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DISCUSSION PAPER SERIES

IZA DP No. 16509

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ABSTRACT

Unemployment Insurance with Response Heterogeneity*

The generosity of social insurance coverage often increases with the beneficiary's age and their contribution time to social security, but existing policies vary considerably. We study the differentiation of unemployment insurance (UI) generosity by evaluating how the insurance-incentive trade-off varies with age and contribution time. We exploit numerous discontinuities in potential benefit duration in Germany. Contribution time in the last three years carries information on job search efforts, as it is associated with lower moral hazard responses and fiscal externality. We find no significant response heterogeneity in age or longer contribution time horizons. Contrasting these gradients with an approximated insurance value for four UI regimes, we document that steepening the potential benefit duration schedule in contribution time and flattening it in age would have increased welfare.

JEL Classification: J08, J64, J65

Keywords: unemployment insurance, response heterogeneity, policy differentiation

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1 Introduction

Adjusting the generosity of social insurance based on observable characteristics is a widespread feature of modern welfare states. In theory, welfare can be increased if the incentive costs or insurance value is heterogeneous, and coverage can be differentiated in the relevant dimensions (Spinnewijn, 2020). In the context of unemployment insurance (UI), coverage typically increases with age and past contributions to social insurance over a given time horizon. The rationale is to discourage recurrent unemployment spells, and provide extended coverage to older workers facing higher unemployment risk. There is substantial variation in the degree of differentiation across countries and over time. In Germany, the setting for our study, the potential benefit duration (PBD) increases stepwise with contribution time at job loss, and the age-specific maximum jumps up at given age cutoffs. Meanwhile in, e.g., the United States, Switzerland and Austria, PBD jumps only at eligibility or at few cutoffs. UI systems also differ in the horizons over which contribution requirements have to be fulfilled.[1] This speaks to a lack of consensus and evidence on how to differentiate UI generosity in age and past contributions. This question is increasingly relevant in light of the rising share of workers with non-standard employment and gaps in mandatory contributions, as well as the aging of the workforce and policies increasing the statutory retirement age.

This paper studies how unemployed workers' responses to UI depend on both age and contribution time within specific horizons, and explores implications for UI design. We provide new evidence of heterogeneity in responses and in the resulting fiscal externality of UI. To this end, we develop a multidimensional regression discontinuity (RD) design that leverages numerous cutoffs in PBD in Germany, as well as changes in these cutoffs over time. Using social security data spanning from 1994 to 2016, our approach yields a comprehensive set of elasticity estimates within a homogeneous institutional setting. We use these to evaluate the fiscal externality and weigh it against the insurance value of UI in the age-by-contribution time space. This enables us to evaluate the optimality of different existing PBD schedules in Germany, and to uncover opportunities for enhancing welfare through changes in UI policy differentiation.

Age and contribution time may be relevant for tagging UI generosity if they carry information on heterogeneity in the value and costs of UI. They are observable and correlate with multiple key determinants of unobserved job search behavior, e.g., human capital, returns to working, labor market attachment, assets and individual preferences. Age determines the time horizon until retirement, and thereby the returns to working (Hairault et al., 2010, 2012; Chéron et al., 2013; Michelacci and Ruffo, 2015). Younger workers have an incentive to build up their human capital and labor market experience

[1] Differentiated systems also exist in, e.g., France, Italy, Spain, and Belgium. Countries that use short contribution time horizons for eligibility and PBD determination include, e.g., the United States with typically 52 weeks, Switzerland with 2 years, and France with 2 to 3 years. In Spain, the relevant time horizon is 6 years.

to improve their long-run employment and earnings prospects. Older workers are closer to retirement and encounter more difficulties in finding jobs. Contribution time measures labor market attachment and human capital. Workers with stable employment tend to have higher returns to working. Notably, contribution time is determined by past UI utilization and possibly by preferences for working (Hopenhayn and Nicolini, 2009). Both characteristics are also related to the value of alternative social insurance programs, such as the option to transition into early retirement through UI (Sander and van Ours, 2010; Baguelin and Remillon, 2014; Inderbitzin et al., 2016; Gudgeon et al., 2023; Ye, 2022). Additionally, the ability to smooth consumption throughout unemployment changes over the life cycle and with past employment stability due to accumulated assets (Michelacci and Ruffo, 2015).

We provide a framework to assess the heterogeneity in the welfare effects of PBD extensions by extending the directed-search model in Nekoei and Weber (2017). This model follows the tradition of Baily (1978) and Chetty (2006) to evaluate the local optimality of UI by weighing the fiscal externality of UI against its insurance value at the margin. Crucially for us, the change in the total welfare effect allows evaluating the degree of differentiation of the existing policy in age and contribution time. The fiscal externality measures by how much income taxes on the employed need to be increased to finance a PBD extension. PBD extensions mechanically increase transfers, but also make working less attractive relative to staying unemployed, leading to a longer time to reemployment. While PBD extensions may help workers find better jobs, they may also drive human capital depreciation, leading to ambiguous effects on future wages and fiscal revenue. The fiscal externality combines these effects as measured by the reduced-form elasticities of nonemployment duration, UI receipt duration, and wages at reemployment. It can be estimated without fully specifying the primitives determining job search behavior, and the heterogeneity therein.

Our multidimensional RD design produces causal estimates of the elasticities across over 400 cells of workers defined by age and contribution time over various horizons. It exploits the stepwise increases in PBD as a function of both these characteristics in the German UI system. These cutoffs were further subject to several reforms within our observation period, which we exploit to isolate different sources of heterogeneity. We subject our estimates to extensive validity checks to flag those that show no evidence of sorting. Germany provides a compelling setting for this analysis, as its UI system is similar in structure to many OECD countries (Schmieder and Von Wachter, 2016; Spinnewijn, 2020). Workers who satisfy eligibility criteria (minimum contribution to social security, and reason for job loss) receive constant benefit payments determined by an income replacement rate for a maximum PBD.

Consistent with the existing literature, our estimates of duration elasticities are positive on average, while the wage elasticity estimates are noisier and ambiguous in sign

(Tatsiramos and van Ours, 2014; Schmieder and Von Wachter, 2016). The adjusted fiscal externality equals 0.66 for the average worker in our sample. That is, if the social planner increased PBD by 1 percent for the average worker, they would have to increase income taxes on the employed by 0.66 percent in order to break even.

We document substantial heterogeneity in duration responses to UI using meta regressions. To isolate heterogeneity in age and contribution time, these regressions hold constant the PBD level, i.e., the actual policy differentiation, as well as the composition of the cells of workers underlying each RD estimate. The elasticity of nonemployment duration (i.e., the time from first to last job) is significantly negatively associated with contribution time in the 3 years before job loss, holding constant age, the PBD level and observable characteristics. Workers with stable short-term employment exhibit lower moral hazard, i.e., smaller reductions in job search effort in response to PBD extensions. The elasticity of UI benefit receipt (i.e., the time covered by payments) is also negatively associated with short-term contribution time. Longer contribution time horizons of 5 and 7 years, as well as age have little predictive power for responses to UI within our sample aged 40 to 55. As for wage elasticities, we find no meaningful heterogeneity as the underlying point estimates are imprecise and small in magnitude, and thus not a key driver of heterogeneity in the fiscal externality.

Combining these components, the fiscal externality significantly decreases with contribution time in the last 3 years. PBD extensions induce substantially smaller costs among workers with stable recent employment. The fiscal externality is positively but nonsignificantly associated with age and longer contribution time horizons. The adjusted fiscal externality for prime-age workers with stable employment equals 0.4, and is less than half of the one in the oldest group with unstable employment at 0.87. These gradients are robust to a battery of checks related to sample construction, as well as RD and meta regression estimation.

The last step of our analysis evaluates the degree of differentiation in the four PBD schedules that existed in our observation period by computing the total welfare effect of UI. That is, the gradient in the fiscal externality is compared to the one in an approximated insurance value. The insurance value requires structure on individual preferences, as it captures the welfare benefits of UI from smoothing consumption between employed and unemployed states. We find that, for a conventional, homogeneous coefficient of relative risk aversion of 2, the insurance value increases in contribution time and slightly in age within our sample. Under the regimes analyzed in our setting, welfare could have been increased by steepening the PBD schedule in contribution time over a shorter time horizon, and by flattening it in age.

Our results yield several key insights for UI design. First, short-term contribution time is the most relevant tag to differentiate PBD. It has a sizable influence on the tax burden of UI extensions, and appears important to discriminate recurrent users of UI, much more so than longer time horizons. Second, age and short-term contribution capture different determinants of the value and costs of UI, and should be used separately for tagging PBD. Third, our evaluation of existing PBD schedules suggests that a welfare-enhancing policy change would have been to increase the PBD in short-term contribution time, while flattening the age profile for workers between 40 and 55 years old. Fourth, the heterogeneity we find highlights that local estimates of the welfare effects of UI cannot be readily extrapolated, especially without accounting for differences in employment histories.

Related literature. This paper adds to the broad literature analyzing the reduced-form effect of UI on unemployment duration and job quality at reemployment. Of particular relevance here are studies analyzing the effects of PBD extensions using RD designs at thresholds in age and contribution time (or tenure).[2] A consistent finding is that PBD extensions induce positive duration responses, but evidence regarding job quality is mixed, with small effects on wages at reemployment, if any. Differences in the magnitude of estimates across studies have proved difficult to reconcile given the discrepancies in institutional settings, populations and methodologies. Our results suggest that these local estimates may mask substantial heterogeneity in responses (even at given cutoffs), and may not be extrapolated without accounting for differences in age and contribution time. Our study thereby helps explain some of the heterogeneity across existing estimates.

Heterogeneity in age has previously been analyzed by Schmieder et al. (2012), who estimate nonemployment duration elasticities at three age cutoffs (42, 44 and 49 years) in Germany. Consistent with our findings, the authors find no gradient in age, but focus on workers with uninterrupted long-term employment, i.e., at least 5 years over the last 7, similarly to other studies (see Appendix A). Meanwhile, little is known on the behavior of workers with short contribution times or tenure, with the exception of Le Barbanchon (2016). The author uses a large PBD extension at 8 months of tenure in the last 12 months. He finds no evidence of an effect on job quality, but a large and statistically significant effect on nonemployment duration. Our novel multidimensional design provides a comprehensive set of elasticity estimates within a homogeneous setting. This yields new evidence on how UI responses of workers with unstable employment (who constitute a substantial share of inflows into UI) compare to those of the long-term employed. Our results suggest that existing studies focusing on the long-term employed may have underestimated the cost of UI.

We also add to the literature on optimal UI design. Based on job search models, this literature derives formulas to assess the local optimality of UI using reduced-form elastiti-

[2] See Appendix A for an overview of the key studies using RD designs, e.g., Card et al. (2007a); Lalive (2008); Schmieder et al. (2012); Caliendo et al. (2013); Le Barbanchon (2016); Schmieder et al. (2016); Nekoei and Weber (2017) and Johnston and Mas (2018). Other recent studies using alternative identification strategies include, e.g., Lalive et al. (2006); van Ours and Vodopivec (2008); Card et al. (2015); Cottier et al. (2020) and Lichter and Schiprowski (2021). See Tatsiramos and van Ours (2014) and Schmieder and Von Wachter (2016) for reviews of the literature on UI effects.

cies as sufficient statistics (Baily, 1978; Chetty, 2006). However, it has long set aside heterogeneity in responses, and the possible welfare effects from differentiating UI across the unemployed (Spinnewijn, 2020). We add to several recent papers that have considered the welfare implications of other dimensions of response heterogeneity, e.g., Kolsrud et al. (2018) and Lindner and Reizer (2020) over the unemployment spell, Schmieder et al. (2012), Kroft et al. (2016) and Landais et al. (2018) over the business cycle.[3] The study closest to ours is Michelacci and Ruffo (2015). It formulates a structural model of optimal UI over the life cycle with nonlinear accumulation of assets and human capital. Using state-level variation in benefit levels in the United States, the authors find duration elasticities close to zero for workers younger than 40, but larger and significant ones for older workers (i.e., as our sample). A key insight from this paper is that younger workers value UI more as they have fewer assets to smooth consumption throughout job loss. The authors conclude that welfare would increase if UI replacement rates decreased steeply with age from a relatively high value until roughly age 40, and then stayed constant at a relatively low level for older workers. In the economy modeled by Michelacci and Ruffo (2015), all periods of unemployment are covered by UI. Our paper complements this work by allowing for incomplete UI coverage, and studying whether welfare could be improved by differentiating the PBD—another key policy parameter. We also shed light on response heterogeneity in both age and contribution time, and on the relevance of these widely-used dimensions for tagging benefits. Our paper thereby contributes to the broader debate on policy differentiation in taxation and social insurance (e.g., Fahri and Werning, 2013; Stantcheva, 2017; Ferey, 2022). Our results also align with Hopenhayn and Nicolini (2009), who show that conditioning UI benefits on past employment is optimal if the planner cannot distinguish quits and layoffs.

The paper proceeds as follows. Section 2 introduces the theoretical framework for our analysis. Section 3 describes the German UI system and social security data. Section 4 presents the RD design used to obtain estimates of heterogeneous UI responses, and resulting estimates. Section 5 relates the estimates to age and contribution time in a meta regression. Section 6 evaluates the welfare trade-off from the observed heterogeneity. Section 7 draws policy implications from our results, and Section 8 concludes.

2 Theoretical Framework

In this section, we set up a partial-equilibrium model of job search and characterize the optimal UI policy. We extend the directed-search model from Nekoei and Weber (2017) to allow for worker heterogeneity in age a and relevant contribution history h at job loss, and differentiation in the PBD, $B(a, h)$.[4] The welfare effects of UI can be expressed in

[3]See also Kroft and Notowidigdo (2016), Farber and Valletta (2015), Chodorow-Reich and Karabarbounis (2016) and Hagedorn et al. (2017) on UI generosity in the United States.

[4]Given our policy variation, we focus on the PBD and take the constant replacement rate as optimal across groups. Michelacci and Ruffo (2015) find that the optimal replacement rate is roughly constant

terms of sufficient statistics that capture the trade-off between the insurance value and incentive costs.[5] Such a characterization is particularly useful to assess a differentiated policy, as it allows going beyond the primitives shaping the environment and individual behavior, and focusing on relevant, higher-level—and possibly heterogeneous—moments that can be estimated empirically. We describe the main components below, and present further details and proofs in Appendix B. See also Nekoei and Weber (2017) for a detailed discussion.

Setup. Workers and the UI policy differ in (a, h). However, since the structure of the problem does not vary with (a, h) (see discussion below), we suppress this differentiation to ease notation. Keep in mind, though, that all elements of the model, including the PBD and the underlying primitives and environment, may depend on (a, h). Such heterogeneity then affects job search behavior and the resulting welfare trade-offs. We will emphasize this heterogeneity whenever relevant.

Workers choose their job search effort s and target job quality $V(w)$ (measured by the wage w) at every time t while being unemployed. These choices determine their job finding rate $\lambda = E(s, V(w))$ via a matching function E. Workers maximize their utility taking the unemployment policy as given. As in the German UI system, the maximum PBD $B(a, h)$ is a stepwise function of a and h—the policy of interest for our analysis. Until the point of exhaustion, the unemployed receive benefits $b = \rho(1-\tau)w$, where ρ is the UI replacement rate, and $(1-\tau)w$ are net-of-tax past earnings. The benefit level is not explicitly differentiated in the two dimensions, but varies with age and employment stability via w. Upon exhausting UI, the replacement rate falls and the worker draws unemployment assistance (UA) benefits $b_0 = \rho_0(1-\tau)w$. Upon finding a job, they pay a proportional income tax τ on their reemployment wage w^e, which is used to finance UI. They remain employed until the end of their working life in T.

Workers have time-separable preferences and live hand-to-mouth. The latter assumption is motivated by our empirical variation and the age range in our sample between 40 and 55. For this age group, Michelacci and Ruffo (2015) show that liquidity effects of UI are negligible and that responses to UI do not differ by wealth (which varies with age and employment stability). Assets only affect the insurance value but not the fiscal externality of UI in our model. We discuss the role of assets further in Section 6. The value function at the beginning of unemployment $(t = 0)$ is:

$$U(0) = \max_{\substack{w,s,\lambda \\ \lambda = E(s,V(w))}} \lambda V((1-\tau)w) + (1-\lambda)(u(b) + \beta U(1)) - s \tag{1}$$

where $V(\cdot)$ is the value of employment at the target job, $u(\cdot)$ is the flow utility of con-

above age 40.

[5]The Baily-Chetty formula (Baily, 1978; Chetty, 2006) is nested as a special case under the assumption of stationarity and no wage effect of UI.

sumption, β is the discount factor, and s is the utility cost of search.

We assume that the PBD for a group (a, h) does not affect the behavior of workers in other groups, nor the composition of inflows into UI beyond observable differences that we control for. In other words, workers only respond to the PBD they are eligible for given their age and contribution time, and cross-elasticities of responses between worker groups equal zero. This within-group perspective is dictated by our source of variation and RD design, which relies on workers having no precise control over the timing of their layoff around discontinuities in the PBD schedule. We thereby consider age and contribution time at entry into UI as immutable characteristics, both locally around given cutoffs and over longer horizons where labor supply decisions determine unemployment risk.[6]

This model has several key advantages for our analysis. First, it fits with our institutional setting and sources of variation given the similarities between the German UI system and the Austrian one, i.e., the empirical setting in Nekoei and Weber (2017). Second, this framework makes explicit the variables through which heterogeneity might occur, while remaining agnostic about deeper primitives. Identifying the factors that drive heterogeneity in the welfare derivative in age and contribution time is challenging, both from a theoretical and an empirical perspective. As outlined above, workers of different ages and employment histories may differ, e.g., in their job opportunities, job search behavior, preferences and characteristics. Third, the model allows for duration dependence both structurally, via a decline in the likelihood of finding a job, as well as institutionally via a decrease in the benefit level over the unemployment spell. Both sources create nonstationarity in job search behavior. They also introduce the possibility of ambiguous wage effects of UI extensions. Workers may become more selective by increasing their target wages, but compromise their job prospects because of human capital depreciation over longer nonemployment durations. This feature fits empirical observations, and is interesting for our application since we might expect heterogeneous wage effects in age and contribution time.

Fiscal externality of PBD extensions. The social planner chooses the benefit level and duration that maximize welfare, while balancing its budget.

$$\max_{B,b} U(0) \equiv W(B, b) \quad \text{s.t.} \quad (T - n)\tau w^e = b\tilde{n} \tag{2}$$

Consider now a PBD extension for group (a, h). This affects the government's budget constraint through the workers' labor supply responses, as workers do not internalize

[6] This claim is generally supported by the literature on UI, which has broadly relied on RD designs to identify the effects of UI, and has found little evidence of local manipulation. We address the issue of local manipulation with the RD validity criteria. Broader selection effects require estimating spillover effects using independent variation across different parts of the PBD schedule (see Spinnewijn, 2020 for a discussion). Our setup also naturally focuses on the intensive-margin (i.e., the transition from unemployment into employment) rather than the extensive-margin effects of UI.

the fiscal externality that they create on the government's bugdet. To break even, the government has to adjust income taxes on the employed. The zero cross-elasticities imply that budgetary effects of PBD changes, and thus optimal policy choices, are contained within group. We thus abstract from redistribution via taxes across groups.[7] In this case, Appendix B shows that the fiscal externality of PBD extensions is measured by the within-group elasticity of the proportional income tax τ with respect to B:

$$\varepsilon_{\tau,B} = \varepsilon_{n,B}\frac{n}{T-n} + \varepsilon_{\tilde{n},B} - \varepsilon_{w^e,B} \tag{3}$$

where $\varepsilon_{Y,B} \equiv \frac{\partial \ln Y}{\partial \ln B}$ denotes the elasticity of Y with respect to B. The elasticity of nonemployment duration n (i.e., the time between the first and last job) w.r.t. PBD, $\varepsilon_{n,B}$ captures the reduction in fiscal revenue from workers engaging in moral hazard in response to longer coverage. The reduction in job search efforts lengthens the time to reemployment. This elasticity is rescaled by the ratio of the nonemployment duration to the remaining working time horizon after nonemployment. This ratio varies with (a, h), and possibly recurrent future unemployment (we discuss this further in Section 5.3). The elasticity of the duration of UI receipt $\tilde{n}$ (i.e., the time over which benefits are actually drawn) captures the mechanical increase in transfers to workers, as well as the behavioral cost from reduced job search efforts by the unemployed over the period of coverage. Finally, the wage elasticity measures wage effects on fiscal revenue at reemployment.

The fiscal externality is a function of moments that can be estimated in reduced form. In the empirical analysis, we estimate these elasticities across groups, and test whether they vary with (a, h). Importantly, this formula only relies on reduced-form effects of UI, and does not require assumptions on workers' utility and assets.

Optimal PBD. UI generates welfare benefits as workers value the transfers that smooth consumption between the employed and unemployed states. Assuming no discounting, the total welfare effect of PBD extensions for each group (a, h) can be written as (see proof in Appendix B)

$$\frac{dW}{dB}(a,h) = \frac{S(B)B}{\tilde{n}}\frac{u(b)-u(b_0)}{bu_c(b)}E\left[\frac{w}{w^e}\frac{u_c((1-\tau)w)}{u_c(b)}\right]^{-1} - \varepsilon_{\tau,B} \tag{4}$$

where $S(B) \equiv \Pr(n \geq B)$. The first term measures the insurance value from increasing B, rescaled by the share of workers still unemployed at benefit exhaustion. All elements of (4) may depend on (a, h). This formula lays out a framework to locally evaluate the existing PBD within each worker group (a, h). An optimal (differentiated) policy maximizes social welfare, i.e., equalizes the marginal value and cost of UI, within each group. If the welfare derivative $\frac{dW}{dB}(a,h) > 0$, $B(a,h)$ is too small, and if $\frac{dW}{dB}(a,h) < 0$, $B(a,h)$ is too large

[7]The welfare effects of age-dependent tax schedules for financing UI are explored by Michelacci and Ruffo (2015). In light of our results, it would be useful for future research to explore contribution-dependent schedules.

(under concavity of the welfare function).

Crucially, differences in the welfare derivative across groups are sufficient to identify gains from marginally altering the existing PBD schedule (Michelacci and Ruffo, 2015; Spinnewijn, 2020). If $\frac{dW}{dB}(a,h) > \frac{dW}{dB}(a',h)$, a marginal PBD extension for group (a,h) and reduction for group (a',h) would improve welfare. This means that, relative to the current schedule, PBD should be more differentiated between these groups if $B(a,h) \geq B(a',h)$, and less differentiated if $B(a,h) < B(a',h)$. The goal of our empirical analysis is to test for such differences.

3 Institutional Setting and Data

3.1 Unemployment Insurance in Germany

In Germany, laid off workers are eligible for unemployment insurance (UI) benefits, provided they have contributed to the social security system for at least 12 months over a given eligibility time horizon before job loss.[8] Eligible workers receive flat UI benefits determined by a constant income replacement rate of 60% for workers without and 67% with dependent children, respectively, for a maximum PBD.[9] Our study period starts in January 1994 because the replacement rate has been unchanged since then. This allows us to focus on changes in the PBD.

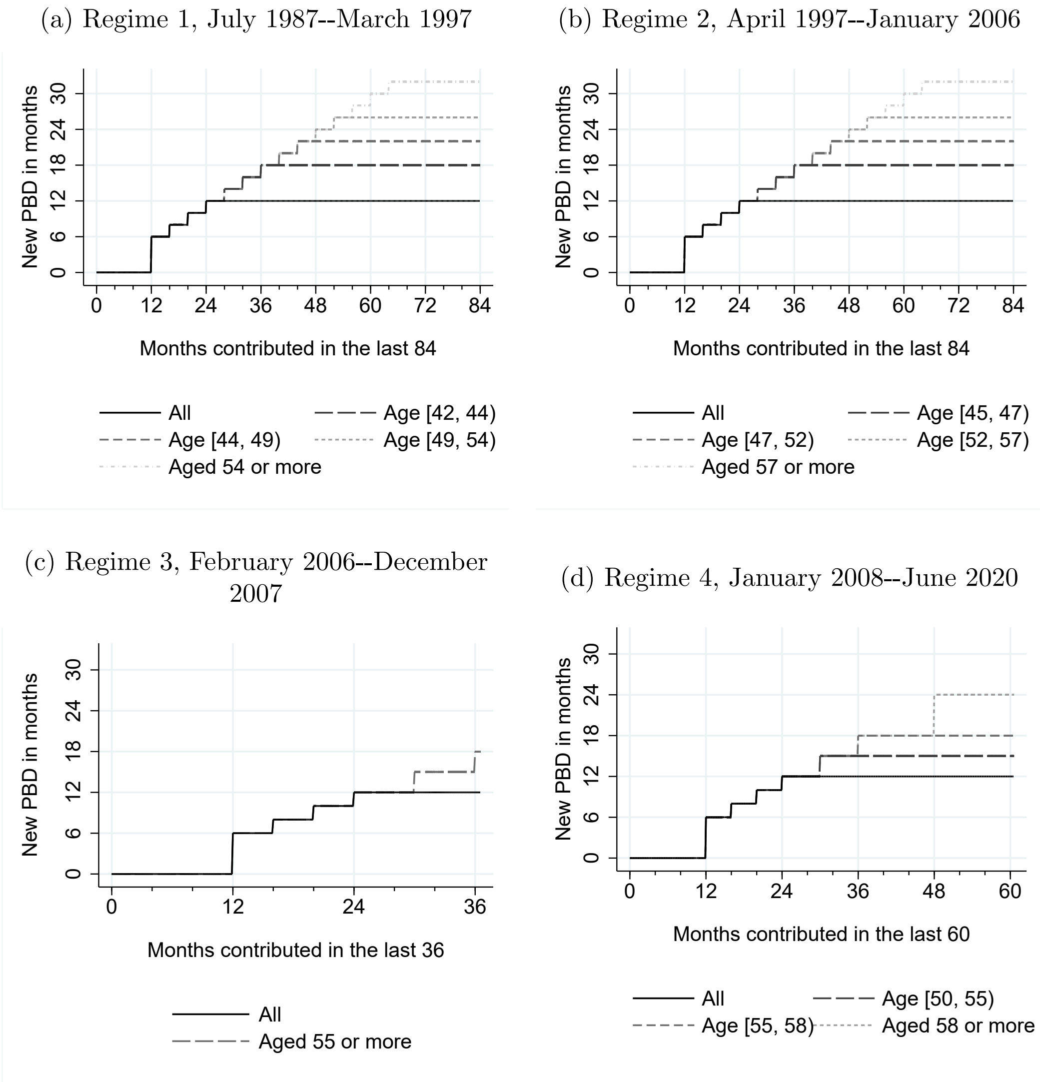
For each new UI claim, the new PBD is a stepwise function of the worker's age at entry into UI, and their contribution time over a given horizon. The total PBD at the beginning of the spell includes any leftover claims from past unemployment spells, up to the age-specific maximum. Four different PBD schedules applied during our study period, as shown in Figure 1. In regime 1 (Panel a), the PBD ranged from 6 to 32 months. It increased by 2 months for every additional 4 months of contribution between the minimum requirement for UI eligibility and the age-specific maximum. Crossing age thresholds allowed workers to access higher durations on the PBD ladder. In regime 2 (Panel b), the age cutoffs were increased by three years. In February 2006 (Panel c), the differentiation between age groups was reduced. The maximum PBD decreased to 18 months for workers who worked the full 3 years before job loss, with only one age cutoff at 55. Finally, in January 2008 (Panel d), the schedule shifted to a middle ground, setting the maximum PBD of 24 months. Retroactive PBD extensions were introduced for ongoing claims. Appendix C.1 provides further details on eligibility conditions and PBD determination.

The regimes also altered the time horizons used to determine eligibility and the PBD

[8]Few exceptions apply to, e.g., seasonal workers. Voluntary quits are penalised with a waiting time of up to 3 months.

[9]Insured earnings and thus UI benefits are capped, such that effective replacement rates are lower for older individuals with longer contribution times since a higher share reach the cap. However, the cap affects less than 1% of our sample, so that we do not believe that it drives our results.

Figure 1: Potential Benefit Duration as a Function of Contribution Time and Age at Entry into Unemployment



Notes: The figures display the new PBD as a function of age in years and contribution time in months at entry into unemployment across regimes overlapping with our study period. Sources: *Arbeitsförderungsgesetz*, and *Sozialgesetzbuch III*.

(see Appendix Table C.1). The eligibility time horizon was set to 36 months until January 2006, and to 24 months afterwards. The horizon for determining the PBD was longer and varied across regimes. It ranged from 36 to 84 months, giving different importance to past and recent contribution histories. The four UI regimes were otherwise comparable.

Non-compliance with job search requirements or refusal to accept suitable jobs could result in PBD sanctions. At benefit exhaustion, individuals could go into means-tested UA until 2005. UA benefits were determined by a lower income replacement rate of 57% with and 53% without dependent children, respectively, and depended on household wealth. In January 2005, the Hartz IV reform introduced means-tested, flat-rate benefits independent of previous earnings.[10]

3.2 Social Security Data

We use social security records from the Integrated Employment Biographies (IEB) provided by the Institute of Employment Research (IAB). The data contain full records for a random sample of 10% of individuals who were ever in UI between January 1, 1994 and December 31, 2016. For each UI spell, the unemployment records contain the exact beginning and end dates of benefit payments, the PBD left at the end of the spell, and the amount of daily benefits. We observe a rich set of individual characteristics, e.g., age, gender, marital status, nationality, education, and region of residence.

Unemployment spells can be matched to the full day-to-day social security records dating back to 1985. This includes employment subject to social security, which represents about 84% of all employment (Price, 2019). The main exceptions are students, self-employed workers, and civil servants. We can hence recover contribution time, as well as labor market histories and outcomes of individuals along with job characteristics (e.g., daily wage, part-time job, industry, occupation, firm identifier).

Samples and running variables. We select unemployment spells that start in the years 1994 to 2010 to avoid censoring outcomes (see the discussion below). We delete duplicates, correct the spells for overlap by truncating the later spell, and regroup spells with end and start dates one day apart due to corrections or ex-post changes in benefits. The sample excludes spells with no previous employment at all, as well as spells with no regular employment nor contribution-relevant social security records since the last UI spell or in the last 3 years. We also exclude inflows occurring in transition periods between regimes, in particular spells starting within 2 months before any regime change, and spells that cross January 1, 2008, for which the PBD might have been prolonged retroactively.

As presented in Section 4, our RD design relies on cutoffs in age and contribution time. The two types of cutoffs are analyzed separately with their respective running variables, and require specific sample restrictions. We only consider spells of workers aged 40 to 55 at entry into UI. The lower bound aims to ensure common support in age and contribution time, since the lowest age cutoff in our study period is at 42 years of age and younger workers have mechanically shorter employment histories. The higher bound aims to avoid confounding incentives from early retirement via UI. These restrictions result in a sample of approximately 1.5 million spells. Appendix C contains further details on the construction of variables, and sample selection. We now describe the construction of the samples used for each type of cutoff.

[10] For more details, see Wunsch (2005) and Price (2019).

Age sample. This sample contains the spells used for estimation at age cutoffs. We observe the worker's exact age at entry into UI in years up to two decimal points (i.e., 3-day precision). We keep spells within a bandwidth of 2 years from the closest age cutoff, who satisfy the minimum contribution requirements to be eligible for the age-specific maximum above the cutoff. We keep those spells where the observed PBD equals the age-specific maximum (97.3% of spells). Finally, we exclude spells in a narrow window of 2 weeks around the cutoff to avoid manipulation issues (Schmieder et al., 2012).

Contribution sample. This sample includes spells relevant for the estimation at contribution time cutoffs. Contribution time is defined as the number of months (1 month equals 30 calendar days) during which the individual contributed to social security within the regime-specific time horizon before entry into UI. This variable is not directly observed, and has to be recovered based on past social security records. We describe our procedure in detail in Appendix C.2. Although labor market histories are observed with daily precision, there might be measurement error in our imputed measures of contribution time.[11] To limit potential bias therefrom, we select spells with a new PBD at given steps of the PBD schedule (e.g., 10, 12, or 14 months, as in Figure 1), rather than solely relying on imputed contribution time. This approach ensures that we only consider spells with a *true* contribution time within a narrow window of 4 months on both sides of each cutoff. The leftover PBD is reported reliably given that it directly defines the duration of benefit payments in the accounting system. It allows recovering the new PBD for spells before the age maximum (see Appendix C.1). This is determined by the UI system at the beginning of the spell based on the relevant inputs. Focusing on the regular steps of the PBD schedule allows us to keep cases subject to standard UI rules and labor supply incentives. We exclude spells with no new claim (i.e., a PBD equal to the leftover, or smaller than 6 months), as well as those at the 6 and 8-month steps to avoid selection effects at the eligibility cutoff.[12] Finally, we also exclude spells at the age-specific maximum as we cannot distinguish new and leftover PBD, and can only bound the true contribution at the maximum time-horizon. In Section 4, we further discuss the advantages of our step-based approach in tackling the challenges to identification and estimation posed by measurement error.

Observable characteristics. Panel (a) of Table 1 presents summary statistics for ob-

[11]This might be due to involved legal provisions as to what types of labor market activity count towards contribution in the German UI system, as well as by incomplete or imprecise social security records, e.g., overlapping spells, unclassifiable gaps in the labor market history, unobserved penalties or sanctions for not accepting a suitable job, waiting times, misreporting of employment durations by firms (especially for short-term contracts), and special eligibility rules for seasonal workers. Our sample restrictions aim to eliminate the cases with more complex employment histories and non-standard incentives to use UI.

[12]E.g., Albanese et al. (2020) find a substantial increase in inflows into UI at the eligibility cutoff. Our data also feature a spike in inflows at 12 months of contribution, which is partly driven by temporary 1-year contracts. This spike also includes participants in active labor market programs that count towards social security contributions, but come with specific job search support and incentives.

servable characteristics. The pooled analysis sample (column 1) is on average of the same age as the age sample (column 2) and the contribution sample (column 3). The age sample is slightly less likely to be female, non-German, and to reside in East Germany. It also displays lower rates of UI use in the last 7 years, and longer leftover and beginning PBDs on average. By construction, the age sample has longer employment histories, as well as tenure in the last job. The differences across samples generally suggest that controlling for observable characteristics is important in relating responses to the running variables. Appendix Table C.3 presents further descriptive statistics, and compares our analysis sample to the initial sample of inflows. Despite our selection criteria, we believe that our sample is representative of inflows eligible for more than the minimum new PBD and to which standard UI incentives apply.

Labor market outcomes. The key inputs needed to compute the fiscal externality are the elasticity of UI receipt duration, nonemployment duration, and wages at reemployment. The duration of UI receipt corresponds to the number of days drawing benefits. Since workers can remain unemployed after their UI benefits exhaust (and their recorded UI spell is terminated), we define nonemployment duration as the time between start of UI benefit payment and the first regular employment spell with a positive wage (Card et al., 2007b). Following Schmieder et al. (2012), we cap this duration at 36 months for longer or censored spells. This cap is higher than the maximum PBD of 32 months in our sample. Our main results are robust to alternative choices of this cap. Wages at reemployment are measured by the difference in log monthly wages at the first reemployment job and the last pre-unemployment job. Taking the difference allows taking out the individual's initial wage level and measuring a percentage change.[13]

Panel (b) of Table 1 shows that the age sample displays a slighly lower average nonemployment duration than the contribution sample at around 17 and 18 months, respectively. These lengths are close to Schmieder et al. (2012) and Schmieder et al. (2016) that use the same data in Germany, but a more restrictive sample of workers with stable contribution histories. They are however longer than averages reported in settings with similar UI institutions and data (e.g., Card et al. 2007a for Austria), and shorter than those reported in the United States (e.g., Card et al. 2015; Johnston and Mas 2018). The share of spells with censored duration is around 31%. The age sample has lower rates of benefit exhaustion, partly due to longer PBDs, but a similar probability of finding a job within the observation period. The age sample has generally better reemployment outcomes in levels as measured by the monthly wage at reemployment, tenure in the first job, and cumulated earnings within 60 months after unemployment, but displays larger wage losses.

The heterogeneity in outcomes with respect to age and contribution time transpires

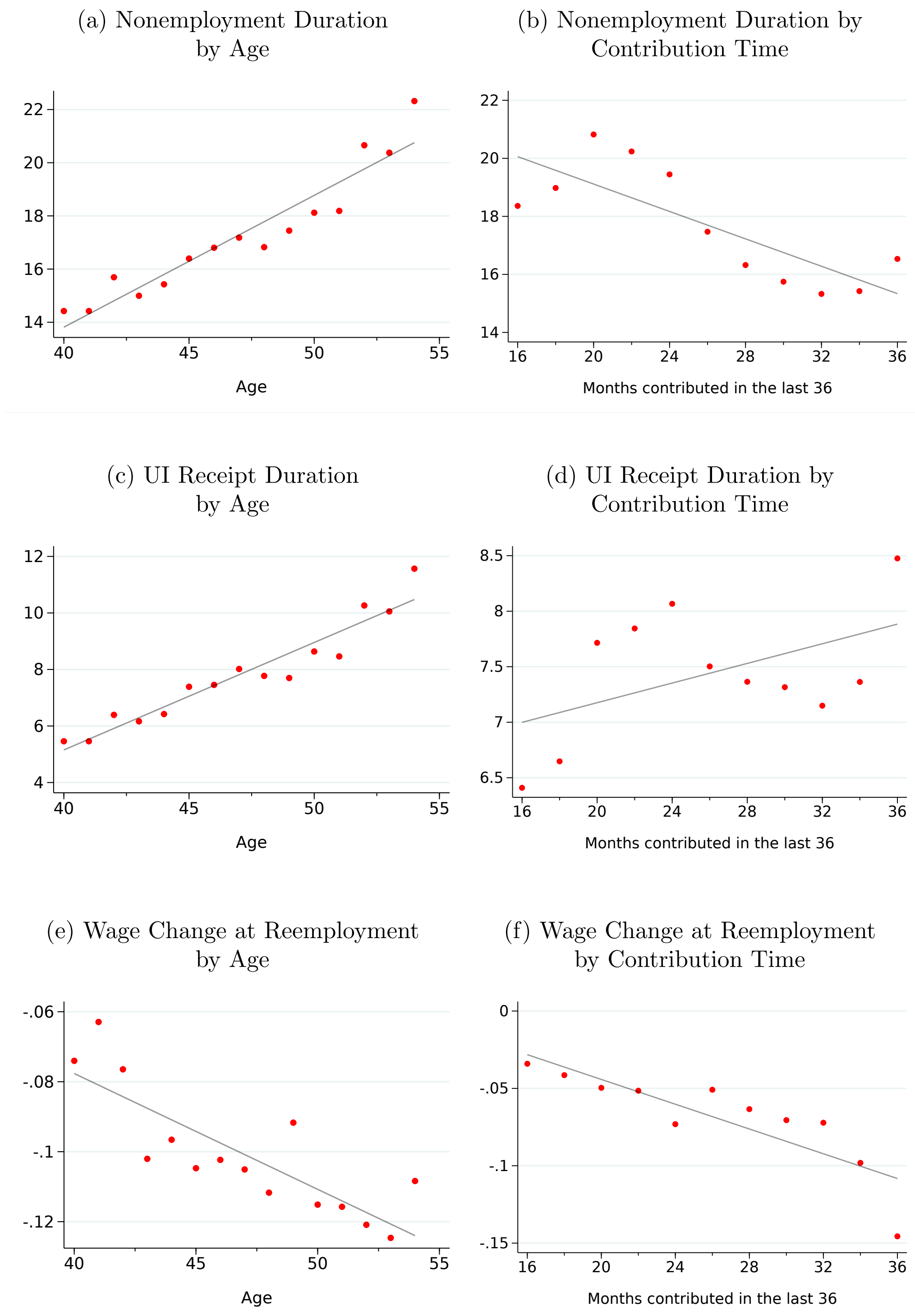
[13]Individuals with missing wage at reemployment within 6 years are excluded from analyses of wages at reemployment. We censor wages at the social security earnings cap.

Table 1: Summary Statistics for the Analysis Samples

	(1) Analysis sample		(2) Age sample		(3) Contribution sample	
Panel (a) Observable characteristics						
Age	47.77	(4.06)	47.74	(4.03)	47.84	(4.13)
Female	0.42	(0.49)	0.41	(0.49)	0.45	(0.50)
Non-German	0.08	(0.27)	0.07	(0.26)	0.10	(0.30)
Residence in East Germany	0.21	(0.40)	0.20	(0.40)	0.23	(0.42)
Residence missing	0.30	(0.46)	0.32	(0.47)	0.27	(0.44)
No secondary schooling (ref.)	0.88	(0.32)	0.88	(0.32)	0.88	(0.33)
Secondary schooling	0.08	(0.27)	0.08	(0.28)	0.08	(0.27)
Schooling missing	0.04	(0.19)	0.03	(0.18)	0.04	(0.20)
Vocational training (ref.)	0.71	(0.45)	0.73	(0.44)	0.68	(0.47)
Without vocational training	0.20	(0.40)	0.18	(0.39)	0.23	(0.42)
Academic degree	0.05	(0.23)	0.06	(0.23)	0.05	(0.22)
Degree missing	0.04	(0.19)	0.03	(0.18)	0.04	(0.20)
Last job part-time	0.16	(0.36)	0.14	(0.34)	0.20	(0.40)
Last job tenure	42.43	(46.89)	53.80	(51.82)	19.59	(20.97)
Contribution in last 36 mths	30.16	(7.58)	33.41	(4.50)	23.65	(8.27)
Contribution in last 84 mths	63.77	(21.97)	74.53	(11.95)	42.14	(21.57)
UI in last 84 mths	4.68	(6.94)	2.50	(4.59)	9.06	(8.59)
New PBD	11.42	(4.08)	.	(.)	11.42	(4.08)
Monthly UI benefits	948	(419)	1,008	(433)	826	(358)
Panel (b) Labor market outcomes						
Nonemployment dur. (capped at 36m)	17.34	(14.50)	17.08	(14.57)	17.85	(14.34)
Nonemployment dur. capped	0.31	(0.46)	0.31	(0.46)	0.32	(0.47)
UI receipt duration	7.89	(6.98)	8.29	(7.66)	7.09	(5.28)
Exhausted UI benefits	0.16	(0.37)	0.12	(0.33)	0.24	(0.43)
First job observed	0.85	(0.36)	0.84	(0.37)	0.86	(0.35)
Wage at reemployment	1,870	(934)	1,984	(957)	1,646	(845)
Log(First wage) - Log(Last wage)	-0.10	(0.53)	-0.13	(0.52)	-0.05	(0.55)
First job tenure	29.57	(43.60)	32.59	(46.31)	23.66	(37.05)
First job part-time	0.18	(0.38)	0.16	(0.36)	0.22	(0.41)
Cum. earnings within 60 mths	52.73	(56.30)	58.28	(59.62)	41.57	(47.04)
Unemployment spells	420448		280716		139732	

Notes: The table shows sample averages with standard deviations in parentheses. Column (1) pools all spells entering the estimations. Columns (2) and (3) use spells entering age and contribution cutoff estimations, respectively. All durations are in months. The new PBD cannot be imputed for spells with PBD at the age-specific maximum, as the PBD at the beginning of the spell (new plus leftover from the last spell) is capped. Nonemployment duration is the time between the first and the last job, capped at 36 months if longer or censored. UI benefits, wages and earnings are in euros, in prices from the year 2010. UI benefits and wages are monthly. Cumulated earnings are in thousands of euros. All figures based on the first job at reemployment are based on the subsample of individuals who find a job within 6 years after job loss.

Figure 2: Labor Market Outcomes as a Function of Age and Contribution Time



Notes: The figures plot unadjusted averages by 1-year bins of age, and 4-month bins of contribution time using the analysis sample together with a linear fit. The wage change is in log differences.

clearly in Figure 2. These raw correlations show that the nonemployment and UI receipt durations both increase with age. Older workers have on average higher survival rates at any given time in the unemployment spell, as well as benefit exhaustion rates. This influences both the value and cost of PBD extensions at the point of benefit exhaustion. In contrast, the durations decrease in contribution time. The wage loss at reemployment generally increases with age and decreases with contribution time.

4 Empirical Strategy

Our characterization of the heterogeneous responses to PBD extensions proceeds in two steps. First, we exploit the many discontinuities in the German PBD schedule in a multi-dimensional RD design to estimate duration and wage elasticities, as well as the implied fiscal externality, across cells of workers defined by age and contribution time. Second, we run meta regressions of these estimates to uncover any systematic heterogeneity.

4.1 Multidimensional Regression Discontinuity Design

Setup with two running variables and multiple cutoffs. Our identification relies on a sharp RD design, whereby we exploit the local randomization of PBD induced by imperfect control over the running variables. As illustrated in Figure 1, the PBD schedule in Germany augments the RD design in two key dimensions compared to single-cutoff, single-score designs implemented in most settings. Unemployed workers have two running variables measured at entry into UI: their age in years, denoted by X_i^a, and their contribution time in months X_i^h.[14] Additionally, workers face multiple cumulative cutoffs in both running variables. This means that workers face a set of two-dimensional boundaries in age and contribution time at which PBD changes discontinuously (Cattaneo et al., 2016).

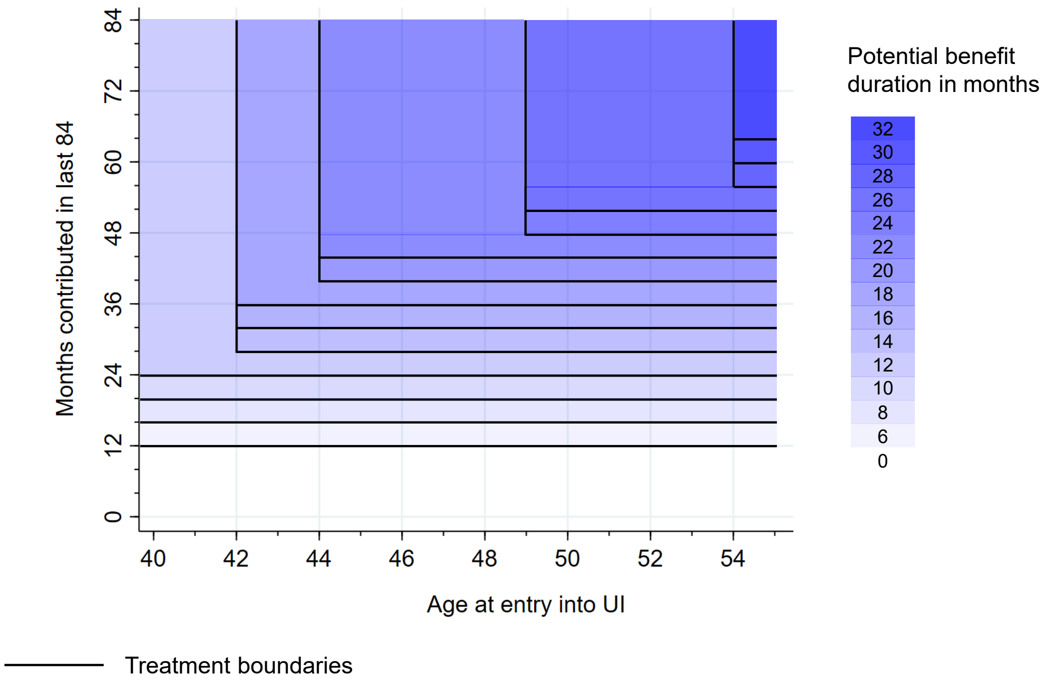
Panel (a) of Figure 3 illustrates the treatment boundaries in the age-contribution plane exemplary for the first regime in our observation period. Take an individual who is 48 years old and has 53 months of contribution. Being one year older would increase their PBD from 22 to 26 months as they would cross the 49-years age boundary. However, increasing their contribution time only would not lead them to increase their PBD, since they are already at the age-specific maximum.

The intuition for sharp identification remains standard: Under the assumption that the value of the running variables cannot be precisely manipulated around the boundaries (e.g., by strategically delaying job separations), observations close to the boundaries are comparable in all but their treatment status. This enables the identification of local causal effects of PBD extensions by comparing observations just above and below the boundaries.

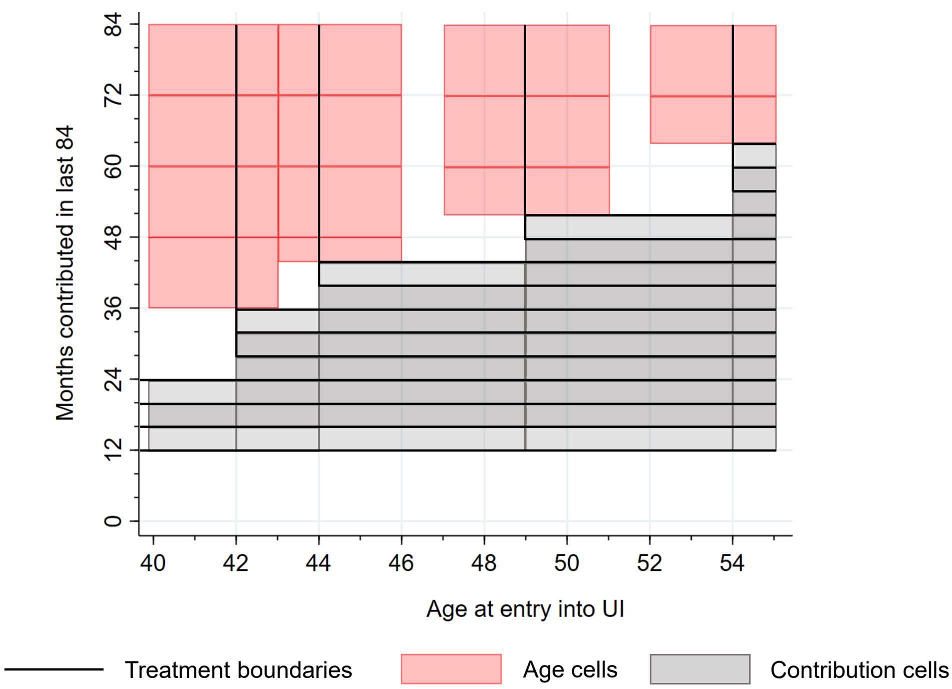
[14] The time horizon for contribution time differs across regimes. We refrain from regime-specific notation for ease of exposure, but take these differences into account in the empirical implementation.

Figure 3: Treatment Plane in Age and Contribution Time

(a) Potential Benefit Duration



(b) Estimation Cells



Notes: The figure illustrates the treatment boundaries (solid black lines) in age and contribution time for regime 1 (the years 1994 to 1997 in our data), and the corresponding PBD level in Panel (a). Panel (b) illustrates the construction of the disjoint estimation cells along the treatment boundaries in which we estimate RD effects, with cells at age cutoffs in red, and at contribution cutoffs in grey.

We leverage this policy variation by splitting the plane into cells defined by age and contribution time as illustrated in Panel (b) of Figure 3, and estimating local treatment effects within each cell. There are several advantages to this approach.[15] First, we obtain a large set of estimates that are allowed to vary flexibly along the age and contribution boundaries. Second, we can assess the plausibility of the identifying assumptions along the boundaries, and focus on credibly-identified estimates. Third, we handle identification separately from extrapolation with tractable and transparent assumptions. The meta regression allows learning about systematic gradients in the effects, with and without controlling for other sources of response heterogeneity across cells, i.e., observable sample composition, business cycle effects, as well as PBD (see Section 5).

Existing studies using German social security data (Schmieder et al., 2012, 2016) have considered age cutoffs only, and focused on individuals eligible for the age-specific maximum PBD (i.e., pooled the red cells in Figure 3, Panel (b)). Our results for these parts of the treatment plane are in line with the existing estimates. Our purpose is however to assess heterogeneity over the treatment plane. Splitting the plane involves a trade-off between the precision of single RD point estimates, and the amount of point estimates to feed into the meta regression. In robustness checks, we show that the precision of our RD estimates increases when cells are aggregated.

Estimation cells. We separate the analysis into single-score RDs by type of cutoff $k \in \{a, h\}$. We construct disjoint estimation cells along the boundaries as illustrated by the shaded cells in Figure 3b. In doing so, we impose minimum age and contribution time requirements to be eligible for a higher PBD given an increase in the other running variable. We thus exclude corner cases that would require an increase in both running variables to get a PBD extension, and avoid overlap in spells across types of cutoff. We consider only individuals older than 40 to have common support over the treatment plane.

For age cutoffs, we assign observations based on age to the closest feasible cutoff C_i^a in the applicable regime. For non-extreme cutoffs, we take observations up to the midpoint to the adjacent cutoff. Our preferred versions use a bandwidth of two years on each side, and exclude observations within two weeks of the cutoff to circumvent local manipulation issues (Schmieder et al., 2012). We then split the age boundary into 12-month contribution bands to allow for effect heterogeneity in contribution time. The bands are meant to be comparable to the bandwidth used in contribution cells, while maintaining power. We compute separate estimates by calendar year of the spell start date to later allow for heterogeneity in time, due to e.g., economic conditions or policy changes.

For contribution cutoffs, we consider observations whose new PBD lies on steps of

[15] Most RD applications pool all observations and center them at their closest cutoff (Cattaneo et al., 2016). This normalized-and-pooled treatment effect is then a weighted average of local average treatment effects at each cutoff value, weighted by the probability of facing that particular cutoff. See Bertanha (2020) for examples of RD applications in economics with multiple cutoffs. For examples with two running variables, see, e.g., Papay et al. (2011), Reardon and Robinson (2012), and Wong et al. (2013).

the schedule, since our imputed measure of contribution is potentially mismeasured (see Section 3.2). We then compare observations on adjacent steps to estimate the effect of PBD extensions at contribution cutoffs C_i^h.[16] This implies that observations on middle steps enter once in a control and once in a treated group. That is, spells with 10 months of PBD (control) are compared to those with 12 months (treated), and 12 months (control) are compared to spells with 14 months (treated). We separate the cells by age group in 5-year bands and calendar year.

Treatment definition and identifying assumptions. The treatment is defined as a binary indicator $D_i = \mathbf{1}\{X_i^k \geq C_i^k\}$ denoting an increase in PBD at the assigned cutoff. In each estimation sample, we compare the outcomes of observations above the cutoff with a higher PBD to those below the cutoff. Within every estimation sample, we impose the following identifying assumptions. Denote by $Y_{di}(c^k)$ the potential outcome for individual i under treatment state $d \in \mathcal{D} = \{0, 1\}$ at each cutoff c^k.

Assumption 1 (Sharp RD). For all $c^k \in \mathcal{C}^k$, and $k \in \{a, h\}$

$$\lim_{\varepsilon \to 0^+} \mathrm{E}[D_i | X_i^k = c^k + \varepsilon, C_i^k = c^k] = 1$$
$$\lim_{\varepsilon \to 0^-} \mathrm{E}[D_i | X_i^k = c^k - \varepsilon, C_i^k = c^k] = 0$$

This assumption means that the treatment switches on for all observations at the cutoff c^k. This is ensured by our construction of the estimation cells.

Assumption 2 (Local continuity of potential outcomes). For all $c^k \in \mathcal{C}^k$, and $k \in \{a, h\}$, $\mathrm{E}[Y_{0i}(c^k) | X_i^k = x^k, C_i^k = c^k]$ and $\mathrm{E}[Y_{1i}(c^k) | X_i^k = x^k, C_i^k = c^k]$ are continuous in x^k at $x^k = c^k$. This assumption states that the expected potential outcomes are continuous functions around the cutoff, and provide valid counterfactuals in both treatment states. In other words, the labor market outcomes would not change sharply at the cutoff in its absence and no other changes than the PBD occur at the cutoff.

Assumption 3 (Local continuity of density). For all $c^k \in \mathcal{C}^k$, and $k \in \{a, h\}$, the density of the running variable $f_{X^k|C^k}(x^k | c^k)$ is positive and continuous in x^k at $x^k = c^k$. In our application, we rely on workers or employers not precisely manipulating the timing of job separations around the cutoffs. In particular, workers cannot delay their entry into UI in order to select into higher PBD steps.

We systematically test the plausibility of these assumptions across cells in Section 4.3. Finally, we assume throughout the analysis that our sample is drawn from a well-defined,

[16]Recall that we cannot directly infer the new PBD in spells with PBD at the beginning of the spell equal to the age-specific maximum. In our estimations using adjacent PBD steps, we drop observations in the control group with leftover PBD such that they would reach the age-specific maximum with the new PBD of the treated group. This ensures that all observations face the same potential change in PBD at the cutoff, i.e., there is a sharp change in treatment status at the cutoff.

representative population of unemployment spells, and that the stable unit treatment value assumption holds, in that there are no spillovers between the unemployed with different PBD levels.

4.2 Estimation

Age cutoffs. We use nonparametric local polynomial regression at age cutoffs since age is measured accurately in our data (Cattaneo et al., 2020). Specifically, we estimate the following specification:

$$Y_{i,s} = \alpha^a_{s,+} + \beta^a_{s,+} f^p(\tilde{a}_{i,s}) + \epsilon^a_{i,s} \tag{5}$$

where $Y_{i,s}$ is the outcome of interest (nonemployment duration, UI receipt duration, or wages at reemployment) of spell i in sample $s = 1, \dots, S^a$. It is modelled as a polynomial function $f^p(\cdot)$ of order p in age normalized to the assigned cutoff, $\tilde{a}_{i,s} \equiv X^a_{i,s} - C^a_s$. Within each sample, we conduct the estimation separately to the right and to the left of the cutoff to reduce boundary issues, using weighted least squares with a kernel $K(\tilde{a}_{i,s}/h)$. The nonparametric RD effect estimate is the difference in intercepts above and below the cutoff, $\hat{\delta}^a_s = \hat{\alpha}^a_{s,+} - \hat{\alpha}^a_{s,-}$. Standard errors are obtained using a block-bootstrap with replacement and clusters at the individual level. Our preferred estimator uses a linear polynomial, a triangular kernel, and a bandwidth equal to the minimum between 2 years and the midpoint to the adjacent cutoff on each side. Our results are robust to these choices, as well as the inclusion of additional covariates (see Appendix Table F.4).

Contribution cutoffs. The potential mismeasurement in the imputed contribution time poses a challenge for RD identification and estimation.[17] Indeed, the discontinuity in the first-stage relationship between the treatment probability and the mismeasured running variable may fade out if the measurement error affects all the observations (Battistin et al., 2009; Davezies and Le Barbanchon, 2017; Pei and Shen, 2017). Furthermore, since we do not know the exact position of observations relative to the cutoff with certainty, we cannot estimate the effect at the boundary using nonparametric methods or conduct standard validity checks (Bartalotti et al., 2021).

To alleviate bias from measurement error in contribution time, we select observations based on actual PBD rather than imputed contribution time. This approach has several advantages. First, it ensures that the true contribution lies within a narrow window of 4 months on each side of the cutoff. We argue that narrowness of this bandwidth limits concerns regarding workers' ability to manipulate their timing of entry into UI and select into specific PBD steps (Le Barbanchon 2016 uses similar bandwidths). Strategic timing would require a thorough knowledge of the complex PBD determination rules in Germany,

[17] The absence of auxiliary information on contribution time prevents us from inferring the distribution of measurement error, which would then require strong distributional assumptions (Davezies and Le Barbanchon, 2017; Bartalotti et al., 2021).

and very precise control over the timing of job separations, especially at the intermediate contribution cutoffs.

Second, we avoid treatment misclassification since we observe the true treatment status rather than impute it based on contribution time. This also allows us to assess the extent of measurement error in our data by checking whether a first-stage discontinuity in treatment assignment exists. Appendix Figure C.1 shows a sharp jump, which suggests that only a share of observations are affected by measurement error (Battistin et al., 2009; Davezies and Le Barbanchon, 2017; Bartalotti et al., 2021). The share of observations whose treatment is correctly classified based on imputed contribution equals 85% within a 4-month bandwidth.

Given this setup, we estimate the following parametric specification:

$$Y_{i,s} = \alpha_s^h + \delta_s^h D_{i,s} + \beta_s^h f^p(\tilde{h}_{i,s}^*) + \epsilon_{i,s} \tag{6}$$

in all contribution cells $s = 1, \ldots, S^h$, where the true treatment status $D_{i,s}$ enters directly in the outcome equation. The specification allows for a direct effect of the mismeasured, centered contribution time $\tilde{h}_{i,s}^* \equiv X_{i,s}^{h*} - C_s^h$, separately for treated and control observations. The treatment effect estimate for sample s is given by $\hat{\delta}_s^h$. The specification imposes functional form assumptions on the potential outcomes on each side of the cutoff. Combined with measurement error, this may lead to an ambiguous misspecification bias in the treatment effect estimate, which depends on the chosen specification (Pei and Shen, 2017). We assess the sensitivity of our estimates to different polynomial degrees.

Additionally, we run robustness checks using a subsample of spells where the PBD imputed based on spell duration and remaining claims equals the PBD imputed based on age and our measure of contribution time, the so-called *consistent sample*. This allows us to test the sensitivity of our results to the variance of measurement error, as contribution is more likely to be precisely measured in this subsample. A similar approach is adopted in Le Barbanchon (2016). Appendix Table C.3 shows descriptive statistics for the consistent sample. The consistent sample is similar to the main analysis sample in terms of demographic characteristics, but has more stable contribution histories with less time in unemployment. Importantly, our main results are not qualitatively altered by these methodological choices. In what follows, we focus on the main analysis sample to maintain power, and relegate any results based on the consistent sample to the Appendix.

4.3 Cell Characteristics and Validity Criteria

Appendix Table D.1 shows descriptive statistics for the 434 estimation cells. The cells include about 1100 spells on average, with the largest cells being located at prime-age and low-contribution cutoffs, as well as older-age and long-contribution cutoffs. Intermediate contribution cutoffs that only apply to older workers have the least observations.

We define a set of validity criteria to flag cells that display evidence of selection around the cutoff, described in detail in Appendix D.1. PBD determines the value of unemployment relative to other transfer programs and may drive selection into UI.[18] Such selection may create systematic differences in the composition of workers around the cutoffs and locally invalidate the RD design. To avoid such effects, we check for common support in covariates, sorting around the cutoff, and nonlinearities in the potential outcome function. We implement this data-driven approach in all our estimation cells and identify 314 valid cells (73%) that pass all criteria. We focus on the valid ones in the main text and provide corresponding results using all cells in the Appendix. We find no systematic relationship between the sample passing the validity criteria, and age and contribution time. Neither our main conclusions nor the coverage of the treatment plane are affected by the exclusion of invalid cells.

4.4 Descriptive Statistics for Elasticity Estimates

We rescale the RD effect estimates $\hat{\delta}_s$ into elasticities:

$$\hat{\varepsilon}_{Y,B,s} = \frac{\hat{\delta}_s/\bar{Y}_s}{\Delta B_s/B_s} \tag{7}$$

where $\bar{Y}_s$ is the sample average of the outcome at the cutoff, B_s is the PBD and ΔB_s is the change at the relevant cutoff. This allows us to bring response estimates onto a comparable metric across cells by accounting for the magnitude of the jump in the PBD at the cutoff, and to compute the fiscal externality of UI as in (3). Table 2 presents summary statistics for the elasticities of nonemployment duration, UI receipt duration, and wages at re-remployment with respect to PBD, as well as the fiscal externality.

Elasticity of nonemployment duration (Panel a). Our results support that PBD extensions trigger behavioral responses through decreased job search effort on average. We find an unweighted average elasticity of 0.38 in age cells, and 0.16 in contribution cells. The inverse-variance weighted mean, as well as the median effect are in the same range. These figures are comparable to the benchmark of 0.2–0.4 discussed in Tatsiramos and van Ours (2014). Between 16 and 20% of the estimates are positive and statistically significant at the 10% level.[19] The share of statistically significant and negative estimates is below 5% in all versions, and likely due to noise. Appendix Figure E.1 shows the distribution of estimates that are significantly different from zero, and those that are not.

[18]On the workers' side, this includes inflows due to early retirement incentives (Sander and van Ours, 2010; Baguelin and Remillon, 2014; Inderbitzin et al., 2016; Gudgeon et al., 2023; Ye, 2022), as well as interaction effects between UI, disability insurance, and other means-tested programs (see e.g., Zweimüller 2018 and Leung and O'Leary 2020). There is also evidence for job separations being timed strategically such that workers become eligible for (more generous) UI (Winter-Ebmer, 2003; Khoury, 2023; Citino et al., 2022).

[19]This is similar to the results in Nekoei and Weber (2017), who find 21% of duration effects significant at the 5% level in a subsample analysis with an average sample size of about 1130 spells. Our share is likely lower due to our sample spanning a broader range of age and contribution values.

Appendix Figure E.1 shows that the moments are generally stable across estimators.

Elasticity of UI receipt duration (Panel b). The unweighted average elasticity equals 0.73 for age cells, and 0.59 in contribution cells. In line with previous studies, the estimates are larger than for nonemployment duration as they reflect a mechanical cost of PBD extensions, and are more precise as they rely directly on UI spell dates. The share of statistically significant and positive estimates is of 29% for age, and 48% for contribution cells, respectively. As expected, only few estimates are negative and significant.

Elasticity of wage change at reemployment (Panel c). Wage elasticities are close to zero on average. The estimates are imprecise, with slightly more negative and significant estimates in the age cells, and positive significant estimates in the contribution cells. The magnitude and range of variation of our elasticities mirror the mixed estimates in the literature (Nekoei and Weber, 2017; Schmieder et al., 2016; Le Barbanchon, 2016).

Fiscal externality of UI (Panel d). The average fiscal externality estimate equals 0.77 in age cells, and 0.60 in contribution cells. That is, a planner who increases PBD by 1 percent has to increase the income tax on the employed by 0.6 to 0.77 percent in order to break even. About half of the estimates are positive and statistically significant at the 10% level. This suggests that the fiscal externality is substantial for a large part of the treatment plane, and mainly driven by duration responses.

Robustness. The distribution of elasticities are robust across different versions of the RD estimators, as seen in Appendix Figure E.2. We use the estimators that control linearly for the running variable and includes covariates for our main analyses. Appendix Table E.1 presents further robustness checks for the fiscal externality. In panel (a), we do not exclude cells on validity criteria. The mean is slightly larger, which suggests that our validity criteria do exclude estimates with negative selection. In panel (b), we provide evidence that the significant estimates are not a result of sampling error by increasing our sample sizes at the RD estimation stage. We do so by pooling years within regimes, but retaining the split along treatment boundaries as depicted in Figure 3. The share of statistically significant and positive estimates increases, while the averages remain similar. We take yearly estimates for our main analyses, but show that our key results are not qualitatively affected by pooling years. In panel (c), we take consistent spells only, whereby the moments are in line with those in the main analysis sample. Finally, panel (d) shows that the fiscal externality estimates are only slightly larger when using a cap of 60 instead of 36 months for nonemployment duration.

Table 2: Summary Statistics for Elasticity Estimates

	(1) Age cells	(2) Contribution cells
Panel (a) Elasticity of nonemployment duration		
Mean elasticity	0.380	0.160
Mean elasticity (IVW)	0.288	0.181
Median elasticity	0.291	0.125
Mean std. err.	0.468	0.464
Share positive sig. at 10%	0.157	0.195
Share negative sig. at 10%	0.043	0.011
Panel (b) Elasticity of UI receipt duration		
Mean elasticity	0.725	0.589
Mean elasticity (IVW)	0.595	0.602
Median elasticity	0.574	0.563
Mean std. err.	0.532	0.486
Share positive sig. at 10%	0.286	0.477
Share negative sig. at 10%	0.014	0.017
Panel (c) Elasticity of wage change at reemployment		
Mean elasticity	0.002	0.006
Mean elasticity (IVW)	-0.002	0.003
Median elasticity	-0.001	0.004
Mean std. err.	0.022	0.035
Share positive sig. at 10%	0.043	0.075
Share negative sig. at 10%	0.064	0.029
Panel (d) Fiscal externality		
Mean elasticity	0.767	0.603
Mean elasticity (IVW)	0.619	0.622
Median elasticity	0.603	0.587
Mean std. err.	0.535	0.493
Share positive sig. at 10%	0.479	0.483
Share negative sig. at 10%	0.014	0.023
Cells	140	174

Notes: The table presents summary statistics for the elasticity w.r.t. PBD, where observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. Based on estimates by year. Age RD estimates using nonparametric local linear regression and controlling for covariates (gender, education, residence, German, last job characteristics). Contribution RD estimates based on a parametric specification where imputed contribution is included as a linear spline, interacted with treatment status, and controlling for covariates. IVW: Inverse-variance weighted to account for precision of the underlying estimates, with weights winsorized at the 10^{th} and 90^{th} percentiles.

5 Response Heterogeneity in Age and Contribution Time

5.1 Meta Regression of Elasticity Estimates

We now explore how the fiscal externality of PBD extensions and the underlying elasticities relate to age and contribution time. To this end, we take the estimates as outcomes for both types of cutoffs and run the following meta regression at the sample level:

$$\hat{\varepsilon}_{Y,B,s} = \theta \bar{a}_s + \zeta \bar{h}_s + \sum_{j=1}^{J} \eta_j \bar{Z}_{j,s} + \mu_t + \upsilon_s \tag{8}$$

where $\hat{\varepsilon}_{Y,B,s}$ is the estimated elasticity of Y (nonemployment duration, UI receipt duration, wages at reemployment, and fiscal externality) w.r.t. PBD in sample $s = 1, \ldots, S$ along the treatment boundaries. $\bar{a}_s$ measures age, and $\bar{h}_s$ years contributed. We use horizons of 3, 5 and 7 years (as implemented in actual regimes) to assess which measure carries information on responses to UI and is therefore relevant for differentiation. For estimates based on age cutoffs, age is equal to the value of the cutoff and contribution to the sample average, and vice versa for estimates based on contribution cutoffs. The coefficient on age θ can be interpreted as the change in the elasticity for a one-year change in age at job loss, holding everything else constant. Similarly, the coefficient on contribution time ζ measures the change for a one-year increase in contribution time.

Importantly, these coefficients inform on the potential of the two characteristics in providing information on the underlying determinants of responses to UI. Although these coefficients cannot be interpreted causally without further assumptions, the meta regression uses as inputs causal effect estimates based on exogenous variation in the two dimensions of interest. These are positively correlated, as older workers tend to accumulate longer contribution times. However, changes in regimes and our comprehensive coverage of the treatment plane provides independent variation that allows us to disentangle the two gradients. The analysis uses variation between groups to estimate an average gradient in responses. It thereby takes the intervals for the steps of the PBD schedule as given and assesses whether more or less differentiation would be justified on average, but does not inform on the optimal split of the groups.

In our main adjusted specification, we hold constant observable differences across cells with a vector of covariates $\bar{Z}_s$. Comparing results with and without adjustment allows assessing the role of confounding sources of heterogeneity in responses to UI that differ across cells and are correlated with age and contribution. First, we control for the PBD level to exclude that the associations with age and contribution time (i.e., the characteristics used for policy differentiation) are driven by the PBD schedule (i.e., the policy differentiation) itself. By design, workers reach a higher PBD level if they increase both running variables. The PBD level may affect the magnitude of the responses at the cutoffs, as extensions occur at different times in the unemployment spell.[20] Controlling for PBD level allows teasing out the pure heterogeneity in responses that is not driven by the policy differentiation itself. Second, we control for observable compositional differences across cells, e.g., in terms of gender, and level of education. All covariates are defined as sample percentages for categorical variables, and sample averages for continuous variables. Shares of spells by quarter account for seasonality. Third, regime fixed effects μ_t account for economic conditions and regime-specific incentives. Finally, υ_s is the error term.

5.2 Main Results

We now document economically-important heterogeneity in the responses to and fiscal externality of UI. Tables 3 and 4 show coefficient estimates on age and contribution time from the meta regressions of the reduced-form elasticities and the fiscal externality, respectively. Figures 4 and 5 present raw and adjusted (based on column 4) estimates by bins of age and contribution time in the last 3 years (i.e. the most relevant horizon for differentiation).

Elasticity of nonemployment duration. The coefficient on age is close to zero and nonsignificant, even when holding compositional differences, time effects and the PBD level constant. We conclude that relatively older workers do not systematically differ in their behavioral response to PBD extensions. The point estimates are positive across the board and close to the mean elasticity at 0.23. The lack of an age gradient and the magnitude of the estimates are in line with previous evidence in Schmieder et al. (2012).

Meanwhile, workers with longer contribution time in the last 3 years have significantly smaller nonemployment duration elasticities, after adjusting for the PBD level. In other words, workers with stable short-term employment reduce their job search efforts relatively less in response to PBD extensions, all else being equal. While this gradient is nonsignificant in the unadjusted specification (column 1), it is significant and equals -31 percentage points for every additional year contributed in the adjusted specification (column 4). Adjusted elasticities range from zero for older workers with stable short-term contribution to about 0.6 for prime-age workers with unstable employment in the last 3 years. Meanwhile, longer horizons of 5 and 7 years are not significantly associated with behavioral responses.

These results yield several important insights. First, contribution in the last 3 years carries the most information on the heterogeneity in underlying unobservables that de-

[20] Moral hazard responses may vary with time in unemployment depending on the strength of anticipation and dynamic selection effects. On the one hand, increasing generosity later in the spell might be costlier as it discourages both short- and long-term unemployed to decrease their job search efforts in anticipation of longer coverage. On the other hand, workers may respond less as the PBD extension comes later in the spell and concerns only a selected group of long-term unemployed. There is however little empirical evidence for a tilt in this profile. Kolsrud et al. (2018) show that the average duration response is the relevant quantity in case of a flat benefit profile. In their implementation, the authors find that the moral hazard cost of UI is larger early in the spell, if anything.

termine job search effort and moral hazard responses to UI. A short contribution horizon is thus more relevant for optimal policy differentiation than longer horizons. For this reason, we focus on the 3-year horizon in our robustness checks. Second, controlling for PBD level matters for unveiling the gradient in contribution time, but not for the one in age. Third, the lack of a gradient in long-term contribution suggests that the role of assets and the liquidity effect of UI is negligible for workers aged 40 or older, which is in line with Michelacci and Ruffo (2015). Long-term contribution time can indeed be seen as a proxy for asset accumulation over longer horizons. We elaborate on these points further below.

Elasticity of UI receipt duration. The part of nonemployment duration that is covered by UI increases mechanically with PBD, even without behavioral responses. Accordingly, the elasticity of UI receipt duration is positive and relatively large for all groups in Figure 4. We find a positive and significant coefficient on age in the unadjusted specifications (columns 1 to 3). However, the raw age gradient is mainly driven by the PBD level itself, as the coefficient becomes smaller and nonsignificant in the adjusted specifications (columns 4 to 6). If anything, there is a slightly larger transfer cost due to higher benefit exhaustion rates among older workers, such that a larger share of them benefit from PBD extensions. Figure 4 confirms the patterns found in the linear specifications.

The elasticity of UI receipt duration significantly decreases by 28 percentage points for each year contributed in the last 3 when adjusting for sample composition and PBD level. The coefficient estimates are close to the ones for nonemployment duration, which suggests that the gradient is mainly driven by behavioral responses. We find no heterogeneity when considering longer contribution horizons. Overall, the adjusted elasticity ranges from 0.4 for prime-age workers with stable employment, to 0.87 for the oldest age group with unstable employment histories.

Elasticity of wage change at reemployment. We find little evidence for meaningful heterogeneity in the wage elasticity. This finding is expected given the low share of significant point estimates at the RD step. If anything, wage responses are not the main driver of the gradient in the fiscal externality of UI, as elasticities are small relative to the duration-related components.

Fiscal externality of UI. The results mirror the ones in duration responses. The estimates in Figure 5 are positive across the board. The raw age gradient is positive, but nonsignificant once we control for the PBD level and sample composition. This suggests that the welfare cost of PBD extensions does not vary with age in the 40-55 range, holding constant the existing policy differentiation. Meanwhile, the coefficient on contribution time in the last 3 years is statistically significant, with a point estimate of –32 percentage points in the adjusted specification (column 3). That is, the planner has to increase the

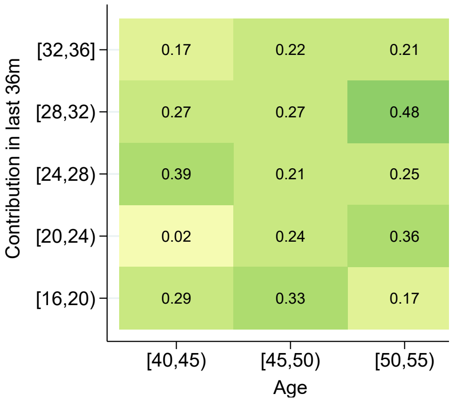
Table 3: Meta Regression of Responses to Unemployment Insurance

	(1)	(2)	(3)	(4)	(5)	(6)
Panel (a) Elasticity of nonemployment duration						
Age	0.0047	0.0052	0.0059	-0.0153	-0.0138	-0.0102
	(0.0071)	(0.0067)	(0.0065)	(0.0122)	(0.0115)	(0.0119)
Years contributed in last 3	-0.0135			-0.3092*		
	(0.0359)			(0.1646)		
Years contributed in last 5		0.0155			-0.0889	
		(0.0233)			(0.0703)	
Years contributed in last 7			0.0188			0.0016
			(0.0151)			(0.0388)
Mean elasticity	0.2264	0.2264	0.2264	0.2264	0.2264	0.2264
Panel (b) Elasticity of UI receipt duration						
Age	0.0141*	0.0143*	0.0146*	0.0100	0.0113	0.0123
	(0.0074)	(0.0074)	(0.0073)	(0.0087)	(0.0085)	(0.0090)
Years contributed in last 3	-0.0334			-0.2800***		
	(0.0326)			(0.0983)		
Years contributed in last 5		-0.0060			-0.0785	
		(0.0180)			(0.0467)	
Years contributed in last 7			-0.0000			-0.0210
			(0.0101)			(0.0195)
Mean elasticity	0.5990	0.5990	0.5990	0.5990	0.5990	0.5990
Panel (c) Elasticity of wage change at reemployment						
Age	0.0000	0.0000	0.0000	-0.0007*	-0.0008**	-0.0009**
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0003)
Years contributed in last 3	-0.0026			0.0037		
	(0.0019)			(0.0042)		
Years contributed in last 5		-0.0018			0.0008	
		(0.0011)			(0.0027)	
Years contributed in last 7			-0.0009*			-0.0004
			(0.0005)			(0.0014)
Mean elasticity	-0.0002	-0.0002	-0.0002	-0.0002	-0.0002	-0.0002
Cells	314	314	314	314	314	314
Regime fixed effects				✓	✓	✓
Sample composition				✓	✓	✓
PBD level				✓	✓	✓

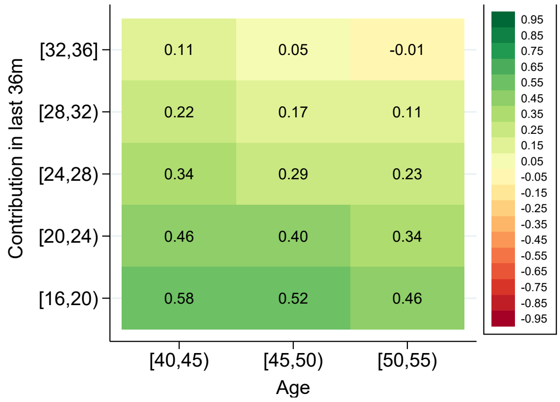
Notes: The meta regression outcome is the estimated elasticity w.r.t. PBD. Observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. RD estimators: nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. Composition variables are share of females, non-German, residence in East Germany, secondary education, higher education, and last job part-time. For the full results of adjusted specifications in column 4, see Appendix Table F.1. $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Figure 4: Elasticities by Age and Contribution Time

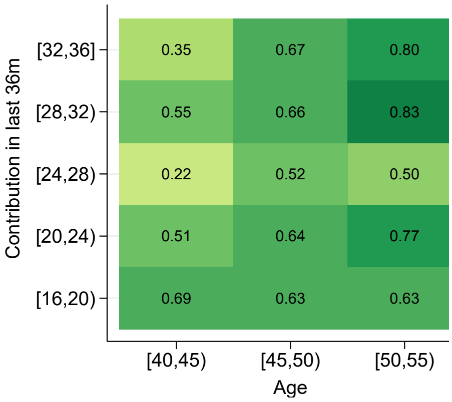
(a) Nonemployment Duration, Raw



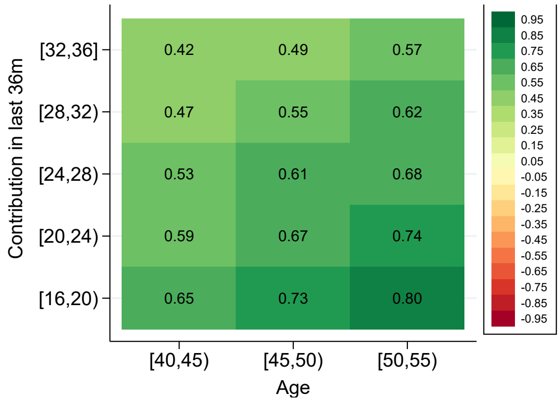
(b) Nonemployment Duration, Adjusted



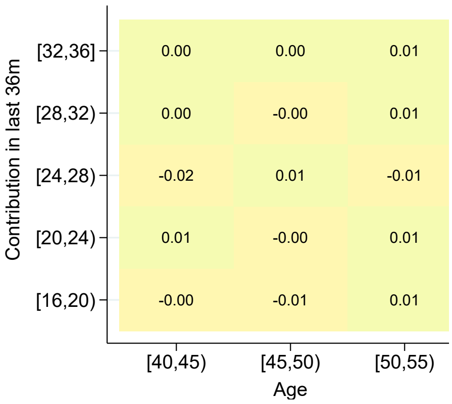
(c) UI Receipt Duration, Raw



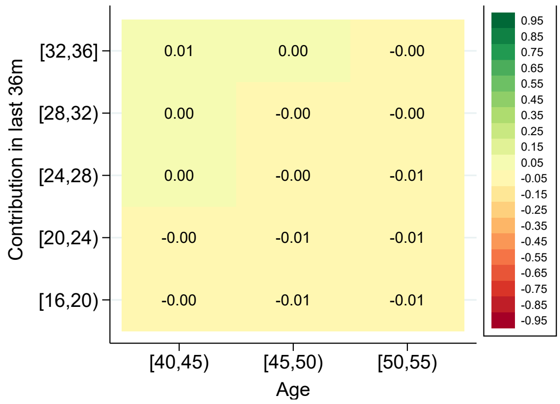
(d) UI Receipt Duration, Adjusted



(e) Wages at Reemployment, Raw



(f) Wages at Reemployment, Adjusted



Notes: The figure displays elasticity estimates by age and contribution time bins in valid cells. The left-hand side panel show raw averages of the estimates. The right-hand side panel shows adjusted values from the meta regression (8) where the estimate is taken as the outcome, and regressed on age, contribution time, PBD level, regime fixed effects, and sample composition.

Table 4: Meta Regression of the Fiscal Externality of Unemployment Insurance

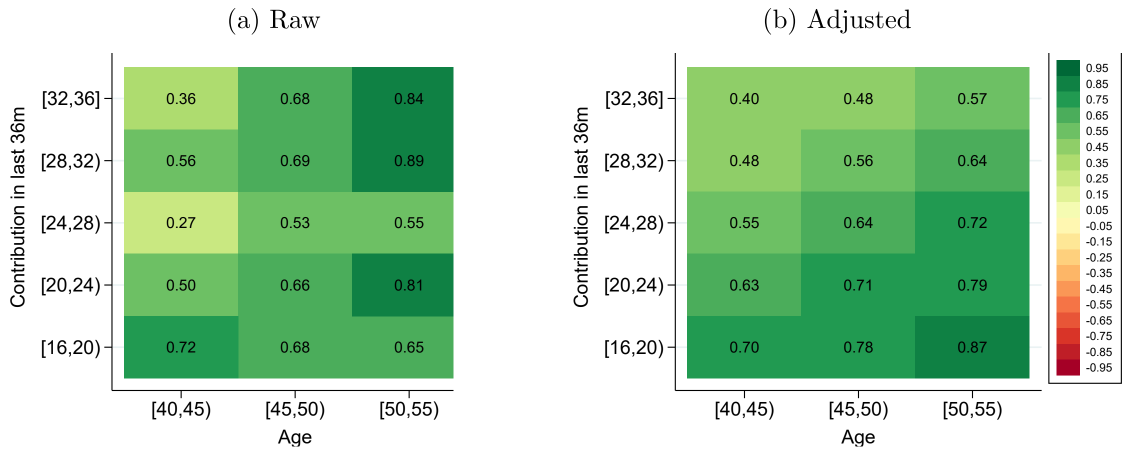
	(1)	(2)	(3)	(4)	(5)	(6)
Panel (d) Fiscal externality of UI						
Age	0.0172**	0.0175**	0.0178**	0.0114	0.0129	0.0143
	(0.0081)	(0.0080)	(0.0078)	(0.0100)	(0.0097)	(0.0102)
Years contributed in last 3	-0.0337			-0.3201***		
	(0.0340)			(0.1066)		
Years contributed in last 5		-0.0045			-0.0902*	
		(0.0192)			(0.0512)	
Years contributed in last 7			0.0017			-0.0213
			(0.0111)			(0.0225)
Mean elasticity	0.6207	0.6207	0.6207	0.6207	0.6207	0.6207
Cells	314	314	314	314	314	314
Regime fixed effects				✓	✓	✓
Sample composition				✓	✓	✓
PBD level				✓	✓	✓

Notes: The meta regression outcome is the estimated fiscal externality w.r.t. PBD. Observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. RD estimators: nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. Composition variables are share of females, non-German, residence in East Germany, secondary education, higher education, and last job part-time. For the full results of adjusted specifications in column 4, see Appendix Table F.1. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

income tax by 32 percentage points less for each additional year of contribution in order to finance a 1-percent PBD extension. The fiscal externality decreases by 9 percentage points for each additional year contributed in the last 5 years, but is not significantly associated with contribution in the last 7 years.

The fiscal externality of UI varies substantially over the treatment plane compared to the estimate of 0.66 (standard error 0.03) for the average worker in our sample aged 48 and with 2.4 years of contribution in the last 3. This means that local response estimates cannot be extrapolated without accounting for the relevant heterogeneity. Adjusted estimates are smallest among prime-age workers with stable employment histories at about 0.4, and largest among older workers with low contribution time at 0.9. However, the latter group represents a small share of inflows (see Appendix Figure D.4). For the largest groups, i.e., 45 to 55-year-old workers with stable employment, the estimates are below the average worker's. Our results speak against workers negatively selecting into more generous parts of the policy, as the fiscal externality is largest for workers with the lowest PBD, after taking out the PBD level effect. Such negative selection effects would work against us finding a negative, behavior-driven gradient in contribution time.

Figure 5: Fiscal Externality by Age and Contribution Time



Notes: The figure displays fiscal externality estimates by age and contribution time bins in valid cells. The left-hand side panel show raw averages of the estimates. The right-hand side panel shows adjusted values from the meta regression (8) where the estimate is taken as the outcome, and regressed on age, contribution time, PBD level, regime fixed effects, and sample composition.

Discussion. In sum, short-term contribution time is the strongest driver of heterogeneity in the fiscal externality among the dimensions we consider. The 3-year horizon is the most informative for policy differentiation, relative to age and longer contribution horizons. Short-term contribution time is a measure of recent or recurrent unemployment, and thereby a direct signal of current human capital. Workers with stable employment may face higher returns to finding a job again relative to workers with unstable work histories. Contribution time may also capture unobservable primitives that define preferences for working and determine UI responses, all else being equal. Our results suggest that contribution time over longer horizons proxies different primitives, which are less informative of heterogeneity in responses to UI.

Age captures the stage of the worker in their life cycle and the remaining time until retirement. It thereby determines the value of searching for a job and accumulating future human capital. However in the range 40-55, i.e. prime-age workers and older workers not yet eligible for early retirement, age itself is not predictive of response heterogeneity. Notice that in the fiscal externality formula (3), the nonemployment duration elasticity is rescaled by the ratio of time in nonemployment to remaining working horizons. In our sample, this ratio increases with age as older workers have a longer nonemployment duration, while having shorter remaining working horizons. However, it does not vary with contribution time. The lack of age gradient in the fiscal externality suggest that this ratio has limited importance in measuring the costs of differentiating UI generosity before (although the value of alternative social insurance programs might also vary in age).

As expected, controlling for PBD level is important given its mechanical correlation with age and contribution time. That is, adjusting for the policy differentiation itself matters for unveiling response heterogeneity in the two tags. Sample composition and time effects matter less for eliciting heterogeneity. This means that contribution time drives behavior beyond other observable characteristics that determine labor market outcomes. The full results for the adjusted specification in column (4) are displayed in Appendix Table F.1. Individual characteristics have little individual predictive power for the elasticities, except for education. To shed more light on the underlying correlations and drivers of response heterogeneity, we regress the sample-level covariates on age and contribution (Appendix Table F.2). Age relates to observable determinants of more precarious employment conditions, i.e., low education, living in East Germany, part-time employment, and past use of UI. The associations hold in the opposite direction for contribution time.

5.3 Robustness Checks

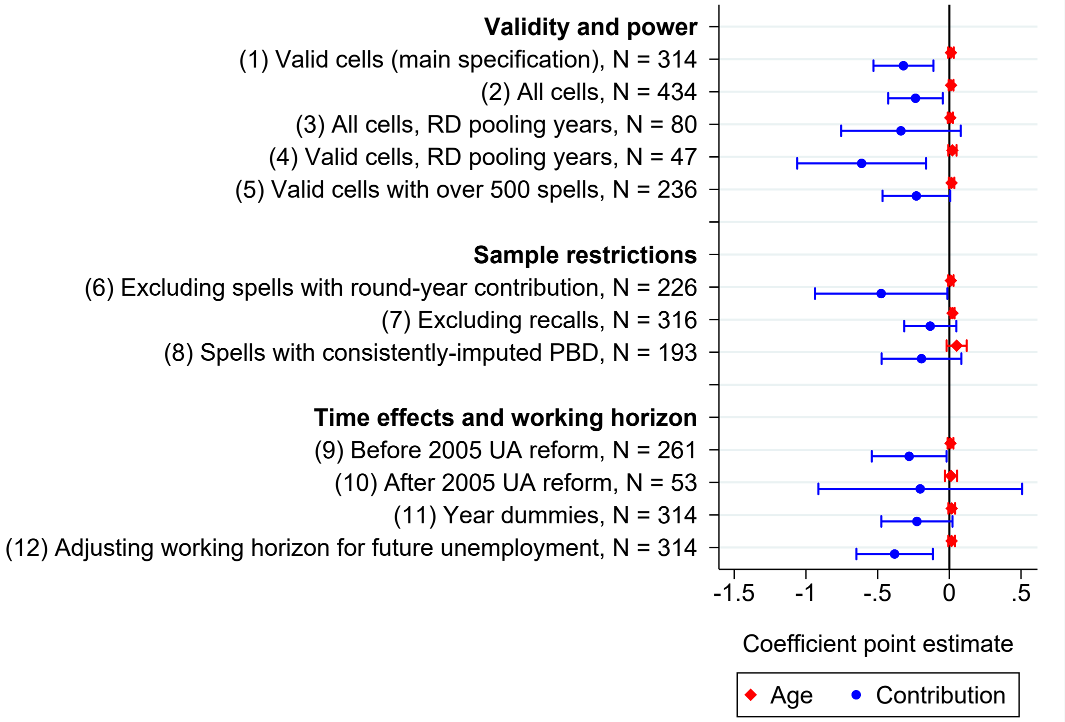
Figure 6 supports the robustness of our key findings across a number of versions where we vary methodological choices and sample restrictions.

Validity and power. We first consider the RD validity criteria (see Section D.1). Our main version (1) uses valid cells, which amounts to discarding estimates displaying selection effects. Version (2) presents the estimations using all the cells. We find that using all cells does not alter the key associations in terms of sign and magnitude, with similar standard errors. The comparison here reassures that our validity criteria—and therefore the underlying methodological choices and compositional changes—do not drive the results. Furthermore, the probability that a cell is valid is not significantly associated with age and contribution time (Appendix Table D.2).

As discussed above, a higher disaggregation at the RD estimation step may produce noisier estimates, but gives more degrees of freedom in the meta regression. To assess the importance of this power trade-off between the two steps, we construct the cells by pooling years within regimes, and otherwise keeping the split of the treatment plane as in Figure 3. Appendix Table E.1 shows that the RD estimates are more precise. In versions (3) and (4) of the meta regression, standard errors increase slightly, but our qualitative conclusions remain robust, despite a larger coefficient estimate for contribution time. Our results are not sensitive to only using cells with more than 500 spells, as seen in version (5).

Sample restrictions. We run the analysis on different subsamples of unemployment spells to reassure that our findings are not driven by specific groups. In version (6), we consider spells with contribution times that are not equal to a round year. These may be due to fixed-term contracts with an ex-ante planned duration, and are over-represented just above contribution time cutoffs. The conclusions remain unchanged, with a larger point estimate for contribution, which reassures that our findings are not only driven by individuals who take up temporary contracts to be eligible for specific parts of the PBD schedule. In version (7), we exclude individuals who were recalled by their last employer

Figure 6: Fiscal Externality -- Robustness Checks



Notes: The figure displays coefficient estimates on age (red diamonds) and years contributed in the last 3 (blue circles), and so for different versions of the meta regression where the outcome is the estimated fiscal externality w.r.t. PBD. Version (1) is our preferred specification where observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. RD estimates based on nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions control for the PBD level, share of females, non-German, residence in East Germany, secondary education, higher education, and last job part-time, and regime fixed effects. They use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles. Confidence intervals are at the 95% level based on standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. Further details on the versions are given in Section 5.3.

(e.g., temporary layoffs due to fluctuations in labor demand), and keep only those who were laid off permanently. In version (8), we run the entire analysis on the spells with consistently-imputed PBD, and find slightly larger coefficients. This provides reassurance that measurement error in imputed contribution time does not substantially influence our findings, and if anything, biases our point estimates towards zero.

Time effects and working horizon. In versions (9) and (10), we test whether the stability of our findings over time, in particular before and after the UA reform in 2005 which altered the benefit system for workers exhausting UI. The estimates do not display significant differences before and after the reform, although the post-2005 contribution point estimate is less precise due to the smaller number of cells entering the meta regression. This reassures that the change in the time profile of post-unemployment benefits does not alter our main conclusions.

Macroeconomic conditions in Germany varied over our study period, and have been shown to matter for the moral hazard costs of UI (Schmieder et al., 2012). In our preferred specification, we account for time effects by including regime fixed effects. The coefficients of interest are robust to alternatively including year fixed effects to control more flexibly for fluctuations in economic conditions (version 11).

Our baseline characterization of the working horizon factor assumes that workers remain employed until retirement once they find a job. However, workers in our data with low contribution times are recurrent users of UI. The assumption of absorbing employment then yields a lower bound on the fiscal externality of UI, as any subsequent unemployment drives it up. To account more finely for differences in the stability of future employment, we specify an alternative version where we scale the remaining time until statutory retirement $T - n$ by the average observed share of time in employment within 5 years after reemployment in each estimation cell. The resulting gradients are displayed in version (12), and suggest our conclusions are not sensitive to assumptions on the stability of future employment.

Further labor market outcomes. Appendix Table F.3 presents meta regressions of the marginal effects of PBD on additional labor market outcomes. Firstly, we show that our results are robust to capping nonemployment duration at 48 or 60 months instead of 36 months. Secondly, we find a positive and significant gradient in contribution time for the probability of finding a job within 5 years after job loss, which is in line with the negative gradient we obtain for nonemployment duration. Finally, and in line with our results for wages, we find no age nor contribution gradient in the probability of earning over 80% of the last wage one year after job loss, the tenure in the first job, the probability of the first job being part-time, and the cumulative earnings within 60 months after job loss as alternative proxies for job quality.

6 Evaluating Existing Policies

We now evaluate the optimality of the policy differentiation in the four PBD regimes spanning our observation period, from 1994 to 2010. We first assess how the insurance value, i.e., the welfare benefit of UI, varies with age and contribution time. We then compare this variation to the one in the fiscal externality to assess how the welfare derivative changes in the two dimensions. In our framework, if the welfare derivative increases with a given characteristic, there is a welfare gain from steeper differentiation in this characteristic relative to the existing PBD schedule. This analysis is a local evaluation, taking all policy parameters at their actual values (i.e. the PBD schedule, contribution time horizon, and flat replacement rate), within the age-by-contribution plane our data covers.

6.1 Estimation

To approximate the insurance value of UI in (4), we have to rewrite it as a function of observable quantities. As we do not have data on consumption, we assume that workers are forward-looking but credit constrained and consume their income every period, which includes earnings and an exogenous fixed component (Card et al., 2007a).[21] Furthermore, let their utility of consumption with constant relative risk aversion (CRRA) be $u(c) = c^{1-\gamma}/(1-\gamma)$ if $\gamma \neq 0$, and $u(c) = \ln(c)$ if $\gamma = 1$, where γ is the coefficient of relative risk aversion. Plugging in these components yields the following expression for the insurance value:

$$IV = \frac{S(B)B(a,h)}{\tilde{n}} \frac{(b+F)^{1-\gamma} - (b_0+F)^{1-\gamma}}{(1-\gamma)b} \frac{w^e}{w} (w(1-\tau)+F)^{\gamma} \tag{9}$$

The details on how we compute this quantity can be found in Appendix G.[22] To elicit how the welfare benefits of PBD extensions are associated with age and contribution time, we regress the insurance value on age and contribution time interacted with regime, and control for observable characteristics. Notice that the insurance value directly depends on the UI policy (b, B), and endogenous outcomes $(S(B), \tilde{n}, w^e)$, and hence on (a, h). This is why we estimate the gradient in the insurance value separately by regime, without controlling for PBD levels. The coefficients on age and contribution time then capture both the degree of differentiation of the existing policy, as well as other age and contribution-related heterogeneity from, e.g., wages. Furthermore, the insurance value increases with risk aversion. Our baseline implementation assumes a conventional, homogeneous value for $\gamma = 2$, but we vary this in a sensitivity check.

To obtain the welfare derivative as in (4), we take the difference between the insurance value and the fiscal externality estimates. Both components are extrapolated across our age-by-contribution plane based on the regression estimates. Importantly, they are allowed to vary in the PBD level to account for the local nature of the evaluation.

6.2 Results

We present our results using heat plots, which display the estimates in 1-year age and 4-month contribution bins. We overlay the regime-specific PBD schedule, to square the estimates with the underlying policy variation. It should be noted that the estimates cannot be directly compared across regimes due to the different contribution time horizons used for PBD determination. Furthermore, although we account for time effects

[21] Datasets with information on consumption in Germany, e.g., the German Socio-Economic Panel, do not allow measuring contribution time.

[22] We conduct the analysis under the UA rules from before 2005, which allows us to plausibly impute the post-exhaustion benefits using the corresponding replacement rate. The post-2005 regime involved complex means testing rules with multiple components determining constant household income based on variables that are unobserved in administrative data. This approach preserves heterogeneity in age and contribution via the replacement rate and the net wage. It requires assuming that the UA schedule itself does not affect the estimated responses, which is supported by Figure 6.

and worker composition, unobservable differences across regimes may limit comparability across regimes.

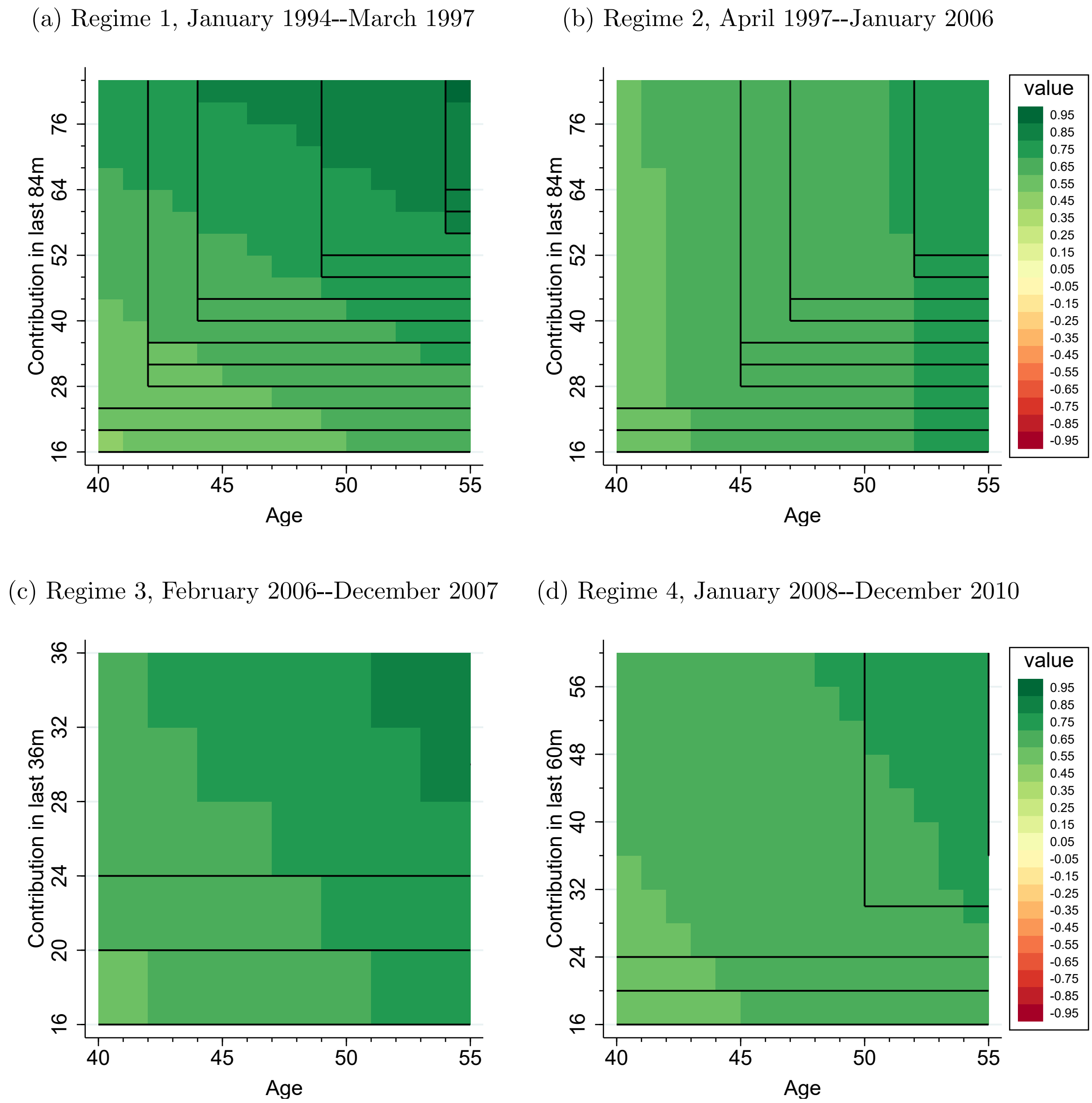
Insurance value. Figure 7 presents the adjusted insurance value estimates by age and contribution time for each regime. In all regimes, the insurance value is positively associated with age and contribution time. As mentioned above, these gradients are driven by the heterogeneity in the formula components. Workers with higher age and contribution time have higher past wages and UI benefits on average. Older workers are more likely to exhaust UI benefits. Furthermore, these gradients are driven by the differentiation in PBD itself. The increase is steeper over longer contribution horizons. This indicates that even with homogeneous risk preferences and assuming away assets, there is heterogeneity in the welfare benefits of UI that might be relevant for policy differentiation.

Welfare derivative. Figure 8 displays estimates of the welfare derivative. These plots can be read as follows. Green shaded areas mark parts of the age-by-contribution plane where increasing the PBD could have increased welfare. Orange shaded areas, respectively, mark those areas where PBD decreases would have been relevant. In terms of differentiation, if the shade varies towards dark green in one dimension, this indicates that more differentiation in this dimension would have been welfare-improving.

Regimes 1 and 2 use a 7-year contribution horizon, which does not capture relevant heterogeneity in moral hazard behavior (based on our meta regression results). The PBD level dominates the gradient, with negative estimates in the upper right corner where PBD is highest. That is, PBD was too generous for older workers with stable employment. In these regimes, flattening the PBD schedule in age could have improved welfare. For instance, this could be done by providing longer PBD for prime age workers and shifting the lowest age cutoff downwards, as well as by eliminating the highest PBD steps for older workers.

Regime 3 eliminated the highest PBD steps, and used the most informative 3-year horizon for differentiation. However, differentiating more by giving longer coverage to workers with more stable employment would have increased welfare. Broadly speaking, regime 4 confirms this insight as it brought workers above 50 closer to the optimum by differentiating more in contribution time. However, doing so across all ages would have been welfare-enhancing. The relevance of steep differentiation in short contribution aligns with the patterns seen in regimes 1, 2 and 4, although the horizon is longer in these regimes. There, PBD is roughly optimal up until 36 months of contribution, above which it tends to be too high.

Figure 7: Insurance Value



(a) Regime 1, January 1994--March 1997

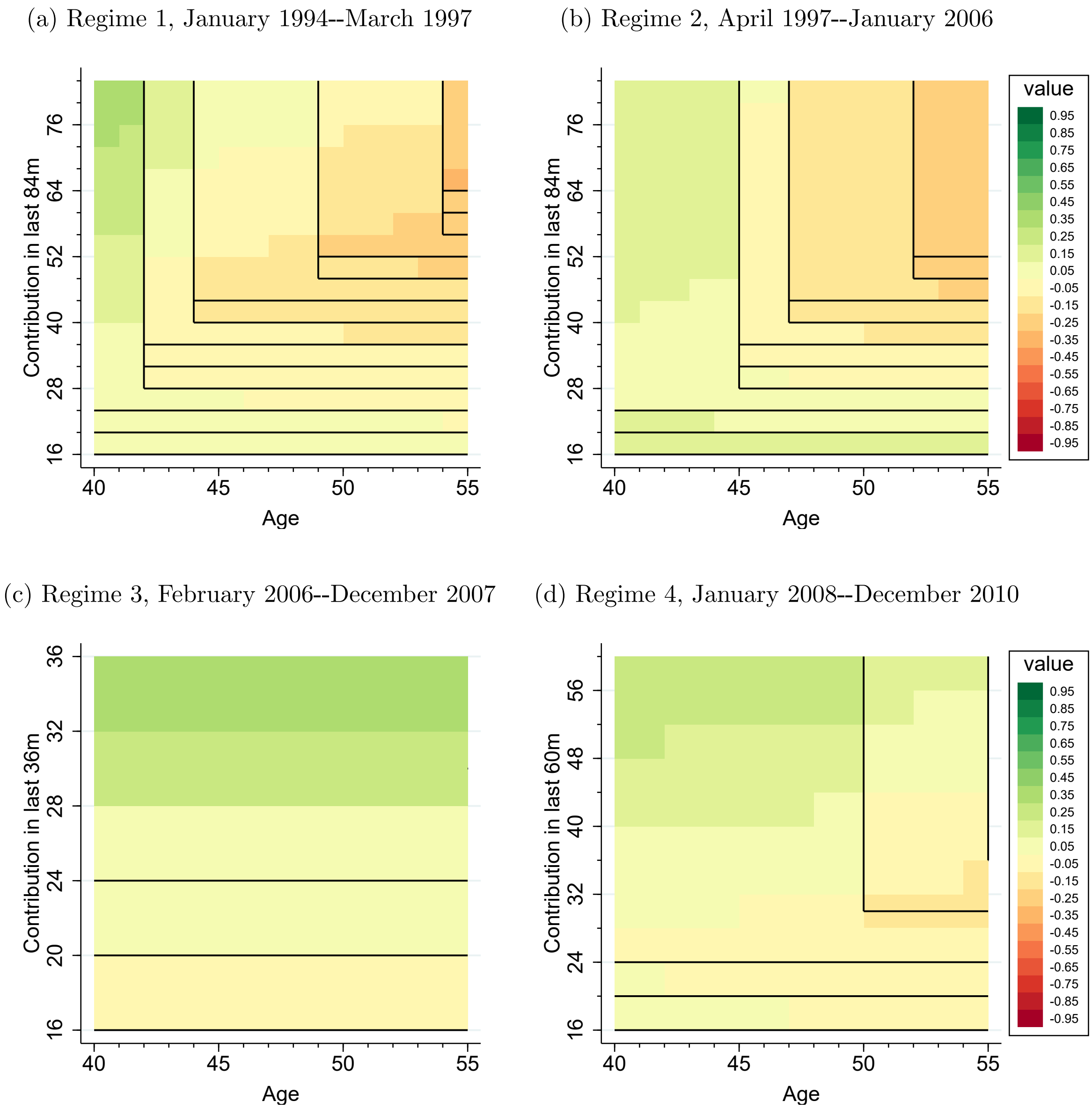
(b) Regime 2, April 1997--January 2006

(c) Regime 3, February 2006--December 2007

(d) Regime 4, January 2008--December 2010

Notes: The figure presents the estimated insurance value by age (1-year bins) and contribution time (4-month bins), separately for each regime in our observation period. The estimates are adjusted values based on age and the regime-relevant contribution horizon, holding constant other observable characteristics at the mean (gender, non-German, residence in East Germany, secondary education, higher education, last job part-time, white collar job, and industry). The approach is described in more detail in Appendix G. The coefficient of relative risk aversion is set to $\gamma = 2$. The black lines denote treatment boundaries where the PBD increases by half of the additional months contributed.

Figure 8: Welfare Derivative



Notes: The figure presents the estimated welfare derivative by age (1-year bins) and contribution time (4-month bins), separately for each regime in our observation period. The estimates take the adjusted insurance value (Figure 7), and subtract the adjusted fiscal externality (Figure G.2) in each group. The coefficient of relative risk aversion is set to $\gamma = 2$. The black lines denote treatment boundaries where the PBD increases by half of the additional months contributed.

6.3 Robustness

Risk aversion. We now explore the sensitivity of our results to letting γ increase linearly from 1 to 3 between 40 and 55 years of age (see Appendix G.2).[23] This induces a steeper age gradient by construction. This strong preference heterogeneity overturns our results

[23] Setting $\gamma = 1$ or $\gamma = 3$ induce level shifts in the insurance value, but the gradients and key conclusions with $\gamma = 2$ remain unchanged relative.

based on homogeneous risk preferences. In regimes 1 and 2, it implies that the optimal policy differentiates less in contribution time among workers older than 50. In regime 3, more differentiation in age and contribution would have been relevant. In regime 4, a welfare-enhancing policy would have been to increase the PBD above age 45 conditional on contribution time. This sensitivity check illustrates that any optimal policy conclusions depend on the assumptions made on preferences, for a lack of a consensus on the empirical values of risk aversion and how it varies with age and employment stability.

Role of assets. In our model, we ignore (heterogeneity in) assets and assume that workers live hand-to-mouth. We motivate our choice with two insights from Michelacci and Ruffo (2015) about the age group of workers we study (40-55). The authors find that by the age of 40, workers accumulate enough assets for the liquidity effect of UI to be negligible. They support this with reduced-form evidence showing that responses to UI do not differ by wealth for workers older than 40. In what follows, we discuss to what extent these findings generalize to our context.

In contrast to Michelacci and Ruffo (2015), we consider differentiation in both age and contribution time. First, with respect to age, Appendix Figure G.1 shows that assets increase steeply to a median value of EUR 90,000 up to age 40 in Germany and remain relatively flat thereafter. This is in line with Michelacci and Ruffo (2015) and supports our assumption. Furthermore, we find no significant age gradient in behavioral responses to UI. Increasing assets in age would drive a negative age-gradient in the welfare derivative, since the insurance value decreases when assets increase. Then, UI should be relatively less generous for older workers conditional on contribution time. Our results with homogeneous risk aversion thus represent a conservative estimate for the negative age gradient in the welfare derivative.

Second, we find that the fiscal externality decreases in contribution time in the last 3 years, holding age and the PBD constant. If the insurance value decreases as well due to increasing assets, then the conclusions regarding optimal differentiation depend on which increase is steeper. Given the strong heterogeneity in the fiscal externality, the insurance value would have to increase substantially to overturn our conclusions. Furthermore, we argue that contribution time in the last 3 years has little impact on assets for workers aged 40 or older, because this period is short relative to the years already spent in the labor market by this age. However, contribution time in the last 7 years is a closer proxy of asset accumulation over longer periods, although it also captures employment stability. We find that UI responses are not associated with contribution time in the last 7 years, all else equal (Table 4). This result supports that the response heterogeneity we find with respect to short-term contribution time is driven by moral hazard responses of recurrent UI users, rather than by liquidity effects.

7 Implications for Policy

Our results yield several insights for UI design. First, our results support policies that condition PBD increases on stable short-term employment. Contribution time in the last 3 years emerges as the relevant observable characteristic to tag the PBD at job loss, after taking out the level effect of the existing policy. It carries information for the social planner on the expected fiscal externality of PBD extensions, and allows screening workers on moral hazard and reducing the welfare cost of UI. In constrast, longer contribution time horizons are not strongly predictive of response heterogeneity.[24] We also do not find purely age-driven heterogeneity in the fiscal externality of UI. In other words, there is no additional welfare cost of PBD extensions for older workers. These results hint at age and contribution capturing different underlying determinants of job search. Although they are correlated, they should be considered separately in differentiating policy, as has been done in Germany.

Second, our welfare analysis highlights that policy-relevant heterogeneity exists on the insurance value side, which is driven by heterogeneity in wages and the existing policy, and so even when assuming homogeneous preferences. Conclusions about optimal policy are however sensitive to modelling assumptions. In the German regimes we analyze, a welfare-enhancing policy change would have been to flatten the PBD schedule in age (e.g., by lowering the age cutoffs), and to steepen it in short-term contribution time. The German system is relatively generous, and bears a high degree of differentiation. Our results suggest that other countries with little differentiation could decrease the fiscal externality of UI by introducing steeper schedules in short-term contribution time.

Third, local estimates for responses at specific cutoffs cannot be extrapolated across the board without accounting for differences in short-term contribution time. They also cannot be readily used to evaluate the optimality of UI for the average worker. Our study adds to estimates in the existing literature, which have mainly focused on the long-term employed so far. These studies underestimate the welfare cost of UI relative to workers with unstable employment histories, as these generate significant externalities in the UI system. This insight is key as a large share of unemployment spells are concentrated in low-contribution segments, as can be seen in Appendix Figure D.4.

8 Conclusion

This paper studies how the welfare effects of UI vary in two key dimensions—age and contribution time at unemployment. It thereby bridges evidence and practice, as both are widely-used tags for the generosity of UI. We set up a job search model that characterizes the moments needed to evaluate the trade-off between the fiscal externality and

[24]Notice that other systems, e.g., in the United States, determine coverage based on the monetary value of past contributions rather than time. This would be another dimension to explore.

the insurance value of UI across worker groups. We estimate these moments by exploiting the rich policy variation in the German UI system, where PBD is highly differentiated and increases with age and contribution time.

We find substantial heterogeneity in the fiscal externality of UI in short-term contribution time, as workers with stable employment exhibit a smaller behavioral duration responses to UI. As a significant determinant of the welfare effects of UI, this observable characteristic carries relevant information for differentiating UI generosity. The fiscal externality is, however, not significantly associated with age nor with longer contribution time horizons in our sample of workers aged 40 to 55. Our results support the use of past employment history for the modelling and assessment of government transfer programs. This insight complements previous studies highlighting the welfare gains from age-dependent taxation and UI systems (Fahri and Werning, 2013; Ferey, 2022). For a broader age range in the United States, Michelacci and Ruffo (2015) find that the optimal policy combining an age-dependent replacement rate with age-dependent taxes yields 90 percent of the welfare gains from a first-best where search effort is observable.

In our local welfare evaluation, the insurance value increases with contribution time and age. The welfare derivative estimates for existing regimes in Germany suggest that the optimal policy differentiates more steeply in contribution in the last 3 years, but less so in age. These results point to a trade-off between the fiscal externality resulting from more generous coverage, and the social preferences for providing longer coverage to workers with worse employment prospects. For instance, the planner could choose to put more weight on older workers, or those with low labor market attachment. Although beyond the scope of this paper, heterogeneous welfare weights across worker groups could rationalize the analyzed policies (Saez and Stantcheva, 2016). Furthermore, while the fiscal externality can be estimated using reduced-form elasticities only, the insurance value requires more structure. Our approximation imposes tractable assumptions on individual preferences and financial markets. A relevant research avenue is to further explore the sources of heterogeneity in the value of UI.

Understanding heterogeneity in age and contribution time is important in light of the rising shares of workers with non-standard employment and gaps in social security contributions, as well as policies increasing the statutory retirement age. Our sample excludes the boundaries of the PBD schedule, especially eligibility cutoffs (e.g. for UI or for alternative social programs such as early retirement), where previous studies have found selection effects that may have sizeable implications for welfare. Our analysis abstracts from selection into specific parts of the UI policy. Adding extensive-margin estimates to our intensive-margin perspective would be valuable for evaluating the differentiated schedule, especially at the boundaries. Finally, exploring general-equilibrium effects induced by the policy differentiation itself would be an interesting avenue for future research.

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APPENDIX

A Existing Literature

Table A.1: Studies on the Effect of Unemployment Insurance Based on RD Designs

Study	**Setting**	**Threshold**	**PBD extension**	**Key sample restrictions**
Caliendo et al. (2013)	Germany	Age 45	from 12 to 18 months	Age 44 to 46, employed at least 36 months in the last 7 years
Card et al. (2007)	Austria	36 months of contribution in previous 5 years	from 20 to 30 weeks	Age 20 to 50, tenure of at least 1 year of the past 5 years (at their last firm)
Johnston and Mas (2018)	Missouri, United States	Cohorts eligible for the maximum PBD	reduction from 73 to 57 weeks	Tenure of 14.5 quarters with previous employer
Lalive (2007)	Austria	Age 50	from 39 to 52 weeks, and from 39 to 209 weeks	Men aged 46 to 53, employed at least 312 weeks in the last 10 years (below cutoff), and 468 weeks over last 15 years (above cutoff)
Le Barbanchon (2016)	France	8 months of employment in the previous year	from 7 to 15 months	Age below 50, employed at most 12 months in the last 2 years
Lalive (2008)	Austria	Age 50 and geographical border cutoff	from 30 to 209 weeks	Age 46 to 53, employed at least 9 years in the last 15 years
Nekoei and Weber (2017)	Austria	Age 40	from 30 to 39 weeks	Age 30 to 50, employed 3 (or 6) years in the last 5 (or 10) years
Schmieder et al. (2012)	Germany	Age 42, 44 and 49	from 12 to 18 months, from 18 to 22 months, and from 22 to 26 months	40 to 49, employed at least 36 (44 and 52, respectively) months in the last 7 years
Schmieder et al. (2016)	Germany	Age 42, 44	from 12 to 18 months, and from 18 to 22 months	Age 40 to 46, employed at least 36 (44, respectively) months in the last 7 years

Notes: The table presents the key design features and sample restrictions of recent papers using regression discontinuity designs to evaluate the effect of UI generosity.

B Welfare Effects of UI

Fiscal externality The social planner maximises welfare w.r.t. the benefit level and duration, and has to break even.

$$\max_{B,b} U(0) \equiv W(B,b) \quad s.t. \quad (T-n)\tau w^e = b\tilde{n} \tag{10}$$

The tax rate is given by:

$$\tau = b\tilde{n}[(T-n)w^e]^{-1} \tag{11}$$

Taking logs and differentiating w.r.t. $\ln b$ and $\ln B$ yields

$$\varepsilon_{\tau,b} \equiv \frac{\partial \ln \tau}{\partial \ln b} = 1 + \frac{\partial \ln \tilde{n}}{\partial \ln b} + \frac{n}{T-n}\frac{\partial \ln n}{\partial \ln b} - \frac{\partial \ln w^e}{\partial \ln b} \tag{12}$$

$$\varepsilon_{\tau,B} \equiv \frac{\partial \ln \tau}{\partial \ln B} = \frac{\partial \ln \tilde{n}}{\partial \ln B} + \frac{n}{T-n}\frac{\partial \ln n}{\partial \ln B} - \frac{\partial \ln w^e}{\partial \ln B} \tag{13}$$

where $\varepsilon_{Y,B} \equiv \frac{\partial \ln Y}{\partial \ln B}$ denotes the elasticity of Y with respect to B. Note that $\varepsilon_{\tau,b} = 1 + \varepsilon_{\tau,B}$ and that

$$\frac{\partial \tau}{\partial b} = \frac{\tau}{b}\frac{\partial \ln \tau}{\partial \ln b} \tag{14}$$

$$\frac{\partial \tau}{\partial B} = \frac{\tau}{B}\frac{\partial \ln \tau}{\partial \ln B} \tag{15}$$

The fiscal externality of PBD extensions (13) contains the three RD responses we measure before normalisation with respect to the change in B at the respective cutoff. Thus, the log changes correspond to the percentage change (normalised response).

$$\varepsilon_{\tau,B} = \varepsilon_{n,B}\frac{n}{T-n} + \varepsilon_{\tilde{n},B} - \varepsilon_{w^e,B} \tag{16}$$

The fiscal externality can be computed by estimating the relevant elasticities across age and contribution time groups.

Optimal UI To compute the conditions for optimal UI, we need the following derivatives:

$$\frac{\partial U(0)}{\partial \tau} = -E\left[w\frac{\partial V((1-\tau)w)}{\partial w}\right] = -E[w(T-n)u_c((1-\tau)w)] \tag{17}$$

$$\frac{\partial U(0)}{\partial b} = \tilde{n}u_c(b) \tag{18}$$

$$\frac{\partial U(0)}{\partial B} = S(B)(u(b) - u(0)) \tag{19}$$

where $S(B) \equiv \Pr(n \geq B)$ is the share of workers who exhaust UI, n is the nonemployment duration, $\tilde{n} = \min(n, B)$ is the duration of UI benefit receipt, and $u_c(\cdot)$ is the marginal utility of consumption.

Optimal UI policy maximizes social welfare and is characterised by:

$$\frac{\partial W}{\partial B} = \frac{\partial U(0)}{\partial \tau}\frac{\partial \tau}{\partial B} + \frac{\partial U(0)}{\partial B} = 0 \tag{20}$$

$$\frac{\partial W}{\partial b} = \frac{\partial U(0)}{\partial \tau}\frac{\partial \tau}{\partial b} + \frac{\partial U(0)}{\partial b} = 0 \tag{21}$$

Rearranging and dividing the optimality conditions, as well as using the government budget constraint to replace b/τ yields:

$$\frac{S(B)B}{\tilde{n}}\frac{u(b)-u(0)}{bu_c(b)} = \frac{\varepsilon_{\tau,B}}{\varepsilon_{\tau,b}} \tag{22}$$

$$E\left[\frac{w}{w^e}\frac{u_c((1-\tau)w)}{u_c(b)}\right] = \frac{1}{\varepsilon_{\tau,b}}. \tag{23}$$

Combining the two equations and plugging in the expression for $\varepsilon_{\tau,B}$ yields the condition for optimal PBD

$$\frac{S(B)B}{\tilde{n}}\frac{\frac{u(b)-u(0)}{bu_c(b)}}{E\left[\frac{w}{w^e}\frac{u_c((1-\tau)w)}{u_c(b)}\right]} = \frac{\partial \ln \tilde{n}}{\partial \ln B} + \frac{n}{T-n}\frac{\partial \ln n}{\partial \ln B} - \frac{\partial \ln w^e}{\partial \ln B} \tag{24}$$

Note that Nekoei and Weber (2017) assume that $\frac{T}{T-n} \approx 1$, i.e., nonemployment duration is short relative to the remaining working life. In our application, this approximation could be violated at older ages, so we set $T(a)$ equal to statutory retirement age. Also, since we may have $n \geq B$ on average for some worker groups, we do not make the assumption that the nonemployment duration is shorter than the PBD.

C Details on the Institutional Setting, Data and Variables

C.1 Unemployment Insurance Regimes in Germany

We conducted a systematic review of the relevant legal provisions and changes therein for the years 1994 to 2016 — our observation window. We begin in January 1994 because the UI replacement rate has been unchanged since then, at 60% without and 67% with dependent children, respectively. This allows us to focus on changes in the duration rather than the level of benefits. The *Arbeitsförderungsgesetz* (AFG) was in effect until 1998, and the *Drittes Buch Sozialgesetzbuch* (SGB III) afterwards, with several revisions. As shown in Table C.1, we split our study period in 4 regimes. We summarise the main regulations and what can be observed in the data in this Appendix. The main relevant differences are in the PBD schedule, as well as the time horizons used to determine eligibility and the PBD.

Note that transition rules applied between some regime changes. In the transition between regimes 1 and 2, the SGB III reform was implemented gradually between 1997 and 1999, so that those workers who contributed at least one year in the three years before 1 April 1997 fell under the old rules until 1 April 1999. We assign the concerned workers to regime 1. In the transition between regimes 3 and 4, workers who were unemployed at the time of the reform had their PBD prolonged retroactively. We exclude them as we cannot infer their initial PBD from the data.

Table C.1: PBD Schedule Regimes in Germany

Regime	Start date	End date	Eligibility determination time horizon	PBD determination time horizon
1	1 July 1987	31 March 1997	36	84
2	1 April 1997	31 January 2006	36	84
3	1 February 2006	31 December 2007	24	36
4	1 January 2008	30 June 2020	24	60

Notes: Time horizons in months of 30 calendar days, from the start date of UI benefits payment. PBD: Potential benefit duration. Sources: *Arbeitsförderungsgesetz* (AFG), and *Drittes Buch Sozialgesetzbuch* (SGB III).

Eligibility. The key elements for UI eligibility and PBD determination rules remained constant over the study period. We focus on spells which constituted a new claim, such that the unemployed is eligible for a new PBD rather than the leftover, if any. The key requirements for a new claim are

- Being below the statutory retirement age at entry into unemployment.
- Job loss (in case of a voluntary separation, a waiting penalty of up to 3 months applies).

– Having contributed to social security at least 12 months within the last 36 months (or 24 after 2006), or since the last UI spell. One month is 30 calendar days, so that one year is 360 calendar days.

PBD determination rules. Conditional on eligibility, the key ingredients for the determination of the new PBD are the exact age at entry into UI and the months contributed to social security within the PBD determination time horizon (see Figure 1). We impute the second variable from past social security spells (see below).

The total PBD at the beginning of the spell includes leftover PBD from past unemployment spells that is added up to the new PBD, up to the age-specific maximum. This is the case if the new claim arises less than 7 years (5 and 4 in regimes 3 and 4, respectively) after accrual of the old claim. If there is no new claim within this period, old unused claims expire 4 years after their accrual.

PBD imputation. Each spell in the unemployment records (LEH) contains information on the leftover PBD from the total at the beginning of the spell (equal to 0 if benefits are exhausted). The start and end dates of the spell capture the time of actual receipt of benefits, since unemployed workers are deregistered from unemployment at benefit exhaustion. Adding the duration of the spell to the leftover PBD allows recovering the beginning PBD P^{begin} as

$$P^{begin} = \min\{P^{max}(a), D + R\} \tag{25}$$

where $P^{max}(a)$ is the age-specific maximum, D is the duration of UI receipt, and R is the leftover PBD of this spell. A similar approach is adopted by Price (2019). It is possible that the PBD is updated while the worker is unemployed, giving rise to a new spell in the data. We correct for this by regrouping all UI spells one day apart, and compute the initial PBD at entry into UI using the duration and the remaining PBD from the first spell in the group.

Our treatment of interest is extensions in new PBD at the threshold. Furthermore, for the estimation of RD effects at the cutoffs, we select spells where the new PBD falls onto a specific step (see Section 3.2). To impute new PBD, we take the beginnng PBD and subtract the leftover from the previous UI spell (if any within the carry-over horizon). Note that the new PBD cannot be directly inferred for spells with total PBD equal to the age-specific maximum due to the capping in (25). The PBD may be adjusted during the course of the spell, e.g., due to penalties for not accepting a suitable job. Our step-based approach should eliminate these cases.

C.2 Imputation of Contribution Time

Contribution time is not directly recorded in the data. We use the comprehensive social security records with daily precision to impute this variable. We proceed as follows

- For each unemployment spell in the initial sample, we identify all recorded past spells that count towards social security. Here, we consider regular employment, employment programs (Arbeitsbeschaffungsmassnahme until 2004, and Beschäftigungszuschuss until 2009), employment with zero wages (up to 4 weeks), and certain types of education.
- We also identify individuals falling under special regulations or transition spells given their observed social security history, and exclude them from the sample (see below).
- We correct past social security spells for overlap, truncating those with the earliest starting date to end one day before the next spell. We also truncate spells overlapping with unemployment benefit receipt.
- We count the distinct days of contribution time in each type of activity, by adding up the days across all relevant spells until one day before the beginning of the unemployment spell. We do so over the eligibility time horizon, and the extended PBD determination horizon.
- We compute the total contribution time by adding up contribution days across different types of activity.
- We assign the remaining unemployment spells to the age and contribution samples, and impose further restrictions required by each type of cutoff.

Special provisions apply to given groups of workers, so we exclude concerned individuals to avoid confounding incentives from these regulations and to focus on workers falling under standard UI incentives. These special provisions include:

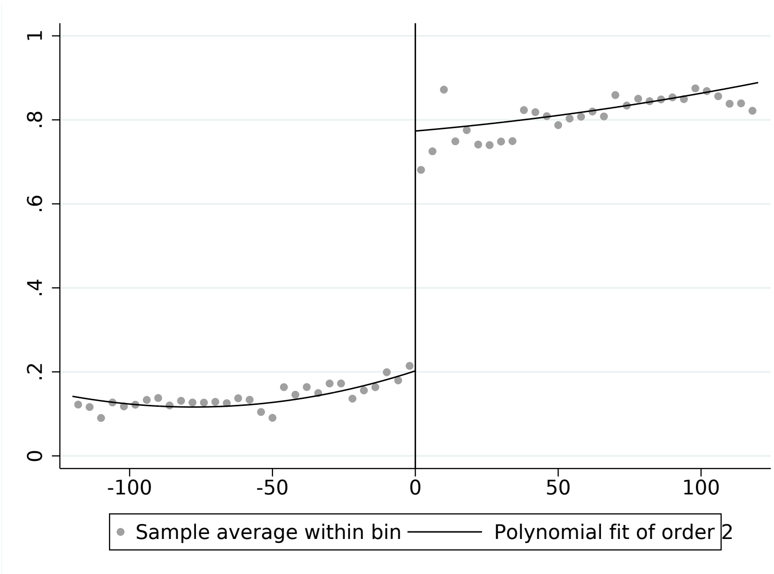
- Some exceptions (e.g., seasonal workers) which are subject to lower minimum contribution requirements for eligibility and PBD determination (e.g., 6 months of work for 3 months of UI). These are excluded automatically since we exclude spells with less than 6 months of new PBD.
- Participation in certain vocational training programs reduced the PBD, so we exclude participants in these programs.
- The receipt of Unterhaltsgeld (UHG) affected the relevant time horizons to determine eligibility and PBD, with changes over time. We exclude workers with any past UHG spells.

Measurement error. There are several potential sources of mismeasurement in contribution time. First, some types of contribution activities cannot be observed in the data. Second, some types of activity do not count towards social security, but prolong the time horizon used for eligibility and PBD determination (e.g., sick leave, parental leave, self-employment, UHG). We can then underestimate contribution time if such gaps are not accounted for. Third, we can impute eligibility incorrectly, which more strongly affects low-contribution spells. Fourth, there can be imprecisions in the recording of spells, especially in ealier years.

We do not dispose of auxiliary information on the true contribution time for any of the workers in our sample. This prevents us from directly inferring the extent of potential measurement error in our imputed measures of contribution time, as in e.g., Davezies and Le Barbanchon (2017). However, observing the true treatment status with the PBD allows us to assess the share of observations affected, and whether the discontinuity in treatment assignment fades out at the cutoffs.

To assess the accuracy of our measure indirectly, we check whether there is still a first-stage discontinuity in the treatment probability as a function of imputed contribution time. Figure C.1 provides suggestive evidence that not all the observations are contaminated by measurement error in our sample, given the sharp jump at the centered contribution cutoff. We then compare the PBD imputed based on the reconstructed contribution time, with the PBD observed in the data. We define as *consistent* the spells where the imputed PBD equals the observed one. Table C.2 shows that the share of consistent spells lies between 60 and 78 percent across regimes.

Figure C.1: True Treatment Probability as a Function of Imputed Contribution



Notes: The figure displays the true treatment probability (based on the PBD) as a function of centered imputed contribution time (in days) using the contribution sample.

Table C.2: Share of Samples among All Available Unemployment Spells

	Regime 1	Regime 2	Regime 3	Regime 4
Age maximum PBD	0.437	0.406	0.519	0.508
No new claim (PBD $<$ 6 mths)	0.087	0.125	0.169	0.131
Classified to PBD step	0.164	0.207	0.123	0.152
Age sample	0.261	0.208	0.016	0.145
Contribution sample	0.105	0.124	0.063	0.092
Consistent	0.783	0.737	0.601	0.736
Observations	423974	663331	92483	210486

Notes: The table shows shares of given samples in the initial sample of drawn unemployment spells. Age maximum PBD: Spells with the age-specific maximum potential benefit duration (PBD), which include the spells entering in the age sample. No new claim: Spells with observed PBD below 6 months (i.e., not eligible for a new claim, but a residual unemployment claim from a previous spell only). Age sample: Spells used in the estimations based on age cutoffs. Contribution sample: Spells used in the estimations based on age cutoffs (see Section 3.2). Consistent: Spells for which we correctly impute the PBD in the analysis sample.

C.3 Variables and Descriptive Statistics

We now provide additional details on the construction of covariates and outcome variables.

– For categorical variables, missing values are coded as separate categories if their share exceeds 5% of the initial sample, and to the highest mode otherwise.

– Employers often fail to report the employees' education. Missing education information for each unemployment spell is filled up using information from past employment spells or corresponding ASU spells if available, following the procedure in Fitzenberger et al. (2005).

– Last-job characteristics are based on the last non-zero wage job before UI entry. Last job tenure is computed as the duration between the start and the end date of this job, regrouping spells that are at most 14 days apart and carry the same firm identifier.

– Following existing studies (Schmieder et al., 2012; Price, 2019), the first job at reemployment is identified as the first regular employment spell after unemployment. This way, we do not consider small or mini jobs used to top-up UI benefits, but rather employment subject to regular social security contributions, that is more likely to reflect a stable reintegration into the labor market. Tenure in the first job at reemployment is computed as for the last job.

Table C.3: Summary Statistics for Alternative Samples

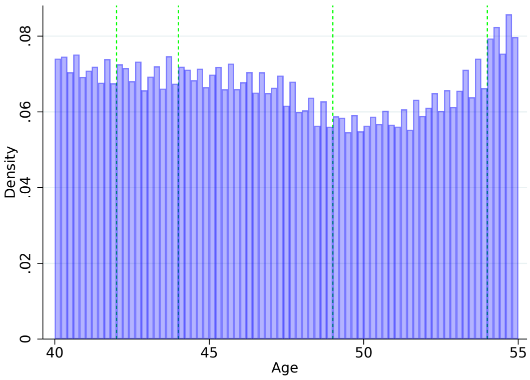
	(1) All		(2) Analysis sample		(3) Consistent sample	
Panel (a) Observable characteristics						
Age	47.12	(4.34)	47.77	(4.06)	47.71	(4.04)
Female	0.41	(0.49)	0.42	(0.49)	0.42	(0.49)
Non-German	0.09	(0.28)	0.08	(0.27)	0.08	(0.27)
Residence in East Germany	0.22	(0.41)	0.21	(0.40)	0.20	(0.40)
Residence missing	0.27	(0.44)	0.30	(0.46)	0.31	(0.46)
No secondary schooling (ref.)	0.88	(0.32)	0.88	(0.32)	0.88	(0.33)
Secondary schooling	0.08	(0.27)	0.08	(0.27)	0.08	(0.28)
Schooling missing	0.03	(0.18)	0.04	(0.19)	0.04	(0.19)
Vocational training (ref.)	0.71	(0.46)	0.71	(0.45)	0.72	(0.45)
Without vocational training	0.21	(0.41)	0.20	(0.40)	0.19	(0.39)
Academic degree	0.05	(0.22)	0.05	(0.23)	0.06	(0.23)
Degree missing	0.03	(0.18)	0.04	(0.19)	0.04	(0.19)
Last job part-time	0.17	(0.37)	0.16	(0.36)	0.15	(0.36)
Last job tenure	32.40	(42.78)	42.43	(46.89)	49.94	(50.22)
Contribution in last 36 mths	26.53	(9.29)	30.16	(7.58)	32.25	(5.77)
Contribution in last 84 mths	57.63	(23.73)	63.77	(21.97)	69.20	(19.10)
UI in last 84 mths	7.34	(7.99)	4.68	(6.94)	2.86	(5.16)
New PBD	4.16	(5.39)	11.42	(4.08)	11.35	(3.91)
Monthly UI benefits	926	(392)	948	(419)	979	(432)
Panel (b) Labor market outcomes						
Nonemployment dur. (capped at 36m)	15.99	(14.41)	17.34	(14.50)	17.24	(14.55)
Nonemployment dur. capped	0.29	(0.45)	0.31	(0.46)	0.31	(0.46)
UI receipt duration	6.28	(5.98)	7.89	(6.98)	8.15	(7.41)
Exhausted UI benefits	0.21	(0.41)	0.16	(0.37)	0.14	(0.35)
First job observed	0.86	(0.34)	0.85	(0.36)	0.84	(0.36)
Wage at reemployment	1,822	(914)	1,870	(934)	1,931	(951)
Log(First wage) - Log(Last wage)	-0.07	(0.57)	-0.10	(0.53)	-0.12	(0.52)
First job tenure	25.43	(39.36)	29.57	(43.60)	31.86	(45.67)
First job part-time	0.18	(0.38)	0.18	(0.38)	0.17	(0.38)
Cum. earnings within 60m	51.96	(53.80)	52.73	(56.30)	56.08	(58.39)
Unemployment spells	1390274		420448		315887	

Notes: The table shows sample averages with standard deviations in parentheses. Column (1) pools all spells for workers between 40 and 55 years of age. Column (2) uses spells entering our main analysis sample around RD cutoffs. Column (3) uses a subsample of column (2) spells where the PBD imputed based on spell duration and remaining claims equals the PBD imputed based on age and our measure of contribution time. All durations are in months. Nonemployment duration is the time between the first and the last job, capped at 36 months if longer or censored. UI benefits, wages and earnings are in euros, in prices from the year 2010. UI benefits and wages are monthly. Cumulated earnings are in thousands of euros. All figures based on the first job at reemployment use the subsample of individuals who find a job within 6 years after job loss.

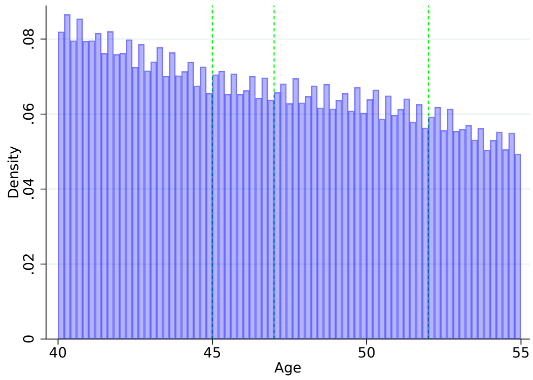
D Validity and Estimation Cells

Figure D.1: Density of Age at Unemployment

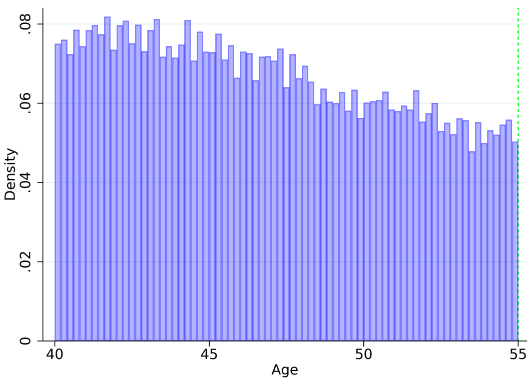
(a) Regime 1



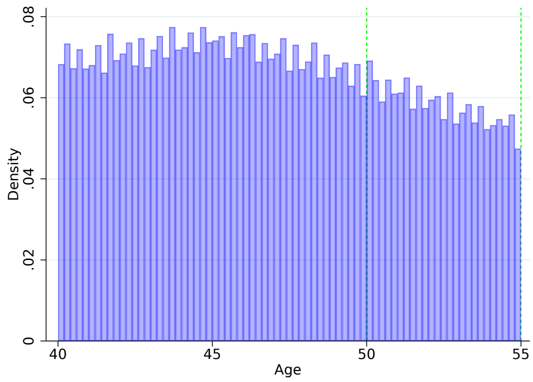
(b) Regime 2



(c) Regime 3



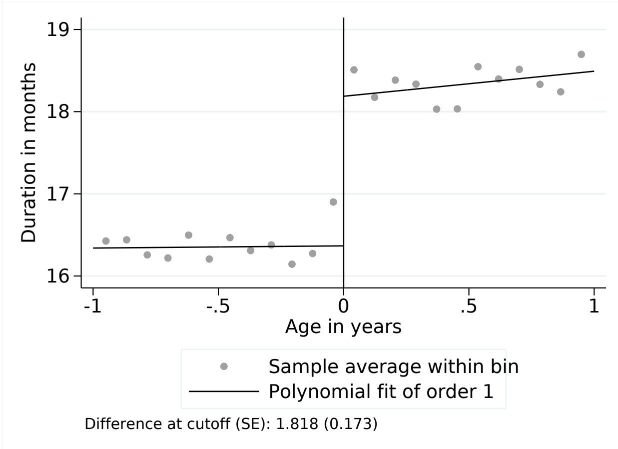
(d) Regime 4



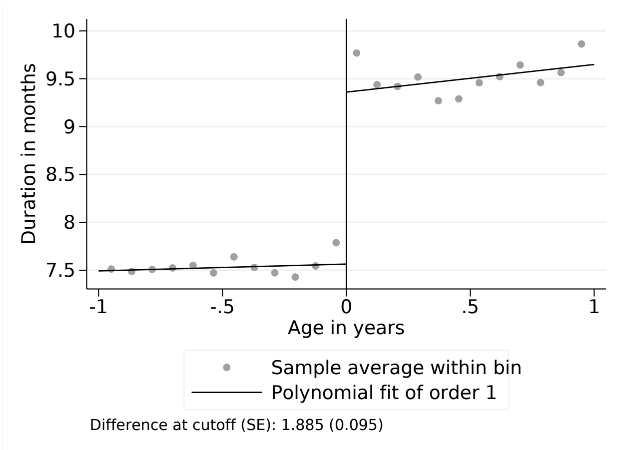
Notes: The figures plot the density of age at unemployment using inflows into unemployment insurance in our analysis sample. Vertical green lines mark age cutoffs where potential benefit duration increases sharply.

Figure D.2: Regression Discontinuity Plots Pooling All Age Cutoffs

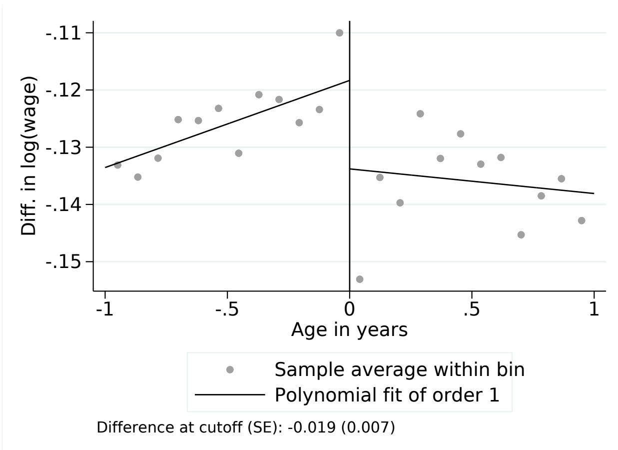
(a) Nonemployment Duration



(b) UI Receipt Duration



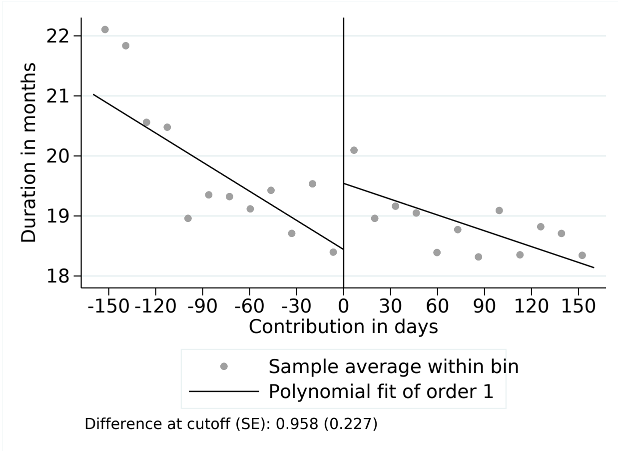
(c) Wage Change at Reemployment



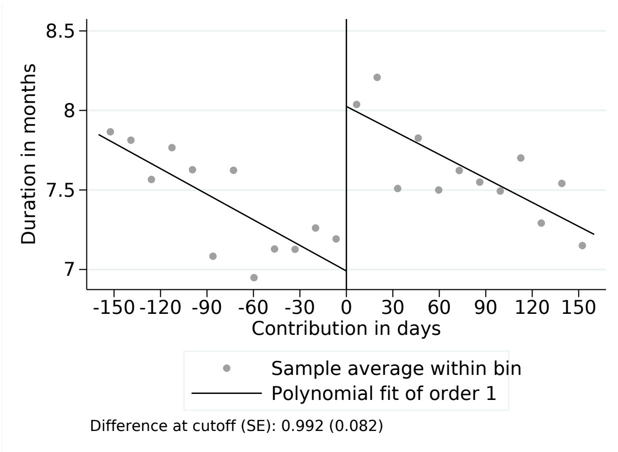
Notes: The figure plots the average for the respective outcomes by age at entry into UI (in 1-month bins), pooling all age cutoffs in the age sample. The solid lines mark linear fits on each side of the cutoff.

Figure D.3: Regression Discontinuity Plots Pooling All Contribution Cutoffs

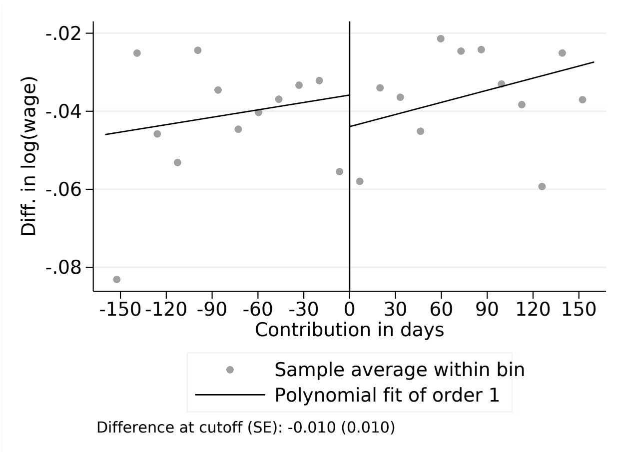
(a) Nonemployment Duration



(b) UI Receipt Duration

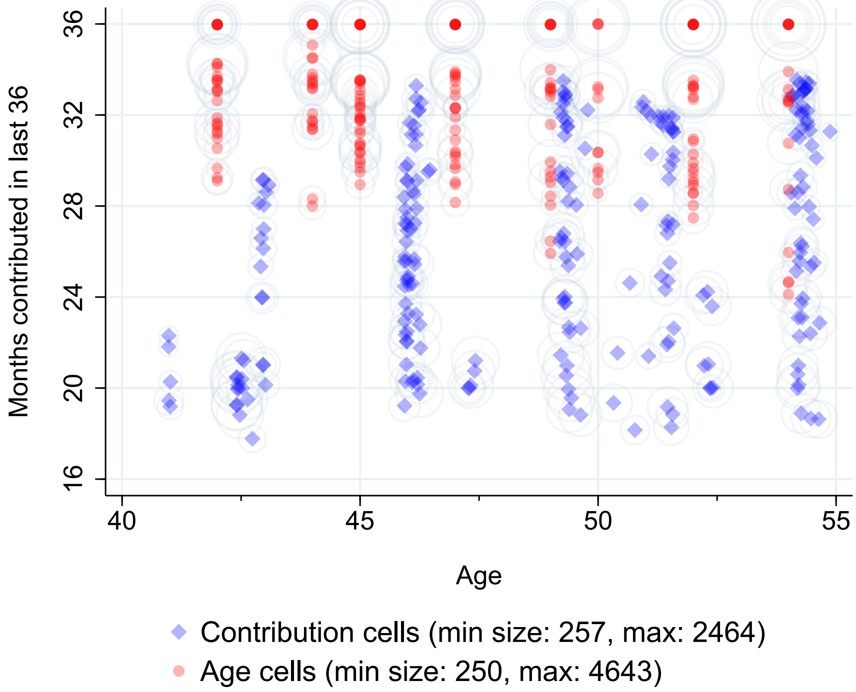


(c) Wage Change at Reemployment



Notes: The figure plots the average for the respective outcomes by contribution time at entry into UI (in 2-week bins, measured within the regime-specific time horizon for PBD determination), pooling all contribution cutoffs in the contribution sample. The solid lines mark linear fits on each side of the cutoff.

Figure D.4: Cells in the Age and Contribution Time Treatment Plane



Notes: The figure displays the cells in the treatment plane (age cells in red and contribution ones in blue). The value of age, contribution time respectively, is defined as the value of the cutoff, or the sample average. Grey circles are proportional to the underlying number of unemployment spells.

D.1 Description of Validity Criteria

We create a set of validity criteria to flag cells that display evidence of selection around the cutoff. Flags are defined to be equal to 1 if the sample fails the corresponding criterion. We exclude ex ante all cells with fewer than 250 spells.

Common support in covariates.[25] We flag the sample if any of the covariates in the sample has an average smaller than the 1st or greater than the 99th percentile of the distribution of sample averages. This allows excluding cells with very particular subpopulations and poor common support.

Sorting in covariates. We take covariates as outcomes, and estimate the RD effect between treated and control groups within each estimation sample. We do so by taking a simple difference-in-means, as well as an RD estimator for each type of cutoff. We use the latter to select our main set of valid cells, as the flags from the two approaches are highly correlated. We flag the sample if the share of covariates with a jump significant at the p level exceeds a share x of all considered covariates. This criterion identifies cells which display evidence of systematic selection around the cutoff in terms of observable characteristics.

[25] Covariates are female, non-German, residence in East Germany, secondary education, higher education, and last job part-time.

Manipulation of the running variable (age cells only).[26] We perform a density discontinuity test to check for evidence of manipulation of the running variable around the cutoff using the method in Cattaneo et al. (2018). We flag cells where the test statistic is significant at the p level and which show evidence of precise manipulation of the timing of entry into UI at the cutoff. Notice that the density plots in Figure D.1 show no evidence for local manipulation at the age cutoffs.

Flatness of potential outcomes (contribution cells only). We regress nonemployment duration on the days in the main activities subject to social security contributions,[27] separately within control and treated groups. We exclude the sample if the share of activities with a slope significant at the p level jointly in the control and the treated group exceeds a share x of all considered activities. This criterion is meant to exclude cells with potentially steep and nonlinear potential outcome functions.

We conservatively define a sample as valid if it does not fail any of the criteria above, using $p = 0.05$ for the level of significance and $x = 0.2$ for the share of covariates that has to be significant. Table D.1 displays summary statistics for sample characteristics and validity. Overall, 314 of the 434, i.e., 73% of cells satisfy all the criteria. The average sample size of about 1,100 spells is similar for the valid cells, with contribution cells smaller on average. The share of treated observations in each sample is around 0.46 on average. The share of cells with significant selection in covariates is around 13%, with larger shares using difference-in-means estimators. The average share of contribution activities with significant slope is of 10%.

[26] This test relies on precise measurement of the running variable and cannot be implemented directly in contribution cells.

[27] Considered activities are regular employment, zero-wage employment (capped at 4 weeks) and some forms of education, mini jobs, UI benefit receipt, and participation in contribution-relevant active labor market programs. These are measured within the regime-specific contribution horizon or up to the last UI spell (time in UI is measured over last 7 years).

Table D.1: Summary Statistics of Sample Validity Criteria

	(1)		(2)		(3)	
	Pooled		Age cells		Contribution cells	
	Mean	(SD)	Mean	(SD)	Mean	(SD)
Panel (a) Yearly cells						
Spells in sample	1,112	(785)	1,460	(934)	830	(483)
More than 500 spells	0.758	(0.429)	0.845	(0.362)	0.688	(0.464)
Share treated	0.457	(0.119)	0.461	(0.155)	0.453	(0.080)
No support in one covariate (1th percentile)	0.071	(0.258)	0.134	(0.342)	0.021	(0.143)
More than 20% covariates sig. 5% (diff. in means)	0.136	(0.343)	0.149	(0.357)	0.125	(0.331)
More than 20% covariates sig. 5% (RD)	0.111	(0.314)	0.119	(0.324)	0.104	(0.306)
Passes all validity checks	0.724	(0.448)	0.722	(0.449)	0.725	(0.447)
Density test sig. 5%			0.057	(0.232)		
More than 20% activities sig. 5%					0.117	(0.322)
Cells	434		194		240	
Spells in sample	1,099	(788)	1,492	(931)	783	(448)
Panel (b) Cells pooling years within regimes						
Spells in sample	6,100	(4,440)	8,158	(5,056)	4,499	(3,107)
More than 500 spells	1.000	(0.000)	1.000	(0.000)	1.000	(0.000)
Share treated	0.462	(0.112)	0.470	(0.148)	0.455	(0.075)
No support in one covariate (1th percentile)	0.113	(0.318)	0.200	(0.406)	0.044	(0.208)
More than 20% covariates sig. 5% (diff. in means)	0.412	(0.495)	0.429	(0.502)	0.400	(0.495)
More than 20% covariates sig. 5% (RD)	0.163	(0.371)	0.029	(0.169)	0.267	(0.447)
Passes all validity checks	0.588	(0.495)	0.657	(0.482)	0.533	(0.505)
Density test sig. 5%			0.114	(0.323)		
More than 20% activities sig. 5%					0.000	(0.000)
Cells	80		35		45	
Spells in sample	6,051	(4,725)	9,109	(5,063)	3,121	(1,352)
Panel (c) Yearly cells, using consistent spells only						
Spells in sample	1,200	(933)	1,460	(934)	400	(134)
More than 500 spells	0.693	(0.462)	0.845	(0.362)	0.222	(0.419)
Share treated	0.458	(0.141)	0.461	(0.155)	0.448	(0.082)
No support in one covariate (1th percentile)	0.086	(0.280)	0.072	(0.259)	0.127	(0.336)
More than 20% covariates sig. 5% (diff. in means)	0.136	(0.344)	0.149	(0.357)	0.095	(0.296)
More than 20% covariates sig. 5% (RD)	0.101	(0.302)	0.119	(0.324)	0.048	(0.215)
Passes all validity checks	0.751	(0.433)	0.778	(0.416)	0.667	(0.475)
Density test sig. 5%			0.057	(0.232)		
More than 20% activities sig. 5%					0.095	(0.296)
Cells	257		194		63	
Spells in sample	1,248	(940)	1,482	(934)	408	(144)

Notes: The table shows summary statistics for the sample characteristics and validity criteria described in this appendix. Observations are estimation cells along the treatment boundaries. Panel (a) uses cells constructed as explained in Section 4. Panel (b) uses cells pooling years within regimes, and Panel (c) is as (a) but uses spells from the consistent sample only. The criteria are defined to equal 1 if the sample fails them. Valid cells are those that pass all the validity criteria. The significance level underlying the tests is 5%.

Table D.2: Meta Regression of the Indicator for a Cell Being Valid

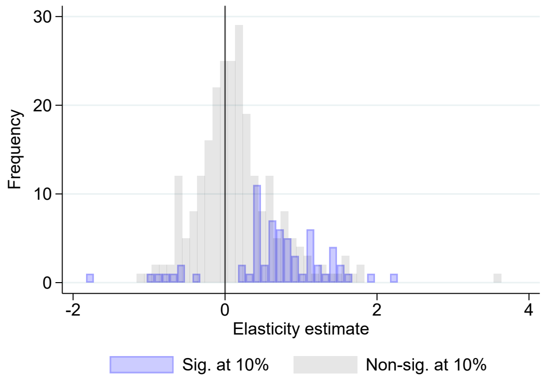
	(1)	(2)	(3)
Age	0.0021	0.0024	0.0016
	(0.0077)	(0.0083)	(0.0085)
Years contributed in last 3	0.1185		
	(0.0976)		
Years contributed in last 5		0.0406	
		(0.0764)	
Years contributed in last 7			0.0094
			(0.0390)
PBD level	-0.0046	-0.0037	-0.0013
	(0.0069)	(0.0082)	(0.0090)
Regime 2	0.0796	0.0883	0.0885
	(0.1020)	(0.1021)	(0.1019)
Regime 3	0.4355***	0.4114***	0.4043***
	(0.1360)	(0.1377)	(0.1392)
Regime 4	0.2525	0.2740	0.2589
	(0.1647)	(0.1695)	(0.1569)
% Female	0.0016	0.0038	0.0036
	(0.0066)	(0.0063)	(0.0067)
% Non-German	0.0035	0.0034	0.0027
	(0.0170)	(0.0172)	(0.0171)
% Residence in Eastern Germany	-0.0096*	-0.0103*	-0.0107*
	(0.0052)	(0.0055)	(0.0053)
% Residence missing	-0.0020	-0.0021	-0.0023
	(0.0019)	(0.0020)	(0.0019)
% Secondary schooling degree	-0.0471	-0.0455	-0.0447
	(0.0324)	(0.0322)	(0.0324)
% Without vocational training	-0.0159	-0.0168	-0.0176
	(0.0137)	(0.0144)	(0.0139)
% Academic degree	0.0322	0.0335	0.0342
	(0.0370)	(0.0372)	(0.0365)
% Last job part-time	0.0030	0.0014	0.0001
	(0.0114)	(0.0118)	(0.0114)
% spells in 2nd quarter	-0.0059	-0.0062	-0.0062
	(0.0068)	(0.0067)	(0.0066)
% spells in 3nd quarter	0.0126	0.0125	0.0129*
	(0.0075)	(0.0074)	(0.0074)
% spells in 4nd quarter	-0.0033	-0.0037	-0.0039
	(0.0052)	(0.0051)	(0.0051)
Constant	0.9139	0.9858	1.1522
	(0.7607)	(1.0351)	(0.9737)
Cells	434	434	434
Outcome mean	0.7235	0.7235	0.7235

Notes: The table presents the full results of the meta regression where the outcome is the indicator for a cell satisfying all validity criteria (Section 4.3). Observations are estimation cells. Standard errors (in parentheses) are clustered by samples' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

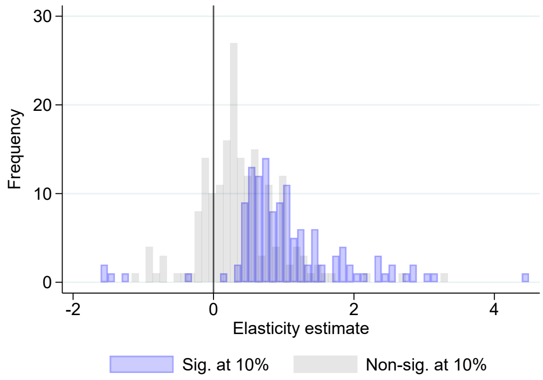
E Further Results for the Elasticity Estimates

Figure E.1: Distribution of Elasticity Estimates

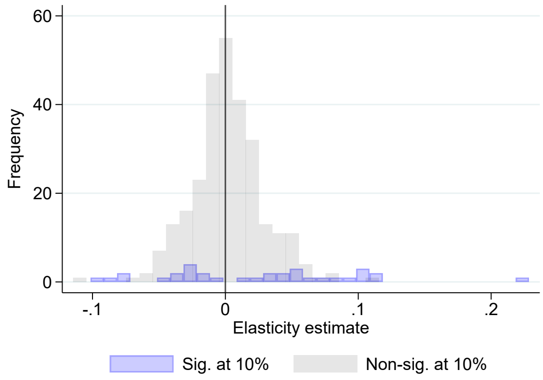
(a) Elasticity of Nonemployment Duration



(b) Elasticity of UI Receipt Duration



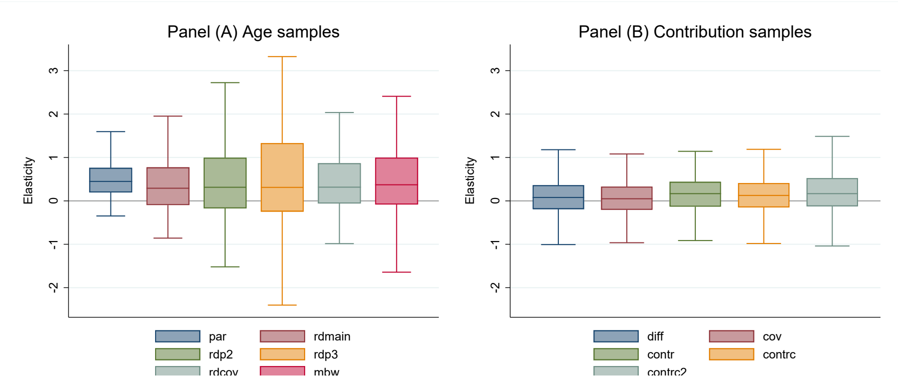
(c) Elasticity of Wage Change at Reemployment



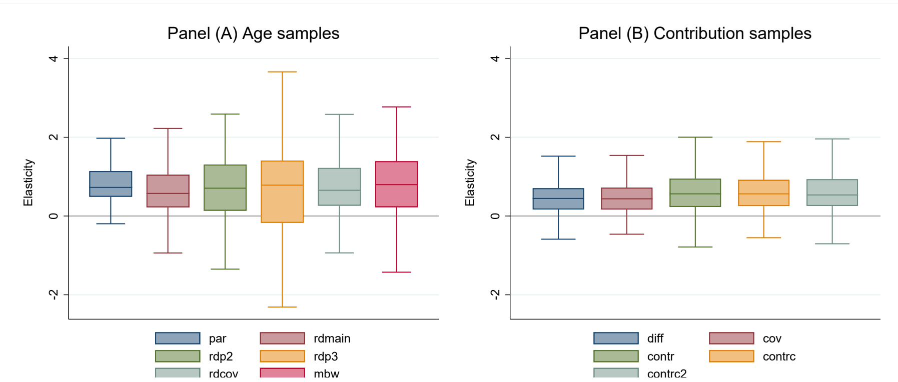
Notes: The figures display histograms of the respective elasticity estimates, separately for estimates statistically significant (in blue), and nonsignificant (in grey) at the 10% level. The underlying observations are valid estimation cells.

Figure E.2: Distribution of Elasticity Estimates Across Estimators

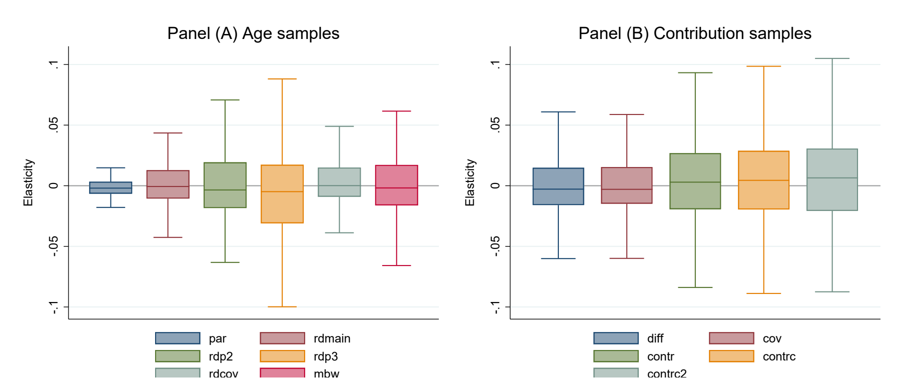
(a) Nonemployment Duration



(b) UI Receipt Duration



(c) Wage Change at Reemployment



Notes: The figures display box plots of the distribution of elasticity estimates across RD estimators. The underlying observations are valid estimation cells. Estimators for age cells.— *par*: Difference-in-means of outcomes between treated and control. *rdmain*: Nonparametric local linear regression with a triangular kernel. *rdp2*: Quadratic polynomial. *rdp3*: Cubic polynomial. *rdcov*: Controlling for covariates. *mbw*: Manually chosen 1-year bandwidth. Estimators for contribution cells.— *diff*: Difference-in-means of outcomes between treated and control. *cov*: Difference-in-means controlling for covariates. *contr*: Allowing for a linear effect of imputed contribution time. *contrc*: Additionally controlling for covariates. *contrc*: Quadratic polynomial.

Table E.1: Summary Statistics for Fiscal Externality -- Robustness Checks

	(1) Age cells	(2) Contribution cells
Panel (a) All cells		
Mean elasticity	0.812	0.654
Mean elasticity (IVW)	0.658	0.663
Median elasticity	0.649	0.661
Mean std. err.	0.545	0.469
Share positive sig. at 10%	0.521	0.529
Share negative sig. at 10%	0.010	0.017
Samples	194	240
Panel (b) Pooling years, valid cells		
Mean elasticity	0.690	0.575
Mean elasticity (IVW)	0.649	0.611
Median elasticity	0.545	0.629
Mean std. err.	0.213	0.241
Share positive sig. at 10%	0.783	0.667
Share negative sig. at 10%	0.000	0.000
Samples	23	24
Panel (c) Consistent spells		
Mean elasticity	0.776	0.684
Mean elasticity (IVW)	0.640	0.525
Median elasticity	0.600	0.608
Mean std. err.	0.530	0.652
Share positive sig. at 10%	0.497	0.310
Share negative sig. at 10%	0.013	0.024
Samples	151	42
Panel (b) Nonemployment duration capped at 60 months		
Mean elasticity	0.424	0.195
Mean elasticity (IVW)	0.295	0.201
Median elasticity	0.295	0.172
Mean std. err.	0.561	0.564
Share positive sig. at 10%	0.136	0.138
Share negative sig. at 10%	0.029	0.000
Samples	140	174

Notes: The table presents summary statistics for the elasticity w.r.t. PBD, where observations are age and contribution estimation cells. Panel (a) uses all estimation cells. Panel (b) uses valid cells pooling years. Panel (c) uses the consistent sample. Panel (d) uses a nonemployment duration elasticity where duration is capped at 60 instead of 36 months as in our main analysis. Age RD estimates using nonparametric local linear regression and controlling for covariates (gender, education, residence, German, last job characteristics). Contribution RD estimates based on a parametric specification where imputed contribution is included as a linear spline, interacted with treatment status, and controlling for covariates. IVW: Inverse-variance weighted to account for precision of the underlying estimates, with weights winsorized at the 10^{th} and 90^{th} percentiles.

F Robustness Checks for the Meta Regression

Table F.1: Meta Regression of the Elasticity Estimates -- Fully-Adjusted Specifications

	Nonemployment duration (1)	UI receipt duration (2)	Wage change at reemployment (3)	Fiscal externality (4)
Age	-0.0153	0.0100	-0.0007*	0.0114
	(0.0122)	(0.0087)	(0.0004)	(0.0100)
Years contributed in last 3	-0.3092*	-0.2800***	0.0037	-0.3201***
	(0.1646)	(0.0983)	(0.0042)	(0.1066)
PBD level	0.0348**	0.0351**	0.0007*	0.0396**
	(0.0129)	(0.0144)	(0.0004)	(0.0155)
Regime 2	0.1598	-0.0026	0.0045	0.0099
	(0.1282)	(0.1283)	(0.0047)	(0.1357)
Regime 3	0.1385	-0.1032	0.0104	-0.1059
	(0.1281)	(0.1640)	(0.0156)	(0.1578)
Regime 4	0.1561	-0.1766	0.0046	-0.1623
	(0.1405)	(0.1460)	(0.0085)	(0.1598)
% Female	-0.0122*	-0.0156*	0.0001	-0.0171*
	(0.0068)	(0.0082)	(0.0003)	(0.0091)
% Non-German	-0.0071	0.0017	0.0016	-0.0014
	(0.0191)	(0.0154)	(0.0010)	(0.0174)
% Residence in Eastern Germany	-0.0135*	-0.0057	0.0004	-0.0070
	(0.0066)	(0.0050)	(0.0003)	(0.0056)
% Residence missing	-0.0026	-0.0019	0.0001	-0.0021
	(0.0022)	(0.0021)	(0.0001)	(0.0023)
% Secondary schooling degree	-0.0512	0.0403	0.0004	0.0356
	(0.0393)	(0.0403)	(0.0027)	(0.0458)
% Without vocational training	-0.0126	0.0154	-0.0015***	0.0163
	(0.0098)	(0.0103)	(0.0004)	(0.0110)
% Academic degree	0.0342	0.0263	-0.0027	0.0335
	(0.0496)	(0.0466)	(0.0029)	(0.0530)
% Last job part-time	0.0105	0.0025	0.0012**	0.0034
	(0.0118)	(0.0103)	(0.0005)	(0.0118)
% spells in 2nd quarter	-0.0095	-0.0086	0.0001	-0.0096
	(0.0074)	(0.0076)	(0.0002)	(0.0079)
% spells in 3nd quarter	-0.0025	-0.0132	-0.0004	-0.0130
	(0.0079)	(0.0084)	(0.0003)	(0.0088)
% spells in 4nd quarter	0.0033	0.0130***	0.0003	0.0130***
	(0.0029)	(0.0037)	(0.0003)	(0.0038)
Constant	2.5827**	0.5102	0.0057	0.6038
	(0.9491)	(0.5955)	(0.0357)	(0.6789)
Cells	314	314	314	314
Outcome mean (IVW)	0.2264	0.5990	-0.0002	0.6207

Notes: The table presents the full results of the meta regression from column (4) of Tables 3 and 4. The outcome is the estimated elasticity of the respective outcome w.r.t. PBD. Observations are valid estimation cells (Section 4.3). RD estimators: nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by samples' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table F.2: Meta Regression of Sample Characteristics as Outcomes

	(1) Female	(2) Non-German	(3) Higher education	(4) East Germany resident	(5) Last job part-time	(6) Months UI in last 7 years	(7) Any UI in last 7 years
Age	0.2084	0.0374	-0.1571***	0.2779	0.2419*	0.2211**	-0.0016
	(0.2439)	(0.0896)	(0.0331)	(0.1723)	(0.1220)	(0.0793)	(0.0033)
Years contributed in last 3	-2.3144	-2.6410***	1.0737***	-2.3216	-5.7187***	-7.4368***	-0.3986***
	(1.8543)	(0.7272)	(0.2713)	(1.5994)	(1.0243)	(0.6811)	(0.0305)
Cells	314	314	314	314	314	314	314
Outcome mean (IVW)	45.5068	9.0269	8.2923	18.6406	17.6643	5.7016	0.4049

Notes: The table presents the coefficient estimates from a meta regression taking sample composition variables as outcomes, i.e., the sample average of the respective variable (in percent for columns 1–5 and 7). Observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. RD estimators: nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table F.3: Meta Regression of Alternative Labor Market Outcomes

	(1) Nonempl. dur. capped 48m	(2) Nonempl. dur. capped 60m	(3) Found job within 60mths	(4) Employed at 80% past wage 1yr	(5) First job tenure	(6) First job part-time	(7) Cumulated earnings 60mths after UI
Age	-0.0184	-0.0203	-0.0016	-0.0142	0.0120	0.0002	0.0125
	(0.0132)	(0.0139)	(0.0058)	(0.0163)	(0.0167)	(0.0184)	(0.0160)
Years contributed in last 3	-0.3552*	-0.3986*	0.1563**	0.2879	-0.2694	0.1565	0.3143
	(0.1797)	(0.1912)	(0.0689)	(0.2128)	(0.2016)	(0.3448)	(0.1993)
Cells	314	314	314	314	314	314	314
Outcome mean (IVW)	0.2367	0.2401	-0.0676	-0.3229	0.1252	0.0298	-0.1919

Notes: The meta regression outcome is the estimated elasticity w.r.t. PBD. Observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. RD estimators: nonparametric local linear regression for age cells, difference-in-means with linear spline in contribution for contribution cells. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. Adjusted for regime fixed effects and sample composition, i.e., share of females, non-German, residence in East Germany, secondary education, higher education, and last job part-time. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table F.4: Meta Regression of the Fiscal Externality -- Alternative Estimators

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	rdmain	rdmain	rdmain	rdcov	rdcov	rdcov	rdp2	rdp2	rdp2
	cov	contrc	contrc2	cov	contrc	contrc2	cov	contrc	contrc2
Age	0.0127	0.0114	0.0142	0.0103	0.0089	0.0118	0.0126	0.0112	0.0140
	(0.0107)	(0.0100)	(0.0105)	(0.0108)	(0.0101)	(0.0106)	(0.0112)	(0.0105)	(0.0109)
Years contributed in last 3	-0.3281***	-0.3201***	-0.3148***	-0.3115**	-0.3028**	-0.2964**	-0.2768	-0.2671	-0.2681
	(0.1058)	(0.1066)	(0.1057)	(0.1266)	(0.1281)	(0.1268)	(0.1899)	(0.1931)	(0.1929)
Cells	314	314	314	314	314	314	314	314	314
Outcome mean (IVW)	0.6254	0.6207	0.6146	0.6505	0.6453	0.6387	0.6244	0.6178	0.6097

Notes: The meta regression outcome is the estimated elasticity w.r.t. PBD. Observations are age and contribution estimation cells that satisfy the validity criteria described in Section 4.3. All meta regressions use inverse-variance weights (IVW) winsorized at the 10^{th} and 90^{th} percentiles, and standard errors (in parentheses) are clustered by cells' age group (in 5-year bands), contribution time group (in 4-month bands) and cutoff type. Adjusted for PBD level, sample composition (share of females, non-German, residence in East Germany, secondary education, higher education, and last job part-time) and regime fixed effects.
Estimators for age cells.— *rdmain*: Nonparametric local linear regression with a triangular kernel. *rdcov*: Controlling for covariates. *rdp2*: Quadratic polynomial. Estimators for contribution cells.— *cov*: Difference-in-means controlling for covariates. *contrc*: Allowing for a linear effect of imputed contribution time and controlling for covariates. *contrc2*: Quadratic polynomial. $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

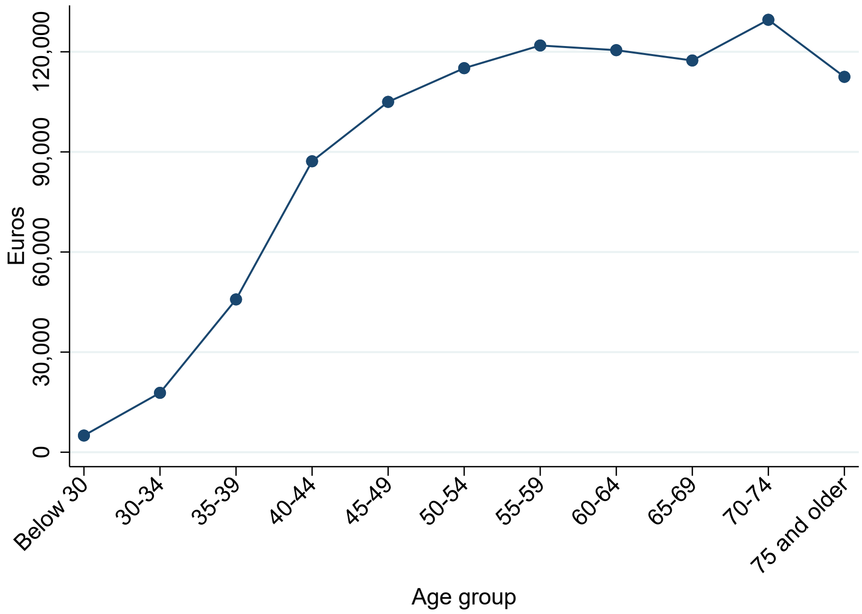
G Welfare

G.1 Approximating the Insurance Value of UI

We impute the single components of the insurance value in (4) on an individual level for the analysis sample as follows. We predict the probability of surviving in unemployment in every month after entry into UI based on observable characteristics (gender, non-German, residence in East Germany, secondary education, higher education, last job part-time, white collar job, types of contribution history, and industry). We then use the survival probability $S(B)$ for each spell based on the observed PBD. PBD and UI receipt duration are taken as observed. We impute the UI and UA replacement rates in the sample of spells starting before 2000, for which the number of children is unobserved. We estimate the probability of having a high replacement rate based on observable characteristics using spells after 2000. We predict this probability for the sample of spells starting before 2000, cutting at 0.3 such that the shares of high-replacement rate stays roughly constant. We use the observed or imputed replacement rate (57% with children, 53% without) to compute UA benefits as a share of past earnings under the pre-2005 rules. The net wage is imputed using unemployment benefits and the replacement rate. We assume a constant additional household income of €400, such that UI benefits are roughly half of total real income in prices from 2010 (Card et al., 2007b). Finally, we censor the insurance value at the 95th percentile.

G.2 Inputs and Robustness Checks

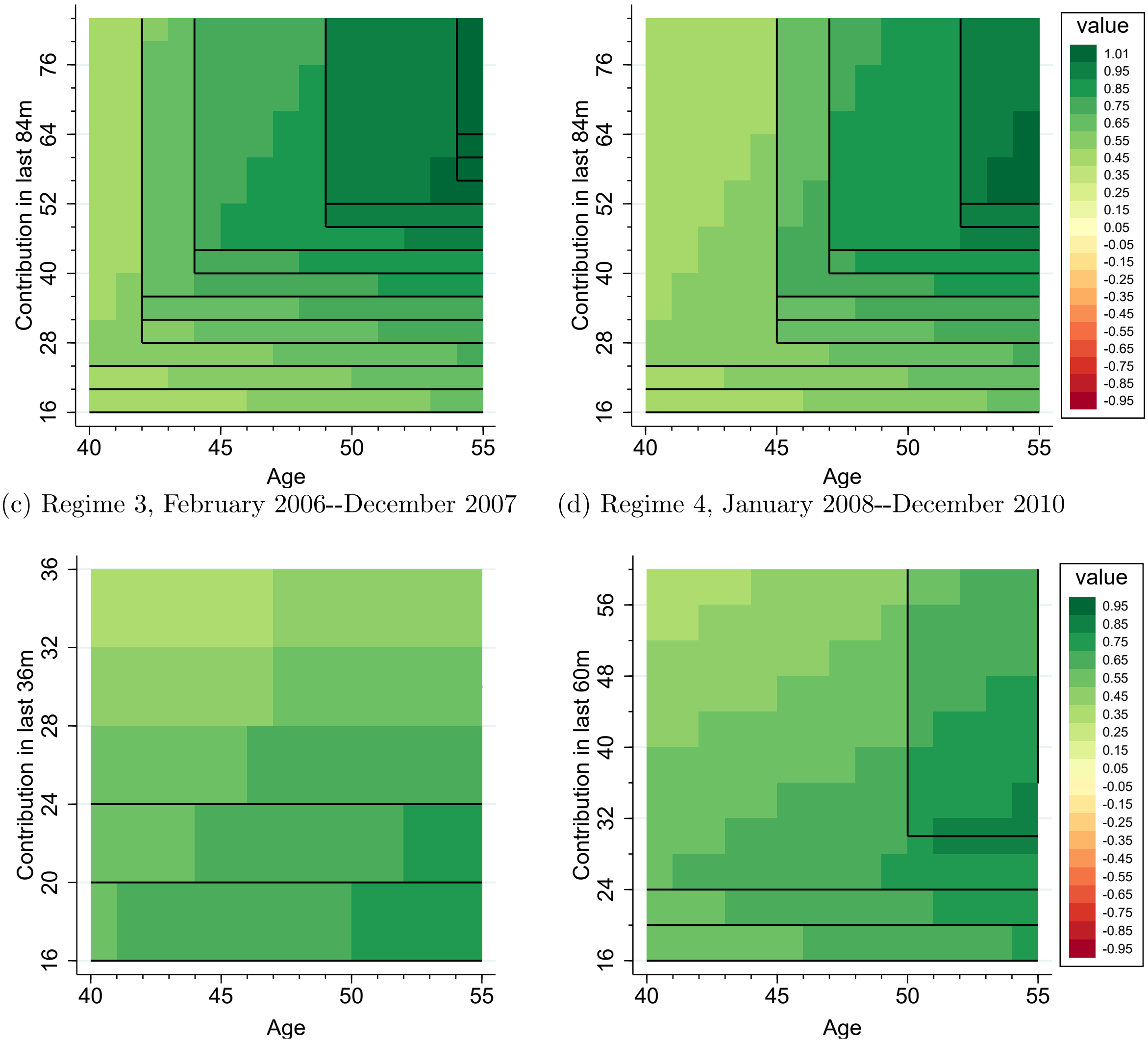
Figure G.1: Median Household Net worth by Age Group



Notes: The figure displays median household net worth within age group in Euros. Net worth includes real estate property and savings net of debt but excludes business assets.
Source: The data has been provided by Institut der deutschen Wirtschaft (2020) and is based on the German Income and Expenditure Survey 2018.

Figure G.2: Fiscal Externality -- Input for Welfare Analysis

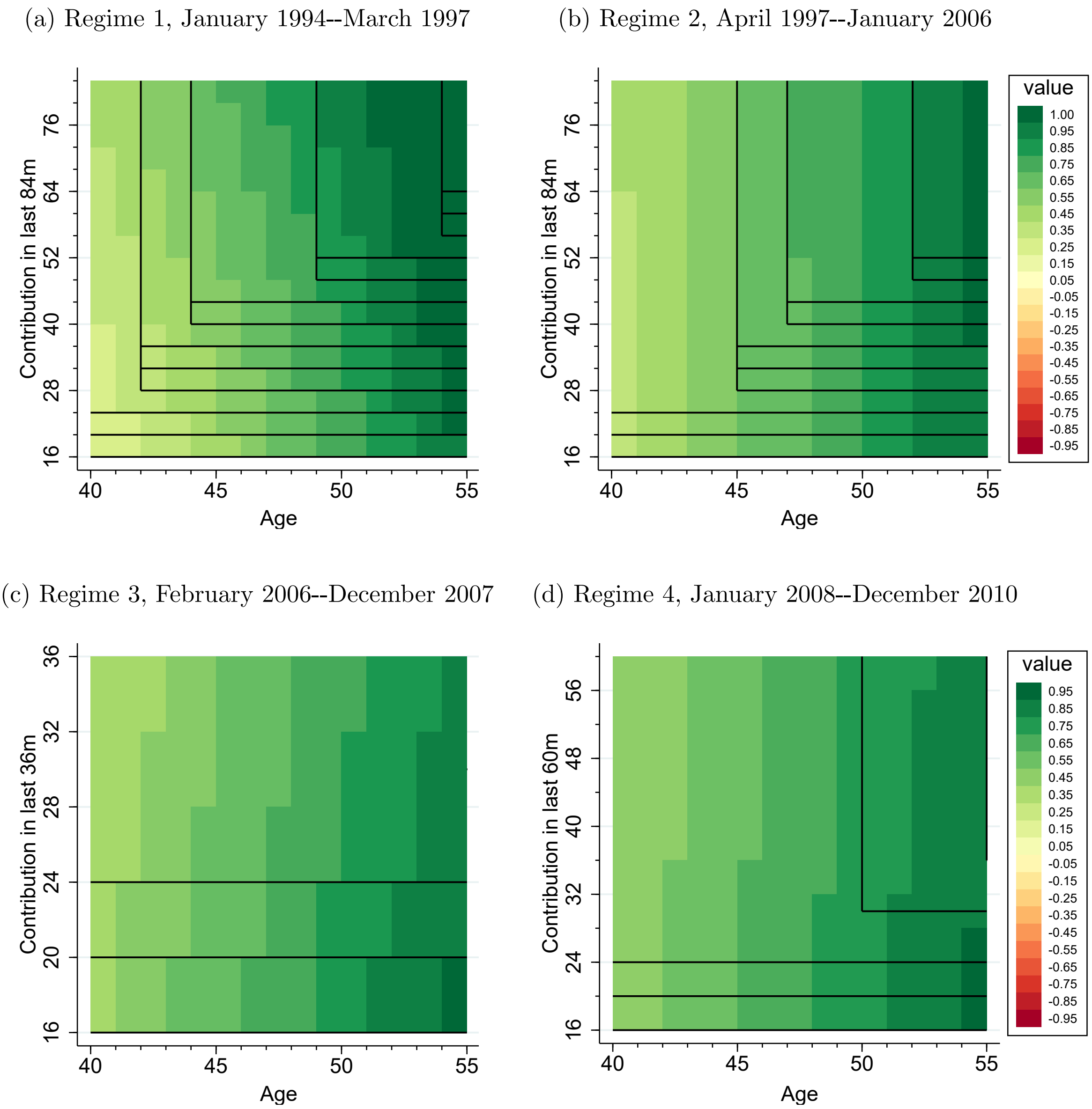
(a) Regime 1, January 1994--March 1997 (b) Regime 2, April 1997--January 2006



(c) Regime 3, February 2006--December 2007 (d) Regime 4, January 2008--December 2010

Notes: The figure presents the estimated insurance value by age (1-year bins) and contribution time (4-month bins), separately for each regime in our observation period. The estimates are adjusted values based on age, the regime-relevant contribution horizon, and the underlying PBD level, holding constant other observable characteristics at the mean (gender, non-German, residence in East Germany, secondary education, higher education, and last job part-time). The black lines denote treatment boundaries where the PBD increases by half of the additional months contributed.

Figure G.3: Insurance Value -- γ Between 1 and 3



(a) Regime 1, January 1994--March 1997

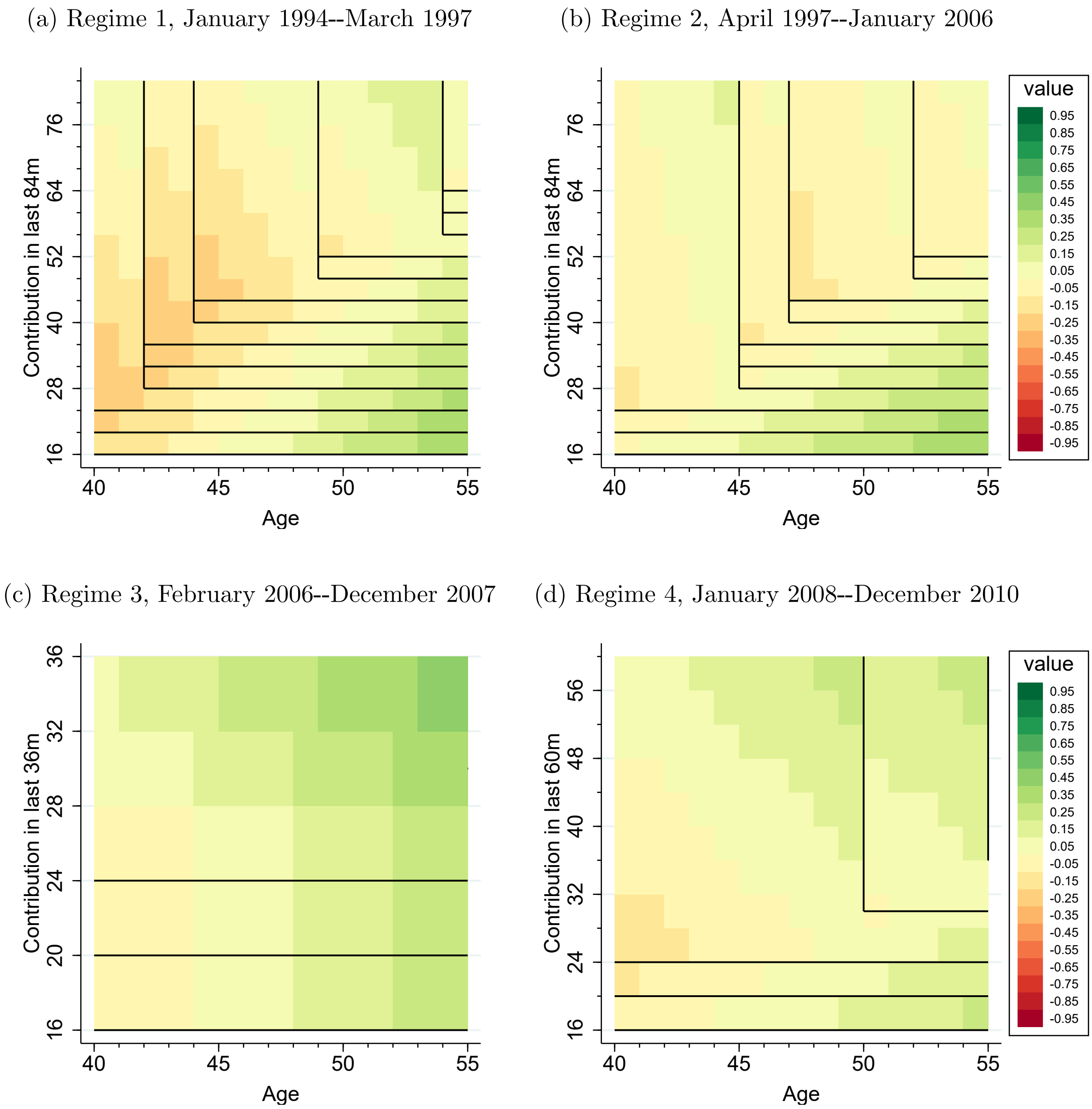
(b) Regime 2, April 1997--January 2006

(c) Regime 3, February 2006--December 2007

(d) Regime 4, January 2008--December 2010

Notes: The figure presents the estimated insurance value by age (1-year bins) and contribution time (4-month bins), separately for each regime in our observation period. The estimates are adjusted values based on age and the regime-relevant contribution horizon, holding constant other observable characteristics at the mean (gender, non-German, residence in East Germany, secondary education, higher education, last job part-time, white collar job, and industry). The approach is described in more detail in Appendix G. The coefficient of relative risk aversion is set to increase linearly from 1 to 3 in age between the age of 40 and 55. The black lines denote treatment boundaries where the PBD increases by half of the additional months contributed.

Figure G.4: Welfare Derivative -- γ Between 1 and 3



(a) Regime 1, January 1994--March 1997

(b) Regime 2, April 1997--January 2006

(c) Regime 3, February 2006--December 2007

(d) Regime 4, January 2008--December 2010

Notes: The figure presents the estimated welfare derivative by age (1-year bins) and contribution time (4-month bins), separately for each regime in our observation period. The estimates take the adjusted insurance value where the coefficient of relative risk aversion increases linearly from 1 to 3 in age between the age of 40 and 55. We subtract the adjusted fiscal externality (see Figure 5) in each bin. The black lines denote treatment boundaries where the PBD increases by half of the additional months contributed.