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IDENTIFYING SELECTION IN SOCIAL INSURANCE: RISK-BASED SELECTION AND SELECTION ON MORAL HAZARD

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Abstract

We investigate selection into a voluntary unemployment insurance (UI) scheme by exploiting variation in enrollment driven by an early retirement program embedded within the UI system. Using Danish administrative data and an event study design, we identify negative selection among UI enrollees with respect to subsequent unemployment risk. Our analysis distinguishes between risk-based selection and selection on moral hazard. We find that risk-based selection accounts for approximately two percentage points, while the selection on moral hazard is about one and a half percentage points. Together, these effects explain two-thirds of the observed difference in unemployment risk between insured and uninsured individuals. Quantitatively, selection on moral hazard is nearly as important as risk-based selection.

JEL codes: C23, D82, J64, J65

Key words: Unemployment, insurance, selection

1 Introduction

Most social insurance systems protecting workers' incomes or consumptions adopt a one-size-fits-all model with mandatory participation and little individual choice. Unemployment insurance (UI) is a prominent example that has duration and level of benefits centrally set. While coverage is broad, UI may fail to accommodate non-traditional workers, leaving many without access to protection (Boeri & Cahuc, 2023; Boeri et al., 2020; Mas & Pallais, 2020). In light of global societal, technological, and labor market shifts—including more diverse populations, fragmented career paths, the rise of gig work and AI, and temporary migration—there is growing policy interest in rethinking the design of social insurance systems.

A central question in this debate is whether social insurance can be made more flexible without sacrificing inclusivity (Barnichon & Zylberberg, 2022; Boeri & Cahuc, 2023; Chetty & Finkelstein, 2013; Feldstein, 2005; Hendren et al., 2021; Le Barbanchon et al., 2024). One key issue is whether participation mandates are necessary, as highlighted by long-standing discussions around health insurance (HI) design in the United States (Einav et al., 2010; Jung & Tran, 2016; Panhans, 2019). Voluntary insurance systems may offer greater flexibility but are susceptible to selection effects.[1] Conversely, when considering mandates for currently uninsured groups—such as covering the self-employed in UI—it is important to evaluate whether expanding the risk pool could reduce average insurance costs.

Landais et al. (2021) make an important contribution analyzing the voluntary Swedish

[1]Theoretical contributions (Chiu & Karni, 1998; Hendren, 2017; Jones, 1986) have raised concerns about voluntary or market-based UI systems, emphasizing their vulnerability to information asymmetries when pooled premiums do not reflect heterogeneous risks.

UI system when asking whether a mandated system can deliver higher welfare than the current system under risk-based selection. Risk-based selection occurs when individuals who privately know they face higher underlying unemployment risk take up insurance.[2] The authors show that the standard Baily (1978)–Chetty (2006) framework for optimal UI design must be adapted to account for selection under voluntary coverage, and find that mandating UI would be suboptimal for certain groups.[3]

However, selection effects can be of a dual nature, as has been emphasized by Einav et al. (2013) in the context of HI. First, risk-based selection occurs when individuals with a higher likelihood of unemployment—due to poor health, or employment in volatile sectors—are more inclined to enroll. Second, selection on moral hazard arises when individuals who expect to adjust their behavior once insured—such as reducing effort at work—are more likely to enroll. The nature of the selection is crucial for the design of policy responses, as Einav et al. (2013) note: cost sharing or monitoring may be more effective against selection on moral hazard, whereas pricing based on observable characteristics is the standard approach to addressing risk-based selection. It is therefore important not only to measure selection effects but also to disentangle their underlying mechanisms empirically.

Our paper is the first to study both risk-based selection and selection on moral hazard in UI using a new empirical identification approach. The context is Denmark, whose voluntary UI system shares important institutional features with the Swedish system. Our panel data at annual frequency are drawn from administrative registers covering the entire Danish resident population between 1980 and 1999. We focus on prime aged men and women. The key outcome is a binary unemployment status indicator, defined

[2] Adverse selection. Risk-based selection may also be advantageous.

[3] See Le Barbanchon et al. (2024) for an extended discussion of the Baily–Chetty formulae.

by registration at a job center—a prerequisite for receiving either UI benefits (for the insured) or social assistance (for the uninsured).

A central empirical challenge lies in distinguishing selection effects from moral hazard, as both mechanisms predict higher unemployment risk among insured individuals. Landais et al. (2021) address this issue by exploiting an exogenous increase in UI premiums, which led some individuals to opt out of UI. In contrast, we leverage a policy reform in 1992 that changed an early retirement (ER) option embedded within the UI system, exogenously encouraging individuals to enroll in UI for reasons unrelated to unemployment risk.

To identify the two types of selection effects, we use a difference-in-differences (DiD) approach that compares unemployment incidence of two groups of new UI enrollees before and after enrollment. We assign those groups as follows: (1) those who enroll to qualify for an early retirement option, and (2) those who enroll for other reasons. Our key assumption is that individuals in the first group are not subject to selection effects, but only to moral hazard. Since both moral hazard and selection on moral hazard arise only after UI coverage begins, any differences in pre-enrollment unemployment rates must reflect risk-based selection. Post-enrollment differences between the two groups capture both risk-based selection and selection on moral hazard, and hence the DiD estimates will uncover selection on moral hazard. To account for potential misclassification in group assignment, we adapt a fuzzy DiD design (Aigner, 1973; Battistin & Sianesi, 2011; De Chaisemartin & D'Haultfœuille, 2018).

Our paper makes three main contributions. First, we provide a comprehensive framework for understanding different types of selection effects in UI and quantify them using an event-study design around UI entry. We complement and extend the findings of Landais et al. (2021), as we identify and measure selection on moral hazard—a dimension that, to our knowledge, has not previously been quantified in the UI context (see Hendren et al., 2021). We document substantial selection effects, accounting for 68 percent of the observed difference in unemployment risk between insured and uninsured individuals.

We show that adverse selection arises both because some individuals have an inherently higher risk of unemployment (i.e., risk-based selection), and because others anticipate higher future unemployment. The latter can occur either through foresight of increased unemployment risk or through anticipated moral hazard effects (selection on moral hazard). Quantitatively, risk-based selection accounts for about two percentage points, while the predicted increase in unemployment adds roughly one and a half percentage points. Importantly, selection on moral hazard can be nearly as large as risk-based adverse selection.

Second, we introduce a novel identification strategy to disentangle behavioral responses (including moral hazard) from selection effects such as risk-based selection and selection on moral hazard. Exploiting exogenous variation in sign-up rates across age groups and calendar years, we estimate both components separately. A key advantage of our approach is that we compare individuals who sign up for UI at the same elapsed time after enrollment, allowing us to account for differential dynamic effects.

Third, we document substantial heterogeneity in selection effects. Selection on moral hazard is particularly pronounced among individuals experiencing life events—such as marriage or childbirth—at the time of enrollment, suggesting that preference changes linked to these events increase susceptibility to moral hazard. We also find significant variation across education and gender: risk-based selection is more prevalent among individuals with at most a high school education, while selection on moral hazard is especially pronounced among women.

The remainder of the paper is organized as follows. Section 2 describes the Danish UI and ER systems and the 1992 reform. Section 3 presents a theoretical model of UI that captures the main features of the Danish system and guides our empirical strategy in Section 4. Section 5 describes the data and the sample. Section 6 presents and discusses the empirical results, and Section 7 concludes.

2 Institutional Setting

In this section we briefly describe the institutional setting. We cover the period 1980–1998 and focus on the unemployment insurance (UI) system and the embedded early retirement (ER) scheme. A key element is a 1992 reform that provides exogenous variation in the UI sign-up rate, which we exploit in our empirical approach.[4]

2.1 Insurance Mechanisms

UI is the main income insurance in Denmark and has roots in the Ghent system (Holmlund & Lundborg, 1999). Along with Sweden, Finland, and Iceland, Denmark is one of the few countries where UI coverage is voluntary. The system is organized around about 35 private, industry- or occupation-specific UI funds. A typical fund is a non-profit organization without selection restrictions for applicants that finances benefit payments through membership fees and government subsidies.[5]

UI benefit duration was generous by international standards: it was 84 months until

[4]This section draws partly on Ejrnæs and Hochguertel (2013, 2014) as well as Economic Council (2011, p. 176–177, Box II.).

[5]Lentz (2009) reports that the average worker pays about one-third of the actual premium, the rest being subsidies.

1996, reduced to 60 months in 1996 and to 48 months in 1998. During the 1990s, activation programs with mandatory participation were gradually introduced, starting within 12 months of first registration for UI benefits. The individual worker fee is independent of earnings,[6] and workers' insurance status is typically unobserved by employers.[7] The benefit level is 90% of previous earnings, subject to a floor and a ceiling. In 1995, the annual ceiling was 132,000 DKK, about half of the median salary of white-collar workers.[8] Benefit eligibility requires at least 12 months of membership in a UI fund (except for individuals who have just finished their education).

Jobless persons not covered by UI benefits, including those who have exhausted the maximum benefit period, can receive social assistance. Social assistance depends on spousal income (means test) and individual circumstances, and is for the vast majority of UI fund members considerably lower than the UI benefit. Eligibility requires that the person is registered as unemployed and actively searching for a job—the same requirement as for jobless persons covered by UI.

The voluntary nature of the UI system arguably leads individuals to enroll when they expect to need protection most. Sign-up rates therefore vary over the business cycle, with age, and across birth cohorts. Figure 1 provides a heat map of UI entry, plotting year of birth against calendar year; age increases from the south-west to the north-east and is constant along any diagonal. Colors change from green over yellow to red as probabilities

[6] Survey of ER recipients by Direktoratet for arbejdsløshedsforsikring 6, kontor 1994: Efterlønsundersøgelsen 1992.

[7] Parsons et al. (2003) report for 1995 that the fees paid by a wage-employed worker amounted to about 3,600 DKK. These figures exclude administration costs, which can vary substantially across UI funds.

[8] Source: The maximum amount of unemployment benefit is stated by "Direktoratet for Arbejdsløshedsforsikring".

rise.

The map shows that older people are less likely to enroll and that higher entry rates are observed in periods with high or rising unemployment, such as 1981 and 1992. There are, however, strong patterns that cannot be explained by age, cohort, or year effects alone. The most salient ones reflect incentives emanating from the ER system, as we explain next.

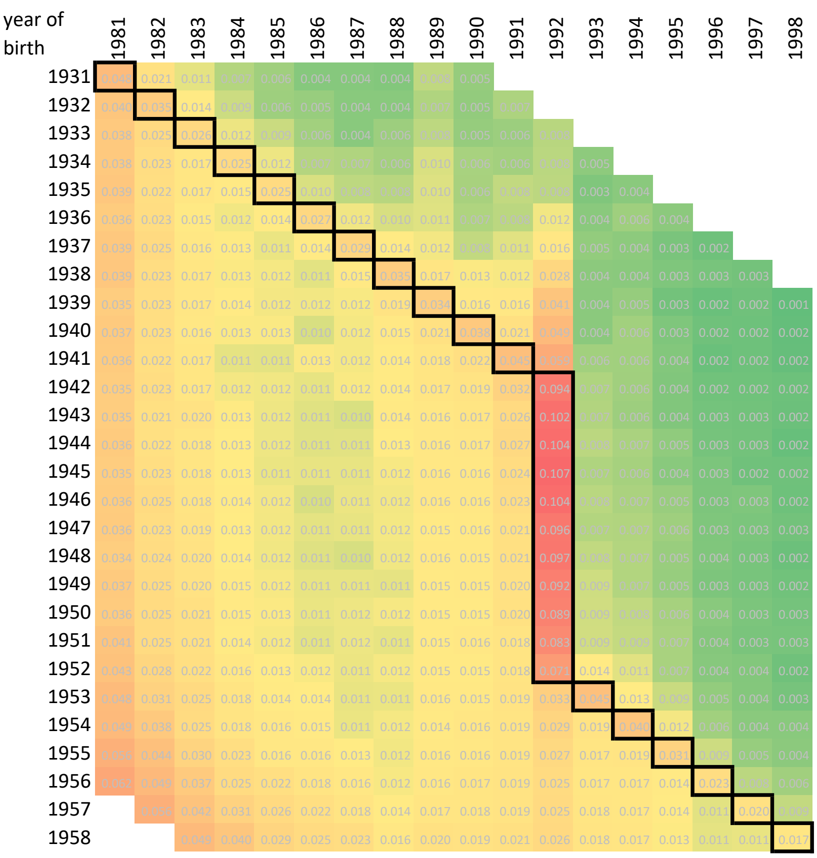
2.2 The Early Retirement System and its 1992 Reform

The Danish ER system was introduced in 1979 and allowed individuals to retire between ages 60 and 67 with a reduced pension. It was organized separately from the compulsory old-age pension system, which provided retirement benefits starting at age 67. Eligibility for ER was open to both blue- and white-collar workers, provided they were members of a UI fund and met minimum membership requirements. During our study period (1980–1998), there was no additional premium associated with benefiting from the ER option; ER was available at zero marginal cost for UI members. Access to ER was not conditioned on previous work experience, and the transition into early retirement was independent of current labor market status. ER benefits corresponded to UI benefits, averaging about 68 percent of previous earnings in 1992. The ER benefit was generally higher than the (flat-rate) old-age pension and was not means-tested. However, once an individual had commenced her or his ER period, other labor market activities—and hence additional income generation possibilities—were largely precluded.[9]

During our study period, the ER system underwent one major reform in 1992. Until then, UI fund members aged 60 and older qualified for ER if they had been enrolled in

[9] Small-scale activities, amounting to not more than 200 hours worked per year, were admissible.

Figure 1: Heat Map of UI Fund Entry



Note: This figure shows empirical entry probabilities of workers into the UI system, per year-of-birth and calendar-year cell, conditional on not having participated in the UI system in the previous year. The color scheme aids in obtaining a quick visual impression of low (green) and high (red) probabilities. Data source: register data, Statistics Denmark.

the UI system for the last 10 years, typically leading to a spike in the enrollment hazard at threshold age 50. Figure 1 shows that the probability of entry almost doubles at this threshold.[10]

The 1992 reform required continued membership of at least 20 (instead of 10) years before retirement, implying that the latest age for joining a UI fund decreased from 50 to 40. Individuals aged between 40 and 50 in 1992 were required to join a UI fund in 1992 and remain members until age 60 if they wished to collect future ER benefits.

Figure 2(a) shows how different cohorts are affected by the age-based rules and the 1992 reform, and Figure 2(b) highlights the groups we define in Section 4 and exploit empirically in Section 6. Comparing Figures 1 and 2 provides clear prima facie evidence that the ER incentive at the ER threshold shifts UI coverage rates, as entry increases substantially for the affected cohort–year cells.

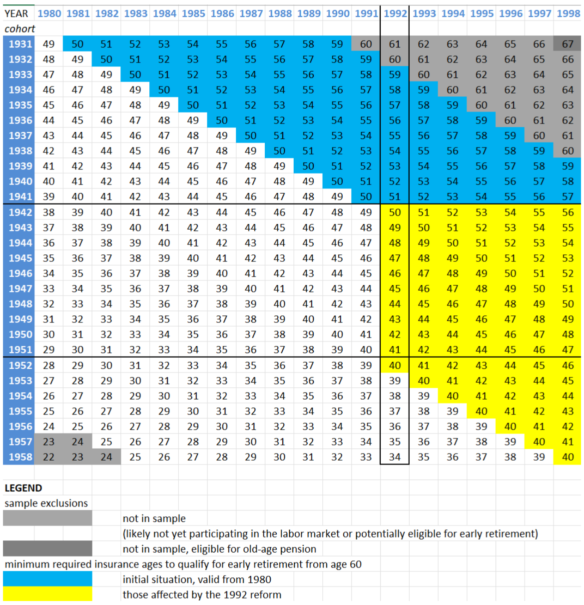
3 A Model of Unemployment Insurance Choice

We present a simple static expected utility model for a risk-averse worker's choice of joining the UI system. The model reflects salient features of the Danish context delineated in Section 2, and we use it to discuss important aspects of our empirical approach.[11]

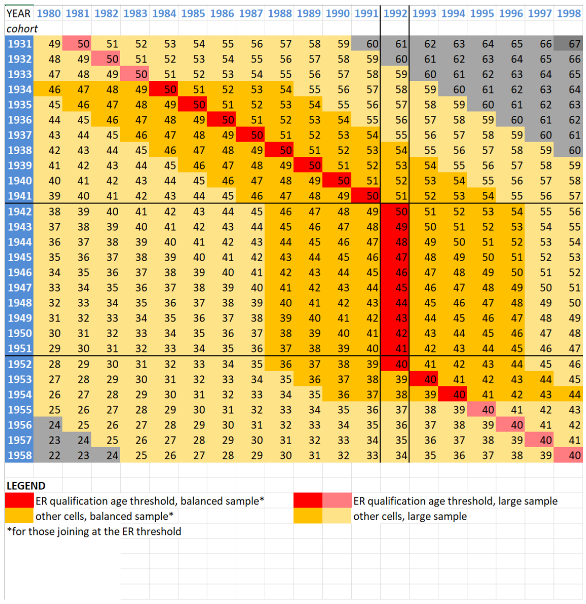
[10] Although it is possible to sign up later, e.g., at 51 and be eligible at 61 for retirement, very few use this option, possibly because information campaigns on the system highlighted the 50-years-of-age threshold.

[11] The present model owes much to our previous work developed in Ejrnæs and Hochguertel (2013), but the question we ask and the empirical approach we take are very different.

Figure 2: Early Retirement Institutions by Year of Birth and Year



(a) Eligibility



(b) ER Threshold

Note: Figure (a) illustrates the cohort-specific age-based membership rules for ER eligibility. The head column contains year of birth, the head row contains calendar year, age is constant along any diagonal. Gray areas are excluded from the data, white and colored areas are included. Blue areas indicate cohorts unaffected by the 1992 reform, yellow areas cohorts that are affected; both colors indicate the ages at which individuals need to be UI fund members for ER eligibility from age 60 on. Figure (b) highlights in red, for each cohort, the latest age to sign up for UI to be fully eligible for ER; our empirical approach contrasts those joining the UI system in the red cells with those joining in the orange cells. The color intensity of the red and orange cells has to do with balancing.

3.1 Expected Utility

We consider a simple two-state environment, employed or unemployed, denoted E and U, respectively.

The individual's utility is a function u of consumption, C, and leisure, l. u is concave in C. Unemployment risk is partially insurable by joining the UI system and paying premium P. Let s indicate the insurance status ($s = 1$ if the individual is insured and 0 otherwise). While in state U, the individual receives UI benefits B if insured, and social assistance A if not insured. Social assistance is lower than the UI benefit and is available without payment of fees. Allowing for additional non-labor income Y^0, the individual's consumption possibilities depend on the following sources: Y^E earnings (in state E), B, A and P. Consumption is state-contingent (we distinguish C^E and C^U) and depends on the insurance status s:

$$C^E = Y^E + Y^0 - P \cdot s, \qquad \text{or} \qquad C^U = Y^0 + A \cdot (1 - s) + (B - P) \cdot s. \tag{1}$$

The unemployment probability $\pi = \pi(\theta, e)$ depends on two factors: an exogenous risk component, $\theta > 0$ and effort $e \geq 0$. Higher exogenous risk or lower effort lead to a higher unemployment probability. Hence, we assume $\pi_\theta > 0$, $\pi_e < 0$ and $\pi_{ee} > 0$.[12] Effort is associated with utility costs (search or time cost, or cost of avoiding employment loss), which we model as λe; $\lambda > 0$ denotes the marginal cost of effort.

Finally, we introduce an additional incentive for joining UI, the early retirement option R, available to insured individuals. By assuming time-separability between today when the insurance choice is made, and the future when the retirement option can be exercised, we model R as additively enhancing utility. β may be seen as a discount factor. Introducing $EV(s, e)$ as the expected utility of insurance status and effort, the problem

[12] We denote partial derivatives by subscripts.

of the expected-utility maximizing individual is then

$$\max_{s=\{0,1\},e} EV(s,e) = \max_{s=\{0,1\},e} (1-\pi(\theta,e)) \cdot u(C^E,0) + \pi(\theta,e) \cdot u(C^U,\gamma) - \lambda e + s\beta R.$$

The budget constraint is directly incorporated into the utility function through state-contingent consumption (1). We can show that the optimal effort as a function of exogenous risk, insurance status, cost of effort and the various income sources is:[13]

$$e = e(\underset{?}{\theta}, \underset{-}{s}, \underset{\leq 0}{\lambda}, \underset{-}{\gamma}, \underset{+}{Y^E}, \underset{-}{A}, \underset{-}{Y^0}, \underset{-}{B}, \underset{+}{P}).$$

Unless we know the sign of $\pi_{e\theta}$ we cannot derive the sign of e_θ. Furthermore, we can show that the insurance decision is affected by the individual risk, the marginal cost of effort, the retirement incentive and the income sources

$$s = s(\underset{+}{\theta}, \underset{+}{\lambda}, \underset{+}{\gamma}, \underset{+}{R}, \underset{?}{Y^E}, \underset{-}{A}, \underset{?}{Y^0}, \underset{+}{B}, \underset{-}{P}).$$

In the empirical model, we do not observe effort but only insurance status and unemployment. For brevity, we shall omit the income sources and premium from the argument list.

$$s = s(\theta, \lambda, \gamma, R, Y^0, Y^E, A, B, P) = s(\theta, \lambda, \gamma, R) \tag{2}$$

$$\pi = \pi(\theta, e(\theta, s, \lambda, \gamma, Y^0, Y^E, A, B, P)) = \pi(\theta, e(\theta, s, \lambda, \gamma)). \tag{3}$$

The exposition will focus on the case of a given set of income parameters.

[13]The sign of the partial derivative of $e()$ with respect to the argument is indicated below the argument. A question mark indicates that the sign is indeterminate without further assumptions. Details of these derivatives are spelled out in Ejrnæs and Hochguertel (2013). The model in this paper is parameterized slightly differently as e and θ are not normalized to be below 1, and we do not impose assumptions on $\pi_{e\theta}$.

3.2 Moral Hazard and Selection

We can now illustrate key aspects discussed in the insurance literature. In our model, moral hazard effects are present if there are costs associated with providing effort: $\lambda > 0$. They imply that insured individuals will, everything else equal, provide less effort compared to non-insured individuals and they will have a higher probability of becoming unemployed.

Insurance choice may have risk-based selection effects if there is heterogeneity in the exogenous risk parameter θ. Under adverse selection, individuals with a higher risk of unemployment will be more likely to sign up for insurance. This will lead to insured individuals being more likely to become unemployed compared to non-insured individuals. We can also have selection on moral hazard, defined by Einav et al. (2013) as "…the possibility that moral hazard effects are heterogeneous across individuals, and that individuals' selection of insurance coverage is affected by their anticipated behavioral response to coverage." In our model this can arise in two ways: (i) if λ is heterogeneous, individuals with high cost of providing effort anticipate that they will provide less effort if insured and therefore take up insurance; (ii) if there is heterogeneity in preferences for leisure, those with a high γ anticipate that they will provide less effort if insured, and they will be more likely to insure themselves.

4 Empirical Implementation

Identification concerns two aspects. For one, moral hazard effects and selection effects are difficult to disentangle because both result in insured individuals being more likely to become unemployed compared to non-insured. In addition, selection effects can be of a dual nature. First, risk-based selection occurs when individuals with a higher likelihood of unemployment are more inclined to enroll in UI. Second, selection on moral hazard arises when individuals who expect to adjust their behavior once insured are more likely to enroll in UI.

4.1 Identifying Selection Effects

To address the first aspect, the often used, or standard approach, considers individuals who are "pushed out" from UI because of an exogenous increase in premium and compares them to individuals who endogenously have chosen never to be insured (see e.g. Einav et al., 2010, Einav et al., 2013 and Landais et al., 2021). Our strategy relies on a "pull factor" that exogenously pulls individuals into UI. We compare the "pulled" individuals with those who endogenously select into UI. In both the standard and our approach, differences in subsequent unemployment between the two groups are an indication of selection effects.

To identify the two types of selection effects, we use a difference-in-differences (DiD) approach that compares unemployment incidence of two groups of new UI enrollees, indexed by g, before and after enrollment. We introduce a binary indicator D and assign those groups as follows: $g = 1$, those who enroll to qualify for an early retirement option have $D = 1$ and we label them "retirement group"; and, $g = 2$, those who enroll for other reasons have $D = 0$ and comprise the "non-retirement group". D is not a treatment indicator. Instead, it signifies the underlying motivation for changing the insurance status.

Our key assumption is that individuals in the $g = 1$ group are not subject to selection effects, but only to moral hazard. We then compare unemployment incidence for groups

$g = 1$ and $g = 2$ before and after UI enrollment. Since both moral hazard and selection on moral hazard arise only after UI coverage begins, any differences in pre-enrollment unemployment rates must reflect risk-based selection. Post-enrollment differences between the two groups capture both risk-based selection and selection on moral hazard, and hence the DiD estimates will uncover selection on moral hazard.

Now consider an individual who is (rationally) not insured, and an event that structurally changes that decision and induces the individual to sign up for UI.[14] Taking the total differential on equation (2) yields:[15]

$$\mathrm{d}s \quad \approx \quad s_\theta \mathrm{d}\theta + s_\lambda \mathrm{d}\lambda + s_\gamma \mathrm{d}\gamma + s_R \mathrm{d}R.$$

This expression highlights the different motivations for signing up: increased exogenous risk of unemployment θ, increased cost of effort λ, increased preference for leisure γ, or the retirement incentive R. Since $s_\theta, s_\lambda, s_\gamma$ and s_R are all positive, observing an individual to sign up ($\mathrm{d}s > 0$) implies that any of θ, λ, γ or R will have increased.[16]

Similarly, we can examine how the unemployment probability π changes for individuals. The total differential of equation (3) is:

$$\mathrm{d}\pi \quad \approx \quad (\pi_\theta + \pi_e e_\theta)\mathrm{d}\theta + \pi_e \mathrm{d}_s e + \pi_e e_\lambda \mathrm{d}\lambda + \pi_e e_\gamma \mathrm{d}\gamma,$$

[14]Insurance decisions are in most cases taken once and not changed later. In our sample, the majority of individuals will be insured, and those that are not will have had the time to consider the option. We essentially study the behavior of individuals in this latter group that may join the UI system after not having been insured for some time.

[15]As s is a binary variable, we take the total derivative of the latent variable s^* where $s = 1$ if $s^* > 0$ and $s = 0$ otherwise.

[16]Clearly, there may be additional preference parameters determining insurance choice that are not specified in our demand function, such as risk aversion. Note that our model only relies on the concavity of the utility function, but does not restrict risk aversion to particular cases.

where $\mathrm{d}_s e = e(\theta, s = 1, \lambda, \gamma) - e(\theta, s = 0, \lambda, \gamma)$ (See Appendix B.1 for details). Accordingly, unemployment is likely to increase for individuals signing up to UI. This follows from the model in Section 3 that has $\pi_\theta > 0, \pi_e < 0$ and all of $\mathrm{d}_s e, e_\lambda, e_\gamma < 0$. The only term that can be negative is $\pi_e e_\theta$, since e_θ is allowed to be positive. The change in unemployment risk can also be further decomposed into (i) moral hazard effects, $\pi_e \mathrm{d}_s e$, (ii) risk-based selection effects, $\pi_\theta \mathrm{d}\theta$, (iii) composite selection on moral hazard effects, $\pi_e e_\lambda \mathrm{d}\lambda + \pi_e e_\gamma \mathrm{d}\gamma$, as well as (iv) a remainder term, $\pi_e e_\theta \mathrm{d}\theta$, reflecting the impact of exogenous risk changes on moral hazard, which can be negative if $e_\theta > 0$.

We operationalize this approach by looking at time differences. Let Δ denote the time-differencing operator. We focus on those individuals that sign up for UI between two adjacent years, such that $\Delta s = 1$. We then know that one of the following must have occurred for those individuals:

$$\Delta\theta > 0 \quad \text{or} \quad \Delta\lambda > 0 \quad \text{or} \quad \Delta\gamma > 0 \quad \text{or} \quad \Delta R > 0.$$

We introduce some assumptions. Assume for the sake of argument that the motivation indicator D is observable. Also, assume that the retirement group $g = 1$ has $\Delta\theta = 0, \Delta\lambda = 0$ and $\Delta\gamma = 0$,[17] and that the non-retirement group $g = 2$ has $\Delta R = 0$. In the retirement group, the change in the unemployment probability for someone signing up for insurance is:

$$\Delta\pi \,|_{\Delta s=1, D=1} \simeq \pi_e \mathrm{d}_s e.$$

The change in unemployment is entirely due to moral hazard as all the selection terms cancel out. In the non-retirement group, we have

$$\Delta\pi \,|_{\Delta s=1, D=0} \simeq (\pi_\theta + \pi_e e_\theta)\Delta\theta + \pi_e \mathrm{d}_s e + \pi_e e_\lambda \Delta\lambda + \pi_e e_\gamma \Delta\gamma.$$

[17] In section 4.3 we are going to relax this assumption.

The DiD parameter will then be

$$\Delta\pi\,|_{\Delta s=1,D=0} - \Delta\pi\,|_{\Delta s=1,D=1} \simeq (\pi_\theta + \pi_e e_\theta)\Delta\theta + \pi_e e_\lambda \Delta\lambda + \pi_e e_\gamma \Delta\gamma, \tag{4}$$

containing only the selection terms.[18] We expect them to be positive under adverse selection. Notice, that this identification strategy does not rely on random selection into the retirement group.

In the empirical application, we do not observe the motivation for why individuals sign up, D. We will instead exploit that we know whether they sign up at the latest possibility to be eligible for the ER program.

4.2 Difference-in-Differences Estimation

We examine the selection effects empirically by embedding the DiD approach in an event study around the event of joining UI (dynamic DID approach).

Denote the event time at which the individuals sign up by $t = 0$. In a balanced design, we follow individuals from T years prior to signing up to T years after: $t = -T, -T+1, \ldots, 0, \ldots, T-1, T$. Define an indicator variable Z_t for the ages at which an individual should be insured in order to be fully eligible for early retirement. Z_t is rule-based and exogenous to individual decisions. We consider individual time series that have exactly one $\Delta Z_t = 1$, i.e., there are no individuals with $Z_t = 0\ \forall t$.

Now, focus on a sample of new enrollees. Consider individuals that join UI (at $t = 0$) when Z changes from 0 to 1, $\Delta Z_0 = 1$ (and $\Delta Z_t = 0\ \forall t \neq 0$). In terms of Figure 2(b),

[18]This result only holds if the two groups initially have the same value of the parameters θ, λ and γ. If not, the moral hazard effect could be different for the two groups. We consider this as a second order effect. Our empirical section explores the sensitivity to conditioning on additional regressors, among them risk-relevant factors such as education and industry.

these fall into the red cells. Conversely, individuals could join UI (at $t = 0$) when Z does not change from 0 to 1, i.e., $\Delta Z_0 = 0$. In Figure 2(b), these are in the orange cells. For the DiD approach, we make the assumption that those with $\Delta Z_0 = 1$ sign up for UI (at $t = 0$) *because* of the retirement motive ($D = 1$) and that those with $\Delta Z_0 = 0$ sign up for UI (at $t = 0$) for other reasons ($D = 0$), i.e., $D = \Delta Z_0$. We relax this assumption in Section 4.3.

For any individual i of group g at event time t, we relate outcome variable Y_{it} (mainly unemployment incidence) to a full interaction expansion of event time and group indicators:

$$Y_{it} = \kappa_0 + \sum_{\tau=-T}^{T} \beta_\tau \mathbf{1}_{(t=\tau)} + \sum_{\tau=-T}^{T} \rho_{\tau;g} \mathbf{1}_{(t=\tau)} \times \iota_g + X_{it}\zeta + \alpha_i + \varepsilon_{it}, \quad t = -T, -T+1, \ldots, 0, \ldots, T-1, T. \tag{5}$$

X_{it} is a vector of additional control variables, α_i denotes individual fixed effects, ε_{it} is an idiosyncratic error term, and ι_g denotes binary variables to flag group membership. To identify the selection effects, it is important to flexibly account for time and age effects, since unemployment incidence varies with both age and business cycle. By exploiting both the ER qualification age threshold (or, ER threshold for short) and the 1992 reform, we obtain variation in the retirement incentive that is not confounded by calendar year, age and cohort effects (for a more elaborate explanation, see Ejrnæs and Hochguertel, 2013, Appendix A). This allows us to include both age effects and calendar year dummies in X. The baseline effects for the various groups, ι_g, will be absorbed by the α_i, as each i belongs to exactly one g. We normalize $\beta_{-1} = 0$, as is customary in event studies.

Our objects of prime interest are defined as differences in Y between the two groups:

$$\delta_{0;1,2} = E(Y_0|g=2) - E(Y_0|g=1) \tag{6}$$

$$\delta_{10;1,2} = E(\Delta Y_1|g=2) - E(\Delta Y_1|g=1). \tag{7}$$

The first object, $\delta_{0;1,2}$ in (6), measures the level differences in unemployment between the two enrollee groups at $t = 0$. The second, $\delta_{10;1,2}$ in (7), is the classical DiD estimator obtained by subtracting $\delta_{0;1,2}$ from a similar expression for $t = 1$. (6) and (7) can be retrieved from linear combinations of the estimated interaction coefficients $\rho_{\tau;g}$ in our regression framework (5). For $\delta_{0;1,2}$, we apply a correction factor reflecting the difference between average group-specific predicted individual fixed effects:

$$\hat{\delta}_{0;1,2} = \hat{\rho}_{0;2} - \hat{\rho}_{0;1} + \left[\frac{1}{N_{g=2}} \sum_{j \in \{g=2\}} \hat{\alpha}_j - \frac{1}{N_{g=1}} \sum_{k \in \{g=1\}} \hat{\alpha}_k \right],$$

where N_g indicates group size. The correction factor in square brackets is needed when we want to interpret $\delta_{0;1,2}$ as structural difference in unemployment between groups driving risk-based selection. The fixed effects approach, as it sweeps the α_i, does not allow to predict the level differences between groups from coefficients $\rho_{\tau;g}$ alone. The correction factor differences out when calculating the time difference in $\hat{\delta}_{10;1,2}$. The crucial assumption for the internal validity of the DiD estimator in (5) is the parallel pre-trends requirement between the groups, which can be easily tested.

We can now explain the main difference between Landais et al. (2021) and our approach.[19] The former authors use an increase in the insurance premium (reduction of government subsidy) between two years to identify the marginal individuals that shift out of UI. They compare the subsequent unemployment of those marginal individuals with that of individuals that never were covered by UI. Our approach compares individuals that opt into UI for non-retirement reasons with individuals that opt into UI because of the ER incentive. This has the advantage that we contrast individuals who all make a transition along the same event time line. Hence, we can examine both the immediate effect and the effect after a couple of years. This may be important if individuals react

[19] We contrast both approaches in Figure B.1 of Appendix B.2.

with delay to the changed incentive, and if the delay is heterogeneous in the population. Furthermore, we can control for inherent differences between the two groups even prior to signing up. Finally, our approach allows us to examine selection on moral hazard.

4.3 Fuzzy Difference-in-Differences

We now discuss how to relax the assumption that those who sign up at $\Delta Z_0 = 1$ only do so because of the retirement motive ($D = \Delta Z_0$). In reality, some individuals in the retirement group will sign up because of other reasons unrelated to early retirement, leading to a misclassification under our assumptions. To correct for misclassified observations, we follow the approach initially proposed by Aigner (1973) and extended to estimating treatment effects by Battistin and Sianesi (2011), and Negi and Negi (2025). The approach is related to the fuzzy DiD estimator introduced in De Chaisemartin and D'Haultfœuille (2018). Details are delegated to Appendix B.3, where we dicuss the underlying assumptions in greater details.

We define $\eta = \Pr(D = 0|\Delta Z_0 = 1)$ as the fraction in the retirement group that is misclassified, and assume that η is independent of age, calendar time and institutional rules (see Appendix B.3, assumptions B.3.2 and B.3.4). We further assume that there are no misclassified individuals in the non-retirement group (see Appendix B.3, assumption B.3.3). In other words, individuals who are motivated by the retirement incentive only sign up at the latest moment in order to be eligible for early retirement. Disregarding misclassification in the non-retirement group is motivated by the fact that UI fund membership involves a non-negligible fee that can be avoided for truly retirement-motivated individuals by not signing up any earlier than strictly needed. There are no other contemporaneous ER-related benefits to be had for this group. If, however, such misclassification were empirically relevant too, the estimator would be biased toward zero. Hence, we see our approach as delivering a conservative estimate (see Aigner, 1973; Battistin & Sianesi, 2011).

Aigner (1973), Battistin and Sianesi (2011) and Negi and Negi (2025) show that the estimated effect $\hat{\delta}$ will be biased toward zero, $\text{plim}(\hat{\delta}) = \delta \cdot (1 - \eta)$. The expression shows that if η is known, we can correct the estimator. As suggested in the abovementioned papers, we estimate η.

The underlying idea is that each calendar year k, and for specific cohorts, one fraction of uninsured will sign up for non-retirement reasons, and another fraction for retirement reasons. We can estimate these two fractions in the following model for insurance entry on a sample of non-insured individuals in $k - 1$ ($s_{ik-1} = 0$):

$$s_{ik} = \nu \mathbf{1}_{(\Delta Z_{ik}=1)} + X_{ik}\psi + v_{ik}, \qquad \text{if} \quad s_{ik-1} = 0, \tag{8}$$

where $X_{ik}\psi$ measures entry for non-retirement reasons, and ν is the additional observed entry due to the ER incentive.[20] Based on estimation of equation (8), we can estimate the share that signs up to the retirement group: $\hat{\nu} + \bar{X}\hat{\psi}$, where $\bar{X}$ denotes the average characteristics of the sample used for estimation of equation (8). Assuming that the share that signs up for non-retirement reasons is constant across years (see assumption B.3.2), we have that this share is estimated by $\bar{X}\hat{\psi}$ and the fraction of misclassified individuals among the retirement group is $\hat{\eta} = (\bar{X}\hat{\psi})/(\hat{\nu} + \bar{X}\hat{\psi})$. The misclassification-corrected estimator for (7) can be obtained through

$$\widehat{\delta}^{FDID}_{10;1,2} = \frac{1}{1 - \hat{\eta}} \cdot \hat{\delta}_{10;1,2} = \frac{\hat{\nu} + \bar{X}\hat{\psi}}{\hat{\nu}} \cdot \hat{\delta}_{10;1,2}. \tag{9}$$

[20]Estimation of the model requires independent variation in $1_{\Delta Z_{ik}=1}$, which is obtained by using the 1992 reform and the ER threshold.

The same correction can be applied to (6). The variance of the estimator is derived in Appendix B.4.

5 Data and Descriptives

5.1 Data Base and Samples

We use longitudinal administrative micro data on the universe of individuals born between 1931 and 1958 who reside in Denmark between 1980 and 1998. Denmark had about 5.1 million residents at the time.[21] We limit the sample to individuals aged 25–59. The data combine information from several government registers and allow us to track individuals and their households at annual frequency.

We select a sample defined by the non-gray area in Figure 2(a) and remove individuals without any change in ER eligibility during 1980–1998. An important part of our empirical analysis relies on an event-study design. We keep a strictly balanced sample of individual time series with 9 observations each.[22] Our analysis targets individuals who were initially uninsured and subsequently enrolled in the insurance scheme.[23]

However, the data reveal two other substantial empirical groups: those who are always insured and those who are never insured. Excluding these groups leads to a distorted

[21] Although we use data to 1999, the variables relating to ER incentives and UI membership are all from 1980–1998 (lagged one year relative to our outcome variable).

[22] In terms of Eq. (5), $T = 4$. We have varied T in robustness checks, available on request.

[23] Figure 2(b) shows that, for instance, an individual born in 1940 and joining at the ER threshold is in the sample at ages 46–54; an individual born in 1940 and joining off the ER threshold can be in the sample between ages 40 and 58 for 9 contiguous years. An individual born in 1931 and joining at the ER threshold cannot be in the balanced sample; an individual born in 1931 and joining off the ER threshold can be in the sample between ages 49 and 59.

sample composition, and we therefore include them in the estimation. Since these two groups do not have an event time for UI enrollment, we define event time with respect to the alternative event of being at the ER threshold (which coincides with the UI enrollment event for the retirement group).[24] We exclude individuals who are ever self-employed and individuals who never had any attachment to the labor force (e.g. those on disability pensions). Our large, unbalanced sample consists of up to 16 million person-year observations from 1.1 million individuals.

A smaller, balanced sample contains about 5.2 million person-year observations from 0.57 million individuals. We use information on UI fund membership, as well as a range of demographic and labor market variables. Our main outcome is the incidence of unemployment in November of a given year. Both insured and uninsured will have to register as unemployed at the local job-center in order to draw either UI benefits or social assistance. We contrast findings from the balanced sample with those from large unbalanced sample.

Our data is organized such that we identify four relevant subsample of individuals according to their insurance history. Most individuals join UI exactly once, and others never. Subsample [1] are those that join at the last moment in order to be eligible for the early retirement option (i.e., at the ER threshold). This group reacts to retirement incentives, and in Section 4.1 was referred to as the "retirement group" among the enrollees. It includes about 50k individuals and almost half a million observations. Subsample [2]

[24]There are two other heterogeneous groups that we exclude from our analyses. One are individuals who were insured already in 1980 and then left the insurance system (either intermittently or permanently). In addition, there are a few individuals that join more than once, we assign them to the retirement group if they also join at the last possibility, else we leave them out. We conduct robustness checks where we gauge the sensitivity of how to classify individuals with multiple insurance spells.

are those that join at any other point in time; this is the "non–retirement group" among the enrollees. It includes about 58k individuals and roughly half a million observations. In addition, the sample also includes two subsamples of non-enrollees: [3] individuals who are always UI insured with more than 400k individuals, and [4] individuals who are never UI insured. Time series for individuals in subsamples [1] and [2] are centered at the UI entry event (4 observations before and 4 after). For subsamples [3] and [4], we instead center around the ER threshold.[25]

5.2 Descriptives

Table 1 reports summary statistics for the main sample and subsamples. In the balanced event-study sample (second column), 63.1 percent of individuals are men and 97.1 percent are Danish nationals. The average age is 45.8. About one third have elementary school as their highest educational attainment, around two fifths are vocationally trained, and about one fifth hold an academic degree (Bachelor's or above). The average annual wage is 299k DKK_{2005} (around 55k USD_{2005}). Roughly four out of five are homeowners, 75% are married or partnered, and three in five have children. More than four fifths are insured in a UI fund.

Relative to the main sample, subsample [1] is of similar age, more likely to be Danish, and has a higher share of academically trained persons. The average wage is 345k DKK_{2005}, the homeownership rate is high (86%), and the incidence of UI insurance is low by construction (57%). Subsample [2] is slightly younger than the total sample, less likely

[25]Figure 2(b) illustrates the difference between the large sample and the balanced sample: all red and orange cells are for the large sample, the more intensely red or orange cells are for the balanced sample (subsamples [1], [3] and [4]; balancing for subsample [2] cannot be easily illustrated, compare footnote 23).

to be male, and in many respects more similar to the total sample than subsample [1]. The average wage is 314k DKK_{2005}, and the incidence of and entry rate into UI insurance are comparable to those in subsample [1]. The statistics show that among the not yet insured about one percent are unemployed. The always-insured group [3] consists of those who are UI insured throughout the 9-year observation window. It is broadly similar to the total sample (but slightly more male). Subsample [4] of never-insured individuals during the 9-year window is better educated than the other three subsamples, and their wages are similar to those in subsample [1].

Table 1: Summary Statistics and Samples

Sample →	Large	Balanced	[1]	[2]	[3]	[4]
Subsample →			joins UI	joins UI	always	never
Variable ↓			at ER	not at ER	in UI	insured
age	42.7	45.8	45.5	45.0	45.8	47.1
Education						
elementary	0.307	0.303	0.175	0.267	0.333	0.237
high school	0.024	0.020	0.027	0.030	0.016	0.029
vocational	0.406	0.426	0.339	0.429	0.460	0.244
short further edu.	0.041	0.042	0.045	0.047	0.036	0.079
bachelor/college	0.156	0.148	0.360	0.172	0.099	0.299
master/PhD	0.052	0.050	0.044	0.040	0.045	0.101
Male	0.588	0.631	0.579	0.573	0.645	0.629
Danish	0.966	0.971	0.980	0.968	0.970	0.972
Experience	16.8	20.0	21.5	19.2	19.9	19.7
Unemployment Incidence	0.054	0.039	0.010	0.030	0.047	0.008
Unemployment Intensity	0.062	0.044	0.010	0.030	0.054	0.008

Continued on next page

Table 1: Summary Statistics and Samples – *continued*

Sample →	Large	Balanced	[1]	[2]	[3]	[4]
Subsample →			joins UI	joins UI	always	never
Variable ↓			at ER	not at ER	in UI	insured
Non-employment	0.059	0.039	0.010	0.031	0.048	0.010
Wage (1000 DKK_{2005})	276	299	345	314	286	344
Houseowner	0.769	0.808	0.858	0.787	0.804	0.813
Partnered	0.710	0.753	0.782	0.744	0.744	0.799
Children in HH	0.613	0.613	0.681	0.621	0.602	0.629
UI fund member	0.830	0.829	0.574	0.601	1.000	0.000
UI fund entry	0.017	0.021	0.112	0.111	0.000	0.000
Spouse UI fund entry	0.027	0.027	0.047	0.041	0.022	0.027
year	1989.1	1990.8	1991.6	1990.1	1990.9	1989.8
Nobs in 1,000	16,268	5,153	452	520	3,702	479
N indiv in 1,000	1,078	573	50	58	411	53

Note: The Table displays summary statistics (means of variables) and defines the various samples used. See text for explanation.

As a first inspection of the data, we perform a descriptive "positive correlation test" to gauge the importance of asymmetric information (Chiappori & Salanié, 2000). We find that uninsured individuals have on average a 5.4 percentage point lower unemployment risk than insured individuals (see Appendix B.5 for details and a breakdown by education and gender). The main part of our empirical analysis characterizes this positive correlation between insurance and unemployment in terms of risk selection.

6 Results

6.1 Event Study Graphs: Difference-in-Difference Results

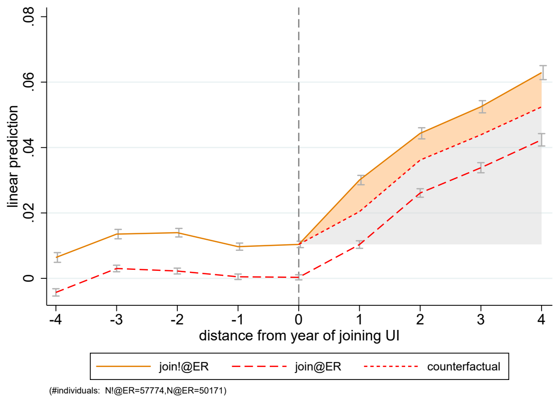
We first present results from our time-of-event study, which provides a graphical representation of the different selection effects of UI. The graphs are based on estimates of equation (5) using the balanced sample of Table 1 with all four subsample groups.[26] We contrast the unemployment experience of the retirement group (subsample [1] in Table 1) and the non-retirement group (subsample [2]), as defined in Section 4.2. Figure 3(a) displays predicted unemployment incidence with 95% confidence intervals over event time, centered at the year of UI entry ($t = 0$). The retirement group is shown as a red dashed line and the non-retirement group as an orange solid line. Figure 3(b) displays the difference in outcomes between the two groups.

Three key findings emerge from Figure 3. First, even before signing up for UI, the non-retirement group has an about one percentage point higher unemployment incidence than the retirement group, indicating adverse selection on inherent risk. Second, after $t = 0$ unemployment increases for both groups; we interpret this common increment as moral hazard. Third, the non-retirement group experiences an additional increase of roughly one percentage point that persists for several years after enrollment. This extra increment, shaded orange in Figure 3(a), reflects selection on moral hazard.

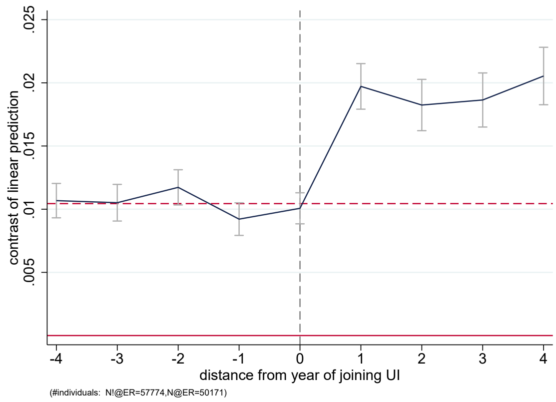
As discussed in Section 4.3, the retirement group may contain misclassified individuals, which would bias selection-effect estimates towards zero. We therefore extend the specification by adding individual fixed effects and correct the DiD estimates from (6) and (7) for misclassification using equation (9). Our estimates suggest that about one

[26]For this illustration, we control for a flexible function of age (a third-order polynomial) and year dummies, but do not include individual fixed effects.

Figure 3: Event Graphs: Unemployment Incidence around the Entry into UI



(a) Level



(b) Difference

Note: Figure (a) shows the evolution of unemployment incidence over time for individuals who join the UI system at event year 0. We use subsamples [1]—[4]. The window is symmetric around the event year and has width 9 years for all individuals. The lines are predictions based on regression model (5) that controls for year dummies and a third-degree polynomial in age, but omits individual fixed effects. We use the balanced sample of Table 1 and display patterns for subsamples [1] and [2]. The dashed red line is drawn for group 1, the orange line for group 2 (corresponding to the intensely red and orange cells of Figure 2(b), respectively). Figure (b) shows the difference between the two groups; the horizontal dashed red line is the average difference before entry. Data source: register data, Statistics Denmark.

third of the retirement group in Figure 3 is misclassified (see Appendix A.1 and Table A.1). Table 2 reports the resulting misclassification-corrected differences between groups. In the baseline specification (row 1), those who sign up for non-retirement reasons have a 2.25 percentage point higher probability of unemployment at $t = 0$ than those who sign up because of the retirement incentive, when neither group is covered by UI.

The first selection effect (i.e., risk-based selection) is thus that the non-retirement group is adversely selected due to (time-invariant) higher unemployment risk. The second selection effect (i.e., selection on moral hazard) arises after UI coverage begins ($t > 0$). Using (9), we estimate an additional 1.41 percentage point increase in unemployment for the non-retirement group (Table 2, row 1, column 3), consistent with selection on moral hazard: individuals who sign up off the ER threshold anticipate that their behavior may change once insured.

Both corrected estimates are larger than the raw differences in Figure 3: the pre-join gap rises from about 1% to 2.25%, and the post-join difference from about 1% to 1.41%.[27] This underscores the importance of correcting for misclassification and including fixed effects.

Finally, we formally test for common linear pre-trends by examining whether the trend coefficients of the two groups are equal before signing up. While visual inspection of Figure 3 suggests parallel pre-trends, the formal test does not reject this hypothesis (Table 2, last column).[28]

[27] Whereas the correction for misclassification can explain the entire increase in the post-join estimate, it only explains around 50 percent of the increase in the pre-join estimate (see Appendix A.1). This suggests that fixed effects account for a large fraction of the additional adverse-selection effect before joining.

[28] Conditioning on functions of age and calendar year is useful because individuals enter at different ages and business-cycle positions (see Figure 1). Not conditioning on these factors does not dramatically

Table 2: Fuzzy Difference-In-Difference Estimates

		Group Diff. at $t = 0$		Diff-in-Diff.		Pre-trend test	
No.	Variation	**coeff.**	**s.e.**	**coeff.**	**s.e.**	**coeff.**	**p.value**
1	Baseline	0.0225	(0.0012)	0.0141	(0.0014)	−0.0003	(0.1973)
	Variation: Alternative unemployment measures						
2	Unempl. Intensity	0.0220	(0.0007)	0.0110	(0.0008)	0.0005	(0.0091)
3	Non-empl. Incidence	0.0165	(0.0011)	0.0124	(0.0013)	0.0008	(0.0128)
	Heterogeneity:						
4	Early joiners	0.0233	(0.0014)	0.0153	(0.0017)	−0.0002	(0.4418)
5	Late joiners	0.0191	(0.0019)	0.0118	(0.0021)	−0.0005	(0.2069)
6	Highest ed.: HS-	0.0262	(0.0033)	0.0222	(0.0036)	−0.0016	(0.0183)
7	Highest ed.: BA+	0.0080	(0.0014)	0.0087	(0.0018)	0.0014	(0.0000)
8	Males only	0.0261	(0.0017)	0.0105	(0.0019)	−0.0006	(0.0976)
9	Females only	0.0163	(0.0018)	0.0182	(0.0021)	−0.0000	(0.9038)
	Variation: multiple treatment groups						
10	join @kids	0.0463	(0.0113)	0.0375	(0.0121)	−0.0004	(0.0948)
11	join @marry	0.0251	(0.0108)	0.0347	(0.0141)	−0.0005	(0.0635)
12	join @home	0.0186	(0.0063)	0.0262	(0.0091)	−0.0024	(0.0000)
13	join @spouse joins UI	0.0179	(0.0045)	0.0124	(0.0061)	−0.0011	(0.0000)

Note: This table shows treatment effects obtained from fuzzy DiD regressions (9) and the fuzzy group difference based on (6), with individual fixed effects and controls for a third-order polynomial in age and year dummies. The balanced sample of Table 1 is used. The notation '@marry' or '@kids', etc. indicates at what time (life event) the individual joins the insurance (at getting married or starting to cohabit, or at the time of arrival of the first child, etc.). The pre-trend test is obtained by testing whether the slopes of the linear trends of the groups prior to joining are the same. The number of observations (in 1,000 individuals) is, for rows 1–2 and 4–5 and 10–13: 573 and 5,153 total; for row 4: 545 and 4,908; for row 6: 185 and 1,665; for row 7: 113 and 1,019; for row 8: 361 and 3,250; and for row 9: 211 and 1,903.

6.2 Robustness Analyses

6.2.1 Alternative Outcomes

We first assess robustness to alternative outcome definitions. As a measure of unemployment intensity, we use the fraction of the year an individual is unemployed. In addition, because receipt of social assistance also requires registered job search, we construct a broader outcome that includes individuals temporarily out of the labor force and classify them as non-employed. If measurement error in unemployment registration were important, non-eligible individuals might fail to appear as unemployed and instead show up as non-employed. On the other hand, the non-employed also contain individuals on sickness benefit or maternity leave, that are not entitled to UI benefits, and for whom we do not expect to see the same selection effect (for a more elaborate discussion see Appendix B.6).

In practice, we find very similar patterns to the baseline, both for unemployment incidence and for non-employment incidence (see Table 2, rows 2 and 3).

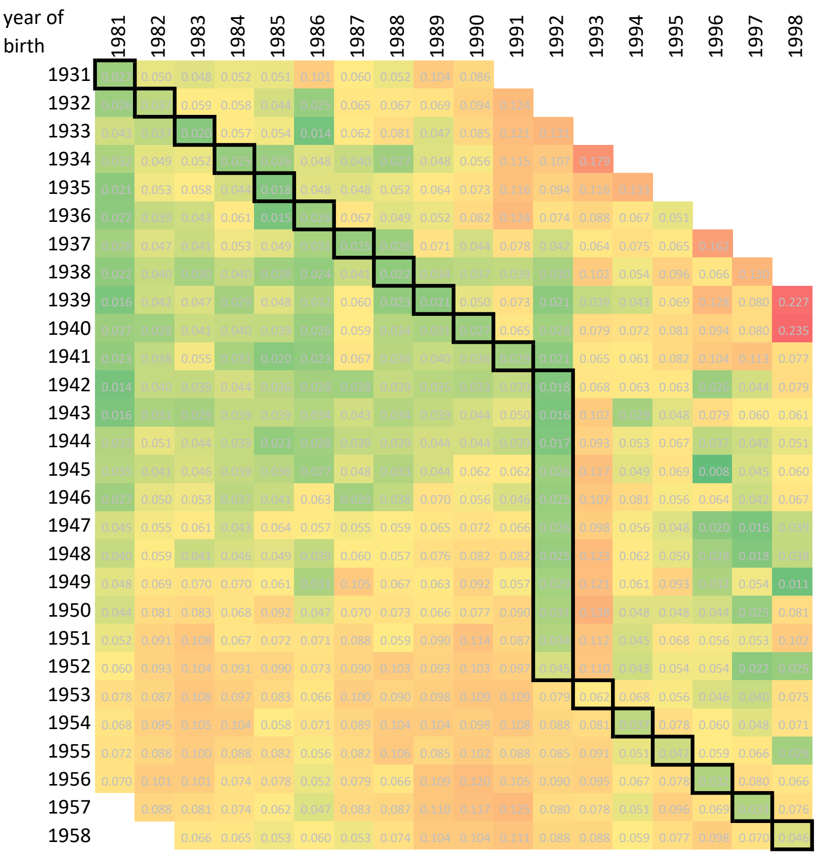
6.2.2 Nonparametric Evidence

To gauge the role of functional form assumptions, we return to the raw data using the large unbalanced sample and condition on signing up for UI in the previous year. We aggregate observations into birth-cohort-by-year cells and compute the average unemployment incidence in the following calendar year. These averages are displayed as a heat map in Figure 4. Cells in which the ER motive is present are indicated with a thick black border. The figure shows that those who sign up because of the ER motive have a lower

alter the patterns in Figure 3 (see Appendix B.7, Figure B.4(a)), but the measured treatment effect is halved, the post-event time pattern diverges more strongly, and the test statistic on pre-trend differences increases. This supports conditioning on at least functions of age and calendar year to avoid conflating their effects with event-time patterns.

subsequent incidence of unemployment (dark-green cells), consistent with our baseline estimates.

Figure 4: Heat Map of Unemployment Incidence



Note: This figure shows empirical unemployment incidence in year $t+1$, per year-of-birth and calendar-year (t) cell, for workers who entered the UI system between years $t-1$ and t. The color scheme provides a quick visual impression of low (green) and high (red) probabilities. Data source: register data, Statistics Denmark.

6.2.3 Robustness with Respect to Sample and Specification

Our main estimates in Table 2 are based on the balanced sample in Table 1, chosen so that we observe both groups that join the UI system over a full 9-year window centered

Table 3: Robustness Checks Fuzzy DiD

	Sample	Balanced		Large Unbalanced	
Nr.		effect	s.e.	effect	s.e.
1	baseline	0.0141	(0.0014)	0.0215	(0.0013)
2	baseline: OLS, Fuzzy DiD	0.0145	(0.0014)	0.0221	(0.0013)
3	baseline: OLS, unweighted DiD	0.0096	(0.0010)	0.0146	(0.0009)
4	year and age dummies	0.0126	(0.0013)	0.0188	(0.0012)
5	neither year nor age	0.0105	(0.0011)	0.0132	(0.0011)
	additional regressors:				
6	— experience polynomial	0.0154	(0.0014)	0.0239	(0.0013)
7	— wage polynomial	0.0131	(0.0014)	0.0210	(0.0012)
8	— experience, wage, industry dummies	0.0068	(0.0010)	0.0063	(0.0010)
9	max. age 55	0.0131	(0.0014)	0.0207	(0.0016)
10	incl. part-time insured	0.0142	(0.0014)	0.0209	(0.0013)
11	lenient: may leave UI	0.0142	(0.0014)	0.0215	(0.0013)
12	strict: only 1st time joiners	0.0130	(0.0013)	0.0236	(0.0015)

Note: The baseline estimate for the balanced sample coincides with row 1 of Table 2 and controls for a third-order polynomial in age, year dummies, and individual fixed effects. Rows 2–8 vary the estimator and the set of controls; rows 9–12 change the sample definition. In the balanced case, the number of observations (in 1,000 individuals) is, for rows 1–5: 573 and 5,153 total; for row 6: 573 and 5,128; row 7: 572 and 5,074; row 8: 572 and 4,905; row 9: 565 and 5,082; rows 10–11: 563 and 5,063; row 12: 548 and 4,932. In the large unbalanced case, the corresponding numbers are, for rows 1–5: 1,078 and 16,268 total; row 6: 1,078 and 15,432; row 7: 1,071 and 15,100; row 8: 1,064 and 14,322; row 9: 797 and 11,934; rows 10–11: 1,071 and 16,200; row 12: 1,022 and 15,358.

at UI entry.

Focusing on the fuzzy DiD estimates, we assess robustness along three dimensions: (1) alternative estimators, (2) different sets of controls, and (3) sample restrictions. Table 3 reports the resulting estimates of equation (9). The estimated selection effect is larger in the large unbalanced sample than in the balanced sample: in the baseline specification it increases from 0.014 to 0.022. Using OLS instead of fixed effects (row 2) has negligible impact, whereas ignoring misclassification (row 3; corresponding to Figure 3) substantially lowers the estimate.

The choice of regressors matters to some extent. Replacing the age polynomial with age dummies (row 4) leads to small changes, while omitting age and year controls (row 5) reduces the estimate more noticeably. In rows 6–8, we add regressors capturing previous labor-market attachment (lagged one year). Including industry dummies, in particular, lowers the estimate in both samples, indicating that individuals who change industry and sign up for UI are negatively selected.

Rows 9–12 vary the sample. Row 9 excludes individuals who ever become older than 55 in our data. Row 10 includes part-time insured individuals. Row 11 allows individuals to leave the UI system after first signup. Row 12 restricts the sample to first-time joiners (removing those who joined earlier than $t = 0$). None of these modifications leads to large changes in the estimated effect in either the balanced or the unbalanced sample.

6.3 Heterogeneity Analyses

In this subsection, we investigate heterogeneous selection effects. As shown in the theoretical model, multiple selection mechanisms may operate and may vary across the population. Results are reported in Table 2, panel ‘Heterogeneity’.

Our approach relies on a distinction in underlying, or latent, retirement motivation. To assess whether we can achieve a clean separation of groups, we first split the non-retirement group according to whether (i) the individual signs up before or (ii) after the ER threshold. In subgroup (ii), those who sign up after the ER threshold has passed will not be fully eligible for ER and, hence, we can more confidently rule out a retirement motive. Subgroup (i), the 'early' joiners, may join in advance in order to qualify for ER. Comparing early and late joiners (rows 4 and 5 of Table 2) shows a slightly above-average effect for early joiners and a slightly below-average effect for late joiners in the first year after joining, but the difference between (i) and (ii) is not statistically significant.[29] This justifies our simple split into a non-retirement and a retirement group.

6.3.1 Heterogeneity in Terms of Education

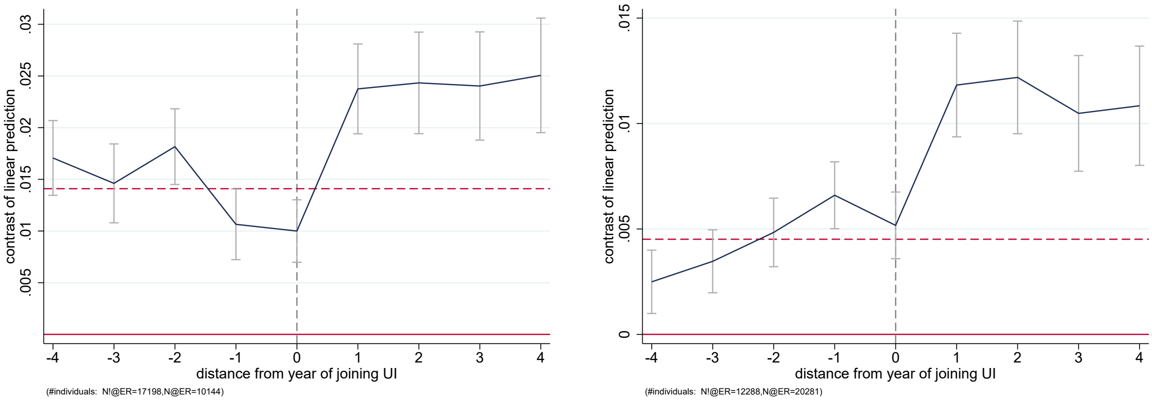
Highest completed education is a key indicator of career profiles and is first-order relevant for differential unemployment experiences.[30]

We consider two broad education groups: (a) at most high school, and (b) at least BA/college. We repeat the analysis of Section 6.1 for each group and display the resulting

[29] Graphical evidence is provided in Figure B.4(b) in Appendix B.7.

[30] A number of studies document differences in unemployment and UI claims across education groups, e.g., Kroft et al. (2016) and Setty and Yedid-Levy (2021). Education shapes entry into different job-contract types and occupations, which may be correlated with cost of effort and preferences for leisure. If individuals with less education have higher costs of effort, our model predicts that they should be more prone to selection effects. Lower education groups may also face unemployment shocks more frequently and thus have a stronger need for insurance, while progressivity in benefit design makes UI relatively less attractive for higher-education groups. Some evidence further suggests that UI claims conditional on unemployment are positively selected on education (Gould-Werth & Shaefer, 2012).

Figure 5: Event Graphs: Unemployment Incidence by Education



(a) at most High School (b) at least Bachelor

Note: This figure replicates Fig. 3, but differentiates between those with a low level of education (high school or below, panel (a)) and a high level of education (bachelor degree or higher, panel (b)). See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

unemployment patterns in Figure 5.[31,32] Table 2 indicates differences in both types of selection. The risk-based selection effect before signing up is 2.62 percentage points for individuals with at most high school and 0.80 percentage point for those with at least a BA/college degree. The selection-on-moral-hazard effect arising after being covered by UI is 2.22 percentage points for the lower-educated group and 0.87 percentage point for the higher-educated group (rows 6 and 7 of Table 2). The fact that we find less selection on moral hazard for the highly educated is consistent with their having lower costs of work effort and weaker preferences for leisure, in line with standard human capital theory. For both education groups, the parallel pre-trends hypothesis is rejected, so these results should be interpreted with caution.

[31] High school alone is not very frequent, as a high school diploma by itself does not imply job qualification. Most workers with a high school diploma therefore have some additional training.

[32] For vocationally trained individuals the results are mixed (figures not shown); there are many subgroups, which are difficult to summarize.

6.3.2 Heterogeneity in Terms of Gender

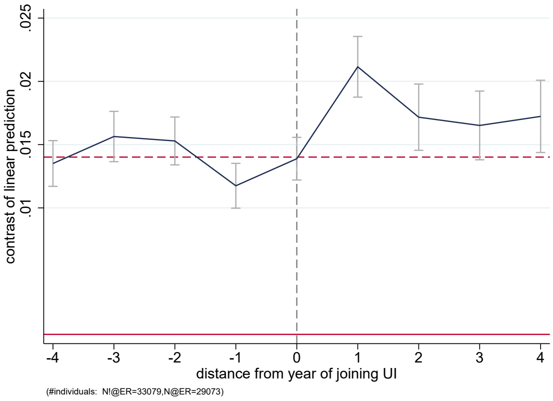
Rows 8 and 9 of Table 2 show different adverse selection patterns for men and women. Men (see also Figure 6(a)) are mainly adversely selected into UI funds based on time-invariant characteristics: the risk-based selection effect is 2.6 percentage points, indicating a persistent difference in unemployment experience between the retirement and non-retirement groups. After signing up, this difference increases by only 1.1 percentage points (selection on moral hazard).

For women, we see a different pattern (Figure 6(b)). The pre-event difference in unemployment between the retirement and non-retirement groups is smaller (1.6 percentage points), but after signing up, those who sign up off the ER threshold have a considerably higher risk of unemployment: the DiD estimate is 1.8 percentage points. This suggests that adverse selection for women is mainly driven by selection on moral hazard, consistent with women being more prone to moral hazard if they face higher costs of effort. Our findings are in line with Ahammer and Packham (2023), who find that women react more strongly to an extension of UI benefit duration and increase their duration of unemployment more than men.

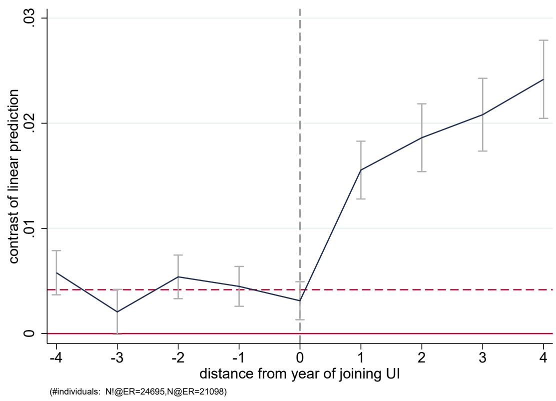
6.3.3 Heterogeneity in Connection with Life Events

Changes in insurance status may be instigated by changes in preference parameters. To explore this, we focus on four life events: (a) first-time home buying, (b) having a first child in the household, (c) marrying (change in adult household composition), and (d) a spouse joining a UI fund. These events may be associated with changes in preferences for leisure or the cost of effort and may prompt individuals to join the insurance system. We choose the event window such that these events occur at $t = 0$. The subgroups are

Figure 6: Event Graphs: Unemployment Incidence by Gender



(a) Men



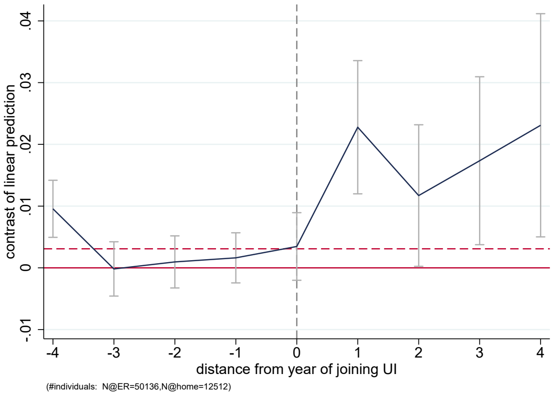
(b) Women

Note: This figure replicates Fig. 3, but differentiates between males (panel (a)) and females (panel (b)). See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

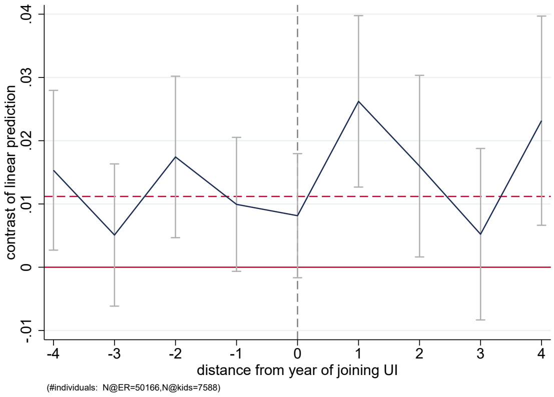
relatively small subsets of the larger group that joins off the ER threshold (see Figure 7(a)–(d)).

Table 2 shows the estimated selection effects associated with the four life events. We find stronger adverse selection for those who either have children or start to cohabit at the time of taking up UI. For the two subgroups we find risk-based selection of 4.6 percentage points for the subgroup becoming parents, and of 2.5 percentage point for the subgroup that starts partnering, and experience larger selection on moral hazard (3.8 and 3.5 percentage points, respectively). Becoming a homeowner is also associated with stronger negative selection effects, though somewhat smaller. The pattern is consistent with life events changing preferences or circumstances—for example, buying a house, having children or cohabiting may restrict mobility, increase the cost of effort or the value of leisure. Such changes raise the value of insurance and strengthen selection on moral hazard. While we do not claim that these life events are strictly exogenous, we

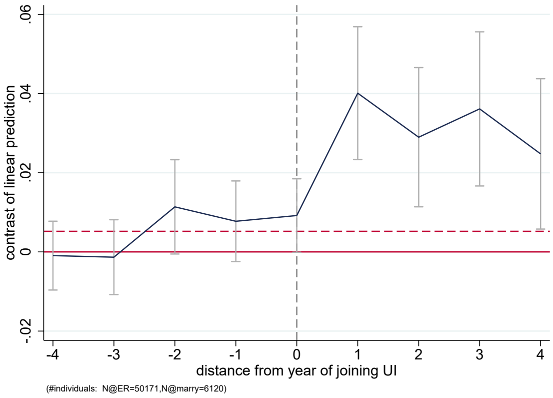
Figure 7: Event Graphs: Unemployment Incidence and Life Events



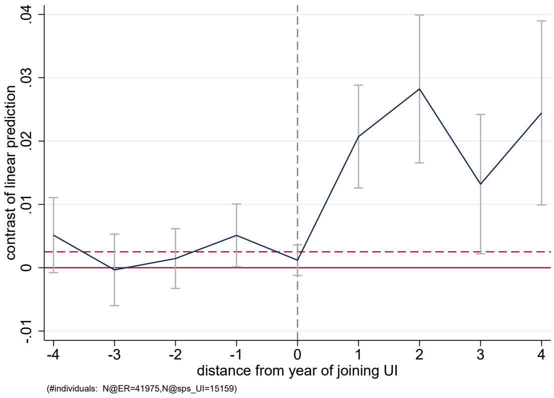
(a) Become home owner



(b) Get children



(c) Become partnered



(d) Spouse insured

Note: This figure replicates Fig. 3, but splits the group that joins the UI system at times other than the ER threshold into a smaller subgroup that joins when experiencing a life-cycle event and a larger subgroup that does not. The reduced subgroup size will explain the larger standard errors (confidence bounds). We contrast the smaller group with the retirement group. The events are: becoming a homeowner (panel (a)), having children (panel (b)), becoming partnered (panel (c)), or the spouse entering the UI system (panel (d)). See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

interpret the documented changes as suggestive evidence of selection on moral hazard.

6.4 Discussion

The positive correlation test in Appendix B.5 shows that insured individuals are 5.37 percentage points more likely to be unemployed than non-insured individuals. This difference can be decomposed into different effects:[33]

$$\underbrace{5.37}_{\Pr(Y|s=1)-\Pr(Y|s=0)} = \underbrace{2.25}_{\text{risk-based selection}} + \underbrace{1.41}_{\text{selection on moral hazard}} + \underbrace{1.74}_{\text{moral hazard + other effects}}$$

The pre-signup difference in unemployment between the retirement and non-retirement groups is 2.25 percentage points, which we interpret as risk-based selection. After signing up, the difference increases by 1.41 percentage points (selection on moral hazard), so selection accounts for $2.25 + 1.41 = 3.66$ percentage points.[34] Hence, adverse selection can explain about two thirds of the total difference between insured and uninsured. We further decompose selection into selection on moral hazard, which accounts for about 26 percent of the difference, and risk-based selection due to time-invariant characteristics, which explains about 41 percent. The latter should be compared to Landais et al. (2021), who report that risk-based selection accounts for 25–30 percent of the difference. One reason why our estimate is larger is that Landais et al. (2021) do not account for misclassification.[35] Citino et al. (2025) investigate Italian workers who manipulate their layoff date to obtain longer UI duration and find that about 80 percent of the increase in unemployment is due to selection on risk, while only 20 percent is due to selection on

[33] In this exercise we assume that the always insured (sample [3], Table 1) behave as the non-retirement group and the never insured (sample [4], Table 1) behave as the retirement group.

[34] This combined estimate has a standard error of 0.0019.

[35] If we do not account for misclassification, the estimated effect is about 50 percent lower (see Appendix A.1) and selection would account for 27 percent of the difference.

moral hazard. Compared to that study, we find that selection on moral hazard accounts for about 39 percent of total selection effects.

Our results indicate that selection-on-moral-hazard effects are substantial and heterogeneous. They are largest when UI entry coincides with major life events such as having children, getting married or starting to cohabit, which may change the cost of (search) effort or preferences for leisure. For instance, if individuals anticipate increased demand for insurance upon becoming parents because of reduced geographical mobility, this is consistent with selection on moral hazard. Looking back at our splits by education and gender, selection on moral hazard appears particularly pronounced among the lower educated and almost fully explains the difference between the non-retirement and retirement groups among women.[36]

Although we cannot recover the exact mechanisms behind the dynamic nature of selection, these effects matter for the design of UI. As explained by Einav et al. (2013) in the context of health insurance, the policy tools to address selection on moral hazard differ from those used to address risk-based selection. When selection on moral hazard is at work, the standard instruments to mitigate moral hazard—such as deductibles, cost sharing, or monitoring—are relevant. In UI, this translates into limited replacement rates and monitoring through labor-market agencies as well as activation programs for the unemployed.[37]

Among the suitable tools to address risk-based selection—where individuals can fore-

[36] A recent study by Brébion et al. (2023) finds that not only employees react to changes in UI but also firms do. We cannot rule out that part of the effects we see are caused by firm behavior, but this seems less likely since employers do not know which workers are covered by UI; there are no employer contributions to the UI system and hence there is no experience rating.

[37] The literature on the optimal design of UI systems (e.g. Hopenhayn & Nicolini, 1997; Kroft & Notowidigdo, 2016; Setty & Yedid-Levy, 2021) has not specifically addressed selection on moral hazard.

see changes in future unemployment risk—is a qualification or waiting period, which alters the incentive to claim UI benefits at the margin. Such a waiting period is natural in a voluntary system, where workers might otherwise sign up at the very day their employment ends, and it may also mitigate dynamic moral-hazard effects that increase unemployment risk over time. Reducing UI replacement rates may discourage benefit claims despite coverage, but this must be weighed against UI's role in consumption smoothing, which varies across workers (Setty & Yedid-Levy, 2021).[38]

7 Conclusions

Global shifts towards alternative work arrangements have fundamentally altered labour markets. Ancillary institutions, such as social income insurance systems, need to be revised to accommodate this changed reality. Many countries have seen a surge in solo self-employment, platform work and other non-traditional employment modes, and workers in such arrangements are often excluded from social insurance coverage (Boeri & Cahuc, 2023; Boeri et al., 2020; Mas & Pallais, 2020).

Voluntary unemployment insurance (UI) is one non-intrusive way to offer protection against joblessness, but such schemes are vulnerable to selection effects rooted in asymmetric information. Our paper characterizes and quantifies these selection effects in a real-world voluntary UI system. Using Danish administrative data, we show that selection into UI is complex and driven by heterogeneity, risk-based selection, and selection

[38]The empirical literature on unemployment search (reviewed in, e.g., Crepon and van den Berg (2016) and Schmieder and von Wachter (2016)) evaluates a wide range of policy measures—search assistance, skill-building, activation programs—that are relevant to moral hazard in various ways. Typically, this literature conditions on unemployment and focuses on spell duration, whereas we study the extensive margin of becoming unemployed. The data and outcomes therefore differ, limiting direct comparability.

on moral hazard. Insured individuals are more than five percentage points more likely to be unemployed than uninsured ones, and roughly two thirds of this difference can be attributed to adverse selection. Individuals who choose to sign up for UI are adversely selected in two ways. First, even before they sign up, they have a higher unemployment rate than individuals who are pulled into UI for exogenous reasons; this accounts for about 61 percent of adverse selection. Second, after signing up, the unemployment risk increases an additional 1.4 percentage points due to selection on moral hazard, accounting for the remaining 39 percent.

We also find suggestive evidence of selection on moral hazard when conditioning on life events such as marriage, having children and first-time home ownership. These events trigger both UI enrolment and higher subsequent unemployment and are plausibly correlated with changes in preference parameters governing moral hazard, providing data-driven evidence that complements more structural approaches that have been proposed for health insurance settings (Cronin, 2019; Einav et al., 2013). Furthermore, selection patterns differ across education groups and gender: the largest effects arise among lower-educated workers, while higher-educated workers are less adversely selected; women are more prone to selection on moral hazard, whereas men are more prone to risk-based selection.

Overall, our results indicate that adverse selection arises both from individual-specific time-invariant characteristics (such as abilities or preferences) and from individuals' ability to foresee higher future unemployment risk or anticipate moral-hazard effects when choosing to insure. This suggests that voluntary UI schemes with continuous enrolment should incorporate minimum insurance periods before benefit eligibility and rely on monitoring, activation and job-search assistance programmes to address selection on moral hazard. Our data do not extend to the period in which such measures are fully implemented, but future work could evaluate these policies in light of the selection-on-moral-hazard effects we document, thereby providing guidance to policy makers seeking to extend insurance coverage to growing groups of currently excluded workers.

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A Appendix

A.1 Estimation of Correction Factor for the Fuzzy Difference-in-Differences

For the fuzzy DiD (9), we estimate the correction factor $(1-\eta)^{-1}$ based on the following linear probability model for individual i signing up for UI at calendar time k:

$$s_{ik} = \nu \mathbf{1}_{(\Delta Z_{ik}=1)} + X_{ik}\psi + v_{ik}, \qquad \text{if} \quad s_{ik-1} = 0. \tag{A.1.1}$$

The correction factor can be calculated as $\hat{w} = (\hat{\nu} + \bar{X}\hat{\psi})/\hat{\nu}$.

Estimation results in Table A.1 show that for the baseline estimation the retirement incentive increases the likelihood of signing up by 18 percentage points and is highly significant. For the retirement group we can calculate the probability of signing up when the incentive is not present using $\bar{X}\hat{\psi}$, where $\bar{X}$ is a vector of average characteristics of the retirement group. This probability is 9 percentage points. Based on these two numbers we can calculate the fraction η of misclassified individuals among the retirement group to be one-third ($\hat{\eta} = 0.09/(0.18 + 0.09) = 1/3$). The weight is then $\hat{w} = (1 - \hat{\eta})^{-1} = (1 - 1/3)^{-1} = 1.5$. Rows 2-10 in Table A.1 correspond to Table 2 and show the estimates for the remaining specifications. The effect of the retirement incentive varies from 15 to 20 percentage points and weights vary between 1.3 and 1.6.

Table A.1: Estimations of correction factor for the Fuzzy DiD

		$\hat{\nu}$		$\bar{X}\hat{\psi}$		$\hat{w}$	
No.		effect	s.e.	effect	s.e.	effect	s.e.
1	Baseline	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)
2	Females only	0.1893	(0.0027)	0.0958	(0.0005)	1.5059	(0.0090)
3	Males only	0.1743	(0.0021)	0.0866	(0.0004)	1.4971	(0.0075)
4	Highest ed.: HS-	0.1530	(0.0031)	0.0936	(0.0006)	1.6116	(0.0146)
5	Highest ed.: BA+	0.2026	(0.0027)	0.0678	(0.0005)	1.3345	(0.0062)
6	late and early joiners (!@ER)	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)
7	join @kids	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)
8	join @marry	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)
9	join @home	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)
10	join @spouse joins UI	0.1803	(0.0017)	0.0903	(0.0003)	1.5007	(0.0057)

Note: Estimation of (A.1.1) using the balanced sample only. See the description of variation in Table 2. The number of observations (in 1,000) are for rows 1,6–10: 573 individuals, and $5,153$ total; row 2: 211 and $1,903$; 3: 361 and $3,250$; 4: 185 and $1,665$; 5: 113 and $1,019$.

B Additional Material (Online Only)

B.1 The Total Derivative of π

As the probability of unemployment, $\pi(\theta, e(\theta, s, \lambda, \gamma))$, depends on both a discrete variable s and continuous parameters θ, λ and γ, we use an approximation based on a binary change in the discrete s. The object we want to evaluate is $\mathrm{d}\pi$, defined as

$$\begin{aligned}
d\pi &= \pi(\theta, e(\theta + d\theta, s = 1, \lambda + d\lambda, \gamma + d\lambda)) - \pi(\theta, e(\theta, s = 0, \lambda, \gamma)) \\
&= \pi(\theta, e(\theta + d\theta, s = 1, \lambda + d\lambda, \gamma + d\lambda)) - \pi(\theta, e(\theta, s = 1, \lambda, \gamma)) \\
&+ \pi(\theta, e(\theta, s = 1, \lambda, \gamma)) - \pi(\theta, e(\theta, s = 0, \lambda, \gamma)) \\
&\approx (\pi_\theta + \pi_e e_\theta) d\theta + \pi_\lambda d\lambda + \pi_\gamma d\gamma + \pi_e (e(\theta, s = 1, \lambda, \gamma) - e(\theta, s = 0, \lambda, \gamma)) \\
&\approx (\pi_\theta + \pi_e e_\theta) d\theta + \pi_\lambda d\lambda + \pi_\gamma d\gamma + \pi_e \mathrm{d}_s e,
\end{aligned}$$

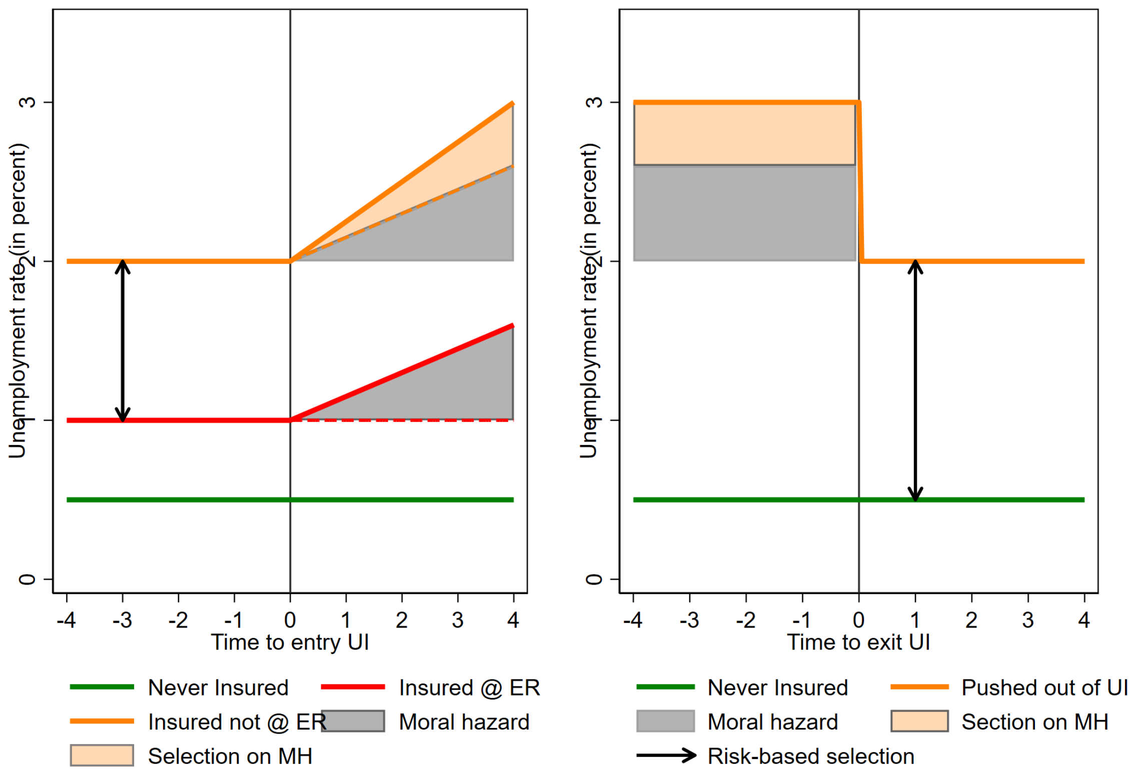
where $\mathrm{d}_s e = (e(\theta, s = 1, \lambda, \gamma) - e(\theta, s = 0, \lambda, \gamma))$. The last equation follows from from taking the total derivatives of $\pi(\cdot, s = 1, \cdot, \cdot)$ keeping s fixed at 1 and taking the differences in effort for $s = 1$ and $s = 0$.

B.2 Comparison between Our Approach and Landais et al. (2021)

Figure B.1 illustrates the differences between our approach and Landais et al. (2021). In our approach (left panel), we consider individuals who opt into UI and compare them with individuals who opt into UI because of the retirement incentive. For the latter group, the timing of entry is exogenously given (by the rules of ER) and therefore this group is not prone to selection effects. Any difference in unemployment rates before entry into UI between the two groups indicates that those opting into UI for other than ER reasons are subject to risk-based selection. After signing up, both groups are subject to moral hazard effects (gray area), which increase the probability of unemployment. If there is gradual adjustment, we expect moral hazard effects to increase over time. In addition, the non-retirement group is also subject to selection on moral hazard (orange area). In our setup we can identify the time invariant selection and the selection on moral hazard.

In Landais et al. (2021), the selection effect is identified from individuals "pushed" out of UI and they are compared to individuals who are never insured. When the two groups are compared, none of them are covered by UI. The selection effect that Landais et al. (2021) identify does not contain the selection on moral hazard.

Figure B.1: Illustration of identification in our approach and in Landais et al. (2021)



Note: This figure illustrates the identification strategy. In our approach (left panel), we identify risk-based selection due to time invariant characteristics (black solid line) and selection on moral hazard (orange area). Landais et al. (2021) identify adverse selection by comparing individuals "pushed" out of UI with never insured individuals.

B.3 Fuzzy Difference-in-Differences

We now show how the estimator correcting for misclassification is related to the estimator proposed by De Chaisemartin and D'Haultfœuille (2018). To simplify the notation, we label our outcome variable Y_t, indexed by year t. Consider two periods for simplicity, $t = 0, 1$. The way we select our sample implies that in the initial period ($t = 0$), all individuals are uninsured ($s_0 = 0$). We then consider those that switch to being insured in period $t = 1$, $s_1 = 1$, such that $\Delta s_1 = 1$.

Our object of interest is the LATE treatment effect, which is the expected change in outcome $Y_{t=1}$ measured in period 1 for the group that signs up at the last possible moment to be eligible for ER ($\Delta Z_{t=1} = 1$), had it ($D_{t=1} = 1$) or had it not ($D_{t=1} = 0$) a retirement motive. In terms of our notation, it is defined through:

$$\Lambda = -(E(\Delta Y_1|\Delta s_1 = 1, D_1 = 1, \Delta Z_1 = 1) - E(\Delta Y_1|\Delta s_1 = 1, D_1 = 0, \Delta Z_1 = 1))$$

De Chaisemartin and D'Haultfœuille (2018) propose to estimate the LATE treatment effect as Wald-DID estimator

$$W_{DID} = DID_Y / DID_D, \tag{B.3.1}$$

where for any random variable Q,

$$DID_Q = \underbrace{E(Q_1|\Delta Z_1 = 1) - E(Q_0|\Delta Z_1 = 1)}_{\text{time difference, group 1}} - (\underbrace{E(Q_1|\Delta Z_1 = 0) - E(Q_0|\Delta Z_1 = 0)}_{\text{time difference, group 0}}).$$

Accordingly, we define the numerator of (B.3.1) as

$$DID_Y = E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 1) - E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 0)$$

and the denominator

$$\begin{aligned} DID_D &= E(D_1|\Delta s_1 = 1, \Delta Z_1 = 1) - E(D_0|\Delta s_1 = 1, \Delta Z_1 = 1) \\ &- E(D_1|\Delta s_1 = 1, \Delta Z_1 = 0) + E(D_0|\Delta s_1 = 1, \Delta Z_1 = 0). \end{aligned}$$

The numerator is straightforward to estimate, but the denominator is problematic because we never observe D. This is in deviation from De Chaisemartin and D'Haultfœuille (2018) where D is treatment status and is at least partially observed. In our case, treatment is the latent reason to insure. We can estimate the denominator if we are willing to impose a set of extra assumptions over and above the ones imposed by De Chaisemartin and D'Haultfœuille (2018). Those are

$$\Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 0) = \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 0) > 0 \quad \text{(B.3.2)}$$

$$\Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 1) = 0 \quad \text{(B.3.3)}$$

$$\Pr(D_t = 1, \Delta Z_t = 1) = \Pr(\Delta Z_t = 1)\Pr(D_t = 1) \quad \forall t = 0, 1 \quad \text{(B.3.4)}$$

Assumption (B.3.2) implies that the fraction signing up for UI conditional on having a non-retirement motive is not depending on year or age. Put differently, it means that a certain fraction of those who have other motives will sign up for UI irrespective of the institutional setting regarding ER. The second assumption (B.3.3) implies that those who value the retirement motive will not sign up when the ER option is not available. The last assumption (B.3.4) implies independence between the institutional rule and the motive.

First, we note that the motive in period 0 is unaffected by the institutional setting in period 1. This implies that:

$$E(D_0|\Delta s_1 = 1, \Delta Z_1 = 1) = E(D_0|\Delta s_1 = 1, \Delta Z_1 = 0).$$

Second, we can calculate the following probability

$$\begin{aligned} E(D_1|\Delta s_1 = 1, \Delta Z_1 = 1) &= \Pr(D_1 = 1|\Delta s_1 = 1, \Delta Z_1 = 1) \\ &= \Pr(\Delta s_1 = 1|D_1 = 1, \Delta Z_1 = 1) \times \frac{\Pr(D_1 = 1|\Delta Z_1 = 1)}{\Pr(\Delta s_1 = 1|\Delta Z_1 = 1)}. \end{aligned}$$

We can determine that

$$
\begin{aligned}
\Pr(\Delta s_1 = 1|\Delta Z_1 = 0) &= \Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D = 1)\Pr(D_1 = 1|\Delta Z_1 = 0) \\
&+ \Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 0)\Pr(D_1 = 0|\Delta Z_1 = 0) \\
&= 0 \cdot \Pr(D_1 = 1|\Delta Z_1 = 0) + \Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 0) \cdot \Pr(D_1 = 0) \\
&= \Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 0) \cdot \Pr(D_1 = 0)
\end{aligned}
$$

where the second equality follows from assumptions (B.3.3) and (B.3.4). Similarly, we can calculate:

$$
\begin{aligned}
\Pr(\Delta s_1 = 1|\Delta Z_1 = 1) &= \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 0)\Pr(D_1 = 0|\Delta Z_1 = 1) \\
&+ \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 1)\Pr(D_1 = 1|\Delta Z_1 = 1) \\
&= \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 0)\Pr(D_1 = 0) \\
&+ \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 1)\Pr(D_1 = 1) \\
&= \Pr(\Delta s_1 = 1|\Delta Z_1 = 0) + \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 1)\Pr(D_1 = 1)
\end{aligned}
$$

where the sign of the last equation follows from assumption (B.3.2). This implies that

$$
\begin{aligned}
\Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 1)\Pr(D_1 = 1) &= \Pr(\Delta s_1 = 1|\Delta Z_1 = 1) - \Pr(\Delta s_1 = 1|\Delta Z_1 = 0) \\
\Pr(D_1 = 1|\Delta s_1 = 1, \Delta Z_1 = 1)\Pr(\Delta s_1 = 1|\Delta Z_1) &= \Pr(\Delta s_1 = 1|\Delta Z_1 = 1) - \Pr(\Delta s_1 = 1|\Delta Z_1 = 0),
\end{aligned}
$$

where the sign of the last equation follows from Bayes' rule. Further,

$$
E(D_1|\Delta s_1 = 1, \Delta Z_1 = 1) = \frac{\Pr(\Delta s_1 = 1|\Delta Z_1 = 1) - \Pr(\Delta s_1 = 1|\Delta Z_t = 0)}{\Pr(\Delta s_1 = 1|\Delta Z_1 = 1)}.
$$

From assumption (B.3.3) it follows that

$$
E(D_1|\Delta s_1 = 1, \Delta Z_1 = 0) = 0.
$$

We can write the Wald DID (B.3.1) when assumptions (B.3.2)–(B.3.4) are satisfied as:

$$\begin{aligned} W_{DID} &= \frac{E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 1) - E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 0)}{\frac{\Pr(\Delta s_1=1|\Delta Z_1=1) - \Pr(\Delta s_1=1|\Delta Z_t=0)}{\Pr(\Delta s_1=1|\Delta Z_1=1)} - 0} \\ &= \Pr(\Delta s_1 = 1|\Delta Z_t = 1)\frac{E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 1) - E(\Delta Y_1|\Delta s_1 = 1, \Delta Z_1 = 0)}{\Pr(\Delta s_1 = 1|\Delta Z_1 = 1) - \Pr(\Delta s_1 = 1|\Delta Z_1 = 0)}. \end{aligned}$$

We can estimate W_{DID} by replacing the means with the sample means.

W_{DID} can be estimated from two regressions. The first regression is based on the fixed effect estimator of model (5). Here we use the estimate $\hat{\delta}$ of δ. The second regression is based on a sample where $s_{it-1} = 0$:

$$s_{it} = \nu \mathbf{1}_{\Delta Z_{it}=1} + X_{it}\psi + v_{it}.$$

W_{DID} can be estimated by

$$\hat{W}_{DID} = (\hat{\nu} + \bar{X}\hat{\psi})\frac{\hat{\delta}}{\hat{\nu}}.$$

B.4 The Variance of the Fuzzy Difference-in-Differences Estimator

The variance of the fuzzy DiD estimator $\widehat{\delta_{FDID}}$

$$\widehat{\delta}_{FDID} = \frac{\hat{\nu} + \bar{X}\hat{\psi}}{\hat{\nu}} \cdot \hat{\delta} = \hat{w} \cdot \hat{\delta}$$

can be calculated as follows.

We start with the variance of the correction term $\hat{w}$:

$$\mathrm{Var}(\hat{w}) = \mathrm{Var}\left(\frac{\bar{X}\hat{\psi}}{\hat{\nu}} + 1\right) = \mathrm{Var}\left(\frac{\bar{X}\hat{\psi}}{\hat{\nu}}\right).$$

We introduce the following notation:

$$\xi' = (\psi', \nu)$$

$$\tilde{X}' = (\bar{X}', 0)$$

$$\iota' = (\mathbf{0}_k, 1)$$

$$\mathrm{Var}(\hat{\xi}) = \Omega$$

where $\mathbf{0}_k$ is a vector of k zeros, $\tilde{X}$ is a $1 \times (k+1)$ matrix. We can then write the numerator as $\tilde{X}'\hat{\xi}$ and the denominator as $\iota'\hat{\xi}$. The correction factor is given by $\hat{w} = \frac{\tilde{X}'\hat{\xi}}{\iota'\hat{\xi}} + 1$. We can now derive the variance of the correction factor $\hat{w}$ by using the delta method:

$$\mathrm{Var}(\hat{w}) = \begin{pmatrix} \frac{1}{\iota'\hat{\xi}} & \frac{-\tilde{X}'\hat{\xi}}{(\iota'\hat{\xi})^2} \end{pmatrix} \begin{pmatrix} \tilde{X}'\Omega\tilde{X} & \iota'\Omega\tilde{X} \\ \tilde{X}'\Omega\iota & \iota'\Omega\iota \end{pmatrix} \begin{pmatrix} \frac{1}{\iota'\hat{\xi}} \\ \frac{-\tilde{X}'\hat{\xi}}{(\iota\hat{\xi})^2} \end{pmatrix}.$$

If we assume that the estimators, $\hat{w}$ and $\hat{\delta}$, are independent, the expression for the variance is given by:

$$\begin{aligned} \mathrm{Var}(\widehat{\delta_{FDID}}) &= \mathrm{Var}(\hat{w} \cdot \hat{\delta}) \\ &= \mathrm{Var}(\hat{w})\mathrm{Var}(\hat{\delta}) + (E(\hat{w}))^2\mathrm{Var}(\hat{\delta}) + (E(\hat{\delta}))^2\mathrm{Var}(\hat{w}). \end{aligned}$$

We calculate the variance of $\widehat{\delta_{FDID}}$ under the assumption that $\hat{w}$ and $\hat{\delta}$ are independent. We provide two checks of this assumption:

- Compare the variance calculated under the assumption of independence with the variance obtained by bootstrap. Furthermore, we estimate the correlation between $\hat{\delta}_1$ and the weights $\hat{w}$, and test if it is significantly different from zero.

- Compare the variance calculated under the assumption of independence with the variance using a split sample approach, which by construction fulfills the assumption of independence (see Angrist & Krueger, 1995).

We compare the standard error of $\widehat{\delta_{FDID}}$ calculated under the assumption of independence (0.0014) with a bootstrapped standard error (0.0014) for our baseline model (see Table B.1), indicating that the independence assumption is fulfilled. Furthermore, we use the bootstrap to calculate the correlation between $\hat{\delta_1}$ and the weights $\hat{w}$ to 0.05 which is insignificantly different from zero.

In the split sample approach, we use a first sample (with half of the individuals) for estimation of w and use a second sample (the other half) for estimation of δ. We calculate the estimate and the standard error for the estimator using the formulae above. In this approach, we have by construction independence between the two estimators. We get a standard error of 0.0020. We compare this standard error to the one obtained when using the same sample to calculate both $\hat{w}$ and $\hat{\delta}$. Here, we get a standard error of 0.0020 (see Table B.1). The standard error is higher in the split sample approach due to the fact that the sample is only half size. Again, these results indicate that the assumption of independence does not invalidate our approach.

B.5 Positive Correlation Test

To assess whether the forces of adverse selection and/or moral hazard may be present at all in the data, we can perform a 'positive correlation test' (Chiappori & Salanié, 2000). We assess whether insured individuals have a higher probability of becoming unemployed. Starting from the large, unbalanced sample in Table 1, we divide the sample into stratified subsamples based on four age groups, two genders, and six education groups. For each combination of age, gender, and education, we split the sample according to the insurance status and calculate the subsequent unemployment risk in the following year.

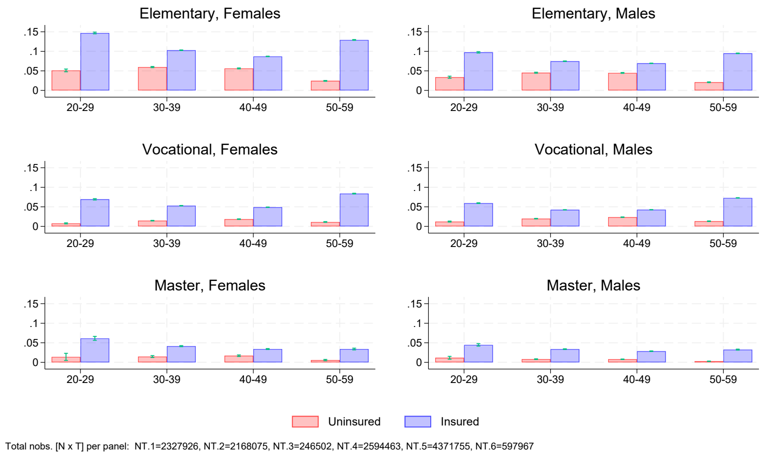
Figure B.2 shows the unemployment incidence for 3 different education groups ele-

Table B.1: Comparisons of estimations of Fuzzy DiD of δ_1

Comparison to Bootstrap estimation		
Estimation	**coeff.**	**se**
Baseline	0.0141	0.0014
Bootstrap	0.0141	0.0014
No individuals (in 1000)	573	
No obs. (in 1000)	5,153	
Comparison to Split sample approach		
Estimation	**coeff.**	**se**
Baseline	0.0122	0.0020
Split sample	0.0121	0.0020
No individuals (in 1000)	287	
No obs. (in 1000)	2,579	

Note: This table displays results for alternative approaches to calculate the standard errors. The baseline is calculated using the Delta method and assumes independence. The bootstrap is based on the approach advocated by Kleiner et al. (2014). The split sample approach draws two independent subsamples of equal size. All the results are based on the Balanced sample. The correlation between the DiD estimator $\hat{\delta_1}$ and weights $\hat{w}$ is 0.05 which is insignificantly different from zero (p-value 0.09).

Figure B.2: UI and Unemployment Incidence: Positive Correlation Test



Note: This figure shows the empirical unemployment incidence in year $t+1$ conditional on insurance status in year t, age group, gender and education level. Data source: register data, Statistics Denmark.

mentary, vocational and Master/PhD. The unemployment incidence displays the usual pattern: women are more likely to be unemployed, unemployment declines with education level and it displays a U-shape in age. Focusing on the difference between uninsured and insured individuals we see heterogeneity across gender and education. In all 48 stratified samples, we find that the insured individuals have on average a higher risk of unemployment compared to uninsured individuals. The difference ranges from 1.7 to 10.5 percentage points. The smallest difference is found for women aged 40–49 with master education as the highest educational attainment, and the largest difference is found for women aged 50-59 with elementary schooling. The mean difference in unemployment incidence is 5.37 percentage points.

B.6 Alternative Outcomes

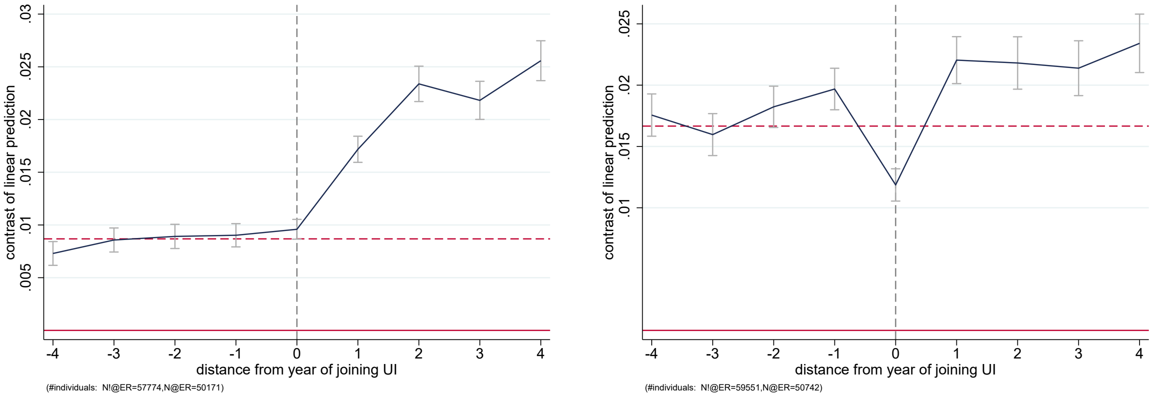
We consider two alternative outcomes: unemployment intensity and non-employment incidence. The first is a continuous measure between 0 and 1 that indicates the fraction of the year an individual has been unemployed, the second is a binary measure of the unemployment status and includes those individuals that leave the labor market. Figure B.3(a) in comparison to Figure 3(b) reveals the close correspondence between the two unemployment measures. Figure B.3(b) shows the incidence of non-employment. The dip at event time zero is driven by individuals who were genuinely out of the labor force—for example, on maternity leave or sickness benefit—and who re-enter the labor force while simultaneously enrolling in UI.

Our preferred outcome measure is the standard outcome that measures if individuals are registered as unemployed. This measure is equivalent to the measure used in Landais et al. (2021) and has the advantage that it is very closely linked to our theory model. In contrast, non-employment will also contain individuals who are out-of-the labor force, e.g., individuals on sickness or maternity leave, which is a considerable fraction of our sample, in particular among women. We have investigated the issue of potential non-registration of unemployment and do not find any indication of that. Furthermore, our approach is not very sensitive to the issues of non-registration as we are comparing individuals with the same insurance status.

B.7 Variations on Specification

Conditioning on age and calendar year is prompted by individuals being at very different ages and positions in the business cycle when confronted with the change in incentives. Figure B.4(a), which leaves that conditioning out, is similar in many respects to Figure

Figure B.3: Event Graphs: Intensity of Unemployment and Non-employment



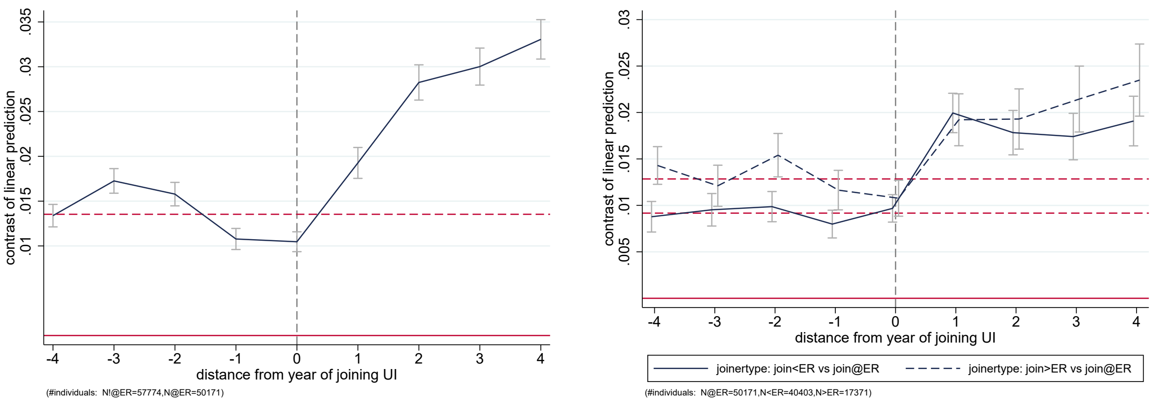
(a) UE Intensity (b) Non emp. Incidence

Note: This figure complements Fig. 3 by focusing on (a) unemployment intensity, and (b) non-employment incidence rather than the unemployment incidence as outcome. See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

3(b), although both the short-run and long-run treatment effects would be very differently assessed.

Another variation concerns the definition of the non-retirement group, based on subsample [2] (see Section 5.1 in the main text). We can split it according to whether [2(i)] the individual signs up before or [2(ii)] after reaching the ER threshold. We expect that those from subgroup [2(ii)] who sign up after the ER threshold will not be eligible for ER and, hence, for that group we are more certain that we can rule out a retirement motive. A comparison across groups [2(i)] and [2(ii)] of Figure B.4(b) reveals that the 'early' joiners have slightly lower unemployment than 'late' joiners before joining, but the difference becomes less pronounced or even disappears after signing up.

Figure B.4: Event Graphs: Variations on Specification



(a) Raw Data (b) Early vs. Late Joiners

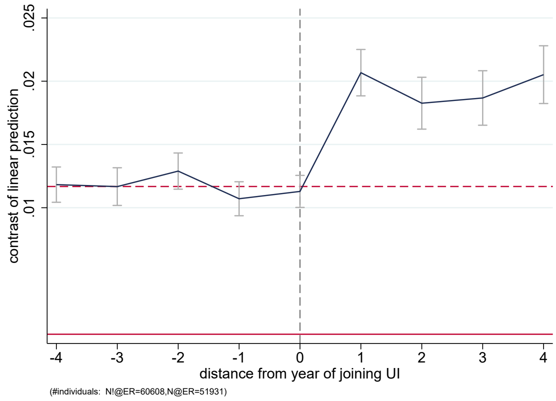
Note: Panel (a) does not condition the underlying regression function on a third polynomial in age and year dummies. In this sense, the figure is based on 'raw' data. Panel (b) splits the non-retirement group and differentiates between those that join the UI system before (dashed) and after (solid) the ER threshold. See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

B.8 Variations on Sample

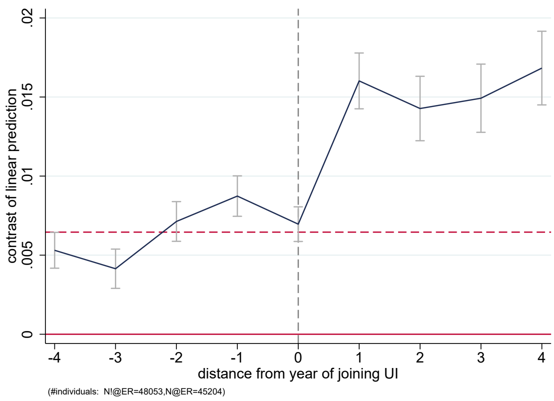
Figure B.5 provides a set of variations on the sample. Figure B.5(a) includes among the individuals that are insured a small sample portion that are 'part-time' insured. The latter insurance mode applies to workers that work part-time. The baseline instead, takes those observations out of the sample. The graph is very similar to the baseline. Figure B.5(b) is based on sample in which joiners to the UI system can also leave again. In that sense, the sample selection is more 'lenient' than the baseline. The difference with Figure 3(b) is minor. The joiners in Figure B.5(c) join the UI system for the first time during their observed time period. In that sense, the sample selection is 'stricter' than in the baseline. But again, compared to Fig. 3(b) the evolution of unemployment is very similar in the post-event period (somewhat less so in the pre-event period).

Figure B.5: Event Graph: Variations on Sample

(a) Incl. Part-time Insured



(b) Allow Leaving after Joining



(c) First-Time Joiners

Note: Panel (a) includes among those insured a small sample portion of individuals that are 'part-time' insured. Panel (b) is based on individuals that join the UI system but can also leave again. Panel (c) is based on individuals that join the UI system for the first time during their observed time period. See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.