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Identifying Risk-based Selection in Social Insurance: New Approaches and Findings

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Abstract

We study risk-based selection into a voluntary unemployment insurance (UI) scheme. To disentangle behavioral effects from selection, we exploit variation in the sign-up induced by an early retirement scheme embedded into the UI system. We combine an event study with a difference-in-difference approach applied to Danish register data to quantify the selection. We find that individuals who sign up for UI are negatively selected in terms of subsequent unemployment. However, we find important heterogeneity across education and gender. In addition, life cycle events (such as buying a first home) point to effects consistent with dynamic selection on moral hazard.

JEL codes: C23, D82, J64, J65

Key words: Unemployment, insurance, selection

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1 Introduction

The design of unemployment insurance (UI) is of prime importance in most developed economies as UI is an important part of the welfare and social insurance system and plays a large role in macroeconomic stabilization and the efficient allocation on the labor market. Global societal challenges like increasingly heterogeneous populations, incomplete and fragmented careers, labor market instability and flexibilization, population aging and migration have sparked considerable interest in redesigning social insurance systems in general, and pension systems, health insurance, and UI in particular.[1] A central question is whether it is possible to make social insurance systems more flexible and at the same time increase efficacy.

One important aspect in this discussion is whether mandating social insurance to citizens is necessary, as the long-standing discussion surrounding health insurance design in the United States exemplifies.[2] While a voluntary insurance system offers more flexibility, it is also vulnerable to risk-based selection. Therefore, before allowing opt-outs from all-encompassing participation, policy makers may want to know whether systemic problems of adverse selection with associated price hikes and underinsurance are likely to occur when individuals can leave the system. Conversely, before mandating insurance to groups that are otherwise uninsured (for instance, the self-employed), policy makers may wish to know the extent to which a larger pool may result in cheaper insurance. Assessing this requires to detect and quantify selection effects in voluntary insurance systems.

We examine such risk-based selection effects in a voluntary UI system as found in some Nordic countries, focusing on the Danish case. Theoretical analyses by Chiu and Karni (1998) or Hendren (2017) are critical of such voluntary systems precisely because of their susceptibility to information-related insurance market failures. What we add is an empirical dimension.

To do so, we set up a theoretical insurance model containing important selection effects such as adverse selection, advantageous selection and selection on moral hazard. Second, we derive a strategy to identify selection effects, and finally we apply this empirical approach to micro data.

The data we use stem from administrative registers covering the entire residential population in Denmark for the period 1980–1999. We focus on a sample of individuals born between 1931 and 1958 living in Denmark in the period 1980–1999. The data contain a large array of important variables that are needed to answer our questions.

When insurance cover can be chosen, theory predicts that different mechanisms can be at work that have insurance and labor market outcomes being correlated. First, wage earners with a higher risk of becoming unemployed are more likely to have insurance cover (adverse selection).[3] Conversely,

[1] See Chetty and Finkelstein (2013), Chetty (2008), Feldstein (2005).

[2] See Jung and Tran (2016) and Panhans (2019).

[3] See Chiappori and Salanié (2000), Cohen and Siegelman (2010), Einav et al. (2010).

advantageous selection (De Meza & Webb, 2001) may arise in this context if individuals with, e.g., high risk aversion and low unemployment risk choose to insure themselves.

The second type of effects are moral hazard effects, i.e., behavioral responses to insurance caused by the mere availability of insurance cover itself. In particular, ex-ante moral hazard, whereby the individual risk of unemployment increases with UI cover, can become first-order important. This can manifest itself through the insured wage earner becoming more likely to quit a job, or otherwise through reduced effort provision with subsequent increased probability of being laid off. Furthermore, selection and moral hazard may occur simultaneously or they may reinforce each other (selection on moral hazard), see Einav et al. (2013). Empirically, it is challenging to disentangle selection effects and moral hazard effects as both predict that insured individuals experience more risk than the uninsured.

Clearly, the precise choice and recommended design of policy measures will depend on the type of selection effects present in the data. For instance, to minimize the impact of selection on moral hazard, cost sharing may be more effective than a mandate.

To disentangle these effects we use some unique features of the Danish UI system. The voluntary UI system in Denmark shares important features with the Swedish system (see Landais et al. (2021) and Kolsrud et al. (2018)). The absence of a mandate implies variation in insurance status in the population. In contrast to the related previous studies that have used price variation to disentangle moral hazard and selection effects, we use a policy reform of an early retirement (ER) option that is embedded in the UI system. We can show that different cohorts becoming exogenously eligible for the ER option at different ages (change of the policy rule) were pulled into joining the UI scheme, getting insurance cover as a side effect.

We rely on a difference-in-difference (DiD) approach combined with an event study approach. We use those who sign up for UI when the ER option becomes available as a comparison group for those who sign up at a different point in time. The idea is that those who sign up at a different point in time are more likely to be subject to risk-based selection while both groups are subject to moral hazard effects. Differencing across groups identifies the selection effects. To allow for imprecision in the assignment, we adapt and use the fuzzy DiD design developed by De Chaisemartin and D'Haultfœuille (2018).

Most related to our work is the study by Landais et al. (2021) who find considerable adverse selection in the voluntary Swedish UI system even when controlling for moral hazard. The focus of their study is to examine whether a mandated UI system dominates a voluntary system. This turns out not to be the case. Our study complements their study by using an alternative identification strategy. Importantly, we study in detail heterogeneous effects not only across gender, but also across education levels. Different education groups show markedly differing unemployment and UI rates in the data.[4]

The contributions of this paper are fourfold. First, we use an event study to look at the dynamics around the entry into UI. This approach allows us to examine different dynamic aspects of adverse selection and we show that adverse selection arises both because some individuals have inherently higher risk of unemployment and because individuals can foresee that the risk of unemployment is increasing. We find that the inherently higher risk of unemployment amounts to 1.5 percentage points while the predicted increase in unemployment is about half a percentage point. This is a marked difference relative to observed unemployment rates in the data.

Second, we consider heterogeneity in risk-based selection. We document considerable heterogeneity across education and gender. Risk-based selection is more prevalent among individuals with at most high school. In addition, we find suggestive evidence of selection on moral hazard when studying life cycle-related events. Those events include marriage, birth of first child, or first-time home-ownership, and affected groups are clearly more likely to become unemployed relative to the group that joins UI when they are instigated by the reform.

Third, we propose a new identification strategy to disentangle behavioral effects (including moral hazard) from selection effects (e.g., adverse selection and selection on moral hazard). Using the exogenous variation in sign up rate both across age and calendar years, we can estimate the risk-based selection of UI on incidence of non-employment, incidence and degree of unemployment. An advantage of our approach is that we compare two groups that both sign up to UI and compare them at the exact same time after sign up. This makes our approach robust to potential biases from differential adjustment periods where individuals have to get used to the new regime.

Finally, this study confirms the findings of Landais et al. (2021) and provides complementary evidence of considerable adverse selection. Looking at the Danish case will not only provide knowledge about risk-based selection effects in Denmark, but also help pointing at larger issues such as the design of an optimal UI system.[5]

The remainder of the paper is organized as follows. In section 2, we discuss the UI system in Denmark and the main reform we are exploiting. In section 3, we present a theoretical model of UI

[4]This point is reinforced by a number of other studies that document differences of unemployment experience and UI claims across education groups, e.g., Kroft et al. (2016) and Setty and Yedid-Levy (2021). Education is a main selection mechanism into job contract types (flexible or permanent) and career choices. Hence, it may be expected that in particular lower education groups are more frequently experiencing unemployment shocks and have a particular need for insurance. Progressivity in benefit design, conversely, makes UI somewhat less attractive for higher education groups. All this suggests that education may play a role in a voluntary UI setting to determine sign-up behavior. Conversely, some evidence exists that UI claims conditional on being unemployed and insured are positively selected on education (Gould-Werth & Shaefer, 2012).

[5]E.g., Hopenhayn and Nicolini (1997), Kroft and Notowidigdo (2016), Setty and Yedid-Levy (2021).

that captures the main features of the Danish system. We use the theoretical model to guide our empirical strategy that is based on a difference-in-difference approach presented in section 4. We present the data and descriptive statistics in section 5. In section 6 the empirical estimation results are presented and discussed, and finally we conclude in section 7.

2 Institutional Setting

In this section we provide a short description of the institutional setting. We cover the period 1980-1998 and focus in particular on the unemployment insurance (UI) and the early retirement (ER) system, which is embedded into the UI system. We focus on an important reform in 1992 that provides us with exogenous variation in the UI sign up rate, which we exploit in our empirical approach.[6]

2.1 Insurance Mechanisms

UI is the main income insurance in Denmark with roots in the Ghent system (Holmlund & Lundborg, 1999). Denmark is together with Sweden, Finland, and Iceland one of the few countries, where UI cover is voluntary.

The insurance system is organized around about 35 private, industry/occupation-specific UI funds. A typical UI fund is a non-profit organization without selection restrictions for applicant members. UI funds finance UI benefits through membership fees, payroll taxes and government subsidies.[7]

Benefit duration was generous by international standards during the period: The duration used to be 84 months until 1996, when it was reduced to 60 months. In 1998 the duration was further reduced to 48 months. During the 1990s, there have been changes to include activation programs with mandatory participation starting within 12 months of first registration for UI benefits. The individual worker contribution is independent of earnings, and workers' insurance status is typically unobserved by employers.[8] For workers the benefit level is 90% of previous earnings subject to a floor and a ceiling. In 1995, the annual ceiling was 132,000 DKK which was about half of the median of white-collar worker salaries.[9] Benefit eligibility requires having been member of a UI fund for at least 12 months (except for individuals that just finished their education).

Jobless persons not covered by UI fund benefits, including those who have exhausted the maximum

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

[7] Lentz (2009) reports that the average worker pays about 1/3 of the actual premium, the rest being subsidies.

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

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

benefit period, can receive social assistance. The social assistance depends on spousal income and individual circumstances, and is for the vast majority of UI fund members considerably lower than the UI benefit. To receive social assistance, the requirements are that the person is registered as unemployed and is actively searching for a job—the same requirement as for jobless persons covered by UI.

The voluntary nature of the UI system leads to people enrolling when they expect to need protection most. Therefore, sign up rates vary over the business cycle, with age, and across birth cohorts. Such patterns are not always very discernible in two-dimensional pictures, but Figure 1 provides what we might call a heat map of UI entry. The column space is made up of the years in our sample (from 1981, since we measure new UI fund members in year t compared to $t-1$). The row space is made up of year-of-birth cohorts, and cells thus allow to define ages. Age changes from the south-west (low) to the north-east (high), and is constant on any diagonal.

The map contains empirical entry probabilities into UI, based on the data described in the next section. Different levels of entry rates are colored from green (low) to red (high). It is clear that older people are less likely to enroll, and higher entry rates are also observed in periods with high unemployment such as 1981 and 1993. There are, however, strong patterns ranging across the figure that are not explained by either age, cohort, or year effects alone. The most salient ones have to do with incentives emanating from the ER system, as we will explain next.

2.2 The Early Retirement System and its 1992 Reform

The Danish ER system has been through one major reform during our study period in 1992.[10] We start by describing the system from 1980 to 1992 and then describe the 1992 reform in detail. The ER system is separately organized from the old-age retirement pension system, which is compulsory and foresees retirement from age 67 onward. Integrated in the UI fund system, however, is an ER option open exclusively to UI fund members, allowing retirement at a reduced pension from age 60 onwards. The ER system was introduced in 1979. One of its main aims was to bring relief to 'worn-out' blue-collar workers. However, access to the ER system is possible for both blue- and white-collar workers as long as they are members of a UI fund.

Until 1992, UI fund members aged 60 and older used to qualify if they had been enrolled in the UI system for the last 10 years, typically leading to a spike in the enrollment hazard at threshold age 50. In the period before 1999, there is no additional premium associated with benefiting from the ER system. In other words, ER was available at zero marginal cost for the interested UI members. ER benefits correspond to the UI benefits, and averaged about 68 percent of previous earnings in 1992.[11] The ER

[10]There was another reform in 1999, outside our observation period.

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

benefit is in general higher than the (flat-rate) old-age pension and is not means-tested. However, once an individual has commenced her or his ER period, other labor market activities, and hence additional income generation possibilities, are largely precluded.[12]

The reform of the early retirement system took place in 1992. This reform concerned a policy shift that required continued membership of at least 20 (instead of 10) years before retirement, implying 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 the UI fund in 1992 and stay members until 60 if they were to collect ER benefits.

Figure A.1(a) illustrates how different cohorts are affected differently by the age-based rules and the 1992 reform. The head column of the figure contains year of birth covering the relevant range in our data, the head row contains years in our data and entries are resulting ages. Gray areas are ages excluded from the data, the white and colored regions are included. The colored areas indicate various regimes that applied for specific cohorts or age groups at different points in time. The figure makes transparent that the ER policy reform affected different age groups differentially. The relation with the heat map of empirical entry rates (Figure 1) is immediately obvious. This comparison provides clear prima facie evidence that the ER incentive shifts UI coverage rates as the entry rate increased by 10 percentage points for the affected cohort/ages. We shall get back to this below in Section 5.

3 A Model of Unemployment Insurance Choice

In this section we present a simple model for the choice of unemployment insurance (UI). We use this model to discuss the important aspects of our empirical approach.[13]

3.1 Unemployment Insurance

The model is a standard microeconomic insurance choice model under risk and reflects salient aspects of the Danish context delineated in Section 2 to first order, for example, we incorporate the extra incentive for signing up for UI stemming from the early retirement option.

We consider a utility maximizing individual with a utility function u defined on current consumption, C, and leisure, l. Since the model is static, consumption equals income. We make the usual assumptions for the utility function: concave and twice differentiable. Income, and hence consumption, depends on the state of the world. We consider two states: the individual is employed, E, or the individual is unemployed, U. To simplify the exposition, we normalize leisure to zero if employed,

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

[13] 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 1: Heat Map of UI Fund Entry

year of birth	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
1931	0.05	0.02	0.01	0.01	0.01	0.00	0.00	0.00	0.01	0.01								
1932	0.04	0.04	0.01	0.01	0.01	0.01	0.00	0.01	0.01	0.01	0.01							
1933	0.04	0.03	0.03	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01						
1934	0.04	0.03	0.02	0.03	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01					
1935	0.04	0.02	0.02	0.02	0.03	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.01				
1936	0.04	0.03	0.02	0.01	0.02	0.03	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.00			
1937	0.04	0.03	0.02	0.01	0.01	0.02	0.03	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.00	0.00		
1938	0.04	0.03	0.02	0.01	0.01	0.01	0.02	0.03	0.02	0.01	0.01	0.03	0.01	0.01	0.00	0.00	0.00	
1939	0.04	0.03	0.02	0.02	0.01	0.01	0.01	0.02	0.03	0.02	0.02	0.04	0.00	0.01	0.00	0.00	0.00	0.00
1940	0.04	0.03	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.04	0.02	0.05	0.01	0.01	0.00	0.00	0.00	0.00
1941	0.04	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.04	0.06	0.01	0.01	0.00	0.00	0.00	0.00
1942	0.04	0.03	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.09	0.01	0.01	0.00	0.00	0.00	0.00
1943	0.04	0.02	0.02	0.02	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.10	0.01	0.01	0.00	0.00	0.00	0.00
1944	0.04	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.03	0.10	0.01	0.01	0.01	0.00	0.00	0.00
1945	0.04	0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.03	0.10	0.01	0.01	0.01	0.00	0.00	0.00
1946	0.04	0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.03	0.10	0.01	0.01	0.01	0.00	0.00	0.00
1947	0.04	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.10	0.01	0.01	0.01	0.00	0.00	0.00
1948	0.04	0.03	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.10	0.01	0.01	0.01	0.01	0.00	0.00
1949	0.04	0.02	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.09	0.01	0.01	0.01	0.01	0.00	0.00
1950	0.04	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.09	0.01	0.01	0.01	0.01	0.01	0.00
1951	0.04	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.08	0.01	0.01	0.01	0.01	0.00	0.00
1952	0.04	0.02	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.07	0.02	0.01	0.01	0.01	0.01	0.00
1953	0.05	0.02	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.04	0.04	0.02	0.01	0.01	0.01	0.01
1954	0.05	0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.01	0.02	0.02	0.03	0.02	0.04	0.01	0.01	0.01	0.01
1955	0.05	0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.02	0.02	0.03	0.01	0.01	0.01
1956		0.03	0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.02	0.02	0.02	0.02	0.01	0.01
1957			0.02	0.02	0.01	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.02	0.02	0.01	0.01	0.02	0.01
1958				0.02	0.01	0.01	0.02	0.01	0.02	0.02	0.02	0.03	0.02	0.02	0.01	0.01	0.01	0.02

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 been participating in the UI system in the year before. The color scheme aids in obtaining a quick visual impression of low (green) and high (red) probabilities. Data source: register data, Statistics Denmark.

$l^E = 0$, and to one if unemployed, $l^U = 1$. Following Chiu and Karni (1998), we introduce a parameter $\gamma \geq 0$ capturing the preference for leisure in the utility function, such that $u = u(C, \gamma l)$.

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 unemployed, the individual receives UI benefits B if insured and social assistance A if not insured. Assistance is lower than the benefit and is available without payment of premia. 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 in state E is conditional on insurance status s and equals

$$C^E = Y^E + Y^0 - P \cdot s$$

and consumption in state U is written

$$C^U = Y^0 + A \cdot (1 - s) + (B - P) \cdot s$$

The unemployment probability π depends on two factors: an exogenous individual risk component, $\theta \in [0, 1]$, capturing both e.g., region-specific unemployment risk, but possibly also macro or industry risks, and secondly, effort $e \in [0; 1]$:[14]

$$\pi = \pi(\theta, e).$$

Higher exogenous risk or lower effort lead to a higher unemployment probability ($\pi_\theta > 0$ and $\pi_e < 0$) and, for given increase of effort, the unemployment probability decreases more when exogenous risks decrease. Put differently, it is easier to prevent unemployment out of own effort when times are good. Effort is associated with utility costs (search or time cost, or cost of avoiding employment loss), which we model as λe, and $\lambda > 0$ denotes the marginal cost of effort.

Finally, we introduce the additional incentive for UI, which is that for some individuals (at some ages) an additional benefit R associated with the insurance is available. R is the option to retire early, and it is available only after the insurance choice is made. By assuming time-separability between today where the insurance choice is made and the future where the retirement option can be exercised, we model R as additively enhancing utility, where β may be seen as a discount factor. The problem of the expected-utility maximizing individual is then

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

The budget constraint, given that we consider a single period with fixed UI system parameters, is directly incorporated into the utility function through consumption. To solve the problem we compare

[14] This is in line with the notation used in Chiu and Karni (1998). Essentially it makes deriving analytical results concerning moral hazard easier, a case we consider below.

the optimal effort provided in the two cases where the individual is or is not insured, and then determine whether utility is higher with or without insurance.

In this model we can show that the optimal effort as a function of insurance status, cost of effort and the various income sources is:[15]

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

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

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

We remark at this stage that the effect of earned and unearned income cannot be signed without making specific assumptions.

In the empirical model we do not observe effort but only insurance status and unemployment:

$$\begin{aligned} s &= s(\theta, \lambda, \gamma, R, Y^0, Y^E, A, B, P) \\ \pi &= \pi(\theta, e(\theta, s, \lambda, \gamma, Y^0, Y^E, A, B, P)). \end{aligned}$$

For brevity, we shall omit the income sources and premium from the argument list in what follows and refer to

$$s = s(\theta, \lambda, \gamma, R) \tag{1}$$

$$\pi = \pi(\theta, e(\theta, s, \lambda, \gamma)). \tag{2}$$

The exposition below hence will focus on the case of a given set of income parameters. The empirical work will condition on income.

3.2 Moral Hazard and Selection

We can now illustrate the key aspects discussed in the insurance literature. They follow directly from how the parameters of the model affect the choice of effort and insurance. 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 hence 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, the individuals with the highest risk of unemployment will,

[15]The sign of the partial derivative of $e()$ with respect to the argument is indicated below the argument. Details of these derivatives are spelled out in Ejrnæs and Hochguertel (2013).

everything else equal, be more likely to sign up for insurance. Again, this will lead to insured individuals being more likely to become unemployed compared to non-insured individuals. Furthermore, in this model 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 if λ is heterogeneous: individuals with high cost of providing effort are more likely to be insured. Selection on moral hazard can also arise if there is heterogeneity in preferences of leisure γ. Since those with a high γ will also provide less effort, they will be more likely to insure themselves.

Advantageous selection (e.g., De Meza and Webb (2001)) can take place, for instance, if individuals with high earnings Y^E are more likely to be insured and face lower probabilities of unemployment. Similarly, such an effect arises if those that are more risk averse are less likely to become unemployed.

4 Empirical Implementation

4.1 Identifying Selection Effects

As discussed above, moral hazard effects and adverse selection effects are difficult to disentangle because both effects result in insured individuals being more likely to become unemployed compared to non-insured. In the following, we will discuss how we detect the selection effects in the context of the theoretical model.

We depart from the situation in which an individual is (rationally) not insured. There may, nonetheless be events that structurally change that decision and make an uninsured individual sign up for UI.[16]

Denote the partial derivative of a function $f(x_1, x_2, ..., x_n)$ with respect to x_j by f_{x_j}. Taking the total differential on equation (1) yields

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

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 all are positive, observing an individual to sign up ($\mathrm{d}s > 0$) implies that any of θ, λ, γ or R will have increased.[17]

[16] Insurance decisions are in many cases taken once and not reviewed 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).

[17] Clearly, there may be additional preference parameters of relevance 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 such as constant HARA classes, and we do not wish to parameterize risk aversion any further.

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

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

Similarly, we can examine how the unemployment probability will change for individuals. The total differential of equation (2) is:

$$\mathrm{d}\pi \quad = \quad (\pi_\theta + \pi_e e_\theta)\mathrm{d}\theta + \pi_e e_s \mathrm{d}s + \pi_e e_\lambda \mathrm{d}\lambda + \pi_e e_\gamma \mathrm{d}\gamma.$$

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 $e_\theta, e_\lambda, e_\gamma < 0$. The change in unemployment risk can also be further decomposed into (i) moral hazard effects, $\pi_e e_s \mathrm{d}s$, (ii) adverse 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.

In the empirical application, we use a difference-in-difference approach for identification. To introduce the approach, we make a distinction between those that sign up only because of a change in the retirement incentive between two periods, ΔR, and those that sign up exclusively for any of the other reasons considered above. We flag the first group using a binary indicator $D = 1$ and label it "retirement group", and the second group by $D = 0$ and label it "non-retirement group". D therefore signifies the underlying motivation for changing the insurance status. The retirement group is assumed to have $\Delta\theta = 0, \Delta\lambda = 0$ and $\Delta\gamma = 0$, the non-retirement group is assumed to have $\Delta R = 0$. In group $D = 1$, the change in the unemployment probability is:

$$\Delta\pi \,|_{\Delta s=1, D=1} \simeq \pi_e e_s.$$

Interpreted in terms of our model, the change in unemployment is entirely due to moral hazard as all the selection terms cancel out. If we compare them with the non-retirement group, $D = 0$, we have

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

The difference-in-difference parameter will then be

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

containing only the selection terms. We we expect them to be positive under adverse selection.

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

4.2 Difference-in-Difference Estimation

Empirically, we examine the selection effects by combining the event study with a difference-in-difference approach. In this exercise, we focus only on individuals that sign up for UI and whom we can follow before and after signing up. To simplify the notation, we denote the time at which the individuals sign up at event time $t = \tau$. We then follow individuals from T years prior to signing up to T years after: $t = \tau - T, \tau - T + 1, \ldots, \tau, \ldots, \tau + T - 1, \tau + T$. We define a variable Z to be an indicator for the ages at which an individual should be insured in order to be eligible for early retirement. This means that ΔZ only takes the value 1 once for each individual time-series. Z is rule-based and exogenous to individual decisions. In Figure A.1(b), ΔZ is represented by the red cells.

In line with what we have outlined above, we divide the individuals who sign up into two subgroups: those who sign up at the latest age to be fully eligible for early retirement ($\Delta Z_\tau = 1$) and those who sign up at another point in time ($\Delta Z_\tau = 0$). The idea here is that among those who sign up at the latest age for being eligible for early retirement we will have a large fraction who only sign up because of the retirement motive. We shall relax this assumption in the next subsection.

We label our outcome variable Y (which in most of our analyses relates to the degree of unemployment). Our object of prime interest is

$$\Lambda = E(\Delta Y_{\tau+1} | \Delta Z_\tau = 0) - E(\Delta Y_{\tau+1} | \Delta Z_\tau = 1).$$

This can be estimated as a classical Difference-in-Difference estimator in a regression framework allowing for additional control variables X and individual specific effects α_i. Our main specification reads:

$$\begin{aligned} Y_{it} &= \beta_0 + \beta_1 \mathbf{1}_{(t>\tau)} + \delta \mathbf{1}_{(t>\tau)} \times \mathbf{1}_{(\Delta Z_{i\tau}=1)} + X_{it}\zeta + \alpha_i + \varepsilon_{it}, \\ & \qquad t = \tau - T, \tau - T + 1, \ldots, \tau, \ldots, \tau + T - 1, \tau + T \end{aligned} \tag{3}$$

where δ captures the difference-in-difference estimator.[18] $\mathbf{1}_{(\cdot)}$ denotes an indicator function taking value 1 if the argument is true and zero otherwise. In the empirical estimations, we include functions of age and calendar year among the set of regressors. The crucial assumption for the difference-in-difference estimator is the parallel pre-trends requirement. We check this by estimating

$$Y_{it} = \beta_0 + \sum_s \beta_s \mathbf{1}_{(t=s)} + \sum_s \delta_s \mathbf{1}_{(t=s)} \times \mathbf{1}_{(\Delta Z_{i\tau}=1)} + X_{it}\zeta + \alpha_i + \varepsilon_{it}$$

and testing the hypothesis $\{\delta_s = 0\}\ \forall s \leq \tau$ for the before-event time period. Finally, we also present graphical evidence based on the event study approach.

Notice that this approach is very different from studies that use policy reforms to capture price variation when identifying adverse selection (e.g., Einav et al. (2010), Einav et al. (2013). For instance,

[18] Notice that δ is negatively related to Λ.

Landais et al. (2021) use an increase in the insurance premium (elimination of state 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. The advantage of our approach, on the other hand, is that we consider individuals who all make a transition, and we compare them at the exact same time after signing up. This can potentially be important if it takes time for individuals to react to the change and if the delay is heterogeneous in the population. Hence, we can examine both the immediate effect and the effect after a couple of years. Furthermore, we can control for inherent differences between the two groups even prior to signing up. Finally, our approach allows us to examine the dynamic process into the future.

4.3 Fuzzy Difference-in-Differences

In this subsection we discuss how we can apply the fuzzy DID approach proposed by De Chaisemartin and D'Haultfœuille (2018).

The reason for considering the approach is that we cannot directly observe the motivation indicator D for signing up. However, we do know whether individuals sign up at the last moment (in terms of age, or year) in order to be eligible for full ER benefits. In reality, some individuals in the group that signs up at the last moment will also sign up because of other, non-early-retirement related reasons. In other words, those who sign up when $\Delta Z = 1$ are more likely to sign up because of the retirement motive.[19]

In particular, we can show that the De Chaisemartin and D'Haultfœuille (2018) Wald-DID estimator, W_{DID}, can be recovered as the ratio of parameters from two different regressions. The data is selected on those that are not insured to begin with. The first regression is based on a subsample where $\Delta s_{it} = 1$ (switchers) and follows the specification in equation (3). The second regression then specifies for all who have $s_{it-1} = 0$

$$\Delta s_{it} = \gamma_0 + \gamma_1 \mathbf{1}_{(\Delta Z_{it}=1)} + v_{it}.$$

W_{DID} can be estimated by

$$\widehat{W}_{DID} = -(\hat{\gamma}_0 + \hat{\gamma}_1)\frac{\hat{\delta}^{FE}}{\hat{\gamma}_1}.$$

Details are delegated to Appendix A.3.

[19] In the notation of De Chaisemartin and D'Haultfœuille (2018), Z is a treatment(1)/control(0) group indicator. It is important to note that some units in group $Z = 1$ will not get treated ($D = 0$), and some units in group $Z = 0$ will get treated ($D = 1$). This is the cause of the fuzziness of the De Chaisemartin and D'Haultfœuille (2018) approach. They have in mind incomplete compliance. We have in mind a distribution of D over Z due to unobserved preference variation.

5 Data and Descriptives

5.1 Data Base and Samples

We use longitudinal administrative register data on the universe of individuals born between 1931 and 1958, and that reside in Denmark between 1980 and 1998. Denmark had about 5.1 million residents at the time.[20] We limit our sample to individuals aged 25–59 in the period 1980–1998.

The data is a combined administrative data set from a number of different government authorities, allowing to track individuals and their households over time at annual frequency.

Applying the age, calendar year, and year-of-birth restrictions, lets us select a sample defined by the indicated area in Figure A.1(a). In particular, we remove those without a change in ER eligibility during the entire period 1980–1998. We delete those individuals that were ever self-employed in this sample. Our main sample then consists of 17 million person-year observations from 1.1 million different individuals.

In our difference-in-difference analyses, we only keep a strictly balanced sample of individuals with 9 observations each, centered around the ER change (4 observations before, 4 after). For instance, an individual born in 1940 is in the sample when he or she is 46-54 years old. We retain 5.3 million person-year observations from 0.59 million individuals.

We also use information on membership in a UI fund, as well as a range of demographic and labor market variables. To study the mechanisms, we analyze the incidence of unemployment, the annual unemployment degree and incidence of non-employment as outcome variables. The degree of unemployment is a continuous variable ranging from 0 to 1, representing the fraction of the year a person is unemployed. The measure differs from the more commonly used binary indicator of unemployment incidence since it reflects better the length of experienced unemployment spells (or, intensity of or exposure to unemployment). The degree is 0 for those that had no unemployment in the past year, and 1 for those that have been unemployed for the entire year.

5.2 Descriptives

In Table 1, we show the summary statistics of the samples. Our event graph sample (second column in Table 1) consists of 63.4 percent men, and of 97.0 percent Danish nationals. The average age in the sample is 45.7. About one third of individuals have elementary school as highest educational attainment, two-fifth are vocationally trained, and about one in five has an academic degree (Bachelor's degree or above). The average wage is 299k DKK_{2005}.[21]

[20] Although we use data to 1999, the variables relating to ER incentives and UI membership are all from 1981-1998 (lagging one year after our outcome variable).

[21] The number corresponds to about 55k USD_{2005}

Four out of five are home owners, 75% are married or partnered, and three in five have children. More than four fifth are insured in one of the UI funds.

The subsamples we consider are (i) a small subsample representing our retirement group, (ii) a slightly larger subsample representing our non-retirement group, (iii) a large subsample of individuals who are always UI insured and finally (iv) is a sample of those who are never UI insured or leave the UI fund. Most of our evidence is based on analyses of subsamples (i) and (ii).

Subsample (i) contains individuals that will sign up at the year in which $\Delta Z = 1$ during a 9-year observation window. This sample contains 51k individuals and is almost half a million total observations. The subsample is the same age as the main sample, more likely to be Danish, and in particular has an above-average percentage of academically trained persons. The average wage is 346k DKK_{2005}. The home owner percentage is quite high (85%), and the incidence of UI insurance is low by construction, amounting to 56%.

Subsample (ii) contains individuals that will sign up at a year in which $\Delta Z = 0$ during a 9-year observation window. This sample contains 60k individuals and is good for a half million total observations. The subsample is 1 years younger than the total sample, less likely to be male, and is in many respects more similar to the total sample than the comparison group in (i). The average wage is 314k DKK_{2005} and is higher than in the other samples. The incidence of UI insurance is comparable to that of the comparison group.

The always insured group (iii) with nearly half a million individuals, has everyone UI insured all the time during the 9-year observation window. It is in many respects comparable to the total sample (but somewhat more male).

5.3 Positive Correlation Test

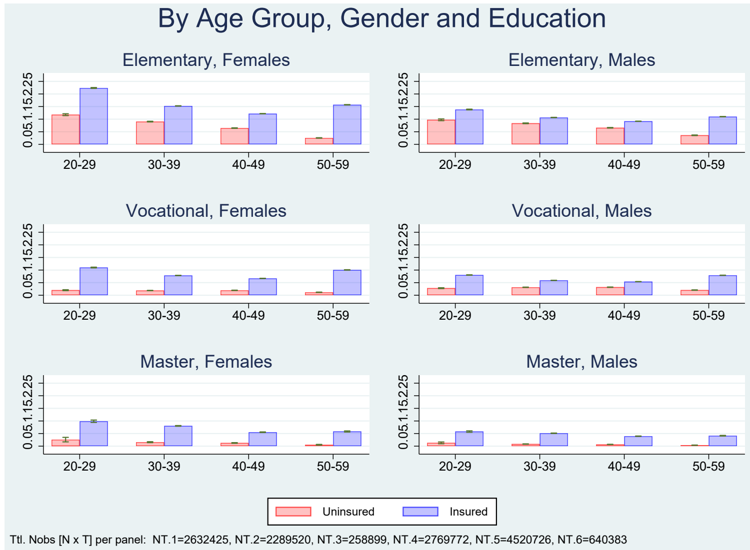
To assess whether the forces of adverse selection and/or moral hazard may be present at all in the data, we can perform a 'positive correlation test' (Chiappori & Salanié, 2000). We simply want to know whether those that are insured have a higher probability to become unemployed. We focus on individuals aged 20–59, not self-employed or being an employer, and being either full-time insured or not at all. 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. 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 2.1 to 13.2 percentage points. The smallest difference is found for men aged 40–49 with vocational schooling as the highest educational attainment, and the largest difference is found for women aged 50–59 with

Table 1: Summary Statistics and Samples

Sample →	Large	Balanced	(i)	(ii)	(iii)	(iv)
Subsample →			joins UI	joins UI	always	never joins,
Variable ↓			at ER	not at ER	in UI	or leaves
Age	42.9	45.7	45.5	44.5	45.7	46.8
Education						
elementary	0.324	0.304	0.179	0.269	0.332	0.249
high school	0.025	0.020	0.027	0.031	0.016	0.029
vocational	0.403	0.426	0.353	0.437	0.459	0.251
short further edu.	0.041	0.041	0.045	0.046	0.035	0.073
bachelor/college	0.154	0.150	0.350	0.174	0.104	0.274
master/PhD	0.053	0.059	0.046	0.041	0.053	0.123
Male	0.579	0.634	0.589	0.580	0.647	0.630
Danish	0.963	0.970	0.980	0.969	0.968	0.969
Experience$_{t-1}$	16.7	19.8	21.3	19.0	19.8	19.3
Unemployment	0.074	0.051	0.010	0.032	0.061	0.038
Wage (1000 DKK$_{2005}$)	270	299	346	314	286	337
Region$_{t-1}$						
Copenhagen area	0.234	0.222	0.245	0.278	0.204	0.273
North Zealand	0.132	0.137	0.173	0.176	0.121	0.174
South Zealand	0.112	0.113	0.103	0.108	0.116	0.109
Funen Island	0.085	0.086	0.075	0.072	0.092	0.071
Aarhus area	0.154	0.155	0.138	0.131	0.164	0.132
South Jutland	0.084	0.084	0.085	0.076	0.086	0.076
Mid Jutland	0.110	0.112	0.106	0.097	0.117	0.096
North Jutland	0.088	0.090	0.074	0.063	0.099	0.069
Home owner	0.736	0.795	0.851	0.762	0.793	0.791
Partnered	0.700	0.748	0.786	0.736	0.740	0.777
Children in HH	0.597	0.607	0.679	0.627	0.594	0.611
UI fund member	0.812	0.824	0.561	0.590	1.000	0.093
UI fund entry	0.018	0.021	0.113	0.113	0.000	0.000
Spouse in UI fund	0.353	0.417	0.375	0.352	0.447	0.312
year	1989.4	1991.0	1991.6	1990.4	1991.1	1990.5
Nobs in 1,000	17,308	5,289	458	541	3,732	557
N indiv in 1,000	1,152	588	51	60	415	62

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

Figure 2: UI and Unemployment Degree: Positive Correlation Test



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

elementary schooling. The mean difference in unemployment degree is about 5.8 percentage points. Figure 2 shows the unemployment degree for 3 different education groups: elementary, vocational and Master/PhD. The unemployment degree 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.

6 Results

6.1 Difference-in-Difference Results

6.1.1 Event Study Graphs

We start by presenting our time-of-event study, which allows for a graphical representation of the difference-in-difference estimator. Based on Figure A.1(a), Figure A.1(b) shows the calendar year and year-of-birth definition of whom we assign to the non-retirement group (orange) and the retirement group (red). Both groups will be uninsured when we first admit them to the sample and sign up for unemployment insurance at $t = 0$.

In particular, the retirement group contains those who sign up for UI at the same time as the ER motive becomes present, and we do not expect that they are strongly selected on the basis of risk. This group will mainly react to the exogenous change of incentives, emanating from being at the crossing of the ER regime threshold, and as argued above, we would attribute their change mainly to moral hazard (unless they truly but coincidentally change for other reasons at the same time).

In Figures 3 (a) and (b), we display the experience of this group over time in terms of two outcome variables, unemployment incidence (panel (a)) and unemployment degree (panel (b)), as a red dashed line.

As alternative comparison group we use those who sign up at the ER threshold and remain insured for the next five years (which is the majority of the first group) (red dotted line in Figures 3 (a) and (b)). In some sense they represent a somewhat 'cleaner' comparison group as they are less likely to be contaminated by individuals that join for non-ER reasons at the ER threshold.

The non-retirement group contains those who sign up but at another time than when the ER motive becomes important (orange solid line in Figures 3 (a) and (b)). This group reacts to a number of stimuli, but for them the retirement incentive is not (very) salient. In terms of our model, their reaction can be ascribed to a mixture of selection effects, among which selection on moral hazard.

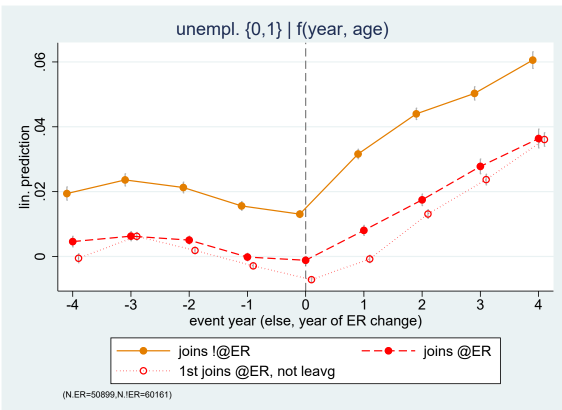
In Table 1, we distinguish the four main groups (retirement, non-retirement, always-members and never insured or leavers). The four groups are on average similar in terms of age, but they differ in terms of educational composition. The group that joins when the ER motive becomes present is in general better educated, has higher wages and has more labor market experience.

In Figure 3, we show the graphs for our two unemployment variables. The effects are the coefficient estimates of event time dummies interacted with group membership, next to calendar year dummies, group membership, and a third degree polynomial in age.

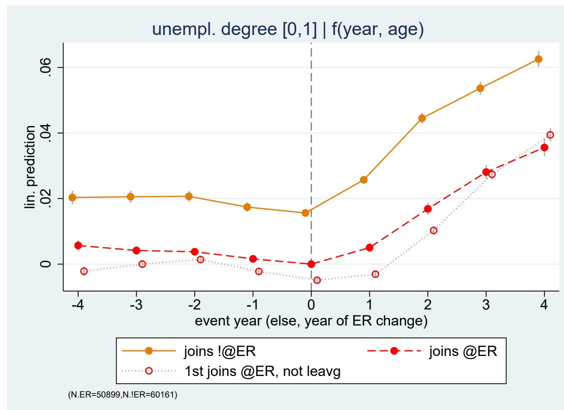
First, we notice that graphs for unemployment incidence and degree look very similar and in the following we concentrate on the graph using the unemployment degree, ignoring the perhaps subtle differences. For individuals who sign up for UI we see that even before signing up for UI, the retirement and the non-retirement groups are different. The group who signs up when the ER motive becomes present has lower unemployment compared to the group that signs up for other reasons, and the difference becomes larger after signing up. The difference in unemployment before signing up is 1.56 percentage points. After signing up the difference increases by 0.51 percentage point. This indicates that the group that signs up for other reasons is adversely selected. The adverse selection can account for about one third of the total difference between insured and uninsured.[22] This number is comparable to the number found in Landais et al., 2021, which reports that adverse selection attributes to 25-30 percent of the difference. In our set-up, we can further decompose the adverse selection and our results

[22]The difference in unemployment between insured and uninsured is 5.8 percentage points and adverse selection amounts to 0.51+1.56 percentage points.

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



(a) UE Incidence



(b) UE degree

Note: This figure shows the evolution of the empirical unemployment over time, for individuals that join the UI system at event year 0. Panel (a) shows the unemployment incidence (binary indicator), panel (b) shows the unemployment degree. The dashed red line is drawn for those that are exactly at the point where they need to sign up in order to fully benefit from the early retirement option, the orange line is drawn for those that join at other points in time. The dotted red line in panel (a) further only selects those that join at the early retirement threshold and are not observed to leave the UI system during the observation window. That window is symmetric around the event year and has a width of 9 years for all individuals. The sample combines the subsamples (i) and (ii) of Table 1. The lines are predictions based on regression models that correct for year and age. Calendar year enters by way of individual year indicators, age enters by way of a 3rd degree polynomial. Data source: register data, Statistics Denmark.

suggest that time invariant characteristics explain about 75 percent of the adverse selection while the remaining 25 percent is due to time-varying characteristics. We also see some indication of moral hazard effects for both groups as unemployment goes up after joining.

Interestingly, the difference between the two groups widens after they have signed up, and that difference stays almost constant the following four years. This suggests that those who sign up at other times actually foresee that in a couple of years they may experience more unemployment.

When comparing to the 'cleaner' retirement group (the dotted red line) the difference is bigger, suggesting that the retirement group (dashed red line) could be contaminated by individuals that sign up for other reasons than the retirement incentive. To deal with this issue we consider the Fuzzy difference-in-difference in next subsection.

To quantify the effect of risk-based selection, we use the difference-in-difference estimator, where we control for year effects, a third-degree polynomial in age. The effect on the unemployment degree is estimated to be 0.51 percentage points with a $t-$value of 7.60.[23] This implies that those who sign up for UI for other reasons than the ER motive, have an additional half percent point higher future unemployment compared to those who sign up when the ER motive is present. This is a substantial effect as the overall risk of unemployment for this sample is around 4-5 percent.

We also formally test for common pre-trend tests. The test is examining whether trend coefficients of the two groups are the same before the signing up. Whereas occular inspection suggests that in both panels of Figure 3 the pre-trends are close to parallel, the formal test rejects the parallel pre-trends, however.

6.1.2 Splitting the Non-retirement Group in Early vs. Late

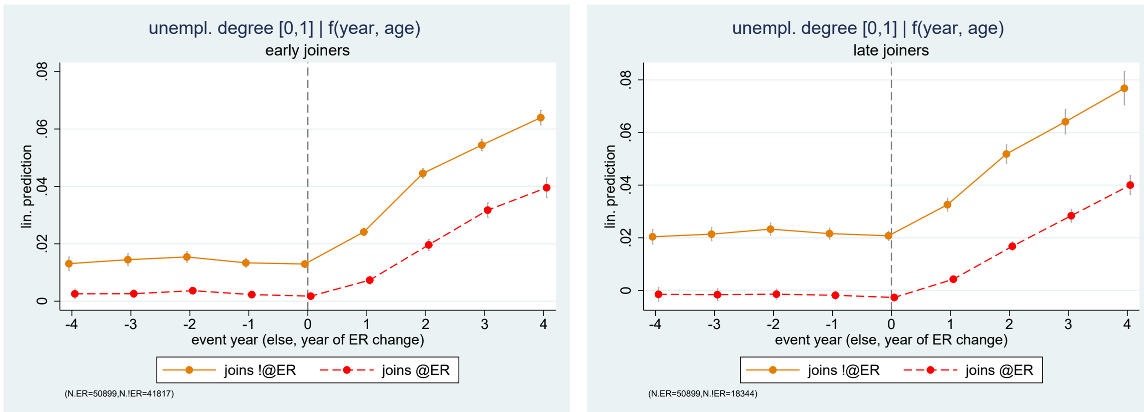
Figure 4 replicates Figure 3(b) for two subsamples of the non-retirement group shown in Figure A.1(b): (a) those that join before the threshold (so, are slightly younger than the non-retirement group), and (b) those that join after the threshold (and are slightly older than the non-retirement group). A comparison across panels (a) and (b) of Figure 4 reveals that the 'early' joiners have lower unemployment than 'late' joiners before joining and the difference becomes even larger after signing up. The assumption of parallel pre-trends cannot be rejected on for the early joiners and is only marginal significant for the late joiners (see Table 2).

6.2 Fuzzy Difference-in-Difference Results

Now we turn to the Fuzzy difference-in-difference estimator to take account of potential contamination of the retirement group. Table 3 summarizes the fuzzy difference-in-difference results for the parameter

[23]These numbers are displayed in the first row of Table 2.

Figure 4: Early and Late Joiners



(a) Before ER Threshold (b) After ER Threshold

Note: This figure replicates panel (b) of Fig. 3 and differentiates between those that join the UI system before (panel (a)) and after (panel (b)) the ER threshold. In both panels, comparison is made with those that join at the threshold (red dashed line). See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

of interest when we apply the methodology set out in Appendix A.3. The interpretation of the parameter is that of a latent average treatment effect (LATE). Since the estimate is obtained as a ratio of various parameters obtained from two different regression models, we bootstrap the standard errors. We use a block-bootstrap and resample individual time series as found in the data. The first row of the table shows a bare-bone specification without additional regressors. We find that individuals that sign up for other reasons than the retirement incentive have a 0.6 percentage point high unemployment degree than individuals signing up for the retirement incentive. The estimate is, as expected, higher than the baseline estimate although the difference is small see Table 2. For that reason, we use the difference-in-difference as our preferred estimation method and view it as a conservative estimator. The second row displays results from a similar exercise, where we control for a number of regressors. The estimate obtained is bigger, and we find that the difference is then 1.2 percentage point.

6.3 Sensitivity: Alternative Outcomes and Nonparametric Evidence

One feature of our data is that we can measure unemployment in the register even when individuals are not entitled to UI fund benefits. It could however be that that registration of the state of unemployment becomes more precise when individuals are entitled to such benefits. To do so, we repeat the analyses and use the incidence of non-employment as our outcome variable, including those that

Table 2: Difference-In-Difference Estimates

	Unemp. Degree		Pretrend test		
	coeff.	**t.v.**	**F-test**	**p-value**	Reference
Baseline	0.0051	7.60	5.56	0.0008	Fig.3(b)
Variation: Alternative unemployment measures					
Unemp. incidence	0.0093	8.48	3.20	0.0022	Fig.3(a)
Non-empl. Incidence	0.0094	8.55	13.83	0.0000	Fig.A.2
Heterogeneity:					
Early joiners	0.0056	7.29	1.99	0.1137	Fig.4(a)
Late joiners	0.0049	4.85	3.13	0.0246	Fig.4(b)
At most HS	0.0049	3.14	1.99	0.1137	Fig.6(a)
At least BA	0.0040	4.53	2.38	0.0672	Fig.6(b)
Men	0.0037	4.28	3.82	0.0095	Fig.7(a)
Women	0.0068	6.63	2.38	0.0680	Fig.7(b)
Variation: multiple treatment groups					
@ER v @house	0.0137	3.36	4.86	0.0001	Fig.8(a)
@ER v @kids	0.0010	0.17	3.13	0.0046	Fig.8(b)
@ER v @married	0.0097	1.64	3.27	0.0032	Fig.8(c)
@ER v @sp_UI	0.0081	2.56	3.17	0.0042	Fig.8(d)

Note: This table shows treatment effects obtained from DID regressions, commensurate with vertical difference between groups in the graph at and just after the threshold, only controlling for functions of age and year. The balanced sample is used, mainly Subsamples (i) and (ii) of Table 1. The notation '@ER' and '@kids' indicates at what time the individuals joins the insurance (at the ER threshold, or at the time of arrival of the first child, etc.).

leave the labor market. If the measurement error in the unemployment registration were an issue, non-eligible individuals would then not register as unemployed, but instead show up as non-employed. The comparison (see Table 2 and Figure A.2 in the Appendix) shows no major differences in the pattern between unemployment and non-employment, although the estimated effect is slightly larger.

As a further robustness check, we also display the raw data to check if the estimation or functional form assumptions impact our results. For this exercise, we use the broader unbalanced sample and select all individuals in the relevant birth cohorts who sign up for UI between the age 25-59. Then we look at the unemployment degree in the following year. We aggregate all data into cells of birth

Table 3: Wald/Fuzzy Difference-in-Difference

	Unemp. Degree	
	coeff.	**t.v.**
no regressors	0.0061	1.81
inc. regressors*	0.0115	2.72

Notes: T-values and standard errors based on 100 bootstrap replications. *Additional regressors include most notably education, labor market experience, demographics and location.

cohort and years and report the average unemployment degree. We show these averages in the heat map of Figure 5. For instance, we have that those individuals born in 1931 who sign up for UI in 1981 have on average degree of unemployment of 0.02 in 1982. We have indicated the cells for which the ER motive is present with a black border. From the figure, it is clear that those who sign up because of the ER motive have a lower degree of unemployment.

6.4 Robustness with Respect to Sample and Specification

Most of our event graphs are based on the subsamples (i) and (ii) in Table 1. The sample is specifically chosen so as to base the comparison between the two groups that join the UI system on individuals we can observe for a full length of 9 years where we observe a person to join exactly in the middle (year τ, or year 0 in our graphs).

Three types of variations are being considered in this subsection: (1) what changes when we change the sample, (2) what changes when we move away from measuring the effect locally at the threshold only, and (3) what changes when we change the set of controls? We focus the discussion on (1) and (2), and briefly allude to (3). Table 4 collects the results.

We shall concentrate on the effects displayed in the various rows that have a basic specification of control variables. Those are a third degree in age and a set of individual calendar year indicators, over and above the event year and group indicators. Those rows are indicated as (i) in the column for the specification.

The main comparison is across samples. We consider which subsamples to include and we consider whether to use the balanced sample at all. The column labeled 'Selection' guides the different sample selections. We focus on the left half of the Table that uses event year indicators to estimate the local treatment effects.

Figure 5: Heat Map of Unemployment Degree

year of birth	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
1931	0.02	0.03	0.04	0.05	0.05	0.09	0.08	0.05	0.08									
1932	0.03	0.03	0.05	0.03	0.03	0.05	0.04	0.05	0.06	0.08								
1933	0.03	0.02	0.02	0.03	0.03	0.05	0.08	0.06	0.06	0.05	0.13							
1934	0.03	0.04	0.04	0.03	0.04	0.04	0.04	0.05	0.06	0.05	0.09	0.06						
1935	0.03	0.04	0.05	0.04	0.02	0.03	0.05	0.05	0.04	0.05	0.09	0.08	0.11					
1936	0.03	0.04	0.05	0.04	0.02	0.02	0.05	0.04	0.05	0.04	0.11	0.05	0.08	0.05				
1937	0.02	0.04	0.04	0.06	0.06	0.03	0.02	0.03	0.04	0.04	0.07	0.05	0.10	0.05	0.08			
1938	0.03	0.04	0.04	0.05	0.04	0.05	0.05	0.02	0.04	0.04	0.05	0.03	0.08	0.06	0.07	0.06		
1939	0.03	0.03	0.04	0.04	0.03	0.06	0.06	0.04	0.03	0.04	0.06	0.02	0.08	0.03	0.06	0.08	0.06	
1940	0.03	0.03	0.04	0.04	0.04	0.05	0.05	0.05	0.04	0.03	0.06	0.02	0.08	0.07	0.08	0.07	0.06	0.07
1941	0.03	0.03	0.05	0.05	0.06	0.05	0.05	0.05	0.04	0.03	0.03	0.02	0.07	0.07	0.08	0.06	0.04	0.10
1942	0.03	0.04	0.05	0.05	0.04	0.06	0.05	0.05	0.04	0.05	0.03	0.02	0.06	0.06	0.05	0.04	0.05	0.04
1943	0.03	0.04	0.05	0.06	0.05	0.07	0.06	0.05	0.05	0.04	0.04	0.02	0.10	0.05	0.05	0.05	0.04	0.04
1944	0.03	0.04	0.05	0.05	0.05	0.04	0.06	0.05	0.05	0.04	0.04	0.02	0.11	0.05	0.07	0.02	0.04	0.05
1945	0.03	0.04	0.04	0.05	0.05	0.05	0.06	0.06	0.06	0.05	0.05	0.02	0.09	0.06	0.07	0.03	0.04	0.06
1946	0.03	0.05	0.05	0.05	0.05	0.06	0.06	0.05	0.06	0.04	0.04	0.02	0.11	0.06	0.05	0.05	0.03	0.05
1947	0.04	0.04	0.05	0.06	0.05	0.07	0.08	0.07	0.05	0.05	0.05	0.02	0.08	0.04	0.05	0.03	0.04	0.06
1948	0.04	0.05	0.05	0.05	0.07	0.07	0.07	0.08	0.07	0.06	0.06	0.02	0.11	0.05	0.06	0.04	0.03	0.07
1949	0.04	0.06	0.05	0.07	0.05	0.08	0.09	0.07	0.06	0.07	0.05	0.02	0.09	0.06	0.07	0.04	0.05	0.04
1950	0.04	0.06	0.07	0.08	0.07	0.08	0.09	0.09	0.07	0.06	0.07	0.02	0.11	0.05	0.06	0.05	0.05	0.07
1951	0.04	0.07	0.07	0.08	0.08	0.10	0.08	0.08	0.08	0.07	0.07	0.03	0.11	0.04	0.06	0.05	0.05	0.07
1952	0.04	0.07	0.09	0.10	0.08	0.11	0.11	0.08	0.09	0.07	0.08	0.03	0.09	0.04	0.05	0.05	0.03	0.04
1953	0.05	0.07	0.08	0.09	0.08	0.11	0.11	0.11	0.09	0.07	0.08	0.06	0.05	0.05	0.05	0.05	0.03	0.05
1954	0.05	0.07	0.08	0.11	0.09	0.12	0.12	0.13	0.09	0.06	0.09	0.06	0.07	0.02	0.06	0.05	0.02	0.05
1955	0.05	0.07	0.09	0.12	0.10	0.10	0.11	0.11	0.09	0.09	0.08	0.07	0.08	0.05	0.03	0.05	0.06	0.06
1956		0.07	0.09	0.11	0.11	0.11	0.12	0.11	0.11	0.09	0.09	0.07	0.07	0.06	0.06	0.03	0.05	0.05
1957			0.10	0.10	0.12	0.10	0.13	0.13	0.12	0.09	0.09	0.08	0.09	0.05	0.09	0.05	0.02	0.06
1958				0.11	0.11	0.12	0.11	0.13	0.13	0.11	0.10	0.08	0.09	0.05	0.06	0.07	0.05	0.04

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

Table 4: Robustness checks DiD

		I – Event Year Indicators				II – Event Year Threshold			
		A: Balanced Sample		**B: Large Sample**		**A: Balanced Sample**		**B: Large Sample**	
Selection	Spec.	**coeff.**	**t.v.**	**coeff.**	**t.v.**	**coeff.**	**t.v.**	**coeff.**	**t.v.**
(a)	(i)	0.0038	6.00	0.0134	18.93	0.0155	20.80	0.0180	24.62
	(ii)	0.0048	7.30	0.0112	16.98	0.0120	21.76	0.0162	36.23
(b)	(i)	0.0051	7.79	0.0121	18.98	0.0142	19.82	0.0144	22.01
	(ii)	0.0059	8.74	0.0121	19.95	0.0106	19.85	0.0134	33.88
(c)	(i)	0.0086	15.14	0.0131	22.40	0.0142	21.46	0.0171	26.82
	(ii)	0.0082	14.20	0.0110	19.56	0.0107	20.97	0.0136	34.05
(d)	(i)	0.0091	16.16	0.0142	24.27	0.0146	22.11	0.0175	27.49
	(ii)	0.0082	14.49	0.0131	23.97	0.0098	20.00	0.0142	36.60

Note: Panel I displays local difference-in-difference estimates measured at the event, based on event-year specific indicators (as in Table 2). Panel II displays estimates that compare individuals during the before-sign-up phase and during the after-sign-up phase. Samples: A: the balanced sample, with 9 observations each, centered at the event year for joiners; B: the large sample without selecting on the number of participations. Specification (i) controls for a third-degree polynomial in age as well as individual calendar year indicators. Specification (ii) controls in addition for further regressors, most notably gender, education, demographics, labor market experience, and income. Selection (a) includes only those that join the UI system during the observation window, keeping only those that make a single transition and do not leave again. Selection (b) includes only those that join the UI system during the observation window, allowing individuals to leave (Subsamples (i) and (ii) of Table 1). Selection (c) includes in addition those that are always insured during the observation window (Subsamples (i)–(iii) of Table 1). Selection (d) includes everyone, that is, also those that are never observed to be insured, or that are insured initially and then leave. For instance, the entry at I.A.(b).(i), corresponds to the baseline entry in Table 2, and displayed in Figure 3(b). Sample sizes: A: (a) 834k from 93k individuals, (b) 999k / 111k, (c) 4,731k / 526k, (d) 5,289 / 588; B: (a) 2606k from 165k individuals, (b) 4198k / 267k, (c) 15,033k / 976k, (d) 17,308k / 1,152k.

We compare to entry at I.A.(b).(i), corresponding to the baseline entry in Table 2. Selection (a) includes only those that join the UI system during the observation window, keeping only those that make a single transition and do not leave again. This sample is slightly ‘cleaner’ as we then have a single transition during the 9 years for everyone, with no-one leaving. Compared to the baseline, the coefficient shrinks from 0.51 percent to 0.38 percent. Selection (b) allows individuals to leave after having joined, and the sample is therefore slightly bigger. This will, using the balanced sample, produce the baseline estimate. Selection (c) includes in addition those that are always insured during the observation window (Subsamples (i)–(iii) of Table 1). In the balanced sample, the effect increases from 0.51 percent to 0.86 percent. Note that the sample composition is substantially different now, since it will be dominated by those that were always observed to be insured. Those are somewhat lower educated on average, have lower incomes, are more likely to be male, and are much more likely to experience unemployment. In our regression, we include a separate indicator for the event year, which is unobserved for this group by construction (as they are always insured in the sample, we do not see them join). Selection (d) includes everyone, that is, also those that are never observed to be insured, or that are insured initially and then leave. Again, the latter group is heterogeneous and we are unable to ascertain when, if at all, they might join the insurance. The estimated coefficient, however, changes only slightly with the addition.

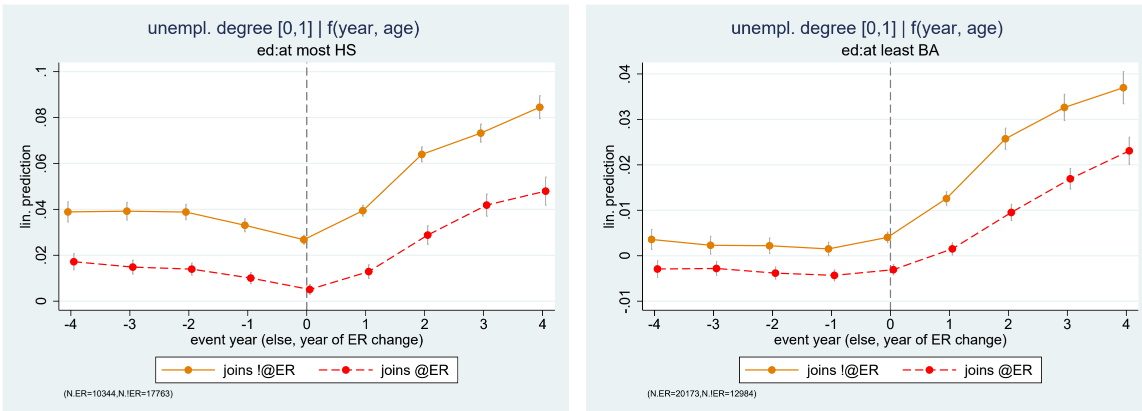
If we use the large, unbalanced sample instead, the coefficient of interest more than doubles. This can be seen by comparing between columns I.A. and I.B. At base line, the coefficient changes from 0.51 percent to 1.21 percent. However, there is more stability when we consider different sample cuts (varying the selections from (a) to (d)), as all estimates are of comparable magnitude (between 1.21 and 1.42 percent). Since the panel is unbalanced, the length of the observation period of an individual varies substantially, and this variation will lead to a different distribution of groups (joiners, never joiners, always insured, leavers, etc.).

The second variation moves away from estimating a local effect at the threshold and instead provides a more global before-after comparison. Compared to the previous effects we now see much more stability again across specifications and samples, and at an again slightly higher level of the effect. At base line, the effect increases from 0.51 percent to 1.42 percent (entry at II.A.(b).(i)), but this number stays quite robust. It reflects the impact of an average of 4 years after the transition, and with rising unemployment risk when insured, the direction of this change is in some sense expected. Again we find even larger estimates when using the large, unbalanced sample, but the differences to the balanced sample results are not as large as under the more local identification strategy.

Finally, turning to the impact of the set of controls used in the underlying regression, we see an overall limited impact when switching from specifications (i) to (ii). We add most notably gender, education, demographics, labor market experience, and income. The estimated effects are in general about 10% smaller in magnitude, but not uniformly so (in some models and samples, adding more controls increases the estimated difference-in-difference parameter). At large, this exercise suggests that measuring the impact is not very sensitive to the set of controls included.

This subsection has emphasized that sample and specification can be important when measuring the effect, but our overall assessment is that there is much robustness in our estimates. The baseline estimate in Table 2 is, if anything, conservative.

Figure 6: Event Graphs: Unemployment Degree by Education



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

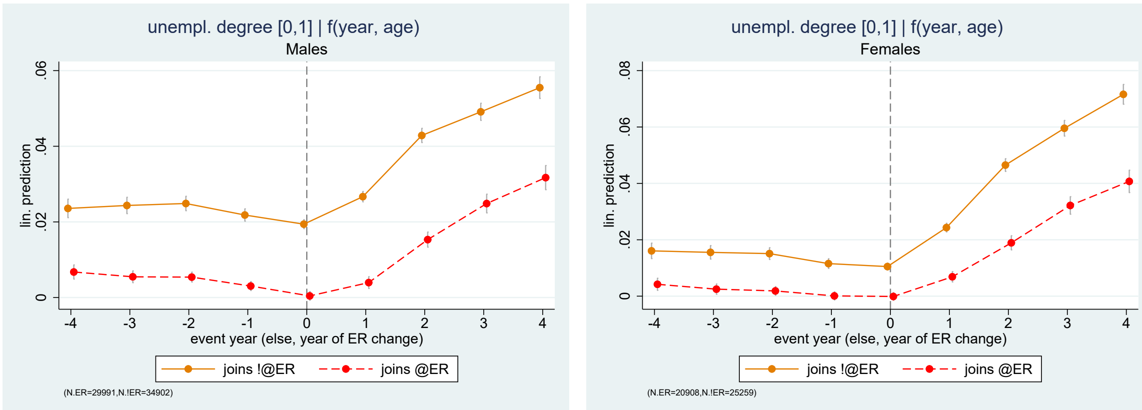
Note: This figure replicates panel (b) of Fig. 3 and differentiates between those that have a low level of education (high school or below, panel (a)) and a high level of education (bachelor degree or higher, panel (b)). See note to Fig. 3 for further explanation. Data source: register data, Statistics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

6.5 Heterogeneity in Terms of Education

We next examine whether there is heterogeneity in adverse selection across education groups. Highest completed education is perhaps the single best indicator of career profiles. We consider two broad education groups: at most high school and at least BA/college education. We repeat the analysis of Section 6.1 for each of the education groups. In Figure 6, we show the time-of-event graphs for individuals with at most high school and at least BA/college education.[24] The graph indicates differences in adverse selection effects for individuals with at most high school and at least BA/college degree. The effect is 0.49 percentage point ($t-$value 3.14) for individuals with at most high school and 0.40 percentage points ($t-$value 4.53) for individuals with at least BA/college degree. Furthermore, we also see only minor differences before signing up for UI between the retirement and non-retirement group for individuals with at least a BA/college degree. This suggests that adverse selection is much less pronounced among highly educated individuals. For vocationally trained individuals the results are mixed (Figures not shown; there are many subgroups, and they are difficult to summarize). In Appendix A.2 we show additional event graphs where the sample is split according to the features of the salaries.

[24]High school alone is not a very frequently observed category, as a high school diploma in itself does not imply job qualification. Most workers with a high school diploma therefore have at least some additional training.

Figure 7: Event Graphs: Unemployment Degree by Gender



(a) Men (b) Women

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

6.6 Heterogeneity in Terms of Gender

To investigate the difference between men and women we split the sample by gender, see Figure 7. The event graphs show interestingly different adverse selection pattern between men and women. Men (see Figure 7(a)) are mainly adverse selected into UI funds based on time invariant characteristics and they have a higher likelihood of unemployment even before signing up. After signing up, the difference between the two groups increases, but only marginally (the estimate is 0.37 percentage point with a t-value of 4.28, see Table 2). For women, we see the opposite pattern (see Figure 7(b)). There is a smaller difference in unemployment before they sign up, but after signing up, those who sign up at the ER threshold have a considerably lower risk of unemployment (the estimate is 0.68 with a t-value of 6.63, see Table 2). This suggests that adverse selection for women is mainly driven by differences in future risk of unemployment.

6.7 Heterogeneity in Terms of Life Cycle Events

Figure 8(a)-(d) repeats the analysis of Figure 3(b) with the difference that we now split the non-retirement group of joiners away from the ER threshold into various subgroups that are exposed to what we may call life-cycle events. The idea is that these life-cycle events may change preferences (e.g preference for leisure) or cost of effort. Those events are: (a) first-time home buying, (b) having a first child in the household, and (c) marrying (i.e., change in adult household composition). We also look at the event that (d) one's spouse joins a UI fund. The window is chosen such that these events happen at $t = 0$. The various groups are all small subgroups of the larger group that joins before the

ER incentive becomes binding.

Whereas the (red) retirement group sees a very slight increase in the propensity to increase their unemployment degree just after the threshold, indicating a small impact of moral hazard, the all other groups clearly have a more pronounced positive change in their unemployment degree, owing, as is likely, much to (composite) selection effects. Especially first-time home owners experience a steep increase in the unemployment degree, which could be related to less mobility. The estimated effect is 1.37 percentage points with a t-value of 3.36 (see Table 2).

All non-retirement subgroups appear to be triggered or induced to increase their unemployment degree through an event that is potentially correlated with underlying model parameters, and shifting their unemployment probabilities. Note that all the solid orange lines are following the main pattern of the total non-retirement group (orange), but their standard errors are large due to the rather stringent group membership attribution based on the timing of the event.

While we make no claim that any of these events is (strictly) exogenous, it is in this picture that we may interpret the documented change as likely effect of selection on moral hazard. This is because the various events may be seen as shifters of some of the parameters in the model, such as the valuation of leisure or the marginal cost of effort (e.g., having a first child may make it more costly to search for a job or may make it more attractive not to work) that are potential reasons for moral hazard effects.

Corresponding numerical estimates can be found in the lower panel of Table 2. Analyses for alternative outcomes tell a similar story.

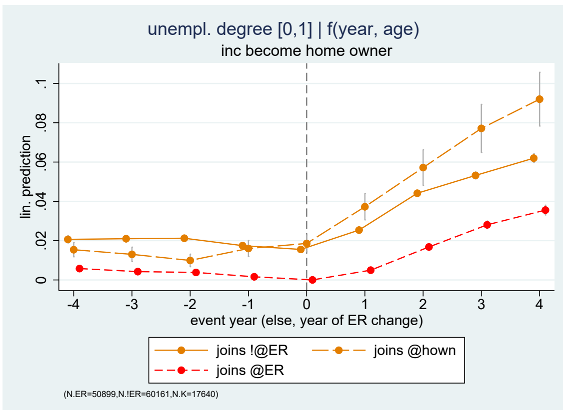
7 Concluding Remarks

Voluntary unemployment insurance systems have recently received attention as they provide more flexibility than the mandatory insurance systems. However, these insurance schemes are vulnerable to selection. Our results show that the selection process into unemployment insurance is complex. We find evidence of adverse selection effects but they are very heterogeneous in the Danish voluntary unemployment insurance system.

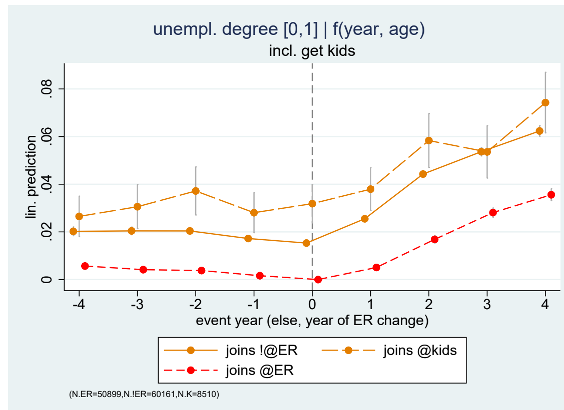
We find that insured individuals are more than twice as likely to be unemployed compared to uninsured individuals. About one third of this difference can be attributed to adverse selection and we find that individuals who sign up for unemployment insurance are adversely selected in two ways. First, even before they sign up, they have on average a higher unemployment rate than other non-insurees. This accounts for about 75 percent of the adverse selection. Second, an individual who chooses to sign up for UI also sees on average an increase in the risk of future unemployment, which accounts for the 25 percent of the adverse selection.

The increase in the risk of future unemployment of those who choose to sign up is half percentage

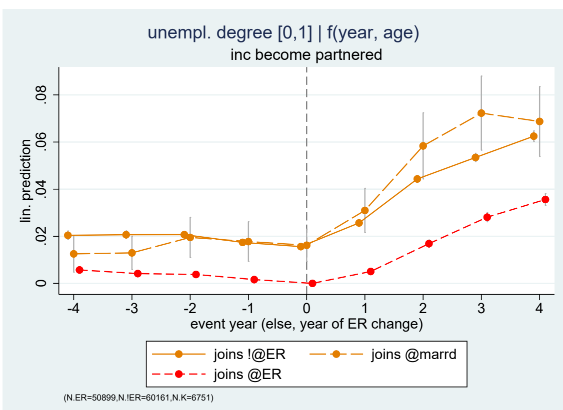
Figure 8: Event Graphs: Unemployment Degree and Life Cycle Events



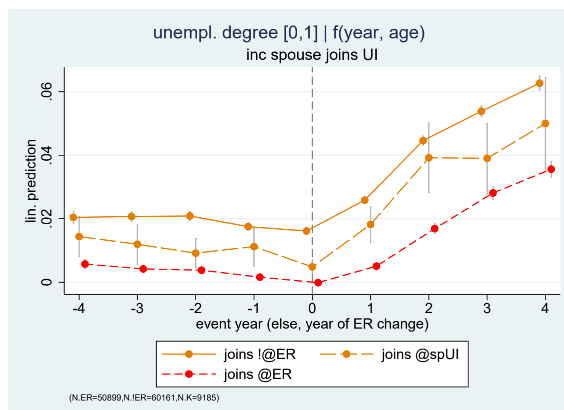
(a) Become home owner



(b) Get children



(c) Become partnered



(d) Spouse insured

Note: This figure replicates panel (b) of Fig. 3 and splits the group that joins the UI system at other times than the ER threshold into a smaller subgroup that joins when experiencing a life cycle event (broken orange line), and a larger subgroup that does not (solid orange). Those events are: becoming a home owner (panel (a)), getting children (panel (b)), become 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.

point higher. This is substantial, compared to the baseline probability of unemployment. Interestingly, we find differences in adverse selection across education groups, with largest effects in particular for lower-educated groups, while more educated groups are to a less degree subject to adverse selection. We also find that women are more prone to be adverse selected due to future risk while men are more prone to adverse selection due to time invariant characteristics. This suggests that adverse selection may be driven by certain groups in the labor market. Furthermore, we also find suggestive evidence on selection on moral hazard, from conditioning on life-cycle events (marriage, first-time home ownership or birth of first child). Those events trigger both insurance and subsequently higher unemployment, and are potentially correlated with changes in preference parameters determining moral hazard. This way, we provide data-driven evidence complementary to that available in Einav et al. (2013) and Cronin (2019) who rely on a more structural modeling approach.

Our results indicate that adverse selection is happening both because of selection due to individual specific time-invariant characteristics correlated with the risk of unemployment such as abilities, but also because individuals who foresee higher future unemployment choose to sign up. This suggests that a voluntary UI scheme that allows continuous enrollment, should be designed such that it requires from individuals to have been insured for some period before being eligible for benefits. However, this is further complicated by the fact that the adverse selection is driven by certain groups in the labor market.

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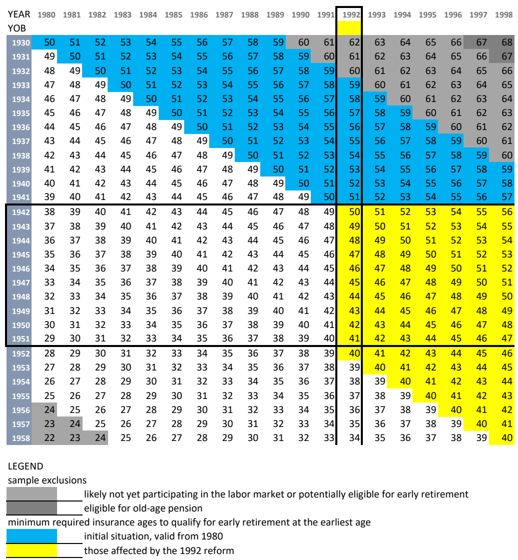
A Appendix

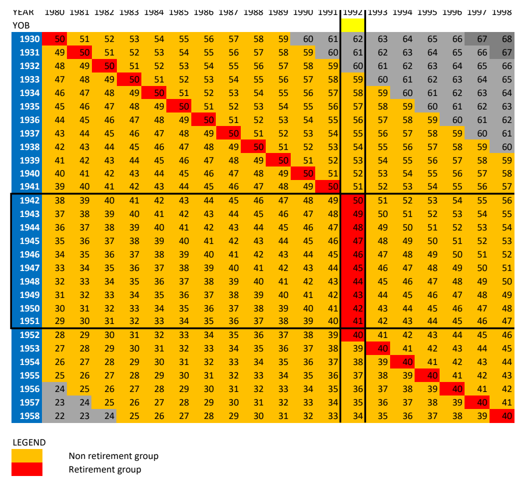
A.1 Additional Figures and Tables

Figure A.1: Early Retirement Institutions by Year of Birth and Year



YEAR / YOB	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
1930	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68
1931	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67
1932	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66
1933	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65
1934	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64
1935	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63
1936	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62
1937	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61
1938	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60
1939	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59
1940	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58
1941	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57
1942	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56
1943	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55
1944	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54
1945	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53
1946	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1947	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51
1948	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50
1949	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49
1950	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48
1951	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47
1952	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46
1953	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45
1954	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44
1955	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43
1956	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42
1957	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41
1958	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40

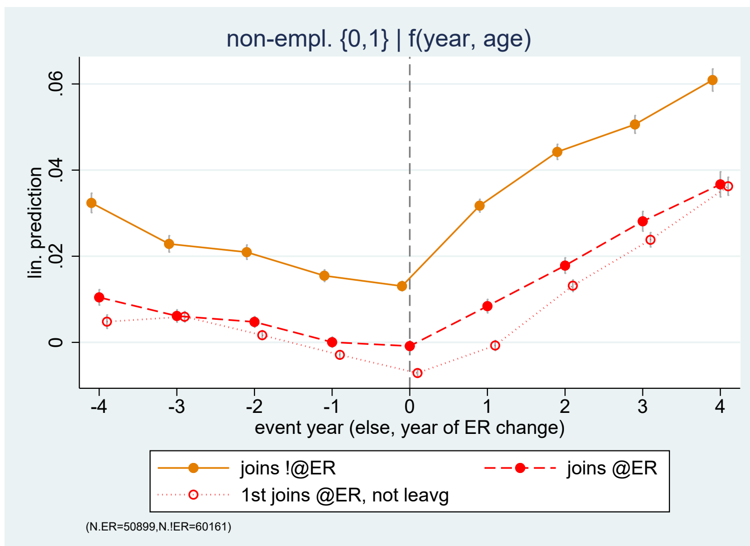
(a) Institutions



YEAR / YOB	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998
1930	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68
1931	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67
1932	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66
1933	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65
1934	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64
1935	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63
1936	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62
1937	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61
1938	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60
1939	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59
1940	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58
1941	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57
1942	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56
1943	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55
1944	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54
1945	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53
1946	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1947	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51
1948	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50
1949	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49
1950	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48
1951	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47
1952	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46
1953	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45
1954	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44
1955	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43
1956	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42
1957	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41
1958	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40

(b) Retirement/non-retirement Groups

Figure A.2: Event Graphs: Non-employment around the Entry into UI



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

A.2 Additional Event Graphs

In this section we propose a different way of grouping individuals across important dimensions of heterogeneity. Focusing on the case of unemployment degree, we want to group individuals according to a career-relevant characterization of background and experiences.

The education classification relies on a variable that provides an occupation-specific education level classification. We start with a sub-classification typically based on the first four digits of that classification. There are some exceptions: some large groups in the vocational education group have lower-level codes (5 or 7 digits), and some other 4-digit groups that were merged. We retain 44 education groups.

The micro data on income (wages, salaries) are then used to generate education-specific income statistics as follows:

1. We consider 5-year moving time series at the individual level (current year and previous four years), and restrict attention to wage series with strictly positive values only.

2. Based on those, we calculate individual-level income statistics (mean; difference in logs (growth); and coefficient of variation, CV). We set to missing growth rates outside the $[-100/+100\%]$ interval.

3. We regress those statistics on a 3rd-order age polynomial and a 5th-order year polynomial, as well as our 44 education indicators.

4. We predict, per education class, the expected income statistic at age 45 and year 1992 (mean of the data).

5. We then group education classes by binary classifications on each of the three dimensions (level, growth rate and spread); out of eight possibilities, four groups are relevant:

   (a) low level, low growth, and high CV

   (b) high level, low growth, and low CV

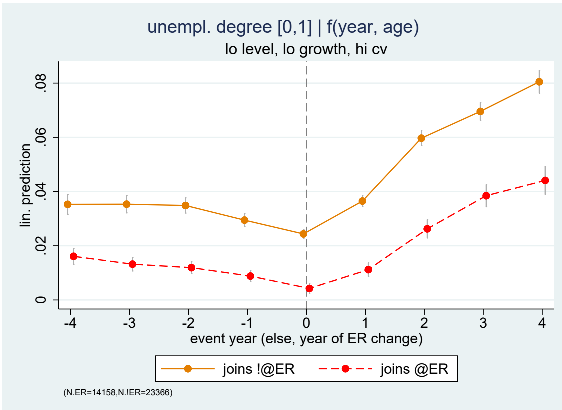
   (c) high level, low growth and high CV

   (d) high level, high growth and high CV.

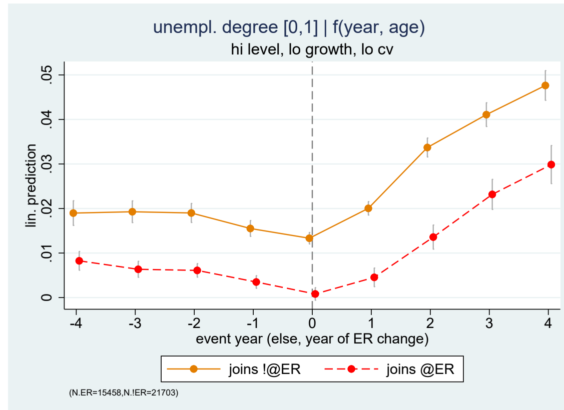
   The distinction into 'high' and 'low' is roughly based on the median.

Figure A.3 shows the event graphs corresponding to Figure 6 when all education groups are classified according to the income features just discussed. Figure A.4 focuses on vocational education groups. In most of these cases, we observe again a distinct difference between the two groups, suggesting that

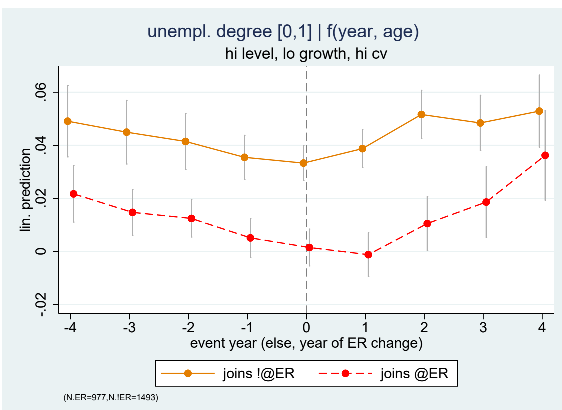
Figure A.3: Event Graph: Unempl. Degree, Aggregate Education Groups by Income Features



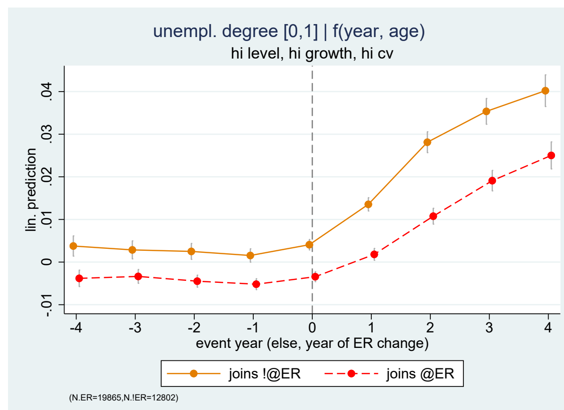
(a) Low level and growth, high CV



(b) high level, low growth and CV



(c) high level and CV, low growth

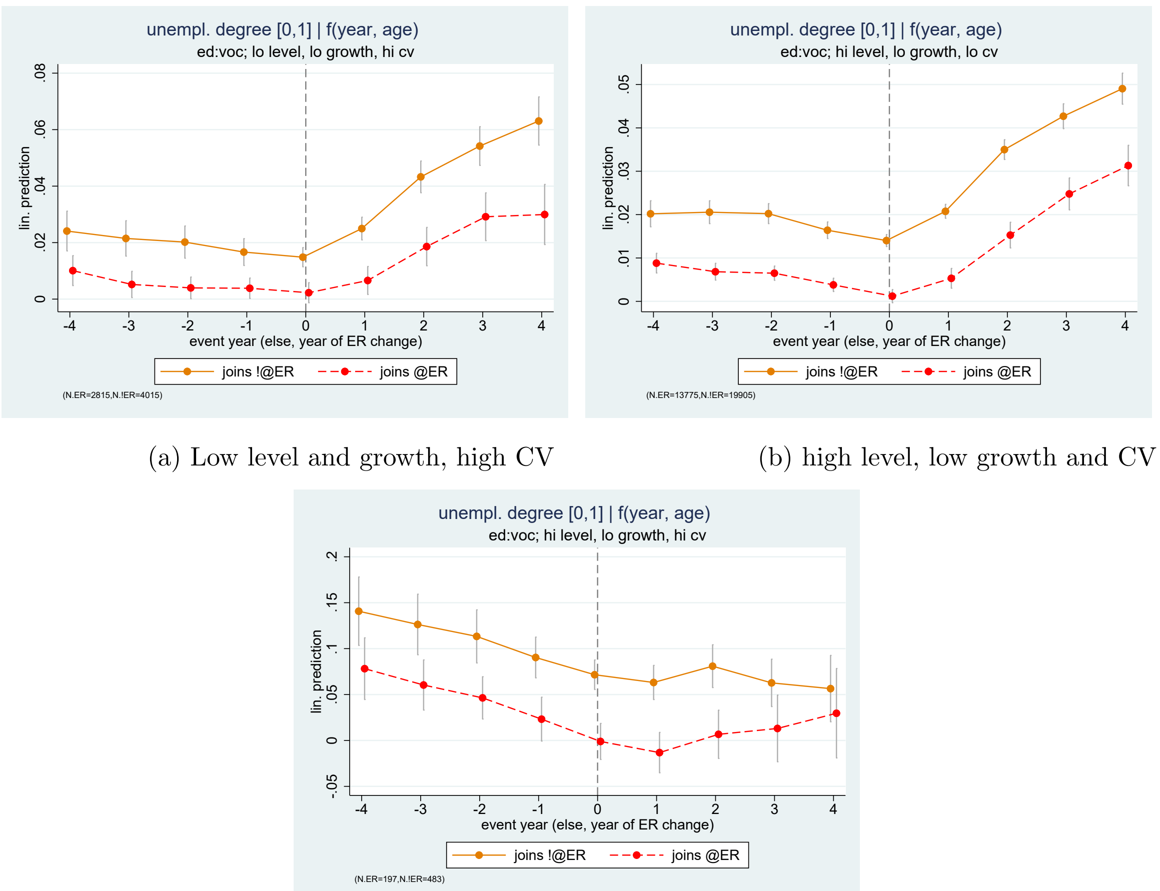


(d) high level, growth and cv

Note: This figure complements Fig. 6 and splits the sample according to features of the income process. See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

adverse selection is at work. The differential effects are stronger for some subsamples than for others, for instance, they appear to be quite pronounced for those with characterized by a low income growth rate, but a high variation (CV)—groups (a) and (c) in the classification above.

Figure A.4: Event Graph: Unempl. Degree, Vocational Education Groups by Income Features



(a) Low level and growth, high CV

(b) high level, low growth and CV

(c) high level and CV, low growth

Note: This figure complements Fig. 6 and focuses on vocational education groups. Like Figure A.3, it splits the sample according to features of the income process. See note to Fig. 3 for further explanation. Data source: register data, Statistics Denmark.

A.3 Fuzzy Differences-in-Differences

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 first 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 retirement group ($\Delta Z_{t=1} = 1$), had it ($D_{t=1} = 1$) or had it not ($D_{t=1} = 0$) the 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

$$W_{DID} = DID_Y/DID_D$$

where for any random variable R,

$$DID_R = \underbrace{E(R_1|\Delta Z_1 = 1) - E(R_0|\Delta Z_1 = 1)}_{\text{time difference, group 1}} - (\underbrace{E(R_1|\Delta Z_1 = 0) - E(R_0|\Delta Z_1 = 0)}_{\text{time difference, group 0}}).$$

Accordingly, we define the numerator for our problem 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 treatment status D 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

$$\Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 0) = \Pr(\Delta s_1 = 1|\Delta Z_1 = 1, D_1 = 0) > 0 \tag{A.1}$$

$$\Pr(\Delta s_1 = 1|\Delta Z_1 = 0, D_1 = 1) = 0 \tag{A.2}$$

$$\Pr(D_t = 1, \Delta Z_t = 1) = \Pr(\Delta Z_t = 1)\Pr(D_t = 1) \quad \forall t = 0, 1 \tag{A.3}$$

Assumption (A.1) 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 (A.2) implies that those who value the retirement motive will not sign up when the ER option is not available. The last assumption (A.3) implies independence between the institutional rule and the motive.

First, we notice that the motives 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 then 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

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

where the second equality follows from assumptions (A.2) and (A.3). Similarly we can calculate:

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

Then it follows that

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

and 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 (A.2) it follows that

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

We can then write the Wald DID when assumptions (A.1)–(A.3) are satisfied as:

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

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

W_{DID} can be estimated from two regressions. The first regression is based on a sample where $\Delta s_{it}=1$:

$$Y_{it}=\beta_0+\beta_1 1_{(t>0)}+\delta 1_{(t>0)}\cdot 1_{(\Delta Z_{i0}=1)}+\alpha_i+\varepsilon_{it}.$$

The second regression is based on a sample where $s_{it-1}=0$:

$$\Delta s_{it}=\gamma_0+\gamma_1\Delta Z_{it}+v_{it}.$$

W_{DID} can be estimated by

$$\hat{W}_{DID}=(\hat{\gamma}_0+\hat{\gamma}_1)\frac{-\hat{\delta}^{FE}}{\hat{\gamma}_1}.$$