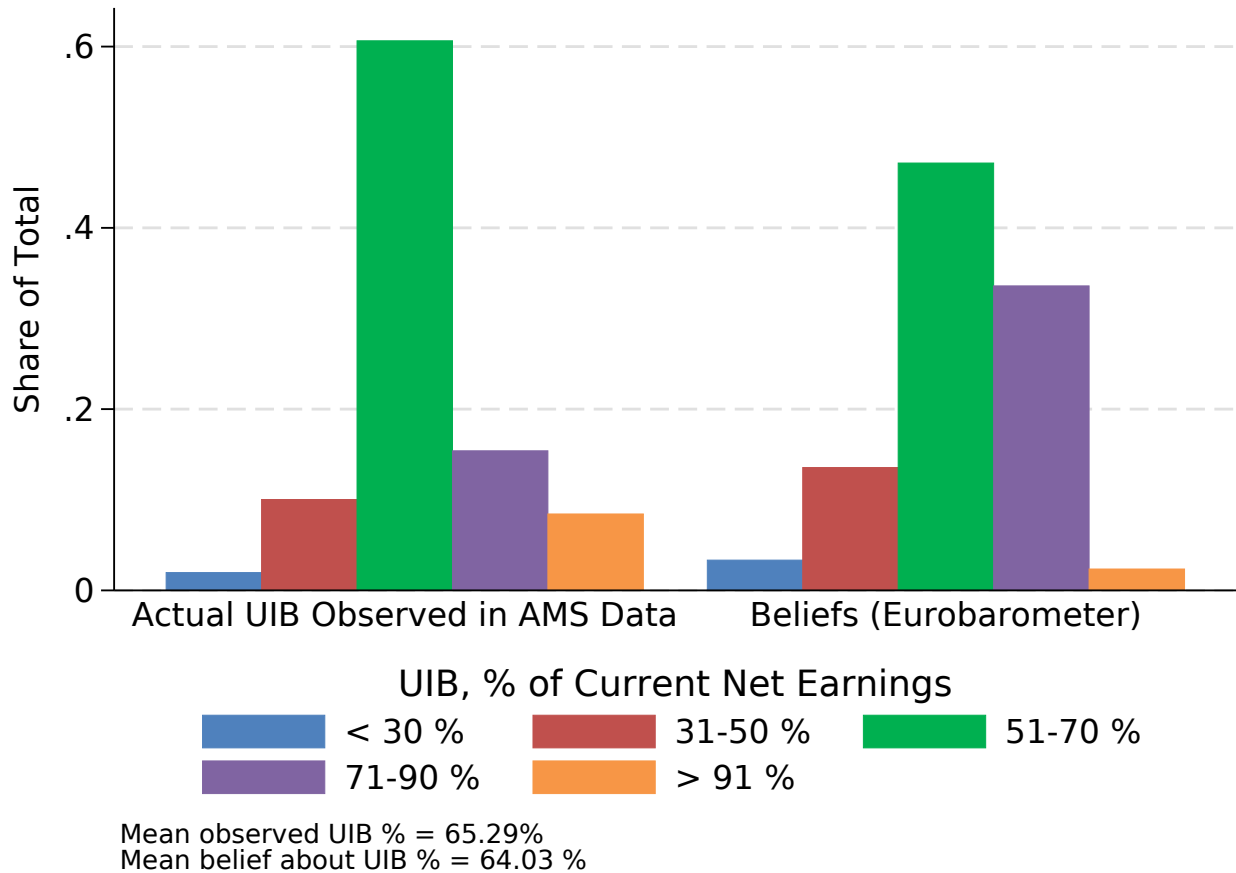
*Note:* The graph presents wage-benefit sensitivities for workers sorted by their predicted fraction of time in unemployment conditional on a separation (“ $\tau$ ”) over the subsequent five years. The x-axis sorts these workers into five quintiles and traces out the median value per quintile. For example, “0.2” indicates that the median worker is expected to spend 12 out of 60 months in unemployment. “Unemployment” means receipt of unemployment insurance benefits. The yellow (top) line plots predicted wage-benefit sensitivity on the basis of each worker’s idiosyncratic predicted  $\tau_i$ . 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 9 and based a Nash bargaining model with worker bargaining power  $\phi = 0.1$ .

Figure 17: 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 49 with the variation in benefits aggregated at the firm- and industry-level. See Section 5.2.2 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 solid 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.

Figure 18: 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), plotted with the navy dot above. We also report the actual replacement rate of unemployed workers in 2006 based on AMS data and find a mean of 65.29%.

# Online Appendix of: Wages and the Value of Nonemployment

Simon Jäger, Benjamin Schoefer, Samuel Young and Josef  
Zweimüller

## A Additional Tables

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

	Prop. of NE Spells	No. Spells
All Spells	.477	2198432
2 Years or Shorter	.483	1994213
2 Days or Longer	.522	2004432
14 Days or Longer	.638	1592033
28 Days or Longer	.674	1417843
Between 28 Days and 2 Years	.717	1213624
Men	.465	1156642
Women	.49	1041790
Blue Collar	.492	1214424
White Collar	.541	702263
Excluding Ages 50-54	.472	2063302
Employed At Least 2 Years	.481	1342093
<b>Spells between 28 Days and 2 Years</b>		
Male	.746	607503
Male Under 50	.744	565954
Female	.688	606121
Female Under 50	.689	565214
Blue Collar	.771	642668
White Collar	.756	425960
Excluding Ages 50-54	.716	1131168
Employed At Least 2 Years	.723	746469

*Note:* This table plots the share of workers who take up unemployment insurance after the end of an employment spell. The sample is restricted to prime-age workers (25-54) whose employment spell prior to nonemployment lasted at least one year and who were not recalled by their previous employer. We also drop workers who immediately transition from employment into other types of spells, e.g., maternity leave or disability. The sample period ranges from 1972 to 2000. To illustrate, the table indicates that 63.8% of nonemployment spells of 14 days or longer led to take-up of unemployment insurance.

Table A.2: Validation Exercise: Difference-in-Differences Regression Design

	1-Year Realized RR Effects			2-Year Realized RR Effects		
	(1)	(2)	(3)	(1)	(2)	(3)
Placebo: 3 Yr Lag	.031 (.074)	.025 (.073)	.091 (.041)	-.068 (.049)	-.071 (.048)	-.041 (.028)
Placebo: 2 Yr Lag	.07 (.013)	.068 (.013)	.084 (.01)	0 (0)	0 (0)	0 (0)
<b>Treatment Year</b>	.752 (.022)	.741 (.022)	.754 (.018)	.451 (.027)	.437 (.026)	.455 (.024)
Base-Year Average	2.268	2.268	2.268	3.194	3.194	3.194
Pre-p F-test p-val	0	0	0	.173	.138	.153
$R^2$	.753	.763	.820	.556	.586	.717
$N$ (1000s)	9655	9652	8647	6647	6644	5922
Mincerian Ctrl		X	X		X	X
Ind.-Occ. FEs		X	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 50. Standard errors based on two-way clustering at the individual and earnings percentile level are in parentheses. The null hypothesis of the F-test is that the coefficients of interest are all equal to 0 in the pre-period. The Mincerian controls include time-varying polynomials of experience, tenure, and age; time-varying gender indicators, and a control for being REBP eligible. The industry-occupation controls are time-varying fixed effects for each four-digit industry interacted with an indicator for a blue vs. white-collar occupation.

## B Additional Figures

**Description of Figures A.1-A.4** Figures A.1-A.4 present additional non-parametric results for the 2001, 1989, 1985, and 1976 replacement rate reforms. The left column in each set of figures contains results for one-year earnings changes and the right column contains results for two-year earnings changes.

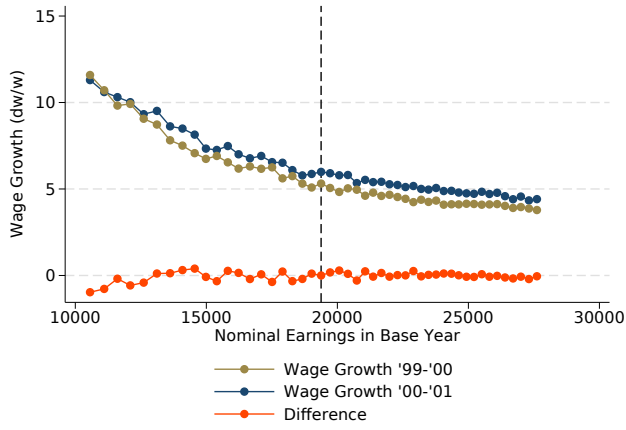
Panels (a) and (b) plot the average wage growth for the treatment year (navy scatter points) and the pre-period year (olive scatter points) over the earnings distribution. Their difference (orange scatter points) is the same earnings growth difference that is plotted in figures 6- 9. The navy and olive scatter point allow us to better assess the (lack of) pre-trends in earnings growth by comparing the earnings growth gradient in the treatment and control time periods.

Panels (c) and (d) plot the average of our predicted replacement rate change (the green line) and the average of the actual replacement rate change (the red line) over the earnings distribution. The predicted replacement rate change is calculated using the *predicted* earnings in the replacement rate reform year. See section 4.1 for more details about this prediction process. The actual replacement rate change is the average of the replacement rate changes each individual actually experiences. For 2001, since UI benefits are determined by lagged earnings, the predicted and actual replacement rate changes are identical for one year outcomes.

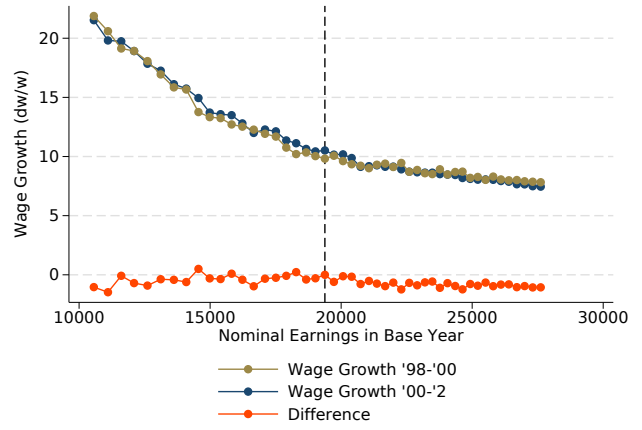
Panels (e) and (f) further assess the parallel trends assumption underlying our identification strategy. Here, we estimate the effects of placebo reforms at the same earnings percentile ranges, but we lag both the reform period and the pre-period by two years. This placebo exercise thus assesses whether the earnings percentiles affected by the reform experienced higher or lower wage growth compared to other earnings percentiles in periods before the reform was enacted. The results presented in these panels are the same as in panels (a) and (b) except all years are lagged by one or two to estimate the effect the placebo effects. For 1976, we cannot run this placebo check because we do not have enough years of pre-period social security records; these two panels are therefore missing for 1976.

Figure A.1: Additional Results: 2001 Reform

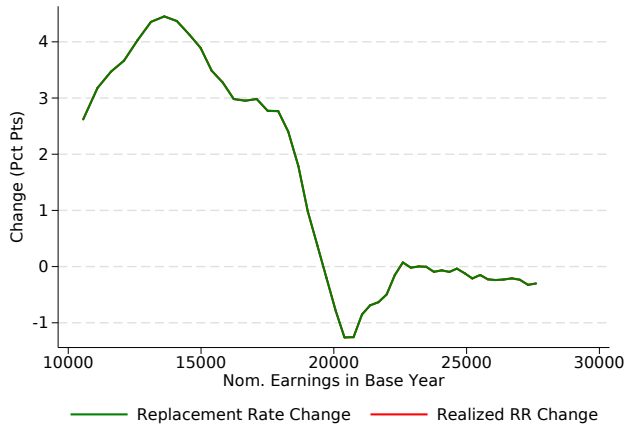
(a) Wage Growth: 2000-2001 vs. 1999-2000; 1 Yr



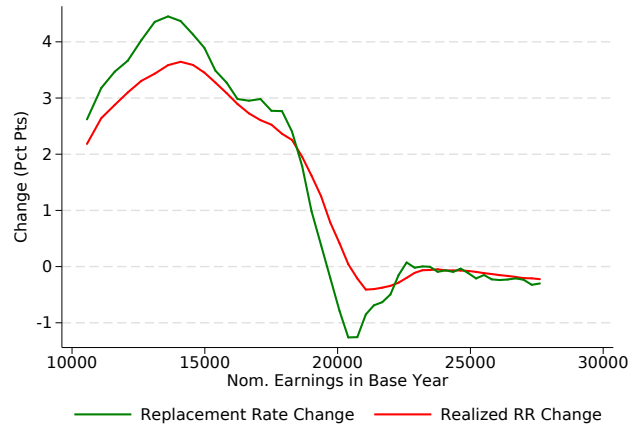
(b) Wage Growth: 2000-2 vs. 1998-2000; 2 Yr



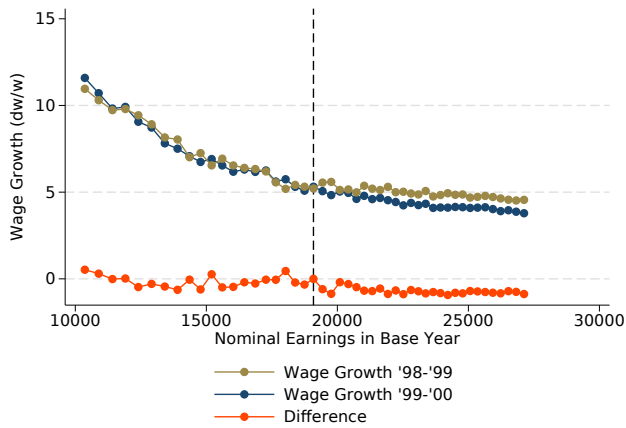
(c) Realized vs. Predicted Benefit Change; 1 Yr



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



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



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

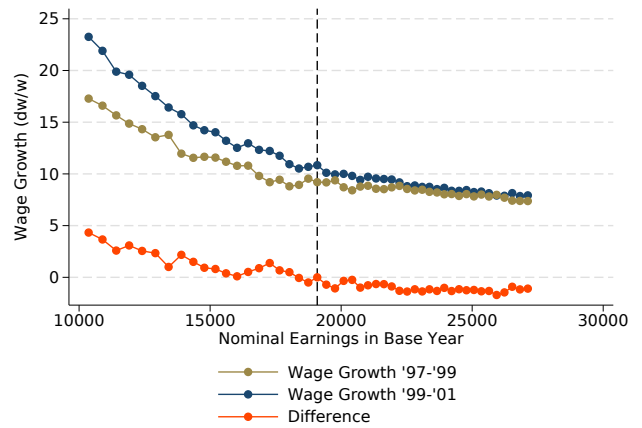
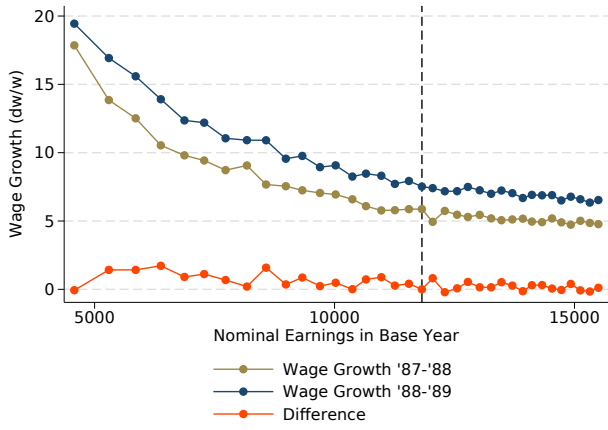


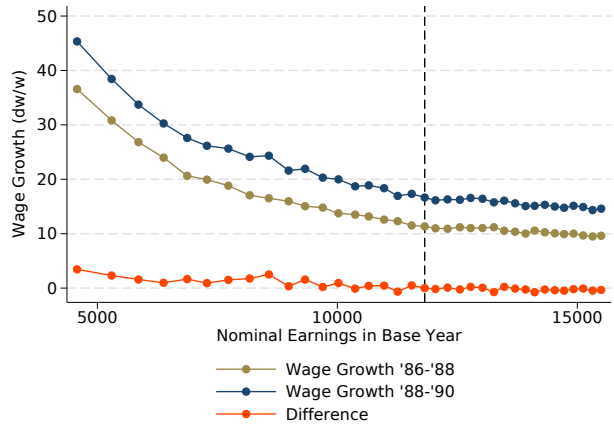


Figure A.2: Additional Results: 1989 Reform

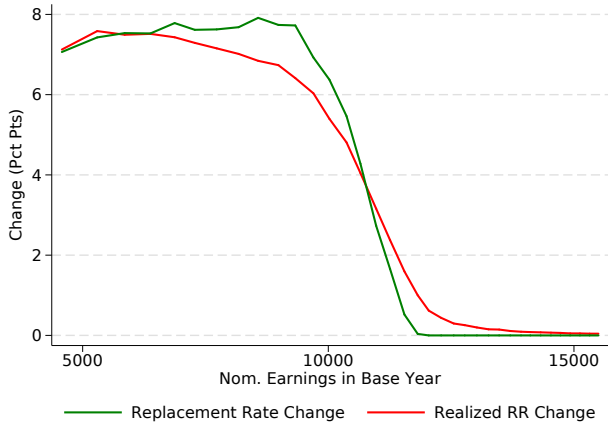
(a) Wage Growth: 1987-8 vs. 1988-9; 1 Yr



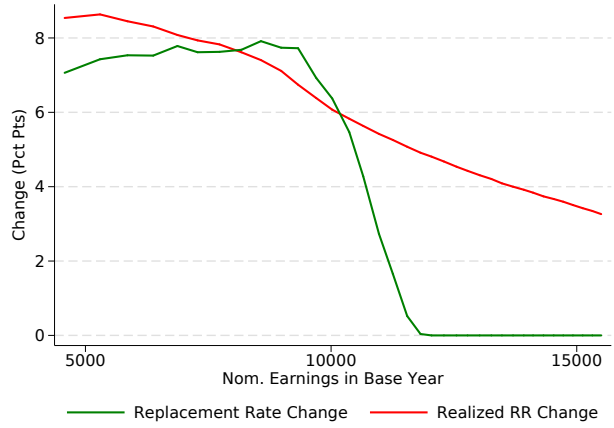
(b) Wage Growth: 1986-8 vs. 1988-90; 2 Yr



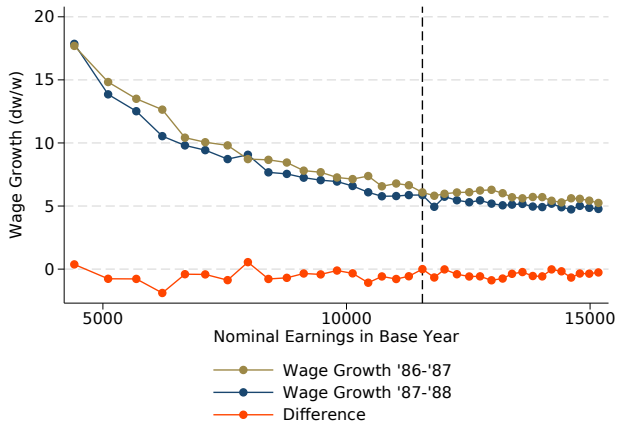
(c) Realized vs. Predicted Benefit Change; 1 Yr



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



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



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

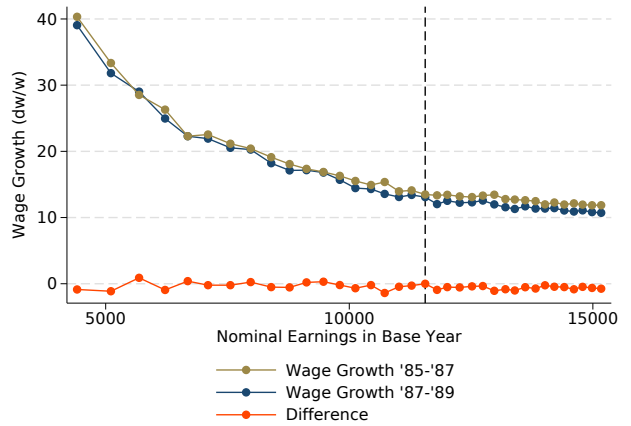
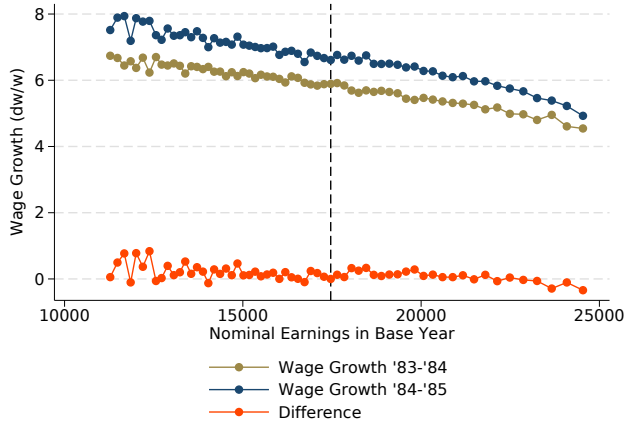
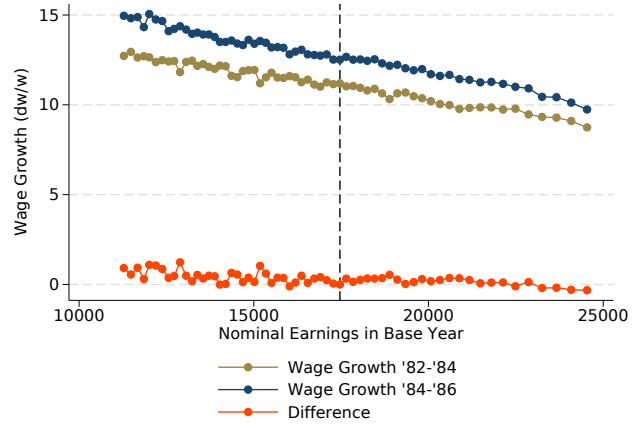


Figure A.3: Additional Results: 1985 Reform

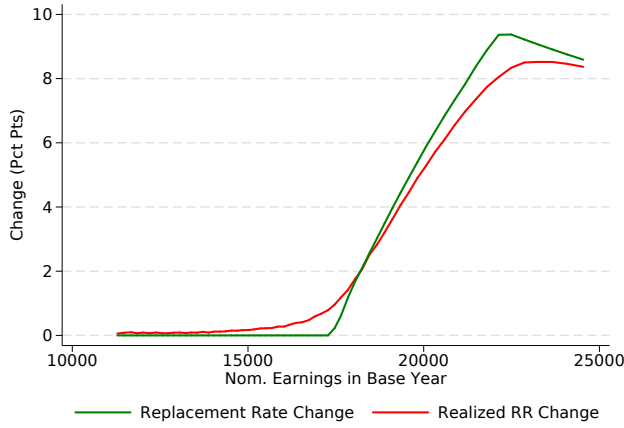
(a) Wage Growth: 1983-4 vs. 1984-5; 1 Yr



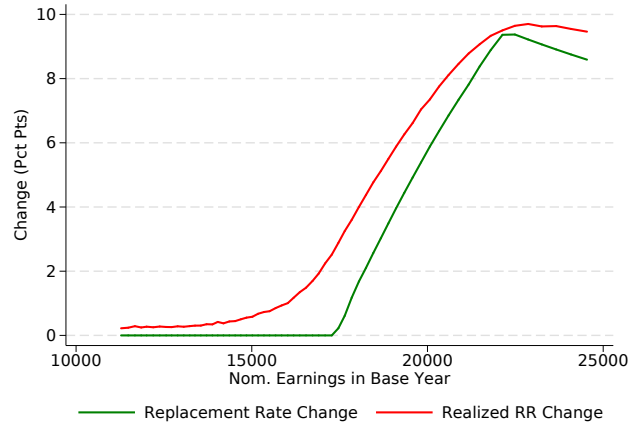
(b) Wage Growth: 1982-4 vs. 1984-6; 2 Yr



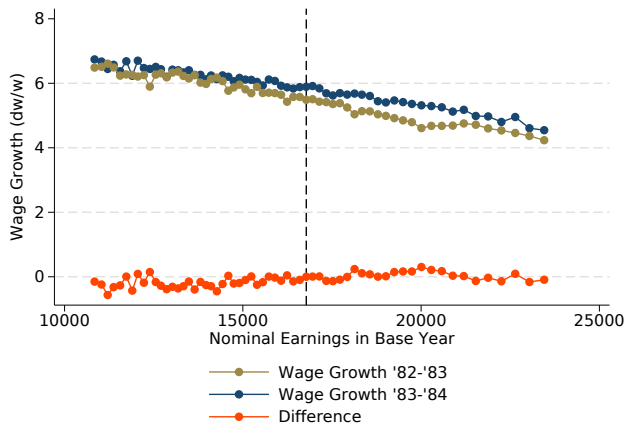
(c) Realized vs. Predicted Benefit Change; 1 Yr



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



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



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

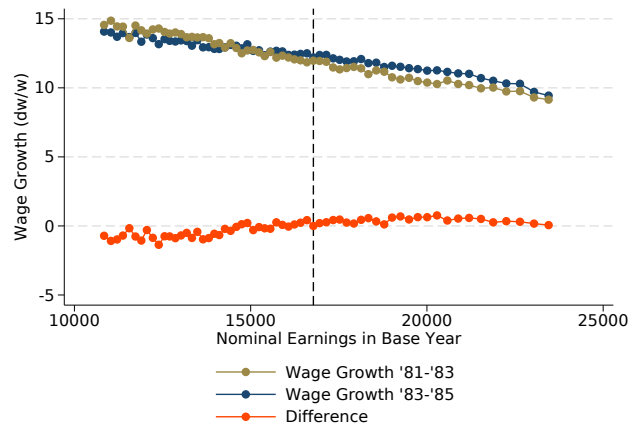
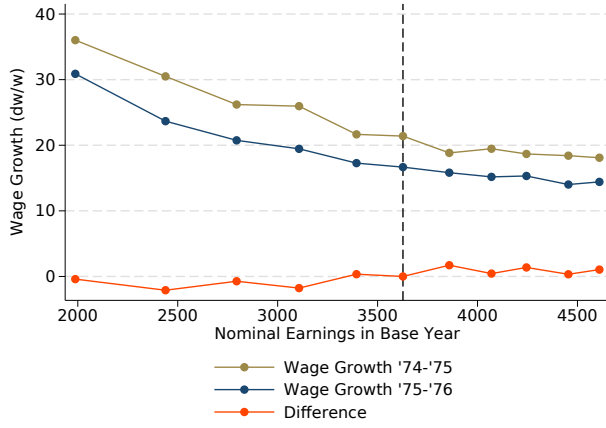
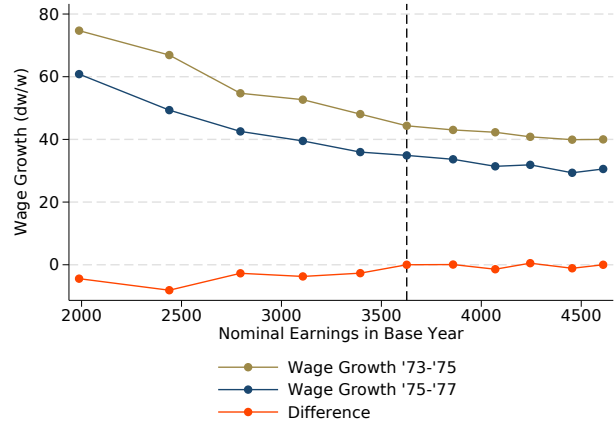


Figure A.4: Additional Results: 1976 Reform

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



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



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



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

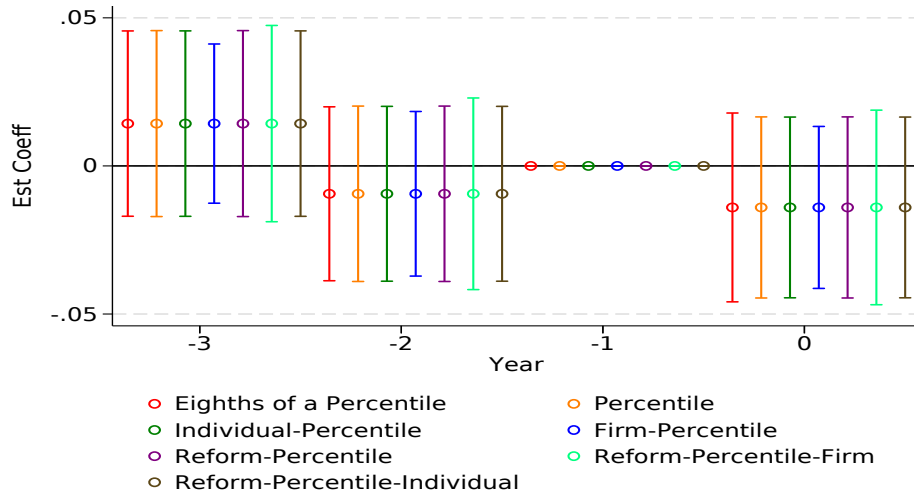


Figure A.5: Robustness Check: Different Parametric Earnings Controls



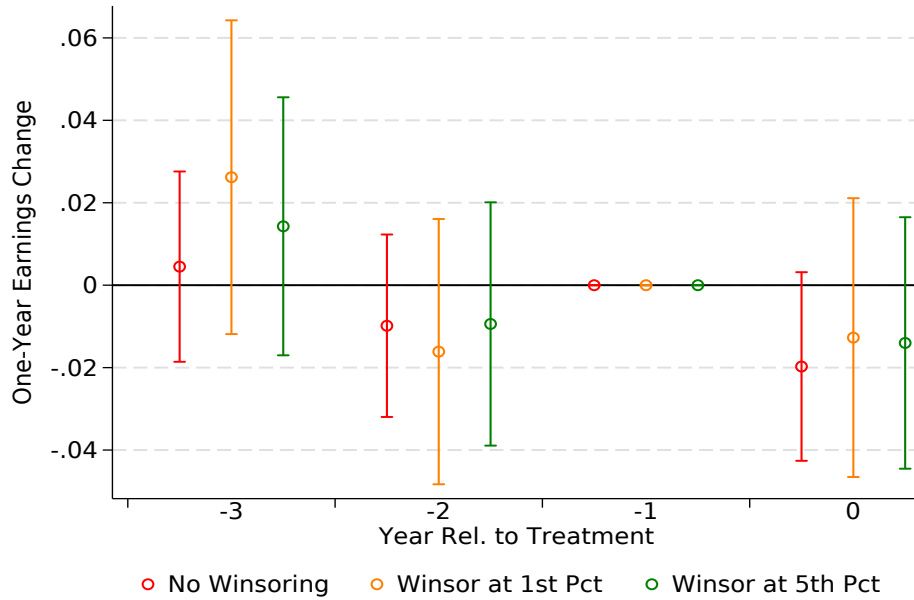
*Note:* The figure plots estimated  $\delta_0$  coefficients and associated confidence intervals based on the difference-in-differences specification in 49. It estimates specification (4) reported in Tables 2 but changes the year-specific parametric earnings controls used. The red estimates controls for log earnings, the yellow estimates controls for earnings linearly, and the green estimates control linearly for earnings percentiles. See Section 4.5 for more details about the importance of assessing robustness to the earnings controls.

Figure A.6: Robustness Check: Different Levels of Clustering



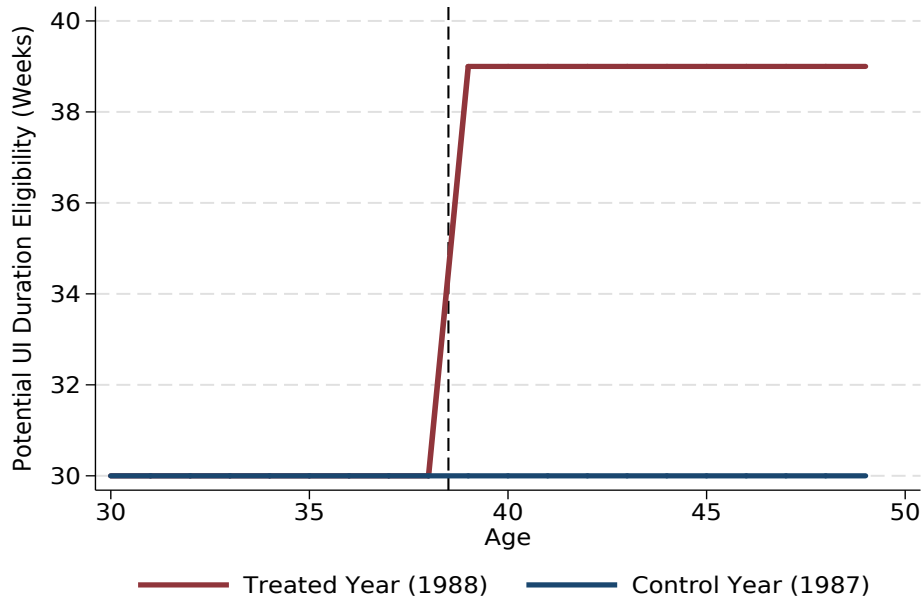
*Note:* The figure plots estimated  $\delta_0$  coefficients and associated confidence intervals based on the difference-in-differences specification in 49. It estimates specification (4) reported in Tables 2 but changes the level of clustering used to calculate the standard errors. Reform-specific clustering would lead us to treat observations in the 5th percentile for the 1976 and the 2001 reform as part of different clusters.

Figure A.7: Robustness Check: Outcome Variable Winsorization



*Note:* The figure plots estimated  $\delta_0$  coefficients and associated confidence intervals based on the difference-in-differences specification in 49. It estimates specification (4) reported in Tables 2 but the level of winsorization we use for the outcome variables.

Figure A.8: PBD Schedule - Treated and Control Years



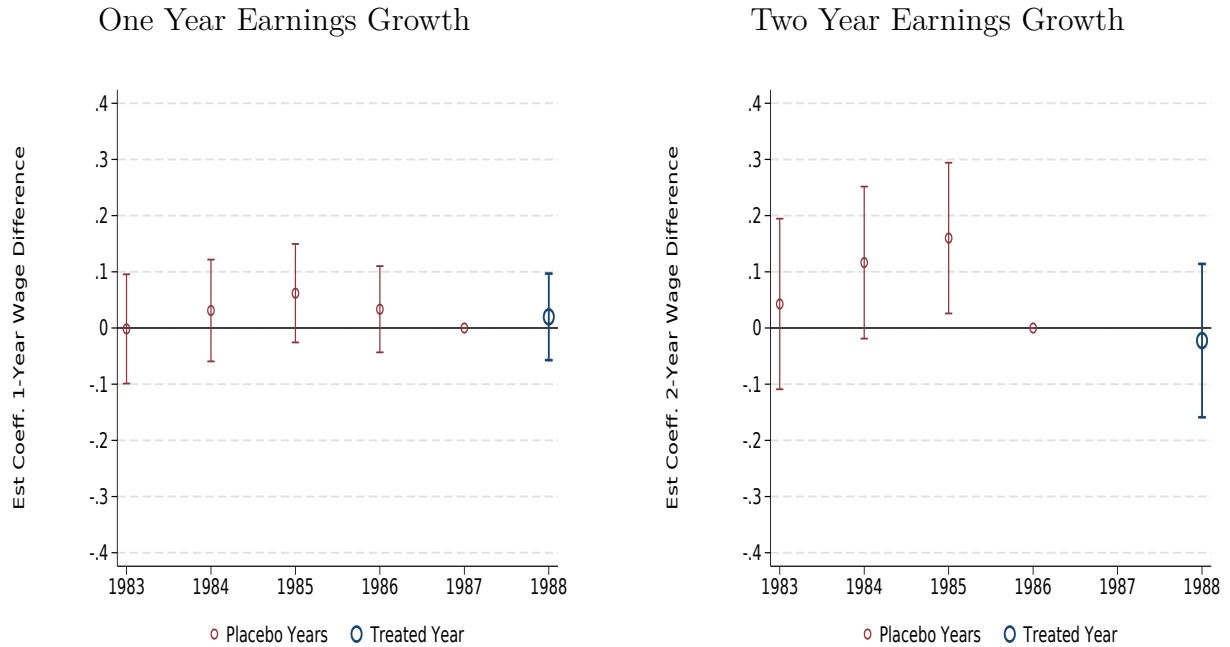
*Note:* The figure plots potential benefit duration (PBD) schedule by age for individuals in 1988 and 1989. Before 1988, all individuals with at least five years of work experience in the past ten years were eligible for 30 weeks of PBD. In 1989, individuals age 40-49 with the same experience were eligible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

Figure A.9: Non-Parametric PBD Figures - One and Two Year Earnings Growth



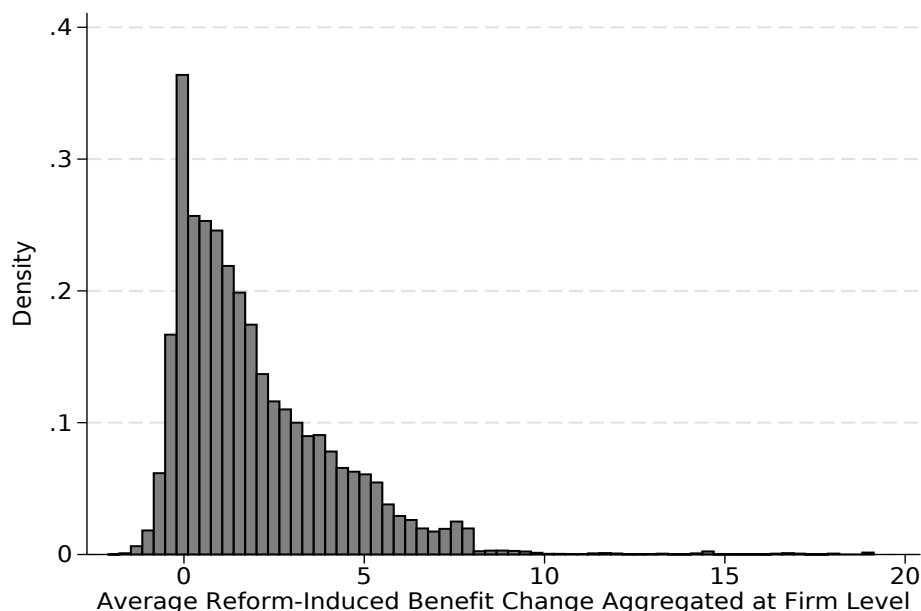
*Note:* These figures plot average earnings growth by age from 1987-1988 and 1989-1989 (the year the PBD extension went into effect). Consequently, they mirror the non-parametric analysis for the replacement rate reforms presented in the first two panels of A.1-A.4.

Figure A.10: Difference in Difference Coefficient Estimates



*Note:* These figures report results from estimating a specification similar to equation 49 but replacing the reform-induced benefit changes with an indicator for being ages 39-42 in 1988 (treated by the PBD reform) and adding age-specific fixed effects. We include the same controls included in specification (4) in table 2.

Figure A.11: Distribution of Benefit Changes at the Firm Level



*Note:* The figure plots the distribution of the average reform-induced benefit change aggregated at the firm level.

## C Additional Wage Setting Models

### C.1 Bilateral Nash Bargaining Between an Individual Household with a Potentially Multi-Worker Firm

The model presented here forms the basis for the additional model variants presented in Section 2.3. Here we generalize the structural wage equation by a variety of dimensions, starting with a bilateral bargaining between a worker and a multi-worker firm, long-term jobs and non-linear utility.

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

**Household.** Labor is indivisible and hours are normalized to one. In a given period  $s$ , the household is either employed or unemployed ( $e_s \in \{0, 1\}$ ). There is no direct labor supply channel; workers accept job opportunities when they emerge. When employed, the worker earns wage  $w_s$ . The employed household incurs labor disutility  $\gamma$ . When unemployed, the worker collects unemployment insurance benefits  $b$ . With probability  $f$ , the worker finds a job and moves into employment (and wage bargaining) next period. With probability  $1 - \delta$ , employed job seekers lose their jobs and become unemployed. Households can borrow and save at interest

rate  $r$ , fulfilling a lifetime budget constraint.<sup>80</sup> Households own firms and collect capital income in form of dividends  $d_t$ .

$$V^H(e_t) = \max_{c_t} \mathbb{E}_t \sum_{s=t}^{\infty} \beta^{s-t} u(c_s) - \gamma \cdot \mathbb{I}(e_s = 1) \quad (\text{A1})$$

$$\text{s.t.} \quad \mathbb{E}_t \sum_{s=t}^{\infty} \frac{c_s}{(1+r)^{s-t}} \leq \mathbb{E}_t \sum_{s=t}^{\infty} \frac{\mathbb{I}(e_s = 1) \cdot w_s + \mathbb{I}(e_s = 0) \cdot b + d_s}{(1+r)^{s-t}} + a_t \quad (\text{A2})$$

$$\mathbb{E}_t[e_{s+1}|e_s = 1] = 1 - \delta \quad \forall s \quad (\text{A3})$$

$$\mathbb{E}_t[e_{s+1}|e_s = 0] = f \quad \forall s \quad (\text{A4})$$

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

$$U_t = \max_{c_t} u(c_t|e_t = 0) + (1-f)\beta\mathbb{E}_t U_{t+1} + f\beta\mathbb{E}_t \widetilde{W}_{t+1} \quad (\text{A5})$$

$$W_t = \max_{c_t} u(c_t|e_t = 1) - \gamma + (1-\delta)\beta\mathbb{E}_t W_{t+1} + \delta\beta\mathbb{E}_t U_{t+1} \quad (\text{A6})$$

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

$$W_t(w) - U_t = \lambda(w - b) - \gamma + (1 - \delta) \cdot \beta\mathbb{E}_t(W_{t+1} - U_{t+1}) - f \cdot \beta\mathbb{E}_t(\widetilde{W}_{t+1} - U_{t+1}) \quad (\text{A7})$$

**Firm.** The multi-worker firm, facing a competitive product and capital market, employs  $N_t$  workers in long-term jobs and rents capital  $K_t$  at rate  $R_t$ . Capital rentals are made given wages after bargaining.<sup>81</sup> Production follows constant returns with all labor being of the same type and thus perfect substitutes, which together with rented capital implies linear production in labor, avoiding multi-worker bargaining complications. Each period, a fraction  $1 - \delta$  workers separate into unemployment exogenously, whereas the firm hires  $H_t$  workers at cost  $c(H_t)$ . Employment follows a law of motion as a constraint in the firm's problem. The firm maximizes the present value of payouts to the households (stockholders):

$$V_t^F(N_t) = \lambda\mathbb{E}_t \max_{H_t, K_t} \sum_{s=t}^{\infty} \beta^{s-t} [F(K_t, N_t) - w_t N_t - R_t K_t - c(H_t)] \quad (\text{A8})$$

$$\text{s.t.} \quad N_{t+1} = (1 - \delta)N_t + H_t \quad (\text{A9})$$

<sup>80</sup>Due to the absence of moral hazard in job search and due to the law of large numbers on the part of the unmodelled lenders, the expected lifetime earnings do not complicate the borrowing potential of households. Since average unemployment spells are short in nature (on the order of 45% at the monthly rate in the US), we abstract from shifts in lifetime earnings in shifting lifetime wealth and therefore the multiplier on the budget constraint. Therefore, we assume that the the budget constraint multiplier is approximately independent of the employment status,  $\lambda(e = 0) \approx \lambda(e = 1)$ .

<sup>81</sup>Rental of capital inputs and this timing conventions precludes the complication of potential investment holdup associated with bargaining.



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

$$V_t^F(N_t) = \max_{H_t, K_t} \left\{ \lambda[F(K_t, N_t) - w_t N_t - R_t K_t - c(H_t)] + \beta V_{t+1}^F(N_{t+1}) \right\} \quad (\text{A10})$$

$$\text{s.t.} \quad N_{t+1} = (1 - \delta)N_t + H_t \quad (\text{A11})$$

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

$$F_K(N_t, K_t) = R_t \quad (\text{A12})$$

$$c'(H_t) = \beta \mathbb{E}_t \frac{\partial V_{t+1}(N_{t+1})}{\partial N_{t+1}} \quad (\text{A13})$$

$$\frac{\partial V_t^F(N_t)}{\partial N_t} = \lambda[F_N(K_t, N_t) - w_t] + (1 - \delta)\beta \mathbb{E}_t \frac{\partial V_{t+1}^F(N_{t+1})}{\partial N_{t+1}} \quad (\text{A14})$$

$$\Rightarrow c'(H_t) = \beta \mathbb{E}_t \left[ F_N(K_{t+1}, N_{t+1}) - w_{t+1} + (1 - \delta)c'(H_{t+1}) \right] \quad (\text{A15})$$

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

$$\Delta V_t^F(N_t, w) = \lambda[F_N(K_t, N_t) - w] + (1 - \delta)\beta V_{t+1}^F(N_{t+1}) \quad (\text{A16})$$

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

$$w^N = \arg \max_w (W(w) - U)^\phi \times (\Delta V^F(N_t, w))^{1-\phi} \quad (\text{A17})$$

$$\Rightarrow W(w^N) = U + \underbrace{\phi (\Delta V^F(N_t, w) + W(w^N) - U)}_{\text{Job surplus}} \quad (\text{A18})$$

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

The model recognizes the long-term nature of jobs.<sup>82</sup> Wages then not only reflect current conditions but also expectations about future inside and outside values, through the continuation values. An important implication of Nash bargaining to apply also in subsequent period, renders

<sup>82</sup>We consider period-by-period bargaining in the main part of this exposition.

the Nash wage identical to the myopic thought experiment except for a continuation term:<sup>83</sup>

$$w^N = \phi F_N(K_t, N_t) + (1 - \phi)(1 - \beta) \frac{U}{\lambda} \quad (\text{A19})$$

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

## C.2 Alternative Bargaining Model: A Simple Version of Credible Bargaining (Hall and Milgrom, 2008)

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

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

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

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

$$E(w) = \frac{w + \beta\delta N}{1 - \beta(1 - \delta)} \quad (\text{A20})$$

$$J(w) = \frac{p - w}{1 - \beta(1 - \delta)} \quad (\text{A21})$$

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

<sup>83</sup>The derivation recognizes that  $\phi\beta\mathbb{E}_t(W_{t+1} - U_{t+1}) = (1 - \phi)\beta\mathbb{E}_t V_{t+1}^F{}'(N_t)$  by Nash bargaining in  $t + 1$  *in the job at hand*. In consequence, the  $(1 - \delta)$ -weighted continuation terms cancel out:

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

**Worker's strategy: offer firm's reservation wage.** The firm's indifference condition defines the worker's strategy, to offer the firm its reservation wage  $\bar{w}$ :

$$\frac{p - \bar{w}}{1 - \beta(1 - \delta)} = -\gamma + \beta(1 - s) \frac{p - \underline{w}}{1 - \beta(1 - \delta)} \quad (\text{A22})$$

$$p - \bar{w} = -(1 - \beta(1 - \delta))\gamma + \beta(1 - s)(p - \underline{w}) \quad (\text{A23})$$

$$\bar{w} = (1 - \beta(1 - \delta))\gamma + \beta(1 - s)\underline{w} - p(1 - \beta(1 - s)) \quad (\text{A24})$$

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

$$\frac{\underline{w} + \beta\delta N}{1 - \beta(1 - \delta)} = z + (1 - s)\beta \frac{\bar{w} + \beta\delta N}{1 - \beta(1 - \delta)} + s\beta N \quad (\text{A25})$$

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

$$\Leftrightarrow \underline{w} = (1 - \beta(1 - \delta))z + \beta(1 - \beta(1 - \delta))N \quad (\text{A26})$$

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

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

$$\frac{\underline{w}}{1 - \beta(1 - \delta)} = \underbrace{z}_{\text{flow value while barg.}} + (1 - s)\beta \overbrace{\frac{\bar{w}}{1 - \beta(1 - \delta)}}^{\text{Follow-up offer}} + \beta \overbrace{\left( (s - \delta) \frac{1 - \beta}{1 - \beta(1 - \delta)} N \right)}^{\text{Rel. unemp. risk: bargaining vs. producing}} \quad (\text{A27})$$

$$\Leftrightarrow \underline{w} = (1 - \beta(1 - \delta))z + (1 - s)\beta\bar{w} + \beta(s - \delta)(1 - \beta)N \quad (\text{A28})$$

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

$$\underline{w} = \frac{(1 - \beta(1 - \delta))z + (1 - s)\beta [(1 - \beta(1 - \delta))\gamma + p(1 - \beta(1 - s))]}{1 - \beta^2(1 - s)^2} + \frac{\beta(s - \delta)}{1 - \beta^2(1 - s)^2} \times (1 - \beta)N \quad (\text{A29})$$

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

$$\frac{d\underline{w}}{d(1 - \beta)N} = \frac{\beta(s - \delta)}{1 - \beta^2(1 - s)^2} \quad (\text{A30})$$

Therefore, for  $s = \delta$ , the wage is insensitive to the nonemployment value. And still, the model

can still accommodate small rent sharing coefficients:

$$\frac{dw}{dp} = \frac{(1-s)\beta(1-\beta(1-s))}{1-\beta^2(1-s)^2} \quad (\text{A31})$$

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

$$\left. \frac{dw}{dp} \right|_{s=\delta \approx 0} \approx \beta \frac{1-\beta}{1+\beta^2} \quad (\text{A32})$$

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

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

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

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

Whether  $s \approx \delta$  is empirically realistic as such is difficult to assess because independently calibrating  $s$  directly to empirical evidence is not straightforward.<sup>84</sup> For example, Hall (2017) calibrates  $s = 0.013$  and  $\delta = 0.0345$ , which here would lead worker reservation wages to *fall* when  $N$  were to increase ceteris paribus. Conversely, Hall and Milgrom (2008) sets  $\delta = 0.0014$  and  $s = 0.0055$  at the daily frequency, which in our version of the AOB model leads increases in  $N$  to *increase* wages (reservation wages of the worker) ceteris paribus.

**The role of  $z$  vs.  $b$ .** While we intentionally define  $z$  (the flow utility of the worker while bargaining, perhaps not containing  $b$  for, e.g., an incumbent worker) separately from  $b$  (the nonemployment payoff, contained in  $N$ ), the original authors and the follow-up literature (see, e.g., Hall and Milgrom, 2008; Christiano et al., 2016; Hall, 2017) set both to be the same, and thus explicitly include unemployment benefits in  $z = b$ . But these authors are interested in new

<sup>84</sup>For example, in a situation with multiple applicants,  $s$  from the perspective of the worker should capture also the risk of losing out to the next applicant, with higher probability  $s$  than the incumbent worker would worry about being displaced by a colleague or get high with a job destruction shock  $\delta$ . This would suggest that  $s \gg \delta$ .

hires and their wage responses; our setting also studied incumbent workers, whose  $z$  is unlikely to contain  $b$ . Yet, for *new hires out of unemployment*, te

## D Interpreting Firm- and Industry-Level Rent Sharing Estimates in a Bargaining Setting

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

$$w^N = \phi \times p + (1 - \phi) \times \Omega \quad (\text{A33})$$

$$\Rightarrow dw^N = \phi \times \underbrace{dp}_{\text{Rent sharing variation}} \quad (\text{A34})$$

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

**Elasticity specifications.** A common empirical estimate comes in an *elasticity* of wages with respect to value added per worker, measured at the firm or industry level:<sup>85</sup>

$$\xi = \frac{dw/w}{dp/p} \quad (\text{A35})$$

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

$$\frac{dw^N/w^N}{dp/p} = \phi \times \frac{p}{w^N} \quad (\text{A36})$$

Rent sharing elasticities  $\xi$  therefore provide *upper bounds* for  $\phi$ :

$$\phi = \frac{w}{p} \cdot \xi \leq \xi \quad (\text{A37})$$

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

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

<sup>85</sup>Some studies consider profit elasticities rather than value added shifts; rescaling into value added elasticities that rely on strong assumptions about homogeneity and the comovement of variable and fixed factors with productivity shifts.

$p$ ,  $b$  and the measured wage–productivity elasticity  $\xi$  as follows:

$$\phi = \frac{b\xi}{p(1 - \xi) + b\xi} = \frac{1}{\frac{p}{b} \cdot \frac{1-\xi}{\xi} + 1} \quad (\text{A38})$$

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

An interesting example is Card et al. (2015), who among many verification tests also estimate the heterogeneity in  $\xi$  for women and men. The elasticity for women is below the elasticity for men. However, even with measured productivity shifts being homogeneous, two distinct factors may cause the elasticity differences within a bargaining framework. First, either men and women wield differential bargaining power  $\phi^g$  where  $g \in \{w, m\}$ . Second,  $\phi^w = \phi^m$  yet  $p^f/w^f < p^m/w^m$  or  $p^f/b^f < p^m/b^m$ . That is, the latter scenario could arise if the opportunity cost of working of women  $b^f > b^m$ , as would also be in line with their larger labor supply elasticities, higher unemployment, and lower participation overall.

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

Identifying  $\phi$  off *level* shifts in  $p$  rather than percentage shifts eliminates the complications arising from elasticities, .

## E Additional Institutional Details and Validation Exercises

### E.1 Legal Basis for Unemployment Benefit Determination Throughout our Sample Period

From 1977 until 1987, the earnings base for calculating unemployment benefits are generally the earnings in the last full month of employment before the beginning of an unemployment spell (§ 21 (1) Arbeitslosenversicherungsgesetz 1977). Importantly, Austrian wage contracts are structured to pay out 14 instead of 12 monthly salaries, with the two additional ones typically paid out at the beginning of the summer and at the end of the year, respectively. These additional payments are proportionally factored into and added to the earnings in the last four weeks before the beginning of an unemployment spell to calculate unemployment benefits (§ 21 (2) Arbeitslosenversicherungsgesetz 1977). To illustrate, someone with constant monthly earnings of ATS 10,000 would be paid an annual salary of ATS 140,000. Unemployment benefits would be calculated based on monthly earnings of ATS 11,667 based on the monthly earnings of ATS 10,000 plus 1/12 of the two additional bonus payments ( $\text{ATS } 10,000 * 2 / 12 = \text{ATS } 1,667$ ). A reform in 1987 changed the calculation period from the last month before unemployment to the last six months before unemployment, while still factoring in the 13th and 14th monthly salary proportionally. A 1996 reform then changed the calculation more substantially by using last year's earnings for unemployment spells beginning after June 30 of a given year and the earnings in the second to last year for spells beginning before June 30. The 1996 reform left the treatment of the 13th and 14th salaries unchanged.

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

### E.2 Predicting Benefit Receipts from Lagged Income

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

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

We validate this monthly earnings concept from the ASSD by predicting unemployment insurance benefits for actual *separators* and comparing these predicted UIBs with actually received UIBs.<sup>86</sup> To this end, we merge the unemployment benefit data (AMS) with the ASSD

<sup>86</sup>The ASSD provides us with administrative data on earnings average earnings the worker received from an employment relationship over the course of the calendar year. Together with daily information on the employment spell duration for each calendar year, we construct average monthly earnings. By construction, our earnings measure cannot capture month to month variation in earnings, and therefore generally (except for separations occurring on February first) do not specifically refer to the full month's earnings before the separation.

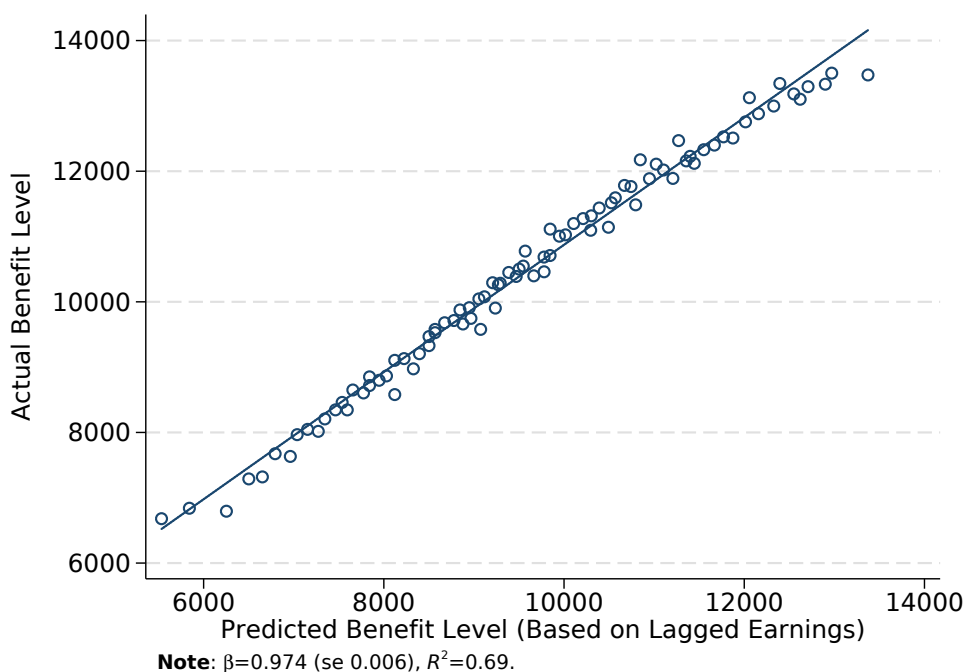


(social security based data), which contains our earnings measure. All measures are nominal and not inflation-adjusted.

Figure A.12 plots the relationship between actual and predicted UI benefit levels for all Austrian separators drawing UI benefits. The relationship traces out a slope that is on average 0.974. We therefore conclude that our approach accurately assigns employed workers by their ASSD-based earnings into the UI benefit levels.

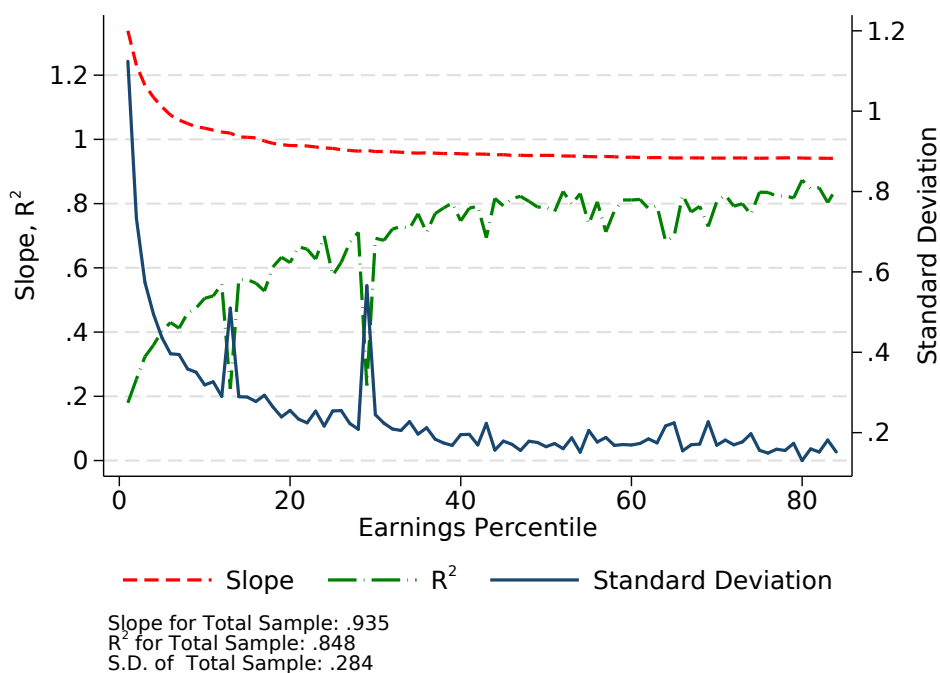
In addition, we also validate that our earnings prediction works well across the earnings distribution with coefficients on predicted and actual benefits close to 1 throughout (Figure A.13).

Figure A.12: Validation: Actual Benefit Receipts vs. Predicted Receipts from Measured Pre-Separation Average Earnings



*Note:* The figure draws on earnings data from the ASSD and benefit data from the AMS. The x-axis shows predicted benefit levels based on earnings data from the ASSD. The y-axis shows actually paid-out benefits based on data from the AMS.

Figure A.13: Quality of Wage Prediction Procedure



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

## F Construction of Variables for Heterogeneity Analysis

This section describes the construction of the variables we use for the analysis of treatment effect heterogeneity. Below, we describe how we divide the heterogeneity groups into quintiles (unless otherwise stated), which we calculate separately for each reform. Throughout, we draw on the sample of all workers, regardless of whether they are employed all year, unless stated otherwise. Prime-age below refers to the ages 25 to 54. The variable `status` refers to workers' employment status in the ASSD status.

### 1. Firm size.

- Begin with the universe of prime-age workers.
- Count the total number of workers at the firm who are employed for the whole year.
- Separate into four groups (*not* quintiles): firms less than 10 people, between 11 and 100 people, between 101 and 1,000 people, and larger than 1,000 people.

### 2. The share of the worker's firm that was nonemployed in the last two years.

- Begin with the universe of prime-age workers.
- Count the number of workers at the firm whose current employment spell is less than 24 months and who was unemployed in the month before their current employment spell (`status = 1`).

- Count the total number of workers at the firm.
- Divide the former by the latter.
- We separate the sample into quintiles by worker.

### 3. Tenure

- Begin with the sample of workers that is included in our analysis.
- Split the tenure variable by quintile.

### 4. Four measures of the time since nonemployment.

- Months since nonemployment** (i.e.  $\text{status} \neq 3$ ) Note that if employment spells are separated by only a single month of illness, then the month of illness and the two spells are counted as a single employment spell.
- Months since last UIB receipt** (i.e.  $\text{status} = 1$ ). Note that the employment spell length keeps counting if the worker becomes sick, goes on disability, or takes a parental leave.
- Months since the last change in labor market status, skipping recalls from illness/leave.** This is the same as (a), but if a worker becomes nonemployed (i.e.  $\text{status} \neq 1, 3$ ) and then returns to the same employer (i.e. the next status change is a change back into employment with the same firm), then the worker remains in the same employment spell throughout. Here, the spell count only resets when a worker receives UIB *or* when a worker becomes ill, goes on parental leave, etc. and does not return to the same firm when they are next employed.
- Months since the last UIB receipt, skipping recalls after unemployment.** This is the same as 2), but if a worker becomes unemployed or nonemployed (i.e.  $\text{status} \neq 3$ ) but then returns to the same employer (i.e. the next status change is a change back into employment with the same firm), then the worker remains in the same employment spell throughout. Here, the spell count only resets when a worker receives UIB and does not return to the same firm when they are next employed.

- We then implement the following procedures:
  - Begin with sample of prime-age workers.
  - Count the number of months for each of the four designations for each worker.
  - Split into quintiles.
  - Time: year  $t$

### 5. Local unemployment rates.

- Begin with the universe of workers aged between 25 and 54 in a given year.
- *A*: Count the number of workers who are unemployed ( $\text{status} = 1$ ) by area of residence using the **gkz** variable. The relevant information is only available starting in 1987, so we use their 1987 location for pre-1987 years.
- *B*: Count all the workers in the area of residence who are unemployed, sick, employed, self-employed, on parental leave, and in minor employment.

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

## 6. Industry growth rates.

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

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

- Count the number of workers in the firm (**benr**), not necessarily employed the whole year.
- Count the number of workers in the industry (**nace08**), not necessarily employed the whole year.
- Subtract, for each firm, its population from the number of workers in the industry.
- Find the same number for the next year  $t + 1$  (i.e. two years pre-reform), but only for workers employed at the same firm between year  $t$  and year  $t + 1$ .
- Calculate the percent difference between the leave-out employment in the industry between year  $t + 1$  and year  $t$ .

## 7. Age

- Sample identical to the one we use for analysis.

## 8. Four measures of within-firm wage dispersion.

- The standard deviation of year-on-year earnings growth within the firm.**
  - Focus on a sample of workers who stay with their firm from one year to the next.
  - Drop workers at the ASSD cap and with missing earnings.
  - Calculate the individual earnings growth relative to last year. Winsorize to the 5th and 95th percentiles.
  - Calculate the standard deviation of the earnings growth by firm-year among workers who were in the same firm across the two years.
- The difference between the 75th and 25th percentile of within-firm earnings growth.**
  - Take the earnings growth variable and sample above.
  - For each firm-year, calculate the percentile for each worker's earnings growth.
  - Take the difference between the average earnings growth for an individual in the 74th-76th percentile to that for an individual in the 24th-26th percentile.
- The residualized standard deviation of log-earnings.** We base this measure on the residuals from a regression of log-earnings on tenure-experience-occupation-industry-year fixed effects, with standard deviations calculated at the firm-year level.

Tenure  $n(i, t)$  is made up of 5 three-year categories and a category for those with more than 15 years of tenure. Experience  $e(i, t)$  is made up of 5 five-year categories and a category for those with more than 25 years experience. Occupation refers to white- vs. blue-collar, for which there are often separate collective bargaining agreements. Calculate the log-earnings for each worker, and winsorize to the 5th and 95th percentile. Regress log-earnings on industry-occupation-tenure-experience-year fixed effects. Calculate residuals from this regression, and take the standard deviation of the residual by firm-year. Split the sample into quintiles.

(d) **The mean squared residuals of log-earnings.**

- Calculate the average by firm-year of the square of the residuals from the previous regression.

9. **Occupation.**

- *Motivation.* Survey data suggest that workers with more education/skills are likelier to bargain. Thus white-collar workers might bargain more and thus be more sensitive to the outside option.
- Place blue-collar workers in occupation group 1 (`whitecoll = 0`) and white-collar workers in occupation group 2 (`whitecoll = 1`).

10. **Three measures of industry-occupation unemployment risk.**

(a) **Separation rate.** This is the probability of being unemployed in the next period in a given industry-occupation, given that one is employed in the current period.

- Sample the universe of prime-age workers.
- Create an indicator for whether the individual is unemployed (`status= 1`) in the next year.
- Regress this indicator on industry-occupation fixed effects for that year, and save these fixed effects. I also run a specification with categories for tenure and experience and a linear control for age and keep the predicted values.

**Regression:** Let  $Y_i$  be an indicator for being unemployed in the year  $t + 1$ . Individual  $i$  has occupation  $o$  (blue or white collar) in industry  $k$ . Then, for all workers in year  $t$ ,

$$Y_i = \beta_0 + \phi_{k(i),o(i)} + \epsilon_i$$

(b) **Expected months of unemployment.** This is the average number of months of unemployment in the next period, conditional on being employed in the current period.

- Sample the universe of prime-age workers.
- Calculate how many months the worker is unemployed in the following year.
- Regress the number of months on industry-occupation fixed effects for that year, and save these fixed effects.

**Regression:** Let  $t$  be the year three years before the reform. Individual  $i$

has occupation  $o$  (blue or white collar) in industry  $k$ . Then, for all workers in year  $t$ ,

$$Y_i = \beta_0 + \phi_{k(i),o(i)} + \epsilon_i$$

(c) **Probability of being unemployed for more than 6 months.** It is another measure of the “severity” of unemployment spells in the industry-occupation.

- Begin with the sample of prime-age workers.
- Create an indicator for whether the individual is unemployed for more than 6 months in the following year.
- Regress the indicator on industry-occupation fixed effects for that year, and save these fixed effects.

**Regression:** Let  $Y_i$  be an indicator for being unemployed for more than six months in the year  $t + 1$ . Individual  $i$  has occupation  $o$  (blue or white collar) in industry  $k$ . Then, for all workers in year  $t$ ,

$$Y_i = \beta_0 + \phi_{k(i),o(i)} + \epsilon_i$$

# G Replacement Rate Schedules in Austria: 1972–2003

Figure A.14: Replacement Rate Schedules 1972-1978

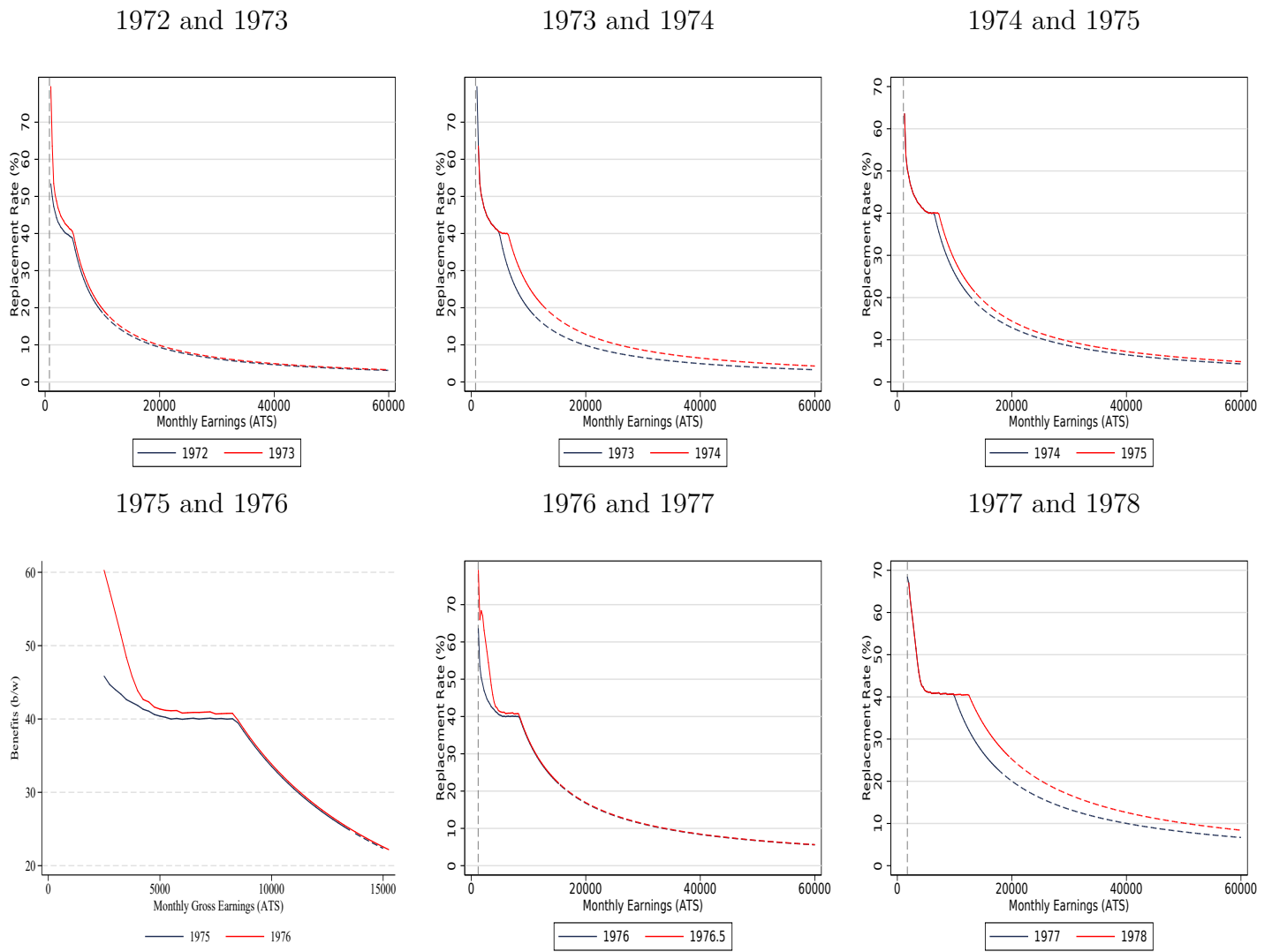
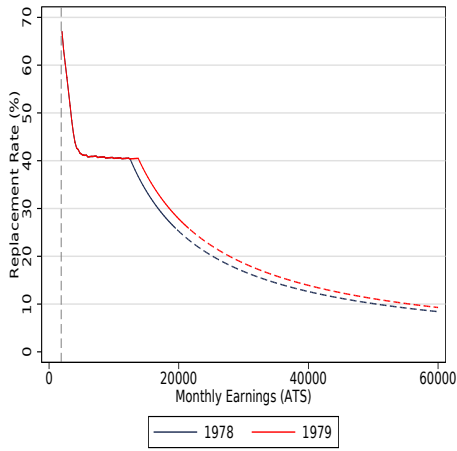
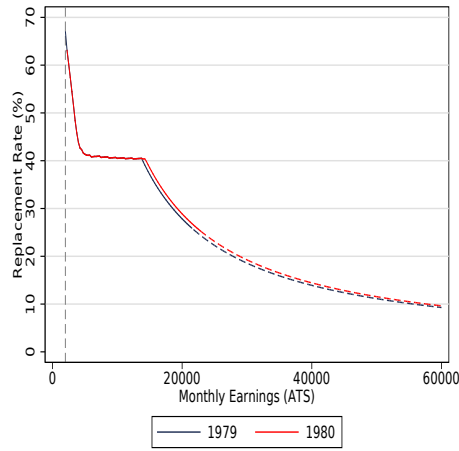


Figure A.15: Replacement Rate Schedules 1978-1987

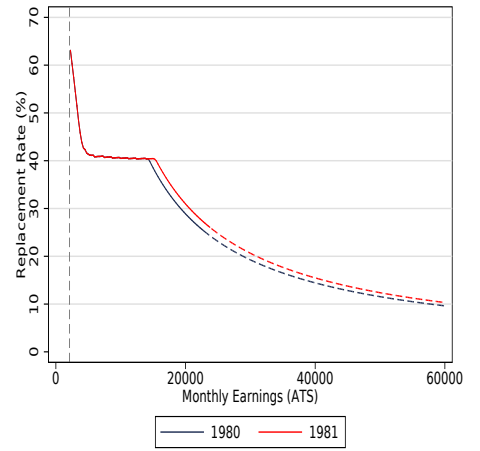
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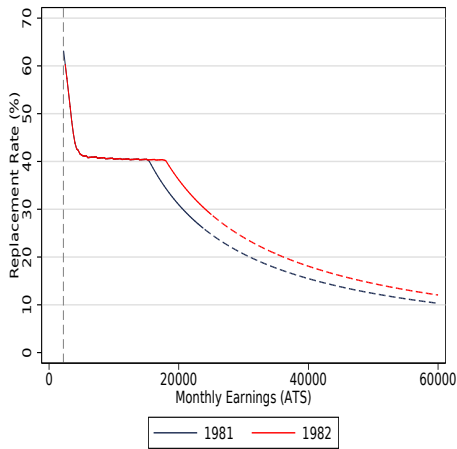
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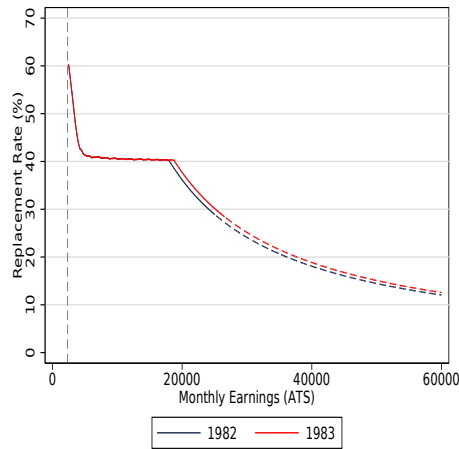
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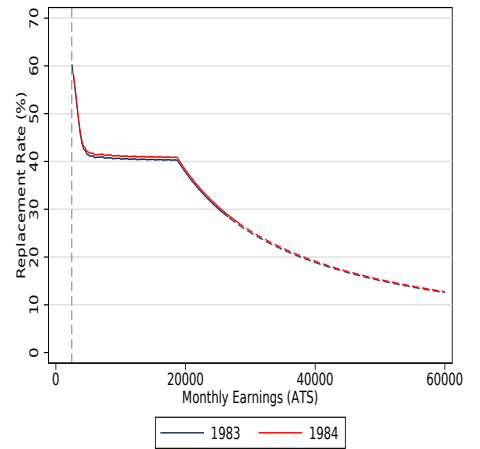
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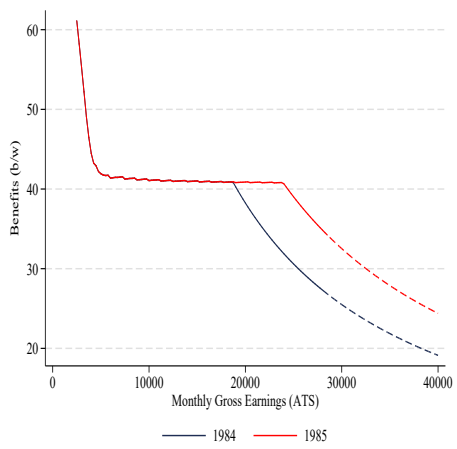
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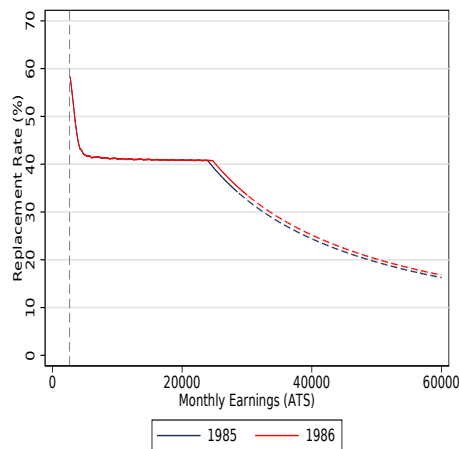
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1984 and 1985



1985 and 1986



1986 and 1987

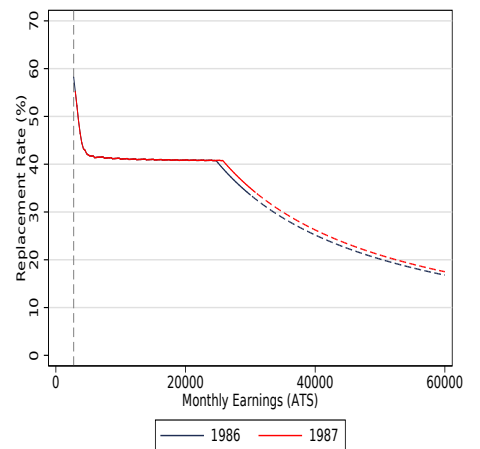
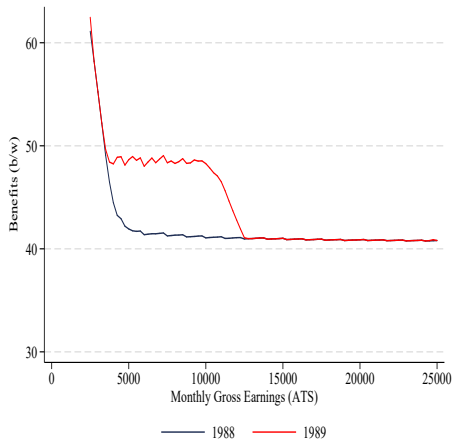


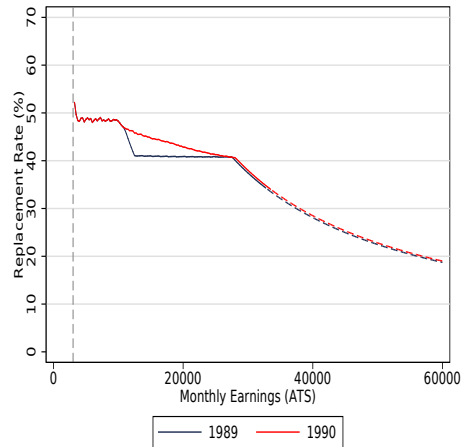


Figure A.16: Replacement Rate Schedules 1988-1997

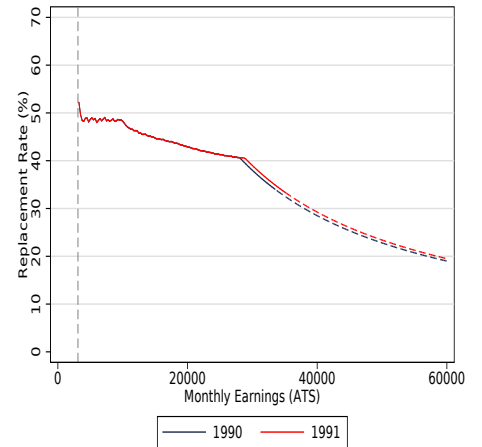
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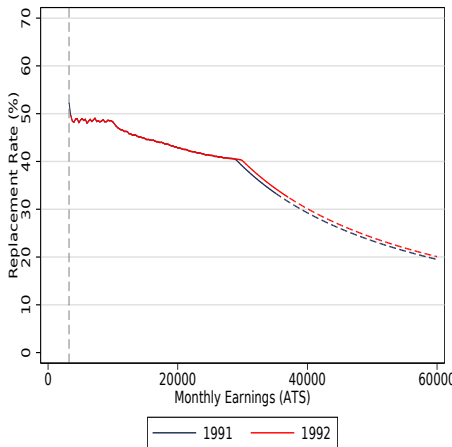
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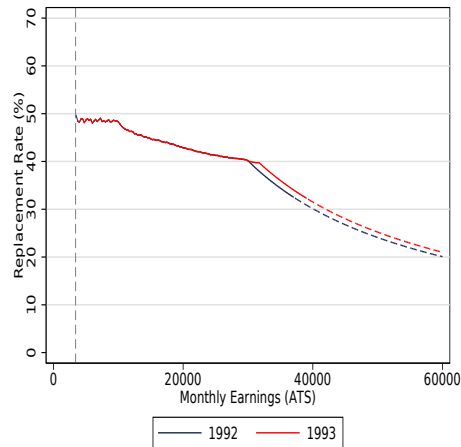
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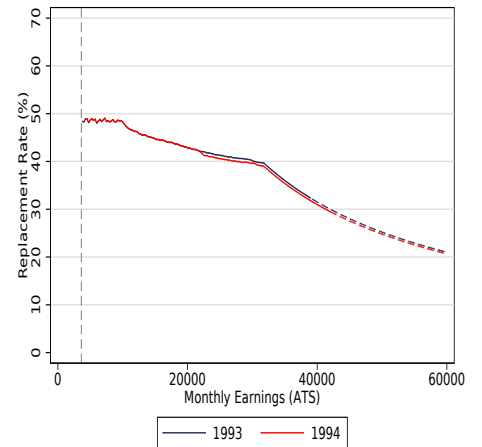
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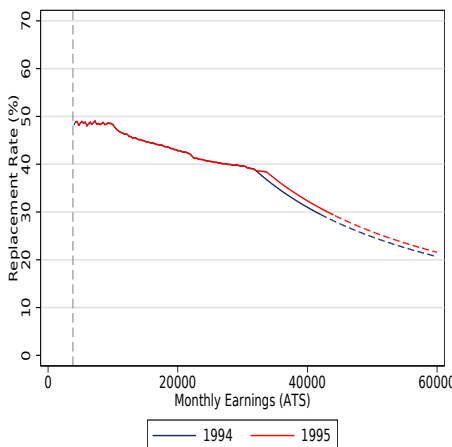
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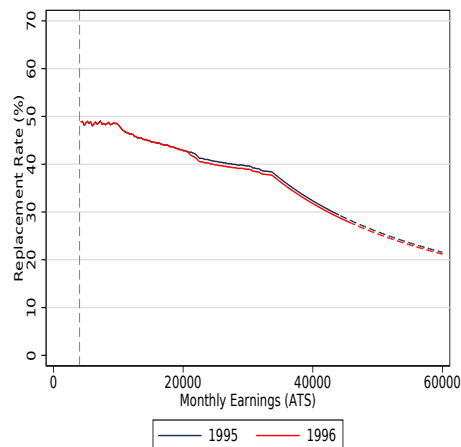
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1994 and 1995



1995 and 1996



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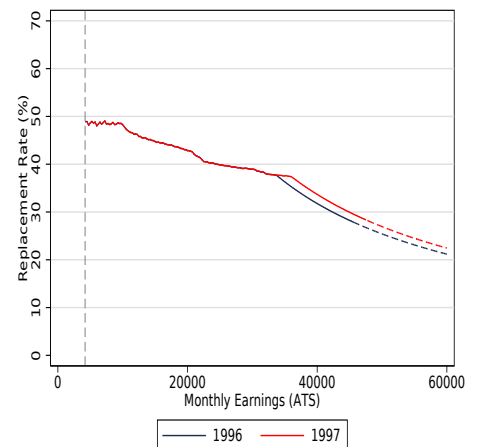
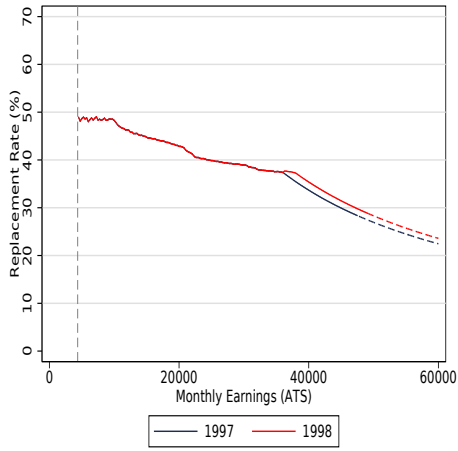
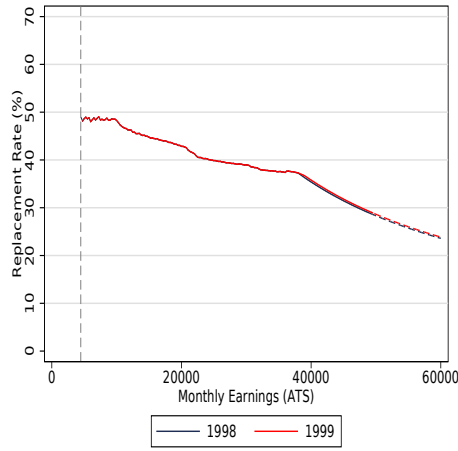


Figure A.17: Replacement Rate Schedules 1997-2003

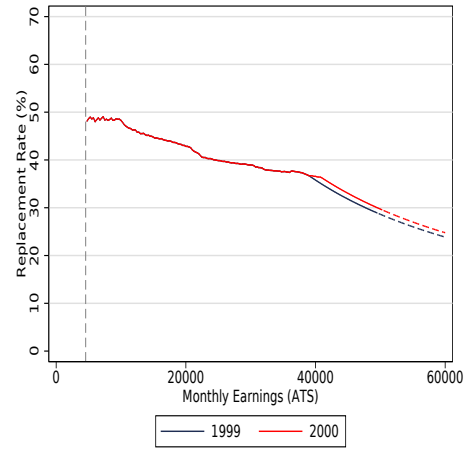
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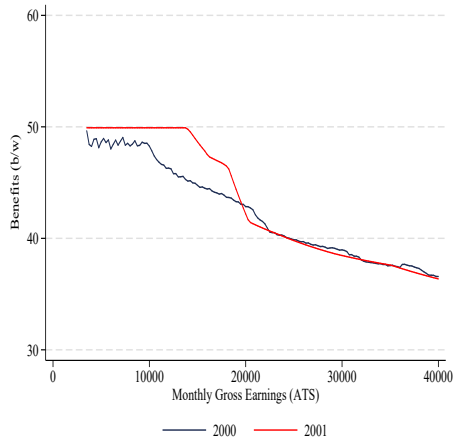
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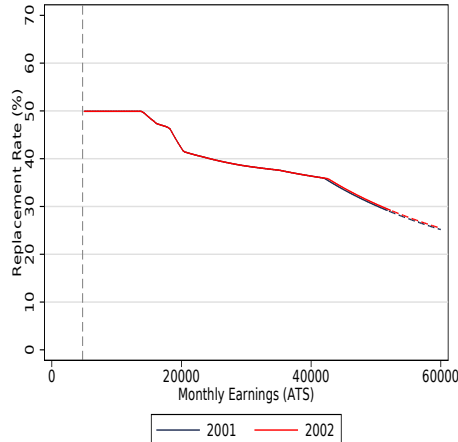
1999 and 2000



2000 and 2001



2001 and 2002



2002 and 2003

