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Working Paper No. 2308
First version: July 2023
This version: April 2024

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*Outside options and worker motivation**

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April 3, 2024

Abstract

We identify the effect of outside options on workers' motivation to exert effort. We evaluate changes in outside options arising from age and experience cutoffs in the Austrian unemployment insurance (UI) system, and use absenteeism to proxy for worker effort. Results indicate that an increase in potential UI benefit duration leads to more absenteeism, especially for workers facing a higher unemployment risk, older workers, female workers, parents, in declining or low-wage firms, and for workers on a flatter wage trajectory relative to their coworkers. These results are consistent with a relational contracting model where effort reductions caused by higher outside options are more pronounced if the perceived relationship value is small.

JEL Classification: D21, D22, J22, J65, M52
Keywords: Outside options, effort incentives, relational contracts

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I. Introduction

Effectively incentivizing workers is a fundamental determinant of firm performance (Lemieux et al. 2009, Prendergast 1999). If contracting frictions prevent the adoption of formal incentive contracts, agreements based on informal, "subjective," performance measures are widely used instead (Frederiksen et al. 2017, Kampkötter & Sliwka 2016). Their efficiency relies on the (future) value of an employment relationship, which is not only determined by its inherent productivity and stability, but also by workers' and firms' outside options. While a large theoretical literature has explored the link between outside options and incentives using models of efficiency wages (Shapiro & Stiglitz 1984, Yellen 1984) and relational contracts (MacLeod & Malcomson 1989, Malcomson 2013), systematic empirical evidence remains scarce.

In this paper, we show that better outside options indeed reduce worker effort. We first set up a theoretical model of an infinitely repeated firm-worker relationship, in which effort benefits the firm and is observable but not verifiable. Therefore, formal, court-enforceable contracts to motivate the worker are not feasible, and self-enforcing relational contracts are used instead. Relational contracts rely on the future relationship value to incentivize workers to perform today, and a higher value generally leads to more worker effort. While the relationship value increases in the stability and inherent productivity of an employment relationship, it decreases in workers' and firms' outside options. Thus, we predict that better outside options reduce effort.

We test this prediction by exploiting age and experience cutoffs in the Austrian unemployment insurance (UI) system, which provide variation in workers' outside options by increasing the potential payoff of unemployment. In particular, workers above the age of 40 are eligible for 39 instead of 30 weeks of UI benefits if they have worked at least 6 of the last 10 years. To construct counterfactuals, we use same-age workers that are not eligible for the benefit extension because they do not fulfill the 6-year experience criterion, comparing eligible and ineligible workers before and after the benefit extension kicks in. As an empirical proxy for non-verifiable effort we use worker *absenteeism*, as in Bennedsen et al. (2019), Ichino & Maggi (2000), or Ichino & Riphahn

(2005). The main advantage of this approach is that sick leaves can be measured consistently across occupations and industries, and we can show that they are correlated with the local unemployment rate, employment prospects, and wages, similar to other effort and productivity measures used in the literature.

In line with our theoretical predictions, we find that the 9-week benefit extension increases absenteeism by 0.4 days of sick leave per half-year, on average. This translates to a 0.18-percent effort reduction for a one-percent increase in potential UI benefits. Consistent with the increase in absenteeism being an effort response, we find that (1) the 9-week benefit extension does not affect healthcare utilization, (2) effects are much stronger when we only consider sick leaves due to 'easy-to-fake' diagnoses, such as common cold or low back pain, (3) effects are stronger for sick leaves on days with good weather, and (4) we find zero effects for placebo tests using sick leaves due to cancer.

Our empirical design requires that ineligible workers be a valid counterfactual for eligible workers. We provide several results in support of this assumption. Most importantly, eligible and ineligible workers follow parallel trends in absenteeism before the change in outside options becomes important. To address dynamic selection, we show that, despite significant baseline differences, the composition of eligible and ineligible workers does not change around the age-40 cutoff. Also, because ineligible workers may become eligible when accumulating more time on the labor market, we show that workers are not systematically more likely to become eligible prior to the age-40 cutoff and that results are similar when we fix eligibility at a certain age or when we omit workers that switch eligibility status after age 25. Relatedly, we find little evidence that the UI benefit extension affects job separations.

Additionally, we provide evidence from an alternative identification strategy that exploits changes in early retirement age (ERA) laws to validate our baseline findings.[1] In particular, the Austrian government enacted two reforms that gradually increased the ERA from 60 to 65

[1] The reason we use this second source of identification solely as a validation check is because the potential complier population—that is, people old enough to be affected by the reforms and on the margin of retiring early—is much smaller than in our main model.

for men and 55 to 60 for women based on quarter-of-birth cohorts. We argue that a higher ERA increases the future value of the relationship because the expected end of a worker's career at the margin of retiring is postponed. Therefore, we predict that a higher ERA leads to higher effort and thus to a reduction in absenteeism. Indeed, results from a fixed effects model which is identified by changes in the ERA between sick leaves of a worker indicate that a one-year ERA increase decreases average sick leave durations by around 0.5 days, on average. Interestingly, this validation exercise shows that also an unanticipated shock to outside options lead to effort responses that are similar to our baseline design, where we consider an anticipated change in outside options.

To gain a better understanding of the mechanisms underlying our main result, we derive additional predictions from our model. Some of these predictions make use of the theoretical result that the negative effect of a higher outside option on worker motivation is more pronounced if the relationship value is smaller to begin with. The reason is that equilibrium effort is determined by a constraint in which today's effort cost cannot exceed the future relationship value. If this constraint binds, a further reduction in the relationship value causes a stronger effort response than if this constraint is slack. To test these predictions, we identify variables in our data that arguably shape the stability and inherent productivity of an employment relationship and thus its value.

First, we find that our effect is stronger for workers facing a higher risk of becoming unemployed. This is true for both blue-collar workers, who are unconditionally more likely to become unemployed than white-collar workers, and for workers with a high *predicted* risk of longer-time unemployment. For those two groups of workers, the relationship value is systematically smaller and unemployment benefits are more important for their outside option. Second, the effect becomes larger over time as workers age. We argue that this is because the relationship value becomes smaller as workers approach retirement. Third, the negative effect of a higher outside option on effort is stronger for shrinking rather than growing firms, which we use as a proxy for expected job stability. Fourth, the effect is smaller for high-wage firms and high-wage workers, which we identify using estimated Abowd, Kramarz & Margolis (1999, AKM hereafter) firm and individual worker wage fixed effects. Following the literature, we suggest that firms and workers with higher AKM fixed effects are inherently more productive, which increases the value of their employment relationships. Fifth, the effect is larger for workers who experience lower wage growth relative to their coworkers, which we argue reflects a smaller value of the worker to the firm. Sixth, female workers and those with children react stronger to higher potential UI benefits. One explanation is that the UI replacement rate is substantially larger for workers with dependents, hence they benefit more from the UI extension. Also, we argue that being responsible for children corresponds to higher opportunity costs of exerting effort and thus a smaller relationship value.

Our paper ties together several strands of the literature. Most importantly, we complement recent work by Jäger, Schoefer, Young & Zweimüller (2020), who show that changes in potential UI benefits have no effects on worker wages. This result seems at odds with the widely-used Nash bargaining model, which predicts positive wage changes when the worker's outside option increases. Our findings suggest that changes in outside options can have real consequences on employment relationships and, in particular, affect their efficiency.[2]

While other papers have considered effort responses to changes in outside options, these studies are mostly descriptive and focus on single firms. Cappelli & Chauvin (1991) show that higher wage premia and higher local unemployment are both associated with fewer disciplinary problems in a large US manufacturing firm. Similarly, Lazear, Shaw & Stanton (2016) use data from a US services firm and observe that worker productivity was significantly higher during the 2009 recession, and that this increase was particularly strong in areas with high unemployment.[3] We add to this literature by providing causal evidence for an entire workforce and across industries.

We are aware of only one design-based paper that studies a similar question. Lusher, Schnorr & Taylor (2022) use scanner data and US state-level variation in UI benefit levels to estimate effects on supermarket cashier productivity. They find that transaction length increases by 2.4 seconds

[2]We note that Jäger et al. (2020) also show results on the share of months spent on sick leave in an appendix, but they only observe sick leaves longer than 6 to 12 weeks, depending on job tenure, where social security steps in and picks up half of the worker's wage bill. This is rarely the case though—in our data, 99 percent of sick leaves are 45 days or shorter. Since we use more granular data on individual sick leaves, we can identify even small effects at the intensive margin of leave taking.

[3]Further support for a positive correlation between the unemployment rate and worker effort is provided by Scoppa & Vuri (2014) for Italy and Burda et al. (2020) for the US.

or 2 percent for cashiers who experienced an 18-week increase in the potential benefit duration. Our contribution relative to Lusher et al. (2022) is threefold: First, because we consider a full population of workers across industries, we can see whether this result holds in a broader setting. Second, we provide a theoretical foundation for empirical results observed by Lusher et al. (2022). In particular, they find that effects are more pronounced for less productive workers, which is in line with our theoretical result that effort responses to changes in outside options are stronger for a smaller relationship value. Third, we provide evidence on mechanisms shaping the link between outside options and effort that have not previously been considered in the literature.

We also contribute to the literature providing empirical evidence for the presence and characteristics of relational contracts (Fahn et al. 2017, Gil et al. 2022, Gil & Marion 2013, Gil & Zanarone 2018, Macchiavello 2022, Macchiavello & Morjaria 2015, 2021). While these contributions mostly rely on between-firm relationships in single industries or markets, we have access to the universe of employment relationships in Upper Austria and present evidence indicating that the respective theoretical mechanisms are also relevant in *within-firm* relationships—that is, between firms and their employees. Some contributions have identified relational contracts in individual firms (Adhvaryu et al. 2021, Akerlof et al. 2020). They do so in the context of developing countries, where weak legal systems often leave no choice than relying on informal arrangements. We argue that relational contracts shape employment relationships also in countries with strong legal institutions, as important aspects of job performance remain difficult to verify. Moreover, while these papers are based on case studies of single firms, we use administrative data from many firms and industries. We also exploit variation in outside options induced by a labor market policy, which makes our analysis more relevant from a public policy point of view. Furthermore, we propose an additional test to distinguish a relational contracting mechanism from potential alternative explanations. Therefore, our predictions are not only based on changes in the relationship value or reneging temptations (the standard approach in the literature), but also on comparisons of effect sizes based on the ex-ante relationship value.

Finally, we speak to the literature on UI and, in particular, on the effects of UI benefit duration.

Most papers study direct impacts on unemployed workers, in particular how they adapt their search behavior and reservation wages (e.g., Baker & Fradkin 2017, Card, Chetty & Weber 2007, Lalive 2007, Le Barbanchon, Rathelot & Roulet 2019, Marinescu & Skandalis 2021, Nekoei & Weber 2017, Schmieder, von Wachter & Bender 2012, van Ours & Vodopivec 2008). We argue that such policies can also have indirect effects on workers that are currently employed by shaping internal processes within firms and therefore firm productivity. Our findings suggest that such indirect effects should not be overlooked when evaluating UI policies.

The rest of the paper is structured as follows. In section II, we set up the relational contracting model and derive first predictions. In section III, we turn to the empirical analysis and first discuss the institutional setting, the design we use, and the data, before we turn to presenting our main result and several robustness checks that lend support to our findings. In section IV, we then derive additional theoretical predictions that we each test using our empirical model. In section V we argue that the observed link between UI benefits and worker absenteeism cannot be generated by alternative models, such as a competitive labor market or search-and-matching models. Section VI concludes.

II. The relationship between outside options and worker effort

In this section, we derive a relational contracting model to formalize the relationship between outside options and worker effort. Relational contracts rely on similar mechanisms as the classic models of efficiency wages, but impose fewer behavioral restrictions (Fahn, MacLeod & Muehlheusser 2023). We discuss similarities and differences between efficiency wage models and our model below. We organize this section as follows: In subsection II.1, we lay out the model environment. In subsection II.2, we formalize payoffs and the first best. In subsection II.3, we introduce relational contracts to the model. In subsection II.4, we describe the optimization problem. In subsection II.5, we derive comparative statics. Finally, in subsection II.6, we discuss the role of outside options and potential UI benefits in the model.

II.1. Environment

In every period t of an infinite time horizon, a risk-neutral principal/firm ("she") makes an employment offer to a risk-neutral agent ("he"). The offer contains an upfront wage $w_t \geq 0$ and a discretionary bonus $b_t \geq 0$. We describe the agent's acceptance decisions with $d_t \in \{0, 1\}$, where $d_t = 1$ corresponds to an acceptance and $d_t = 0$ to a rejection. Upon acceptance, the agent receives w_t and chooses an effort level $e_t \in \mathbb{R}_+$ which is associated with effort costs $c(e_t)$, where $c'(\cdot)$, $c''(\cdot) > 0$, $c'''(\cdot) \geq 0$ and $c(0) = c'(0) = 0$. Effort generates an (expected) output $e_t\theta$ (with $\theta > 0$) which is subsequently consumed by the principal.

Future payoffs are discounted with a common factor $\delta \in (0, 1)$; δ not only captures time preferences, but also reflects the probability with which the relationship is continued. Continuation probabilities can be driven by industry- or firm-wide, as well as personal characteristics. For our baseline model, we assume for simplicity that δ is constant over time. Later, we take into account that δ may decrease for older workers as retirement approaches.

II.2. Payoffs and first best

If the agent rejects the principal's offer in a given period, both consume their outside options which are $\bar{\pi} \in \mathbb{R}^+$ for the principal and $\bar{u}_t \in \mathbb{R}^+$ for the agent. $\bar{u}_t$ may include alternative job opportunities as well as UI benefits. We allow $\bar{u}_t$ to vary over time to capture anticipated changes in UI benefits, as used in our empirical analysis. The principal's outside option is kept constant for simplicity. Thus, players' discounted payoff streams in a period t are

$$U_t \equiv \sum_{\tau=t}^{\infty} \delta^{\tau-t} \left[d_\tau \left(w_\tau + b_\tau - c(e_\tau) \right) + (1 - d_\tau)\, \bar{u}_\tau \right]$$

$$\Pi_t \equiv \sum_{\tau=t}^{\infty} \delta^{\tau-t} \left[d_\tau \left(e_\tau \theta - w_\tau - b_\tau \right) + (1 - d_\tau)\, \bar{\pi} \right].$$

Moreover, $\bar{U}_t \equiv \sum_{\tau=t}^{\infty} \delta^{\tau-t} \bar{u}_\tau$ and $\bar{\Pi} \equiv \bar{\pi}/(1-\delta)$. We also define

$$S_t \equiv \Pi_t + U_t$$

as the period-t surplus generated within the relationship, and $\bar{S}_t \equiv \bar{\Pi} + \bar{U}_t$. Thus, the per-period surplus if $d_t = 1$ equals $e_t\theta - c(e_t)$, and first-best effort e^{FB} is characterized by

$$\theta - c'(e^{FB}) = 0. \tag{1}$$

For the following, we assume that

$$e^{FB}\theta - c(e^{FB}) > \bar{\pi} + \bar{u}_t \tag{2}$$

holds for all t. Therefore, it is efficient for the agent to work for the principal if e^{FB} is implemented.

II.3. Contractibility, payoffs, and relational contract

Effort and output are observable to both the principal and the agent, but not verifiable to a third party, such as courts. This assumption takes into account that a worker's performance in many of today's jobs involves dimensions that are difficult to display to outsiders, such as quality, dependability, or flexibility (Gibbons & Henderson 2012). Therefore, formal incentive contracts are not possible, and only a self-enforcing *relational contract* can (potentially) be formed. In our setting without asymmetric information, it determines a subgame perfect equilibrium of the game.

We derive a relational contract that maximizes the total surplus at the onset of the game, S_1. However, note that predictions would be the same if our objective was to maximize the principal's profits, the agent's utility, or any weighted average of those (Levin 2003).

Discussion of Assumptions. Before deriving equilibrium outcomes and empirical predictions, we briefly discuss our modelling assumptions. First, effort in our setting relates to the agent's motivation on the job, and we abstract from other, easily measurable, aspects such as working hours. Such measurable dimensions could be taken care of by not-further-modeled incentive contracts. Then, our results survive as long as, without this extra motivation, employing the agent would not be valuable. Second, observability of the agent's effort is not important for our results. In Web Appendix section F.2, we demonstrate that if effort is the agent's private information and generates an observable output measure, all our predictions survive. Third, although we use the term "wage" when referring to the agent's compensation, it might go beyond monetary payments. In particular, if salaries are constrained by collective bargaining agreements or contractual obligations, firms can be restricted in setting them. Therefore, the wage in our setup reflects everything that is costly to the principal and valued by the agent. For example, it might include good working conditions, flexibility in working times, or perks. Finally, in Appendix section F.1, we show that our predictions hold even if the agent's compensation is taken as given and only firing threats are used to provide incentives. This links our approach to classic efficiency-wage models, which are the basis of relational contracting models but rely on firing threats for non-performance instead of performance-based bonus payments. We demonstrate that the underlying mechanisms are closely related. It will turn out that a relational contracting model better matches our data in one dimension: The use of firing threats in classic efficiency-wage models would result in more on-path separations as effort goes down, a link that our relational contracting model does not generate. We show later that this is not the case in our data, where separations are hardly affected by a change in workers' outside options.

II.4. Optimization Problem

Our objective is to maximize the period-1 surplus S_1. Outcomes are restricted by a number of constraints which must be satisfied in all periods t. In the following, we display these constraints for an equilibrium in which, on the equilibrium path, the agent accepts the employment offer in every period. Later on, we make precise which conditions must hold for this to be optimal.

Since all deviations from equilibrium play are publicly observable, it is optimal to punish any deviation by a reversion to the worst possible outcome for the deviator (Abreu 1988), which in our case means that players consume their outside options forever thereafter.[4]

First, it must be in the agent's interest to accept the firm's offer, which is captured by his participation constraint (PC),

$$U_t \geq \bar{U}_t. \tag{PC}$$

Second, given the agent has accepted the contract, equilibrium effort e_t must satisfy his incentive compatibility constraint (IC),

$$-c(e_t) + b_t + \delta U_{t+1} \geq \delta \bar{U}_{t+1}, \tag{IC}$$

which takes into account that, if the agent decides to deviate from equilibrium effort, he will choose zero effort instead. Note that, if effort were verifiable, a formal incentive contract would only need to satisfy these sets of constraints. Such a contract could induce the agent to choose e^{FB} in every period, for example by setting $w_t = \bar{u}_t$ and $b_t = c(e^{FB})$.

Third, since formal incentive contracts are not feasible, also the principal needs incentives to pay the b_t specified by the relational contract. This is captured by so-called dynamic enforcement (DE) constraints,

$$-b_t + \delta \Pi_{t+1} \geq \delta \bar{\Pi}, \tag{DE}$$

which implies that paying b_t must be profitable for the principal. This requires the subsequent continuation profits be sufficiently high compared to the principal's payoff after a termination. Finally, given $b_t \geq 0$, a participation constraint for the principal ($\Pi_t \geq \bar{\Pi}$) is implied by (DE) and can hence be omitted.

To conclude, our objective is to maximize S_1 subject to (PC), (IC), and (DE). These constraints must hold in every period t.

[4]However, note that equilibrium outcomes would be the same if a deviation did not lead to a termination, but instead to a continuation of the relationship in which the deviator would only receive their outside option (Levin 2003).

II.5. Results

II.5.1. Preliminaries

First, we simplify the optimization problem and obtain the following results.

Lemma 1. *The equilibrium is sequentially efficient, i.e., maximizing S_1 is equivalent to maximizing $e_t\theta - c(e_t)$ in every period t. Moreover, given $\Pi_t \geq \bar{\Pi}$ and $U_t \geq \bar{U}_t$, the following enforceability constraint* (EC) *is necessary and sufficient for implementing equilibrium effort e_t^*:*

$$-c(e_t^*) + \delta S_{t+1} \geq \delta \bar{S}_{t+1}. \tag{EC}$$

These results follow from Levin (2003). Sequential efficiency implies that destroying surplus on the equilibrium path cannot improve incentives, hence on the equilibrium path the agent is never fired.[5]

The (EC) constraint is obtained by adding the (IC) and (DE) constraints. It states that the cost of exerting effort today must be covered by the net future value of continuing the relationship which captures the fundamental mechanism of relational contracts. Sufficiency follows because of the substitutability between current and future incentives (if this condition holds, a payment scheme exists that satisfies the individual constraints stated above).

Lemma 1 implies that, if e^{FB} satisfies (EC) in period t, $e_t^* = e^{FB}$. Otherwise, $e_t^* < e^{FB}$ and is determined by the binding (EC) constraint. Finally, for the employment relationship never to be terminated on the equilibrium path, $e_t^*\theta - c(e_t^*) \geq \bar{\pi} + \bar{u}_t$ must hold for all t.

[5]Note that this outcome relies on the principal's ability to design an individual compensation scheme for the agent. Naturally, such flexibility may seem too strong an assumption given our objective to analyze a firm's relationship with one of many employees. There, wages are (at least partially) determined by collective bargaining agreements or a firm's central policy and not tailored to an individual employment relationship. Then, using firing threats to motivate the agent may indeed be optimal. However, we show in Appendix section F.1 that our main results continue to hold in such a setting.

II.5.2. Comparative Statics

Next, we demonstrate how the level of the inherent marginal productivity of effort, θ, and the discount factor determine equilibrium outcomes.

Lemma 2. *S_t strictly increases in θ and δ. It weakly decreases in $\bar{S}_{t+1}$.*

The proof of Lemma 2 can be found in Appendix B. Higher θ and δ have a direct positive effect on the within-relationship surplus, which is further amplified by a relaxed (EC) constraint. An increase in outside options tightens (EC) and thus potentially reduces the surplus. Next, we present a general result that is the foundation of the predictions we derive later on.

Proposition 1. *Assume (EC) binds in a period t. Then, equilibrium effort e_t^* decreases in $\bar{S}_{t+1}$. This effect is more pronounced if S_{t+1} is smaller or if $\bar{S}_{t+1}$ is larger to begin with. If (EC) in a period t is slack, equilibrium effort e_t^* is unaffected by a marginal change in $\bar{S}_{t+1}$.*

The proof of Proposition 1 can be found in Appendix B. The intuition for this Proposition is as follows. If the future relationship value is sufficiently high, (EC) is slack and first-best effort is implemented. Then, a marginal change in the value has no effect on equilibrium effort. If the relationship value is small and (EC) binds, a reduction decreases equilibrium effort. Because the effort cost function is convex, this reaction is stronger for a small relationship value and the corresponding low effort.

II.6. Outside options

Now, we put more structure on the development of the agent's outside option to better relate to the empirical environment we study. There, we analyze the consequences of an anticipated increase of employees' unemployment benefits at the age of 40, which we model as a permanent increase in the agent's outside option. Hence, we assume that there is a $T > 1$ such that

$$\bar{u}_t = \begin{cases} \bar{u} & \text{for } t < T \\ \bar{u}^H & \text{for } t \geq T, \end{cases} \tag{3}$$

with $\bar{u}^H > \bar{u}$.

Note that we use this specification for expositional simplicity. Assuming that the higher outside option only materializes if the agent actually works in period T or later does not affect our results qualitatively. In such a setting, the future outside option would determine the future surplus within the relationship and consequently equilibrium effort in earlier periods.

Proposition 2. *For all periods $t \geq T-1$, equilibrium effort $\tilde{e}$ is constant; moreover, there is a $\tilde{\delta} < 1$ such that $\tilde{e} = e^{FB}$ for $\delta \geq \tilde{\delta}$ and determined by $-c(\tilde{e}) + \delta\left[\tilde{e}\theta - \left(\bar{\pi} + \bar{u}^H\right)\right] = 0$ otherwise.*

If $\delta \geq \tilde{\delta}$, $e = e^{FB}$ also in all previous periods $t < T-1$. If $\delta < \tilde{\delta}$, $e_t > \tilde{e}$ for all $t < T-1$. Then, there exist $\tilde{\delta}_t < \tilde{\delta}$ such that $e_t = e^{FB}$ for $\delta \geq \tilde{\delta}_t$ and determined by the binding (EC) *otherwise, with $\tilde{\delta}_t$ increasing. Finally, $e_t \geq e_{t+1}$, with a strict inequality if $\delta < \tilde{\delta}_{t+1}$.*

The proof of Proposition 2 can be found in Appendix B. Proposition 2 states that equilibrium effort is smaller in later than in earlier periods. This effort reduction is caused by the change in the agent's outside option which permanently increases in period t. Importantly, the effort reduction already unfolds in period $T-1$ or earlier because today's effort is constrained by the *future* relationship value. Moreover, moving to earlier periods diminishes the weight of the higher outside option which steadily increases the (future) value of the relationship. Therefore, equilibrium effort goes up as we move backwards, either until the very first period or until e^{FB} can be implemented.

II.6.1. First Prediction

As an agent's outside option also contains his payoff when being unemployed, Proposition 2 directly yields the first prediction.

Prediction 1. *An increase in UI benefits at a given age permanently reduces an affected worker's effort. This effort reduction already materializes earlier, before the increase in UI benefits is realized.*

This prediction relates to all dimensions that make UI benefits more generous in the future. Both a higher replacement rate and a longer duration of UI benefit payout increase $\bar{U}_{t+1}$.

III. Empirical analysis

Now we establish our main empirical result. This section is organized as follows. We first discuss the institutional setting in subsection III.1, covering details about the social security system, the labor market, and sick leaves in Austria. In subsection III.2, we discuss our empirical design. In subsection III.3, we describe our data. In subsection III.4, we show the main result for the effect of outside options on worker incentives. In subsections III.5 and III.6, we provide several robustness checks and discuss whether our proxy for worker motivation is viable. Finally, in subsection III.7 we show results from an alternative identification strategy as a validation exercise.

III.1. Institutional setting

III.1.1. Social security and the labor market

Austria has a *Bismarckian* social security system with universal access to public healthcare, pension, disability, and unemployment benefits. Workers are automatically enrolled to the system and insurance is extended to spouses and children, unemployed people, pensioners, and disabled people. In this paper we focus on Upper Austria, which is one of the nine Austrian federal states with around 1.5 million residents or 20 percent of the Austrian population. The labor market is characterized by broad institutional regulation with collectively bargained wages and working conditions.[6] At the same time, the labor market is highly flexible, with particularly weak job protection (OECD 2020) and high turnover (Böheim 2017).[7] Employment contracts can generally be terminated without specifying a reason, but unilateral terminations require a notice period.

[6]Note that, although our model seems to allow for more wage-setting flexibility than the Austrian labor market, we show in subsection F.1 that our results can also be generated in a model where compensation is given and only firing threats are used to incentivize workers.

[7]In terms of the OECD employment protection legislation indicator, Austria places 33rd of 37 countries, with the United States ranking last. Job turnover rates are 7.9 percent for men and 8.3 percent for women, which are larger than the European Union averages of 6.7 percent for men and 7.4 percent for women.

III.1.2. Unemployment insurance

Austria's UI program is compulsory and funded through a 6 percent payroll tax that is shared equally by workers and firms. It applies to all workers who earn more than the marginal employment threshold, which was €438.05 per month in 2018. The minimum replacement rate amounts to 55 percent of daily net income, which is calculated based on pre-unemployment wages. Workers with dependents can be eligible for replacement rates of up to 80 percent. A prerequisite to receiving UI benefits is that claimants are willing and able to work. This implies that they must prove they frequently apply for new jobs and undergo retraining, if necessary. Importantly, workers are entitled to UI benefits regardless of the reason of the job separation, and even workers who are fired for poor performance or misconduct are eligible. Benefits for laid-off workers are payable immediately upon entry into unemployment, for job quitters there is a one-month waiting period. After UI benefits are exhausted, unemployed people are eligible for means-tested income support.

The potential duration of UI benefits changes discontinuously at age and experience cutoffs. Baseline eligibility is 20 weeks for workers that have been employed for at least one year. After a total of three years of employment, the potential benefit duration is 30 weeks. At age 40, benefits are extended to 39 weeks, provided that the worker has been employed for at least 6 of the last ten years prior to claiming UI. At age 50, benefits are extended up to a year conditional on having worked for at least 9 of the last 15 years. In this paper, we focus on the age-40 cutoff for three main reasons. First, almost all workers in the labor market are eligible for at least 30 weeks of benefits, so the experience cutoff extending eligibility from 20 to 30 weeks is not informative.[8] Second, we know from previous literature (Ahammer & Packham 2023, Nekoei & Weber 2017) that the age-40 cutoff increases actual UI takeup conditional on becoming unemployed, while the age-50 cutoff has little impact. Third, our design is ill-suited to analyze the age-50 cutoff if the age-40 cutoff already pushes eligible and ineligible workers onto different absenteeism trends. In Appendix C, we show that there are no other labor-market related age cutoffs that could interfere with our design.

[8]In 2018, 92 percent of all 30 year old non-marginally employed workers were eligible for at least 30 weeks of benefits. At age 40, this share increases to 97 percent.

III.1.3. Sick leave

Sick leave insurance in Austria compensates workers' earnings losses due to both occupational and nonoccupational disease. Workers are entitled to full wage compensation for 6 to 12 weeks, depending on job tenure. After this period, workers receive 80 percent of their wage for another 4 weeks, but the wage bill is shared equally between firms and social security. After these 4 weeks, workers are entitled to public sickness benefits that replace 60 percent of the current wage (Halla et al. 2015).[9] Workers may also take leaves with full wage replacement to care for dependents, and such leaves are *not* recorded as sick leaves in our data.

To take sick leave, workers have to produce a sick note to the employer. These sick notes are usually issued by primary care physicians who also directly report the sick leave to the health insurance provider. Sick notes do not reveal a medical diagnosis to the employer, and employers must not ask workers to disclose a diagnosis. Workers are not obliged to produce sick notes for leaves of less than 4 days, unless the firm explicitly requires it. Firms are generally free to enforce such a rule, as long as it applies to all its workers, and there are no further contracts or agreements necessary. In our data, it is, in fact, quite common: 97.5 percent of firms in Upper Austria have at least one short sick leave per year. Even if fewer firms employed such a policy, this would likely not be a problem for our design, because (1) eligible and ineligible workers are equally likely to work in firms that require short sick leaves,[10] (2) we focus on intensive margin responses, conditioning on observing a sick leave in the first place, and (3) we show that results hold if we control for firm fixed effects which hold firm sick leave policies constant.

III.2. Design

To test for the effect of a change in outside options on worker absenteeism, we estimate differences in sick leave takeup before and after a 9-week UI benefit extension kicks in at age 40 for workers with enough experience (the "eligible" group). To avoid comparing older with younger workers, we

[9]Only few workers have such long sick leaves. In our data, 99 percent of leaves are 45 days or shorter.

[10]The probability that eligible workers in our data work in a firm that requires short sick leaves is 0.9985, the probability for ineligible workers is 0.9992.

use workers without enough experience (the "ineligible" group) to partial out age effects. Before we discuss the specifics of our empirical strategy, we note two important design choices.

First, we focus mostly on intensive margin responses in absenteeism. This is because being absent is arguably less costly for workers at the intensive than at the extensive margin (it is easier to take an additional day off when already on sick leave than taking a sick leave in the first place). Hence, our design will allow us to pick up more nuanced effort responses.[11] Also, health effects are less relevant at the intensive than at the extensive margin,[12] and the fact that we do not observe some very short sick leaves is less problematic if we condition on having a sick leave to begin with. Therefore, we construct our data set on the sick leave level, and our primary outcome measures the duration of a sick leave j of individual i. A potential issue related to focusing on intensive margin responses is differential selection into types of sick leaves that changes over time, but we show in section III.5 that this is most likely not a problem in our data.

Second, because Prediction 1 states that we expect workers to already react to the change in outside options at age 40 *before* they actually turn 40, and because there is ample evidence for forward-looking behavior in anticipation of labor market policies (Artmann et al. 2023, French et al. 2022, Hairault et al. 2010), we need to consider changes relative to some arbitrary reference period b. For now, we fix b at 37.5 years of age, which is informed by a breakpoint detection method we describe in Appendix D. In section III.5, we additionally show that the reference period choice has little influence on our estimates.

The estimand we are interested in is

$$\beta = \left(\overline{\text{duration}}^{\text{eligible}}_{t>b} - \overline{\text{duration}}^{\text{eligible}}_{t\leq b}\right) - \left(\overline{\text{duration}}^{\text{ineligible}}_{t>b} - \overline{\text{duration}}^{\text{ineligible}}_{t\leq b}\right), \qquad (4)$$

where we subtract the change in average sick leave duration before and after the UI benefit extension becomes important at age b between workers that are eligible for the benefit extension (the left

[11] In fact, we do find that extensive margin effects are not economically relevant (see section III.4).

[12] We elaborate on this in more detail in section III.6 below. The idea is that, even if a worker takes sick leave because they are actually sick, the decision when to return to work will be influenced by their motivation to perform on the job.

term) and those not eligible (the right term). We can write this in regression form as

$$\text{duration}_{jit} = \beta \left(\text{eligible}_{jit} \times \mathbb{1}[t > b]\right) + X'\gamma + u_{jit}, \tag{5}$$

where duration_{jit} is the duration of sick leave j of individual i at age t, eligible_{jit} is one if worker i is eligible for the benefit extension when taking sick leave j at age t and zero otherwise, $\mathbb{1}[t > b]$ indicates the post-treatment period, and X is a vector of covariates that includes flexible tenure and year fixed effects, a female dummy, as well as the eligibility and post-treatment dummy individually. The main coefficient of interest is β, which measures the average effect of the UI benefit extension on eligible workers. In event study form, equation (5) reads

$$\text{duration}_{jit} = \sum_{k \neq b} \beta_k \left(\text{eligible}_{jit} \times \mathbb{1}[t = k]\right) + X'\gamma + u_{jit}, \tag{6}$$

where $\mathbb{1}[t = k]$ indicates age k.

A key assumption is that the difference in sick leave taking between eligible and ineligible workers would remain constant absent the UI benefit extension at age 40. In support of this assumption, we show that eligible and ineligible workers follow parallel trends in sick leave takeup before the change in outside options becomes important at age b. We also note that, in our baseline specification, workers are allowed to switch from being ineligible to being eligible. This is because we track worker outcomes over a long period of time, and we do not want to lose information of workers that switch eligibility status in our baseline model. However, we show that workers are actually not more likely to switch eligibility before the age-40 cutoff, which mitigates concerns about dynamic selection into treatment. Also, we provide robustness checks which show that our results are not sensitive to (1) fixing eligibility status at age 37.5 and (2) dropping workers that switch eligibility status at some point. Relatedly, we show that the UI benefit extension does not affect job separations.

III.3. Data

We combine two sources of administrative data, which allow us to track workers over time and observe their sick leave taking. First, we use data from the *Austrian Social Security Database* (ASSD, Zweimüller et al. 2009). The ASSD is structured as a matched employer-employee panel that covers the universe of Austrian workers from 1972 to today. We use the ASSD to obtain individual-level employment histories, wages, and basic demographic information. One limitation of the ASSD is that it does not contain working hours, wages are top-coded at a social security contribution cap, and that there is no information on occupations apart from blue-collar or white-collar status.

Second, we have access to health records from the *Upper Austrian Health Insurance Fund* (UAHIF). The UAHIF is the main health insurance provider in Upper Austria, covering all private-sector employees apart from those working in railway and mining.[13] The UAHIF database contains information on healthcare service utilization in both the inpatient and outpatient sector, including drug prescriptions, hospital days, and physician visits. Most importantly, however, the UAHIF tracks sick leaves for all private-sector employees, which we can match to our employer-employee data set. Sick leaves are recorded as the actual number of days an employee stays away from work, not the expected duration on sick notes (these may not necessarily coincide, for example if the worker returns to work early).[14] We also observe a primary diagnosis for each sick leave, which is coded according to the *International Classification of Diseases, 2010 revision* (ICD-10).

To construct our sample, we first draw all sick leaves between 1998 and 2018 that can be matched to employment spells in the ASSD data. If a worker has multiple jobs, we match the one where they earn the highest wage in a given year. This gives us a total of 9,577,046 sick leave spells for 889,889 workers. We then drop 258,660 sick leaves that are taken by marginally employed workers, because they are not covered by UI. For our analysis, we only consider workers aged 25 through 45, which leaves us with an unbalanced panel of 4,664,982 sick leaves for 558,290 workers,

[13] We have no health information on public sector employees, farmers, and self-employed persons.

[14] Patients must notify physicians if they are returning to work early, and the physician is responsible for informing the health insurance that the sick leave has ended. The physician must also be consulted if a sick leave is to be extended.

where 412,052 workers are at some point eligible and 251,408 workers are at some point ineligible.

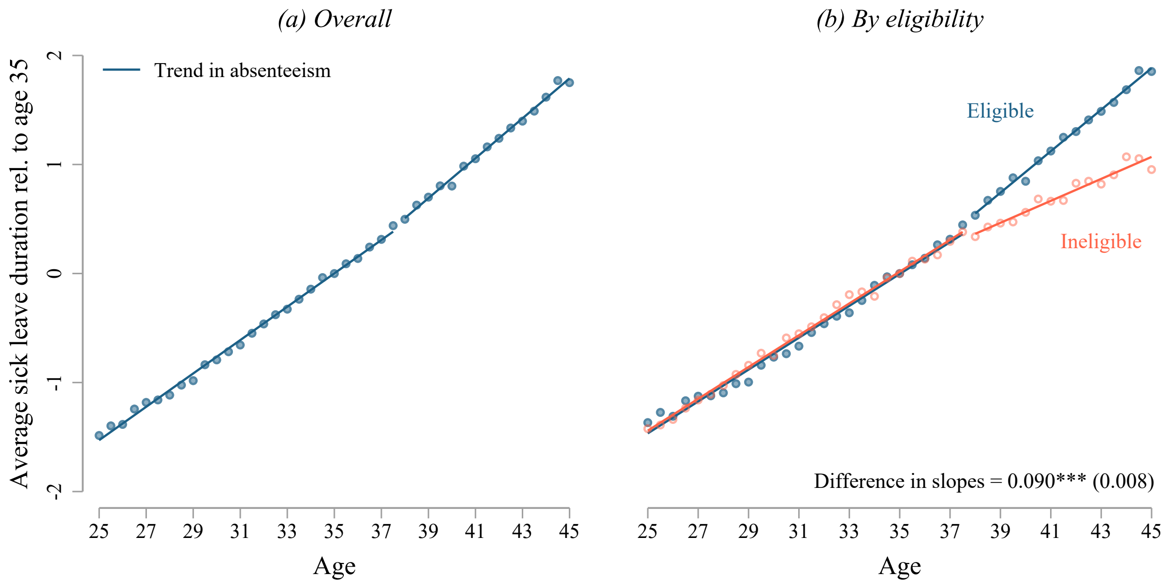
Descriptive statistics are presented in Appendix Table A.1. Using only data for workers aged 37.5 or younger, the average duration of a sick leave is 7.22 days and average experience is 10 years. Perhaps unsurprisingly, there are significant baseline differences between eligible and ineligible workers. Eligible workers are much less likely to be female, slightly less educated, and much less likely to be parttime workers. Also, they have longer job tenure and higher wages, on average. Importantly, however, the composition of eligible and ineligible workers is remarkably stable over time and does not change around the age-40 cutoff. We report shares of the variables in Appendix Figure A.1, where we discretize tenure and wage using their respective sample medians. All variables move (almost) in parallel, which suggests that systematic compositional changes at age 40 are unlikely to explain the findings below.

III.4. The effect of outside options on worker motivation

We start by descriptively examining absenteeism patterns over worker age. In Figure 1, panel (a), we plot average sick leave durations by half-year of age for the entire workforce. We see that sick leaves generally become longer the older workers get, with leaves being, on average, 2 days longer at age 45 than at age 35. In panel (b), we divide workers by whether they experience an increase in outside options because they are eligible for the UI benefit extension at age 40. Importantly, eligible and ineligible workers are on almost exactly the same trend prior to the change in outside options. Consistent with our theoretical prediction that an anticipated change in outside options affects behavior already in earlier periods, we see a gap opening around age 38, with eligible workers increasing sick leave taking at a higher rate than ineligible workers. The difference in slopes amounts to 0.09 days of sick leave per half-year of age, which is significantly different from zero at the 1 percent level. We consider this as strong evidence that outside options affect absenteeism and that this pattern is already apparent in the raw data.

In Figure 2, we plot the differences between the trends for eligible and ineligible workers (the red and blue lines) from Figure 1 in an event study, similar to equation (6). Here we control for

Figure 1 — Absenteeism over age by UI benefit extension eligibility



Notes: These figures show trends in average sick leave durations, conditional on taking at least one sick leave, per half-year of age, averaged over all workers in our sample (panel a) and by whether workers are eligible for the 9-week UI benefit extension (panel b). In both graphs, we center average sick leave durations around the average sick leave duration for workers at age 35 in the sample. In panel (b), we do this separately for eligible and ineligible workers.

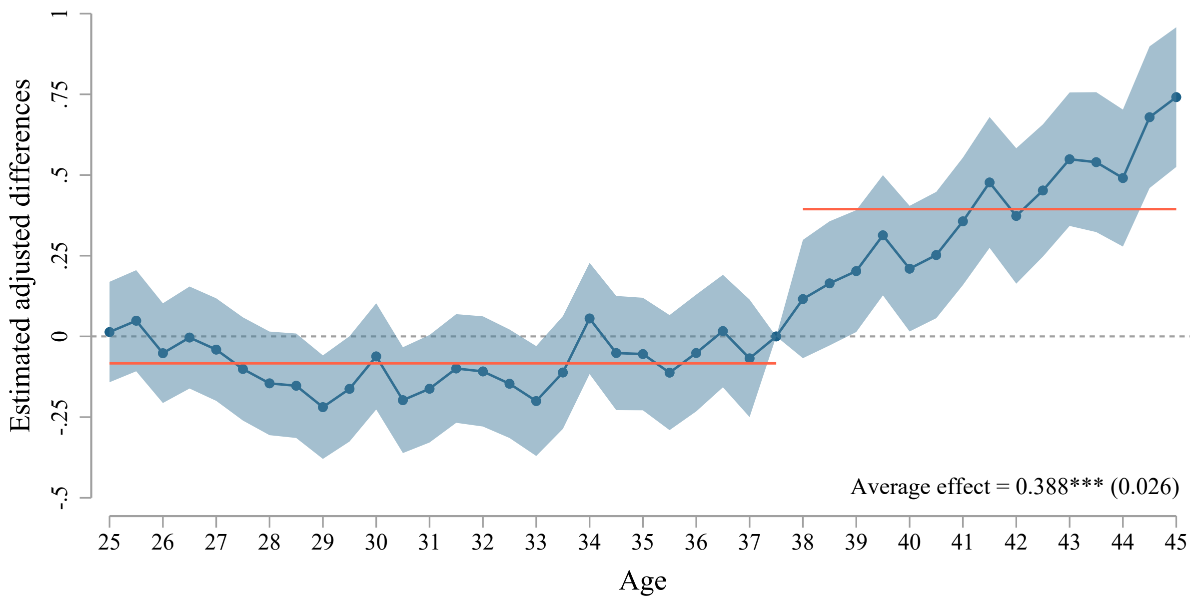
tenure and year fixed effects as well as gender. Our estimates suggest that the gap in sick leave taking between eligible and ineligible workers is constant prior to age 37.5 and becomes progressively larger around age 38.[15] This forward-looking behavior is consistent with our theoretical model, where workers and firms care about *future* changes in outside options when making decisions today, and with evidence from the literature indicating that workers indeed respond to labor market policies that will affect them several years ahead (Artmann et al. 2023, French et al. 2022, Hairault et al. 2010).[16] Also, the fact that the gap in absenteeism keeps growing over time is in line with a decreasing relationship value as workers approach retirement.[17]

[15]We also test if our results are robust to allowing for nonlinear differential pretrends using the inference approach described in Rambachan & Roth (2023). At age 45, the breakdown value of the degree of nonlinearity is $M = 0.0025$, which is almost six times the size of our estimated linear pretrend. In other words, we can allow for a sixfold deviation from linear pretrends at every year of age following treatment and still reject the null hypothesis.

[16]These papers find that pension reforms change the labor supply and search behavior of workers far from retirement. See also section III.7, where we present evidence for forward-looking behavior using ERA reforms in Austria.

[17]Note that although our baseline model does not predict the treatment effect to increase over time, this observation is perfectly consistent with an extension that takes into account that the real-world future relationship value declines

FIGURE 2 — The effect of an increase in outside options on absenteeism



Notes: This figure plots event study estimates from equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration. Because we expect eligible workers to react already prior to age 40, we fix the reference period to $b = 37.5$, and point estimates can be interpreted as changes in average sick leave duration due to the benefit extension at a given age relative to age 37.5. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is from equation (5). All regressions control for tenure and year fixed effects as well as gender.

Comparing absenteeism before and after age 37.5 suggests that the 9-week UI benefit extension increases absenteeism, on average, by around 0.4 days per half-year of age.[18] Compared to the average sick leave duration in our sample, this is equivalent to a 5.4 percent increase, which implies an elasticity of 0.18 for a one-percent increase in the potential benefit duration.[19] In section III.5

with age. We discuss this in more detail in section IV.

[18] We also estimate our event study on wages instead of sick leaves in Appendix Figure A.2. Consistent with Jäger et al. (2020), we find no evidence that a change in the value of unemployment that comes with the UI benefit extension has an effect on log wages. Reassuringly, we also find no evidence that eligible and ineligible workers are on different wage trajectories prior to treatment. While Jäger et al. (2020) state that their results indicate that outside options have no (or only minor) effects on wages, our theoretical approach could generate such a seemingly absent link even if wages were affected by outside options. Note that our current setting does not yield wage predictions since we maximize the total surplus and allow for any distribution of it. However, if we assumed that each party gets their outside option plus a fixed share of this surplus (as standard bargaining models do), a higher outside option would not only yield a positive direct but also a negative indirect effect on wages. The latter is caused by a lower relationship value, since less effort can be implemented, thus the total sign of the wage change would be ambiguous.

[19] To calculate this elasticity, we divide the percent change in sick leave duration by the percent change in the benefit duration, $e = 0.054/[(39/30) - 1]$. The 95 percent confidence interval for e is $(0.16, 0.20)$.

we discuss how this estimate changes if we vary the reference period b.

While absenteeism clearly reacts at the intensive margin to changes in outside options, effects at the extensive margin are not economically relevant. In Appendix Figure A.3, we build a yearly panel of workers and test whether the UI benefit extension affects the probability of taking sick leave at all in a given year. Despite the average effect being weakly positive—which is in line with our theoretical predictions—dynamic effect estimates across the pre- and post-treatment period are hovering between -0.01 and 0.01. These are minuscule effects compared to the sample mean of 60 percent.

III.5. Robustness

In this section we discuss how robust the effect of outside options on absenteeism is to different specification and design choices. In Appendix Table A.2, we use different covariate sets and fixed effects when estimating model (5). In column (1), we omit covariates altogether. In column (2), we add controls for gender as well as tenure and year fixed effects. In column (3), we additionally control for occupation, parttime status, education, and wage. Because we do not observe parttime status and education for all workers, we replace missing values with zero and add a missing indicator dummy to the regression. Doing so increases our average effect estimate, but since these control variables are potentially endogenous, we omit them from our other regressions. In column (4), we estimate the model using worker fixed effects, which increases our average effect estimate substantially as well.[20] Column (2) is the most conservative and therefore our preferred specification.

Our analysis focuses on intensive margin sick leave responses, and we discuss several reasons for this choice in section III.2. This could potentially be problematic if eligible and ineligible workers selected into different types of sick leave and the underlying selection mechanism changes discontinuously around age 40. In Appendix Table A.3, we therefore list the five most common sick leave diagnoses at different ages separately for eligible and ineligible workers. By far the most

[20]If we estimate equation (5) with firm fixed effects, which account for unobserved differences in firm policies, we obtain an estimate of $\hat{\beta} = 0.382$ (0.037). This is statistically indistinguishable from our baseline estimate.

common diagnosis is upper respiratory infection, followed by gastroenteritis and musculoskeletal problems. Importantly, this is true for all age groups and diagnosis shares are similar between eligible and ineligible workers. In Appendix Table A.2, column (5), we additionally provide results for a specification with diagnosis fixed effects, which only compares workers with the same diagnosis over time. This does not affect our results.

An important aspect of our design is choosing an appropriate reference period, since Prediction 1 tells us we should expect workers to react already before the UI extension actually kicks in. In Appendix Figure A.4, we therefore estimate equation (5) for different reference periods b from age 30 through age 39.5, which is the last half-year before the increase in outside options actually kicks in. This does not change our main conclusion, and estimates are remarkably stable across reference periods. In fact, point estimates range from $\hat{\beta} = 0.27$ with $b = 30$ to $\hat{\beta} = 0.39$ with $b = 38.5$, so the difference between the smallest and the largest estimate is only 0.12 days of sick leave per half-year of age.

Another important design choice is that we allow workers to switch from being ineligible to being eligible and vice versa. This is because we consider sick leaves over a relatively long time span, and we want to allow workers to contribute to estimated effects both at times when they are ineligible and when they are eligible. A potential concern is that workers systematically switch eligibility before the UI extension kicks in. However, we show that the probability of switching in our sample is smooth along the age distribution and actually decreases over time (Appendix Figure A.5). In Appendix Table A.4 we additionally provide evidence employing two alternative constraints on eligibility status. First, in column (2), we fix eligibility at age 37.5. This means that we consider workers that did not accumulate enough experience by age 37.5 as untreated, even if they cross the experience threshold later on.[21] Second, in column (3), we drop workers that switch between being ineligible and being eligible at some point. This does not affect our main conclusions. In fact, point estimates become considerably larger if we use more restrictive treatment definitions.

[21] This is similar to how other papers in the literature define treatment status in settings where treatment status can change over time (e.g., Harju, Jäger & Schoefer 2021).

A related concern is that the UI benefit extension affects job separations and that this effect differs between eligible and ineligible workers. While some previous work does find a link between UI benefit generosity and job separations (e.g., Albanese et al. 2020, Baguelin & Remillon 2014, Brébion et al. 2022, Jäger et al. 2023, Khoury 2023, Rebollo-Sanz 2012), papers that consider the same Austrian reform as we are analyzing do not detect differential selection into UI at the age-40 cutoff (Ahammer & Packham 2023, Nekoei & Weber 2017). This is also true in our data. We find that the probability of job separation at a given age for workers in our sample does not change discontinuously at age 40, and trends for eligible and ineligible workers are parallel along the age range (Appendix Figure A.6). One explanation for this apparent discrepancy may be that most of the aforementioned papers consider *unanticipated* shocks after which the net value of some relationships becomes negative. In contrast, the *anticipated* change in the relationship value caused by lower outside options that we analyze would already have negative spillover effects on earlier periods. Some affected relationships will then already dissolve early on or not even be formed in the first place, which is why we would expect only a limited effect on separations at (or around) the cutoff.

III.6. Do changes in absenteeism actually measure changes in motivation?

Our results rely on absenteeism indeed being a good proxy for worker motivation. We are not the first to use this approach. For example, Bennedsen, Tsoutsoura & Wolfenzon (2019) estimate AKM models to separate worker and firm components of motivation, which they measure using days of sick leave, Ichino & Riphahn (2005) consider absenteeism to test for the effects of employment protection on worker effort, and Ichino & Maggi (2000) use sick leave taking to study regional shirking differentials in Italy. Generally, using absenteeism has several advantages: it can be consistently measured across occupations and industries and is readily available in administrative data sources. Also, we know from previous literature that sick leaves causally affect employment prospects (Ahammer 2018, Markussen 2012) and earnings (Andersen 2010). We also find descriptive evidence for such a relationship: workers with more sick leaves are more likely to become unemployed and earn lower wages, even after adjusting for age (Figure A.7). Lastly, sick leaves are negatively correlated with the local unemployment rate (Figure A.8), which is consistent with Lazear et al. (2016). A natural question is, however, if the change in outside options affects worker health and, if so, whether we can separate the health effect from the motivation effect.

Generally, we believe that health effects are not a problem for our design. Most importantly, there is no evidence that the UI extension affects healthcare takeup. In Appendix Figure A.9, we run our event study from equation (6) on total healthcare expenses, which is the sum of physician fees, drug expenses, and inpatient expenses. Our estimates suggest that there is no difference in expenses between eligible and ineligible workers, neither before nor after UI benefits are extended. We note that, even in the presence of health effects, our model would be informative. Assume a worker takes sick leave because they are actually sick. On the margin, motivation will still affect the worker's decision to extend the sick leave or return to work.[22]

Additionally, in Appendix Figure A.10 we provide heterogeneity estimates by diagnosis of the sick leave and by weather conditions. We find that the change in outside options has a stronger effect on sick leaves with arguably easy-to-fake diagnoses (common cold and low back pain) and those starting on days with good weather.[23] If sick leaves were taken purely for health reasons, we should not see such effect heterogeneity. This is consistent with research showing that only little of the variation in sick leaves can be explained by differences in health status. Ahammer & Schober (2020) find that only 28 percent of the variance in sick leaves can be explained by patient

[22]Note that, even if the increase in sick days partially reflects a reduction in presenteeism along with an increase in absenteeism, our interpretation is not affected. Both an increase in absenteeism and a decrease in presenteeism are to some degree negatively related to effort and motivation. But if a shock in the outside option would reduce *contagious* presenteeism (Pichler & Ziebarth 2017)—that is, presenteeism when workers have influenza-like diseases—firms may actually benefit from a positive shock to outside options. Descriptively we find little evidence for a reduction in contagious presenteeism: The ratio of per-worker flu infections in a firm relative to flu infections in other firms in the same calendar quarter (we call this the excess flu infection rate) is not related to the share of eligible workers aged 40 or older in the firm (Appendix Figure A.12).

[23]We define common cold and low back pain as potential easy-to-fake diagnoses because their symptoms are not immediately visible to a doctor and hence difficult to verify. To define easy-to-fake diagnoses we follow previous research that connects such sick leaves to shirking (e.g., Ahammer 2018). In our sample approximately 6 percent of sick leaves are due to an easy-to-fake diagnosis. Good weather days are defined as outside conditions that are at least 0.5 standard deviations better than the monthly regional average. From April to September we use the average daily temperature and hours of sunshine while from October to March we use the amount of fresh snow (hinting at the fact that skiing could be possible). In our sample approximately 15 percent of sick leave spells start with a good weather day.

observables or variation in physician prescribing behavior, the rest remains unexplained. In our data, a simple regression of aggregate days of sick leave in a year on cubics in physician fees, drug expenses, and inpatient expenses returns an R^2 of around 0.1. This suggests that only 10 percent of the variation in absenteeism days can be explained by observable healthcare variables.

We also use sick leaves due to cancer as a placebo check. Cancer cannot possibly be affected by outside options, especially in the short-and medium-run, unless there are secular trend differentials between eligible and ineligible workers we fail to take into account. In Appendix Figure A.11, we therefore run our event study on the probability of having a sick leave due to cancer in a given half-year of age. This gives a robust zero effect, suggesting no effect of the change in outside options on cancer-related sick leaves.

III.7. Alternative identification strategy

So far we have relied on changes in the potential UI benefit length to identify the value of outside options. As a validation exercise, we now consider an entirely different source of variation, namely exogeneous changes in the Austrian early retirement age (ERA). To this end, we exploit two reforms in Austria that gradually increased the ERA from 60 to 65 for men and from 55 to 60 for women based on birth cohorts. In Appendix E, we provide an in-depth discussion of these two reforms and the relevant institutional setting.

Before we turn to the empirical analysis of the ERA reforms, it is useful to discuss what effects we should expect based on our model. A higher ERA reduces the worker's future outside option, because the payoffs of not working go down for cohorts affected by the reform.[24] It follows that, if it becomes more difficult for workers to retire early, the expected relationship value with their current employer increases. A change in the ERA therefore has the opposite effect of a change in UI benefits, and we predict that effort increases (and consequently absenteeism decreases) with a

[24]Note that our model assumes an infinite time horizon, thus it does not capture a fixed retirement date. However, retirement could be incorporated by having the discount factor decrease over time, which resembles a situation in which retirement becomes increasingly likely once a certain age is reached. Alternatively, we could assume that there is a predetermined last period and extend the model to still allow for positive effort. For example, as in Fahn (2023), the agent's preferences might also contain history-dependent social preferences which disappear after the principal reneged on a bonus payment.

Table 1 — Effects of an increase in the ERA on absenteeism

	OLS	Fixed effects		Interaction
	(1)	(2)	(3)	(4)
Statutory ERA	–0.015**	–0.492***	–0.205***	–0.135***
	(0.007)	(0.033)	(0.039)	(0.043)
Statutory ERA × $\mathbb{1}\{\text{age} \geq 55\}$				–0.044***
				(0.012)
Worker fixed effects	No	Yes	Yes	Yes
Age fixed effects	No	No	Yes	Yes
Quarter fixed effects	Yes	Yes	Yes	Yes

Notes: This table reports OLS and fixed effects estimates for the effect of a one-year increase in the statutory ERA on sick leave duration. The sample is based on all sick leaves in Upper Austria taken by workers born between 1940 and 1957. The average sick leave duration in this sample is 12.2 days, the number of observations in all cells is 1,714,371. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

higher ERA.

Prediction 2. *An increase in the ERA leads to higher equilibrium effort.*

To test this prediction, we restrict our sample to workers who experienced a change in the ERA, namely those born between 1940 and 1957, and estimate a simple fixed effects model,

$$\text{duration}_{jiy} = \alpha \cdot \text{ERA}_{iy} + \theta_i + X'\delta + \varepsilon_{jiy}, \qquad (7)$$

where the duration of sick leave j of individual i in year y is regressed on the statutory ERA_{iy} in the year the sick leave is taken (measured as years of age) and worker fixed effects θ_i. Additionally, we control for flexible age and quarter-year fixed effects, which are summarized in X. Importantly, because we include worker fixed effects in model (7), the parameter α is identified only through changes in the ERA between different sick leaves of one worker. The ERA is statutory and thus not a choice variable, which mitigates endogeneity concerns.

We report these estimates in Table 1 for different sets of control variables. In column (1), we estimate model (7) with OLS, only controlling for quarter-year fixed effects that account for seasonal trends in sick leave taking. This gives a small negative coefficient, suggesting that the ERA is indeed negatively related to average sick leave durations. In column (2), we add worker fixed effects, which allows us to exploit changes in ERA between sick leaves of a single worker. The coefficient is now much larger in magnitude. Our preferred specification is in column (3), where we control for both worker and age fixed effects. The estimate suggests that, consistent with our theoretical predictions, a one-year increase in the statutory ERA decreases the average duration of sick leaves by around half a day, which is significant at the 1 percent level.[25] If we estimate column (3) as a log-log model, we obtain an approximate elasticity of −0.7, meaning that a one-percent increase in the ERA decreases sick leave durations by 0.7 percent, and we cannot reject elasticities as low as −0.4. This is slightly larger than the 0.28 elasticity we find for our main setting. In column (4), we additionally check whether effects are stronger for older workers closer to retirement, for whom the remaining future value is smaller and the effect on effort should therefore be more pronounced (Proposition 1). This is indeed the case.

We think that this exercise is useful for four reasons. First, even when using a different source of identifying variation compared to our main empirical design, our evidence suggests that changes in outside options do matter for worker incentives. Second, while our main design relies on *anticipated* changes in outside options, this exercise confirms that workers also respond to *unanticipated* changes in outside options. Third, these results confirm that workers are forward looking to labor market policies that affect them only years in advance. Fourth, we provide new evidence that changes in ERA laws can have spillovers on workers that still participate in the labor market, similar to Bianchi et al. (2023).

IV. Mechanisms

Having established the general negative link between outside options and worker motivation, we now derive additional results that provide conditions under which the effect size is particularly pronounced. This lends support to the underlying mechanism, which is that a worker's effort is determined by the future relationship value.

[25] Similar to our UI benefit extension analysis, these effects are much stronger for sick leaves with easy-to-fake diagnoses ($\hat{\alpha} = -0.645$). We do not find differences in effect sizes between sick leaves on good and bad weather days.

IV.1. Threat of unemployment

Becoming unemployed does not affect all workers' outside options to the same extent. Some will immediately find an alternative job, for others this is more difficult and unemployment potentially more costly.We propose two strategies to measure the threat of unemployment. First, we distinguish between blue-collar and white-collar workers. Appendix Table A.5 compares the risk of becoming unemployed and, conditional on being unemployed, the average unemployment duration between blue-collar workers and white-collar workers. Blue-collar workers face almost twice the risk of becoming unemployed as white-collar workers and, if they become unemployed, the average duration until they find a job is slightly longer. Both differences are statistically significant. Second, we predict an individual worker's risk to become longer-term unemployed based on a range of observable characteristics and distinguish between workers with low and high risk.[26] For both blue-collar workers and those with high predicted unemployment risk, a potential extension of UI benefits is more relevant. This implies that we expect the resulting reduction in equilibrium effort to be more pronounced for blue-collar workers and those with higher predicted unemployment risk.

Prediction 3. *The effort reduction caused by an increase in UI benefits is more pronounced for blue-collar workers and workers with higher predicted unemployment risk, who are more likely to face extended phases of unemployment.*

Prediction 3 not only follows from a larger weight UI benefits have for $\bar{u}$, but also from the fact that a higher likelihood to actually become unemployed is equivalent to a smaller discount factor δ. Since a smaller δ implies a lower surplus (Lemma 2), Proposition 1 applies as well, which states that the effort reduction caused by higher (future) outside options is more pronounced if the relationship surplus has initially been small. This link is further applied in the subsequent predictions.

To test Prediction 3, we run our main regression separately for blue-collar and white-collar

[26]To obtain predicted long-time unemployment risk, we use a full panel of Upper Austrian workers and estimate a probit of the probability that a worker claims UI for more than 30 days in the next year on occupation, gender, a cubic in age, quadratics in tenure and firm size, and industry sector dummies. We then match predicted probabilities from this estimation to our sample and split at the sample median of predicted probabilities.

Table 2 — Mechanisms I: Threat of unemployment

		(a) By occupation	
	Baseline	Blue collar	White collar
	(1)	(2)	(3)
Average effect	0.388***	0.680***	0.156***
	(0.026)	(0.033)	(0.042)
t-test for effect homogeneity		-9.67 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		7.65	6.65
Number of observations	4,648,387	2,705,903	1,942,484
		(b) By predicted unemployment	
	Baseline	Low	High
	(1)	(2)	(3)
Average effect	0.388***	0.230***	0.428***
	(0.026)	(0.048)	(0.032)
t-test for effect homogeneity		3.46 ($p = 0.001$)	
Avg. sick leave duration in $t < b$		7.09	7.34
Number of observations	4,647,962	2,331,380	2,316,582

Notes: This table reports average effect estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration for different subgroups. In panel (a), we split the sample by occupational collar. In panel (b), we split by predicted unemployment. The heterogeneity variables could not be computed for some workers, hence we report the baseline for all observations with non-missing heterogeneity variables in column (1). All regressions control for tenure and year fixed effects as well as gender. The t-test indicates whether estimates from column (2) and (3) are statistically different—it comes from a separate model where we fully interact the average effect and all covariates with the heterogeneity split variable. Standard errors are clustered on the worker level. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

workers and for workers with low and high predicted unemployment risk. We report estimated effects in Table 2, panels (a) and (b). All four groups increase absenteeism significantly in response to the UI extension, but the effect is almost four times as large for blue-collar workers than for white-collar workers and around twice as high for workers with high predicted unemployment risk than for those with low predicted unemployment risk. The differences in coefficients between blue-collar and white-collar workers and between workers with low and high predicted unemployment risk are both statistically significant at the 1 percent level.

IV.2. Size of relationship value

Our next predictions are based on Proposition 1, i.e., that the negative effect of higher UI benefits on effort is more pronounced if the relationship value is smaller, in combination with factors we

Table 3 — Mechanisms II: Size of relationship value

		(a) By firm growth	
	Baseline	Firm shrinks	Firm grows
	(1)	(2)	(3)
Average effect	0.375***	0.424***	0.296***
	(0.027)	(0.036)	(0.037)
t-test for effect homogeneity		-3.05 ($p = 0.002$)	
Avg. sick leave duration in $t < b$		7.08	7.22
Number of observations	4,308,755	2,453,082	1,855,673
		(b) By AKM firm fixed effect	
	Baseline	Low-wage firm	High-wage firm
	(1)	(2)	(3)
Average effect	0.391***	0.548***	0.275***
	(0.026)	(0.036)	(0.039)
t-test for effect homogeneity		-5.79 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		7.31	7.13
Number of observations	4,640,660	2,221,688	2,418,972
		(c) By AKM worker fixed effect	
	Baseline	Low-wage worker	High-wage worker
	(1)	(2)	(3)
Average effect	0.380***	0.613***	0.032
	(0.027)	(0.033)	(0.071)
t-test for effect homogeneity		-7.71 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		8.10	6.72
Number of observations	4,420,286	2,207,823	2,212,463
		(d) By wage growth relative to coworkers	
	Baseline	Slower or similar	Faster
	(1)	(2)	(3)
Average effect	0.391***	0.473***	0.234***
	(0.026)	(0.030)	(0.048)
t-test for effect homogeneity		-3.50 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		7.33	6.98
Number of observations	4,616,555	3,225,603	1,390,952

Notes: This table reports average effect estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration for different subgroups. In panel (a), we split the sample by whether the worker's firm shrank or grew compared to the previous year. In panels (b) and (c), we split at the sample median of the estimated AKM firm and worker fixed effect distributions, respectively. In panel (d), we first calculate firm-specific wage profiles for 5-year age intervals $[15, 20), [20, 25), ..., [60, 65]$, where the wage profile is the average difference in wages between workers at the upper bound and the lower bound of the age interval. Faster wage growth indicates that, within an age interval, the individual worker's wage grew faster than the firm average wage growth plus one standard deviation of the firm wage growth in the same interval. The heterogeneity variables could not be computed for some workers, hence we report the baseline for all observations with non-missing heterogeneity variables in column (1). All regressions control for tenure and year fixed effects as well as gender. The t-test indicates whether estimates from column (2) and (3) are statistically different—it comes from a separate model where we fully interact the average effect and all covariates with the heterogeneity split variable. Standard errors are clustered on the worker level. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

suggest shape this value. There, we first pick up the discussion underlying Prediction 2, where we argue that the relationship value decreases as time passes. This interaction also has implications for the negative consequences of higher UI benefits on effort, and indicates

Prediction 4. *The effort reduction caused by an increase in UI benefits increases over time.*

Prediction 4 is supported by Figure 2, where we can see that the difference in absenteeism between treatment and control group increases over time.

Next, the size of δ in an employment relationship is affected by its future prospects. If these are worse, the likelihood that any match may be terminated is larger. The reason is that either the firm's bankruptcy risk or the chances that it has to lay off further employees are higher, and both imply a smaller continuation probability of a given match. As an indicator for the perceived stability of an employment relationship, we assess whether the firm's workforce is growing or shrinking.

Prediction 5. *The effort reduction caused by an increase in UI benefits is larger in firms with a shrinking than with a growing workforce.*

We test Prediction 5 in Table 3, panel (a), where we report effects by whether a worker's firm is smaller or larger in a given year relative to the previous year. Indeed, we find that workers in shrinking firms react more strongly, and the difference in effects between shrinking and growing firms is statistically significant at the 1 percent level.

The (future) value of a firm-worker relationship is based on many other dimensions that we cannot directly detect. Still, there are observable measures that have a clear link to or are an immediate consequence of this value. In the following we use two such measures, AKM fixed effects and a worker's wage growth relative to coworkers within the firm.

First, the relationship value is affected by the firm's or worker's inherent productivity, which in our model corresponds to the value θ. If θ is larger, the same effort generates a higher output. Therefore, if (EC) binds and effectively constrains equilibrium effort, a higher θ increases the (future) surplus and consequently reduces the negative consequences of a higher outside option on equilibrium effort. To explore such a link empirically, we follow the literature and use AKM firm-wage fixed effects as a proxy for a firm's and AKM worker fixed effects as a proxy for a worker's inherent productivity.[27] This yields

Prediction 6. *The effort reduction caused by an increase in UI benefits is more pronounced in low-wage firms and for low-wage workers.*

Note that θ also determines first-best effort, with $de^{FB}/d\theta > 0$. To generate Prediction 6 for all values of θ, we therefore must additionally demonstrate that a higher θ makes it "more likely" that e^{FB} can be implemented. Indeed, in Appendix B we show that this holds for commonly used effort cost functions. Furthermore, it can be argued that a worker's inherent productivity not only increases their productivity in a given job, but also their outside option, which would generate a negative effect on the relationship value. However, as long as the labor market has matching frictions and firms have at least some wage setting power (for which there is substantial evidence; e.g., Manning 2021), the positive effect on the relationship value would dominate.

In Table 3, panel (b), we estimate effects by whether the AKM firm fixed effect is above or below the sample median. We find that workers in low-wage firms react more strongly to the UI benefit extension, and the difference between workers in low-wage and high-wage firms is significant at the 1 percent level. In panel (c) we split the sample into low-wage and high-wage workers. We find that our effects are much more strongly driven by low-wage workers. The difference in coefficient estimates is significant at the 1 percent level.

Second, a worker's value to the firm should—at least to some extent—manifest in their compensation. If a worker's perceived future value exceeds that of their coworkers, they will also expect a higher wage growth. Although we do not know workers' expectations about future wage growth, which are most likely to affect effort at any point in time, we can observe realized wage growth ex post. Presuming that workers' expectations were at least partially correct, realized wage profiles are an informative measure of expected wage growth. This yields

[27] We estimate the AKM model on a full panel of all Austrian workers between 1998–2021 with wage information in a given year. This is different from the data we use for our other analyses. The reason is that we have information on sick leave taking only for Upper Austria, and these data end in 2018.

Prediction 7. *The effort reduction caused by an increase in UI benefits is more pronounced for workers who are on a lower wage trajectory than their coworkers.*

In Table 3, panel (d), we split the sample by whether, at a given age, a worker is on a steeper wage trajectory than their coworkers or not (see the table notes for a detailed explanation of how we compare wage trajectories). Results suggest that workers with faster wage growth react less to the change in outside options.

Finally, the unemployment rate is an important dimension of a worker's outside option. Several studies have confirmed a positive correlation between unemployment rate and worker effort (Lazear et al. 2016), a link that can also be found in our data (Appendix Figure A.8). Therefore, the (local- or sector-specific) unemployment rate may affect how much effort goes down in response to the UI benefit extension. However, this relationship is ambiguous, because the lower chances of finding a new job when facing higher unemployment induce two countervailing forces. On the one hand, the relationship value goes up, which would imply a weaker treatment effect. On the other hand, UI benefits assume a more prominent role in a worker's outside option (as we have argued for blue-collar workers), which would imply that the treatment effect is larger. Without making strong assumptions on the functional forms of the components of our model, we are not able to state that one effect dominates the other.

Prediction 8. *The effort reduction caused by an increase in UI benefits may be more or less pronounced for workers facing a higher unemployment rate.*

In Appendix Figure A.13, we provide estimates by quartiles of the sectoral unemployment rate in a region, separately for blue-collar and white-collar workers. It appears that effects are generally stronger when the unemployment rate is very low, but we can also not reject relatively large effects when the unemployment rate is very high, especially for blue-collar workers.

IV.3. Care obligations

In this section, we argue that the link between outside options and worker incentives is intensified for workers who have care obligations for their children or elderly parents. This can have several reasons. First and foremost, the UI replacement rate is substantially higher for workers with dependents (it can reach up to 80 percent of pre-UI income while the baseline replacement rate is 55 percent). This implies that the increase in outside options for workers with care obligations, and consequently also the expected treatment effect, is mechanically larger than for workers without care obligations. Moreover, care obligations may increase the opportunity costs of returning to work versus extending sick leave. To formalize this link, suppose that effort costs are $c(e, k)$, where k measures care obligations, with $c_k, c_e, c_{ee}, c_{kk} > 0$ and $c_{ek} \geq 0$, thus a higher k increases the total and marginal costs of exerting a given level of effort. Then, as we argue in Appendix B, a higher k can indeed magnify the negative effect of a higher outside option on effort.

While we do not have time use data on care activities, we can use gender as a proxy. There is an abundance of evidence that women in Austria are responsible for most of family care work (e.g., Danzer et al. 2022). As a second proxy, we use information on whether workers have children or not. Assuming that k is larger for women and for workers with children, this yields the following prediction.

Prediction 9. *The increase in absenteeism caused by higher UI benefits is more pronounced for women and for workers with children.*

We test Prediction 9 in Appendix Table A.6. Women react much stronger to the change in outside options than men, and the difference in point estimates is significant at the 1 percent level (panel a). In panel (b), we split the sample by whether workers have children or not. We find that effects are generally stronger for workers with children.

V. Alternative models of the labor market

Our mechanism relies on two building blocks. First, workers' productivity depends on their costly effort, thus they need to be motivated accordingly. Second, formal, court-enforceable, contracts are not feasible for that purpose and relational contracts based on the future relationship value are used instead. These features are sufficient for generating our predictions, and we do not need to rely on a specific set of assumptions (see Appendix F). We argue that this mechanism also does a better job explaining our observations compared to other models of the labor market. For this comparison, we focus on the most widely used alternatives, the competitive model as well as search-and-matching models.

V.1. Competitive model of the labor market

We argue that a standard, competitive model of the labor market is not well suited to generate our predictions. Thereby, we first identify potential links between prospective UI benefits and worker absenteeism in this model (if such a link does not exist, we do not need to proceed). For example, higher UI benefits might reduce stress levels on the job, but then we would expect eligible workers' sick days to go down instead of up. Alternatively, a higher outside option might have a direct positive effect on the worker's inherent productivity on the job and therefore allow them to reduce effort. Even though such a mechanism could generate our main prediction, it would be more difficult to argue that it causes the negative effect of higher outside options on effort to be stronger for older employees or those employed in shrinking or low-wage firms, and thus rationalize predictions 4, 5, and 6.

V.2. Search models

Next, we discuss a model in which labor markets are characterized by search-and-matching frictions. There, we focus on on-the-job search as a means that can potentially affect worker absenteeism.[28] Consider a worker who spends some of their working time searching for alternative jobs and assume that more search increases absenteeism. Then, higher UI benefits can affect the worker's incentives to conduct search if there is some probability that they will lose their job and if this probability increases in their search effort. In this case, extended UI benefits would indeed increase absenteeism, caused by a mechanism that is very similar to the one captured by our model: More

[28] Naturally, more generous UI benefits could also reduce incentives to search for those without a job and consequently increase the unemployment rate, thereby influencing incentives for the employed. However, we do not expect such a link to matter in our setting because the policy we utilize is not a labor market reform but instead an institutional feature that affects some employees differently than others.

search increases absenteeism and thus decreases productive effort. Thereby, the worker captures private benefits (as with an effort reduction), and the firm's payoff goes down. However, if higher absenteeism indeed was caused by more on-the-job search instead of other consequences of a reduced motivation, this would result in better job offers and consequently more job-to-job transitions (presuming that at least some of these outside offers are not matched by the current employer). In Appendix Figure A.14, we therefore test whether eligibility for more generous UI benefits increases job-to-job transitions and find little evidence that this might be the case.

VI. Summary and discussion

We have demonstrated that better outside options can decrease workers' incentives to exert effort. Exploiting age and experience cutoffs in the Austrian UI benefit schedule, we find that a one-percent increase in potential UI benefit duration increases absenteeism by 0.18 percent. This result can be explained by a relational contracting model where effort is constrained by the future value of an employment relationship. Consistent with such a model, we find evidence that effects are particularly strong when UI benefits are more important for workers' outside options and when the perceived relationship value is small.

To put our effect sizes into perspective, it is useful to compare them to Lusher, Schnorr & Taylor (2022). They find that a one-percent increase in potential UI benefits decreases productivity by 0.03 percent, which is lower than the elasticity we find in our setting. This can have several reasons. Most importantly, our effort measure—sick days—captures many dimensions of a worker's motivation, while Lusher et al. (2022) focus on one specific aspect, namely supermarket cashier transactions. Moreover, there may be macroeconomic effects of higher UI benefits that mute effort responses, and these macroeconomic effects likely are more relevant in Lusher et al.'s setting. Indeed, evidence suggests that more generous UI benefits increase unemployment (Hartung, Jung & Kuhn 2022, Jessen, Jessen, Gałecka-Burdziak, Góra & Kluve 2023, Schmieder & von Wachter 2016), and that a higher unemployment rate increases worker productivity (Lazear, Shaw & Stanton 2016). However, since in our setting benefit extensions happen on an individual level once a certain age cutoff is reached, effects on the economy-wide unemployment rate arguably are substantially smaller than in Lusher et al. (2022), who study state-wide extensions of UI benefits.

To conclude, our findings suggest that UI benefit policies can have consequences that go beyond the well-studied effects of improving the welfare of unemployed people or reducing their incentives to search for and take up new jobs. In particular, such policies can also have an indirect effect on the relational contracts firms use to motivate their employees. Since the effectiveness of relational contracts depends on the payoffs employees capture upon a separation (even if it never materializes), policies that are aimed at the unemployed can have a significant impact on the productivity of a firm's workforce. This link is not captured by the competitive model of the labor market which is still commonly used as the basis for policy evaluations but should be taken into account by governments when evaluating labor market policies.

The effect of outside options on within-firm relational contracts can also inform firms how to manage their workforce. For example, we have demonstrated that a negative impact of higher UI benefits on equilibrium effort is less pronounced for high-productivity, stable relationships. Therefore, investments into firm-specific human capital not only increase productivity directly, they also have an indirect positive equilibrium effect by relaxing the enforceability constraint on effort and mitigating potential negative consequences of better outside options. Follow-up work may consider recent developments such as the technological process that will affect relational contracts and thereby the role of outside options. For example, the monitoring of employees' activities could improve and make more dimensions of their effort verifiable. In any case, labor market studies should not neglect workers' incentives to exert effort, especially in times at which firms are struggling with phenomena like quiet quitting, i.e., employees only do what they are contractually obliged to. Then, it is particularly important to consider the role of relational contracts that can incentivize workers beyond the levels specified by their formal employment contracts.

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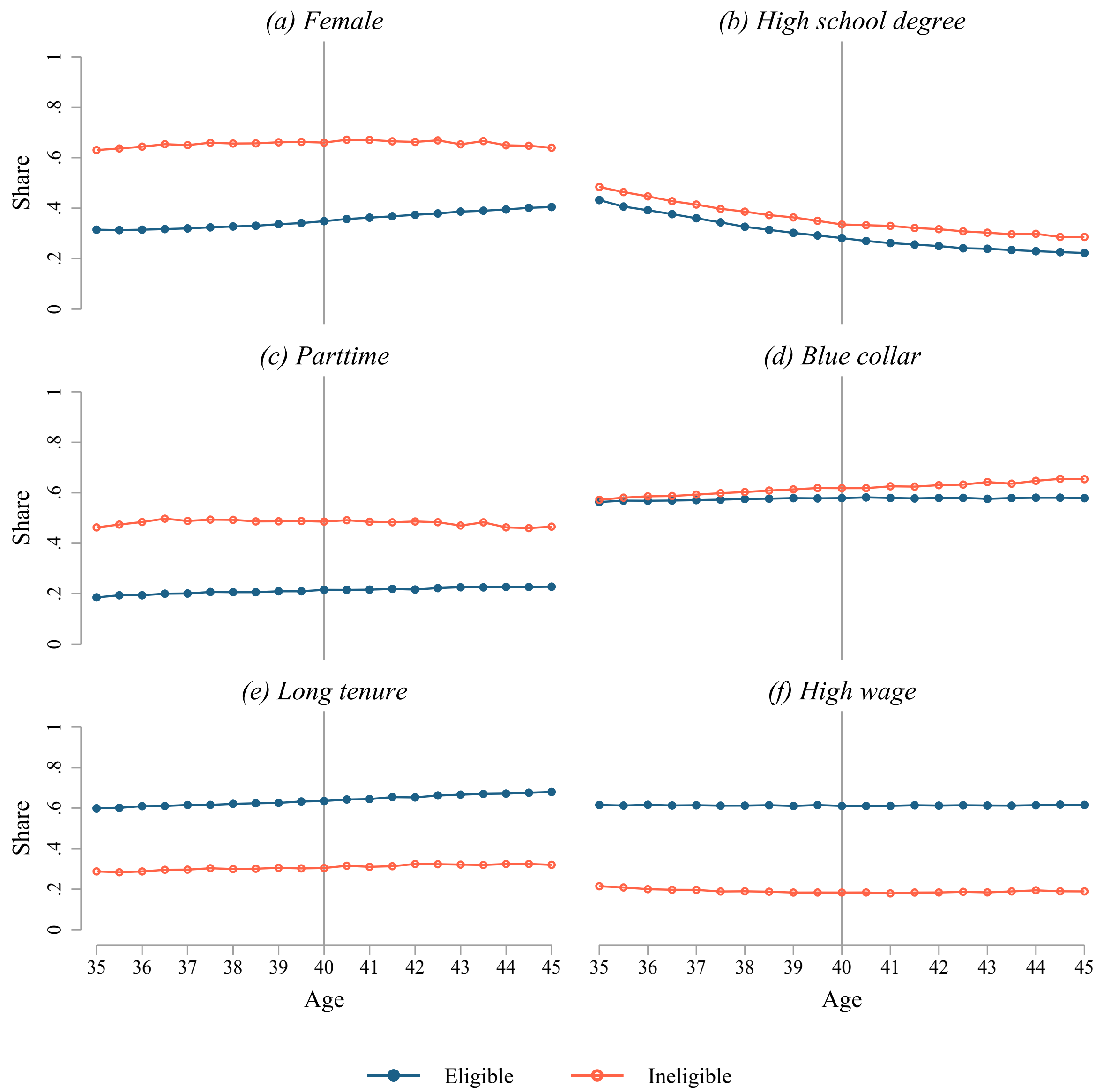
Web appendix

This web appendix contains additional tables and figures for the paper *"Outside options and worker motivation"* by Alexander Ahammer, Matthias Fahn, and Flora Stiftinger.

Contents

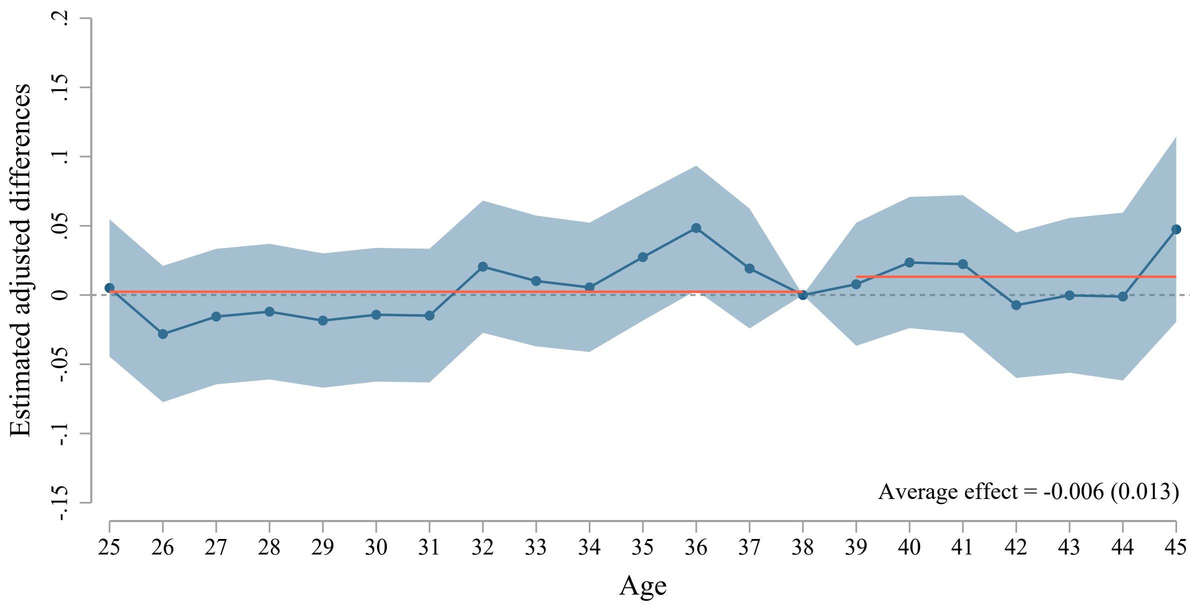
A. Additional tables and figures

Figure A.1 — Composition around the age-40 UI benefit extension cutoff



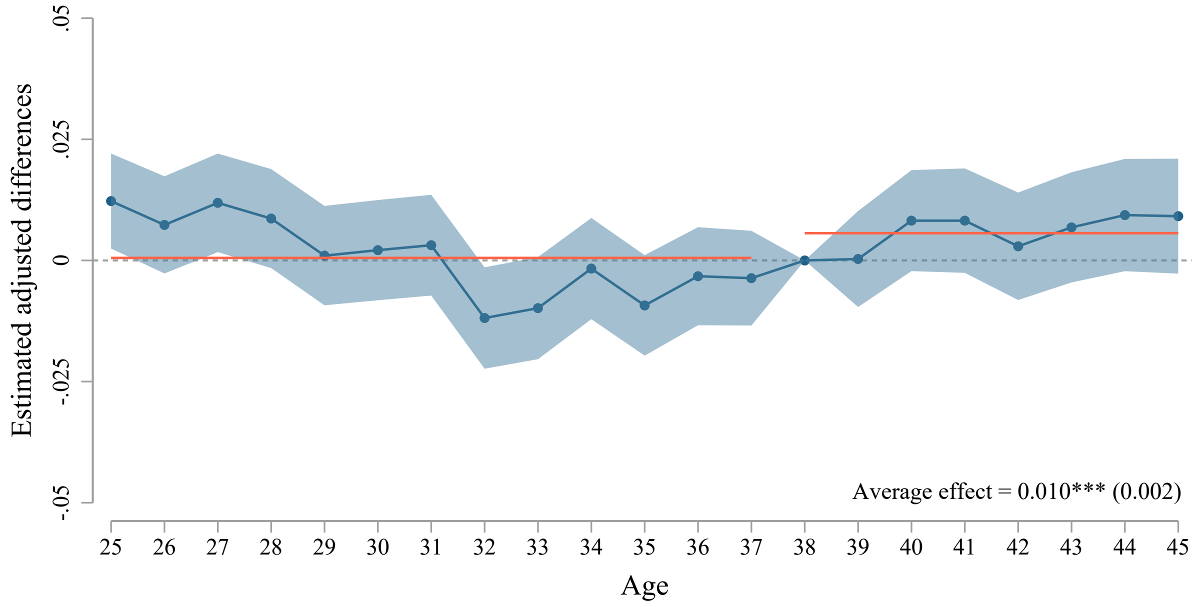
Notes: This figure shows the share of female workers (panel a), workers with a highschool degree (*'Matura,'* panel b), parttime workers (panel c), blue-collar workers (panel d), workers with above-median tenure (the median is 1.96 years, panel e), and workers with above-median wage (€22,902, panel f) for eligible and ineligible workers having a sick leave at a given age.

FIGURE A.2 — The effect of an increase in outside options on log wages



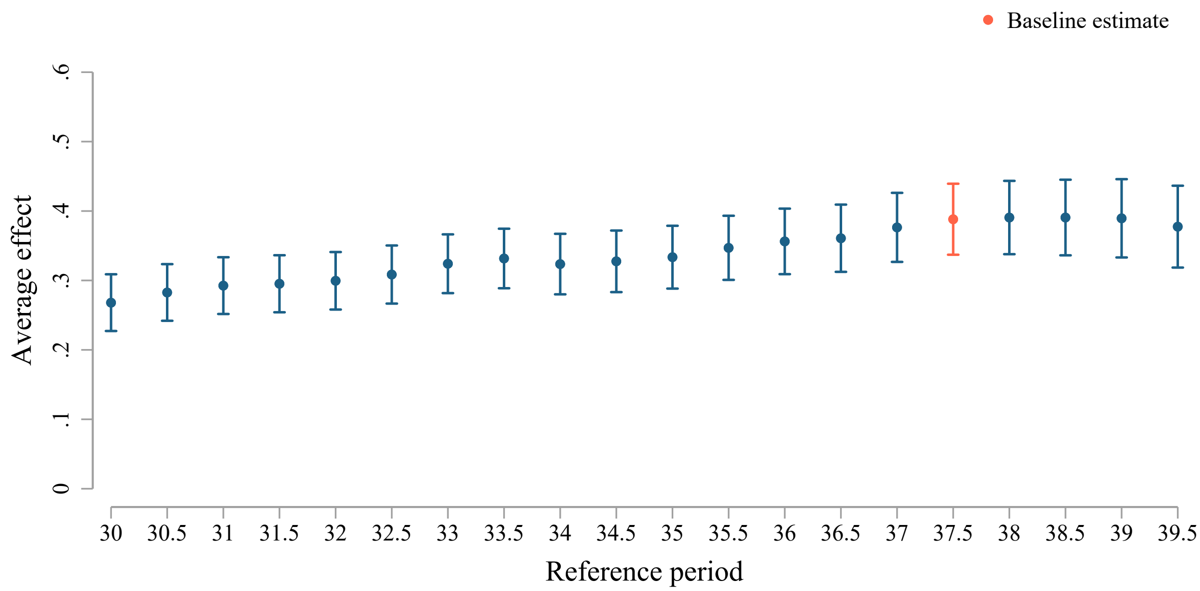
Notes: This figure provides event study estimates similar to those in equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on log annual wages. For this analysis, we collapse the data to a worker-year panel, including years where workers do not take any sick leave. Because we expect eligible workers to react already prior to age 40, we fix the reference period to $b = 38$ (because we use yearly data we cannot use 37.5), and point estimates can be interpreted as log changes in wages due to the benefit extension at a given age relative to age 38. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is from a model similar to equation (5). All regressions control for tenure and year fixed effects as well as gender.

FIGURE A.3 — Extensive margin effect on sick leaves



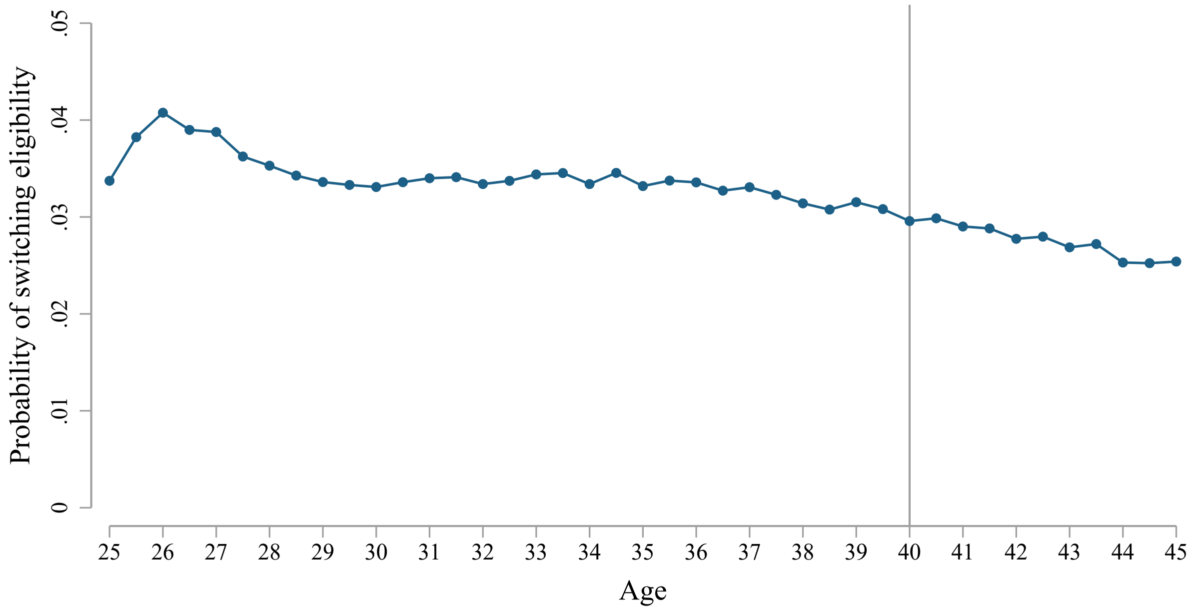
Notes: This figure provides event study estimates similar to those in equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on the probability of taking any sick leave in a given year. For this analysis, we collapse the data to a worker-year panel, including years where workers do not take any sick leave, and we omit workers in their first year of tenure (which includes a probation period with minimal job protection where workers hardly take sick leave). Because we expect eligible workers to react already prior to age 40, we fix the reference period to $b = 38$ (because we use yearly data we cannot use 37.5), and point estimates can be interpreted as percentage point changes in the probability of taking sick leave due to the benefit extension at a given age relative to age 38. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is from a model similar to equation (5). All regressions control for tenure and year fixed effects as well as gender.

FIGURE A.4 — Robustness to different reference period choices



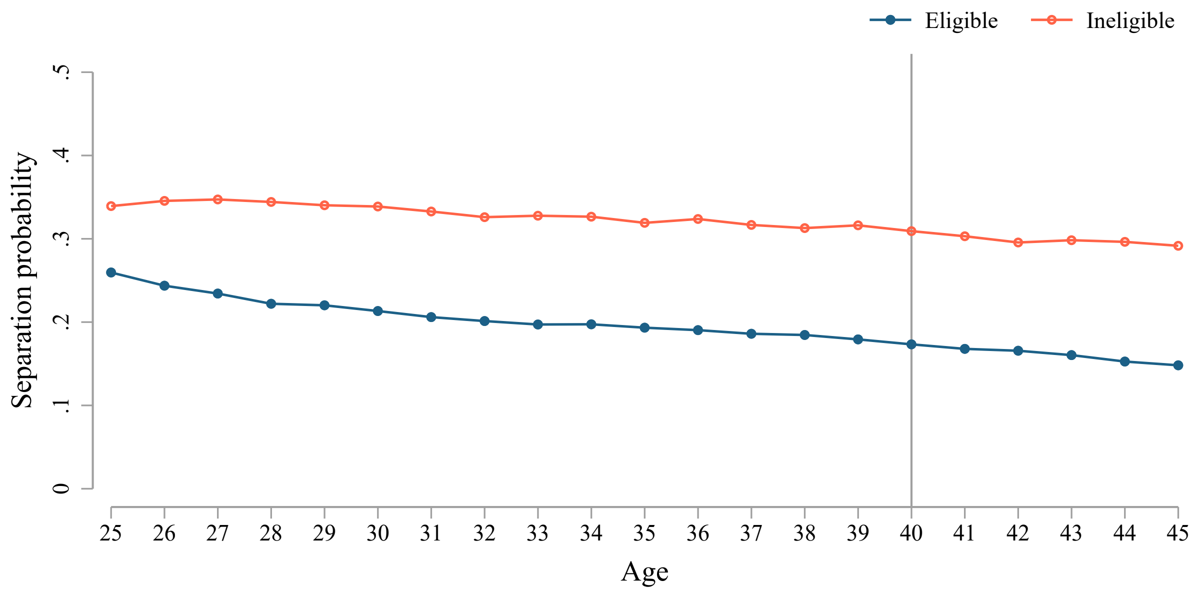
Notes: This figure shows estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers for different reference periods b. All regressions control for tenure and year fixed effects as well as gender.

Figure A.5 — Probability of switching eligibility by age



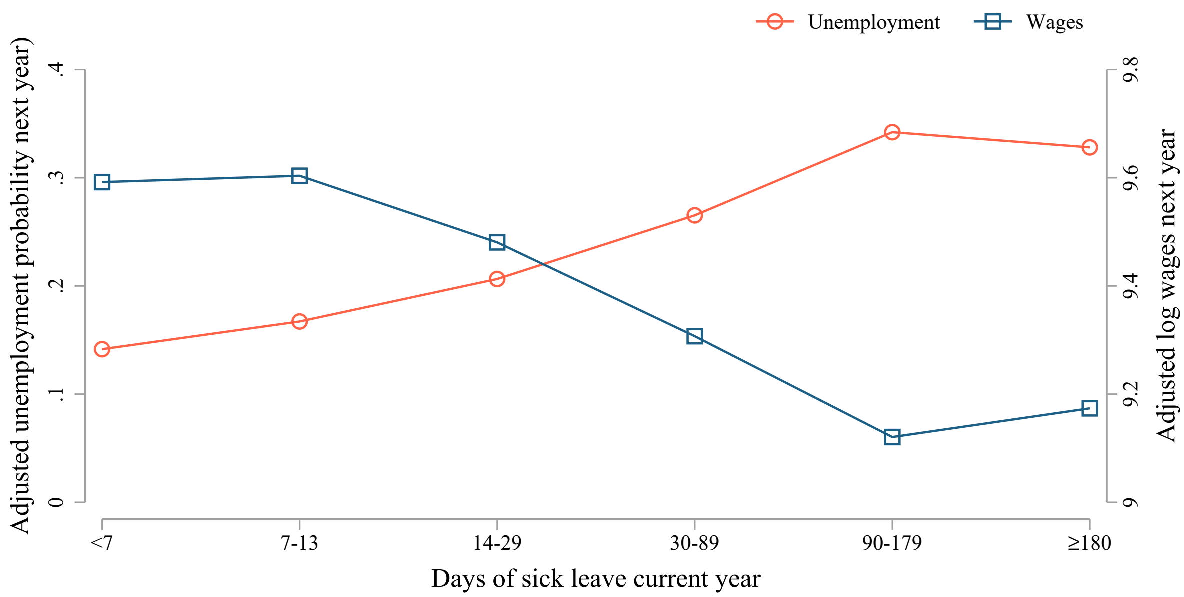
Notes: This figure plots the probability of switching eligibility status at a given age for workers in our sample. For this analysis, we collapse the data to a worker-year panel, including years where workers do not take any sick leave.

Figure A.6 — Probability of job separation by age and eligibility



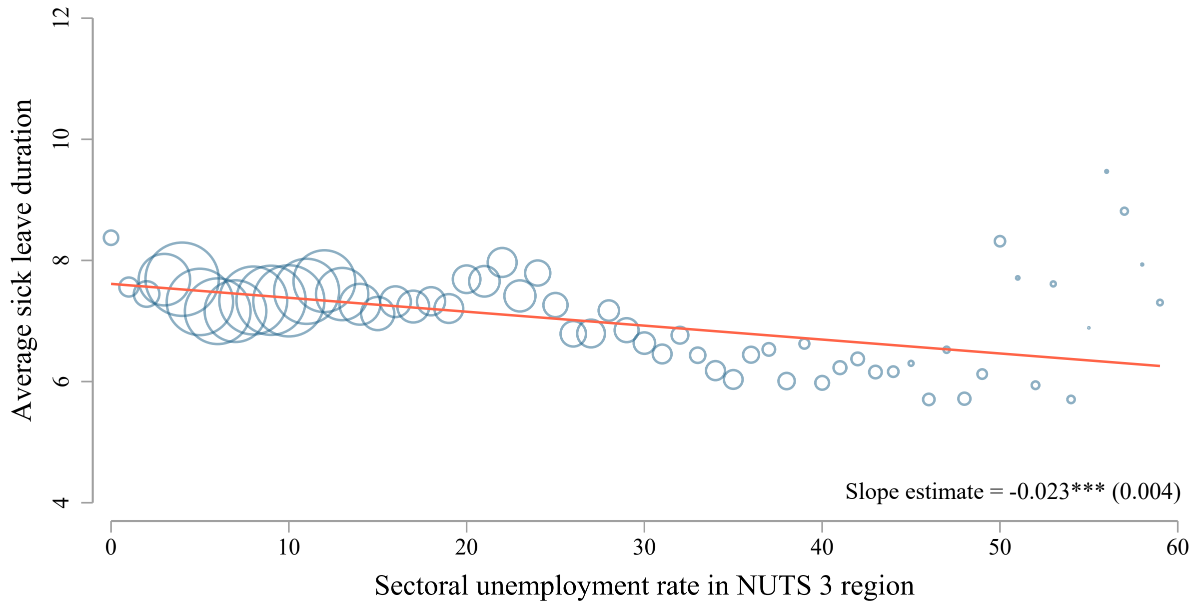
Notes: This figure plots the probability of experiencing a job separation at a given age for workers in our sample, separately by eligibility status. For this analysis, we collapse the data to a worker-year panel, including years where workers do not take any sick leave.

Figure A.7 — Relationship between sick leaves and unemployment



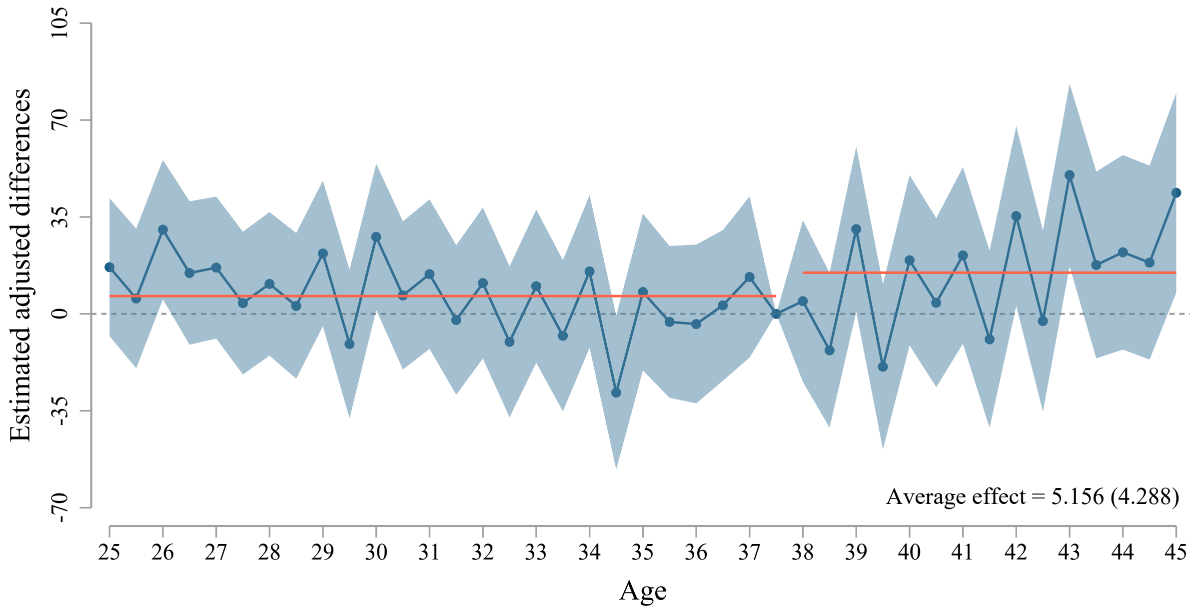
Notes: This graph shows the relationship between aggregate sick leaves in the current year and the age-adjusted probability of becoming unemployed in the next year (left axis) and age-adjusted log wages in the next year (right axis).

Figure A.8 — Relationship between sick leave duration and the unemployment rate



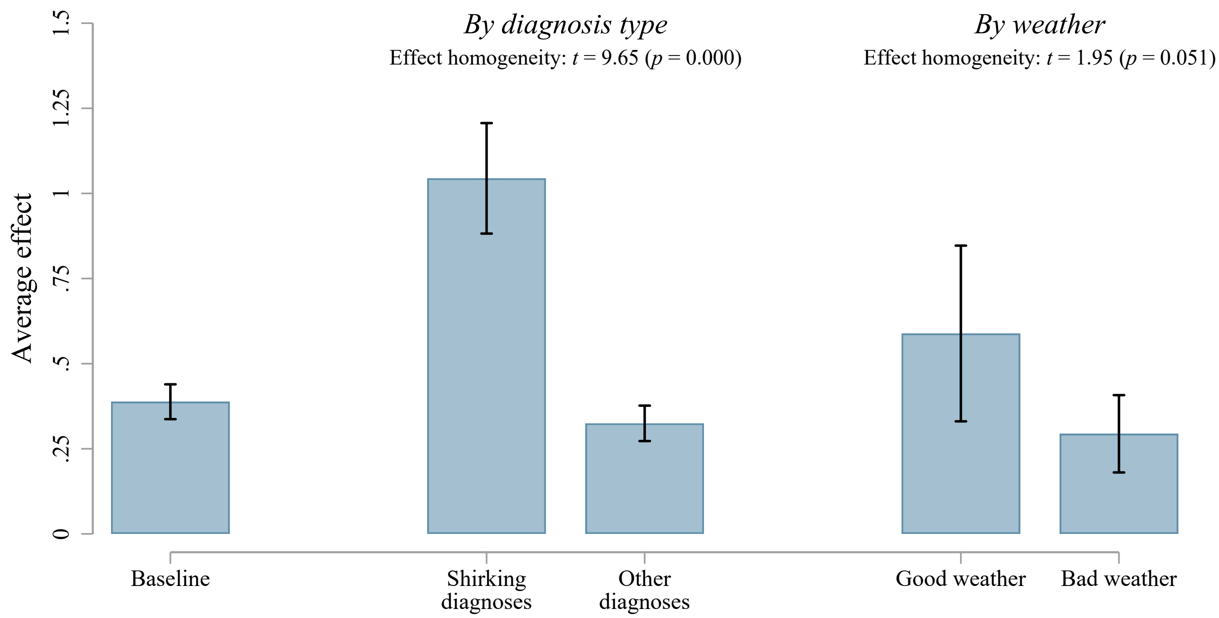
Notes: This graph depicts the relationship between sick leave duration and the sectoral unemployment rate in a region, averaged over time. The unemployment rate is calculated for every NACE95 2-digit sector and NUTS 3 combination. Both the scatters and the regression line are weighted by the number of workers at each point, which accounts for the fact that most workers are employed in sectors with relatively low unemployment rates.

Figure A.9 — The effect of an increase in outside options on total healthcare expenses



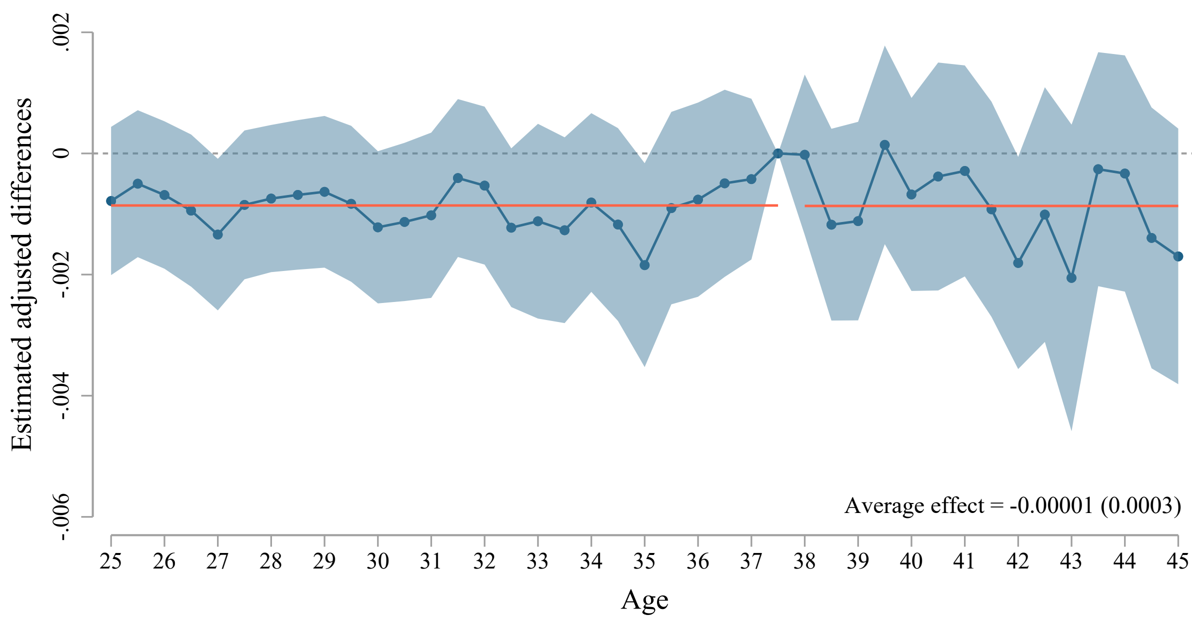
Notes: This figure provides event study estimates similar to those in equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on total healthcare expenses, which is the sum of physician fees, drug expenses, and hospital expenses. Because we expect eligible workers to react already prior to age 40, we fix the reference period to $b = 37.5$, and point estimates can be interpreted as changes in average healthcare expenses due to the benefit extension at a given age relative to age 37.5. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is from a model similar to equation (5). All regressions control for tenure and year fixed effects as well as gender.

Figure A.10 — Heterogeneity by diagnosis type and weather



Notes: This figure plots estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration by diagnosis type and weather. The error bars indicate 95 percent confidence intervals. Shirking diagnoses are defined as common cold and low back pain. Good weather is defined differently for Summer and Winter. In Summer (April–September), we define good weather as the temperature on the first day of the sick leave being half of a standard deviation higher and the sunshine duration on the first day of the sick leave being half a standard deviation longer than the monthly average in a zip code. During Winter (October–March), good weather is defined as fresh snow on the first day of the sick leave being half a standard deviation higher than the monthly average in a zip code. We test for effect homogeneity by estimating a separate model on the full sample where we interact the average effect with dummies for shirking diagnoses and good weather and reporting the t-values from these interaction terms. The null is that there is no difference in effects between shirking and other diagnoses or days with good and bad weather. All regressions control for tenure and year fixed effects as well as gender.

Figure A.11 — The effect of an increase in outside options on sick leaves due to cancer

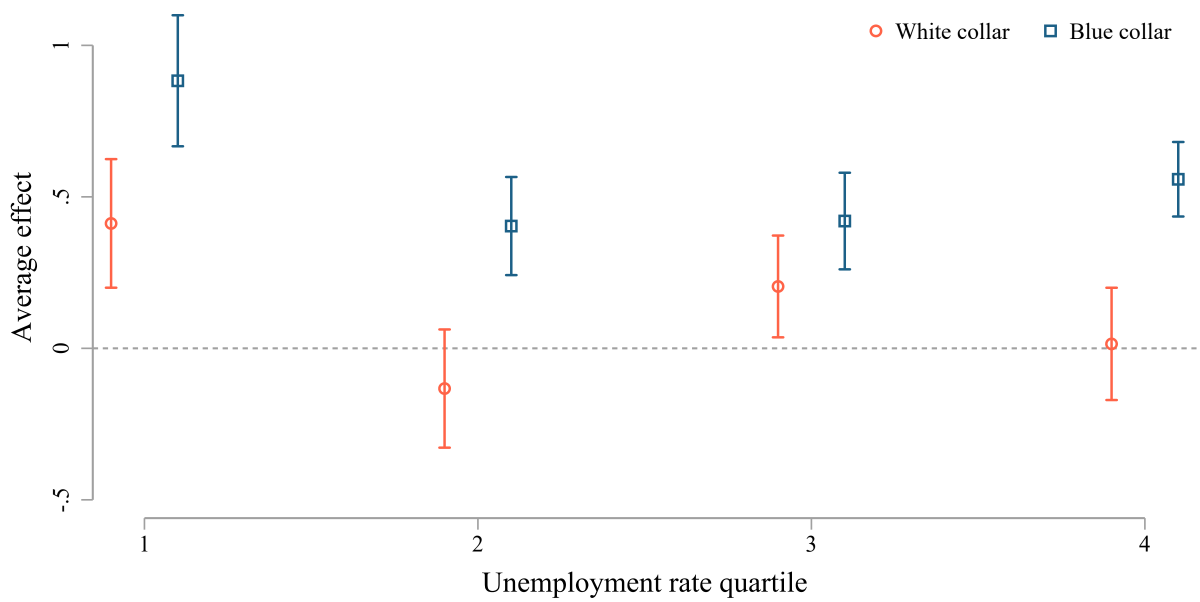


Notes: This figure plots event study estimates from equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on the probability of having a sick leave due to cancer in a certain half-year of age. We fix the reference period to $b = 37.5$, and point estimates can be interpreted as changes in average sick leave duration due to the benefit extension at a given age relative to age 37.5. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is similar to that from equation (5). All regressions control for tenure and year fixed effects as well as gender.

FIGURE A.12 — Excess flu infection rate and the number of eligible workers aged 40 or older in a firm

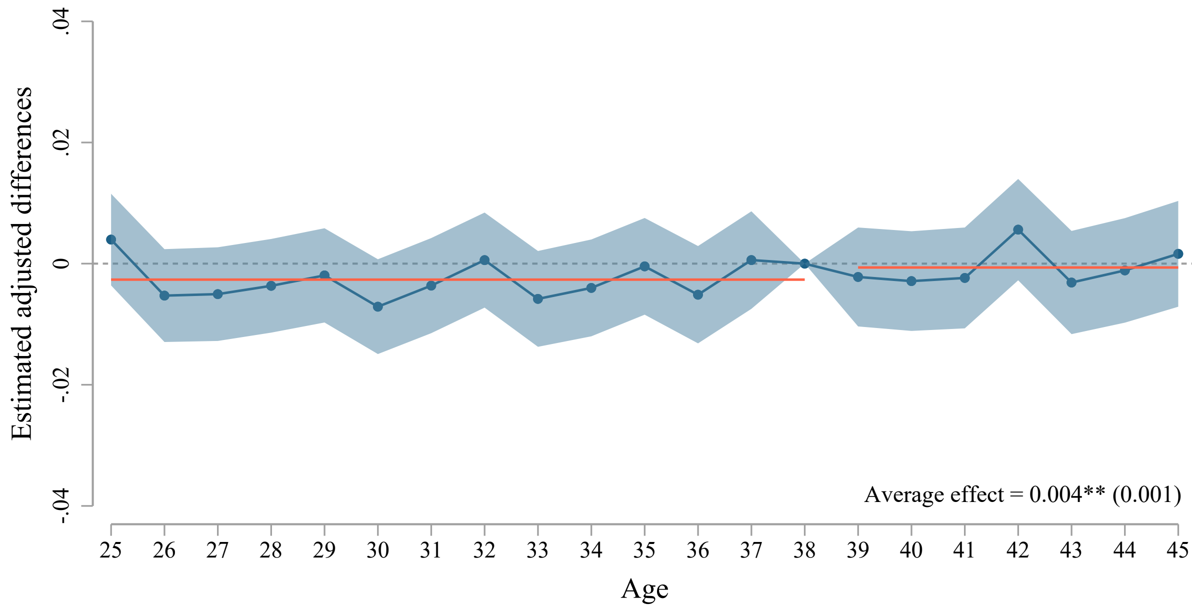
Notes: This figure depicts the relationship between excess per-worker flu infection rate and the share of eligible workers aged 40 or older in a firm. Excess flu cases are calculated as the difference of average per-worker flu cases in the firm and average per-worker flu cases across all firms in a calendar quarter. The share of eligible workers aged 40 or older is grouped into 20 quantiles. The horizontal axis plots the average share of eligible workers aged 40 or older within these quantiles. The vertical axis plots, for each quantile, average excess flu cases. The t-test in the bottom right is against the null of no relationship between excess flu cases and the share of eligible workers aged 40 or older.

Figure A.13 — The effect of an increase in outside options on sick leaves by quartiles of the sectoral unemployment rate in a region, separately for blue-collar and white-collar workers



Notes: This figure plots estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration by quartiles of the sectoral unemployment rate in a region and occupation in a given calendar quarter. The unemployment rate is calculated for every NACE95 2-digit sector and NUTS 3 combination. The error bars indicate 95 percent confidence intervals. All regressions control for tenure and year fixed effects as well as gender.

Figure A.14 — The effect of an increase in outside options on job-to-job transitions



Notes: This figure plots event study estimates similar to equation (6) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on the probability of changing jobs in a certain year of age. For this analysis, we collapse the data to a worker-year panel, including also years where workers do not take sick leave. We fix the reference period to $b = 38$ (because we use yearly data we cannot use 37.5), and point estimates can be interpreted as changes in average turnover due to the benefit extension at a given age relative to age 38. The shaded area represents a 95 percent confidence band. The red horizontal lines indicate averages of age-specific estimates for both the pretreatment period $t < b$ and the posttreatment period $t \geq b$. The average effect estimate is similar to that from equation (5). All regressions control for tenure and year fixed effects as well as gender.

Table A.1 — Summary statistics

	Mean	Std. dev.	By eligibility status		
			Eligible	Ineligible	Difference
	(1)	(2)	(3)	(4)	(5)
(a) Outcome					
Sick leave duration (days)	7.22	8.47	7.42	6.76	–0.67***
(b) Treatment assignment information					
Experience (years)	10.05	4.84	12.21	5.14	–7.06***
(c) Socioeconomic and job information					
Female	0.42	0.49	0.35	0.58	0.23***
High school degree†	0.56	0.50	0.54	0.61	0.07***
Parttime worker†	0.22	0.42	0.16	0.36	0.20***
Blue-collar worker	0.57	0.49	0.59	0.53	–0.05***
Tenure (years)	3.46	3.17	4.13	1.98	–2.16***
Annual wage (€ 1,000)	23.10	12.29	25.53	17.74	–7.79***

Notes: This table provides summary statistics using only data for workers aged 37.5 years or younger. We provide statistics for the overall sample (means in column 1 and standard deviations in column 2) and by whether workers are eligible for the UI benefit extension at age 40 or not at a given age (columns 3 and 4). Column (5) gives the difference between columns (3) and (4), with the stars indicating p-values from a two-sample t-test with significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The number of observations is 3,153,475.

† Education is missing for 0.8 percent of observations and parttime status is missing for 44.02 percent of observations. Means and standard deviations are calculated based on all observations with non-missing values.

Table A.2 — Robustness to different regression specifications

	OLS			Fixed effects	
	(1)	(2)	(3)	(4)	(5)
Average effect	0.437***	0.388***	0.536***	0.421***	0.411***
	(0.027)	(0.026)	(0.026)	(0.038)	(0.023)
Covariates					
Female		−0.064***	0.000		
		(0.013)	(0.015)		
Blue-collar worker			0.702***		
			(0.014)		
Parttime worker			0.124***		
			(0.018)		
High school degree			−0.468***		
			(0.014)		
Annual wage (€ 1,000)			−0.023***		
			(0.001)		
Tenure and year fixed effects	No	Yes	Yes	Yes	Yes
Worker fixed effects	No	No	No	Yes	No
Diagnosis fixed effects	No	No	No	No	Yes

Notes: This table reports average effect estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration using different regression specifications. In column (1), we run model (5) using OLS without any covariates. In column (2), we control for a gender dummy as well as tenure and year fixed effects, which is our baseline. In column (3), we additionally control for occupation, parttime work, education, and wage. In column (4), we estimate model (5) with worker fixed effects and tenure and year fixed effects. In column (5), we estimate the model with ICD-10 3-digit diagnosis group fixed effects. Whenever we control for education and parttime work, we replace missing values with zero and add a missing indicator dummy to the regression. The average sick leave duration in the pretreatment period $t < b$ is 7.22, the number of observations in each cell is 4,648,387. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3 — Five most common diagnoses by age and eligibility

Age 25		Age 30		Age 35		Age 40		Age 45	
Eligible	Ineligible	Eligible	Ineligible	Eligible	Ineligible	Eligible	Ineligible	Eligible	Ineligible
J06	J06	J06	J06	J06	J06	J06	J06	J06	J06
(26.22)	(27.89)	(27.70)	(26.53)	(26.89)	(24.56)	(25.31)	(22.95)	(23.30)	(22.17)
A09	A09	A09	A09	A09	A09	A09	A09	A09	A09
(10.41)	(10.40)	(9.16)	(9.35)	(7.91)	(8.53)	(6.64)	(7.64)	(5.85)	(6.96)
J02	J02	M54	M54	M43	M54	M54	M54	M54	M54
(2.52)	(2.95)	(2.81)	(3.49)	(3.69)	(4.04)	(4.23)	(4.67)	(5.36)	(5.55)
K08	M54	M43	M53	M54	M53	M43	M53	M43	M53
(2.50)	(2.47)	(2.75)	(2.74)	(3.31)	(3.47)	(4.14)	(4.27)	(4.41)	(4.48)
M54	K08	J02	J02	M53	M43	M53	M43	M53	M43
(2.30)	(2.46)	(2.60)	(2.72)	(3.03)	(3.32)	(3.86)	(3.49)	(4.22)	(3.37)

Notes: Shares in parentheses are calculated over all ICD-10 diagnoses at a given age and for an eligibility group.
A09: Other gastroenteritis and colitis of infectious and unspecified origin
J02: Acute pharyngitis
J06: Acute upper respiratory infections of multiple and unspecified sites
K08: Other disorders of teeth and supporting structures
M43: Other deforming dorsopathies
M53: Other dorsopathies, not elsewhere classified
M54: Dorsalgia

Table A.4 — Robustness to different eligibility status constraints

	Baseline	Eligibility constraints: Fixed at age 37.5	Eligibility constraints: Drop switchers
	(1)	(2)	(3)
Average effect	0.388***	0.466***	0.637***
	(0.026)	(0.032)	(0.035)
Tenure fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Gender and occupation	Yes	Yes	Yes
Average sick leave duration in $t < b$	7.22	7.22	7.33
Number of observations	4,648,387	4,228,846	3,138,720

Notes: This table reports average effect estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration using different eligibility constraints. In column (2), we consider all workers that are (in)eligible at age 37.5, regardless of whether they change eligibility status later. In column (3), we drop workers from the sample that switch eligibility status at some point. All regressions control for tenure and year fixed effects, and gender. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5 — Probability of becoming unemployed by occupation

	Overall (1)	White collar (2)	Blue collar (3)	Difference (4)
Probability of becoming unemployed	0.14	0.10	0.18	−0.08***
	(0.35)	(0.29)	(0.38)	
Avg. unemp. duration | being unemployed	96.16	95.25	96.55	−1.30***
	(74.22)	(73.24)	(74.64)	
Expected unemp. duration per year	17.35	9.83	23.38	−13.54***
	(46.73)	(36.94)	(52.51)	

Notes: This table reports the probability of becoming unemployed and the average unemployment duration per year by occupational collar in our data. We do not consider workers that are recalled to the same firm within 9 months as unemployed. The value in column (4) is the difference between columns (2) and (3) and the stars indicate whether the difference is statistically significantly different from zero based on a two-sample t-test with significance levels * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6 — Effects by gender and whether workers have children

		(a) By gender	
	Baseline	Women	Men
	(1)	(2)	(3)
Average effect	0.388***	0.688***	0.102**
	(0.026)	(0.037)	(0.040)
t-test for effect homogeneity		-12.29 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		7.06	7.34
Number of observations	4,648,387	1,956,733	2,691,654
		(b) By having children	
	Baseline	No children	Children
	(1)	(2)	(3)
Average effect	0.388***	0.184***	0.502***
	(0.026)	(0.046)	(0.032)
t-test for effect homogeneity		5.65 ($p = 0.000$)	
Avg. sick leave duration in $t < b$		7.01	7.44
Number of observations	4,648,387	2,121,307	2,527,080

Notes: This table reports average effect estimates from equation (5) for the differential impact of the UI benefit extension at age 40 between eligible and ineligible workers on sick leave duration for female and male workers (panel a) and for workers with and without children at the time of the sick leave (panel b). All regressions control for tenure and year fixed effects, in panel (b) we also control for gender. Stars indicate significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B. Proofs

Proof of Lemma 2. Assume a profit-maximizing equilibrium with effort levels e_t^*, and that θ goes up to θ'. Holding e_t^* constant, this directly increases each per-period surplus $e_t^*\theta - c(e_t^*)$ and thus each S_t. Moreover, all (EC) are relaxed in t and higher effort levels can and will be implemented because a higher θ also increases e^{FB}. Holding equilibrium effort levels constant, a higher δ increases S_t, which further relaxes all (EC) constraints. Finally, a higher $\bar{S}_{t+1}$ has no direct effect on S_t, but tightens (EC) in t and therefore reduces S_t if the constraint binds. ■

Proof of Proposition 1. Assume $e_t^* < e^{FB}$ is characterized by the binding (EC) constraint. Then, the implicit function theorem yields

$$\frac{de_t^*}{d\delta_t(S_{t+1} - \bar{S}_{t+1})} = \frac{1}{c'(e_t^*)} > 0,$$

which implies $de_t^*/d\bar{S}_{t+1} < 0$. Moreover,

$$\frac{d^2 e_t^*}{d\left(\delta_t(S_{t+1} - \bar{S}_{t+1})\right)^2} = -\frac{c''(e_t^*)}{\left(c'(e_t^*)\right)^2}\frac{1}{c'(e_t^*)} < 0.$$

The last statement follows since $e_t^* = e^{FB}$, and e^{FB} is unaffected by outside options. ■

Proof of Proposition 2. The stationarity of effort in all periods $t \geq T-1$ follows from standard arguments (see Levin 2003). Regarding existence of $\tilde{\delta}$, note that $\tilde{e}$ is constrained by $-c(\tilde{e}) + \delta\left[\tilde{e}\theta - \left(\bar{\pi} + \bar{u}^H\right)\right] \geq 0$. For $\left[\tilde{e}\theta - \left(\bar{\pi} + \bar{u}^H\right)\right] > 0$, the left hand side is increasing in δ. For $\delta \longrightarrow 1$, the constraint holds for e^{FB}, for $\delta \longrightarrow 0$, it is violated for e^{FB}.

Now, take period $T-2$. There, effort is constrained by

$$-c(e_{T-2}) + \delta\frac{\tilde{e}\theta - c(\tilde{e})}{1-\delta} \geq \delta\left(\bar{\pi} + \bar{u} + \delta\frac{\bar{\pi} + \bar{u}^H}{1-\delta}\right), \tag{EC}$$

which can be rewritten to

$$-c(e_{T-2}) + \delta\left[\tilde{e}\theta - \left(\bar{\pi} + \bar{u}^H\right)\right] + \delta\left[c(e_{T-2}) - c(\tilde{e}) + \left(\bar{u}^H - \bar{u}\right)(1-\delta)\right] \geq 0. \tag{B.1}$$

If $\tilde{e} = e^{FB}$, (EC) also holds for $e_{T-2} = e^{FB}$. If $\tilde{e} < e^{FB}$ and $\delta\left[\tilde{e}\theta - \left(\bar{\pi} + \bar{u}^H\right)\right] \geq c(\tilde{e})$, (EC) becomes $\delta\left(\bar{u}^H - \bar{u}\right) \geq \left(c(e_{T-2}) - c(\tilde{e})\right)$, thus $e_{T-2} > \tilde{e}$. Existence of $\tilde{\delta}_{T-2}$ is immediate: At $\tilde{\delta}$, $\bar{u} < \bar{u}^H$ implies that (EC) is slack for $e_{T-2} = e^{FB}$. Continuity and monotonicity in δ deliver the stated properties.

The rest of the proposition follows: The earlier a period, the smaller the weight of $\bar{u}^H$ relative to $\bar{u}$ in the respective (EC) constraint. ■

Formal Discussion of Prediction 6. Assume two productivity levels, θ^l and θ^h. If the (EC) constraint binds for both values, the prediction follows from Lemma 2, which states that the surplus increases in θ, as well as Proposition 1. It thus remains to show that (EC) is "more likely to bind" for θ^l than for θ^h. To do that, we compute the critical discount factors above which first-best effort can be implemented and explore whether it is indeed larger for θ^l.

For that, we focus on period $T-1$ after which the relational contract is stationary. Then, first-best effort – characterized by $\theta - c'(e^{FB}) = 0$ – can be implemented if it satifies $-c(e^{FB}) + \delta\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right) \geq 0$, or if

$$\delta \geq \bar{\delta} \equiv \frac{c(e^{FB})}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)}.$$

Moreover,

$$\begin{aligned}
\frac{d\bar{\delta}}{d\theta} =& -\frac{c(e^{FB})e^{FB}}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2} + \frac{c'(e^{FB})\left(\theta e^{FB} - \bar{u} - \bar{\pi}\right) - c(e^{FB})\theta}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2} \frac{de^{FB}}{d\theta} \\
=& -\frac{c(e^{FB})e^{FB}}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2} + \frac{c'(e^{FB})\left(\theta e^{FB} - \bar{u} - \bar{\pi}\right) - c(e^{FB})\theta}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2 c''(e^{FB})} \\
=& \frac{c'(e^{FB})\theta e^{FB} - c(e^{FB})\theta - c''(e^{FB})c(e^{FB})e^{FB}}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2 c''(e^{FB})} \\
&- \frac{c'(e^{FB})\left(\bar{u}^H + \bar{\pi}\right)}{\left(\theta e^{FB} - \bar{u} - \bar{\pi}\right)^2 c''(e^{FB})} \\
=& \frac{c'(e^{FB})c'(e^{FB})e^{FB} - c(e^{FB})c'(e^{FB}) - c''(e^{FB})c(e^{FB})e^{FB}}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2 c''(e^{FB})} \\
&- \frac{c'(e^{FB})\left(\bar{u}^H + \bar{\pi}\right)}{\left(\theta e^{FB} - \bar{u}^H - \bar{\pi}\right)^2 c''(e^{FB})}.
\end{aligned}$$

Since $\bar{u}^H + \bar{\pi} > 0$, for $d\bar{\delta}/d\theta < 0$ to hold it is sufficient that the numerator of the first term is non-positive. For a standard effort cost function $c(e) = \frac{e^n}{n}$, with $n \geq 2$, this term becomes (for any e)

$$\begin{aligned}
&e^{2n-1} - \frac{e^{2n-1}}{n} - (n-1)\frac{e^{2n-1}}{n} \\
=&e^{2n-1}\left(\frac{n-1}{n} - \frac{n-1}{n}\right) = 0.
\end{aligned}$$

■

Formal Discussion of Prediction 9. Assume effort costs are $c(e,k)$, with $c_e, c_k, c_{ee}, c_{kk} > 0$ and $c_{ek} \geq 0$. k indicates the amount of care work the agent is responsible for, with a higher k increasing the total and marginal costs of exerting effort on the job. We want to explore whether the negative effect of a higher $\bar{u}$ is more pronounced if k is larger. We again focus on period $T-1$ after which the relational contract is stationary. Then, the (EC) constraint equals $-c(e,k)+\delta(e\theta-\bar{u}-\bar{\pi}) \geq 0$. First, we assume that (EC) binds, hence

$$\begin{aligned}
\frac{de}{dk} &= \frac{c_k}{-c_e+\delta\theta} < 0 \\
\frac{de}{d\bar{u}} &= \frac{\delta}{-c_e+\delta\theta} < 0 \\
\frac{d^2e}{dk d\bar{u}} &= \frac{c_{ek}\left(-c_e+\delta\theta\right)+c_k c_{ee}}{\left(-c_e+\delta\theta\right)^2}\frac{de}{d\bar{u}}.
\end{aligned}$$

This term is negative if $c_{ek}\left(-c_e+\delta\theta\right)+c_k c_{ee}$ is positive, which holds for $c_{ek}=0$. For $c_{ek}>0$, assume the standard effort cost function $c(e,k)=k\frac{e^n}{n}$, for which

$$\begin{aligned}
&c_{ek}\left(-c_e+\delta\theta\right)+c_k c_{ee} \\
=&e^{n-1}\left(-ke^{n-1}+\delta\theta\right)+k\frac{e^n}{n}(n-1)e^{n-2} \\
=&e^{n-1}\left(\delta\theta-\frac{ke^{n-1}}{n}\right) \\
=&e^{n-1}\left(\bar{u}+\bar{\pi}\right)>0.
\end{aligned}$$

To assess whether a higher k also makes it "less likely" that the (EC) binds, we again state the critical discount factor above which e^{FB} can be implemented,

$$\bar{\delta} \equiv \frac{c(e^{FB},k)}{\theta e^{FB}-\bar{u}-\bar{\pi}},$$

with

$$\begin{aligned}
\frac{d\bar{\delta}}{dk} =&\frac{c_k}{\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)}+\frac{c_e\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)-c(e^{FB},k)\theta}{\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)^2}\frac{de^{FB}}{dk} \\
=&\frac{c_k}{\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)}-c_{ek}c_e\frac{\left(c_e e^{FB}-c(e^{FB},k)\right)}{c_{ee}\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)^2} \\
&+\frac{c_{ek}c_e}{c_{ee}}\frac{\left(\bar{u}+\bar{\pi}\right)}{\left(\theta e^{FB}-\bar{u}-\bar{\pi}\right)^2},
\end{aligned}$$

which clearly is positive for $c_{ek}=0$. For $c_{ek}>0$, we again assume the standard effort cost function

$c(e,k) = k\frac{e^n}{n}$, with $n \geq 2$, for which this term becomes (for any e)

$$\begin{aligned}
&\frac{\frac{e^n}{n}}{(\theta e - \bar{u} - \bar{\pi})}\left(1 - \frac{ke^n}{(\theta e - \bar{u} - \bar{\pi})}\right) + \frac{c_{ek}c_e}{c_{ee}}\frac{(\bar{u} + \bar{\pi})}{(\theta e - \bar{u} - \bar{\pi})^2} \\
=& -\frac{e^n}{n}\frac{(\bar{u} + \bar{\pi})}{(\theta e - \bar{u} - \bar{\pi})^2} + \frac{e^n}{(n-1)}\frac{(\bar{u} + \bar{\pi})}{(\theta e - \bar{u} - \bar{\pi})^2} \\
=& \frac{e^n}{n(n-1)}\frac{(\bar{u} + \bar{\pi})}{(\theta e - \bar{u} - \bar{\pi})^2} > 0.
\end{aligned}$$

■

C. Other age cutoffs

We would wrongly attribute absenteeism effects to a change in potential UI duration if eligible, but not ineligible workers were affected by other policies at a cutoff around age 40. In Table C.1 we present other potentially relevant age-based cutoffs. Importantly, other age-based cutoffs and labor market policies are not relevant for individuals aged between 35 and 45, and these cutoffs are not experience-rated.

C.1. UI benefits

Apart from the UI benefit extensions we exploit in this paper (as described in section III.1), there are some additional age-based cutoffs in the Austrian UI system. The base of UI benefits of workers aged 45 or older is protected. Since UI benefits are based on earnings from the prior year, UI benefits could decrease when a worker takes a lower paid job. Workers aged 45 or older and become unemployed are protected against a decrease in UI benefits. Additionally, while there is no general job protection in Austria, employees aged 50 or older benefit from more generous protection against unfair dismissal on social grounds if they decide to sue their former employer. Importantly, both features affect eligible and ineligible workers to the same extent.

C.2. Retirement

The Austrian retirement system consists of different schemes. Prior to full retirement, workers can take up partial retirement, which allows them to reduce their working time by 40 to 60 percent with full wage compensation in the five years prior to their retirement. The usual retirement age in Austria in the relevant time period was, at the minimum, 55 for women and has increased up to 65 for men (for a more detailed discussion, see Appendix Section E). Generally, workers need to have contributed to their retirement fund for at least 15 of the last 30 years. Note that this experience cutoff is tremendously higher than the one we use in our design (6 of last 10 years). Workers contribute to retirement insurance either by being employed, receiving unemployment insurance, being on parental leave, and can also voluntarily self-insure. Thus, being eligible for retirement is primarily driven by age.

C.3. Labor market policies

The Austrian labor law does not stipulate age limits for public sector jobs, such as public service or police, other than being at least 18 years old. However, there are specific labor market policies that apply to people aged 50 or older as well as long-term unemployed people. In 2017, the Austrian government enacted a job guarantee program to employ long-term unemployed people over 50 in public employment with the program financing 100 percent of their wage cost. This program aimed to reintegrate long-term unemployed people in the labor market, but was terminated in December 2017. The Austrian ministry of labor additionally subsidizes firms with a distinct focus on hiring older employees, long-term unemployed people, as well as other vulnerable groups. There are further wage subsidies for firms that hire employees older than 50 and long-term unemployed people.

C.4. Adult education subsidy

Workers who want to obtain further education in Austria can take government-subsidized leave while attending university or other adult education programs. In such a case, workers remain employed with the current firm but the government covers part of their wage bill for up to one year. There is no age or tenure limit on the eligibility of the adult education subsidy.

C.5. Fertility and school starting age

Additionally, we show that the mean age at birth in our sample is approximately 26.5 for mothers (25.9 for ineligible and 26.8 for eligible mothers) and 31.3 for fathers (31.8 for ineligible and 31.3 for eligible fathers). The usual school starting age is 6 years, but this does not differ between eligible and ineligible workers.

Table C.1 — Summary of other relevant age-based regulations

	Age	Description
(a) Unemployment insurance		
Dismissal protection[†]	≥ 50	Increases protection against unfair dismissal on social grounds.
UI benefits base protection[‡]	≥ 45	After the 45th birthday the assessment base of UI benefits cannot decrease anymore.
(b) Retirement		
Partial retirement	≤ 5 years before regular retirement	Allows working hours reduction by 40 to 60 percent with full wage.
Retirement age[††]	≥ 55	The minimum retirement age in our sample is 55 and increases over time.
(c) Labor market policies[‡‡]		
Public service	no age limit	There is no age limit to become a public servant.
Employment subsidy	≥ 50	Aims to create public sector jobs for older long-term unemployed people and subsidize up to 100 percent wage costs (only in 2017).
Secondary labor market institutions[¶]	≥ 50	Subsidized employment to build bridge to primary labor market
Wage subsidy	≥ 50	Aims to integrate older employees into the labor market by partly substituting wage costs.
(d) Adult education subsidy		
Adult education subsidy	no age limit	Subsidized leave for employees to obtain adult education.
(e) Fertility		
Age at birth[*]	Mothers: 26.5, Fathers: 31.3	

Notes: This table provides an overview of other age-cutoffs, labor market policies, education and training, as well as fertility.

[†] Austrian labor law only provides protection against dismissal in specific cases, e.g., pregnancy, unfair dismissal on social grounds, and union membership.
[‡] UI benefits are calculated based on prior wages. If an employee becomes unemployed after 45, takes a lower paid job, and then becomes unemployed again, their UI benefits cannot be lower than the initial UI benefits.
[††] For further discussion of the retirement age see Appendix Section E.
[‡‡] Many labor market policies also apply for long-term unemployed persons (≥ 1 year or ≥ 6 months for persons aged ≤ 25).
[¶] Institutions on the secondary labor market have a distinct focus on hiring vulnerable groups, e.g., workers 50 or older and long-term unemployed people.
[*] Ages are based on own calculations using births in in our sample.

D. Breakpoint detection

To find an initial value for the reference group b in our event studies, we perform a simple breakpoint detection method that follows Evans et al. (2019). Define D_t as the difference in average sick leave durations between eligible and ineligible workers at age t in the raw data,

$$D_t = \overline{\text{duration}}_t^{\text{eligible}} - \overline{\text{duration}}_t^{\text{ineligible}}. \tag{D.1}$$

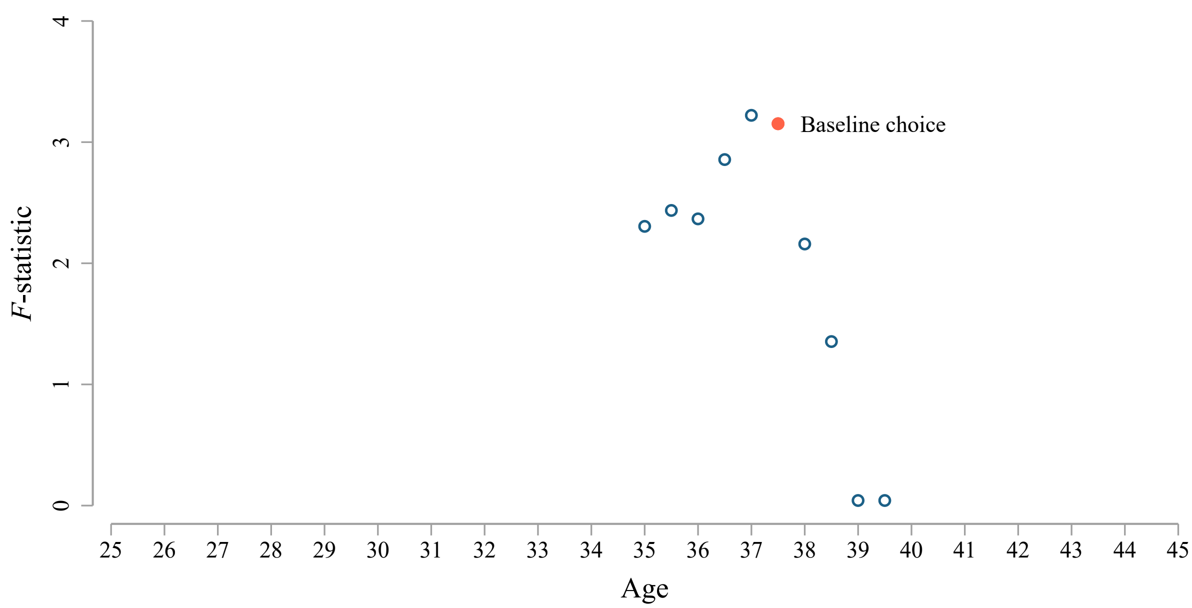
Then, for each potential break point c along the age distribution, estimate the quadratic splines

$$D_t = \alpha + \beta_1 t_c (1 - A_t^c) + \beta_2 t_c^2 (1 - A_t^c) + \gamma_1 t_c A_t^c + \gamma_2 t_c^2 A_t^c + u_t \tag{D.2}$$

where $A_t^c = \mathbb{1}[t \geq c]$ and t_c is a linear age trend centered around c, $t_c = t - c$. Break points will have large F-statistics on the test of a break in trend ($\beta_1 = \gamma_1$ and $\beta_2 = \gamma_2$). Similar to Evans et al. (2019), we first pick a window where the gap in sick leave durations visually begins to open based on Figure 1, which is between 35 and 39.5 years of age. In Figure D.1, we plot F-statistics for different values of c in this age window.

We see that the F-statistic is largest for $c = 37$ and $c = 37.5$, before it drops off significantly at age 38. We pick 37.5 as the baseline reference group because it is closer to the UI cutoff at age 40. In any case, we show that the reference group choice has little impact on our estimates in section III.5.

FIGURE D.1 — F-statistics for different potential breakpoints



Notes: This figure shows F-statistics for the test that $\beta_1 = \gamma_1$ and $\beta_2 = \gamma_2$ based on equation D.2 for different possible break points c between age 35 and 39.5.

E. Alternative identification strategy

As a validation exercise, we use changes in the Austrian early retirement age (ERA) as an alternative source of variation in outside options. In this Appendix we describe the institutional setting and the two pension reforms we consider in more detail. We draw heavily from Staubli & Zweimüller (2013) and Manoli & Weber (2016), who study employment effects and actual pension takeup of the ERA reforms.

E.1. The pension system

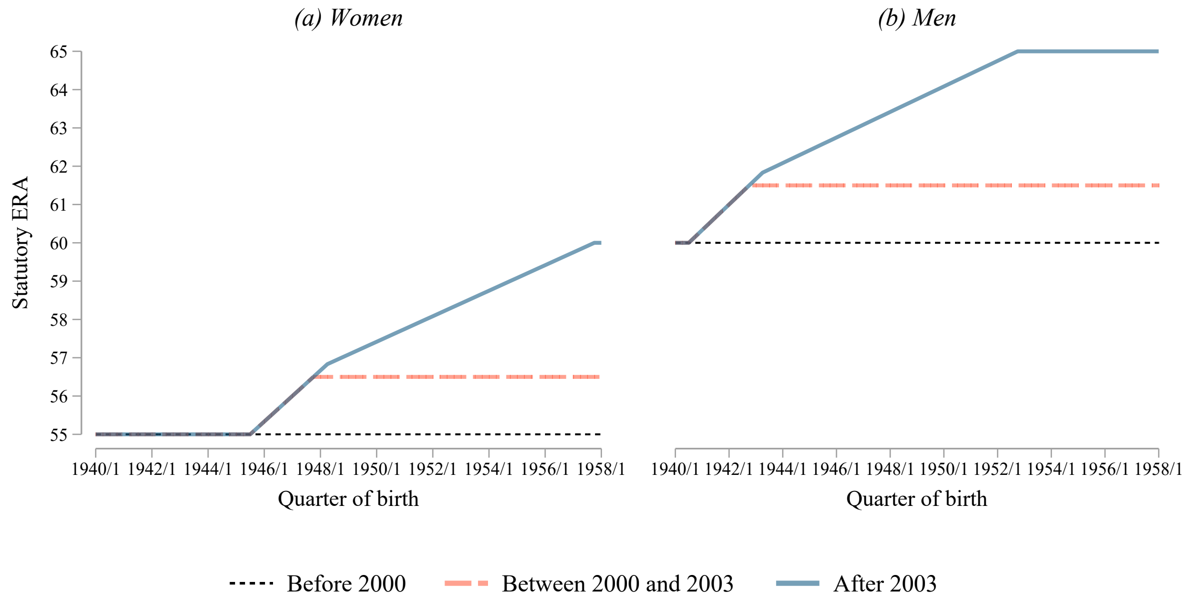
The Austrian public pension system is universal and private-sector workers are automatically enrolled. Financing is based on a pay-as-you-go scheme, with payroll taxes being withheld from the worker's salary up to a contribution cap. The system covers three types of pension: regular old age pension, early retirement, and disability pension. Currently, the statutory retirement age is 65 years for men and 60 years for women. Until 2000, early retirement was possible from age 60 for men and age 55 for women, provided they had worked for at least 35 years or if they had been long-term unemployed but had worked for at least 15 years in total. Pension benefits are assessed based on pre-retirement wages multiplied with a 'pension coefficient,' which is a factor of up to 0.8 that increases with work experience. The penalty for retiring early amounts to around 2 percentage points per year of retiring before the regular retirement age.

E.2. The pension reforms

For the purpose of fiscal consolidation, the Austrian government enacted two pension reforms in 2000 and 2003 that increased the ERA gradually by quarter of birth cohort. Figure E.1 provides a graphical representation. The 2000 reform, which became effective on October 1, 2000, increased the ERA by 1.5 years in two-month steps for every quarter of birth for both men and women. The first affected cohort for men was 1940/q4, where the ERA was increased to 60 years and 2 months, and 1945/q4 for women, where the ERA was increased to 55 years and 2 months. The last affected cohorts were 1942/q4 (men) and 1947/q4 (women), for whom the ERA was increased to 61.5 (men) and 56.5 (women). The statutory ERAs by cohort after 2000 are represented by the red dashed line in Figure E.1.

The second reform was enacted in 2003 and became effective on January 1, 2004, effectively abolished early retirement by increasing the ERA to the regular old age pension age (65 for men and 60 for women). Again, the ERA increase was phased-in based on quarter of birth cohorts. For men, the ERA was raised in two-month steps for those born in 1943/q1 and 1943/q2 and then in one-month steps for those born between 1943/q3 and 1952/q4. Similarly, for women, the ERA was

Figure E.1 — Statutory ERAs over time, by quarter of birth, and by gender



Notes: This figure depicts the statutory ERAs by quarter of birth and for both genders. The black dashed line represents the initial situation where the ERA was 55 for female workers and 60 for male workers. The red dashed line represents the situation between after the first reform in 2000 and before the second reform became effective in 2004. The blue line represents the current ERAs that are in effect since January 1, 2004.

increased in two-month steps for those born in 1948/q1 and 1948/q2 and in one-month steps for those born between 1948/q3 and 1957/q4. This is depicted as the blue line in Figure E.1.

F. Theoretical Robustness

Our predictions are based on a model of a relational contract which we assume is optimally designed for an individual agent. Here, we argue that the specific interpretation of the model is not important for our results. In fact, it only matters that the future relationship value determines today's actions.

F.1. Fixed wages and a standard efficiency wage model

Although a worker's compensation contains many components besides monetary payments, one might argue that our model allows for too much flexibility in determining individual compensation when our objective is to capture the situation in Austria. As described above, the Austrian labor market is characterized by centrally bargained wages and working conditions, and on top by weak job protection. In the following, we therefore show that our results do not rely on the principal's ability to tailor compensation systems to individual workers, but can also be generalized in a more constrained setting. Suppose wages are exogenously given and incentives are solely provided by firing threats upon non-performance. We thus rule out the use of an informal performance-based bonus. Such a setting resembles classic models of efficiency wages which generally are relational contracting models with restrictions on the forms of compensation (see MacLeod & Malcomson 2023). With a given compensation, the only individualized aspect of the employment relationship is the agent's effort. Thus, assume that the agent is supposed to exert effort e_t^*. If he complies, he remains employed, otherwise he is fired at the end of the period. We focus on an equilibrium in which the agent remains employed on the path of play—which implicitly requires the wage to be high enough to satisfy the agent's participation constraint and low enough to satisfy the principal's participation constraint—hence his utility in a period t is

$$U_t = w - c(e_t^*) + \delta U_{t+1}.$$

Equilibrium effort is constrained by his (IC) constraint,

$$-c(e_t^*) + \delta U_{t+1} \geq \delta \bar{U}_{t+1}, \tag{IC}$$

where $\bar{U}_{t+1}$ is defined as in our main model. Now, equilibrium effort and comparative statics are not determined by the total future surplus, but by the agent's continuation payoff. It is immediate that $\delta\left(U_{t+1} - \bar{U}_{t+1}\right)$ increases in w and δ (given $U_\tau - \bar{U}_\tau > 0 \, \forall \, \tau$) and decreases in $\bar{U}_{t+1}$. If this continuation rent is large enough, $e_t = e^{FB}$, otherwise, e_t^* is determined by the binding (IC). Equivalently to the proof to Proposition 1, we can show that e_t^* increases in $\delta\left(U_{t+1} - \bar{U}_{t+1}\right)$ if (IC) binds and otherwise does not respond to it. Moreover, the effect of a higher continuation rent on effort is more pronounced if this rent has initially been smaller. If we also suppose that $\delta\left(U_{t+1}-\bar{U}_{t+1}\right)$ goes down as time passes, this setting can as well generate Predictions 1–5. If a higher inherent productivity θ and consequently a higher relationship rent also increases w_t, we can furthermore generate Prediction 6. Therefore, treating wages as exogenously given and providing incentives only via firing threats does not change our predictions. The reason is that the main mechanism, that workers are motivated by future rents from employment, still drives the results.

F.2. Effort is private information

In this subsection, we demonstrate that our results do not rely on the principal being able to observe the agent's effort. Suppose effort is the agent's private information, and the principal can observe a non-verifiable output measure $y_t = \{0, \theta\}$, where $y_t = \theta$ with probability e_t and $y_t = 0$ with probability $1 - e_t$. Moreover, we assume effort is between 0 and 1 and θ sufficiently small to always guarantee an interior solution. Now, the agent is incentivized if his payoffs are larger after a high than after a low output. As in our main model, such a reward can either take the form of a bonus paid at the end of a period or higher payments in the future. Here, we assume that only future payoffs are used for that purpose, thus no bonuses are paid but only wages. This assumption is without loss of generality because any bonus paid after a high output provides the same incentives as an equivalent increase of next period's wage (multiplied by δ). We do so to not have to deal with potentially negative bonuses which we would otherwise have to consider after a low output (and consequently a (DE) constraint for the agent). In the following we use the superscript "+" to indicate continuation payoffs after a success, and "−" for continuation payoffs after a failure. Therefore,

$$\begin{aligned}U_t &= w_t + e_t\delta U^+_{t+1} + (1-e_t)\delta U^-_{t+1} - c(e_t)\\ \Pi_t &= e_t\left(\theta + \delta\Pi^+_{t+1}\right) + (1-e_t)\delta\Pi^-_{t+1} - w_t.\end{aligned}$$

Now, the following constraints must be satisfied by a self-enforcing relational contract:

$$-c'(e_t) + \delta\left(U^+_{t+1} - U^-_{t+1}\right) = 0 \tag{IC}$$

$$\Pi^+_{t+1}, \Pi^-_{t+1} \geq \bar{\Pi} \tag{PCP}$$

$$U^+_{t+1}, U^-_{t+1} \geq \bar{U}_{t+1}, \tag{PCA}$$

where (PCP) and (PCA) indicate the principal's and the agent's participation constraints, respectively, and effort in (IC) is determined by the agent's first-order condition.

As Levin (2003) has demonstrated, we can again separate the provision of incentives from the allocation of the resulting surplus. Therefore, we once more focus on maximizing the relationship surplus at the beginning of the relationship. Moreover, this problem is sequentially efficient, hence maximizing the initial surplus is equivalent to maximizing the surplus in every period t. This also implies that no firing threats are used to provide incentives, and equilibrium effort and incentives in a period are independent of whether a success or failure has been previously observed. Finally, it is without loss to let the period-t wage constitute the only difference between U_t^+ and U_t^-, and Π_t^+ and Π_t^-. Thus, $U_t^+ > U_t^-$ and $\Pi_t^+ < \Pi_t^-$, as well as $U_t^+ + \Pi_t^+ = U_t^- + \Pi_t^- \equiv S_t$, and the problem becomes to maximize S_t in every period t, subject to

$$-c'(e_t) + \delta\left(U_{t+1}^+ - U_{t+1}^-\right) = 0 \tag{IC}$$

$$\Pi_{t+1}^+ \geq \bar{\Pi} \tag{PCP}$$

$$U_{t+1}^- \geq \bar{U}_{t+1}. \tag{PCA}$$

Multiplying both sides of (PCP) and (PCA) with δ and adding all constraints yields the enforceability constraint

$$-c'(e_t) + \delta\left(S_{t+1} - \bar{S}_{t+1}\right) \geq 0, \tag{EC}$$

where $\bar{S}_{t+1} = \bar{\Pi} + \bar{U}_{t+1}$.

Levin (2003) shows that, as long as each player at least gets their outside option, this constraint is necessary and sufficient for obtaining equilibrium effort e_t. Therefore, $e_t = e^{FB}$ if it satisfies this condition, otherwise e_t is determined by the binding (EC) constraint. In the latter case, e_t increases in the future net surplus $\delta\left(S_{t+1} - \bar{S}_{t+1}\right)$, thus in δ and θ, and decreases in $\bar{S}_{t+1}$. Hence, Predictions 1 and 3 would also be generated in a setting with private effort. For our other predictions, it can immediately be shown that the positive effect of the future net surplus is more pronounced for an initially smaller surplus if and only if $c''' > 0$.

The same holds if, as in Section F.1, the principal cannot tailor compensation to an individual employment relationship and is not able to use performance-based compensation. Then, firing threats are the only means to provide incentives. Here, we assume that the principal fires the agent after a low output with some probability $1 - \alpha \in [0, 1]$. For simplicity, we take α as given; if α was set to maximize profits, its level would not be stationary and potentially depend on the whole history of the game (Fong & Li 2017). Such an analysis is beyond the scope of this paper, though, and would not affect our results qualitatively. The agent's utility in such a setup is

$$U_t = w_t - c(e_t) + \delta\left[e_t U_{t+1} + (1 - e_t)\left(\alpha U_{t+1} + (1 - \alpha)\bar{U}_{t+1}\right)\right], \tag{F.1}$$

hence effort is characterized by

$$c'(e_t) = \delta (1 - \alpha) \left(U_{t+1} - \bar{U}_{t+1}\right).$$

It follows that, for $c''' > 0$, comparative statics are as in Section F.1.