Wages and the Value of Nonemployment*

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Abstract

Nonemployment is often posited as the outside option in macroeconomic models with wage bargaining and in models of labor market 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 the nonemployment option on employed workers' wages. Our variation in nonemployment values arises from four large reforms of UI benefit levels in Austria, which we study quasi-experimentally by measuring wage responses in existing and new jobs using administrative data. Our analysis reveals a precisely estimated, low sensitivity of wages to UI benefit levels ranging between 0 and 4 cents on the dollar. This insensitivity holds even among workers with a priori low bargaining power and for workers with low labor force attachment, in areas of high unemployment, with high predicted unemployment duration, and among recently unemployed workers, and despite high take-up and eligibility – factors that either eliminate confounders or ought to render wages even more sensitive to nonemployment values. The insensitivity holds for job stayers and job switchers and persists when we zoom out to the firm or industry as the bargaining unit. This insensitivity of wages to the nonemployment option presents a puzzle to widely used wage setting protocols in macroeconomics and implies that nonemployment scenarios may not constitute a relevant threat point in bargaining. Our evidence supports wage setting mechanisms that largely 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, and potential utility from leisure, constitutes workers' outside option in wage bargaining. The value of nonemployment is thus a core determinant of equilibrium employment levels.[1] The view that nonemployment values affect wages also helps explain aggregate wage fluctuations such as the cross-sectional wage curve and the aggregate Phillips curve, since high unemployment weakens workers' threat point, nonemployment.[2] This framework has shaped policy debates such as the wage pressure channel of unemployment insurance.[3] The value of nonemployment is also a cornerstone scaling the equilibrium wage distribution in modern wage posting models.[4] The theoretical sensitivity of wages to fluctuations in the value of unemployment also determines the capacity of macroeconomic models to generate realistic employment fluctuations.[5] Yet, there exists no direct empirical estimate of the sensitivity of wages to the value of nonemployment.

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

$$\text{Wage} = \phi \times \text{Productivity} + (1-\phi) \times \underbrace{\text{Nonemployment Value}}_{\text{UI Benefits } + \text{ Other}}$$

Shifts in the outside option such as from UIBs should pass through into wages by one minus worker's bargaining power, such that wages move one to one for workers with low bargaining power. This prediction holds across a broad set of model refinements of bargaining models. While our benchmark model is bargaining, the prediction extends to alternative models such as wage posting, where the value of nonemployment pushes up employed and unemployed workers'

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

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

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

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

[5] See, e.g., Shimer (2005), Hall and Milgrom (2008), and Hall (2017).

wages, due to workers' increase in reservation wages.

We document that real-world wages appear *insensitive* to sharp increases in workers' nonemployment values. This insensitivity presents a puzzle to core predictions of leading Nash bargaining models with nonemployment as the outside option, and supports wage setting models that are insulated from the nonemployment value. Specifically, our estimates for the sensitivity of wages to the value on nonemployment range from 0.00 to 0.05, such that a \$1 increase in workers' nonemployment value increases wages by at most \$0.05. This insensitivity of wages to the nonemployment value presents a puzzle to leading bargaining models, unless one is willing to believe that workers have close to *full* bargaining power ($\phi = 0.95$ or higher, in a static or myopic bargaining setting), an interpretation empirically rejected by existing small estimates of inside value effects on wages (0.1-0.2). We juxtapose these estimates with our implied bargaining power estimates in Figure 1. Instead of full worker bargaining power, our interpretation is that nonemployment is not the relevant outside option in wage bargaining.

The specific setting of our study is a unique set of four large reforms that generated quasi-experimental variation in UI benefit levels in Austria. Reforms to benefit levels are much rarer than changes in potential UI benefit duration, which do not provide a clear money metric of nonemployment value changes and are moreover vastly heterogeneous in workers' expected duration of unemployment. The reforms occurred in 1976, 1985, 1989 and 2001 and increased benefits for subsets of workers, e.g., by as much as 28% in 1985.[6] The impact of the reforms were sharply differentiated by workers' *previous* salary (the UI reference wage that determines UIBs in the event of a UI claim), permitting 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, leveraging the full set of UI reforms and tracing workers through various labor force statuses and transitions, with daily information on UI claims and benefit receipts, as well as wages.

Besides the unique set of UIB reforms, the Austrian UI system is a particularly suitable setting to obtain variation in nonemployment values for institutional reasons, allowing us to benchmark our findings against quantitative predictions from wage setting models. First, Austrian unemployment insurance shifts the nonemployment value for most workers. Conditional on a separation, we document that around 60% of workers receive UI due to broad eligibility and high take-up, likely due to mandatory registration (for continuity of health insurance coverage) and because of long unemployment spells. Second, Austrian workers who *quit* are eligible for benefits – crucial for UI to indeed shift workers' threat points.[7] Third, Austrian UI does not

[6]Reforms were large shifts in the real maximum benefit level (1985) or sharply increased the replacement rates in low parts of the earnings distribution. Replacement rates refer to the ratio of unemployment benefits to gross earnings before unemployment. To our knowledge, only the 1989 reform has been studied, with a focus on unemployment duration (Lalive et al. (2006)).

[7]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

feature experience rating.

We analyze the reforms nonparametrically and in a regression framework and find a wage sensitivity close to zero for both strategies. First, we present transparent scatter plots for various cross sections of *employed* workers differentially treated by each respective reform.[8] On the x-axis, we sort those workers by their UI reference wage, a pre-determined lagged wage that assigns the benefit changes to workers. On the first y-axis, we plot the reform-induced change in UIBs, which exhibits the sharp between-worker variation we exploit. On the second y-axis, we present wage growth before and after the reform. As a benchmark, we also plot the *predicted* effect on wage growth (UIB growth times one minus worker bargaining power calibrated to 0.05–0.2 implied by rent sharing estimates).[9] *If indeed nonemployment is workers' outside option in wage bargaining*, the large UIB treatments would predict clearly visible wage effects of multiple percentage points, tightly scaling the shape of the reform-induced benefit changes. A visual inspection of the raw data does not reveal such large wage increases, in any of the reforms we examine. Even after two full years, the wage gradients remain parallel to pre-reform years.

Next, we estimate the sensitivity of wages to UI-induced shifts in the nonemployment value in a regression-based difference-in-differences analysis drawing on data from all four reforms, which allows us to precisely measure even potentially small effects.The regression-based estimates confirm a tight range for the sensitivity between \$0.00 and at most \$0.05. 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. While our main specification features treatment and control workers that are currently employed, the insensitivity extends not only to stayers, but also to workers switching employers with and without intervening nonemployment spells, implying that wages even of new hires and in new employment relationships are insensitive to the nonemployment value.

We also conduct robustness checks and subsample analyses motivated by theory and potential identification concerns and find that the insensitivity extends across worker and firm subgroups, and states of the business cycle and cannot be explained by wage stickiness or limited salience. First, standard wage stickiness cannot explain the findings because wages remain unresponsive even after two or three years, and because our reforms increase generosity and should entail wage *increases*.[10] Moreover, when we split the firm sample by revealed dispersion in worker-level wage

months in Germany. Quitters in the United States are never eligible 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 potential de-facto UI eligibility of quitters in U.S.

[8] See Figures 3 to 6.

[9] Empirical examples include Abowd and Lemieux (1993) who find an elasticity of around 0.2; Card et al. (2018) provide a meta study of evidence from a variety of settings and propose 0.05–0.15 as a reduced-form elasticity that we scale into a bargaining power upper bound in Appendix D and visualize in Figure 1.

[10] Adjusting for wage stickiness would have only halved the observed response in year one because half of wage

growth, even firms with flexible wage policies do not adjust wages for the workers with improved nonemployment options. 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 at higher future risk of becoming unemployed or UI take-up or with recent UI receipt, who are thus plausibly more aware of the UIB schedule (see Lemieux et al. (1995) and Lemieux and MacLeod (2000)). As a complement to our analysis of benefit *levels*, we document that reform-induced increases in benefit *duration* do not increase wages among incumbents either. Finally, our robustness checks also rule out offsetting composition effects from attrition (turnover) or efficiency wage mechanisms through which outside option changes could affect worker effort.

We test and reject a potential alternative explanation for our findings—bargaining occurring at the firm or industry rather than the individual level—by showing that wages are only slightly ore responsive to reform-induced benefit shifts at the firm or industry level.[11] We do so by measuring the wage responses to benefit changes aggregated at the firm or industry level. We find that these more aggregate shifts in workers' nonemployment value yield wage sensitivities that are quantitatively similar to those measured at the individual levels. Finally, while Austria is heavily unionized, it leaves substantial room for idiosyncratic deviation from the collective wage floors: actual wages are more than a third higher than the wage floors set by central bargaining agreements (Leoni and Pollan (2011)), suggesting substantical scope for firm-level or idiosyncratic negotiations.

Alternatively interpreted, our evidence may be consistent with wage setting mechanisms featuring no bargaining, although several 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. In addition, as bargaining theory would predict, we find (slightly) larger wage sensitivity among workers with plausibly lower bargaining power, e.g., in blue-collar occupations. To add, we find evidence that women's wages are more sensitive to outside option changes—mirroring findings of lower sensitivity for women to inside option changes (Card et al. (2015)) consistent with lower bargaining power among women. Nonetheless, the effect size remains small at no more than

contracts appear to reset each year (see, e.g., Barattieri et al. (2014) for the United States, and Sigurdsson and Sigurdardottir (2016) for Iceland, or a meta study of new and incumbent workers Pissarides (2009)). Relatedly, Schmieder et al. (2016) and Nekoei and Weber (2017) document zero or small reemployment wages among the unemployed from potential benefit duration changes, likely due to selection rather than bargaining. Finally, the empirical literature on inside-option rent sharing documents contemporaneous wage effects for incumbent workers, in one instance *only* for incumbent workers (Kline et al. (2017)).

[11] Saez et al. (2017) find that incidence of targeted payroll tax cuts does not pass through into treated worker's wages idiosyncratically, but show up as broad-based firm-level rent sharing. By contrast, worker-level wages *can* reflect some idiosyncratic factors, such as group-specific marginal product shifts due to worker exits (Jäger (2016)). A long literature has documented cross-sectional wage differentiation between observationally similar workers even within the same firm (Abowd et al. (1999)). Carneiro et al. (2012) documents cyclical within-firm wage differentiation between new and incumbent workers in the same jobs.

6 cents on the dollar relative to a theoretical benchmark with nonemployment as bargaining outside option.

Alternative bargaining protocols in which employed workers wield other job offers as their outside options are a promising candidate to explain our main finding of wage insensitivity to nonemployment changes, although 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), and Bagger et al. (2014)), *recently reemployed* workers – for lack of other job offers – need to resort to nonemployment as outside option – and therefore renegotiate wages upwards when UI benefits increase, just as in our benchmark model. In the data, we find slightly larger wage effects among recently unemployed workers, although not for workers transitioning through unemployment during the reform periods. Overall, the magnitude of the wage sensitivity remains small at no more than 4 cents on the dollar even among workers coming out of nonemployment and thus suggests that nonemployment does not appear to serve as the outside options even among those workers.

In conclusion, the insensitivity of wages to UI-induced shifts in the nonemployment value indicates that the nonemployment scenario is not the relevant outside option in wage bargaining. The evidence therefore favors models of wage setting where wages are insulated from the nonemployment value. Promising classes of models consistent with our evidence include alternative bargaining protocols that reduce the sensitivity of wages to the nonemployment option, in particular Hall and Milgrom (2008).[12] Alternative theories of wage setting incorporate wage posting rather than bargaining. However, while a calibration of the baseline wage posting model with pure wage dispersion à la Burdett and Mortensen (1998) yields smaller wage sensitivity to UI and is quantitatively in line with our main results, larger sensitivities emerge with richer models that match empirical wage distributions (e.g. with firm heterogeneity) and are thus harder to reconcile with the evidence we present. We thus conclude that the insensitivity of wage setting to the nonemployment scenario may constitute a puzzle to a broader class of wage setting models.

Outline. In Section 2, we discuss the role of nonemployment values in variants of wage bargaining models. We describe institutional details, the reforms, and the data in Section 3. In 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 discuss implications of our findings for models of wage setting. We conclude in Section 7.

Literature review: existing evidence on the effect of nonemployment value on wages. Hagedorn et al. (2013) study the effect of potential benefit duration (PBD) extensions in the

[12]Building on results in Rubinstein (1982), Rubinstein and Wolinsky (1985) and Binmore et al. (1986), Hall and Milgrom (2008) 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.

United States for the geographical dispersion in unemployment and its dynamics during the Great Recession, focusing on hiring effects of the wage pressure channel. They also include a reduced-form investigation of equilibrium (macro) wage effects among stayers.[13] By contrast, we use UI benefits as a source of identification and intentionally select a particular setting where take-up is very high, whereas e.g. in the United States quitters are de-jure ineligible for UI.[14] Schmieder et al. (2016) and Nekoei and Weber (2017) study the effects of UI PBD on unemployment duration and reemployment wages in Germany and Austria respectively, finding a weakly negative and a small positive relationship respectively between UI duration and post-unemployment wages. To add, Schmieder et al. (2016) study the effect of UI duration on reemployment wages conditional on unemployment duration and find no wage effect. To the extent that the McCall reservation wage channel exerts a positive effects on wages, this finding suggests a limited effect of bargaining on wages among unemployed workers. Nekoei and Weber (2017) find evidence that the positive wage effects they document are associated with individuals having a higher chance of reemployment at higher-paying firms, lending support to a McCall reservation wage channel rather than a bargaining mechanism. In addition, Le Barbanchon et al. (2017) find that variation in potential benefit duration is not associated with reservation wage differences among unemployed job-seekers in France. 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.

Blanchflower and Oswald (1994) discuss an empirical regularity known as the wage curve: the negative relationship between incumbent workers' wages and unemployment that extends to local labor markets. Winter-Ebmer (1996) confirms the wage curve relationship for the case of Austria. A leading interpretation of the wage curve relationship is the bargaining channel, since high unemployment weakens workers' threat point by making nonemployment less valuable. However, the reduced-form pattern may reflect a variety of mechanisms (see Card (1995) for a discussion). Related evidence includes the effect of contemporaneous and past labor market conditions on incumbent workers' wages (Beaudry and DiNardo (1991)), whereas interpretations alternative to the (re-)bargaining one include, e.g., selection of match quality along a job ladder (Hagedorn and Manovskii (2013)).

[13]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 decisions.

[14]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.

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 statistic to guide our empirical analysis. We first derive the Nash bargaining problem and the resulting wage in a simple one-period game. We show that one minus worker bargaining power is the pass-through of outside options into wages. We then show that this basic relationship is obtained in a wide variety of richer model variants common in the literature. To calibrate this expression, we briefly review existing work on inside and outside options, which directly implies high wage sensitivity under the assumption when nonemployment is the outside option in bargaining.

A natural question arises about 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.

2.1 Wage Bargaining in Single-Period Jobs

Simplified, static environment. We first discuss a baseline model to illustrate the role of the nonemployment value in wage bargaining. Our model mirrors a canonical derivation discussed, e.g., in Manning (2011). Effectively, the model considers a single-period employment relationship with quasi-linear utility on the worker's part. Worker i's productivity is p_i, she does not incur disutility of labor, and her outside option is Ω_i. In the simplified setup, the firm's outside option is zero. Due to sunk investments (e.g., specific human capital, or search frictions), $p_i > \Omega_i$, such that the job has strictly positive joint match surplus. In this situation, a variety of wages would implement the bilaterally efficient allocation of employment: the worker would accept any wage of at least Ω_i, and the firm would accept any wage up to p_i. How the joint surplus, or rent, $p_i - \Omega_i$ is split between the parties is therefore not obvious – quantitatively and by which mechanism. Bargaining models solve this distribution problem as well as make clear predictions for wage setting, in form of a wage bargain w within the bargaining set $[\Omega_i, p_i]$ formed by the worker's and firm's reservation wages.

Nash bargaining over the wage. The Nash bargaining protocol follows a simple derivation and generates tractable wage solutions. We illustrate the protocol in a simple economic setting that we extend to richer contexts in Section C.1.[15] The firm and the worker bargain over joint job surplus: the inside value of the job MPL_i in excess of the sum of the parties' outside options.

[15] We naturally consider generalized Nash bargaining (worker bargaining power $0 \leq \phi \leq 1$), rather than symmetric bargaining ($\phi = 0.5$).

They do so by transferring utility via wage w:

$$w_i^N = \arg\max_w (w - \Omega_i)^\phi \times (p_i - w)^{1-\phi} \tag{1}$$

The resulting Nash wage allocates the worker her outside option Ω_i, plus a share of the surplus $MPL_i - \Omega_i$ determined by bargaining power ϕ. Alternatively, the Nash wage is the ϕ-weighted sum of the inside value of the job (productivity) and the outside option of the worker:

$$\Rightarrow w_i^N = \Omega_i + \phi \times \overbrace{(p_i - \Omega_i)}^{\text{Job surplus}} \tag{2}$$

$$= (1 - \phi) \times \Omega_i + \phi \times p_i \tag{3}$$

The sensitivity of wages to the outside option. The overarching implication of wage bargaining is that wages comove $1 - \phi$ to one with shifts in the outside option:

$$\frac{dw_i^N}{\Omega_i} = 1 - \phi \tag{4}$$

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.

The nonemployment outside option. We focus on measuring the effect of outside options Ω_i on wages, thereby estimating $1 - \phi$ rather than ϕ. The commonly modeled outside option income is (at least a brief spell) of nonemployment, and therefore contains unemployment benefits b plus alternative components of the nonemployment value y_i, such as any other nonemployment-contingent income, reemployment opportunities, an extensive-margin version of the marginal rate of substitution, stigma or psychological costs from nonemployment, and a variety of other factors separate from UI receipt.

Assuming that the outside option is entering nonemployment is congruent with the canonical implementation of Nash bargaining in, e.g., matching models, where job surplus is the difference between worker productivity and non-employment value (e.g., Pissarides (2000), Shimer (2005), Chodorow-Reich and Karabarbounis (2016), or Ljungqvist and Sargent (2017)). Despite this common specification, the role of non-employment outside options on wages is not obvious; we discuss alternative models in Section 6.2.

Having specified the outside option, the resulting wage is:

$$w_i^N = \phi \times p_i + (1 - \phi) \times [b_i + y_i] \tag{5}$$

Empirical strategy: variation in the nonemployment value from UI benefit levels. Our empirical strategy exploits quasi-experimental and easily quantifiable variation in b_i as a dollar-valued shifted in the value of nonemployment. Bargaining with nonemployment as the outside option then predicts a simple linear relationship between the change in the benefit level for worker i, db_i and the change in her wages dw_i:

$$dw_i = (1 - \phi) \times db_i \tag{6}$$

In practice, our estimation comes in form of a straightforward treatment effect estimator, relating *between-worker* variation in benefit levels with worker-level wages as the outcome variable.

Importantly, this is a *dollar-for-dollar sensitivity in levels*, not requiring us to take a stance on the relative importance of b in workers' total nonemployment value $b_i +$
y_i, overcoming a challenge in theory (Ljungqvist and Sargent (2017)) and measurement (Chodorow-Reich and Karabarbounis (2016)).

Ideal empirical setting: comprehensive eligibility and perfect take-up of UI benefits. 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, and a high take-up rate.[16]

Benchmark: calibrating the sensitivity in Nash using existing estimates worker bargaining power. While there is limited evidence on the effect of outside options on wages, Nash bargaining also prescribes a tight link between inside value (proxies in the data: profits and productivity) and wages, guided by ϕ (rather than $1 - \phi$) for outside options). A large body of work in labor economics investigates the pass-through of firm-specific shifts in the marginal product of labor p_i into worker's wages (e.g., Manning (2011) and Card et al. (2018) review the empirical literature).[17] In Figure 1, we plot these implied worker bargaining power parameters from rent sharing estimates. Under the assumption of Nash bargaining and nonemployment as the outside option, these results imply large (≥ 0.8) sensitivity of wages to nonemployment value shifts. We also include macroeconomic calibrations of worker bargaining power, which are

[16] See Hagedorn et al. (2013) and Chodorow-Reich et al. (2018) for a discussion of whether UI shifts the outside option for U.S. workers.

[17] The reduced-form elasticities is between 0.05 and 0.2. 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 estimate is an *upper bound* for ϕ.

larger, though largely treated as a free parameter.[18] Under the assumption that nonemployment values are the outside option in bargaining, these estimates imply *high* sensitivity of wages to the *outside* option, of 0.8 and above.

Under the crucial assumption that the nonemployment option indeed constitutes outside options in wage bargaining, our analysis would identify the bargaining power parameter:

$$\widehat{\phi} = 1 - \frac{dw_i^N}{db_i} \tag{7}$$

Reading the potential wage responses through this lens yields a useful quantitative interpretation: we can judge the *implied* bargaining power parameter and compare it to existing estimates and calibrations. In Figure 1, we therefore preview our key result by plotting our implied worker bargaining power parameter: $\hat{\phi} \geq 0.9$, as we find wages to be insensitive to variations in the nonemployment flow value b. That is, our preferred interpretation is that we reject Nash bargaining with nonemployment as the relevant outside option, implying that real-world wage setting follows protocols that largely insulate wages from the nonemployment value.

2.2 Robustness of the Strategy Across Model Variants

Next, we show that the key prediction from the static benchmark model carries over to a wide variety of richer models considered in the literature. In Section 6 we additionally discuss a small set of models in which the relationship does *not* hold, and which may therefore rationalize the zero effect of b on w that we document in the empirical Section 4.

The model variants here builds on Appendix Section C.1, where we present a model with richer features, such as long-term jobs and thus dynamics, potentially multi-worker firms using a multi-factor production function,nonlinear consumption utility (which generates budget multiplier λ), and additively separable disutility of labor γ.

I. Myopic bargaining ($\beta = 0$) or single-period jobs ($\delta = 1$) between an individual household and a multi-worker firm. The myopic Nash wage returns the simple wage rule: the average of the outside option (unemployment benefits b plus the marginal rate of substitution, i.e. the disutility of labor γ divided by the marginal utility of consumption λ), and the marginal

[18] This view is the textbook summary of macroeconomic calibrations of working bargaining power in Cahuc and Zylberberg (2004): "We do not have at our disposal a reliable order of magnitude representing the bargaining power of workers γ. [...] The usual procedure is to assume that [bargaining power parameter] γ is equal to the elasticity of the matching function with respect to the unemployment rate." Deviations from standard Nash are therefore often justified on the basis of improved matching of aggregate time series rather than on the basis of direct micro-empirical validation (e.g. Christiano et al. (2016)). Our paper provides causally identified evidence for insensitivity of wages to nonemployment values, and thus complements the macro evidence.

product of labor – weighted by worker bargaining power ϕ:

$$w^N_{\beta=0} = (1-\phi)\left(b + \frac{\gamma}{\lambda}\right) + \phi F_N(K_t, N_t) \tag{8}$$

II. Dynamic considerations. Our full model, which we spell out in Appendix Section C.1, recognizes the long-term nature of jobs and dynamic consideration of the actors. Wages then not only reflect current conditions but also expectations about future inside and outside values, through the continuation values. An implication of Nash bargaining to apply also in subsequent periods and free entry for labor demand renders the Nash wage identical to the myopic thought experiment except for one continuation term:

$$w^N = (1-\phi)\left(b + \frac{\gamma}{\lambda}\right) + \phi F_N(K_t, N_t) + (1-\phi) f \beta \mathbb{E}_t \frac{\widetilde{W}_{t+1} - U_{t+1}}{\lambda} \tag{9}$$

The first components precisely mirror the standard quasi-spot conditions in myopic wage bargain (A21). That is, *over the course of the job at hand*, the worker's idiosyncratic variation in outside option b pass through into the Nash wage w^N exactly through $(1-\phi)$.

The continuation term captures dynamic considerations of the outside option of unemployment, specifically the prospect of reemployment.[19] This term, and its shift in response to b, is naturally more difficult to price than the flow terms, and it moreover depends on assumptions about market structure, the shape of labor supply and labor demand, and thus various potential margins in the equilibrium adjustment to b.[20] Generally, this term decreases in b, attenuating the wage response to b, in part due to equilibrium concerns that may or may not be netted out by our difference-in-differences design due to treatment and assumably closely aligned control groups. The particular attenuation depends on calibration choices as well as the particular model structure assumed. For example, allowing for endogenous labor supply, with a cost of labor force participation margin e.g. arising from a potentially heterogeneous disutility of search, an indifference condition for all workers or a marginal participant is $\beta f(W-U) = O - U$, where O denotes the value of being out of the labor force (and where the flow value may include b).

In DMP models, this worker-facing term is replaced with firm's value of a filled job (recognizing the Nash sharing rule such that $(1-\phi)\beta f(W-U) = (1-\phi)\phi\beta J$); depending on the recruitment cost specification, this term becomes a function of labor market tightness, as we discuss next.

[19] The derivation recognizes that other, $(1-\delta)$-weighted, continuation terms on the firm and worker side cancel out, since $\phi\beta\mathbb{E}_t(W_{t+1} - U_{t+1}) = (1-\phi)\beta\mathbb{E}_t V'_{t+1}(N_t)$ by Nash bargaining in $t+1$ *in the job at hand*:

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

[20] We have implicitly eliminated the analogous term on the firm side, containing the vacancy value V, due to the assumption of free entry, implying that the value of a vacant job is pushed to zero, and thus insensitive to b.

III. DMP model with free entry in vacancy posting. Next, we show that our model nests the DMP model of matching frictions with five specializations: (i) linear utility, (ii) linear production in labor only i.e. $F_N(K_t, N_t) = p_t$, (iii) single-worker firms, (iv) hiring costs determined by $c_{\text{DMP}}(H) = H \cdot k/q$ and $c'_{\text{DMP}}(H) = k/q$, where k is the vacancy posting cost and q is the vacancy filling cost, and $q = q(\theta_t)$, and (v) $f = f(\theta_t)$ with a constant-returns matching function such that $\frac{f(\theta_t)}{q(\theta_t)} = \theta_t$. Free entry has firms post vacancies until the value of vacancies is pushed to zero, which in our case implies: $c'_{\text{DMP}}(H_t) = \frac{k}{q_t} = \beta \mathbb{E}_t \frac{\partial V^F_{t+1}(N_{t+1})}{\partial N_{t+1}} = \beta \mathbb{E}_t J_{t+1}$. We can then replace the continuation term in condition (A22), $(1-\phi) f \beta \mathbb{E}_t (\widetilde{W}_{t+1} - U_{t+1})/\lambda$, by $\phi f \beta \mathbb{E}_t J_{t+1} = fk/q_t$, where the last reformulation arises from free entry. Due to the CRS matching function, $\theta_t = v_t/u_t$ (the ratio of vacancies to unemployment) and with random search, $f(\theta_t)/q(\theta_t) = \theta_t$, the canonical DMP Nash wage emerges:

$$w^N_{\text{DMP}} = (1-\phi)\left(b + \frac{\gamma}{\lambda}\right) + \phi p_t + \phi \theta_t k \tag{10}$$

The intuitions for wage setting and our empirical identification of $1-\phi$ are preserved, but the reformulation of the continuation terms in terms of θ_t in expression (10) shrouds the worker-facing search origins of this term in comparison to our more general formulation of the Nash wage (A22).

IV. Stole and Zwiebel (1996) bargaining with multi-worker firms. We consider bilateral bargaining. Extensions to multi-worker contexts highlight the tensions that the splitting of the *inside* option entails with multi-worker firms. These considerations largely arise from the diminishing returns in production in the labor input. For a parametric version assuming Cobb-Douglas production and allowing for differences in TFP x_f with curvature α, we present a discrete-time analogue of the augmented Nash condition by Acemoglu and Hawkins (2014), who build on Stole and Zwiebel (1996), which we augment with a worker-specific outside option b_i, indicating that despite multi-worker bargaining considerations, *idiosyncratic*, worker-specific outside options still pass through into wages as in the baseline model:[21]

$$w^N(b_i; x_f, n_f) = (1-\phi) b_i + \frac{\alpha\phi}{1-\phi+\alpha\phi} \cdot x_f \cdot n_f^{\alpha-1} + (1-\phi) f \beta \mathbb{E}_t \frac{\widetilde{W}_{t+1} - U_{t+1}}{\lambda} \tag{11}$$

where expectation $\mathbb{E}_t$ is now also taken over the heterogeneity of firm productivities and sizes in potential subsequent jobs. Multi-worker firm bargaining leaves the role of outside options intact in wage setting and primarily augments the role of the marginal product.[22]

[21] 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 $w_i(\mathrm{N}) = (1-\beta) r U_i + \int_0^1 z^{\frac{1-\beta}{\beta}} F_i(\mathrm{N}z)\, dz$.

[22] 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 $\alpha < 1$.

V. Representative household with full insurance. 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). Our individual household bridges these setups with the assumption of perfect capital markets (and negligibly long unemployment spells). The fictional representative household is particularly easy to handle with homogeneous workers, although our sample of real-world households have different earnings potentials that ultimately in our Austrian experiment will render them eligible or not for the shifts in b_i. It would be a trivial extension of the representative household setup to consider households to be grouped by earnings and thus applicable group-specific b_i.

VI. Endogenous separations. Similar expressions of Nash bargaining hold in models that allow for the potential of endogenous separations among existing jobs. Inframarginal surviving matches, i.e. those that we track in the data, will exhibit the same pass-through of b_i into wages. The same Nash expression arises but now takes explicitly idiosyncratic productivity p_i as the argument, and expectations $\mathbb{E}_t$ about continuation values and reemployment opportunities also capture heterogeneity in job quality.[23]

VIII. 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 In this setup, the worker simply transitions into the higher-productivity (higher surplus) match whenever presented this opportunity (net off a potential switching cost); but conditional on a transition or no transition, nonemployment is the outside option in wage bargaining. This tractable route is taken by for example Mortensen and Nagypal (2007) and Fujita and Ramey (2012). We discuss the implications of allowing for outside options in bargaining to be determined by competing job offers during on-the-job search in Section 6.2. In this model however, new hires from nonemployment maintain nonemployment as their outside option in their initial bargain, where wages thus follow our baseline model. We do not find evidence for stronger wage sensitivity to this sample in our data (although this particular prediction is more difficult to test).

IX. Additional components of the nonemployment value, e.g. UI take-up costs, skill depreciation, job ladders, stigma, work attachment. Another dynamic dimension the

[23] b would, however, be reflected in equilibrium labor demand (or supply) and in continuation terms. b_i will also change the threshold at which matches are formed, maintained or destroyed. Jäger et al. (2018) study a large extension of potential duration of UIBs for older and document substantial separation responses of that policy, perhaps using the extension as a bridge into early retirement. In this paper, we do not detect significant separation effects to increases in b.

baseline model ignores are other components of the opportunity cost of nonemployment vis-à-vis employment. The baseline model focuses on the differential between the market wage and the benefit level b (or any other non-employment-contingent benefit). Additional components of the cost may include skill depreciation, fall from the job ladder, stigma, or other psychological costs associated with the unemployment state. In fact, these additional components have shown to be crucial in explaining the low level of reservation wage behavior of unemployment job seekers (Hornstein et al. (2011)). Importantly these factors shift the *level* of the value of nonemployment downward, and therefore increase surplus. These factors may help explain high job finding rates or low quit rates. However, the essence of bargaining and job surplus is that even in inframarginal matches, shifts in the size of the surplus is split by means of wage bargains. Therefore, shifts in the UI component of nonemployment will still pass through one to one into the nonemployment value. Finally, our research design uses a *level* specification, estimating a dollar-for-dollar sensitivity, rather than an elasticity. The share of UI in the total nonemployment value is therefore not necessary to compute, a key strength of our design. (Measuring the level or cyclicality of the *total* nonemployment value is considerably more complex, and done in Chodorow-Reich and Karabarbounis (2016) and Chodorow-Reich et al. (2018).)

X. Finite vs. infinite potential benefit duration. While a common approach is to model benefits as having infinite potential duration, it is limited in Austria just as in other UI systems, as we discuss in Section 3. To show robustness to this assumption, we derive and discuss this model variation in Appendix Section C.2. This extended model mirrors Austria's two-level approach of generous constant UI benefits b; after the PBD is exhausted, the still-jobless worker moves into a post-UI substitute $s < b$ (with limited eligibility and lower level, potentially moving in lock-step with the shift in b. We show that from the perspective of an incumbent worker, the attenuation of the wage effects of shifts in b is negligible, because her outside option would start off with *full* potential duration. For our design focusing on *incumbent workers*, infinite benefit duration therefore turns out to be a particularly good approximation.[24] Indeed, in Austria, only 10% of unemployment spells exhaust the PBD, implying that accounting for finite benefite duration would not quantitatively alter the interpretation of our main result as being at odds with models of wage bargaining with nonemployment outside options. Perhaps this fact also explains why we also do not find wage effects of from potential benefit extensions in Section 5.3. In Section 5.1, we additionally test whether workers with differential PBD exhibit different wage sensitivity to the unemployment benefit level, which also forms an empirical test for the sensitivity of our results to variation in PBD.

[24]By contrast, a research design focusing on the potentially duration-dependence strategies of already-*unemployed* job seekers complicates measurement due to dynamic selection (e.g. Schmieder et al. (2016)).

XI. 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 τ, then changes in *net* benefits b enter the worker's outside option relatively as $\frac{b}{1-\tau}$. For $\tau \approx 0.3$, accounting for the tax system would therefore *amplify* the predicted sensitivity of wages to b by $\frac{1}{1-0.3} \approx 1.41$ for any given ϕ. (Analogously, a given wage response will, structurally interpreted in a model of Nash bargaining with nonemployment as the 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 potentially 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. At a broad level, UI benefits in Austria are governed by the potential duration of benefits (PBD) and the benefit levels (UIB). 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.[25] Individuals whose UI benefits have expired or who were initially ineligible can apply for means-tested transfer payments ("Notstandshilfe").[26]

[25]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).

[26]This benefit is capped at 92% of unemployment benefits. For 1990, Lalive et al. (2006) report that median "Notstandshilfe" was about 70 % of the median UIB. Based on data from 2004, Card et al. (2007) gauge the average "Notstandshilfe" at 38 % of UIB for the typical job loser.

Financing of benefits. There is no experience rating in Austria because the 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. UI benefit payments in Austria are not means tested but 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.16. 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% above a minimum benefit level and up to a maximum benefit level and our analysis builds on a series of reforms to replacement rates and the maximum benefit level. The net replacement rate $\frac{b_i}{(1-\tau_i)w_i}$ is linear and 55% for most earners starting in 2001; before 2001, the benefit schedule refers to 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 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.[27] 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. A consequence of broad eligibility, relatively long unemployment 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

[27]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.

low eligibility and take-up potentially attenuate the role of UI benefits in the nonemployment value, see e.g. Chodorow-Reich and Karabarbounis (2016)). We quantify take-up in Appendix Section E.3, where we conclude that more than 60% of workers separating will take up UI – unconditionally on the particular reason of separation. We reach this conclusion by tracking workers separating from a job and entering employment and focusing on a sample with more than 28 days between two employment spells. Among the workers eventually returning into employment over time, we also plot the survival rate of workers not having taken up UI as well as the survival function of unemployed workers returning to work. This count includes separators that perhaps separated for reasons such as maternity leave or retirement (with return to work), and therefore likely provides a conservative statistic for take-up.

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 because some workers became eligible for higher UI benefits as a consequence of the reforms.[28] 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 the 1976 reform or an increase in the maximum benefit level by 28% in 1985. Figure 2 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 Figure, we describe the new schedule induced by the reform, and compare it to the most recent pre-reform schedule in the previous year.

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

[28]Between 1974 and 1989, the schedule had a flat rate of around 41%, between a minimum benefit level and a maximum benefit level, leading to the from these thresholds, generating substantial variation in the replacement rate as a function of gross earnings. In 1989, a "hump" was introduced for low earners above the minimum benefit level, whose rate was increased to almost 50%. This the right side of this hump was smoothed out in 1990, such that through 2000, the flat portion of the schedule was tilted into a downward sloping line in gross earnings. Since 1990, the schedule has remained fairly stable except for small adjustments in the maximum benefit level. In 2001, the system switched from gross of tax to net of tax reference wages, generating a flat (55%) replacement rate between two kinks (maximum and minimum benefit levels, then adjusting in lockstep with aggregate wage growth).

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 that we study in detail.

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 2.(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.[29]

1985 reform. In January 1985, the maximum monthly UI benefit increased by 29% from around 7,600 ATS to around 9,800 ATS. Figure 2.(b) shows that this increase in the cap caused an increase in the replacement for individuals above the 54th percentile. This resulted in a replacement rate increase for these individuals of up to 8 ppt.[30]

1989 reform. On August 1st, 1989, reforms were enacted that increased benefits for low earners, depicted in Figure 2.(b).[31] 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 the monthly UI benefit.[32]

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 references to from *gross* wage earnings. The income tax schedule generated tremendous variation in benefits, in particular for lower earners below

[29] 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, and relegate maximum benefit extension analysis to the 1985 reform.

[30] 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.

[31] 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.

[32] 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.

the 26th percentile of the earnings distribution. Figure 2.(d), cast in terms of gross earnings, illustrates this variation at the lower part of the earnings distribution.

3.3 Data Description

Our primary data source is the Austrian Social Security Database (ASSD), described in Zweimueller 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. Annual earnings are defined to include two additional bonus payments received in May or June and December and 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 Zweimueller et al. (2009)).[33] 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.

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 even mandates for growth of actually paid wages would not be constrained upward.

[33]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.

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 benefit changes on wages. Pooling the four reforms, we find that a \$1.00 increase in benefits raises wages in existing matches by \$0.02 after one year and of \$0.07 after two years. Interpreted through the standard Nash bargaining framework with nonemployment as the outside option, this result implies a bargaining power parameter of $\hat{\phi} = 0.98$ or $\hat{\phi} = 0.93$. 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 at least a \$0.80 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 σ, a *dollar-for-dollar sensitivity* of wages to the nonemployment value by comparing variation in UI benefits $db_{i,t}$ with wage changes $dw_{i,t} = w_{i,t} - w_{i,t-1}$:

$$dw_{i,t} = \sigma \cdot db_{i,t}. \tag{12}$$

Our initial restriction will be that σ 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:

$$\Leftrightarrow \frac{dw_{i,t}}{w_{i,t-1}} = \sigma \cdot \frac{db_{i,t}}{w_{i,t-1}}. \tag{13}$$

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 σ 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. In a bargaining framework with nonemployment as outside option, the effect of nonemployment values on wages is guided by one minus worker bargaining power ϕ, as described by our structural bargaining Equation (6). We can provide an economic benchmark of this effect with the calibration to a bargaining framework, by which $\sigma^{\text{Nash \& NE OO}} = 1 - \phi$. Existing estimates of rent-sharing elasticities imply $\phi \leq 0.2$ and thus $\sigma \geq 0.8$ (see the meta study in Figure 1). In other words, the framework directly predicts a \$0.80 on the dollar effect of shifts in nonemployment values on wages.

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 $b_t(.)$. 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: $b_{t-1}(\mathbf{x}_{i,t})$:

$$db_{i,t} = b_t(\mathbf{x}_{i,t}) - b_{t-1}(\mathbf{x}_{i,t}). \tag{14}$$

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.[34] 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:

$$db_{i,t}(\tilde{w}_{i,t}) = b_t(\tilde{w}_{i,t}) - b_{t-1}(\tilde{w}_{i,t}). \tag{15}$$

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 $w_{i,t}$. 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. Second, we require that individuals are employed for 12 months out of the base year relative to which we calculate wage growth.[35] We impose this sample restriction because we are interested

[34] 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.

[35] 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

in actual *wage growth* among workers with high labor force attachment. 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.[36] 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.[37]

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 by construction not affected themselves 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* $t-1$: $\tilde{w}_{i,t}^{t\geq 1996} = w_{i,t-1}$.[38] 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 *month's* wage.[39] 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 $\bar{g}_{t,t-1}$

want to exclude them from the treatment and control regions.

[36]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.

[37]The treatment (T) and control (C) regions for the four reforms that we analyze are:
1976: T: 1201 to 3,700 ATS, 209,848 person-years, C: 3,700 to 6,730 ATS, 206,518 person-years 1985: T: 17566 to 25282 ATS, 2,060,721 person-years, C: 10929 to 16481 ATS, 2,061,255 person-years
1989: T: 3966 to 11864 ATS, 1,066,227 person-years, C: 11822 to 15658 ATS, 1,057,168 person-years
2001: T: 9976 to 20514 ATS, 1,861,022 person-years, C: 20460 to 26919 ATS, 1,849,468 person-years

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

[39]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.

by taking the average of individual nominal wage growth $g_{i,t,t-1} = w_{i,t}/w_{i,t-1}$.[40] This simple wage inflation procedure almost perfectly predicts wages and thus benefit levels.[41]

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.

4.3.1 Graphical Analysis

2001 reform: large benefit increase for lower earners. Figure 3 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.[42] We group

[40] 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.

[41] 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.

[42] The benefit schedule $b_{2001}(.)$ is a function of *net* earnings (while $b_{2000}(.)$ is a function of gross earnings, as with all schedules through 2000). We use a tax calculator to translate gross earnings (which our administrative data provide) into net earnings to compute $b_{2001}(.)$. To keep our wage concept plotted on the x-axis consistent

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 asses 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 wage effects at each percentile by calculating, e.g., for the one-year wage effect case 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.[43] 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 -0.08 and 0.54, respectively. That is, the 2001 reform which increased benefits relative to previous earnings by about 4 percentage points was associated with a 0.54 percentage point increase in wages after two years.

To provide a quantitative benchmark for these effects, we show differential wage growth *predicted* by a calibrated Nash bargaining model with nonemployment as outside option: $dw = (1-\phi)db$ for $\phi = 0.2$, the upper bound of estimates surveyed in Card et al. (2018). We plot this predicted wage growth effect with the dashed brown line. Values of $\phi < 0.2$ imply wage effects even above the 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. 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

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.

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

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 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. We can discern no such effects for a placebo reform in 1999 and thus conclude that our finding that the 2001 reform did not affect wages of treated workers is not spurious.

1989 reform: increase in benefits for low earners. We conduct an analogous analysis for the 1989 reform and present results in Figure 4. The 1989 reform increased benefits for workers with base-year earnings below ATS 12,000 by up to eight percentage points.[44] For that reform, we detect moderate, positive wage effects: we find a 0.55 percentage point effect on wages at the one-year horizon and of 1.25 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 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.[45] 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.

[44] 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.

[45] Similarly, had we found a large treatment effect on wages, this relationship would be weaker for the treated workers.

1985 reform: increase in maximum benefit levels Figures 5 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 -0.18 percentage points at the one-year horizon and of -0.35 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 6. The 1976 reform affected benefits for workers with base year earnings below ATS 3,700 ATS. Our analysis of wages in panel (b) reveals, if anything, wage decreases among those workers that are associated with the benefit increase. The point estimate for the relative wage decrease is -2.93 over one year and -4.31 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 provides a quantitative estimate of the sensitivity of wages to the nonemployment value. Figure 7 plots these data points across all reforms, using colors to differentiate between the four 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 $\hat{\sigma} = -0.057$ (with a standard error 0.04) at the one-year horizon and of $\hat{\sigma} = -0.069$ (s.e. 0.077) at the two-year horizon. At both horizons, the confidence interval includes zero and we can rule out effects larger than 0.02 and 0.08 at the one- and two-year horizon, respectively.

This sensitivity of wages to the nonemployment value is smaller than expected in the Nash bargaining setup with nonemployment as the outside option. Here, if structurally interpreted in a simple static setup, this sensitivity would therefore reject worker bargaining parameter values of $\phi^{\text{OO:NE}} = 1 - \sigma \leq 0.92$ – i.e. implausibly high values.

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 a wage sensitivity to nonemployment values between 0 and 4 cents on the dollar. With tight confidence intervals, we can reject worker bargaining power parameters of $\phi^{\text{OO:NE}} = 1-\sigma \leq 0.93$ under the assumption that nonemployment constitutes the outside option in wage bargaining.

4.4.1 Econometric Framework

The variation we use for identification are reform-induced benefit changes occurring within a year across percentiles of the earnings distribution, comparing percentiles that experience a benefit reform to those that do not, 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 (13), 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 $b_{r-1}(.)$ 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 $t-1$, $w_{i,r,t-1}$.[46] 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:

$$\frac{dw_{i,r,t}}{w_{i,r,t-1}} = \sum_{e=-L}^{0} \delta_e (\mathbb{1}_{(t-r=e)} \times \frac{db_{i,r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}) + \tau_{r,P_t} + \theta_{r,t} + \gamma_{r,t} \ln w_{i,r,t-1} + X'_{i,r,t}\phi_{r,t} + \epsilon_{i,r,t}. \quad (16)$$

Treatment effect. We let $e = t - r$ denote event time and the δ_e are the coefficients of interest. In $e = 0$, $db_{i,r,t}$ corresponds to the actual reform-induced change in the benefit level,

[46] In addition to the one-year horizon exposited here, we also conduct our analysis with wage changes $dw_{i,r,t}$ at the two-year horizons.

$db_{i,r-1}(w_{i,r-1})$, from $r-1$ to r so that δ_0 captures the effect of the reform-induced benefit change on wages from the pre-reform year, $r-1$, to r. Intuitively, the coefficient δ_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 7 – 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 δ_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.[47] In years $t < r$, the $db_{i,r,t}$ capture placebo benefit changes and we normalize δ_{-1} to zero (since we omit one coefficient due to earnings percentile fixed effects).[48] If differentially affected earnings percentiles were on different trends, then the δ_e in the pre-period would be systematically different from zero.

Controls. The model includes reform-specific percentile fixed effects τ_{r,P_t} which absorb any permanent differences in wage growth across percentiles, e.g., due to mean reversion.[49] 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

[47] 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. $d\bar{b}_{i,r}^{\text{Reform}}(P_{r-1}(w_{i,r-1}))$. 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 $d\bar{b}_{i,r}^{\text{Reform}}$ by the average nominal wage growth rate $\bar{g}_{r,t}$ between a placebo year t and the reform year r:

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

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.

[48] We normalize δ_{-2} to zero and omit δ_{-1} for specifications in which we consider outcomes over $n = 2$ periods.

[49] By reform-specific, we mean that the percentiles are added separately for each of the four reform samples that we stack.

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 (16) 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 have also run specifications with standard errors clustered at other levels, leading to quantitatively similar results.

Validation exercise. We supplement the reduced form analysis in Equation (16) with a validation exercise to assess whether reform-induced benefit changes led to realized benefit changes. Formally, we estimate the following specification, letting superscripts V denote coefficients for the validation exercise:

$$\frac{b_{i,r,t} - b_{i,r,t-1}}{w_{i,r,t-1}} = \sum_{e=-L}^{0} \delta_e^V (\mathbb{1}_{(t-r=e)} \times \frac{db_{r,t}(w_{i,r,t-1})}{w_{i,r,t-1}}) + \tau_{r,P_t}^V + \theta_{r,t}^V + \gamma_{r,t}^V \ln w_{i,r,t-1} + X'_{i,r,t}\phi_{r,t}^V + \epsilon_{i,r,t}^V \quad (17)$$

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

4.4.2 Results

Mirroring the non-parametric analysis, the difference-in-differences analysis reveals that wages are close to insensitive to benefit changes. While we sometimes find small positive effects that are in some specifications statistically significant, wages remain economically close to insensitive to changes in the nonemployment option and we do not find point estimates larger than $\hat{\sigma} = 0.05$. Specifically, the point estimate for the effect of benefit changes on wages is $\hat{\sigma} = -0.004$ (se 0.01) after one year and $\hat{\sigma} = 0.018$ (se 0.02) after two years in our preferred specifications. Stated alternatively, we find that a \$1.00 increase in the nonemployment value increases wages by about \$0.02 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 of $\hat{\phi} = 1 - 0.02 = 0.98$ for the two-year specification. The estimates are precise so that even after two years, our estimates reject $\hat{\phi} \leq 0.91$.

Panel (a) of Figure 8 shows the results for the difference-in-differences specification for wage effects after one year and reveals an effect of $\hat{\sigma} = -0.004$ (se 0.01). The figure plots the δ_e, i.e. the interaction of actual (reform-induced) and placebo benefit changes with event time in a specification with demographic controls and industry-occupation-year effects. The regressor of interest is δ_0, capturing the wage growth associated with reform-induce benefit changes. We have normalized δ_{-1} to zero and assess pre-trends with the δ_{-3} and δ_{-2}. In the pre-period, the coefficients are close to zero and pre-trends are flat (p=0.648 in F-test), thereby supporting the common trends assumption underlying our research design. As a complement, we also run specifications with different controls for base-year earnings and find quantitatively similar results.[50]

We show specifications analogous to the one in Figure 8.a with different sets of control variables in Table 2. Throughout, we find quantitatively similar effect sizes centered at zero. Specifically, we find effects of $\hat{\sigma} = 0.007$ in a specification without control variables (column 1) and one with demographic controls (column 2). Coefficients are slightly smaller at -0.006 and -0.004 (columns 3 and 4) when including industry-occupation-year fixed effects and including all controls jointly, our preferred specifcation. In columns 1 through 4, we find that pre-trends are flat with p-values between 0.307 and 0.766 in the corresponding F-tests.

In a next step, we analyze longer-term effects of the benefit reforms and study effects at the two-year horizon in panel (b) of Figure 8 as well as in Table 3. In a specifications with all control variables (Figure 8.b and column 4 of Table 3), we find an effect of $\hat{\sigma} = 0.018$ (se 0.023). In specifications with fewer control variables (columns 1 through 3), we again find effect sizes of similar magnitude ranging between 0.014 and 0.048. The effects of placebo reforms in the pre-period are statistically insignificant, providing additional support for the common trends assumption underlying our research design.

[50] See Tables A.1 to A.4.

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 outside options. 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. At the two-year horizon (Table 3), effects are attenuated in specifications with firm-by-year effects, even though we cannot formally reject equality. Overall, the evidence suggests small, if any, wage effects at the two-year horizon that seem to be driven by firm-level rather than by within-firm variation between workers.

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) estimating effects on employment, retention, and (ii) sickness, (iii) assessing whether reform-induced variation based on lagged wages led to actual benefit variation, (iv) probing different levels of clustering, and (v) winsorization.

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).[51] To assess whether there is selective attrition by wage growth, we also study whether benefit level reforms affected employment and retention. Figure 9 reports treatment effects: the months employed in the reform year in panels (a) and (b), and the probability of staying in the same job in panels (c) and (d).[52] We do not find a statistically or economically significant effect of the improved nonemployment option on these outcome variables. 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 (although we do not consider potential effects on the employment stock through reduced search of seperators).

Efficiency wage effects: sickness incidence. Efficiency wage mechanisms may mask bargaining-related wage effects by lowering productivity, if workers are more likely to reduce

[51] 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.

[52] We also report additional specifications in Appendix Tables A.6 and A.7.

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 panels (e) and (f) of Figure 9.[53] 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.[54]

Validation exercise. We implement a validation exercise as outlined in Equation 17 and report results in Appendix Figure A.6 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 0.795 (se 0.022) coefficient at the one-year horizon and of 0.407 (0.025) 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.[55] In the pre-period, the estimated coefficients are an order of magnitude smaller than the coefficient in the reform year. While the coefficients are very precisely estimated in the pre-reform periods and thus the point estimates are statistically significantly different from zero in the pre-reform period, 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.

Levels of clustering. In Appendix Figure A.7, we assess the robustness of our empirical conclusions to the level of clustering. We estimate the specifications reported in Figure 8 that reported confidence intervals based on two-way clustering at the individual and percentile level and also include confidence intervals based on clustering at the percentile level and two-way clustering at the firm and percentile level as well specifications where we make the level of clustering reform-specific.[56] As Appendix Figure A.7 reveals, standard errors and the resulting confidence intervals are of similar size across the different clustering levels that we consider.

Winsorizing the wage growth variable. In addition, we also assess whether winsorizing the outcome variable affects the results we find. Appendix Figure A.8 reports results with no winsorization as well as winsorization at the 5th and 99th percentile. The estimates are

[53] Additional specifications are reported in Appendix Tables A.6 and A.7.

[54] 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 $\frac{1-0.2}{0.2} \times 8ppt = 32ppt$ decline in productivity to offset the bargaining channel and leave wages unchanged on net.

[55] We also run a validation specification excluding the 2001 reform and find overall similar results.

[56] 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.

quantitatively robust across specifications and do not differ significantly from the ones in our main specification that winsorized at the 1st and 99th percentile.

5 Dissecting the Insensitivity of Wages: Heterogeneity Analysis and Extensions

The insensitivity of wages to nonemployment poses a puzzle to leading bargaining models. Here, we dissect the wage insensitivity further to assess which alternative theories may help to explain our findings. In Section 5.1, we conduct a theory-informed heterogeneity analysis, confirming the insensitivity of wages to the nonemployment value to a broad set of subgroups. Next, we test for firm- and industry-level bargaining in specifications aggregating nonemployment value shifts at the firm and industry level in Section 5.2, finding that wages remain insensitive to nonemployment value shifts at the firm and industry level. Finally, in Section 5.3, we focus on reform-induced variation in potential benefit duration and find no evidence for wage sensitivity to nonemployment value shifts induced by potential benefit variation in existing jobs. We provide an interpretation of our full set of findings for models of bargaining and wage setting in Section 6.

5.1 Heterogeneity Analysis

We analyze heterogeneity in the treatment effect motivated by theory as well as potential concerns about our empirical research design. First, we assess whether wages are more sensitive to UI-induced nonemployment shifts among workers with more previous or expected exposure to the UI system. Next, we assess the role of wage adjustment frictions by considering several measures of wage flexibility at the firm level. Finally, we consider heterogeneity by basic demographic characteristics.

Specifically, we estimate our main specification (column (4) in tables 2 and 3) but interact the treatment variable with different exhaustive heterogeneity groups indicators. We estimate a separate specification for each set of heterogeneity groups. Table A.8 presents these estimates for a large number of heterogeneity groups. We focus on the two-year estimates because most of the one-year estimates are statistically indistinguishable from each other and since our overall one-year effect is zero, it is less interesting to unpack heterogeneity in the one-year results. Figure 10 presents our main heterogeneity results for the two-year earnings effects. 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.

The clearest heterogeneity in our estimated results is for the four measures of within firm wage flexibility at the bottom of the *firm characteristics* section of figure 10. Across all four measures, individuals at firms with a higher dispersion of wages or of wage growth, indicating more flexible wages, see larger wage effects from UI benefit extensions. This result is consistent with our result below that the pass-through of firm-level treatment to wages is slightly higher than the individual-level treatment. Finally, in that last panel of figure 10 we find some evidence that individuals with a higher risk of unemployment have a higher pass-through to their wages although we find no heterogeneity by the local unemployment rate.

The bottom estimates in the *individual characteristics* section figure 10 show that we estimate slightly larger effects for females than males which supports previous work that has found lower bargaining power for females. We also find a somewhat higher estimate for blue-collar workers. The bottom three estimates in the *individual characteristics* section proxy for how long since an individuals has been non-employed. Across the three measures we include, there is no clear evidence of any effect heterogeneity. As we discuss below, this result does not provide any evidence that the recently unemployed have more knowledge of the UI system and also does not support a prediction of models with job offers as possible outside options for incumbent workers (see Postel-Vinay and Robin (2002) and Cahuc et al. (2006)).

5.1.1 Exposure to Unemployment Risk and the Unemployment Insurance System

We first undertake heterogeneity analyses to see whether wages are more sensitive to UI-induced shifts in the nonemployment value for workers for whom changes to the UI system might be more salient or non-employment might be a more relevant outside option. Besides simply larger exposure to the program, knowledge of the UI system and larger salience of UI would facilitate pass-through. The heterogeneity dimensions we consider are unemployment risk, predicted unemployment spell duration, and the local unemployment rate. We also sort workers by time since last unemployment spell experienced, proxying for the first-hand experience with the UI system as well as 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) and Cahuc et al. (2006)). In addition, these variables also arguably capture awareness of the UIB system and benefit generosity (see Lemieux et al. (1995) and Lemieux and MacLeod (2000)). As shown in Figure 10 and Table A.8, we find that wages remain insensitive to shifts in the nonemployment value even for workers with recent exposure to the unemployment system. However for the two-year earnings growth heterogeneity results, individuals with a high predicted unemployment risk or spell duration do have slightly larger effects.

Time since worker's last nonemployment spell. We sort our sample of workers by time since their last nonemployment spell.[57] We separately present results separately for nonemployment and unemployment. We also then zoom into nonemployment spells by those entailing UI receipt, again with and without a proxy of a recall, i.e. those workers who return to the same employer.

Experience. We additionally test for heterogeneity by standard measures of labor force attachment and perhaps match quality, by considering experience.[58]

Worker's predicted risk of unemployment. We predict individual unemployment risk in fine-grained cells based on the risk of becoming unemployed and the expected unemployment duration at the industry-by-occupation-by-experience-by-tenure cell. Specifically, we consider the set of workers who are employed for a full year three years before each reform and consider three different outcome variables measured in the year after: (i) an indicator for being unemployed, (ii) the months spent in unemployment, and (iii) and indicator for an unemployment spell of more than six months. We then regress these indicators on industry-by-occupation fixed effects as well as fixed effects for six categories of experience and tenure, respectively. We then take the estimated coefficients to predict unemployment risk in our estimation sample and split workers into five quintiles based on their predicted unemployment risk based on each of the three outcome variables. We then show wage effects in the top and bottom category for each of these risk measures in Figure 10.

Local unemployment. We assign each worker the unemployment rate of the fine-grained local labor market level in the year before the reform. We construct that unemployment rate in our administrative data at the municipality level and, for each reform sample, rank local labor markets (weighted by number of workers in our sample) and split the sample into quintiles.

5.1.2 Wage Adjustment Frictions

Perhaps Nash bargaining with nonemployment as the outside option is an empirically relevant wage setting protocol, yet our empirical design to fails to detect wage responses due to frictions in wage adjustments or of informational nature. The empirical literature on inside-option rent sharing documents contemporaneous wage effects for incumbent workers. 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

[57] We aggregate our daily spell data into months, and define nonemployment spells to be a full month of nonemployment.

[58] While our panel data set starts in 1972, experience has been recorded for administration of the social security system beforehand, such that our data set includes these two variables starting 1972 without truncation.

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.

Yet, when we split the firm sample by dispersion in worker-level wage growth, individuals at firms that normally do appear to differentiate wages between similar workers see a slightly higher pass-through of improved nonemployment options into wages. Even for these individuals, however, the pass-through is much lower than the theoretical benchmark of Nash bargaining.

Firm-level measures of wage flexibility. We consider four empirical measures to identify firms with more flexible wage policies and split firms into quintiles based on their measured wage flexibility. Throughout, we calculate the flexibility measures in the pre-reform period. The first measure we consider is the **dispersion 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. We calculate the measure based on workers who are employed at the same firm across two years and calculate wage growth. We then calculate the standard deviation of wage growth at the firm level and split firms into quintiles. Second, 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.[59] This measure is intended to capture firms that pay observationally similar workers different wages. Finally, we calculate a measure to proxy for **distance from CBA-level wage setting**. To do so, we calculate the squared residuals from the regression for the third measure and calculate a firm-by-year average. The underlying idea is that CBAs set wage floors at the industry-by-occupation level and often further differentiate wage floors by experience or tenure. Here, we calculate the squared distance from the average level of wages in the dimensions in which collective bargaining agreements set wages. Firms will be scored higher on this metric if they pay wages that differ from the CBA-level average.

Firm size. 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.

Industry growth rates. We consider effect heterogeneity by industry growth rates for two reasons. First, wage rigidity might prevent shrinking industries from increasing wages. And,

[59] Tenure is split into seven three-year categories. Experience is split into six five-year categories.

second, workers in low-growth industries may be less likely to have other job offers, so that the non-employment outside option might be more relevant. Importantly, we calculate leave-out mean industry growth rates three years before each reform and omit a workers' own firm's observation from the calculation of industry growth rates. We then split the sample into quintiles by reform and report the effect sizes in the lowest and highest category in Figure 10.

5.1.3 Demographic Heterogeneity

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**). Second, we consider effect heterogeneity by **sex**, since we are particularly interest in demographics associated with *lower* bargaining power, which should correlate with *larger* pass-through. Allowing for different effects for male and female workers, this strategy builds on previous work by Black and Strahan (2001) and Card et al. (2015) who find evidence consistent with lower bargaining power for female workers. If nonemployment is the relevant outside option in bargaining, then female workers' wages should exhibit stronger wage comovement with unemployment benefit shifts. Figure 10 shows that we do find a higher pass-through for women and blue-collar workers.

5.2 Extension: Firm- and Industry-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 option but occurs at the firm level.[60] An alternative model of bargaining posits that employers does not negotiate with individual workers but instead bargains with its workers as a collective, for instance with industry-level unions or plant-level works councils in the Austrian case.[61] 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.[62]

Firm- and Industry-level versions of the reform-induced benefit variation. We study the role of group-level bargaining by aggregating the reform-induced benefit varation at the *group*

[60] 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.

[61] 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. (ming)) does not change the pass-through of outside options into wages.

[62] Chetty et al. (2011) documents intensive margin adjustment of workers to tax incentives to be mediated by the fraction of affected workers. Based on the example of employer payroll tax cuts for young workers in Sweden, Saez et al. (2017) document that rent-sharing appears to occur at the firm level rather than the directly treated workers.

level. Our analysis tests whether nonemployment outside options matter for wage bargaining at the group-level, such increases in the firm's employees' average nonemployment value affect wage growth. Our regression specification remains structurally identical to the specification in 16. However, rather than the worker-level treatment variable, the regression now features its group-level *average* as the regressor of interest.

We consider two plausible group levels: firms and industries. We associate workers with the firm (industry) they work at in the pre-reform year. As before, we also assess pre-trends based on placebo reforms, similarly aggregating the individual-level placebo treatments assigned at the earnings group to to the firm (industry) level.

Figure 11 and Tables 4 and 5 report estimates based on firm- and industry-level variation in outside options and shows that we find slightly higher wage sensitivity to nonemployment value changes in those specifications. For firm-level variation in benefit changes, we find a wage sensitivity of $\hat{\sigma} = 0.013$ (se 0.013) at the one-year horizon and of $\hat{\sigma} = 0.057$ (0.022) at the two-year horizon.[63] Zooming out to the four-digit industry level, we find $\hat{\sigma} = 0.055$ (se 0.024) at the one-year horizon and of $\hat{\sigma} = 0.083$ (0.048) at the two-year horizon. While the point estimates at the industry level are larger, we are careful to note that uncertainty due to pre-trends impedes putting too much weight on the specific estimate we find. That is, we find a large and marginally statistically significant pre-trend at the two-year horizon so that pre-trends may shroud potentially different effect sizes.

Overall, our read of the evidence of group-level bargaining lends support to the idea that bargaining occurs at the firm level, as we find slightly larger effects compared to individual-level specifications. Moreover, the individual-level specifications with firm-by-year effects had revealed zero wage sensitivity at the two-year horizon, suggesting that positive point estimates in the individual-level specifications without firm-by-year effects were driven by firm-level variation in the nonemployment option. With regards to Nash bargaining with nonemployment outside options, our point estimates at the firm and industry level can still not be reconciled with evidence on rent-sharing elasticities which would imply substantially higher wage sensitivity at the group level than what we find in the data.

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.[64] While a thorough digitalization of Austrian CBA wage floors is beyond the scope of

[63] At the two-year horizon, the placebo treatment year exhibits a positive wage effect so that potential trends might shroud slightly larger effects.

[64] Austrian CBAs typically either specify across-the-board nominal growth of the wage floors, or specifiy explicitly group-specific wage floors that ultimately grow in lockstep. These mandates for typically homogeneous

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 Variation in UI Generosity From Potential Benefit Duration

Next, we investigate the role of the potential benefit duration of UIBs (rather than the UIB level) on incumbent wages. This approach complements our strategy of analyzing money-metric changes in nonemployment value with an analysis of economically harder-to-quantify but more commonly studied changes to nonemployment values. In addition, perhaps the potential benefit duration (PBD) of benefits rather than their level is the dimension that may be more salient for real-world wage negotiations.

The effect of PBD on wages. To study the wage effects of this second dimension of the UI-related component of the nonemployment value, we study a reform that changed the PBD. While benefit reforms were income-specific, Austrian PBD reforms and schedules exhibit sharp quasi-experimental variation with respect to worker *age*. 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 more salient and non-complex (a simple cutoff in age).

The particular reform we study occurred in 1989. Figure A.12 shows how the PBD schedule changed for individuals age 30-49 in 1989. Before 1989, the PBD was only experience and not age-dependent.[65] In 1989, these eligibility rules were changed so that individuals age 40-49 with at least five years of experience in the past 10 years were eligible for 39 weeks while individuals below age 40 were still only eligible for 30 weeks. For the analysis below, we focus on the PBD reform for individuals age 40-49 and compare their earnings growth to individuals age 30-39.[66] We apply the same sample restrictions as in our main result for the full sample but drop all individuals present in particular Austrian regions where workers aged 50 and above were eligible for even larger PBD reform since 1988.[67]

Figures A.13 and A.14 plot the average earnings log differences (one and two years) by age groups in the treated and control years. The right panel plots the difference between earnings

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).

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

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

[67]We do not study the latter reform because of a regional reform that further increased PBD for workers older than 50 and led to separations (and thus attrition) among those older workers (Jäger et al. (2018)).

growth in the treated and control years for each age. The right panel is normalized to be mean zero. 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.

6 Interpretation and Implications for Models of Labor Markets

The insensitivity of wages to the nonemployment option we document is puzzling to a wide variety of influential models of wage bargaining frequently used in macroeconomic analysis of the aggregate labor market. Here, we describe the class of models with nonemployment outside options that the data reject as well as models of credible bargaining and on-the-job search that can account for our main result and some of our additional findings. Next, we discuss the predictions of non-bargaining, wage posting models for our setting and evaluate the models in light of the evidence. We next discuss and dismiss several alternative mechanisms, including wage stickiness and limited salience of UI, that would also lead to insensitivity of wages to nonemployment value shifts but that are harder to reconcile with our additional findings and features of our research design. 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 Lessons for Nash Bargaining with Nonemployment as the Assumed Outside Option

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 incorporate, e.g., dynamic considerations or multi-worker firms, and that we review in Section 2.2. In addition, we also find that allowing for bargaining at the firm or industry level also does not resolve the puzzle.

The core puzzle is that a large body of existing evidence implies substantially larger passthrough of nonemployment value shifts into wages if indeed nonemployment constitutes the outside option in bargaining. The studies of rent-sharing that we summarize in Figure 1 use quasi-experimental variation in *inside options*, such as productivity of profits, and find evidence consistent with bargaining power $\phi \leq 0.2$. In contrast, our research design estimates the effect of *nonemployment outside options* on wages and reveals a near insensitivity of wages to nonemploy-

ment value shifts. Viewed through the framework of nonemployment as the outside option and Nash bargaining, wages are a weighted average of the inside and outside options, and thus low sensitivity to inside options directly implies large sensitivity to the outside option. Analogously, the small sensitivity of wages to the outside option directly implies a bargaining power parameter that we include in the meta study – which substantially exceeds all micro-estimates from inside option variation, as well as macro calibration targets (not based on direct empirical evidence but often appealing to the Hosios (1990) condition or otherwise calibrated to equilibrium outcomes of the model). Juxtaposing these values clarifies the conceptual and quantitative puzzle.

Our preferred interpretation of the evidence is not that workers hold all the bargaining power but rather that the benchmark model as well as several important extensions are rejected by our results. Beyond the static Nash bargaining model, the evidence is inconsistent to dynamic extensions, including to the DMP with free entry in vacancy costs, and also to Stole and Zwiebel (1996) bargaining with multi-worker firms. Additional extensions of the model that allow for richer components of the nonemployment value, including UI take-up costs, skill depreciation, or stigma, also cannot resolve the discrepancy between the evidence and the predictions from Nash bargaining with nonemployment outside options. In addition, accounting for the finiteness of benefit duration also does not quantitatively change our conclusions and would still lead to the conclusion that nonemployment cannot constitute the outside option in wage setting.

In addition to bilateral bargaining between one worker and a (multi-worker) firm as described above, our evidence is also inconsistent with nonemployment options as threat points when bargaining occurs at the firm or industry level. In Section 3.4 we have discussed potentially relevant features of wage setting in Austria, including works councils and collective bargaining, which may directly or indirectly create constraints on the differentiation of wages by the nonemployment option. When we specifically create firm- and industry-level versions of the benefit shift variation in Section 5.2, we find similar pass-through at group levels. In conclusion, the main puzzle also remains when one is willing to model bargaining to occur at the firm or industry level.

6.2 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 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 on-the-job search (e.g., Cahuc et al. (2006)). These models can account are consistent with the broad insensitivity of wages to nonemploymet 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.3we derive the wage and discuss the role of b in wage setting.[68] 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 comovement with the inside option. The credible bargaining framework can reconcile our finding with the large body of evidence that finds significant but potentially limited pass-through of inside values into worker wages.[69]

Rebargaining in corner cases. Alternative models (MacLeod and Malcomson (1993) and MacLeod and Malcomson (1993)) have wages be 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.

On-the-job search, outside job offers, and negotiation capital (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 on-the-job search in which nonemployment remains the outside option when bargaining with the next employer (e.g. Fujita and Ramey (2012) or Beaudry et al.

[68]The expression for the credible wage bargain is:

$$\underline{w} = \frac{(1-\beta(1-\delta))z + (1-s)\beta\left[(1-\beta(1-\delta))\gamma + y(1-\beta(1-s))\right]}{1-\beta^2(1-s)^2} + \frac{\beta(s-\delta)(1-\beta)}{1-\beta^2(1-s)^2} \times U \tag{18}$$

[69]Our findings about the explanatory power of Nash bargaining for wage formation in the labor market 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)).

(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 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. Caldwell and Harmon (2018) provide evidence in support of the idea that outside options with other employers matter in bargaining and document that information about job opportunities at other employers raises job-to-job mobility and earnings among previous coworkers. This view may reconcile a zero or small effect of non-employment outside options on wages, while not implying full bargaining power and thus hard-wiring workers to carry full bargaining power over inside options (the share of the pie of production on the job).

However, we find less support for more nuanced predictions from models of 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 sensitity of wages to the nonemployment option of our baseline model (where all workers' outside option is nonemployment).[70] While we found some evidence consistent with a slightly higher sensitivity for workers with shorter time since nonemployment as well as among firms hiring a high share of workers out of nonemployment, effect sizes remain small. This insensitivity is harder to square with models of 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.

[70]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:

$$W(w) = (1-\phi) \cdot U(b_i) + \phi \cdot (W(w) + J(x_f, w)) \tag{19}$$

where x_f is the match- or firm-specific productivity. An employed worker having received outside offer $x_{f'}$ dominating unemloyment yet dominated by the current job ($W(w)+J(x_f,w)-U(b) > W(w)+J(x_{f'},w)-U(b) > U(b_i)$) renegotiates the current wage with that external job offer as the outside option:

$$W(w) = (1-\phi) \cdot [W(w) - W(w_{f'})] + \phi \cdot (W(w) + J(x_f, w)) \tag{20}$$

6.3 Wage Setting Models Beyond Bargaining: Wage Posting and Monopsony

Waste posting and monopsony. Besides wage bargaining, wage posting is the second leading alternative model to the Walrasian market-clearing theory of wages and has been a core feature in models of monopsony. In such models, firms post wages with full commitment. We review canonical versions of wage posting models and show that the nonemployment value remains the cornerstone of the wage distribution in these models, with some model variants featuring mean wages moving *linearly* in those models with the nonemployment value too. Calibrations of the canonical wage posting model in Burdett and Mortensen (1998) generate small wage responses consistent with our evidence, while richer calibrations may lead to larger sensitivities that are harder to square with our evidence.

The canonical baseline model of wage posting generates "pure" wage *dispersion* as a mixed equilibrium strategies homogenous firms play when recruiting homogeneous workers with random search (see, e.g., Burdett and Mortensen (1998)). We discuss the quantitative implications of this model detail in Appendix C.4, and derive the model-implied wage sensitivity to the unemployment benefit level, which is around 5%, and turns out to be roughly consistent with the range we estimate. In this model, due to random search, a given job vacancy with a posted wage may either meet an employed worker (who accepts if the posted wage exceeds her current wage), or an unemployed job seeker. For that reason, all wage offers are rejected that fall short of the nonemployment value, i.e. the reservation value of the unemployed job seeker for accepting a job. The prospect of meeting unemployed job seekers with low reservation wages (rather than already employed and thus selective workers) leads the firm to take the nonemployment value into account. 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.[71]

In contrast to additional implications from the basic wage posting model, our additional heterogeneity results have found only limited evidence for the prediction at the core of this class of model that wage pass through is higher in firms whose workforce typically goes through unemployment, in areas with high unemployment, or for individual workers going through unemployment.

In Appendix C.4 we also state the wage equations for richer wage posting models that 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. These models are more suited to quantitatively match empirical wage distributions and worker flows. In those

[71] 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.

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.[72] The pass-through of b into wages can be higher in those models. Yet, these models feature the same prediction that pass-through is higher among unemployed workers, high unemployment areas, or for firms usually drawing from the unemployment pool, predictions our heterogeneity cuts in Section 5.1 only lend limited support to.

6.4 Alternative Mechanisms: Wage Stickiness, Absence of Bargaining, and Limited Salience of UI Benefits

Here, we review several alternative mechanisms—wage stickiness, the absence of bargaining, and limited salience of UI benefits—that could account for our main result of wage insensitivity but that are harder to reconcile with the whole body of evidence that we present.

6.4.1 Alternative Mechanism: Wage Stickiness

Wage stickiness in continuing employment relationships may slow down adjustment and mask pass-through. While this could account for an insensitivity of wages to nonemployment value shifts, we find 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.[73] Second, we have also found only small wage effects for worker 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

[72] There are additional specifications that use functional form assumptions to derive linear relationships between the wage and a notion of an outside option. For example, Manning (2011) features a term b that shifts the distribution of tastes for leisure and thereby parametrically changes the elasticity of labor supply. Card et al. (2018) include a b-term denoting a minimum wage term in the workers' utility function that changes the wage level in the economy by parametrically shifting the relative importance of wages and tastes for specific employers (the latter being the source of monopsony). Interpreting these wage floor terms as the nonemployment option, among the interpretations the authors suggest, we would obtain a wage expression that is linear (specifically a weighted average) in firm-specific productivity and the nonemployment value b.

[73] 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 (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.

growth. Third, we found no evidence for nonlinear effects such that even large shocks in the nonemployment value did not entail noticeable wage effects.[74] Fourth, we have also found no evidence for substantially larger wage effects when considering more aggregate shocks to outside options at the firm or industry level. Finally, we have additionally measured wage effects in *new* jobs in Section 5.1 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.

6.4.2 Alternative Mechanism: Absence of Bargaining

We also consider whether the absence of bargaining can account for our findings: one potential rationalization of the insensitivity of wages to the nonemployment option 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 within a year of shifts in the inside job value (which are more difficult to rationalize in wage posting models). Moreover, worker and employer survey evidence on the actual presence of bilateral bargaining imply that both sides of the labor market perceive wages to be set in 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 proxies associated with this self-assessed prevalence of wage bargaining in those surveys are associated with larger sensitivity of wages to our variation.[75] However, we surprisingly do not find larger pass-through for firms and workers where survey evidence suggests a high bargaining prevalence. This suggests that even in pockets of the Austrian labor markets where we expect bargaining to occur, nonemployment value shifts do not entail wage effects.

[74] This is 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.

[75] 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).

6.4.3 Alternative Mechanisms: 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.[76] However, several pieces of our evidence are hard to square with a salience-based explanation of our findings. First, even over multiple years, workers would have to not learn about the shift in the system. Second, we have found that even large shifts do not entail wage responses, which are arguably more salient and could also overcome adjustment costs (see, e.g., Chetty (2012))). Third, even recently unemployed and UI-receiving workers themselves and their peers within the firm, who are plausibly more aware of the UIB schedule (see Lemieux et al. (1995) and Lemieux and MacLeod (2000)), do not exhibit higher wage sensitivity. Fourth, even workers at higher risk of future unemployment events for whom the UI system is likely more salient, do not respond.[77] 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, for *continuing* employment relationship (but not necessarily for job switchers), the benefit level is a function of previous wages, a piece of information that should be readily accessible even for the employer.

6.5 Reconciling the Insensitivity of Wages to UI with Existing Evidence on Wage Sensitivity 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 labor market – typically proxied for by the unemployment rate – appears to have contemporaneous as well as long-lasting effects on workers' wage levels. For example, the influential wage curve documented by Blanchflower and Oswald (1994) relates local unemployment rates with wage growth among existing workers, often rationalized with a bargaining view where nonemployment is the outside option, whose value falls with high unemployment (and long unemployment spells). However,

[76] For instance, Abeler and Jäger (2015) find evidence consistent with lower responses to incentives in more complex systems. For our study, the specific statistic of interest would be whether employed workers are aware of their *own applicable* benefit level – and reform-induced changes therein. We are aware of one study (Altmann et al. (2018)) which analyzes a representative survey evidence of German households, and finds evidence that respondents identify benefit differences across workers with different wage levels, which—in the German system—are associated with different benefit levels.

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

alternative sources may underlie this empirical regularity (for a discussion see Card (1995)). 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 composition and job selection due to job ladders (Hagedorn and Manovskii (2013)). Lastly, a key source of the failure of modern business cycle models to generate realistic fluctuations in employment appear to be due to the sensitivity of wages to the nonemployment value, where the incipient increases in unemployment caused by shocks to the return to hiring are offset by a large decline in hiring due to the sensitivity of Nash wages to the outside option (Hall and Milgrom (2008)). Our evidence supports the adoption of wage setting mechanisms that insulate wages from the nonemployment value.

6.6 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.

Unemployment benefits vs. nonemployment value. In principle, to estimate a dollar-for-dollar sensitivity of wages to the nonemployment value, *any* component of the nonemployment value would be suitable. Our design exploits quasi-experimental unemployment insurance benefit reforms are a particularly suitable source of identifying variation in the nonemployment value that is directly quantifiable in money units and comes in clear flow units (compared to e.g. potential benefit duration shifts). In the Introduction, we have reviewed the literature that directed or indirectly provides evidence for wages to depend on variations in components of the the nonemployment value other than UI benefits. A motivation for our study is that alternative pieces of evidence use designs that permit alternative channels. For example, the wage curve may be driven by selection or labor supply margins (see e.g. Hagedorn and Manovskii (2013) and Blanchflower and Oswald (1994)). Finally, the unemployed job seekers' reemployment wages respond to potential benefit duration due to multiple margins such as selectivity, bargaining and skill depreciation or statistical discrimination (see, e.g., Kroft et al. (2013), Schmieder et al. (2016), Nekoei and Weber (2017), and Kroft et al. (2016)), which makes it harder to pin down the bargaining mechanism as in our study of incumbent workers.

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. This contrasts with e.g. the US context, where a smaller fraction of unemployed job seekers collect UI (see, e.g., Chodorow-Reich et al. (2018)); although some features such as the absence of experience rating are features in most OECD countries.

The role of collective bargaining. We have discussed the role of institutional constraints on wage differentiation and collective bargaining in Section 3, where we characterize the Austria labor market to permit substantial wage flexibility despite the presences of minimum wages set through collective bargaining (from which actually set wages regularly depart upwards, and our predicted wage changes would all be positive). We also test for for industry- and firm-level bargaining in Section 5.2, not finding larger pass-through than implied by the individual-specific treatment effects.

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 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 (e.g., Hagedorn et al. (2013)) and Chodorow-Reich et al. (2018)), which document 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, where we provide complementary evidence on the insensitivity of job destructions and separations.[78]

Long-run implications. Our identification strategy uses panel variation in the nonemployment value brought about by UI reforms. 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 argument naturally applies to aggregate shifts in the nonemployment value, a variation

[78]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.

that would preclude a difference-in-differences design, although we have not found consistently larger effects at the firm or industry level.

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 macro- 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 economic mechanisms other than outside options in bargaining.

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 findgings imply that this wage bargaining protocol is not a useful model for understanding real-world labor markets: either the specification to nonemployment scenarios as the relevant outside option is at fault, or even deeper structural assumptions of Nash bargaining.

Third, the empirical insensitivity of wages to the nonemployment value, for which we provide causally identified micro-empirical evidence, is good news for (some) macroeconomists: the *theoretical* insensitivity of wages to the nonemployment value is a crucial ingredient to successful models of aggregate employment fluctuations. In contrast, Nash bargainining, exactly due to its sensitivity to the nonemployment value, stabilizes labor demand reductions and thus largely offsets incipient unemployment increases and therefore underpredicts aggregate employment fluctuations. Our research design therefore supports alternative bargaining protocols that insulate wages from nonemployment values.[79]

Fourth, the view that nonemployment values constitute workers' outside options in bargaining according to the Nash protocol, and the calibration choices that lead these values to have large effects on wages, also underlie the active policy debate about the distortion of labor demand from policies that boost workers' nonemployment values, such as unemloyment insurance. Our findings suggest that this wage pressure channel of UI on hiring is likely small. In consequence, potential effects of such policies on equilibrium employment may largely come from workers' labor supply responses.

[79]Examples or such wage setting models are ad-hoc sticky wages (e.g. Shimer (2004)) or micro-founded alternative protocols such as credible bargaining (Hall and Milgrom (2008)). Models of employer competition and on-the-job search (e.g. Cahuc et al. (2006) may rationalize small sensitivity of wages for high-tenured incumbent workers, but our additional evidence have shown that workers going through nonemployment and recently hired exhibit the insensitivity too, stands in tension with this class of 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	.89	.897	.479	.227	.64	.875	.482	.820	.54	.615
	(.312)	(.304)	(.5)	(.419)	(.48)	(.331)	(.5)	(.384)	(.498)	(.487)
Age	39.772	39.971	38.274	39.068	37.866	39.086	38.14	38.994	38.244	39.106
	(8.291)	(8.208)	(8.399)	(7.983)	(8.358)	(7.937)	(8.027)	(7.651)	(8.264)	(7.876)
White Collar	.385	.289	.386	.549	.384	.409	.46	.519	.412	.492
	(.487)	(.453)	(.487)	(.498)	(.486)	(.492)	(.498)	(.5)	(.492)	(.5)
Experience in last 25 Years	9.812	9.306	15.659	18.096	14.61	12.833	14.91	12.955	14.792	14.604
	(6.46)	(6.131)	(6.162)	(6.302)	(5.953)	(5.901)	(6.375)	(6.104)	(6.373)	(6.765)
Tenure	2.299	2.284	6.834	8.196	6.891	5.831	7.226	5.821	6.683	6.471
	(1.059)	(1.066)	(4.069)	(4.125)	(4.994)	(4.677)	(6.4)	(5.549)	(5.227)	(4.937)
Avg. Monthly Earnings	4303.292	2688.017	13876.424	20869.185	13897.982	8574.365	23816.079	15682.933	16791.216	15310.58
	(354.536)	(687.984)	(1553.728)	(2168.942)	(1111.461)	(2153.694)	(1834.624)	(3051.524)	(5937.023)	(6129.038)
Observations in Base Year	60755	62038	342564	342364	182804	184365	325430	327471	911553	916238

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.

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

	1-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	-.022	-.037	-.021	-.022	.02	.021
	(.032)	(.029)	(.032)	(.031)	(.012)	(.013)
2 Years Pre-Treatment	-.007	-.017	-.01	-.011	.003	-.002
	(.012)	(.012)	(.012)	(.012)	(.009)	(.01)
Treatment Year	.007	.007	-.006	-.004	0	-.006
	(.009)	(.009)	(.01)	(.01)	(.01)	(.01)
Base-Year Average	-.886	-.886	-.886	-.886	-.886	-.886
Pre-period F-test, p-value	.766	.307	.690	.648	.137	.072
R^2	.03	.043	.06	.072	.252	.267
N (Thousands)	7318	7318	7315	7315	6365	6361
Mincerian Ctrls		X		X		X
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 16. 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.

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

	2-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	-.014	-.027	-.01	-.009	.019	.023
	(.023)	(.02)	(.024)	(.021)	(.016)	(.017)
2 Years Pre-Treatment						
Treatment Year	.037	.048	.014	.018	.011	.003
	(.021)	(.023)	(.022)	(.023)	(.013)	(.013)
Base-Year Average	-.654	-.654	-.654	-.654	-.654	-.654
Pre-period F-test, p-value	.557	.171	.672	.685	.224	.171
R^2	.049	.067	.089	.106	.274	.296
N (Thousands)	5226	5226	5223	5223	4475	4472
Mincerian Ctrls		X		X		X
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 16. 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.

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

	1-Year Earnings Effects		2-Year Earnings Effects	
	(1)	(2)	(1)	(2)
3 Years Pre-Treatment	-.036	-.003	.035	.067
	(.026)	(.021)	(.043)	(.033)
2 Years Pre-Treatment	-.037	-.009		
	(.014)	(.014)		
Treatment Year	.021	.013	.093	.057
	(.014)	(.013)	(.025)	(.022)
Base-Year Average	-.886	-.886	-.654	-.654
Pre-period F-test, p-value	.024	.811	.419	.043
R^2	.031	.072	.049	.106
N (Thousands)	7252	7248	5111	5109
Mincerian Ctrls		X		X
Ind.-Occ. FEs		X		X
Firm-Year FEs				

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification 16 with the variation in benefits aggregated at the firm-level. 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. See Section 5.2 for more details.

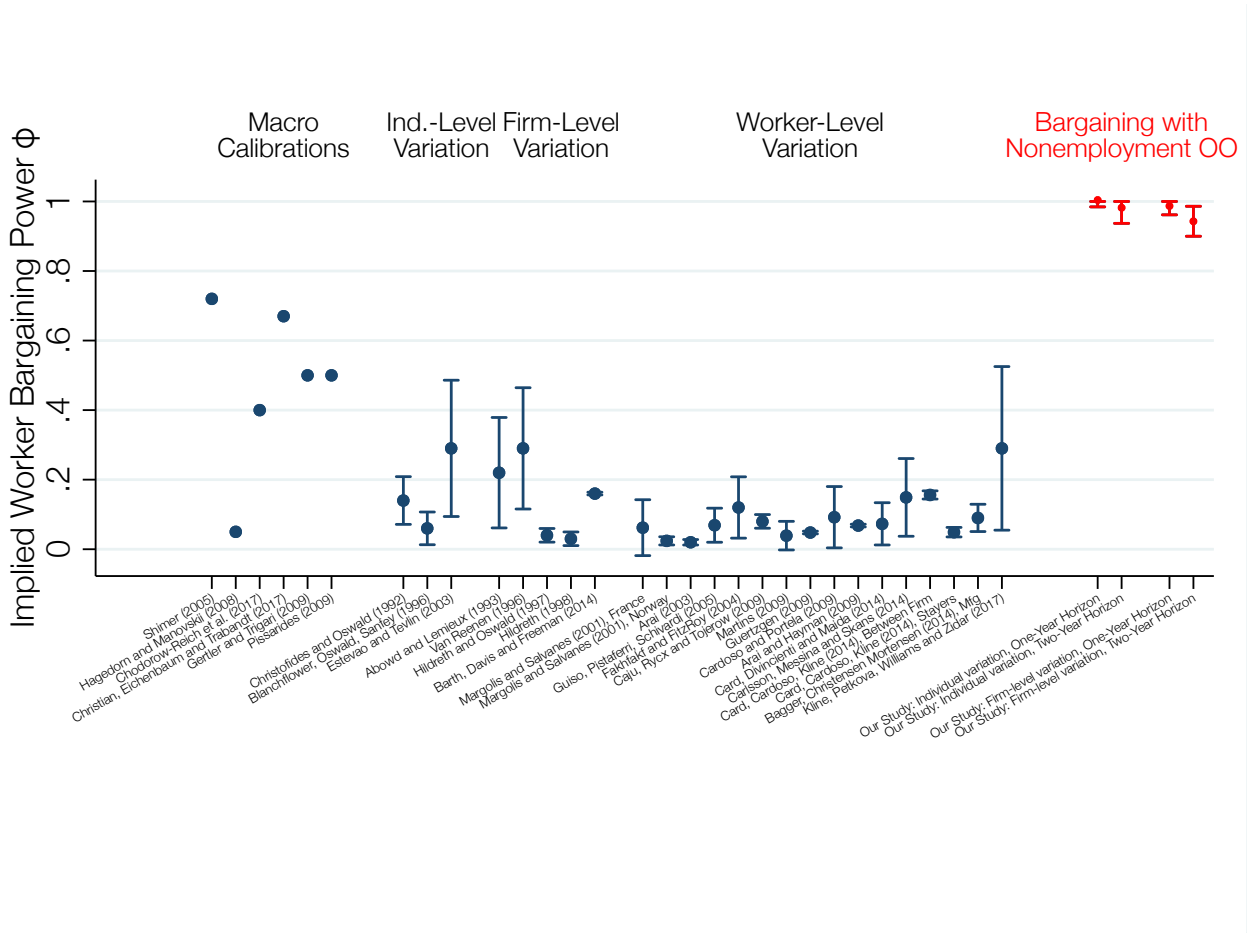
Table 5: Wage Effects: Difference-in-Differences Regression Design with Industry-Level Variation

	1-Year Earnings Effects		2-Year Earnings Effects	
	(1)	(2)	(1)	(2)
3 Years Pre-Treatment	.138	.117	.294	.254
	(.098)	(.09)	(.156)	(.138)
2 Years Pre-Treatment	-.025	-.022		
	(.035)	(.034)		
Treatment Year	.056	.055	.088	.083
	(.026)	(.024)	(.05)	(.048)
Base-Year Average	-.886	-.886	-.654	-.654
Pre-period F-test, p-value	.226	.273	.06	.067
R^2	.032	.045	.051	.069
N (Thousands)	7249	7249	5111	5111
Mincerian Ctrls		X		X
Ind.-Occ. FEs				
Firm-Year FEs				

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification 16 with the variation in benefits aggregated at the four-digit industry level. Standard errors are in parentheses and clustered at the industry level. The null hypothesis of the F-test is that the coefficients of interest are all equal to 0 in the pre-period. See Section 5.2 for more details.

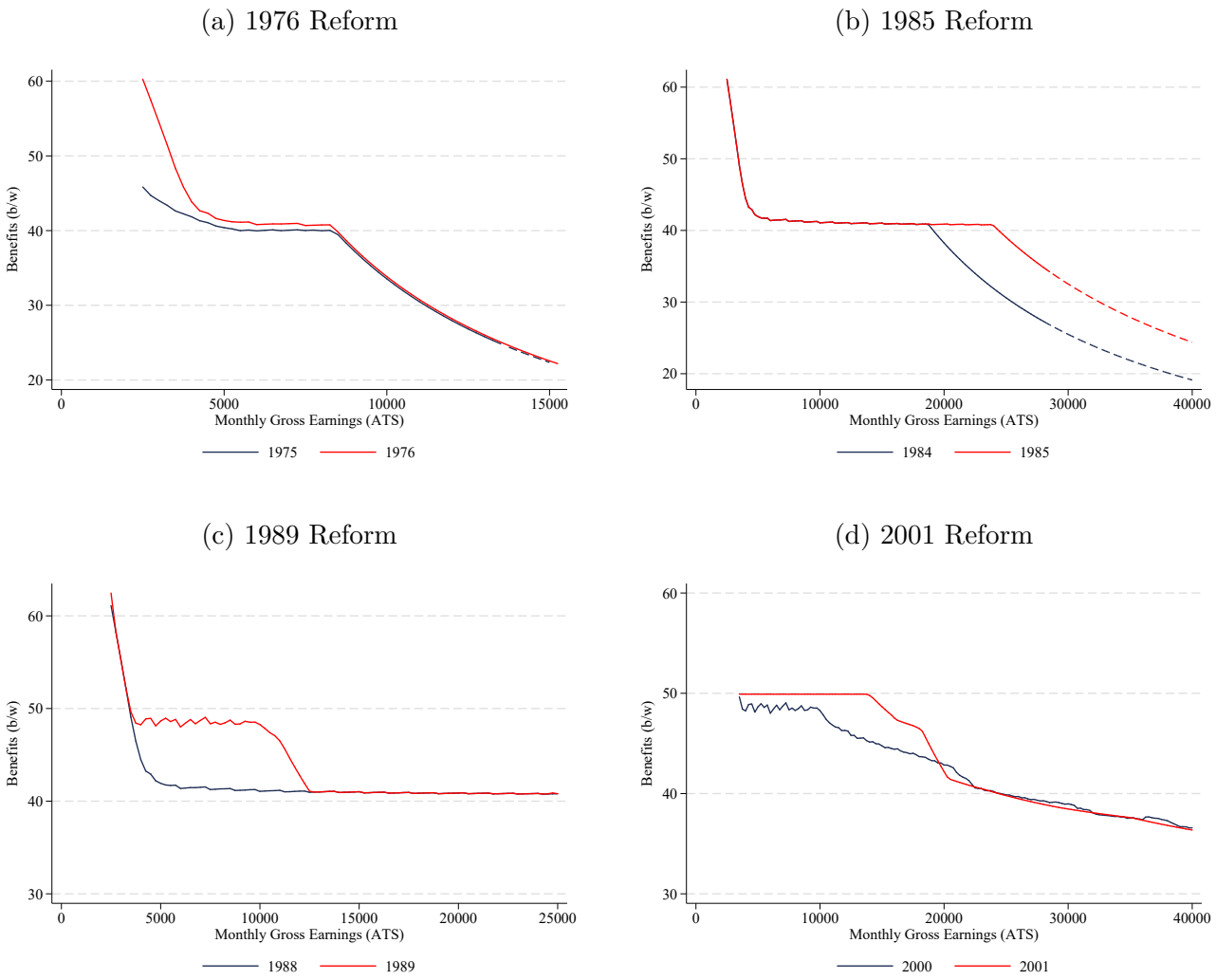
Figures

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



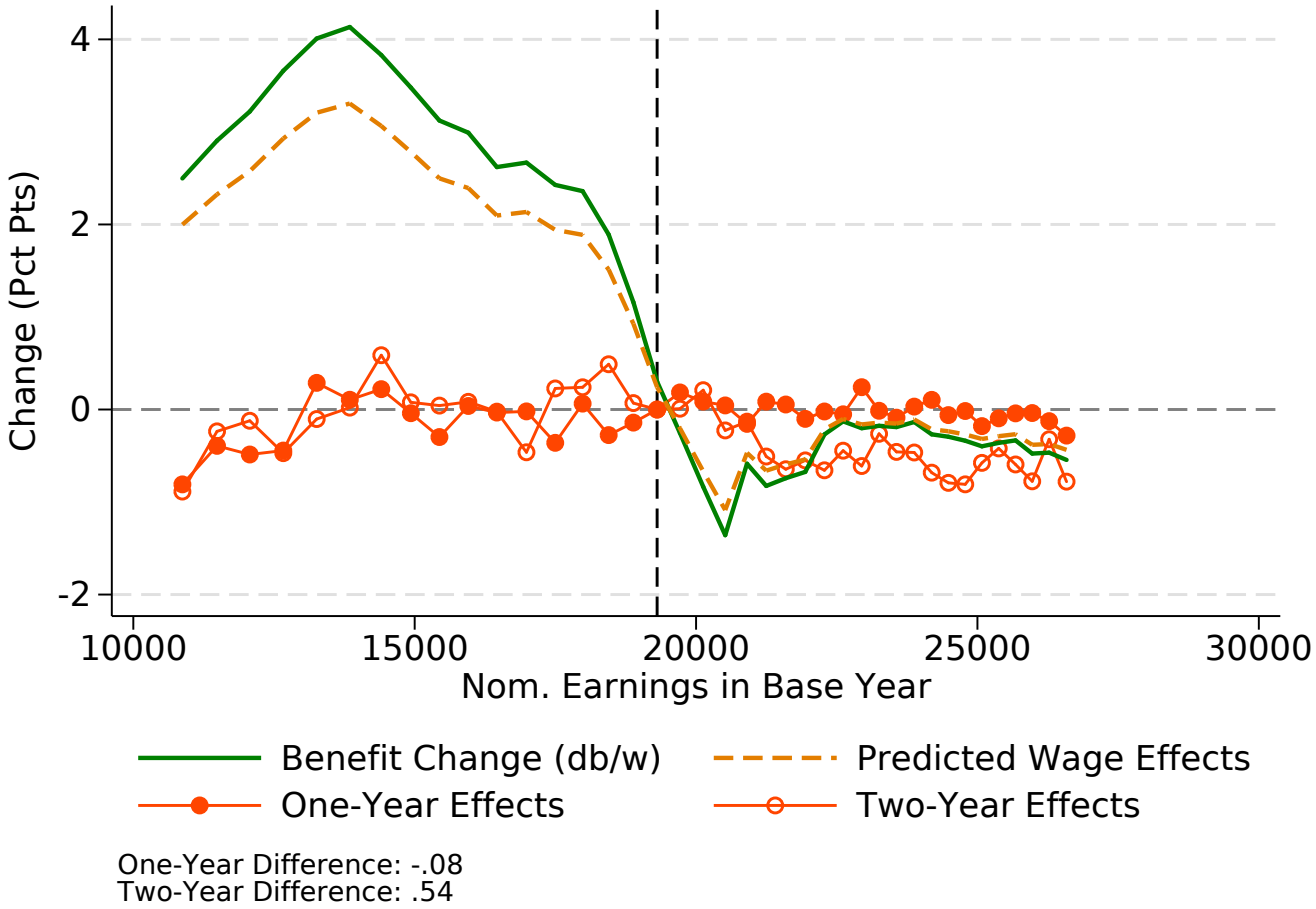
Note:: The figure shows an overview of calibrations as well as implied estimates of worker bargaining power. For the calibrations, we plot the values used in the respective papers. For the estimates, we build on the meta-study in Card et al. (2018) and add the estimate from Kline et al. (2017). The estimates surveyed in Card et al. (2018) are cast as rent-sharing elasticities and are thus upper bounds for the implied worker bargaining power (see Section D). 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 8.

Figure 2: Unemployment Benefit Schedules and Reforms



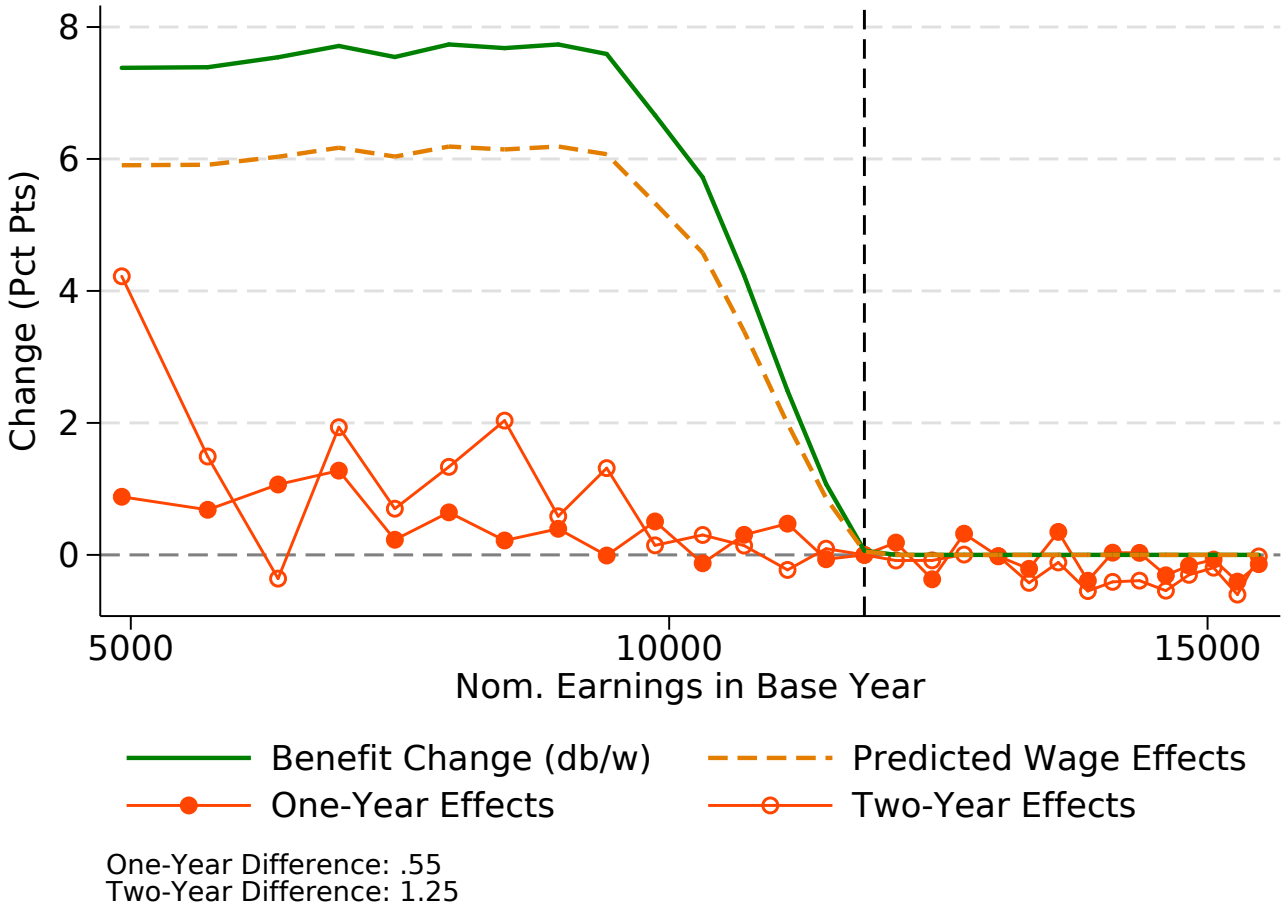
Note: The figures plot the unemployment benefit schedule before and after each of the four reforms we analyze. The x-axis shows the income relevant for calculating benefits while the y-axis plots the benefits, calculated as the fraction of unemployment benefits divided by income. The dashed segments of the lines indicate incomes above the social security earnings maximum.

Figure 3: 2001 Reform: Benefit Changes and Wage Effects



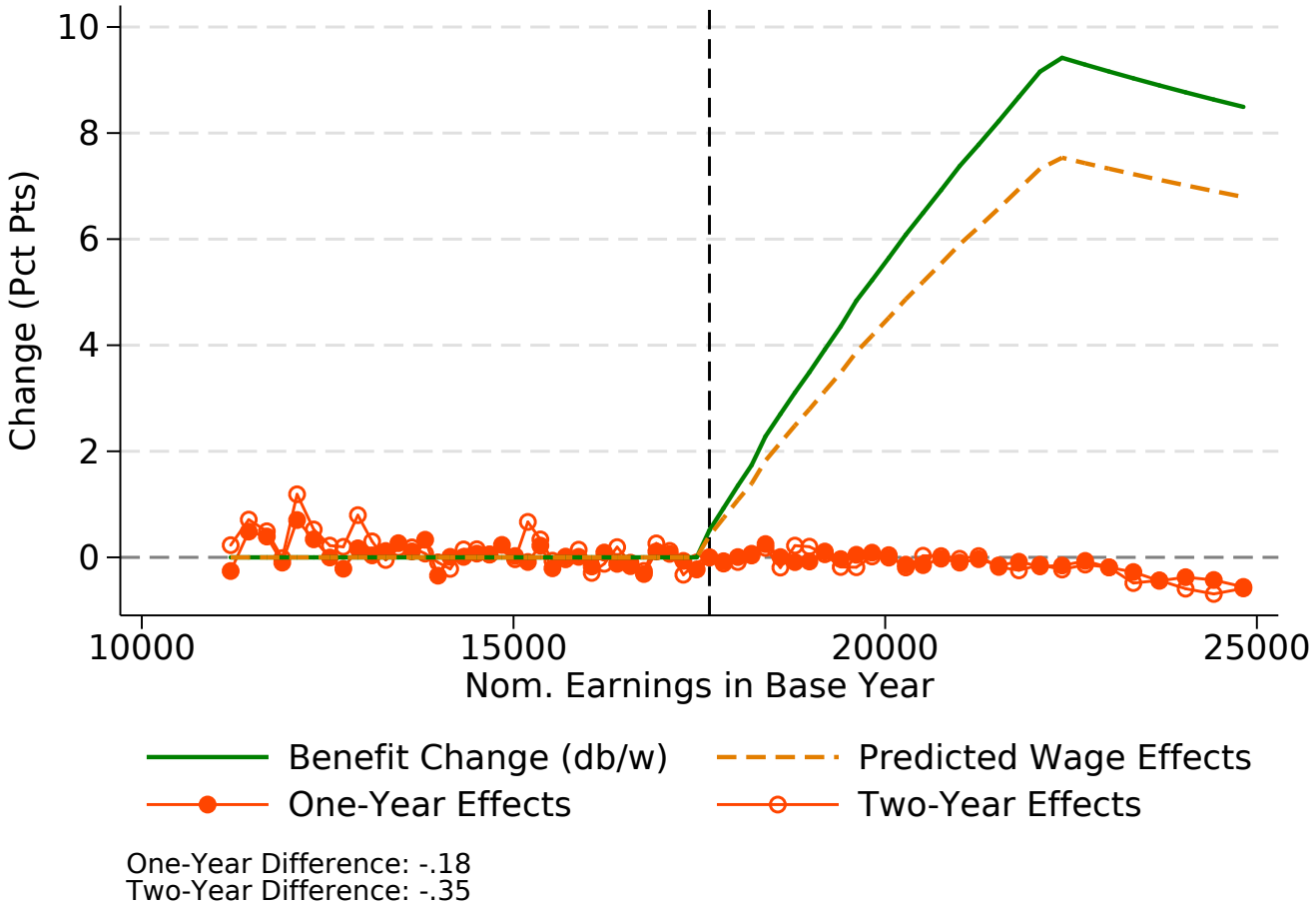
Note: The figure plots reform-induced replacement rate changes and wage effects for the 2001 reform. Observations are binned by their base year (2000) earnings percentile on the x-axis. The 2001 reform increased replacement rates below the 26th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 2001 reform would induce if non-employment 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 4: 1989 Reform: Benefit Changes and Wage Effects



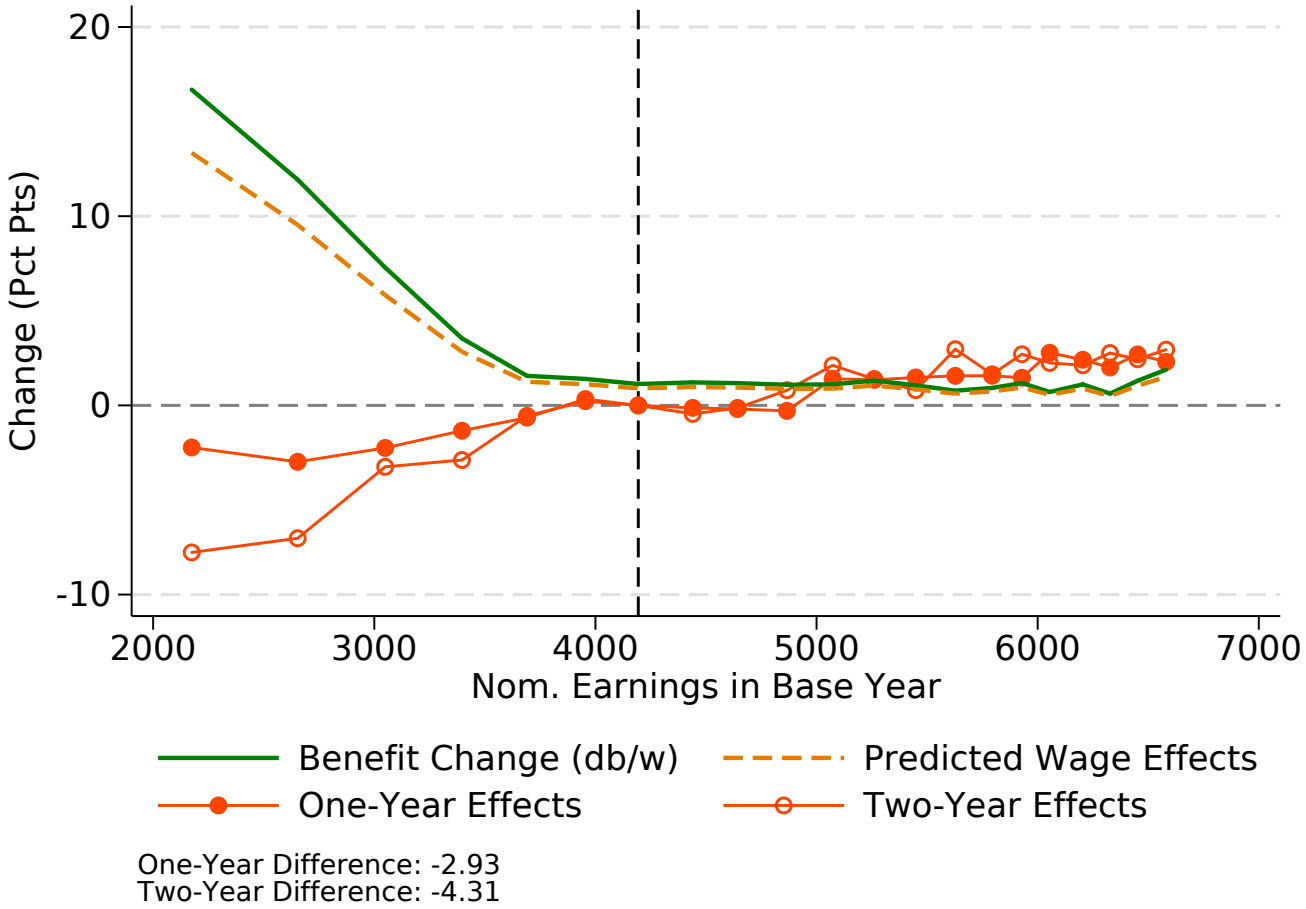
Note: The figure plots reform-induced replacement rate changes and wage effects for the 1989 reform. Observations are binned by their base year (1988) earnings percentile on the x-axis. The 1989 reform increased replacement rates below the 16th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1989 reform would induce if non-employment 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 5: 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 54th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1985 reform would induce if non-employment 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 54th percentile. Section 4.3 provides more information.

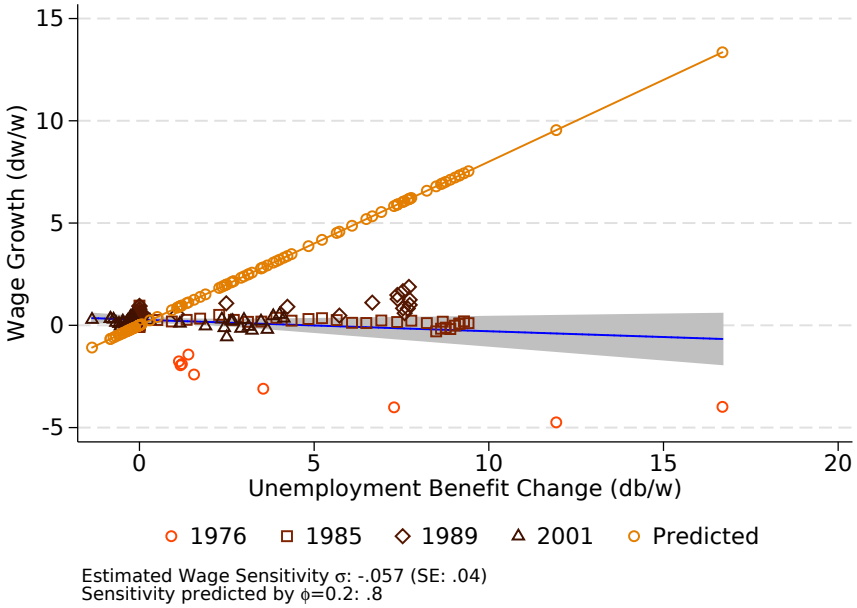
Figure 6: 1976 Reform: Benefit Changes and Wage Effects



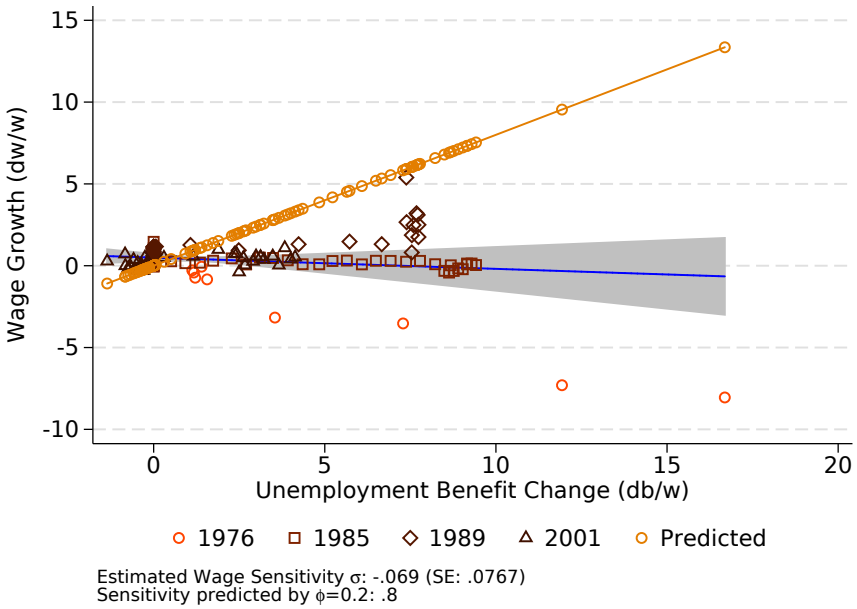
Note: The figure plots reform-induced replacement rate changes and wage effects for the 1976 reform. Observations are binned by their base year (1975) earnings percentile on the x-axis. The 1976 reform increased replacement rates below the 6th percentile as indicated by the green circles. The orange triangles indicate the wage growth that the 1976 reform would induce if non-employment 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 7: Scatter Plots of Wage Growth and Unemployment Benefit Changes

(a) One-Year Horizon



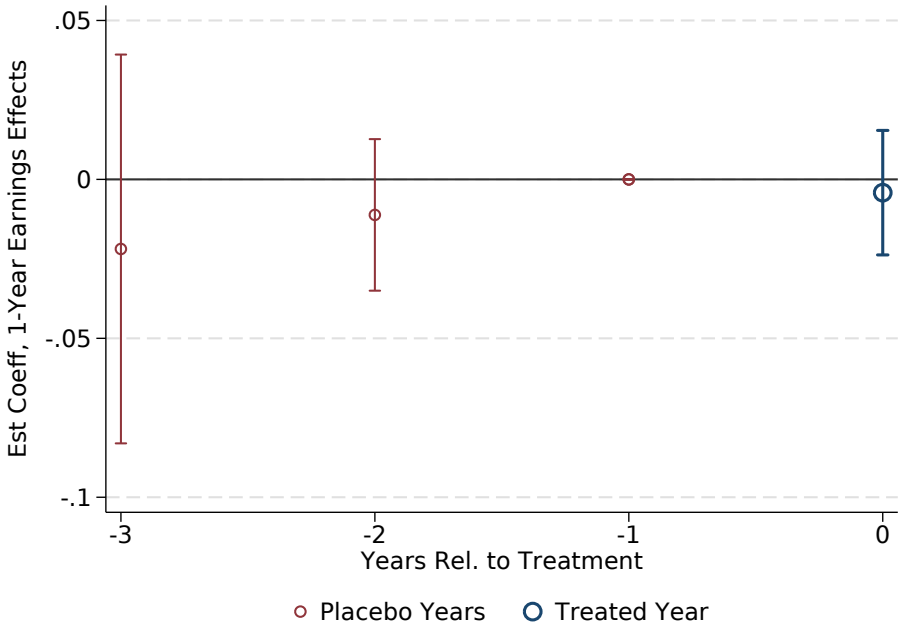
(b) Two-Year Horizon



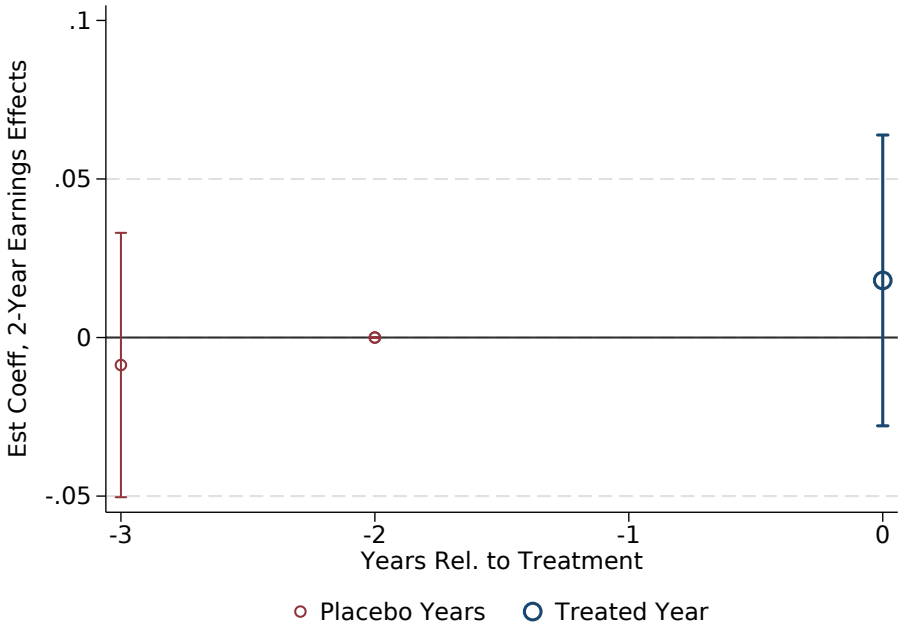
Note: The figures show scatter plots of wage growth (y-axis) and reform-induced replacement rate changes (x-axis), db/w, pooling the four reforms outlined in Figures 3 through 6. Each dot corresponds to a percentile observation from one of the 3 through 6. The upper panel shows wage effects after one year and the lower panel effects after two years. The orange triangles indicate the wage growth that the reforms would have induced if non-employment was the outside option and worker bargaining power was $\phi = 0.2$. 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 8: Wage Effects: Difference-in-Differences Regression Design

(a) One-Year Wage Effect



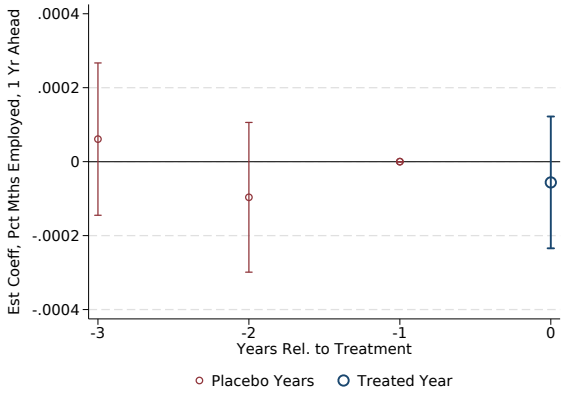
(b) Two-Year Wage Effect



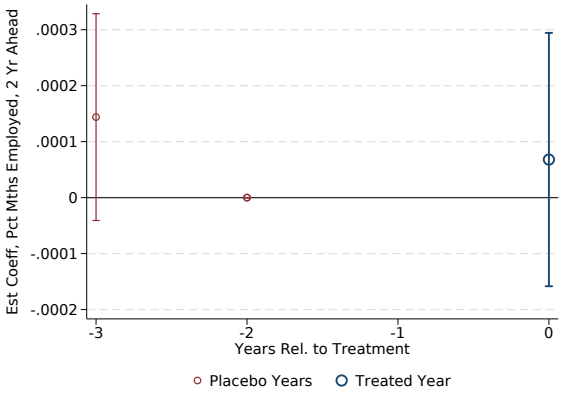
Note: The figures show the effects of nonemployment value shifts, db/w, on wages based on the difference-in-differences specification in 16. They plot the estimated δ_e coefficients and associated confidence intervals. The coefficient in year 0 captures the treatment effect of outside option changes on wages while the coefficients in the pre-period capture the effect of placebo reforms and can be used to gauge the common trends assumption. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The specifications include demographic controls and industry-occupation-year effects. Alternative specifications are reported in Tables 2 and 2; validation analyses relating predicted and realized benefit changes are reported in Figure A.6. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level.

Figure 9: Employment Outcomes: Difference-in-Differences Results

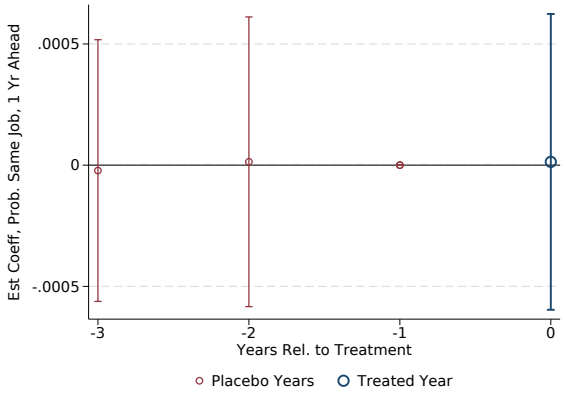
(a) Months Employed: 1Yr



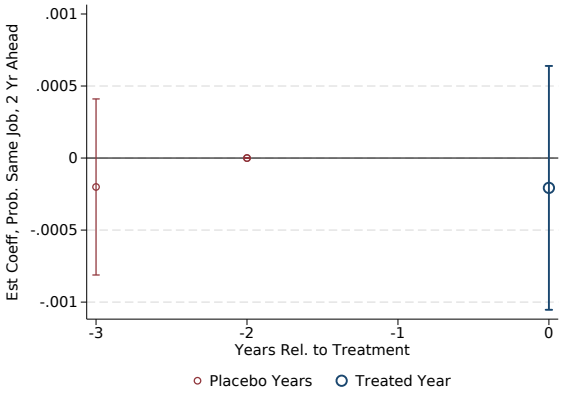
(b) Months Employed: 2Yr



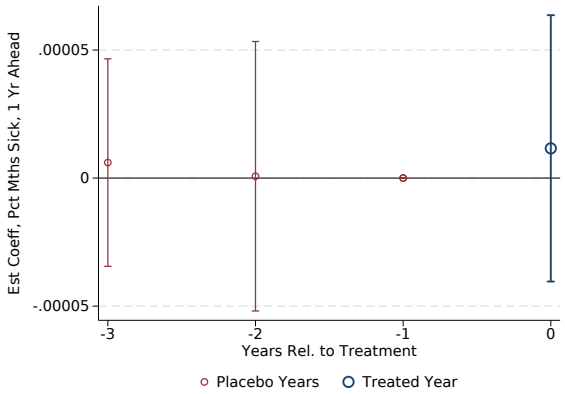
(c) Fraction in Same Job; 1 Yr



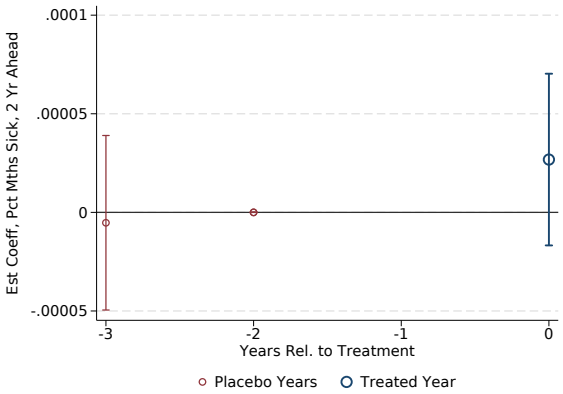
(d) Fraction in Same Job; 2 Yr



(e) Months of Sick Leave; 1 Yr

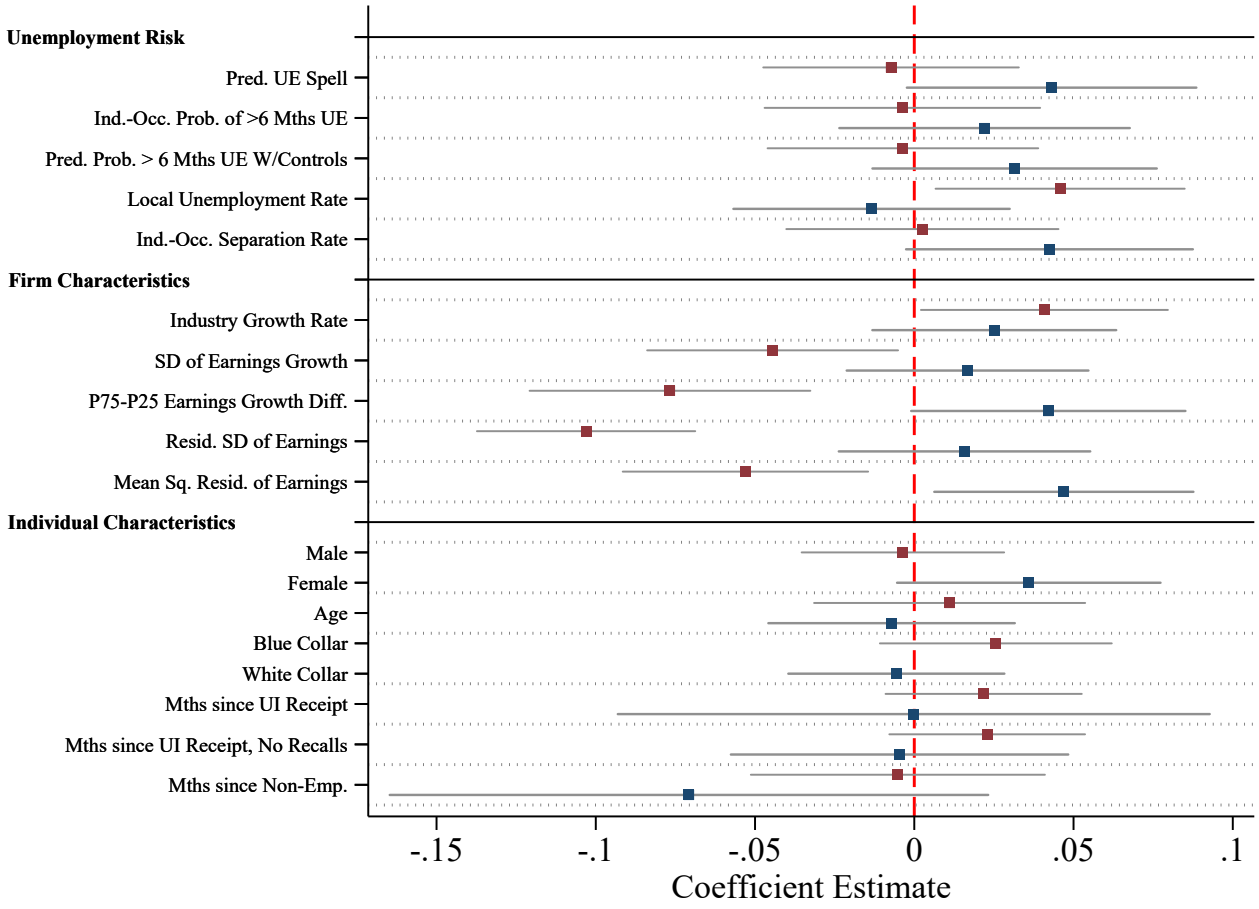


(f) Months of Sick Leave; 2 Yr



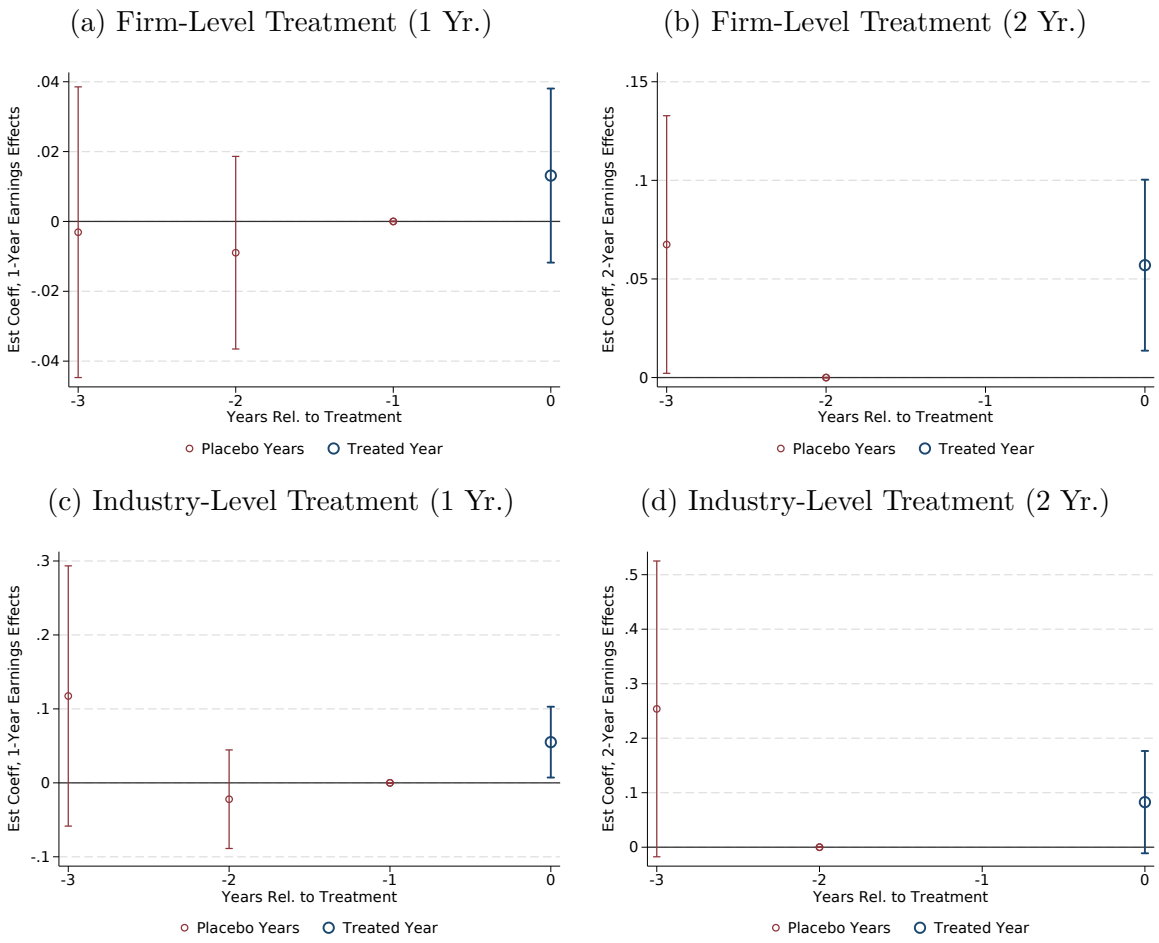
Note: The figures show the effects of nonemployment value shifts, db/w, on employment outcomes based on the difference-in-differences specification in 16. They plot the estimated δ_e coefficients and associated confidence intervals. The coefficient in year 0 captures the treatment effect of outside option changes on the respective employment outcome while the coefficients in the pre-period capture the effect of placebo reforms and can be used to gauge the common trends assumption. The outcome variables are the share of months in employment (panels (a) and (b)), and an indicator for staying in the same job (panels (c) and (d)), b the share of months in a sickness or disability spell (panels (e) and (f)). The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The specifications include demographic controls and industry-occupation-year effects. Alternative specifications are reported in Appendix Tables A.6 and A.7. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level.

Figure 10: Heterogeneity of Nonemployment Effects on Wages: Two-Year Effects



Note: The figure shows heterogeneity in the treatment effect of nonemployment shifts, db/w, on wages. Different specifications are delineated by the horizontal dashed lines and the treatment effect in the top and bottom category for each heterogeneity dimension is reported. The outcome variable is one-year relative wage growth dw/w. We also report results in Table A.8. The horizontal lines indicates 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level. Unless otherwise specified, within each category the top (red) estimate refers to the lowest category and the bottom (blue) estimate refers to the highest category (e.g., 1st and 5th quintile of Months since UI Receipt). See Section 5.1 for additional information.

Figure 11: Wage Effects of Firm- and Industry-Level Shifts in the Nonemployment Option



Note: The figures show wage effects effects of shifts in the nonemployment option aggregated at the firm and industry level, respectively, based on the specification in 16. The figures plot the estimated δ_e difference-in-differences coefficients and associated confidence intervals. Nonemployment shifts are measured as average changes in the replacement rate at the firm-level (panels (a) and (b)) or four-digit industry level (panels (c) and (d)). The coefficients in year 0 capture the treatment effect of outside option changes on wages while the coefficients in the pre-period capture the effect of placebo reforms and can be used to gauge the common trends assumption. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The specifications include demographic controls and industry-occupation-year effects. Alternative specifications are reported in Tables 4 and 5. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level. See Section 4.5 for additional information.

Online Appendix of:
Wages and the Value of Nonemployment

Simon Jäger, Benjamin Schoefer, Samuel Young and Josef Zweimüller

A Additional Tables

Table A.1: Robustness Check for Wage Effects at One-Year Horizon: **Linear Earnings Controls**

	1-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	.001	-.016	.005	.005	.036	.043
	(.031)	(.029)	(.033)	(.032)	(.015)	(.017)
2 Years Pre-Treatment	-.003	-.015	-.005	-.006	.006	.004
	(.013)	(.014)	(.014)	(.014)	(.011)	(.012)
Treatment Year	.016	.015	.002	.004	.008	.004
	(.01)	(.011)	(.011)	(.012)	(.011)	(.011)
Base-Year Average	-.886	-.886	-.886	-.886	-.886	-.886
Pre-period F-test, p-value	.963	.572	.861	.833	.029	.009
R^2	.03	.043	.06	.072	.252	.267
N (Thousands)	7,252	7,252	7,248	7,248	6,365	6,361
Mincerian Ctrls		X		X		X
Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: The table reports a robustness check to the specifications in Table 2. Instead of controlling for the logarithm of earnings with year-specific coefficients, the controls here include linear earnings effects with year-specific coefficients.

Table A.2: Robustness Check for Wage Effects at Two-Year Horizon: **Linear Earnings Controls**

	2-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	.002	-.014	.01	.012	.033	.044
	(.026)	(.023)	(.028)	(.025)	(.021)	(.022)
Treatment Year	.04	.051	.019	.022	.026	.02
	(.025)	(.027)	(.026)	(.028)	(.015)	(.016)
Base-Year Average	-.654	-.654	-.654	-.654	-.654	-.654
Pre-period F-test, p-value	.945	.551	.731	.631	.123	.049
R^2	.049	.067	.089	.106	.274	.296
N (Thousands)	5,114	5,114	5,111	5,111	4,475	4,472
Mincerian Ctrls		X		X		X
Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: The table reports a robustness check to the specifications in Table 3. Instead of controlling for the logarithm of earnings with year-specific coefficients, the controls here include linear earnings effects with year-specific coefficients.

Table A.3: Robustness Check for Wage Effects at One-Year Horizon: **Linear Earnings Percentiles Controls**

	1-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	0	-.015	.003	.003	.023	.025
	(.019)	(.018)	(.021)	(.019)	(.014)	(.015)
2 Years Pre-Treatment	-.001	-.011	-.006	-.006	.003	-.001
	(.013)	(.013)	(.013)	(.013)	(.01)	(.011)
Treatment Year	.007	.007	-.01	-.008	-.008	-.012
	(.01)	(.011)	(.012)	(.012)	(.009)	(.01)
Base-Year Average	-.886	-.886	-.886	-.886	-.886	-.886
Pre-period F-test, p-value	.998	.62	.813	.8	.206	.102
R^2	.03	.043	.06	.072	.252	.267
N (Thousands)	7,252	7,252	7,248	7,248	6,365	6,361
Mincerian Ctrls		X		X		X
Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: The table reports a robustness check to the specifications in Table 2. Instead of controlling for the logarithm of earnings with year-specific coefficients, the controls here include linear earnings percentile effects with year-specific coefficients.

Table A.4: Robustness Check for Wage Effects at Two-Year Horizon: **Linear Earnings Percentiles Controls**

	2-Year Earnings Effects					
	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	-.012	-.025	-.005	-.003	.014	.02
	(.021)	(.019)	(.023)	(.021)	(.019)	(.02)
Treatment Year	.014	.025	-.006	-.003	.012	.006
	(.02)	(.023)	(.023)	(.025)	(.015)	(.016)
Base-Year Average	-.654	-.654	-.654	-.654	-.654	-.654
Pre-period F-test, p-value	.559	.188	.817	.886	.475	.329
R^2	.049	.067	.089	.106	.274	.296
N (Thousands)	5,114	5,114	5,111	5,111	4,475	4,472
Mincerian Ctrls		X		X		X
Ind.-Occ. FEs			X	X		X
Firm-Year FEs					X	X

Note: The table reports a robustness check to the specifications in Table 3. Instead of controlling for the logarithm of earnings with year-specific coefficients, the controls here include linear earnings percentile effects with year-specific coefficients.

Table A.5: Validation Exercise: Difference-in-Differences Regression Design

	1-Year Realized RR Effects						2-Year Realized RR Effects					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
3 Years Pre-Treatment	.072	.069	.067	.066	.128	.127	-.041	-.041	-.046	-.043	-.024	-.023
	(.065)	(.065)	(.065)	(.065)	(.037)	(.037)	(.039)	(.038)	(.039)	(.038)	(.027)	(.027)
2 Years Pre-Treatment	.079	.077	.078	.077	.098	.097						
	(.016)	(.016)	(.015)	(.015)	(.011)	(.011)						
Treatment Year	.778	.776	.77	.769	.791	.788	.458	.457	.441	.449	.468	.472
	(.018)	(.018)	(.018)	(.018)	(.013)	(.013)	(.026)	(.026)	(.025)	(.025)	(.023)	(.023)
Base-Year Average	2.324	2.324	2.324	2.324	2.324	2.324	3.683	3.683	3.683	3.683	3.683	3.683
Pre-period F-test, p-value	0	0	0	0	0	0	.303	.287	.235	.261	.368	.385
R^2	.301	.302	.304	.305	.35	.352	.234	.28	.243	.286	.332	.372
N (Thousands)	7412	7412	7411	7411	6487	6483	5604	5604	5604	5601	4773	4769
Mincerian Ctrls		X		X		X		X		X		X
Ind.-Occ. FEs			X	X		X			X	X		X
Firm-Year FEs					X	X					X	X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification 16. 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.

Table A.6: 1-Year Alternate Outcomes, Percentile-Based 1-Year Instrument

	Pct Mths Employed, 2 Yr Ahead			Prob. Same Job, 2 Yr Ahead			Pct Mths Sick, 2 Yr Ahead		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
3 Years Pre-Treatment	.000142	.000144	.000063	-.00014	-.000201	.000122	.000018	-5.00000000000e-06	-1.00000000000e-06
	(.000131)	(.000094)	(.0001)	(.000379)	(.000312)	(.000285)	(.000027)	(.000023)	(.000028)
2 Years Pre-Treatment									
Treatment Year	.000069	.000068	-.000052	.000038	-.000207	-.000349	.000019	.000027	.000054
	(.000109)	(.000115)	(.000123)	(.000401)	(.000432)	(.000244)	(.000025)	(.000022)	(.000031)
Base-Year Average	.939	.939	.939	.789	.789	.789	.007	.007	.007
Pre-period F-test, p-value	.281	.131	.532	.713	.521	.669	.501	.816	.984
R^2	.009	.052	.209	.019	.109	.41	.002	.025	.127
N (Thousands)	5274	5271	4618	5287	5284	4630	5274	5271	4618
Mincerian Ctrls		X	X		X	X		X	X
Ind.-Occ. FEs		X	X		X	X		X	X
Firm-Year FEs			X			X			X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification 16. 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.

Table A.7: 2-Year Alternate Outcomes, Percentile-Based 1-Year Instrument

	Pct Mths Employed, 2 Yr Ahead			Prob. Same Job, 2 Yr Ahead			Pct Mths Sick, 2 Yr Ahead		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
3 Years Pre-Treatment	.000142	.000144	.000063	-.00014	-.000201	.000122	.000018	-5.00000000000e-06	-1.00000000000e-06
	(.000131)	(.000094)	(.0001)	(.000379)	(.000312)	(.000285)	(.000027)	(.000023)	(.000028)
2 Years Pre-Treatment									
Treatment Year	.000069	.000068	-.000052	.000038	-.000207	-.000349	.000019	.000027	.000054
	(.000109)	(.000115)	(.000123)	(.000401)	(.000432)	(.000244)	(.000025)	(.000022)	(.000031)
Base-Year Average	.939	.939	.939	.789	.789	.789	.007	.007	.007
Pre-period F-test, p-value	.281	.131	.532	.713	.521	.669	.501	.816	.984
R^2	.009	.052	.209	.019	.109	.41	.002	.025	.127
N (Thousands)	5274	5271	4618	5287	5284	4630	5274	5271	4618
Mincerian Ctrls		X	X		X	X		X	X
Ind.-Occ. FEs		X	X		X	X		X	X
Firm-Year FEs			X			X			X

Note: These results pool four reforms to the replacement rate schedule in Austria, and are based on specification 16. 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.

Table A.8: Heterogeneity of Nonemployment Effects on Wages

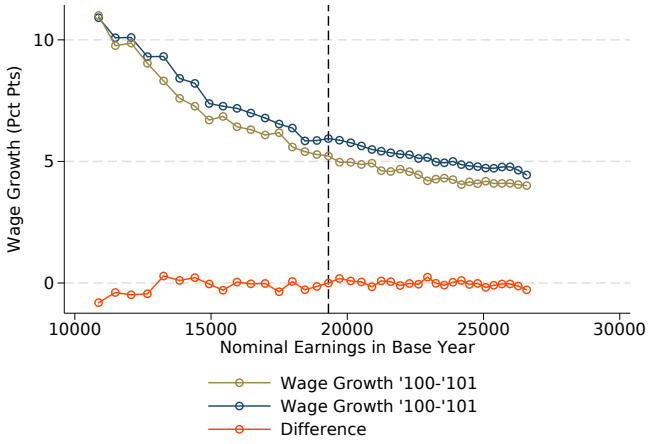
	One-Year Effects		*Two-Year Effects*	
Unemployment Risk				
Pred. UE Spell, 1st Quintile	-.014	(.017)	-.007	(.002)
Pred. UE Spell, 5th Quintile	-.013	(.015)	.043	(.023)
Ind.-Occ. Prob. of >6 Mths UE, 1st Quintile	-.037	(.014)	-.004	(.022)
Ind.-Occ. Prob. of >6 Mths UE, 5th Quintile	.000	(.016)	.022	(.023)
Pred. Prob. >6 Mths UE W/Controls, 1st Quintile	-.034	(.014)	-.004	(.022)
Pred. Prob. >6 Mths UE W/Controls, 5th Quintile	-.006	(.015)	.032	(.023)
Local Unemployment Rate, 1st Quartile	-.022	(.013)	.046	(.02)
Local Unemployment Rate, 4th Quartile	-.015	(.015)	-.013	(.022)
Ind.-Occ. Separation Rate, 1st Quintile	-.032	(.014)	.003	(.022)
Ind.-Occ. Separation Rate, 5th Quintile	-.001	(.016)	.042	(.023)
Firm Characteristics				
Industry Growth Rate, 1st Quintile	.002	(.016)	.041	(.02)
Industry Growth Rate, 5th Quintile	-.003	(.013)	.025	(.02)
SD of Earnings Growth, 1st Quintile	-.025	(.013)	-.044	(.02)
SD of Earnings Growth, 5th Quintile	-.019	(.014)	.017	(.019)
P75-P25 Earnings Growth Diff., 1st Quintile	-.027	(.012)	-.077	(.022)
P75-P25 Earnings Growth Diff., 5th Quintile	-.007	(.016)	.042	(.022)
Resid. SD of Earnings, 1st Quintile	-.048	(.013)	-.103	(.017)
Resid. SD of Earnings, 5th Quintile	-.008	(.014)	.016	(.02)
Mean Sq. Resid. of Earnings, 1st Quintile	-.046	(.014)	-.053	(.02)
Mean Sq. Resid. of Earnings, 5th Quintile	.004	(.015)	.047	(.021)
Individual Results				
Male	-.029	(.012)	-.004	(.016)
Female	.015	(.014)	.036	(.021)
Age, 1st Quintile	-.018	(.017)	.011	(.022)
Age, 5th Quintile	-.013	(.012)	-.007	(.02)
Blue Collar	.002	(.014)	.026	(.019)
White Collar	-.016	(.013)	-.006	(.017)
Mths since UI Receipt, 1st Tercile	-.012	(.012)	.022	(.016)
Mths since UI Receipt, 3rd Tercile	.038	(.029)	.000	(.047)
Mths since UI Receipt, No Recalls, 1st Half	-.009	(.012)	.023	(.016)
Mths since UI Receipt, No Recalls, 2nd Half	-.002	(.016)	-.005	(.027)
Mths since Non-Emp., 1st Quartile	-.007	(.016)	-.005	(.024)
Mths since Non-Emp., 4th Quartile	.000	(.019)	-.071	(.048)

Note: The table shows heterogeneity in the treatment effect of nonemployment shifts, db/w, on wages. The outcome variable is one- and two-year relative wage growth dw/w. We also report results in Figure 10. Standard errors with two-way clustering at the individual and earnings percentile level in parentheses. See Section 5.1 for additional information.

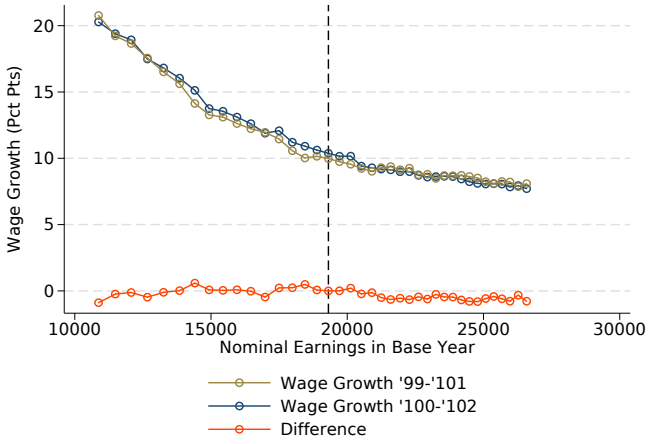
B Additional Figures

Figure A.1: Additional Results: 2001 Reform

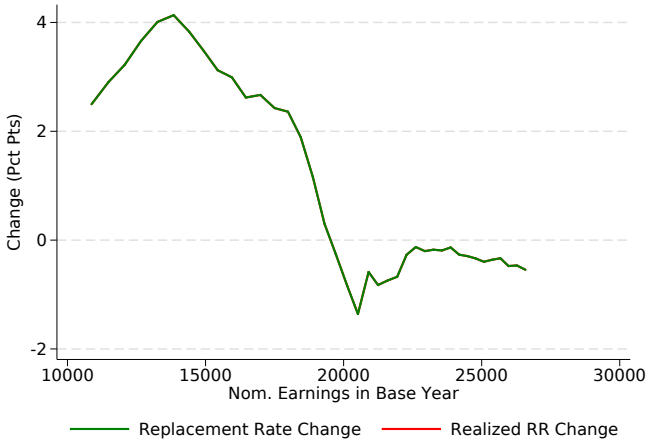
(a) Wage Growth: 2000-2001 vs. 1999-2000; 1 Yr



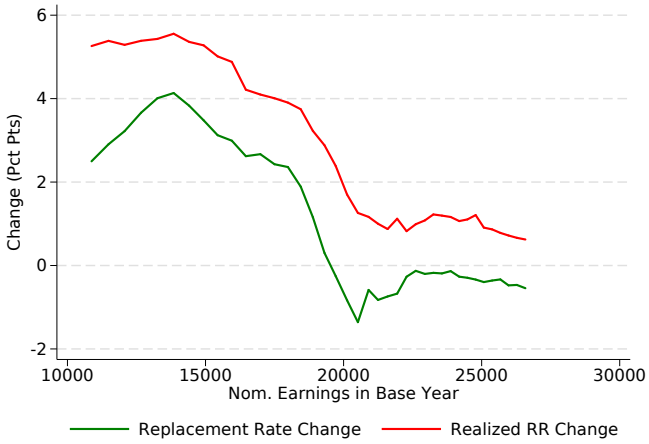
(b) Wage Growth: 2000-2 vs. 1998-2000; 2 Yr



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



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

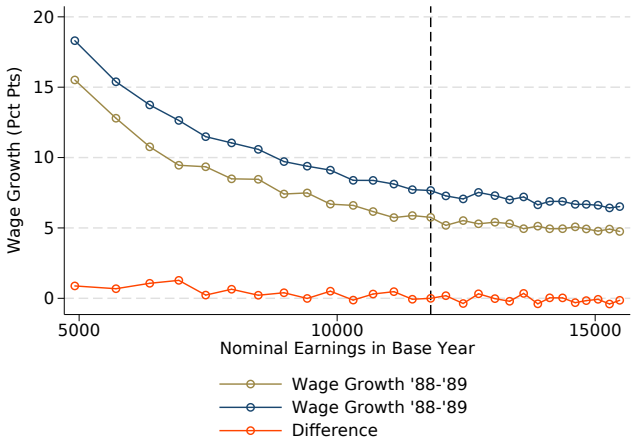


(e) Placebo 2000: 1998-9 vs. 1999-2000; 1Yr

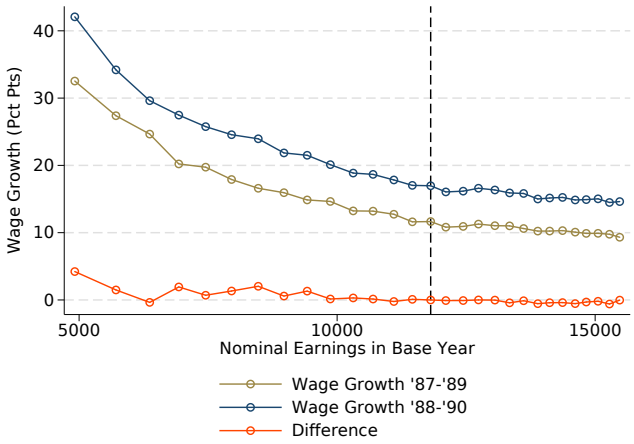
(f) Placebo 1999: 1996-8 vs. 1998-2000; 2 Yr

Figure A.2: Additional Results: 1989 Reform

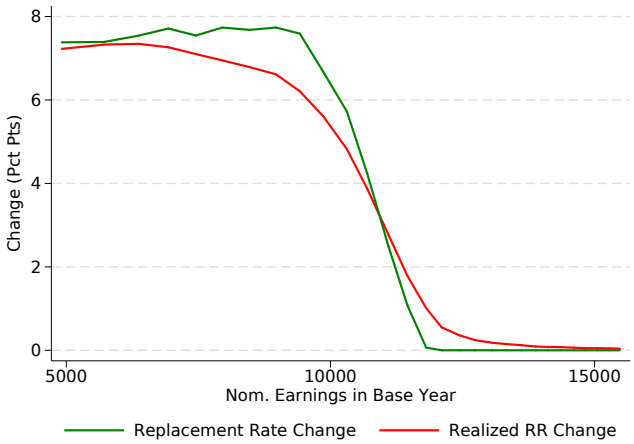
(a) Wage Growth: 1987-8 vs. 1988-9; 1 Yr



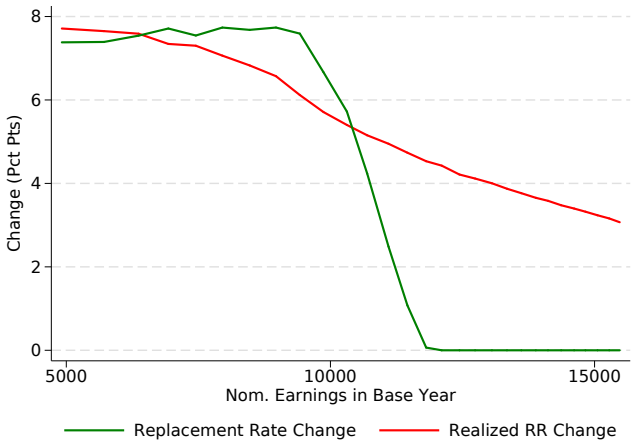
(b) Wage Growth: 1986-8 vs. 1988-90; 2 Yr



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



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

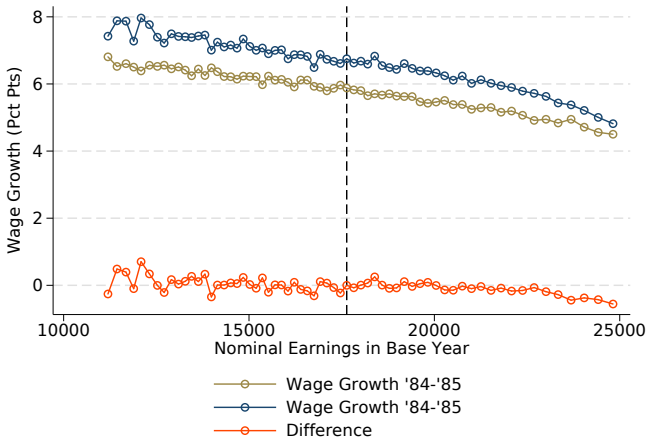


(e) Placebo 1988: 1986-7 vs. 1987-8; 1Yr

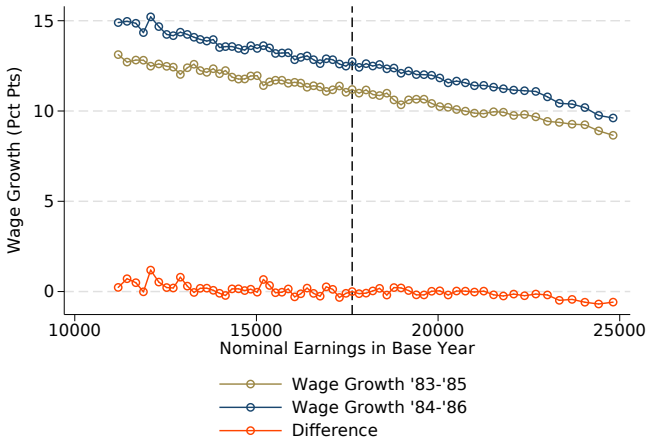
(f) Placebo 1987: 1984-6 vs. 1986-8; 1Yr

Figure A.3: Additional Results: 1985 Reform

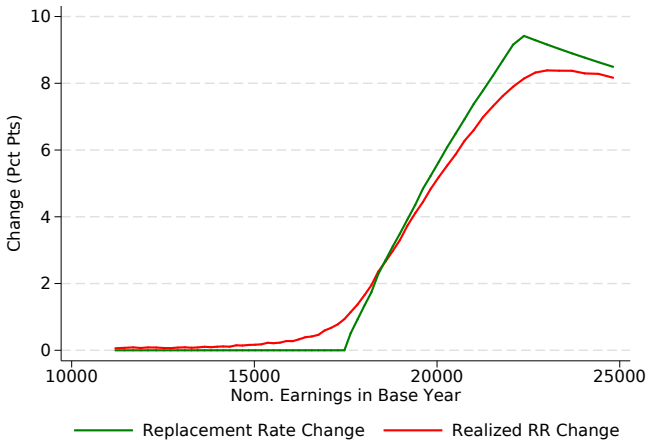
(a) Wage Growth: 1983-4 vs. 1984-5; 1 Yr



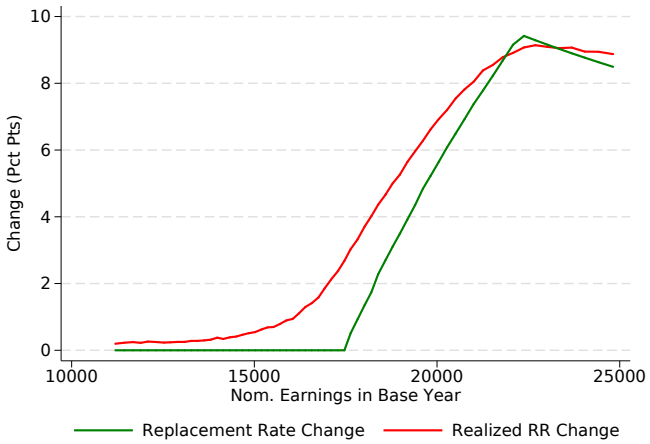
(b) Wage Growth: 1982-4 vs. 1984-6; 2 Yr



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



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

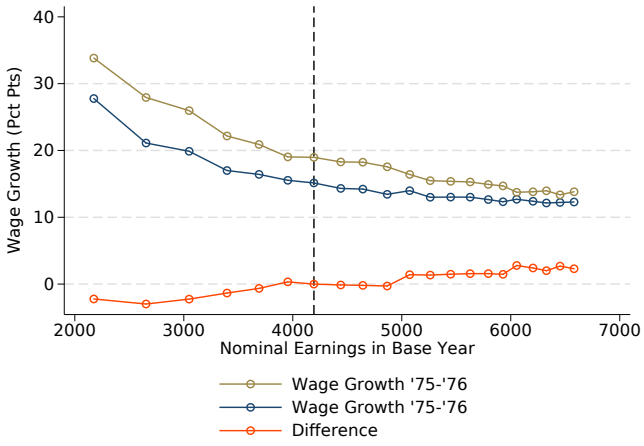


(e) Placebo 1984: 1982-3 vs. 1983-4; 1Yr

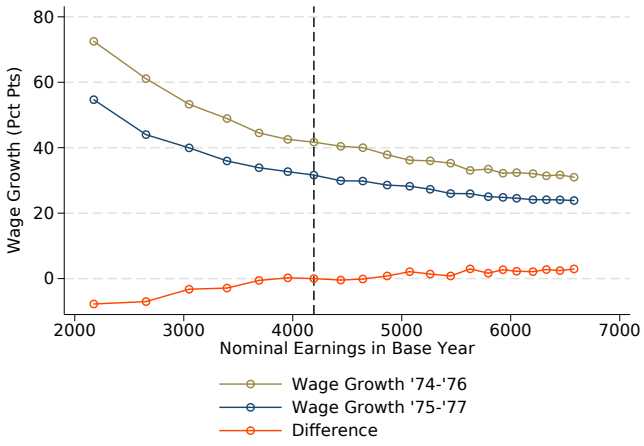
(f) Placebo 1983: 1980-2 vs. 1982-4; 1Yr

Figure A.4: Additional Results: 1976 Reform

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

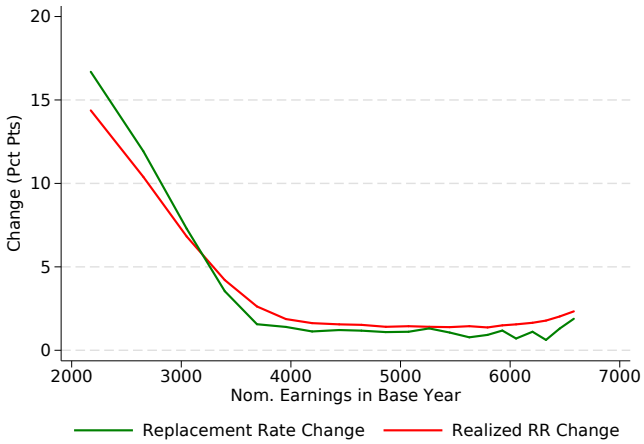


(b) Wage Growth: 1973-5 vs. 1975-7; 2 Yr

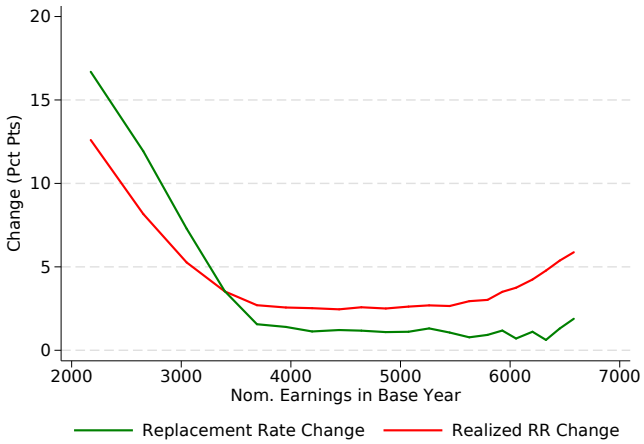


(c) Realized vs. Predicted Benefit Change; 1 Yr

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



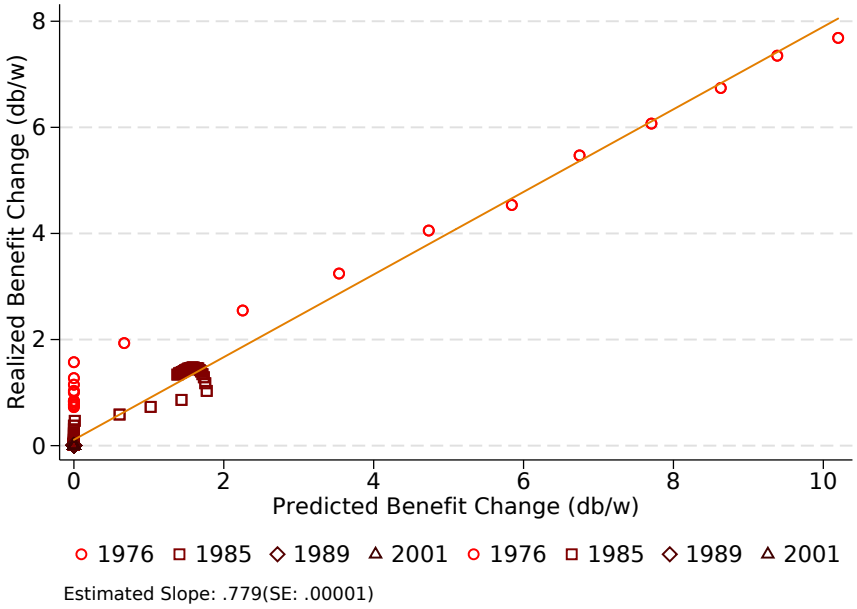
(e) Placebo 1975: 1973-4 vs. 1974-5; 1Yr



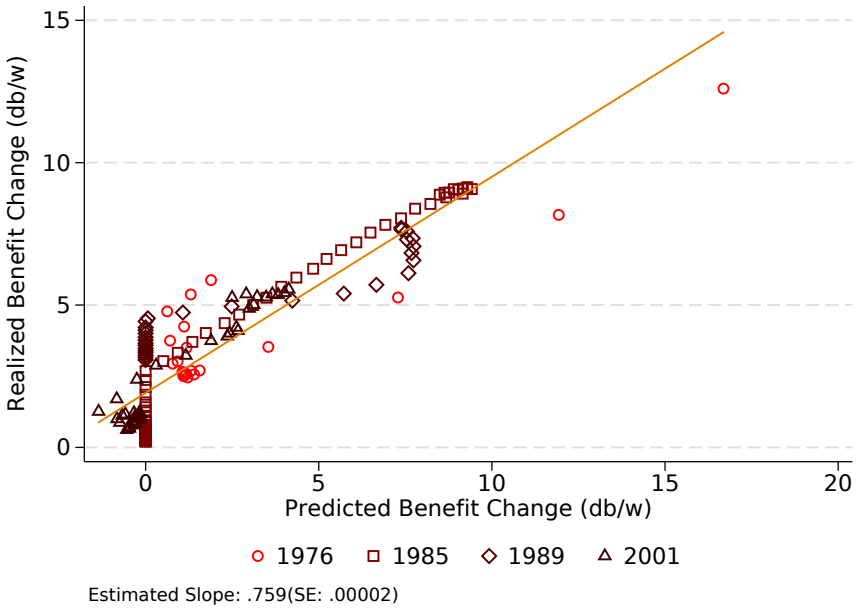
(f) Placebo 1974: 1971-3 vs. 1973-5; 1Yr

Figure A.5: Validation Exercise: Actual UI Benefit Changes Against Predicted Changes

(a) One-Year Horizon



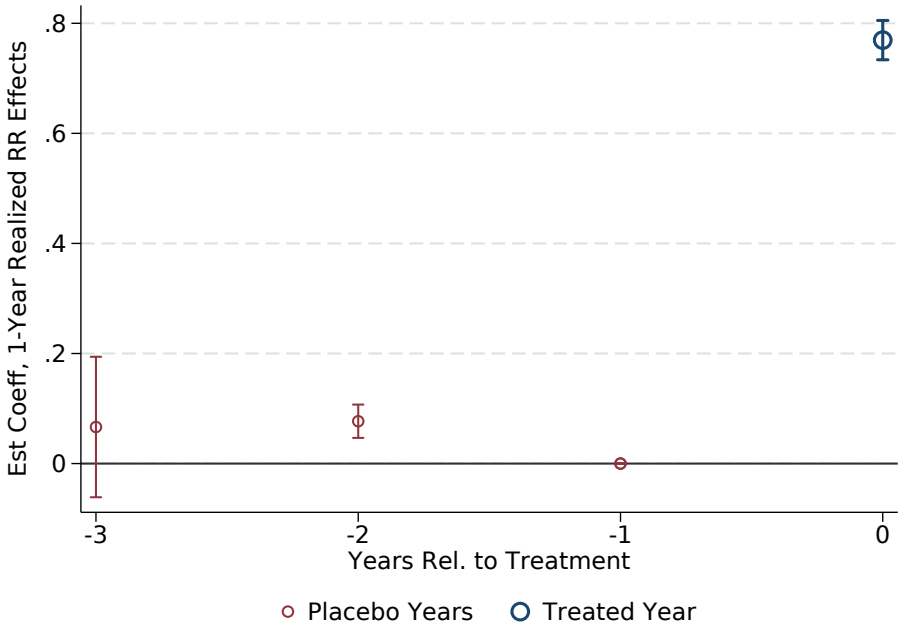
(b) Two-Year Horizon



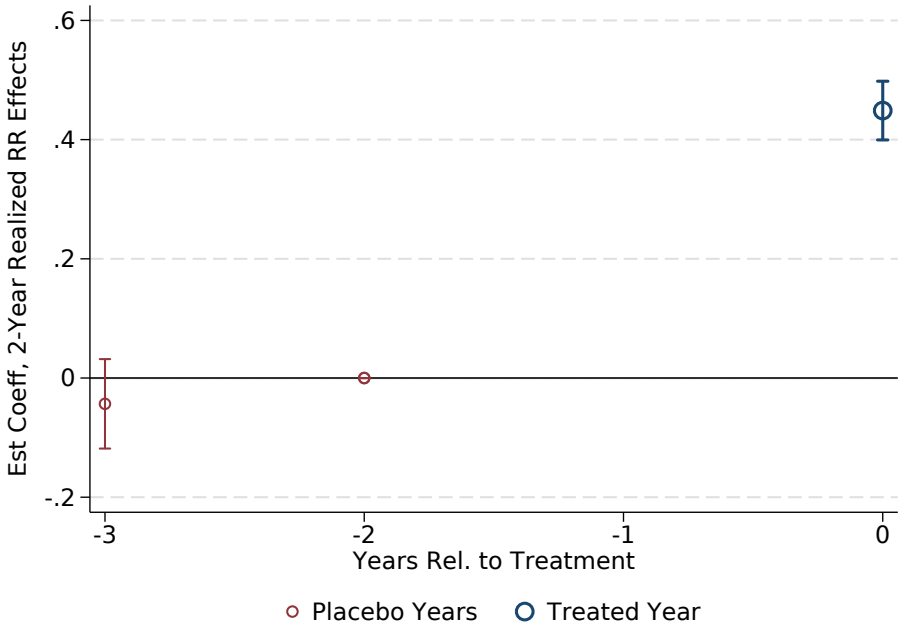
Note: The figures show scatter plots of realized benefit change (y-axis) and reform-induced benefit changes (x-axis), pooling the four reforms outlined in Figures 3 through 6, where each dot corresponds to a percentile observation. The upper panel plots realized benefit change after one year and the lower panel plots the effects after two years. The orange line represents the estimated slope.

Figure A.6: Validation Exercise: Difference-in-Differences Regression Design

(a) Realized Benefit Change: 1 Yr



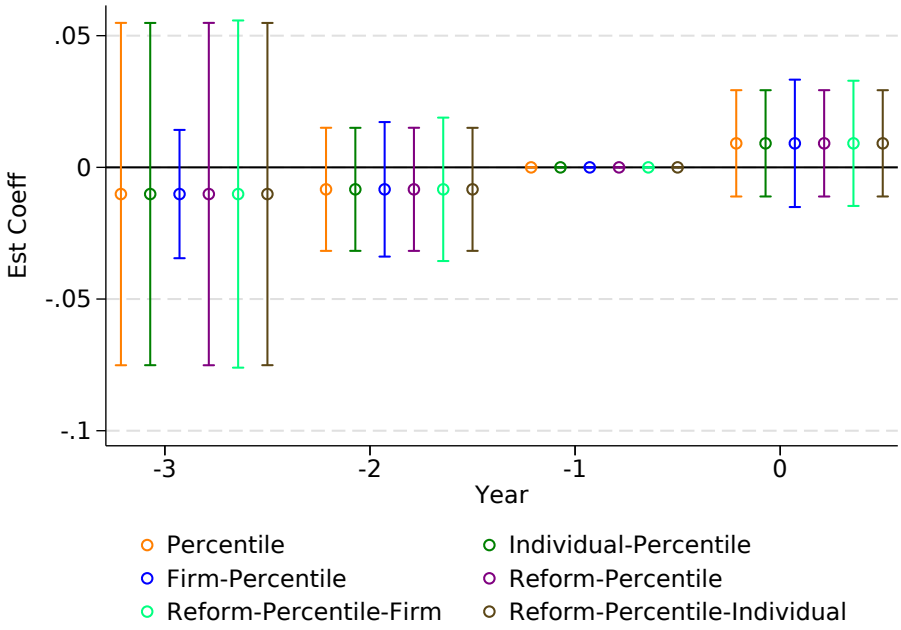
(b) Realized Benefit Change: 2 Yr



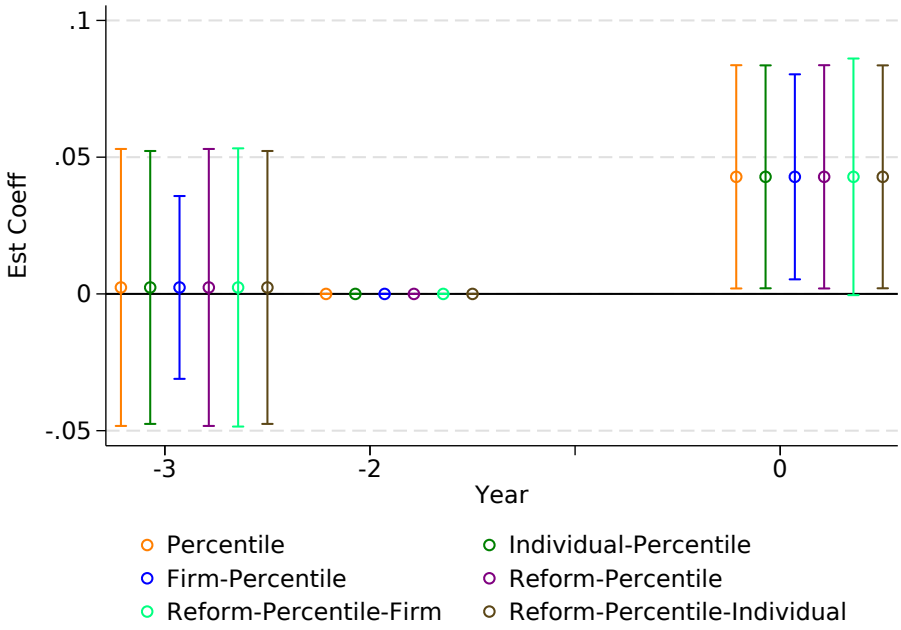
Note: The figures show the effects of reform-induced benefit changes on realized benefit changes based on the difference-in-differences specification in 17. They plot the estimated δ_e^V coefficients and associated confidence intervals. The coefficient in year 0 captures the treatment effect of outside option changes on wages while the coefficients in the pre-period capture the effect of placebo reforms and can be used to gauge the common trends assumption. The sample pools observations from the 1976, 1985, 1989, and 2001 reforms. The specifications include demographic controls and industry-occupation-year effects. Alternative specifications are reported in Table A.5. The vertical capped lines indicate 95% confidence intervals based on standard errors with two-way clustering at the individual and earnings percentile level.

Figure A.7: Robustness Check: Clustering At Different Levels

(a) One-Year Wage Effect



(b) Two-Year Wage Effect

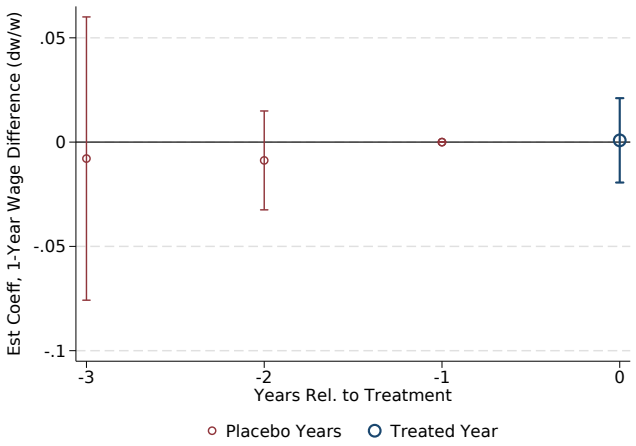


Note: The figure reports robustness checks with confidence intervals based on varying levels of clustering. The point estimates are the same as in the specifications reported in 8 and capture the effect of benefit changes on wages in a difference-in-differences design.

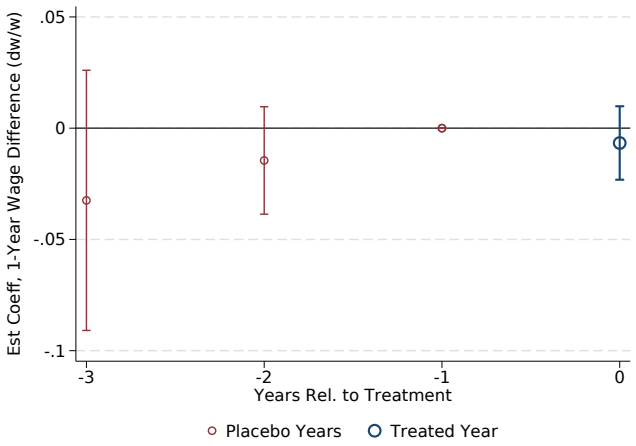
Figure A.8: Robustness Check: Winsorization of Wage Growth

One-Year Horizon

(a) No Winsorization

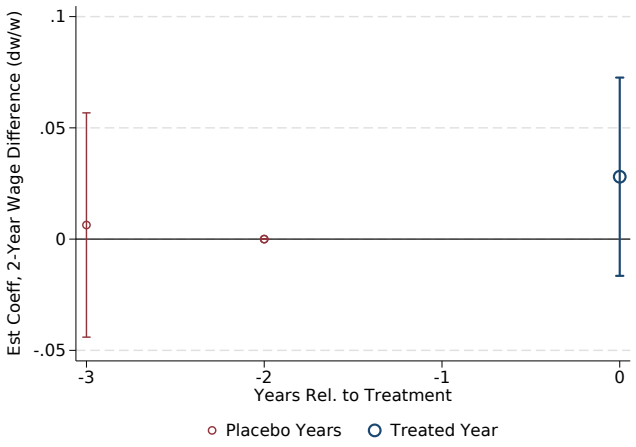


(b) Winsorization at 5th and 95th percentile

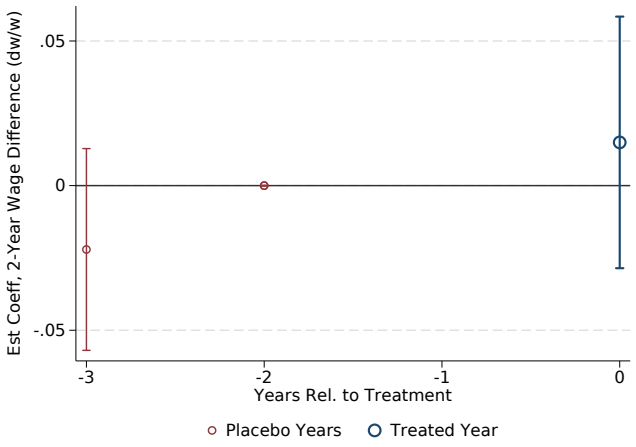


Two-Year Horizon

(c) No Winsorization



(d) Winsorization at 5th and 95th percentile



Note: The figure reports robustness checks that vary the winsorization of the outcome variable for the specifications reported in Figure 8. The specification in Figure 8 was based on clustering at the 1st and 99th percentile. Here, we report results with no winsorization (panels (a) and (c)) and winsorization at the 5th and 95th percentile (panels (b) and (d)).

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 2.2. Here we generalize the structural wage equation by a variety of dimensions, starting with a bilateral bargaining between a worker and a multi-worker firm, long-term jobs and non-linear utility.

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

Household. Labor is indivisible and hours are normalized to one. In a given period s, the household is either employed or unemployed ($e_s \in \{0, 1\}$). There is no direct labor supply channel; workers accept job opportunities when they emerge. When employed, the worker earns wage w_s. The employed household incurs labor disutility γ. When unemployed, the worker collects unemployment insurance benefits b. With probability f, the worker finds a job and moves into employment (and wage bargaining) next period. With probability $1 - \delta$, employed job seekers lose their jobs and become unemployed. Households can borrow and save at interest rate r, fulfilling a lifetime budget constraint.[80] Households own firms and collect capital income in form of dividends d_t.

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

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

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

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

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

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

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

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

[80] Due to absence of moral hazard in job search and due 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, $\lambda(e = 0) \approx \lambda(e = 1)$.

labor, plus the shift in the continuation value:

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

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.[81] 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):

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

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

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:

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

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

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

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

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

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

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

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) – such as the individual household with whom the firm is bargaining – at an arbitrary wage w – such as the one that will be to be determined in the bargaining process – is:

$$\Delta V_t^F(N_t, w) = \lambda[F_N(K_t, N_t) - w] + (1 - \delta)\beta {V_{t+1}^F}'(N_{t+1}) \tag{A16}$$

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

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

surplus of the match to the worker $W(w) - U$ and of the firm $\Delta V_t(N_{t-1}, w)$, weighted by exponents ϕ and $1 - \phi$:

$$w^N = \arg\max_w \left(W(w) - U\right)^\phi \times \left(\Delta V_t^F(N_t, w)\right)^{1-\phi} \tag{A17}$$

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

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

Myopic bargaining ($\beta = 0$). If the firm and worker bargaining with $\beta = 0$, the worker's outside option is non-employment, in which case she collects unemployment insurance benefits b. Labor supply disutility is γ. Under these conditions, the worker's surplus from the job wage w is vs. unemployment is:

$$W_t(w) - U_t = \lambda(w - b) - \gamma \tag{A19}$$

And the firm's value from employment the incremental worker at wage w is:

$$\Delta V_t^F(N_t, w) = \lambda[F_N(K_t, N_t) - w] \tag{A20}$$

The myopic Nash wage returns the simple wage rule: the average of the outside option (unemployment benefits plus the marginal rate of substitution (the disutility of labor divided by the marginal utility of consumption), and the marginal product of labor – weighted by worker bargaining power ϕ:

$$w^N_{\beta=0} = (1 - \phi)\left(b + \frac{\gamma}{\lambda}\right) + \phi F_N(K_t, N_t) \tag{A21}$$

Bargaining with dynamic considerations. The full model recognizes the long-term nature of jobs.[82] Wages then not only reflect current conditions but also expectations about future inside and outside values, through the continuation values. An important implication of Nash bargaining to apply also in subsequent period, renders the Nash wage identical to the myopic thought experiment except for a continuation term:[83]

$$w^N = (1 - \phi)\left(b + \frac{\gamma}{\lambda}\right) + \phi F_N(K_t, N_t) + (1 - \phi) f \beta \mathbb{E}_t \frac{\widetilde{W}_{t+1} - U_{t+1}}{\lambda} \tag{A22}$$

The first components precisely mirror the standard quasi-spot conditions in myopic wage bargain (A21). That is, *over the course of the job at hand*, the worker's idiosyncratic variation in outside option b pass through into the Nash wage w^N exactly through $(1 - \phi)$.

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

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

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

C.2 Finite vs. Infinite Potential Benefit Duration

Here we discuss a simple model of finite benefit duration summarized in Section 2.2.

Unemployed job seekers on and off UI. Let $e \leq T$ be the days elapsed since nonemployment duration, and T is the potential benefit duration. The unemployment job seeker collects b while being eligible for UI, and collects potential alternative program substitutes s while being nonemployment after exhausting the potential benefit duration.[84] The values of an unemployed worker of elapsed days of UI PBD T $e <$, $e = T$, a worker on a post-UI-exhaustion program substitute S, and of a worker coming out of unemployment with elapsed days W_e, and W_0 is:

$$\begin{aligned} U_e &= b + \beta f_e \cdot W_{e+1} + \beta(1 - f_e) \cdot U_{e+1} \quad \text{if } e < T \\ U_e &= b + \beta f_e \cdot W_{e+1} + \beta(1 - f_e) \cdot S \quad \text{if } e = T \\ S &= s + \beta f_s \cdot W_s + \beta(1 - f_s) \cdot S \end{aligned}$$

Bargaining and the types of worker surplus. Nash bargaining post-match formation proceeds by the standard formula, except that workers' threat point of nonemployment Ω depends on UI status and elapsed days e:

$$w_i^N = \arg\max_w \left(W(w) - \Omega\right)^{\phi} \times \left(V_n(w)\right)^{1-\phi} \tag{A23}$$

Next, we discuss worker surplus $W(w) - \Omega$ for employed workers (who have unemployment with full PBD as the outside option) and newly hired workers (where the initial wage bargain depends on the remaining UI eligibility, and thus elapsed days e).

Incumbent worker in a continuing job with thus full PBD. A worker's PBD resets over the course of the match.

$$\begin{aligned} W(w_0) - U_0 &= w_0 - b + \beta(1-\delta)W_0^+ + \delta\beta U_0^+ - \beta f_0 \tilde{W}_1^+ - (1 - f_0)\beta U_1^+ && \text{(A24)} \\ &= w_0 - b + \beta(1-\delta)(W_0^+ - U_0^+) + \beta U_0^+ - \beta f_0 \tilde{W}_1^+ - (1 - f_0)\beta U_1^+ && \text{(A25)} \\ &= w_0 - b + \beta(1-\delta)(W_0^+ - U_0^+) - \beta f_0(\tilde{W}_1^+ - U_1^+) + \beta(U_0^+ - U_1^+) && \text{(A26)} \end{aligned}$$

which nests the workhorse model with infinite benefit duration for $T \to \infty$.

New hire from unemployment. The new worker that found the job with elapsed duration $e-1$ enters the job and thus wage negotiations with eligibility e in hand if she were to redeem the outside option. However, she would turn into an incumbent worker if the job continues, resetting e to 0, an "entitlement effect". The expression for worker surplus in bargaining then is:

$$\begin{aligned} W(w_e) - U_e &= w_e - b + \beta(1-\delta)W_0^+ + \delta\beta U_0^+ - \beta f_e \tilde{W}_{e+1}^+ - (1 - f_e)\beta U_{e+1}^+ && \text{(A27)} \\ &= w_e - b + \beta(1-\delta)(W_0^+ - U_0^+) + \beta U_0^+ - \beta f_e \tilde{W}_{e+1}^+ - (1 - f_e)\beta U_{e+1}^+ && \text{(A28)} \\ &= w_e - b + \beta(1-\delta)(W_0^+ - U_0^+) - \beta f_e(\tilde{W}_{e+1}^+ - U_{e+1}^+) + \beta(U_0^+ - U_{e+1}^+) && \text{(A29)} \end{aligned}$$

which is mirrors the workhorse model with permanent benefit duration except for three difference, which disappear with $T \to \infty$. For finite T, three differences emerge.

[84] We assume that taking up employment fully resets eligibility.

First, the continuation worker surplus $\beta(1-\delta)(W_0^+ - U_0^+)$ now references the continuing job, such that the outside option is full PBD. This term will be equal to the firm's continuation value in this job, and therefore still cancel out, as in standard Nash with infinite PBD.

Second, continuation component of the outside option, $\beta f_e(\tilde{W}_{e+1}^+ - U_{e+1})$, is from the perspective of the worker that has lost one period of eligibility vis-à-vis the bargaining situation. For example, in a DMP model with directed search by e, this value is pinned down by the free entry condition. With random search, this mapping also depends on the distribution of searchers by elapsed duration.

Third, the novel term $\beta(U_0^+ - U_{e+1})$ captures the value difference between the unemployed worker with full eligibility $e = 0$ and with one more period elapsed (e.g. $e = 1$ for a currently incumbent worker). This captures the fact that redeeming the outside option rather than forming the match lowers surplus through an entitlement effect.

Nash wage. The same expression guides the Nash wage:

$$w_e^N = \arg\max_w (W_e(w) - U_e)^\phi V_{n,e}(w)^{1-\phi} \tag{A30}$$

$$(1-\phi)(W_e(w_e^N) - U_e) = \phi V_{n,e}(w_e^N) \tag{A31}$$

$$w_e^N = (1-\phi)b + \phi F_N + (1-\phi)\beta f_e(\tilde{W}_{e+1}^+ - U_{e+1}^+) - (1-\phi)\beta(U_0^+ - U_{e+1}^+) \tag{A32}$$

Gauging the effect of finite durations on the wage bargain. Next, we reformulate the wage expression with finite benefit duration to mirror the standard infinite-PDB wage bargain plus an adjustment term. We then evaluate this adjustment term quantitatively, to assess the attenuation in the predicted wage sensitivity to b the incorporation of this institutional feature may bring.

We consider an employed worker $e = 0$. We therefore need to evaluate $U_0^+ - U_{e+1}^+ = U_0^+ - U_1^+$. To make progress, we first suppose that $f_0 \approx f_1$ (e.g. due to random search and without job search effort choices by workers). Moreover, suppose that $w_e = w_{e'} \forall e \geq 1$, i.e. wages of new hires from nonemployment are not differentiated by duration of the unemployment spell; although we allow for wages of incumbent workers to differ from those of hires from nonemployment. These simplifications focus the exercise to a tractable result. In this case, the difference in unemployment values between a full-PDB ($e = 0$) worker and one with elapsed UI claims e is simply the present value of the difference in flow benefits while unemployed. Specifically, for the case of the incumbent worker, we have:

$$U_0 - U_1 = (1-f)^T \beta^T (b - s) \tag{A33}$$

That is, the size of this effect depends on the difference between b and s, the probability of the unemployment spell to go beyond the potential benefit duration T, and discount factor T. The Nash for this worker is then:

$$w_0^N = (1-\phi)b + \phi F_N - (1-\phi)\beta f(\tilde{W}_1^+ - U_1^+) + (1-\phi)(1-f)^T \beta^{T+1}(b-s) \tag{A34}$$

$$= (1-\phi)b \underbrace{\times \left(1 - \left[1 - \frac{s}{b}\right](1-f)^T \beta^{T+1}\right)}_{\text{attenuation: finite vs. infinite PBD}} + \phi F_N + \beta f(\tilde{W}_1^+ - U_1^+) \tag{A35}$$

That is, the pass-through from b into wages is attenuated by vis-à-vis the benchmark model by a factor that depends on the ratio of s to b, the probability of the unemployed worker not exiting the

unemployment state until T and discount factor β.

Only around 10% of job seekers exhaust their potential benefit duration in the 2000s. This implies that $(1-f)^T = 0.1$. Therefore, even if $\beta \approx 1$ and $s = 0$, the expression is very close to the expression ignoring the finite benefit duration. The reason is that we examine the *incumbent* worker, where the probability of exhausting benefits is in expectation low. Finally, post-UI program substitute s maps disability insurance or unemployment assistance for Austrian workers, where take-up is lower and benefits are capped at 92% of unemployment benefits. For 1990, Lalive et al. (2006) report that median "Notstandshilfe" was about 70 % of the median UIB. Based on data from 2004, Card et al. (2007) gauge the average "Notstandshilfe" at 38 % of UIB for the typical job loser.

Finite duration vs. constant hazard of benefit loss. Given the complications associated with keeping track of the duration elapsed, a pragmatic approach in the literature is to impose a constant probability of switching from an eligible state into the ineligible state. From the perspective of a currently employed worker, this approach would be particularly suitable as our focus is not on tracking unemployed job seekers with potentially evolving remaining duration.

C.3 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) and Hall (2017)).

The firm and the worker make alternating wage offers. In between bargaining rounds, the firm incurs a delay cost γ. Importantly, in our discussion here we allow the worker's flow utility is 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 breaks up. The probability of this bargaining-stage separation is s, which may be different from the probability of standard exogenous job destruction during production, δ. U will therefore enter the problem either through s or δ, with importantly opposite effects on the worker's reservation wage, as we show below.

Inside values. Preserving unemployment value U 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):

$$W(w) = \frac{w + \beta\delta U}{1 - \beta(1-\delta)} \tag{A36}$$

$$J(w) = \frac{y - w}{1 - \beta(1-\delta)} \tag{A37}$$

Strategies for wage offers. The optimal strategies are described by reservation wages. The worker's reservation wage is $\underline{w}$, and the firm's reservation wage is $\overline{w} > \underline{w}$, which we have yet to

derive. When it is the worker's (firm's) turn to make an offer, she (it) will offer $\overline{w}$ ($\underline{w}$), leaving the firm (worker) indifferent between rejecting and rebargaining.

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

$$\frac{y-\overline{w}}{1-\beta(1-\delta)} = -\gamma + \beta(1-s)\frac{y-\underline{w}}{1-\beta(1-\delta)} \tag{A38}$$

$$y-\overline{w} = -(1-\beta(1-\delta))\gamma + \beta(1-s))(y-\underline{w}) \tag{A39}$$

$$\overline{w} = (1-\beta(1-\delta))\gamma + \beta(1-s))\underline{w} - y(1-\beta(1-s)) \tag{A40}$$

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

$$\frac{\underline{w}+\beta\delta U}{1-\beta(1-\delta)} = z + (1-s)\beta\frac{\overline{w}+\beta\delta U}{1-\beta(1-\delta)} + s\beta U \tag{A41}$$

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:

$$\Leftrightarrow \underline{w} = (1-\beta(1-\delta))z + \beta(1-\beta(1-\delta))U \tag{A42}$$

The worker's reservation wage is maximally sensitive to U 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:

$$\Leftrightarrow \frac{\underline{w}}{1-\beta(1-\delta)} = \overbrace{z}^{\text{flow value while barg.}} + (1-s)\overbrace{\beta\frac{\overline{w}}{1-\beta(1-\delta)}}^{\text{Follow-up offer}} + \beta\overbrace{(s-\delta)\frac{1-\beta}{1-\beta(1-\delta)}U}^{\text{Rel. unemp. risk: bargaining vs. producing}} \tag{A43}$$

$$\underline{w} = (1-\beta(1-\delta))z + (1-s)\beta\overline{w} + \beta(s-\delta)(1-\beta)U \tag{A44}$$

While we can derive the analytical expression, given U, for the worker's reservation wage, we focus on the expression with $\overline{w}$ for simplicity.[85]

The role of s vs. δ in mediating the effect of U on worker reservation wages. As in the standard Nash model, U denotes both the outside option of the worker in case of bargaining

[85] Combining expressions for worker and firm reservation wages, the worker's reservation wage (and the optimal wage the firm would offer the worker) is:

$$\underline{w} = \frac{(1-\beta(1-\delta))z + (1-s)\beta\left[(1-\beta(1-\delta))\gamma + y(1-\beta(1-s))\right]}{1-\beta^2(1-s)^2} + \frac{\beta(s-\delta)(1-\beta)}{1-\beta^2(1-s)^2}\times U \tag{A45}$$

breakdown during the bargaining process (weighted by s) as well as the value of an exogenous job destruction (arriving with probability δ). The net effect of U on the worker's reservation wage $\underline{w}$ depends on the relative size of s and δ in the alternating offer bargaining protocol.

A useful benchmark is $s = \delta$. Here, the worker is exposed to U 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 δ, and thus she puts a δ weight on U one period earlier). In this knife-edge case, the worker's reservation wage $\underline{w}$ turns *completely insensitive to U – and thus b*, and is only driven by the while-bargaining flow utility z (which need not contain b) and the (present value of the) wage gain resulting from getting the chance to make the (in subgame perfect equilibrium expected to be accepted) counteroffer, $\overline{w}$.

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

Calibrating our evidence (assuming that $dU(1-\beta) \approx db$) suggests that $s \approx \delta$, enabling AOB to accommodate our results.

Whether $s \approx \delta$ is empirically realistic as such is difficult to assess because independently calibrating s directly to empirical evidence is not straightforward.[86] For example, Hall (2017) calibrates $s = 0.013$ and $\delta = 0.0345$, which here would lead worker reservation wages to *fall* when U 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 U to *increase* wages (reservation wages of the worker) ceteris paribus.

The role of z vs. b. While we intentionally separately define z (the flow utility of the worker while bargaining, perhaps not containing b for e.g. an incumbent worker) from b (the non-employment value outside of the bargaining situation, contained in U). The original authors and the follow-up literature (Christiano et al. (2016), Hall and Milgrom (2008), Hall (2017)) set both to be the same, and thus explicitly include unemployment benefits in $z = b$. Therefore, the insensitivity of w to b that we document in this paper would be a puzzle to the standard specifications of the protocol. We close by stating that the original specification aimed to reduce the sensitivity of w to *unemployment duration* and labor market tightness rather than to the flow utility of a given unemployed job seeker. To quote Hall (2017), "the credible bargaining equilibrium is less sensitive to conditions in the outside market" (p 310). It is true that the wage will be less sensitive to θ, although not to z. Perhaps the key to understanding AOB in the context of incumbent workers' wage negotiation lies therefore in rethinking z not as the non-employment value but as the flow utility of the employed worker during negotiation, which likely remains unaffected by b. We explore this augmented bargaining model in ongoing research as a promising resolution.

C.4 Labor Market Monopsony

C.5 Directed Search

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

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 pertubation in the inside value of the employment relationship the "myopic" version of the myopic bargaining setup is:

$$w^N = \phi \times \underbrace{p_i}_{\text{Rent sharing variation}} + (1-\phi) \times b \tag{A46}$$

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:[87]

$$\xi = \frac{dw/w}{dp/p} \tag{A47}$$

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

$$\frac{dw^N/w^N}{dp/p} = \phi \times \frac{p}{w^N} \tag{A48}$$

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

$$\phi = \frac{w}{p} \cdot \xi \leq \xi \tag{A49}$$

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

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

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

wage–productivity elasticity ξ as follows:

$$\phi = \frac{b\xi}{p(1-\xi)+b\xi} = \frac{1}{\frac{p}{b}\cdot\frac{1-\xi}{\xi}+1} \tag{A50}$$

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

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

The information needed to translate a given value added rent sharing elasticity into the point estimate for ϕ therefore requires strong assumptions or empirical knowledge about b. Measuring the level of the worker's flow valuation of non-employment 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 $\frac{MPL}{MPL-z}$).

Surplus (profit) elasticities. Alternative elasticity specifications feature profits rather than value added, often interpret as flow surplus $s = z - b$. The structural equation would then imply:

$$\xi^S = \frac{\partial w/w}{\partial s/w} = \frac{\phi(p-b)}{w} = \frac{\phi(p-b)}{b+\phi(p-b)} \tag{A51}$$

where the elasticity represents the fraction of the wage that comes from the worker's share of the surplus. A given estimate of ξ^S then implies the following bargaining parameter:

$$\phi = \frac{b}{p-b}\frac{\epsilon^S}{1-\epsilon^S} \tag{A52}$$

The estimated wage elasticity therefore maps into ϕ scaled by the ratio of b to the surplus. Economic theory bounds $b/s \in [0, \infty)$, and therefore leaves $\phi \in (0, 1]$ for any given positive elasticity, absent information on b and $s = p - b$. Empirical estimates suggest the elasticity to be small, around 0.1. The *highest* ϕ rationalized by this shift is $\overline{\phi} = 1$ for $\frac{b}{p-b} = 9$. For lower bound $\underline{\phi} \approx 0$ can be rationalized with $\frac{b}{p-bz} \approx 0$. Importantly, $\phi(\epsilon) \in (0, 1]$ holds for any given shift in ϵ, absent a stance on $\frac{b}{p-b}$.

In fact, *if* the required values of b and $p - b$ *were* known, we would not require any quasi-experimental shifts in p to identify ϕ to begin with. Instead, one could simply read ϕ off the Nash wage equation for a given w, p and b:

$$\phi = \frac{w-b}{p-b} \tag{A53}$$

The wage–surplus elasticity therefore would not actually provide any additional information once one takes a stance that would translate surplus elasticities into point estimates of ϕ.[88]

Level specifications. To avoid the complications arising from elasticities, one route is to measure ϕ with *level* shifts to surplus rather than percentage shifts. Provided a clean and quantifiable measure of the marginal product of a worker in absolute levels, this variation would identify ϕ. One recent example is Kline et al. (2017), who estimate the effect of patents on profits and wages. We do not know of attempts to isolate individual- or firm-level productivity shifters in levels, which would map more directly into the model and avoid profit variation from, e.g., markups.

[88]Perhaps the elasticity may reveal how marginal shifts in surplus are split, in case average and marginal surplus were shared differently (not the case in the leading models).

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 (ATS 10,000 * 2 / 12 = ATS 1,667). A reform in 1987 changed the calculation period from the last month before unemployment to the last six months before unemployment, while still factoring in the 13th and 14th monthly salary proportionally. A 1996 reform then changed the calculation more substantially by using last year's earnings for unemployment spells beginning after June 30 of a given year and the earnings in the second to last year for spells beginning before June 30. The 1996 reform left the treatment of the 13th and 14th salaries unchanged.

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.[89] To this end, we merge the unemployment benefit data (AMS) with the ASSD (social security based data), which contains our earnings measure. All measures are nominal and not inflation-adjusted.

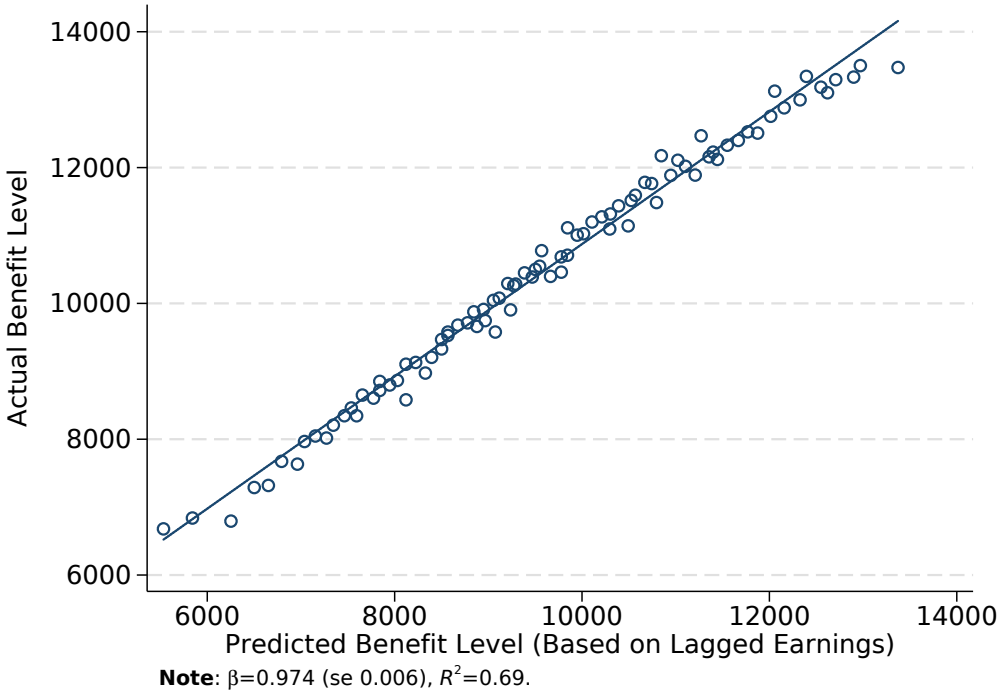
Figure A.9 plots the relationship between actual and predicted UI benefit levels for all Austrian separators drawing UI benefits. The relationship traces out a slope that is on average 0.974. We

[89]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.

therefore conclude that our approach accurately assigns employed workers by their ASSD-based earnings into the UI benefit levels.

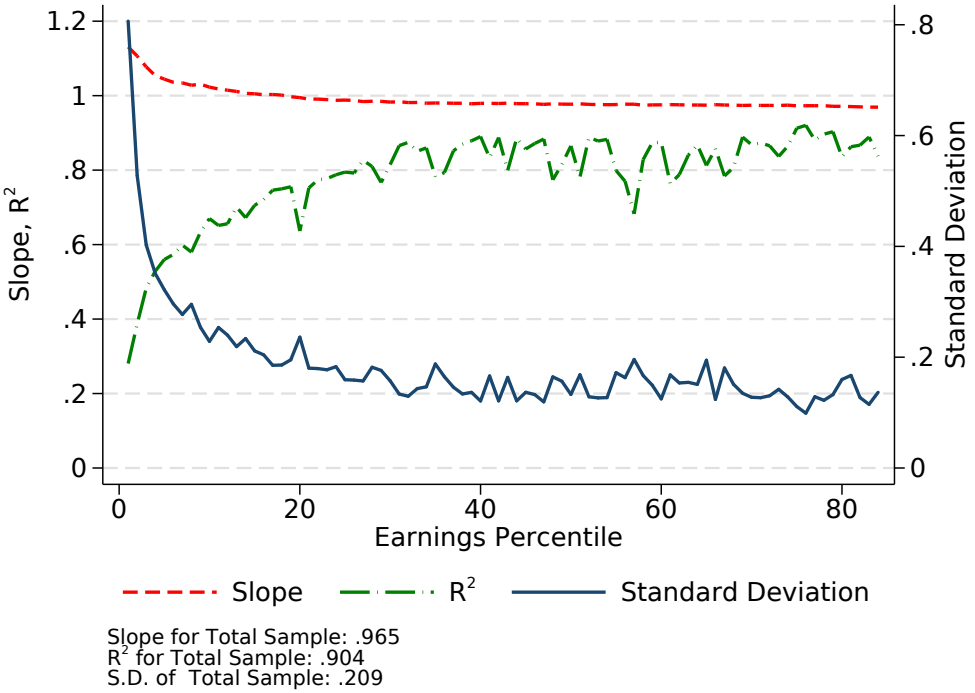
In addition, we also validate that our earnings prediction works well across the earnings distribution with coefficients on predicted and actual benefits close to 1 throughout (Figure A.10).

Figure A.9: 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.10: Quality of Wage Prediction Procedure



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

E.3 UI Take-Up Among Austrian Separators

Besides the quasi-experimental variation in benefits, the second motivation to study our question in the Austrian setting is the large take-up of UI conditional on a separation into nonemployment. This fact arises from a confluence of broad eligibility rules (incl. quitters), lax experience tests, and low job finding rates compared to e.g. the United States. In addition, Austrian job seekers receive health insurance through the unemployment insurance system when registered as unemployed.

To supplement our institutional review, we now provide a direct analysis of UIB take-up of Austrian workers separating from a job into nonemployment, finding that more than 60% of workers separating into nonemployment receive UIB. Therefore, UI is indeed part of the nonemployment value, validating our research design to use a shift in the level of UIBs as a shift for the value of nonemployment.

We study a sample of workers undergoing a transition from employment to nonemployment for years 1978 to 1999. Our definition of nonemployment is at least one day at which the worker does not have an employment spell associated with her. Our only sample restriction is that we require workers to ultimately return to work after two years, to preclude e.g. retirement or other permanent transitions. Yet, we otherwise do not directly restrict the sample of transitions by type – layoff or quit, or even maternity leave of potentially a temporary layoff. In consequence, we do include transitions into disability or sickness statuses, which in turn would administratively render the individual ineligible for UI; those individuals would be counted as nonemployment but not among the UI recipients. Analyzing this broad a range of E-N transitions likely provides a lower bound on take-up of UI conditional conditional on separating. Measurement error (e.g. in the ASSD

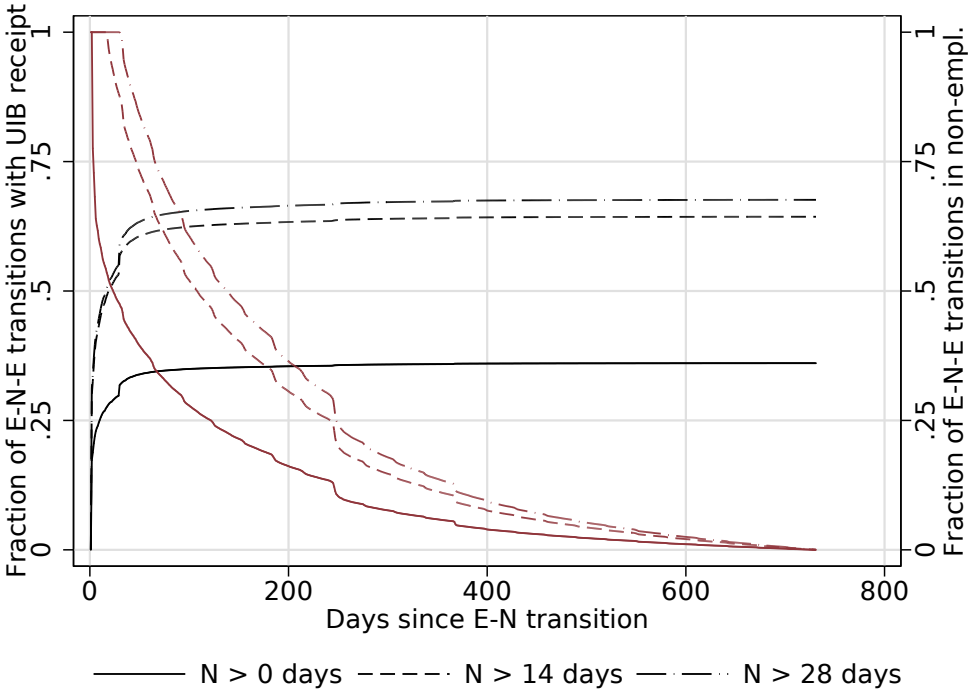
self-employment or government jobs are not covered and thus show up as nonemployment) likely leads us to further underestimate take-up.

We summarize our findings in Figure A.11, which traces out the survival function of (staying nonemployed and becoming employed) as a function of days nonemployed, along with the fraction of initial job separators that have so far received UIBs at the given day in this nonemployment spell. Together, these two figures reveal: first, how many workers with nonemployed spells that lasted at least x days have received UI benefits (red line)? Second, how many workers that separated into nonemployment are still nonemployed after x days (black line)?

The key statistic from this is how many workers will ultimately have collected UIBs at any point. This number is around 60%. Therefore, among separating workers and staying nonemployed for more than 28 days, i.e. including those who quit their last job, more than 60% will receive UIBs during the subsequent nonemployment spell. Interestingly, this graph is also very steep away from zero days: workers that become nonemployed quickly collect UIBs. Even in the first weeks, already more than 50% of workers have collected UI.

We close by noting that the nonemployment option in wage bargaining refers to a hypothetical scenario in which bargaining were to break down absent an agreement in the first round. In subgame perfect equilibrium, such while-bargaining separations never occur because the first offer would always be accepted. It is therefore impossible to measure the UIB receipt path and nonemployment path entailed by such while-bargaining separations: are they quits, or layoffs? How selected are those workers? How does a potential stigma from such nonemployment look like? Extrapolating from our empirical analysis of average separations, does imply that UI should be on the bargaining parties' mind more than in most other countries. Still, fewer than 100% of workers receive UI benefits during their nonemployment spells.

Figure A.11: Take-up of UI Benefits among all workers undergoing E-N transitions.



Note: The Figure tracks the sample of job separators entering nonemployment for at least 1, 15 or 29 days. The red lines describe the survival functions, i.e. the fraction of workers that are still in nonemployment (y-axis) after a given amount of days (x-axis). The black lines describe the fraction of workers that have received unemployment insurance benefits at a given duration of the nonemployment spell. The sample is all separators between 1978 and 1999, with the restriction that all nonemployment spells are completed after two years.

E.4 Beyond Unemployment Benefit Levels: No Effect for Potential Benefit Duration Reform on Incumbent Wages in 1989

This reform increased potential benefit duration for workers aged 40 and above in 1989. We plot the wage growth for workers by age.

Figure A.12: PBD Schedule - Treated and Control Years

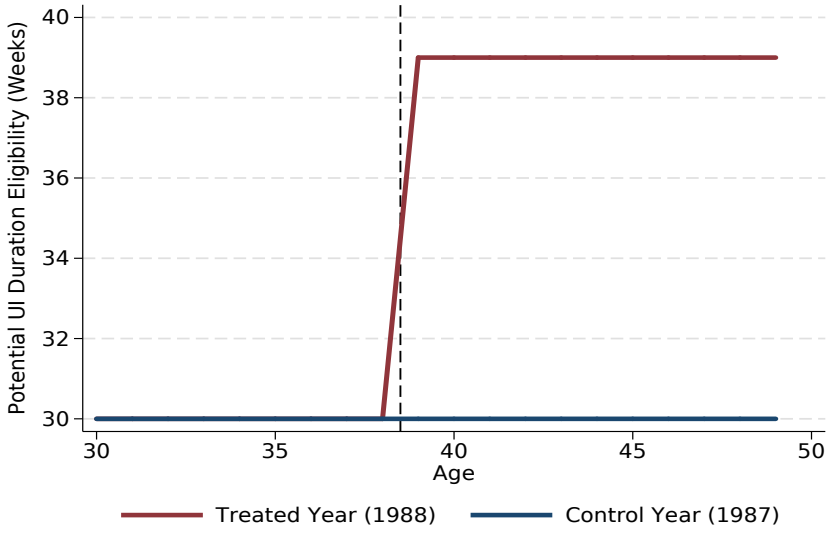
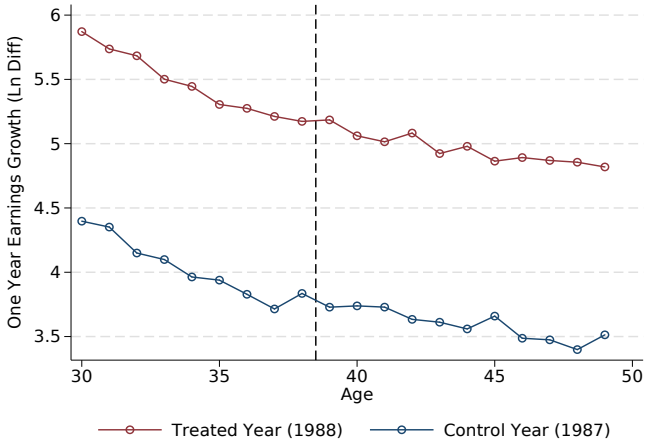


Figure A.13: Estimated effect of the PBD increase on earnings growth: **One** Year Earnings Growth (Ln Diff)

Earnings Growth - Treatment and Control Years



Earnings Growth Difference

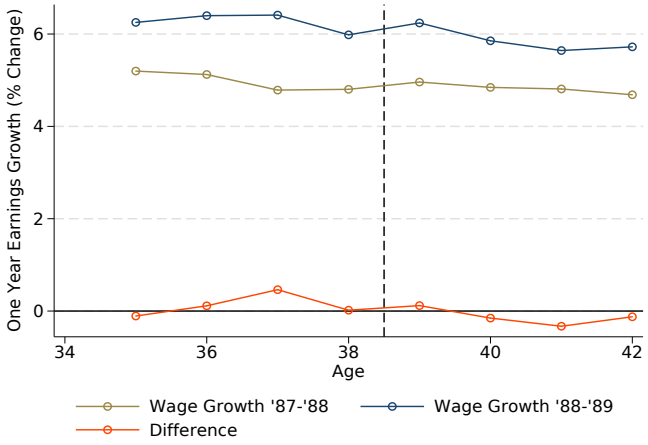
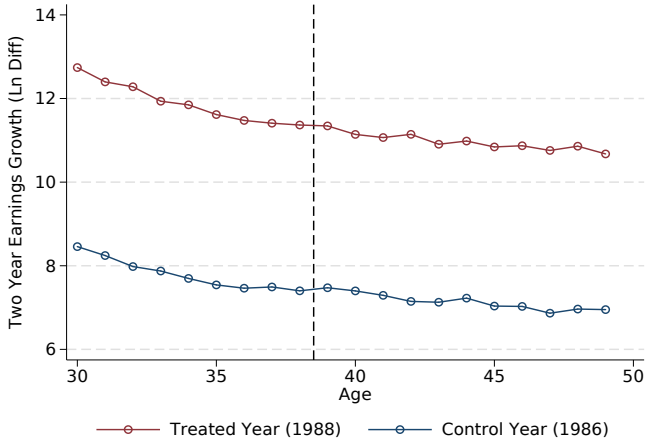
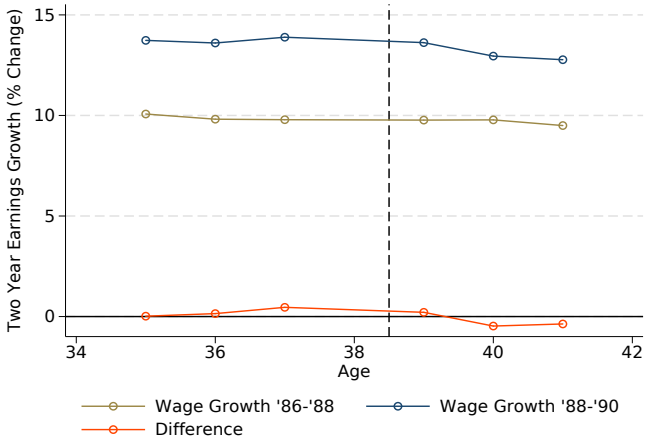


Figure A.14: Estimated effect of the PBD increase on earnings growth: **Two** Year Earnings Growth (Ln Diff)

Earnings Growth - Treatment and Control Years



Earnings Growth Difference



F Replacement Rate Schedules in Austria: 1972–2003

Figure A.15: Replacement Rate Schedules 1972-1978

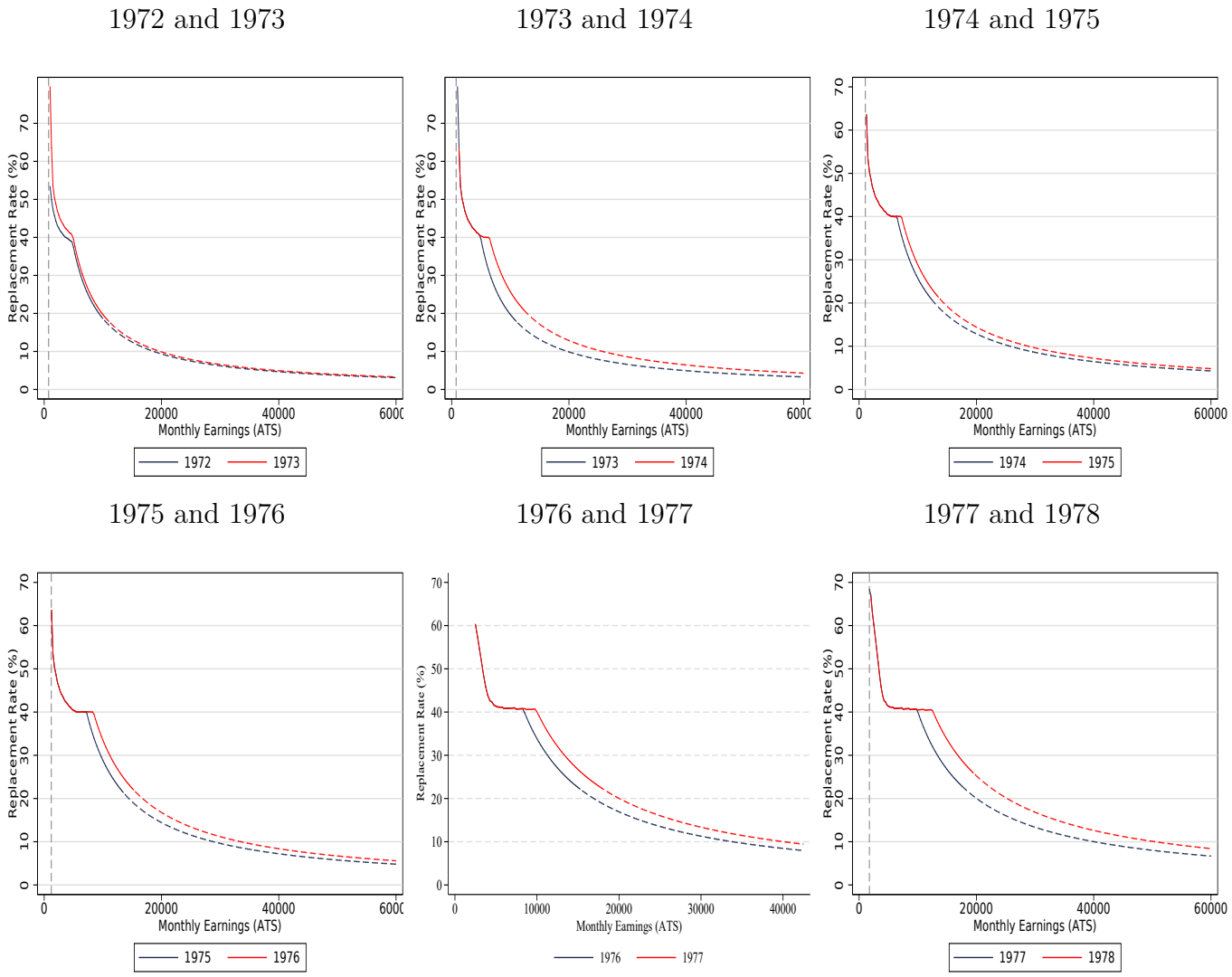
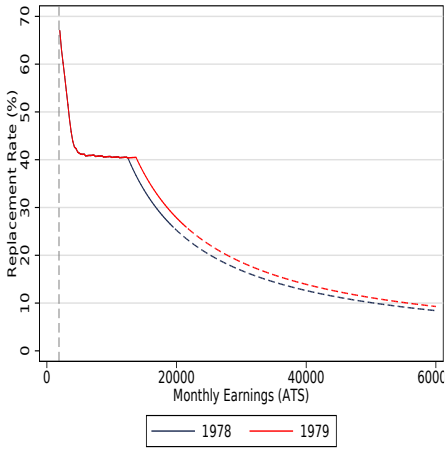
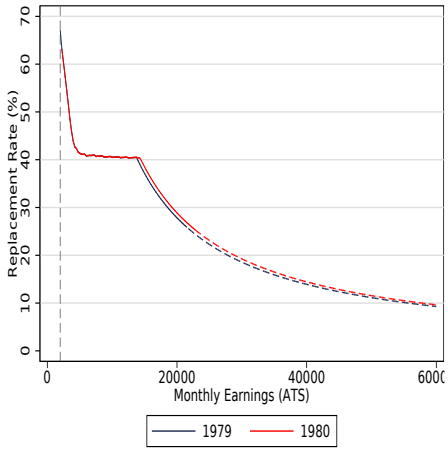


Figure A.16: Replacement Rate Schedules 1978-1987

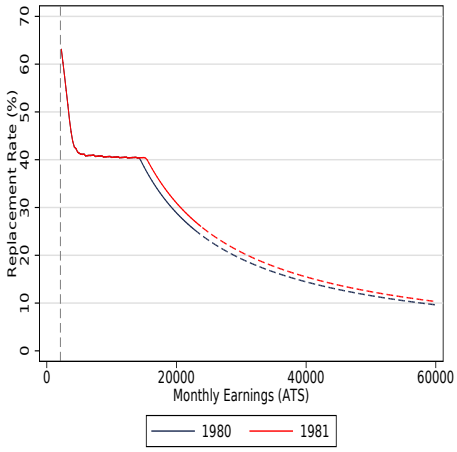
1978 and 1979



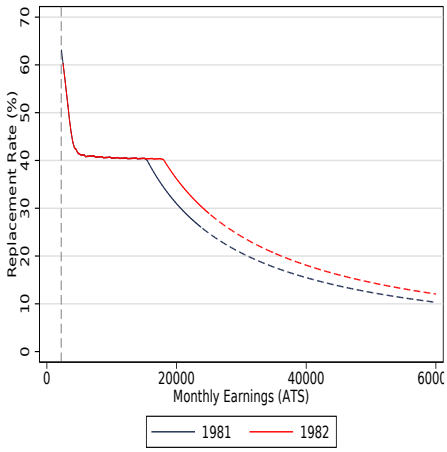
1979 and 1980



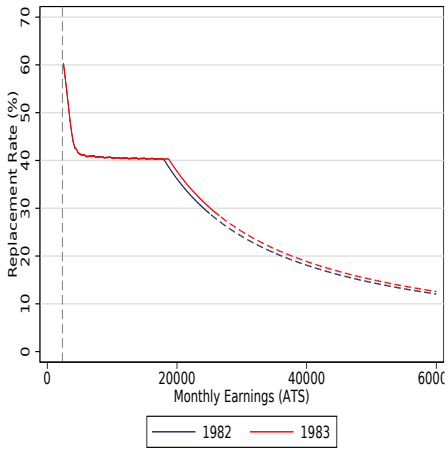
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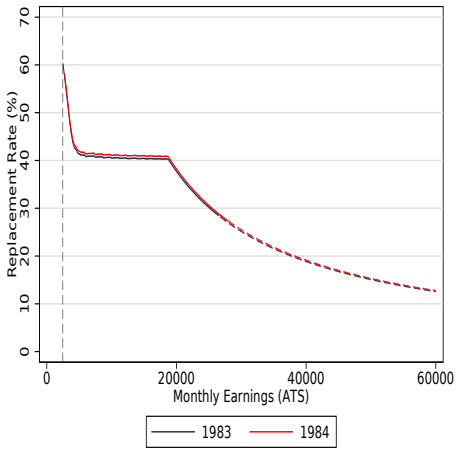
1981 and 1982



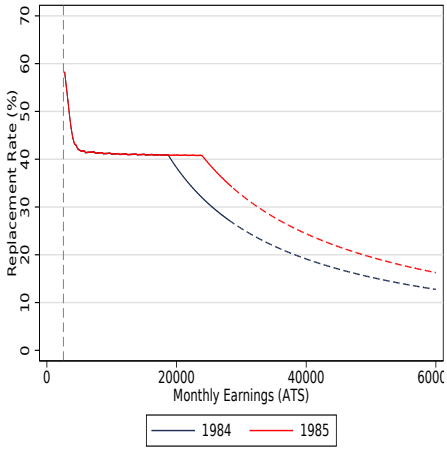
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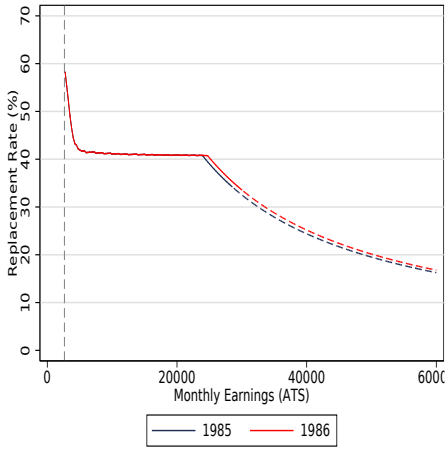
1983 and 1984



1984 and 1985



1985 and 1986



1986 and 1987

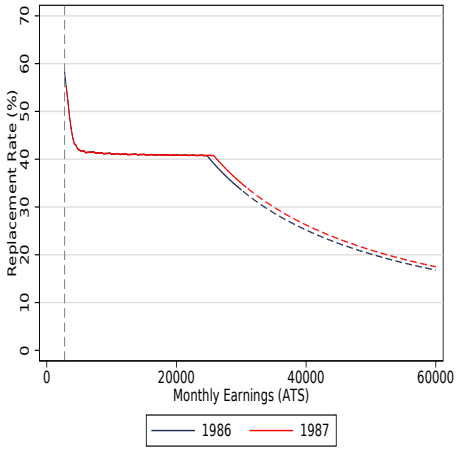
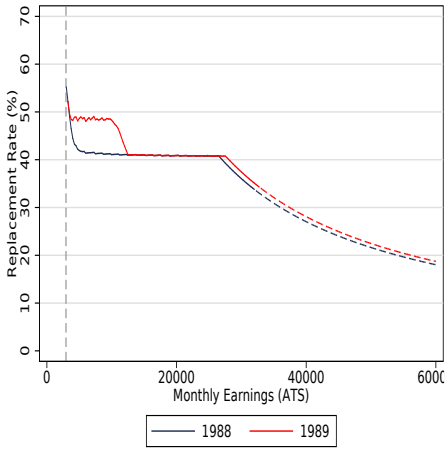
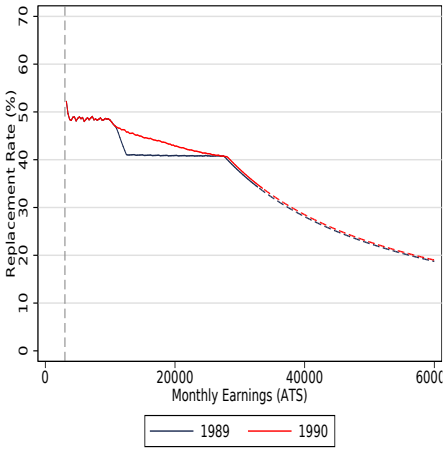


Figure A.17: Replacement Rate Schedules 1988-1997

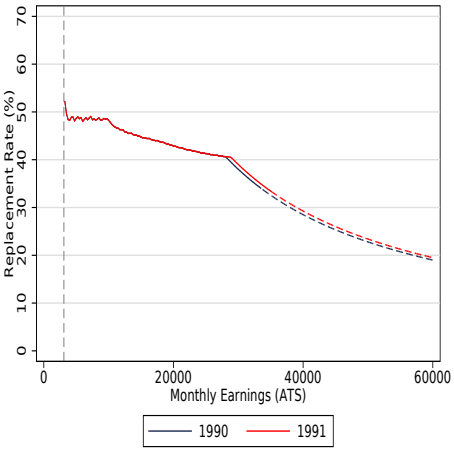
1988 and 1989



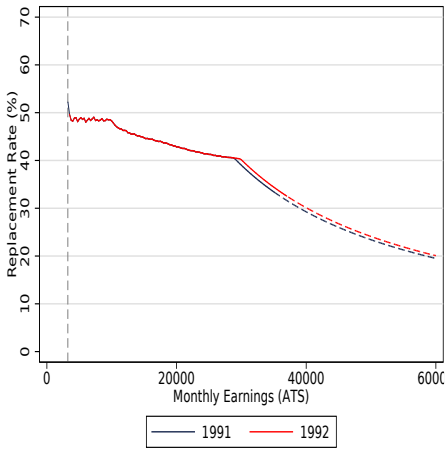
1989 and 1980



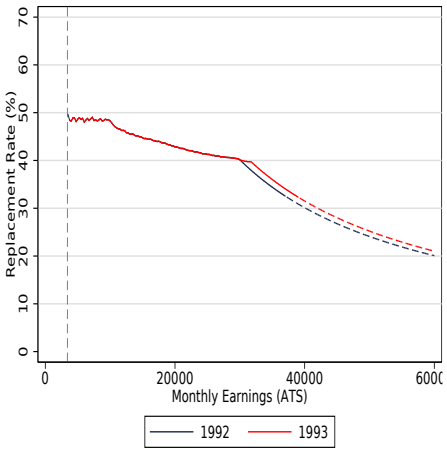
1990 and 1991



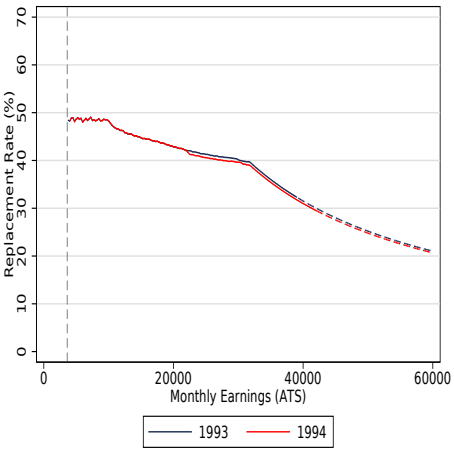
1991 and 1992



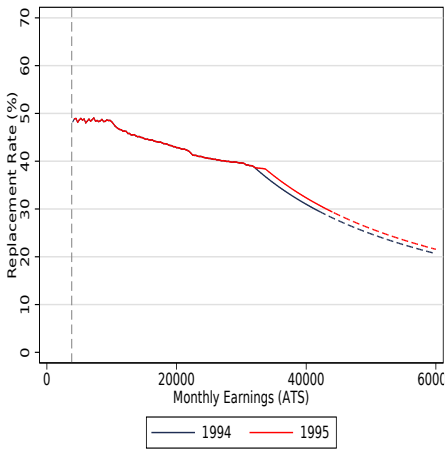
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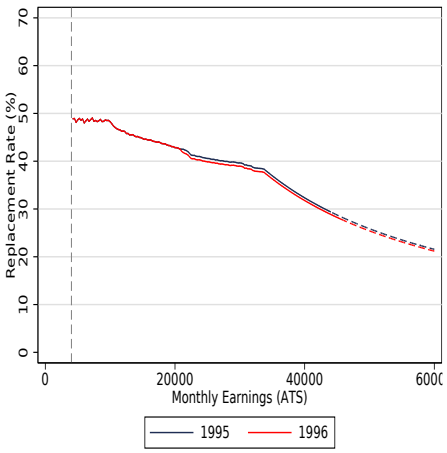
1993 and 1994



1994 and 1995



1995 and 1996



1996 and 1997

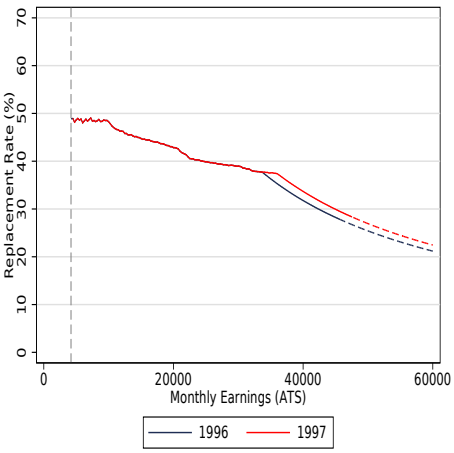
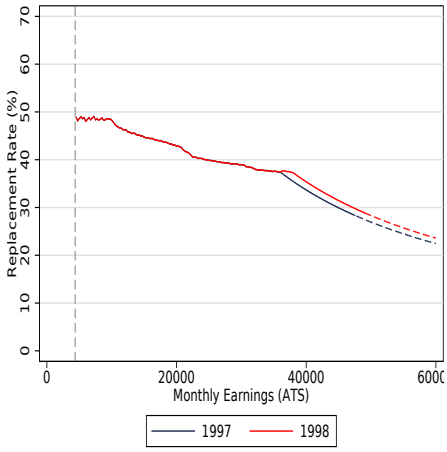
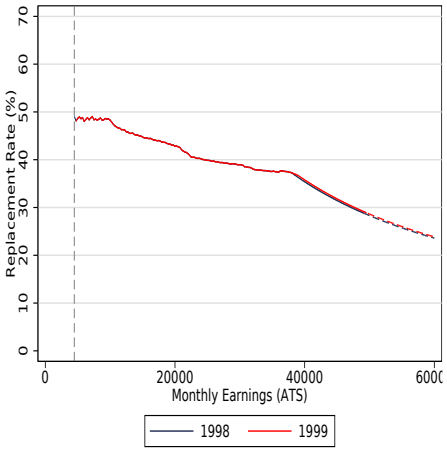


Figure A.18: Replacement Rate Schedules 1997-2003

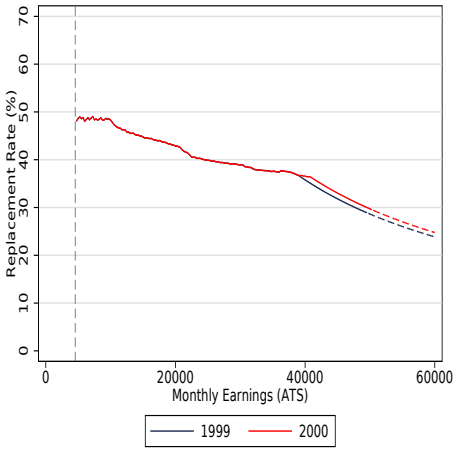
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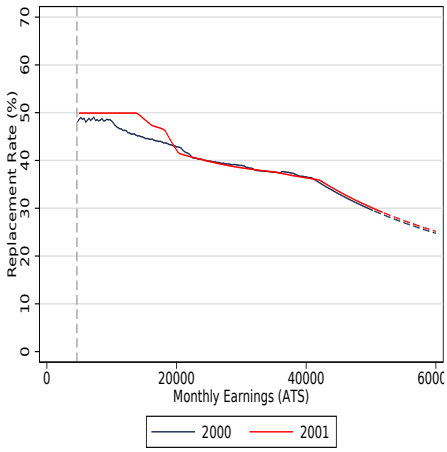
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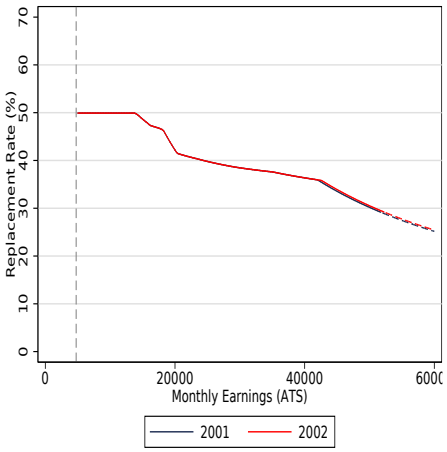
1999 and 2000



2000 and 2001



2001 and 2002



2002 and 2003

