Leaving Already? The Swedish Unemployment Insurance as a Pathway to Retirement*

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Abstract

This paper studies whether Swedish workers use the unemployment insurance (UI) system as a pathway to retirement. We use longitudinal register data consisting of weekly UI benefit payments for the period 1999 to 2015. In Sweden, individuals are eligible for UI benefits for a maximum of 427 days. This creates an artificial benefit threshold at 427 days prior to the official retirement age. We use a novel quasi-experimental approach that exploits the distribution of UI benefits and started UI spells close to retirement. We find that a disproportionate large share of total UI spells start at this threshold, which indicates that individuals seek to maximize benefits before retiring. Our results detect moral hazard and suggest that UI benefits induce early retirement. Furthermore, the results suggest that individuals use UI benefits in a strategic manner.

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1 Introduction

Labor force participation among older workers have fallen in almost all developed economies (Dorn and Sousa-Poza, 2010). Studies often explain the decline as a consequence of generous pension systems (e.g., Hausman and Wise, 1985; Feldstein and Liebman, 2002). Yet, more recent literature emphasizes alternative pathways, such as the unemployment insurance (UI). For instance, job search requirements are typically less demanding for older workers (Hairault et al., 2012), and elders might even be exempt from job search while receiving benefits (Gruber and Wise, 1998). In addition, empirical evidence shows that an extension of the maximum length of benefits increases the incidence of early retirement (Inderbitzin et al., 2016). These indices propose that unconventional pathways to retirement are important to understand early retirement patterns.

While previous studies highlight the impact of UI benefits, the amount of quasi-experimental evidence is scarce. Most studies use policy changes to evaluate causal effects (e.g., Lalive, 2008); however, these findings are likely restricted to the particular institutional setting. For example, while most developed economies have mandatory UI schemes, the Swedish system builds on voluntary insurance provided by private unemployment insurance funds (A-kassor). This type of fundamental institutional differences among countries will likely result in different outcomes. Hence, further studies focusing on unexplored institutional settings are necessary to provide further understanding how UI affects early retirement.

In this paper, we identify how Swedish workers use UI benefits as a pathway to retirement. More precisely, we apply a novel quasi-experimental approach to detect workers who self-select into UI spells prior to the official retirement age of 65. In Sweden, individuals are eligible for UI benefits for a maximum of 427 days. This creates an artificial benefit threshold at 427 days prior to the age of 65. We exploit the variation in benefit usage at the threshold to detect distortive effects on older worker's labor supply. In addition, our empirical approach allows us to distinguish moral hazard in the Swedish UI system.

We use detailed data from the Swedish Unemployment Insurance Board (IAF), which provides a longitudinal register of individual unemployment insurance payments for the period 1999 to 2015. The data includes the reason for the individual's deregistration from the UI system and at what date the deregistration took place. This allows us to separate the individuals who left the system at the official retirement age. In addition, the data allows us to cleanly categorize benefit payments, started UI spells, and entries into the UI system with respect to days to retirement. In addition, the register includes additional information on demographics, region of residence and whether the person is born in Sweden. These variables serve as controls in our regressions.

Our analysis relies on graphical evidence of the unconditional distribution of payments and started spells. In addition, we develop a simple theoretical framework in which we interpret the results. The hypothesis is that individuals seek to retire at 427 days prior to retirement to maximize the benefit duration while minimizing labor income losses. We study the pattern of payments to establish the correlation between payments and days to retirement, which is tested through placebo regressions. Furthermore, we use density tests to test for manipulation in the number of started spells around the 427-day threshold. Last, we check the robustness of our results by examining entries into the UI system prior to retirement, and use descriptive evidence to see how entitlement to benefits correlate with durations.

The results contain three main findings. First, benefit payments increase exponentially before retirement. However, while benefit payments depict a discontinuity in means at the 427-days threshold, this discontinuity is almost zero when narrowing the bandwidth and allowing for different slopes at each side of the threshold. Likewise, the result is similar for our placebo regressions. Hence, we find that there is no discontinuity in benefit payments due to the UI setting. This is consistent with moral hazard in the UI system and worsen labor market opportunities for older workers.

Second, we turn to started UI spells as a potential cause to the increased benefit payments. We find a discontinuity and bunching in started UI spells at 427 days prior to the retirement age of 65, and a decline in started spells thereafter. This contradicts our previous finding, and indicates that people adjust their behavior to exploit the full length of benefits. To test the significance of the bunching, we use density tests to simultaneously check for selection around the threshold. These results show significant manipulation around the threshold. The manipulation indicates that individuals prefer to retire at, or close after, the threshold, to avoid an income penalty. Similarly, we interpret the significant discontinuity as moral hazard in UI system.

Third, we show that our results are robust to entries into the UI system. There is no sign of excessive entries prior to retirement. Thus, neither the increase in benefit payments nor the started UI spells are driven by new members. Furthermore, there is heterogeneity in durations among individuals. More specifically, people with low historical use (high entitlement) have higher durations close to retirement than further away. This indicates that individuals might justify excessive benefit use with their previous contribution to the system.

This paper relates to the literature on institutional causes for early retirement. Several studies focus on social security benefits (e.g., Hausman and Wise, 1985; Gruber and Wise, 2000; Feldstein and Liebman, 2002) and the statutory early retirement age (Manoli and Weber, 2016) as causes of early labor market exits. In contrast, we focus on UI benefits as an alternative pathway to retirement in Sweden. Earlier studies find that UI benefits prolong unemployment durations and induce early retirement (Lalive et al., 2006; Kyyrä and Ollikainen, 2008), interacts with early retirement plans (Fitzenberger and Wilke, 2010) and might complement or substitute other insurances to further induce early retirement (Inderbitzin et al., 2016). In addition, Manoli and Weber (2016) exploit discontinuities in a national rule for employer-provided retirement benefits in Austria and find spikes in the retirement frequency at different benefit thresholds.

We add to the literature on UI benefits and early retirement in three ways. First, we use a unique and detailed data set of individual UI payments in Sweden. This allows us to study the link between UI benefits and early retirement in a new institutional context. Second, while previous studies focus on unemployment durations for older workers and the likelihood of early retirement, we evaluate selection into early retirement through the UI system. We provide a novel empirical strategy that draws from the bunching literature (see e.g., Busse et al., 2013; Allen et al., 2016) to examine the distribution of started spells for older workers. Further, we apply common manipulation tests and find that selection occurs at the maximum length of benefits prior to the retirement age. Third, we are able to cleanly illustrate, through both graphical and statistical evidence, that the timing of labor market exits depends on the UI rules.

We also contribute to the literature on moral hazard in UI systems. Earlier studies find that UI benefits cause longer unemployment durations which are interpreted as evidence of moral hazard (Moffitt, 1985; Katz and Meyer, 1990). However, we acknowledge that the duration alone is often an incomplete measure of moral hazard. For example, UI durations are uninformative about employment after the end of benefits (Card et al., 2007) or fail to incorporate liquidity constrains (Chetty, 2008). Likewise, our empirical strategy allows us to identify moral hazard while also examining the underlying mechanism. We apply the theoretical framework of Lindbeck (1995) which separates moral hazard into two separate channels. Our results indicate that early labor market exits is characterized by a strategic use of benefits.

Last, we draw insights from the behavioral economics literature. Social norms affect to what extent individuals are willing to exploit social insurances (Lindbeck and Persson, 2017). This is because deviating from a norm results in a utility loss (Akerlof and Kranton, 2000). Our results contribute by concluding that pecuniary incentives for early retirement seem to outweigh the social stigma of exploiting the UI system.

The remainder of this paper is structured as follows. Section 2 provides information on the Swedish institutional framework for the unemployment insurance. Section 3 presents our rational agent model and adds a complementary theoretical framework. Section 4 describes our data and Section 5 explains our empirical strategy. Section 6 presents our results and the corresponding robustness and heterogeneity checks. Section 7 discusses and concludes our findings.

2 Institutional Background

The Swedish unemployment insurance (UI) system is characterized by two components: voluntary unemployment insurance fund membership and state subsidized financing of the system (Clasen, 2011). The UI scheme is administrated by 28 private unemployment insurance funds with around 3.5 million members in total. The funds have historically been directly affiliated with the Swedish trade unions, although they are nowadays required to be completely independent from other organizations. The close historical relationship between UI funds and trade unions is major explanation for both the high union density and the large number of unemployment fund memberships in Sweden (Kjellberg and Ibsen, 2016). This close relationship created a norm of a dual membership in both a trade union and the related unemployment fund.

The Swedish UI system consists of two protections: an universal basic insurance provided solely by the state and a voluntary insurance offered by the unemployment insurance funds. The universal insurance offers benefits to unemployed workers who lack an unemployment fund membership or individuals who been members for less than 12 months. The universal insurance provides SEK 365 for a maximum of 300 workdays. The voluntary part is based on the individual's former income and it offers benefits for 300 workdays at maximum. However, since unemployed workers do not receive any benefits on weekends, the length of the benefit period spans over 427 days in total (the same logic applies to the universal insurance). The daily benefit is at most 80 percent of an individuals former pay, or a maximum of SEK 680 per day, during the initial 200 days of unemployment.[1] The maximum percentage is reduced to 70 percent during day 201 to 300. The unemployment funds provide insurance to all persons above 20 who have been members for at least 12 months, been working for at least 20 hours per month over the last six months, and are registered at the Swedish Public Employment Service (Arbetsförmedlingen) as unemployed. Furthermore, when workers turn 65, they are automatically unregistered from the system.

The unemployment funds are financed through two sources. The main part, approximately 62 percent, was in 2013 financed by the state through payroll taxes and self-employed contributions (Kjellberg, 2010). The other part is financed through the insurance fund's monthly membership fees, which varies between SEK 85 and SEK 145 depending on fund affiliation.

[1] These benefit levels were active between 2007 and 2015. In 2015, the benefit levels were raised to just above the level prior to 2007. However, the rule of 300 days has been the standard throughout the whole sample period. In our empirical approach we exploit the maximum length of benefits and not the maximum amount of daily benefits. This implies that the exact level of daily benefits is not crucial to our analysis. Furthermore, we use the same argument for individuals only receiving universal insurance; it is the maximum length of benefits that matters, not the benefit level.

3 Theoretical Framework

In this section we present a theoretical framework in three separate sections. First, we introduce the concept of moral hazard within the scope of UI benefits. Second, we use the assumption of a rational agent to model early retirement as a consequence of UI benefits. Third, we draw insights from the behavioral economics literature as an alternative explanation for the retirement decision.

A. Moral Hazard in Unemployment Insurance Systems

The market failure associated with moral hazard arises from information asymmetry where the behavior of the insured agent is unobservable to a principal (Hindriks and Myles, 2013). In terms of UI systems, moral hazard implies that benefits reduce effort and increase both the likelihood and the duration of unemployment. The UI benefits distort relative prices of leisure and consumption, which reduces the marginal incentive to search for a new job. The principal's ability to address the information asymmetry depends on the level of available information and its capacity to monitor the agent. In most cases, the principal attempts to specify precise contracts to induce desired behavior from the agent. For instance, UI contracts always restrict the maximum period of benefits. Furthermore, government interventions are likely to improve efficiency when moral hazard is present. The beneficial effect stems from the government's capacity to tax and subsidize. More precise, taxation of insurances cause firms to offer insurance at less than the fair price. As a consequence, individuals overconsume insurance to a less extent and expend more effort, which improves efficiency.

To what degree moral hazard affects work effort and work absence behavior might be explained by the type of benefit dependency. Lindbeck (1995) differentiates between two types of benefit dependency: those who become pacified due to benefits and those who actively live at the expense of taxpayer's money. This distinction can be interpreted as two channels through which UI benefits affect early retirement. The former would be associated with individuals who stop looking for jobs after becoming unemployed in the end of their working life. The later is more in line with our model and is characterized by individuals who rationally calculate and adjust their labor market participation according to the specific benefit rules.

B. A Simple Rational Agent Model

We construct a model showing how UI benefits induce moral hazard and early retirement. The model builds on the assumption of a rational agent who tries to maximize utility. Consider an old worker who chooses between continue working or retire: $L \in \{0, 1\}$. Here, $L = 1$ denotes leisure (early retirement), and we assume that early retirement is an irreversible decision.[2] The individual selects the alternative that maximizes future utility pay-offs before the official retirement age. The decision takes place over t days, [0, ..., T], where t is defined as number of days prior to the official retirement age. $U_t(L_0)$ defines the utility derived from continue working until the official retirement age, while $U_t(L_1)$ denotes the utility from retiring in the same day. We assume that each day is independent and that utility is fully separable over periods. If the individual chooses an early labor market exit, she is eligible for daily benefits $\bar{B}$ for a maximum amount of days, k.[3] For simplicity we assume that $\bar{B}$ is the same amount in every t. Furthermore, we assume that individuals do not receive any daily wage Y_t or $\bar{B}_t$ after the official retirement day, defined as T.

Overall, there are three factors determining how much an individual can receive in total benefits B when retiring in different t. Formally, the benefit scheme is defined as:

$$B = \begin{cases} \bar{B}_t \times k & \text{if } t \geq k \\ \bar{B}_t \times t & \text{if } t < k \end{cases} \tag{1}$$

An individual who retire at $t > k$ will receive the maximum benefit amount, given by B times k. However, since the worker retire "too early" she will obtain a certain amount of residual days, between the last day of benefits and the official retirement day T. During these residual days, the individual receives neither wage payments nor benefits. We denote these lack of income as daily income penalties P_t. In contrast, an individual who retire at $t = k$ receives also the maximum amount of benefits, although she does not obtain any residual days. Last, retirement at a later stage, $t < k$, results in less than maximum benefits, given by $\bar{B}$ times t.

Figure 1 illustrates the problem of choosing the optimal t with a pay-off tree, where the individual can retire in three different states. Even though some individuals with high enough wages prefer to continue working until the official retirement age, we exclude this option in our model. We also define that Y_t is higher than the daily benefit amount $\bar{B}$ for all t. In addition, there is a (dis)utility from working, φ_t, corresponding to each workday. Furthermore, given that we assume no uncertainty regarding pay-offs, the aggregated utilities are known in every state. The individual will choose to retire in the state where the total benefits from early retirement is as large as possible compared to the utility derived from continued work until the official retirement day. Hence, aggregated income from continue working $\sum Y_t$ can be interpreted as the reservation utility of early retirement.

[2] This is plausible assumption if we believe that workers, prior to retirement, are less attractive on the labor market (Göbel and Zwick, 2009).

[3] For the Swedish UI system, k is equal to 300 work days (427 calendar days).

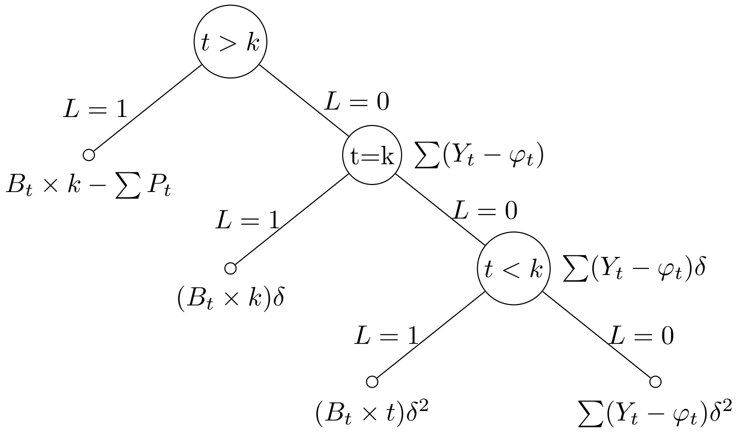

Figure 1: Utility based pay-offs

Notes: This figure shows the utilities from retirement, $L = 1$, and continued work, $L = 0$, for the three states: $t > k$, $t = k$ and $t < k$. For simplicity we assume that the discount factor is $\delta = 1$.

Before we determine the preferred state, we define the different states in terms of utility

$$U_t(L_1) = \begin{cases} \bar{B}_t \times k + \sum \varphi_t - \sum P_t > \sum Y_t & \text{if } t > k \\ \bar{B}_t \times k + \sum \varphi_t > \sum Y_t & \text{if } t \leq k \end{cases} \tag{2}$$

We start from the bottom node, $t < k$, and move upwards to find the optimal state. First, we note that the discounted total benefits at $t < k$ is lower than at $t = k$. Hence, an individual with a strictly increasing and concave utility function will never continue to node $t < k$, which is dominated by $t = k$. Further, node $t > k$ results in a certain amount of penalty days, $\sum P_t$, which means that $t = k$ also dominates $t > k$. Therefore, the model suggests that it is most beneficial to retire in the second state where the number of penalty days are zero and the individual receives maximum benefits. We therefore expect retirement decisions to take place around $t = k$.[4] The logic behind this prediction is that individuals, at this particular t, both maximizes the number of benefit days and the number of payment days, while minimizing the total disutility from working prior to the official retirement age.

C. Bounded Ratonality and Behavorial Economics

The prediction of the model outlined above builds on assumptions of a rational agent. Yet, theories within behavioral economics have either questioned the degree of rationality or added other aspects – such as social norms and identity – as bases for economic decisions. We therefore highlight behavioral concepts that might influence early retirement decisions.

[4] In the Swedish context, this would imply that individuals seek to retire at $t = 427$.

Bounded rationality is the idea that rational decision-making is restricted by cognitive limitations and the tractability of the decision problem. According to Simon (1955) and Conlisk (1996), psychological limitations make the predictive ability incomplete. In the context of our model, individuals might miscalculate the different utility streams from either continued work or early retirement. This implies that workers might choose a retirement day that is not optimal from an individual economic viewpoint. Instead, workers seek a satisfactory day of retirement.

Individuals are not only driven by economic motives. Instead social norms influence decisions and preferences (Akerlof and Kranton, 2000) that affect economic outcomes. To what extent individuals adhere to norms is determined by the private cost – or the social stigma – that individuals experience when opposing the desirable behavior. According to Lindbeck and Persson (2017), the discomfort decreases as more people violate the social rule. This means that norms are endogenous. Lindbeck (1995) claims that a basic dilemma for the welfare state is that generous benefits tend to create many beneficiaries. He hypothesizes that disincentive effects on work effort from UI benefits will increase gradually as others decide to misuse benefits. Thus, to what degree moral hazard inherited in the UI system affects early retirement is determined by how individual's benefit dependency has formed other worker's attitudes towards UI usage. Social norms might initially constrain the influence of economic disincentives in UI systems, but the same disincentives might erode the same constraints in the long run.

4 Data

Our empirical analysis uses data from the Swedish Unemployment Insurance Board (IAF), which contains a detailed longitudinal register over unemployment insurance payments for the period 1999 to 2015. The data covers benefit payments and the corresponding date of compensation. One main feature of the register is the detailed individual enrollment information. More precise, the data contains date specific information on A-kassa membership status, ranging from the first entry to the last exit (retirement). At the age of 65, individuals become ineligible for receiving UI benefits and are automatically unregistered from their respective A-kassa. Thus, we are able to sort out individuals who left the UI system because of retirement. Furthermore, we link payment and membership data to demographic and geographic registers. This provides a rich longitudinal data set with controls, including educational level, whether the person was born in Sweden or not, county of residence, age and gender.

The final data covers 21 135 retirees unregistered from their A-kassa at the retirement age of 65. These retirees have corresponding 2 236 095 weekly benefit payments over the period 1999 to 2015. We also construct a longitudinal series for the following outcome variables: weekly benefit payments, started UI spells and entries into the UI system. Thus, we get a panel for the outcome variables over days to retirement with binary variation in the outcome variables. The benefit of this approach is that we exclude the individual dimension in the data, allowing us to plot the unconditional distribution of payments. Thus, we can cleanly examine the distributions of our outcome variables over days to retirement in an intuitive way. Furthermore, we are able to reintroduce the individual aspect of the data in our econometric analysis.

Table 1: Descriptive statistics

Panel A: Control variables					
	N	Mean	Std.Dev	Max	Min
Education	21,135	2.038	0.732	3	0
Native	21,135	0.886	0.318	1	0
Birth year	21,135	1946	5.583	1954	1934
Gender	21,135	0.400	0.490	1	0
Panel B: Outcome variables					
	N				
Payments	2,198,228				
Started spells	23,090				
Entries	21,135				

Notes: The table presents summary statistics for our sample. Panel A presents the summary statistics for our control variables. Panel B presents the frequencies for the outcome variables. Since the outcome variables are unconditional these are presented as frequencies. We examine the outcomes further in the result section. In addition, we only include the latest individual entry into the system.

Table 1 shows the descriptive statistics for the control and the outcome variables. The sample consists of 60 percent males, the mean level of education is high school and almost 90 percent of the sample was born in Sweden. Note that the outcome variables are presented unconditional of the individual dimension. Hence, these are left as frequencies, which are further examined in detail in the result sections. In addition, entries into the system had missing information for multiple entrants. Consequently, we include the latest entries into the system which are equal to the total number of individuals examined. We proceed with our empirical strategy.

5 Empirical Strategy

To estimate selection into early retirement through the Swedish UI system, there are at least three challenges that are likely to confound the results. First, benefit payments are not random, and thus, comparisons at different days to retirement might contain biases. Without credible exogenous variation in benefit payments, we cannot exclude sorting among payments. Second, payments are insufficient to fully control for selection. For example, the underlying cause might depend on longer durations or excessive initiations of UI spells. Third, empirical evidence suggests that early retirement decisions involve strategic interaction between the employee and the employer (Dorn and Sousa-Poza, 2010). Strategic interactions lead to a reverse causality problem, which result in biased estimates. Thus, we cannot distinguish whether the cause of retirement is an active choice by the employee or the consequence of downsizing in a specific sector.

To tackle these challenges, we use the specific setting of the Swedish UI system prior to the official retirement age. One essential feature of the Swedish UI setting is that individuals are eligible for benefits up 427 days. This creates an artificial threshold where individuals, based on the previous theoretical discussion, are encouraged into early retirement. Thus, the probability of using UI benefits increases exogenously at the threshold which provides plausible exogenous variation in the number of benefit claimants. We also expect that strategic individuals, who seek to retire early through the UI system, are well informed about their own retirement situation.

This setting is particularly useful to tackle the first two challenges. We use the maximum length of benefit as a threshold providing exogenous variation. Accordingly, our hypothesis is that there should be a jump in benefit payments at 427 days prior to retirement. In addition, the Swedish register data is well suited to examine selection in benefits payments through the number of started spells. In the case of selection, we expect started UI spells to bunch at the maximum length of benefits prior to the official retirement age. Last, our strategy use that the Swedish UI system is voluntary, and thus, individuals can strategically enter the UI system. This allows us to check for excessive entries to control for potential selection prior to the threshold.

We address the statistical challenges through a graphical analysis, and further apply appropriate regressions and tests to control the significance of discontinuities. Note that we do not interpret jumps in the number of payments or started spells as causal effects, since our identification strategy assumes that there is manipulation in the outcome variables. The excess payments or started spells at the threshold are likely to be caused by strategic use of benefits. Furthermore, we use advances in the bunching literature (for overview, see Kleven, 2016) to address the manipulation. Formally, we apply density tests with different bandwidths for our running variable, which controls whether the manipulation is significant or not. A significant result rejects the hypothesis of no manipulation.

The third challenge of the potential biases is likely to be a minor issue in the institutional setting of the Swedish UI system. The problem of strategic layoffs is associated with flexible labor markets with weak employee protection (Card et al., 2007). In contrast, Swedish employees are protected by the Employment Protection Act, which applies a "First-in, last-out" principle. This rule explicitly inhibits age discrimination. For example, Dorn and Sousa-Poza (2010) find that the early retirement age is the main determinant for forced layoffs. However, the early retirement age is common knowledge for the employers, which makes a strategic layoff initiated by the employer more likely. In contrast, UI association is private information and not necessarily known by the employer, and consequently making a strategic layoff by the employer less likely. We now proceed to our result section where we present our results in three sections.

6 Results

In this section, we present our results in three separate subsections. First, we examine frequencies of weekly benefit payments close to retirement, in particular around the threshold of 427 days prior to the official retirement age. This establishes the fundamental relationship between days to retirement and benefit payments. Second, we test for selection close to the same threshold by examining the number of started UI spells at different stages. Given the logic of our theoretical model and the Swedish institutional setting, a started UI spell late in life implies retirement. Third, we test both the robustness of our findings and heterogeneity by evaluating entries into the system and by introducing entitlement to benefits. For each separate subsection, we provide necessary empirical specifications and placebo tests.

A. Weekly Payments

We start by plotting the unconditional distribution of weekly payments of benefits over days to retirement. Figure 2 plots benefit payments for 1 800 days prior to retirement binned into monthly averages. Here, zero marks the official retirement age of 65 and the distribution is divided into spans of 427 days. There is an exponential increase in benefit payments starting two spans away from the threshold. This establishes a positive correlation between benefit use and worker's age. However, while the underlying distribution resembles a smooth exponential function, the regressed means between time-spans shows a discontinuity at the 427-days threshold.

This potential discontinuity is the main focus for the proceeding analysis. Although we intuitively expect older unemployed to use more benefits because of declining productivity or worse labor market opportunities (Göbel and Zwick, 2009), such factors do not necessarily explain the discontinuity. Instead, the disproportionate increase in benefits use at 427 days prior to retirement might suggest that the Swedish UI setting induce moral hazard. However, we cannot exclude confounding factors by just looking at the graph, nor that the discontinuity is significant at the 427-days threshold. Thus, we analyze the threshold in two steps: first by testing the discontinuity in a local linear regression framework, and second, by evaluating a placebo threshold.

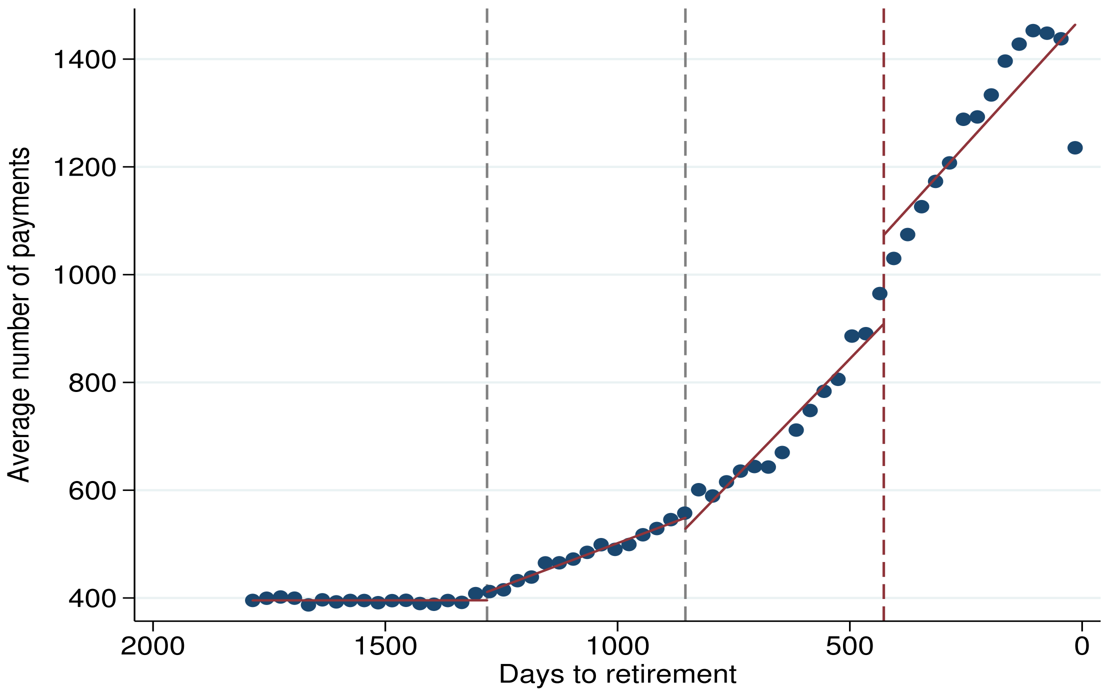

Figure 2: Weekly payments of benefits

Notes: This figure shows weekly benefit payments over days to retirement. Each payment is plotted unconditional of other payments (there is no individual dimension in the figure). Payments are binned into monthly averages for 60 months prior to retirement. The dashed red line is the 427-days threshold and the corresponding dashed grey lines are 427 days intervals.

To test the discontinuity, we estimate local linear regressions for different bandwidths and polynomials. A larger bandwidth widens the sample away from the threshold, which reduces the variance while retracting significance from the threshold. Formally, we estimate the following specification:

$$B_t = \alpha + \gamma D_t + \beta_1(R_t - k) + \beta_2(R_t - k)^2 + \beta_3(R_t - k)D + \beta_4(R_t - k)^2 D + X_t\phi + \eta_t \tag{3}$$

where B_t is a dummy variable that indicates if there is payment at day t, D is a treatment dummy that denotes where the chosen threshold occurs, R_t is days to retirement, which is our running variable. R_t is defined as $k - \tau \leq X \leq k + \tau$ where k is the chosen threshold and τ is the bandwidth. In addition, X_t is a vector of control variables attached to each payment, which includes demographics variables, geographic information of counties, time of entry into the UI system, A-kassa membership status and individual identification.

While each payment is separated from the individual identification to construct the running variable, we reintroduce the personal identification into the regressions as a control. This makes it possible to control for individual confounders. We also use interactions between the treatment dummy and the running variable to let the slopes flexibly differ at both sides of the threshold. This is necessary to model the correct functional form of the running variable (Lee and Lemieuxa, 2010). In addition, we estimate the regression for both the first and second order polynomial of the running variable, which allows for a non-linear functional form. We exclude higher order polynomials because it leads to noisy estimates and poor approximation of standard errors (Gelman and Imbens, 2014).

Table 2 reports the results from equation (3) without the vector of controls for different bandwidths. The columns without interactions or polynomials show a significant increase around the threshold. These coefficients are quite robust over bandwidths and becomes increasingly significant with the larger samples. However, by introducing more flexible specifications the results become noisy and even shift sign. This suggests that there is no discontinuity in number of payments around the 427-days threshold. Instead, benefit payments seem to follow a smoothly increasing function, which is also supported by the monthly bins of payments in Figure 2. Thus, this general increase in the use of benefits are in line with the hypothesis of declining labor market opportunities for older workers. In addition, moral hazard could both explain the narrow increase at the threshold and the overall rise in benefits use. The corresponding regressions with a full set of control variables does not alter the coefficients (see Table A.1 in the appendix). This provides evidence that confounding factors play a minor role determining the relationship between benefits use and days to retirement.

We continue by running equation (3) with a placebo threshold, 854 days away from retirement. Table 3 reports the results from this local linear regression. Here, we note that the previous results might be deceptive, as there is a comparable increase at 427-days away from the original threshold. Likewise, these placebo thresholds are not significant for more flexible specifications. The results from the corresponding placebo regressions with controls show similar estimates (see Table A.2 in the appendix). Nonetheless, the results resemble that of an exponential function, similar to the distribution of payments in Figure 2. This function is smooth and there is no significant discontinuity in the number of payments. Consequently, these regressions indicate that there is no disproportionate use in payments around the 427-days threshold. We further explore discontinuities in started spells to explain the increase in benefits use.

B. Started Unemployment Insurance Spells

In this subsection, we examine started UI spells around the 427-days threshold. The method draws upon the findings in the previous subsection, where we established a positive correlation between payments and the worker's age. Because there was no discontinuity in payments, we proceed to test whether the increase in payments is driven by started UI spells at the specific threshold. The intuition builds on the theoretical section, where people seek to retire at 427 days prior to retirement to receive the full length of benefits

Table 2: Local linear regressions of benefit payments

	Bandwidth +/- 45 days			Bandwidth +/- 90 days			Bandwidth +/- 180 days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post (427 days)	0.00807**	-0.184***	9.944***	0.0133***	-0.00585	-1.153***	0.0106***	-0.00769	-0.0937
	(0.00388)	(0.0636)	(2.352)	(0.00275)	(0.0227)	(0.429)	(0.00194)	(0.00828)	(0.0836)
Observations	245,262	245,262	245,262	489,309	489,309	489,309	979,605	979,605	979,605
Interaction	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table shows the output from equation (3), excluding control variables (see Table A.2 in the appendix), with benefit payments as the dependent variable. The bandwidths are 45, 90 and 180 days. The post variable marks the 427-days threshold. Columns (1), (4) and (7) show local linear regressions without interactions or polynomials. Columns (2), (5) and (8) use interactions and allows the slope to differ at both sides of the threshold. Columns (3), (6) and (9) use both interactions and quadratic polynomials. Statistical significance: *** $p<0.01$, ** $p<0.05$, *$p<0.1$

Table 3: Placebo local linear regressions of benefit payments

	Bandwidth +/- 45 days			Bandwidth +/- 90 days			Bandwidth +/- 180 days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post (854 days)	0.00818**	-0.215*	33.56***	0.00850***	-0.0166	0.155	0.00991***	-0.0151	-0.792**
	(0.00400)	(0.130)	(9.487)	(0.00284)	(0.0465)	(1.709)	(0.00201)	(0.0166)	(0.312)
Observations	194,025	194,025	194,025	386,250	386,250	386,250	771,655	771,655	771,655
Interaction	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic	No	No	Yes	No	No	Yes	No	No	Yes

Notes: This table shows the output from equation (3), excluding control variables (see Table A.3 in the appendix), with a placebo threshold at 854 days to retirement. Similar to Table 2, benefit payments is the dependent variable and the bandwidths are 45, 90 and 180 days. The post variable marks the 427-days threshold. Columns (1), (4) and (7) show local linear regressions without interactions or polynomials. Columns (2), (5) and (8) use interactions and allows the slope to differ at both sides of the threshold. Columns (3), (6) and (9) use both interactions and quadratic polynomials. Statistical significance: *** p<0.01, ** p<0.05, *p<0.1

while holding income losses at a minimum. This argument assumes that people are able to self-select into unemployment.

Figure 3 shows the distribution of started spells binned into monthly averages. The first bin starts at 1 800 days prior to retirement. Similar to the distribution of payments, started UI spells increase quickly around 854 days prior to the official retirement age. But, unlike the distribution of payments, there is a peak at the 427-day threshold followed by sharp decline. There is also a notable jump is the distribution at the 427-days threshold. Hence, this distribution contradicts the hypothesis of declining productivity with age. If this hypothesis would be the main explanation to early retirement, we expect an increase in the number of started UI spells up until retirement. Instead, the plot indicates that individuals are aware of the specific rules in Swedish UI system. Because the distribution peaks exactly at 427 days prior to 65 indicates some sort of maximizing behavior, and that individuals try to receive the maximum amount of benefits before retirement.

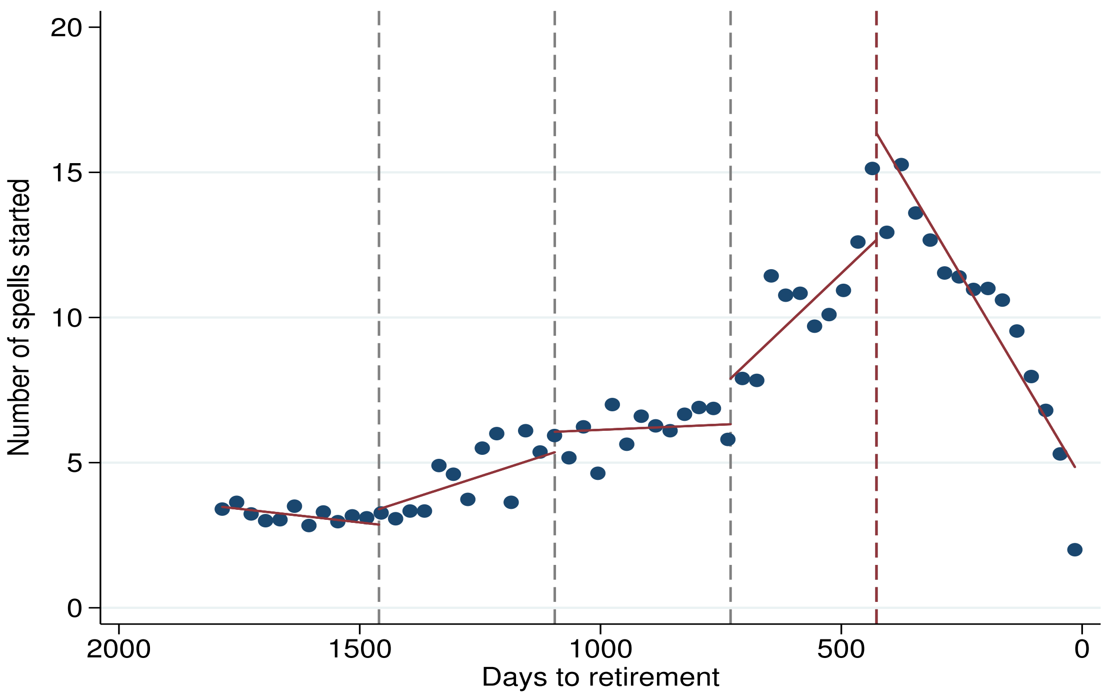

Figure 3: Started UI spells

Notes: The figure plots the distribution of started UI spells binned into monthly averages. The red dashed line is the 427-days threshold. The grey dashed lines mark yearly intervals to the retirement age of 65.

This result is in line with our hypothesis saying that individuals should respond to incentives inherited in the UI system. Also, the pattern in the data corresponds to the prediction of our model, which suggest that individuals should time their early retirement through the UI system according to the maximum length benefits. Figure 3 does also propose that moral hazard is present in the Swedish UI system. However, despite the bunch of started spell around the 427-days threshold, we cannot exclude that some individuals seem to be content with starting spells slightly before or after this point. This relates to our discussion on bounded rationality, which proposes that individuals seek a satisfactory rather than an optimal solution.

In order to test the significance of the discontinuity of started UI spells at the 427-days threshold, we use common manipulation tests. This allows us to simultaneously assess whether there is selection around the threshold and if the discontinuity is significant. It is increasingly common in the bunching literature to use manipulation tests to check for selection around arbitrary thresholds (Kleven, 2016). If the test shows significant selection, it implies that there is a discontinuity and that a disproportionate number of spells start at the threshold.

First, we employ McCrary's density test (McCrary, 2008) that divides the running variable into equally sized bins with their respective frequencies and estimates a local linear regression with the frequencies as the dependent variable. Figure 4 shows the result from McCrary's density test for a restricted sample, starting with observations at 900 days prior to retirement. In addition, the red line marks the 427-days threshold and the inner lines show the point estimates for the discontinuity. The discontinuity is significant at a five percent level for the pre-selected number of bins and indicates that there is selection after the threshold.

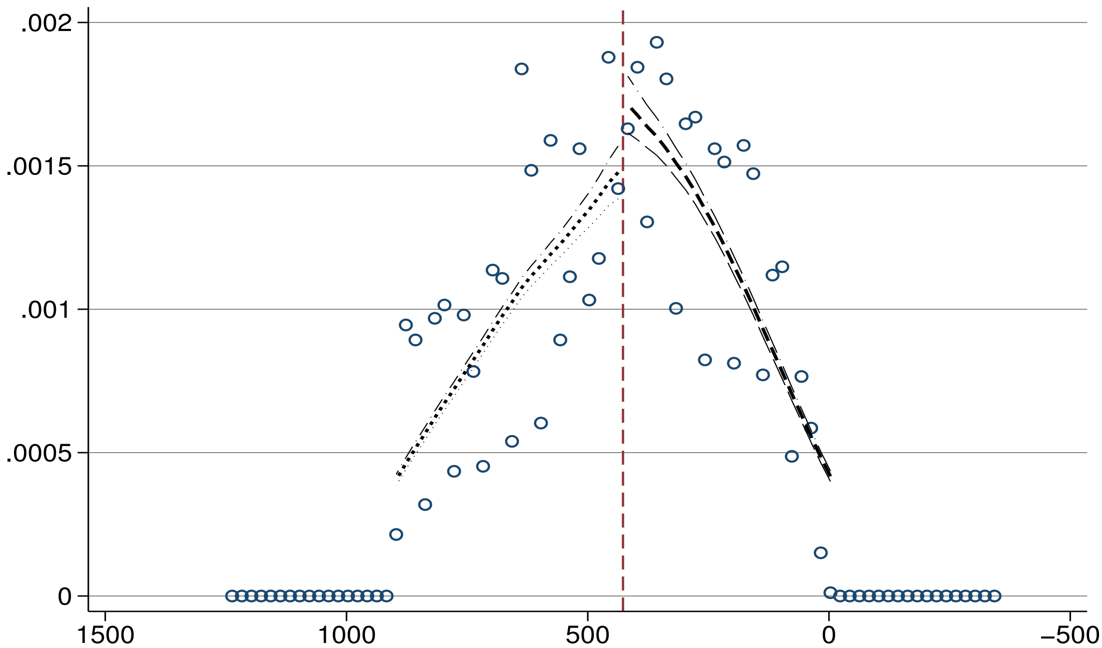

Figure 4: McCrary's density test

Notes: The figure plots McCrary's density test. Around the threshold, the outer lines marks the confidence intervals for the point estimates (the bold inner lines). This graph use 20 bins (pre-selected) with a p-value equal to 0.045. See Table A.3 in the appendix for additional bin selections and p-values.

Although the McCrary test is commonly used to test for manipulation, it is less suitable for handling a discrete running variable and smaller samples. In our case, the significance for the discontinuity is slightly below the five percent level and it is sensitive to different bin selections (see Table A.3 in the appendix). This is because the McCrary test uses pre-binning of the running variable. Consequently, pre-binning lowers the standard errors for the null hypothesis of no manipulation, and might excessively reject that there is no manipulation (Frandsen, 2017). To test the robustness of the McCrary test, we use a similar method proposed by Cattaneo et al. (2016a) that avoids the pre-binning, which performs better in small samples and allows for both bandwidth selection and flexible specification of polynomials. One concern raised in Cattaneo et al. (2016b) is that first-order polynomials also wrongfully rejects the null of no manipulation.

Table 4 shows the results from Cattaneo's density test for different bandwidths and polynomial orders. Reassuringly, our estimates are significant over different polynomial orders, which provides further robustness that selection occurs at the threshold. The smaller bandwidths are significant although they use fewer observations, while the wider bandwidths are insignificant. This is in line with the finding that selection happens quite close to threshold. Furthermore, larger bandwidths are likely to miss the mechanism at the threshold.

Table 4: Discontinuity test for selection

	Polynomial order			
Bandwidth (Days)	$p = 1$	$p = 2$	$p = 3$	$p = 4$
25	.0013***	.015***	.0028***	0.0034***
	(.0003)	(.0004)	(.0006)	(.0012)
50	.0001	.001***	.003***	.003***
	(.0002)	(.0003)	(.0005)	(.0007)
100	-.0002	-.0003	0.0008***	.0011***
	(.00012)	(.0002)	(.0003)	(.0004)
150	.00002	-.0003	-0.00032	.0002
	(.0001)	(.0002)	(.0002)	(.0002)

Notes: The table shows manipulation density estimates for several bandwidths and polynomial orders (for the running variable). The bandwidth B=25 use 310 effective observations on each side of the threshold, B=50 use 654 effective observations, B=100 use 1220 effective observations and B=150 use 1714. For details regarding *rddensity* see Cattaneo et al. (2016a). Statistical significance: *** p<0.01, ** p<0.05, *p<0.1

We highlight three properties from these results. First, while the selection is significant it is likely to understate the mechanism. This is because people are unable to precisely manipulate when to enter unemployment. Hence, people who seek to use the full length of benefits before retirement could be content with a second-best alternative, which could explain the slopes at both sides of the threshold. Second, there is a considerable increase in started spells, more than three times as high at the threshold compared to the initial level. It is likely that the excess in started spells drives the increase in benefit payments. This suggest that there is moral hazard in the use of benefits. Furthermore, the downward sloping pattern after the threshold strengthen the case that it is moral hazard and not productivity that drives benefits use. Otherwise, the started UI spells would continue to grow until retirement. Third, the manipulation around the threshold implies a certain degree of awareness. This corresponds to the category of people who use benefits in a calculated way. In accordance, the pecuniary gain seems to exceed the stigma associated with living on benefits. However, we cannot exclude that started spells would be fewer in some alternative social context.

C. Robustness Tests and Heterogeneity

In the previous subsections, we established that there is a steep increase in benefits payments close to retirement, and that these payments are driven by started UI spells. Yet, these results exclude two potential explanations: first, does the increase in initiated UI spells depend on excessive entries into the UI system, and second, is the increase in benefit payments driven by increased durations? The latter adds a complementary question whether individuals differ in their durations depending on their historical benefits usage. Thus, we examine if longstanding members feel entitled to claim benefits close to retirement. According to Lindbeck (1995), benefits from welfare systems are often described today as "citizens' rights" or entitlements, for which the individual has qualified by paying contributions earlier. In general, individuals are assumed to be less hesitant to live on benefit schemes the more they have previously contributed to the system. We therefore construct an entitlement measure where we divide historical usage of benefits with the length of the membership. Thus, a low value means high entitlement, and vice versa.

In Figure 5, we plot entries starting at 1 800 days prior to retirement, where the entries are binned into monthly averages. We expect strategic individuals to enter at least one year prior to the threshold, which is the minimum membership length for becoming eligible for full benefits. The distribution of entries is noisy and reveals no clear pattern. Those few bins that are seemingly high are no outliers considering the full sample of entries (see Figure A.3 in the appendix). Furthermore, entries close to retirement are negatively correlated with days to retirement. This could be because the benefit of joining an A-

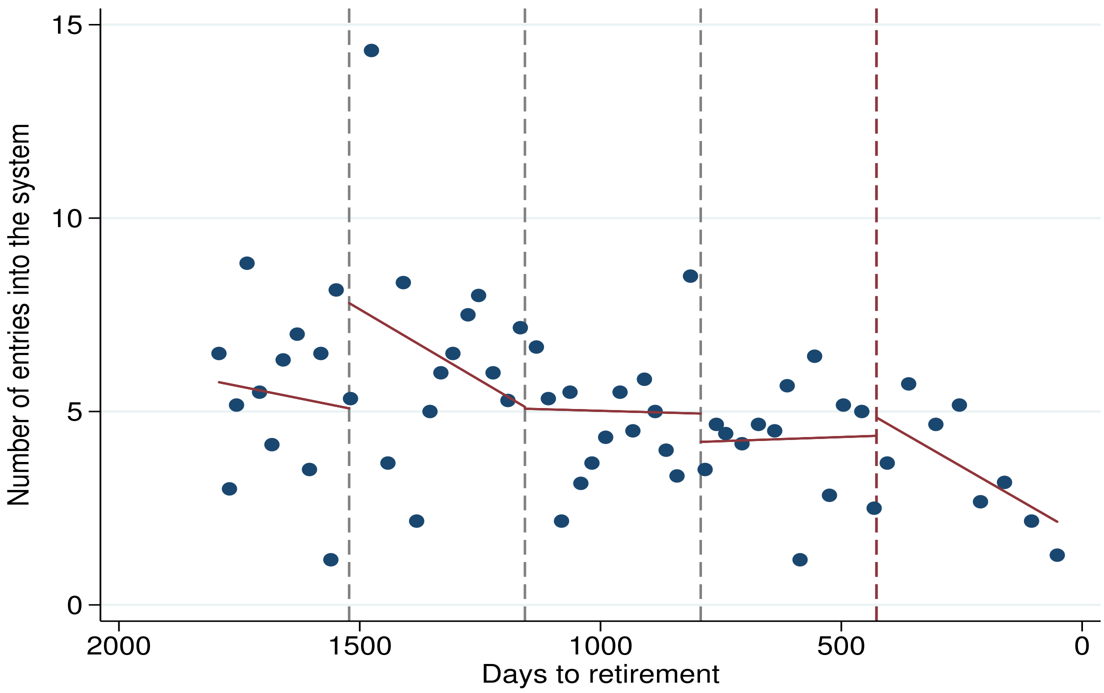

Figure 5: Entries into the UI system

Notes: The figure shows entries into the UI system over days to retirement for 1800 days prior to retirement age. Entries are binned into monthly averages. The red dashed line marks the 427-day threshold while the grey lines are yearly markers away from the threshold.

kassa decreases with age, or due to reduced uncertainty regarding income and health as an older worker. Overall, the distribution of entries shows no clear sign of selection close to retirement. This indicates that excessive entries do not drive the bunching in started UI spells.

The other potential explanation to increased benefits use is UI durations. If the durations are longer, the frequencies of unconditional payments will increase. Figure 6 plots the average number of UI weeks, given the individual entitlement, for three different time-spans. We restrict the sample to cover people with at least ten years of membership. There is a clear difference in durations given time period and entitlement. The spans of 427 days at each side of our 427-days threshold plots almost identical distributions; a sharp increase for high entitlements (low ratio) that flattens out for individuals with lower entitlements. In contrast, the green line marks the average duration for an additional span of 427 days away from the threshold. In this time span, those with high entitlement use about half the durations compared to those close to the threshold. Furthermore, because the average duration is considerably longer closer to the official retirement age, around the 427-days threshold, strengthens the hypothesis that individuals use UI benefits as an early retirement scheme. The extended length of later UI spells suggest that individuals do not reenter the labor market after becoming unemployed. Instead they seem to exit the labor market through the UI system.

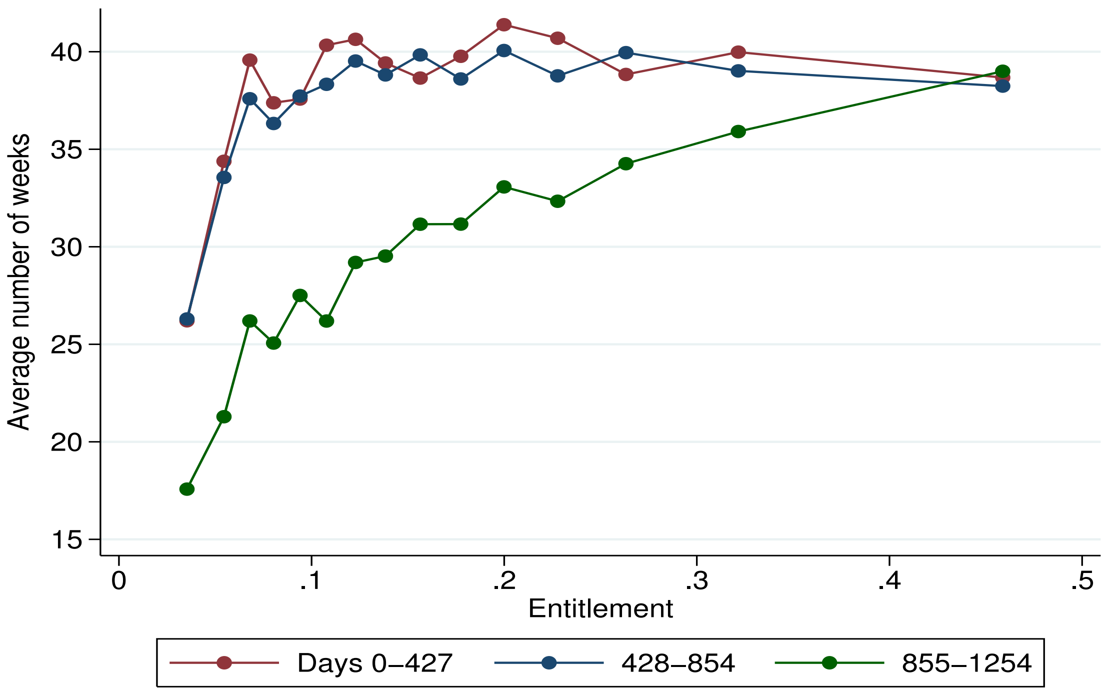

Figure 6: Entitlement and average duration

Notes: This figure plots the average duration (weeks) per period over the individual entitlement level. The series shows 427 days spans prior to retirement. High entitlement (closer towards zero) equals a low total use of UI benefits relative to the membership duration.

We highlight two findings from Figure 6. First, without entitlement as an component which affects the retirement decision, we expect people with low historical usage of benefits to use less benefits than those with high historical usage. This is because our entitlement measure also measures the historical risk of unemployment. Thus, without entitlement, the average durations should be shorter for those with low historical use, which is similar to the green line. In contrast, the red and blue show high average durations even for those with low historical use. Likewise, the lines converge for those with low entitlement (high ratio), which suggests that those individuals behave similar irrespective of days to retirement. Consequently, this suggest that entitlement affects the retirement decision more for those with higher entitlement. Second, the red and blue lines are similar in durations and entitlements. Previous results indicate that both benefit payments and started UI spells starts to increase sharply about 854 days prior to the retirement age. Hence, the selection into early retirement might occur before the threshold. Although this evidence is descriptive, it sheds light on the potential underlying mechanism that drives benefits use.

7 Discussion and Conclusion

In this paper, we investigate whether Swedish workers use UI benefits as a pathway to retirement. In Sweden, workers are eligible for benefits for a maximum of 427 days. This creates an artificial benefit threshold at 427 days prior to the official retirement age that provides exogenous variation. We exploit this variation by using a novel quasi-experimental approach that is inspired by the empirical literature of bunching. The analysis builds on unique Swedish data consisting of a detailed longitudinal register of UI benefit payments for the period 1999 to 2015.

Our quasi-experimental strategy allows us to both identify moral hazard and to detect how UI benefits induce early retirement. The sharp peak in the number of started UI spells at the 427-days threshold shows that individuals try to maximize benefits before exiting the labor market. In addition, the extended average length of unemployment spells for older workers indicates that they do not re-enter the labor market after becoming unemploymed. Instead, they seem to exit the labor force through a final unemployment spell. Given these evidence, we conclude that Swedish workers self-select into early retirement by using UI benefits.

Our main results have several implications. First, our results suggest that individuals act in a strategic and well-planned manner. The clear pattern in the data would not be possible unless a significant proportion of the individuals are informed regarding how to maximize benefits. Hence, the prediction of our model, which suggested that early retirement decisions should bunch at the maximum length of benefits, is fairly accurate. On the contrary, we cannot disregard that a fairly large share of the UI spells did not start at the threshold. Thus, we cannot completely reject complementary explanations, such as bounded rationality, which suggest that individuals seek a satisfying outcome rather than an optimal one. Second, we cannot exclude the possibility that started spells at the 427-days threshold are employer initiated rather than driven by the employee. However, given the exact timing of the spell start and given the strong labor market protection for older workers in Sweden, we find it more likely that the decision is voluntarily. Third, to what degree social norms influence the early retirement decision is hard to derive from our results. According to Akerlof and Kranton (2000), individual's willingness to adhere to social norms is determined by the social stigma they experience when opposing a desirable behaviour. Our results show that the there is no social stigma strong enough to prohibit all workers from using the UI system as an early retirement scheme.

One limitation of our findings is the degree of external validity. The Swedish UI system is rather unique and it is unclear whether the maximum length of benefits would impose similar discontinuities in benefit usage for workers in other countries. Nonetheless, our results confirm basic economic theory and previous studies showing that individuals respond to incentives inherent in UI systems. It is therefore reasonable that similar early retirement behavior could be observed in other contexts.

In terms of further studies, there are several potential research questions that follow from our results. First, due to the construction of our data we were unable to divide our data into subgroups. Hence, a potential research question is to investigate whether various occupational groups in Sweden, in different unemployment insurance funds, act dissimilar at the threshold. Another relevant research question is to include all UI members – and not only those that use benefits prior to retirement – by scaling up the sample size considerably. This would clarify the extent of the early retirement problem in proportion to the whole UI system.

Based on the theory of moral hazard, the state subsidies of the Swedish UI system contribute to early labor market exits. However, if policy makers would address the issue of moral hazard by withdrawing subsidies it would affect the UI protection of younger workers as well. The purpose of a UI system is to ease economic hardship of job losers and improve matching on the labor market, among other things (Inderbitzin et al., 2016). Thus, by adjusting benefit levels or the maximum benefit duration, the government also has to consider adverse effects on other groups. A more concentrated policy implication is to address the issue of asymmetric information through monitoring. While earlier studies focus on the increased likelihood of early retirement due to UI benefits, our approach implies that we can identify the timing of the retirement decision. Hence, by developing methods for better monitoring of older workers around the threshold, it might be possible to both detect and avoid early retirement through the UI system. Provided these methods are efficient enough to address moral hazard, compared to its implementation costs, it could increase social welfare.

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Appendix

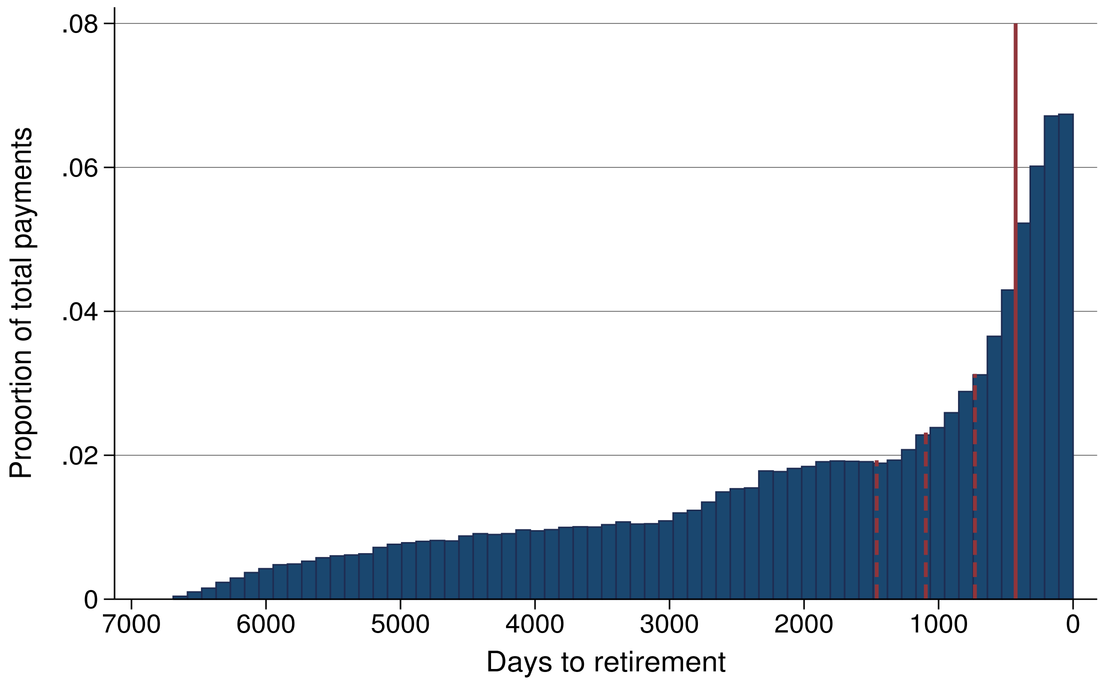

Figure A.1: Unconditional distribution of weekly payments

Notes: The figure shows the full distribution of payments for the sample, 2,198,228 in total, over days to retirement. The solid red line marks the 427-days threshold and the red dashed lines are yearly markers away from the retirement age of 65. The sample covers payments for the period 1999-2015.

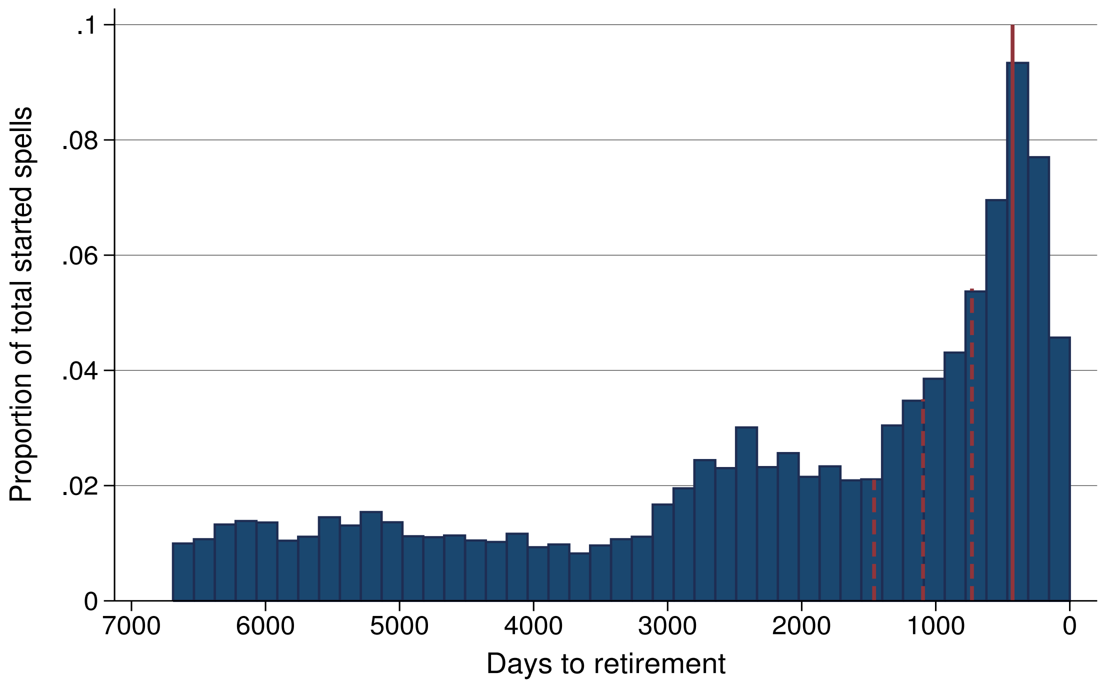

Figure A.2: Unconditional distribution of started UI spells

Notes: The figure plots the full distribution of started UI spells, 23,090 in total, over days to retirement. The solid red line marks the 427-days threshold and the red dashed lines are yearly markers away from the official retirement age.

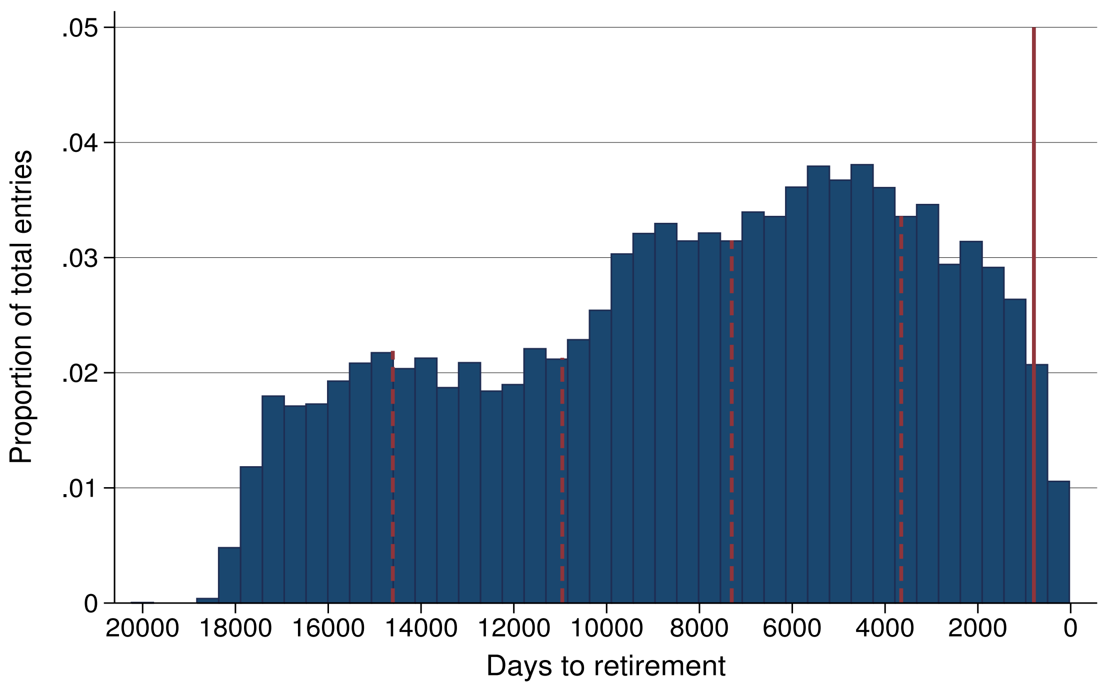

Figure A.3: Unconditional distribution of entries into the UI system

Notes: The figure shows the full distribution of entries into the UI system over days to retirement, for the whole sample. These are 21,135 in total. The solid red line marks the 427-days threshold and the red dashed lines are 10-year markers away from the retirement age of 65.

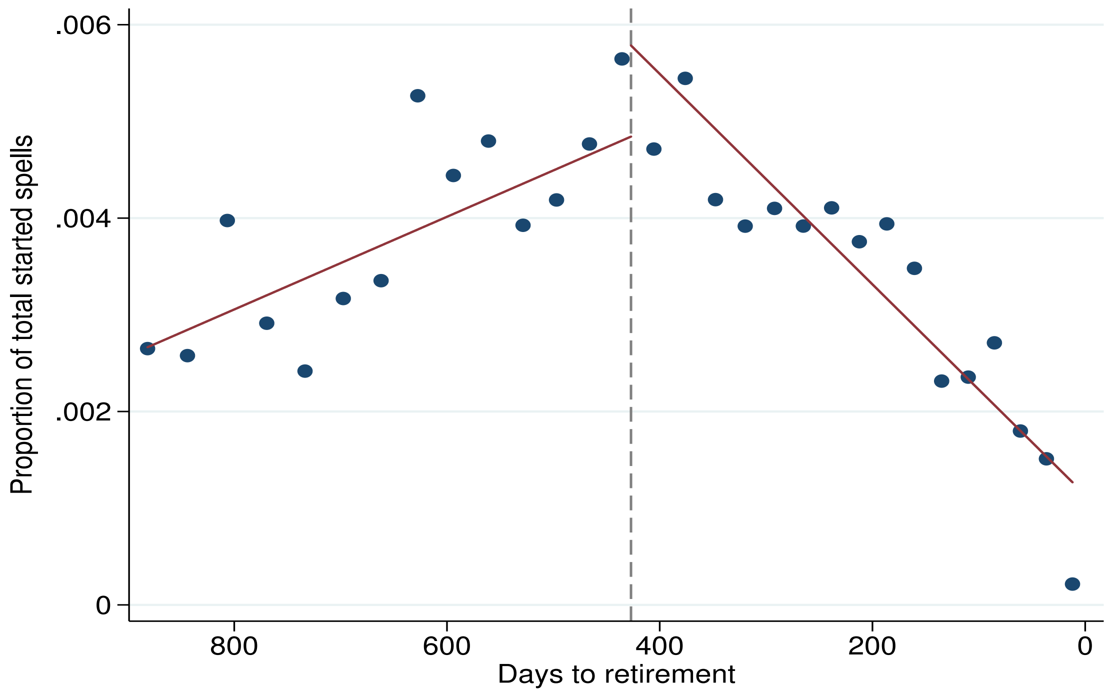

Figure A.4: Started UI spells close to the threshold

Notes: The figure shows the distribution of started UI spells (900 days prior to retirement). The dashed grey line is the 427-days threshold. This distribution is used in the McCrary tests.

Table A.1: Local linear regressions of benefit payments with controls

	Bandwidth +/- 45 days			Bandwidth +/- 90 days			Bandwidth +/- 180 days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post (427 days)	0.00860**	-0.182***	9.865***	0.0136***	-0.00492	-1.118***	0.0109***	-0.00473	-0.0941
	(0.00386)	(0.0632)	(2.336)	(0.00273)	(0.0226)	(0.426)	(0.00193)	(0.00822)	(0.0830)
Observations	245,262	245,262	245,262	489,309	489,309	489,309	979,605	979,605	979,605
Interaction	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic	No	No	Yes	No	No	Yes	No	No	Yes
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the output from equation (3) for the threshold at 427 days to retirement. In addition, controls are included for individual identifiction, A-kassa membership, year of payment, education, gender, county and whether the person is born in Sweden or not. Benefit payments is the dependent variable and the bandwidths are 45, 90 and 180 days. The post variable marks the 427-days threshold. Columns (1), (4) and (7) shows local linear regressions without interactions or polynomials. Columns (2), (5) and (8) use interactions and allows the slope to differ at both sides of the threshold. Columns (3), (6) and (9) use both interactions and quadratic polynomials. Statistical significance: *** p<0.01, ** p<0.05, *p<0.1

Table A.2: Placebo local linear regressions with controls of benefit payments

	Bandwidth +/- 45 days			Bandwidth +/- 90 days			Bandwidth +/- 180 days		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post (427 days)	0.00808**	-0.211	32.61***	0.00845***	-0.0144	0.0962	0.00983***	-0.0135	-0.776**
	(0.00398)	(0.130)	(9.446)	(0.00283)	(0.0463)	(1.702)	(0.00200)	(0.0165)	(0.311)
Observations	194,025	194,025	194,025	386,250	386,250	386,250	771,655	771,655	771,655
Interaction	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Quadratic	No	No	Yes	No	No	Yes	No	No	Yes
Main controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the output from equation (3) for the placebo threshold at 854 days to retirement. In addition, controls are included for individual identifiction, A-kassa membership, year of payment, education, gender, county and whether the person is born in Sweden or not. Benefit payments is the dependent variable and the bandwidths are 45, 90 and 180 days. The post variable marks the 427-days threshold. Columns (1), (4) and (7) shows local linear regressions without interactions or polynomials. Columns (2), (5) and (8) use interactions and allows the slope to differ at both sides of the threshold. Columns (3), (6) and (9) use both interactions and quadratic polynomials. Statistical significance: *** $p<0.01$, ** $p<0.05$, *$p<0.1$

Table A.3: McCrary test statistic

	Number of bins		
	20	25	30
Discontinuity	.183*	.135**	0.072*
P-value	0.045	0.055	0.070

Notes: This table shows point estimates and corresponding p-values for the McCrary test with different bin selections. The p-value grows with the number of bins which is a possible problem with the McCrary test for discrete variables (Frandsen, 2017). Statistical significance: *** p<0.01, ** p<0.05, *p<0.1