Essays on the Economics of Household Finance and Social Insurance

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Proefschrift

ter verkrijging van

de graad van doctor aan de Universiteit Leiden
op gezag van rector magnificus prof.dr.ir. H.Bijl,
volgens besluit van het college voor promoties
te verdedigen op woensdag 18 december 2024
klokke 10:00 uur

door

Pim Leonard Koopmans
geboren te Zoetermeer
in 1995

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The research in this book is sponsored by Instituut GAK.

Preface

I thank Leiden University for providing me the opportunity to work for the Economics department. Performing research at and interacting with the department allowed me to develop as a researcher and as an individual.

My gratitude goes out to Marike Knoef, Max van Lent, and Kees Goudswaard for their supervision. I also thank Jim Been, Marike Knoef, and Max van Lent for our work together on the papers in the dissertation.

I additionally thank my colleagues in the economics department for the past years and the interesting discussions we held. I enjoyed writing my dissertation and happily look back on the past years I spent at the Leiden University's Economics department.

Additionally, I wish to express my gratitude to the members of the Ph.D. committee Lisa Brüggen, Wilco van Dijk, Egbert Jongen, and Pierre Koning for being part of the Ph.D. commission.

My gratitude also goes out to Stefan van Woelderen and Dominic Keyzer for setting up the data for Chapter 3 and the assistance they provided along the way. This chapter would not be possible without them.

I thank Marloes Lammers, Carla van Deursen, and Frank Schreuder for preparing the data used in Chapter 5. My gratitude additionally goes out to Marloes Lammers, Margaretha Buurman and Miranda de Vries for useful comments, discussions, and suggestions.

My gratitude goes out to Henri de Groot, Zichen Deng, Sándor Sóvágó, and Pierre Koning for their support prior to my Ph.D. trajectory, as well as more generally helping me enter the academic world of Economics. I also thank Heike Vethaak for informing me of the Ph.D. vacancy at Leiden University.

Finally, I thank my family and friends. In particular, I extend my gratitude to my parents, Carl and Péronne, who have helped me tremendously throughout my life. Without their support, I cannot emphasize enough that I would not be where I am now.

My Ph.D. at Leiden University was an excellent learning experience. I look forward to hearing about future developments at the department, and to keep in touch irrespective of where future research takes me.

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Chapter 1

Introduction

The life cycle model has been extensively studied (Ando and Modigliani (1963); Heckman (1976); Modigliani and Brumberg (1954)). According to this model, individuals smooth consumption over time. In case the life cycle model holds, any shock that affects income and spending behavior is either anticipated and smoothed over the life cycle, or smoothed over the rest of the life cycle if unanticipated.

Empirical studies, however, repeatedly find deviations from the life cycle model (Bikker et al. (2012); J. R. Brown et al. (2008); Deaton (1986); White (1978)). These deviations have various implications for household finance over the life cycle, often manifesting in intertemporal consumption behavior characterized by significant heterogeneity and short-term planning. Departures from the life cycle model have important implications for household finance.

Individuals face many risks and events throughout the life cycle that impact household finance. These include but are not limited to disability, retirement, unemployment, and childbirth. Risks and events impact income and expenditure may justify policies that assist individuals in smoothing their life cycle consumption.

The Netherlands is characterized by a strong degree of social insurance and employment protection to cover the aforementioned risks (OECD (2021)). These policies are designed to provide safety nets and help households smooth their consumption over time. In particular, retirement benefits help smooth and maintain consumption in old age, maternity leave and childcare subsidies provide income and return-to-work options for taking care of children, and insurance covers disability risk. Understanding whether these social insurance schemes have the intended effect is key to informing policy.

This dissertation empirically investigates the impact of retirement, childbirth, and disability insurance on household income, spending, and time use. This involves assessing how the availability and utilization of social insurance shape individuals' decisions regarding income, expenditure, savings behavior, time use, employment and labor force engagement. To investigate these effects, this dissertation uses various types of microdata, ranging from survey data to bank transactions. Notably, the bank transactions are almost unique in the literature. The analyses aim to contribute to a more comprehensive understanding of how social safety nets shape individuals' economic behavior and outcomes. Additionally, this research endeavors to inform policy discussions and decision-making processes in this field.

Four chapters of this dissertation address various issues related to retirement, childbirth, and disability insurance. After an introduction in chapter 1, the second chapter examines the willingness-to-pay among self-employed individuals for retirement products offering early withdrawal options and reduced fiscal reporting requirements. The third chapter delves into the causal effects of retirement on financial outcomes. The fourth chapter focuses on understanding the phenomenon of child penalties in the Netherlands and investigates the role of household time allocation in explaining these penalties. The fifth chapter assesses whether private disability insurance facilitates the transition back to work, shedding light on its impact on workforce participation. The thesis concludes with discussing its findings and appropriate policy options in chapter 6.

1.1 Retirement savings of self-employed workers: The role of withdrawal flexibility and administrative burden

In the Netherlands, significant disparities exist in the retirement savings of self-employed workers, with about 40% of self-employed workers retaining less than 70% of their pre-retirement income after retirement (Knoef et al. (2017)). These lower savings of self-employed can be explained by the fact that they do not accumulate occupational pensions and do not accumulate enough private pension savings to compensate for this.

This paper investigates how to encourage self-employed workers to save for retirement. Specifically, this paper examines whether reducing administrative burden and offering early withdrawal options can encourage greater savings for retirement.

Using a stated choice experiment, this paper presents hypothetical retirement products featuring varying post-retirement benefits, early withdrawal options, and administrative simplifications. Through this approach, the analysis assesses the Willingness-to-Pay (WTP), expressed as a percentage of the post-retirement benefit, for reduced administrative burden and early withdrawal options. The survey, administered to approximately 800 self-employed workers and 800 employees, allows us to estimate the WTP separately for each group.

In the experiment, participants are initially presented with a series of eight questions, each of which presents a choice between two hypothetical retirement products. Then they are asked to assign choice probabilities to each option. Subsequently, in a set of eight questions, participants are shown a single hypothetical retirement product and are requested to indicate their probability of purchasing that product. Additionally, this paper collects data on individual characteristics, including but not limited to, time preference, age, life expectancy, anticipated income fluctuations, the impact of COVID[1], and risk preference.

We find demand for both reduced administrative burden and early money withdrawals. These results indicate that offering the aforementioned options may assist self-employed workers in saving more for retirement.

1.2 The effect of retirement on household finances: Causal evidence from transaction data

The literature has extensively studied how retirement affects personal finances. Findings include mixed evidence as to whether expenditure drops (Agarwal et al. (2015); Aguila et al. (2011); Banks et al. (1998a); Battistin et al. (2009); Been and Goudswaard (2020); Bernheim et al. (2001); Luengo-Prado and Sevilla (2013a); Lührmann (2010); Luengo-Prado and Sevilla (2013b)) and long-run wealth accumulation (Kieren and Weber (2022); Love et al. (2009); Olafsson and Pagel (2018); Poterba et al. (2011)) that contradict the predictions of the life cycle model. These deviations from the life-cycle model have been attributed to factors such as bequest motives (Lockwood (2018a, 2012a)), bounded rationality (Olafsson and Pagel (2018)), and home production (Been and Goudswaard (2020)).

However, existing estimates of retirement on household finances typically rely on yearly administrative datasets or surveys. These data are limited both in the accuracy of the information provided as well as in attenuating short-term dynamics. To address the aforementioned

[1]The survey was administered in 2020

issues, this paper provides causal estimates of how retirement affects net flow balances, end-of-of month balances, and the fraction of individuals with debts using monthly transaction data from ING, a large Dutch bank.

In the Netherlands, most individuals are eligible to receive both state pensions and occupational pensions. The former is provided to all Dutch citizens after reaching a specified age threshold, determined by their birth year. Conversely, occupational pensions are accrued throughout one's employment history. Occupational pensions can be initiated at any time, although payouts are lower (higher) if initiated earlier (later). Since the state pension age is determined by one's cohort, and the data includes multiple cohorts, this paper utilizes the state pension age as an instrumental variable for occupational pensions. This allows us to present causal estimates of the income and expenditure effects of retirement.

The dataset, after applying sample selection criteria, comprises approximately 12,000 individuals who have received occupational pensions at some point within the sample period, allowing us to observe their financial behavior both before and after receiving these pensions. Furthermore, the dataset includes details on cash flows, account balances, and a concise set of demographic characteristics for each individual.

We estimate Fuzzy Instrumented Regression Discontinuity (RD) and Instrumented Difference-in-Difference (DiD) models to obtain causal effects. In the first stage, the analysis uses the state retirement age and the change in the state retirement age as instruments for occupational pension, respectively. In the second stage, the analysis estimates financial outcomes based on the predicted occupational pension recipiency. This approach allows us to obtain causal estimates of the impact of occupational pensions on spending behavior.

We find modest end-of-month balance effects as a result of retirement. For low-income and low-wealth groups, however, we discover strong short-run liquidity effects, both with respect to end-of-month balances and the percentage of individuals in debt. Overall, our findings indicate that retirement alleviates liquidity constraints at the bottom of the income- and wealth distribution.

1.3 Child penalties and the role of household time allocation

The earnings gap between men and women decreased over the past decades, but persists to this day (Cortés and Pan (2020)). International literature extensively studies these differences, and finds that they are primarily the result of childbirth (Andresen and Nix (2019); Cortés and Pan (2020); De Quinto et al. (2020); Kleven, Landais and Søgaard (2019); Kuziemko et al. (2018); Lundborg et al. (2017); Meurs and Pora (2019); Rabaté and Rellstab (2021); Sieppi and Pehkonen (2019)): prior to childbirth, earnings profiles are relatively equal. After childbirth, however, a divergence appears: women reduce labor market activity on both the intensive and the extensive margin, and never recover from this activity reduction even years after childbirth. This phenomenon is known as the child penalty.

The child penalty is typically attributed to gender norms (Bedi et al. (2018); Kleven, Landais, Posch et al. (2019); Kleven (2022); Rabaté and Rellstab (2021); Rellstab (2023)) and time allocation in the household (Blau and Kahn (2017); Casarico and Lattanzio (2023)). However, only one paper thus far directly links time use in the household with the (short-run) child penalty. Chapter 4 provides the first long-run evidence of how child penalties can be explained by time use in the household.

To estimate child penalties in the Netherlands, this paper implements an event study design as laid out in Kleven, Landais and Søgaard (2019). This design involves estimating the effects on both labor market outcomes and household time use, focusing on the years before, during, and after childbirth.

Using the LISS-panel survey, from 2008 to 2021, this paper estimates how childbirth in the Netherlands affects both labor market outcomes and time use in the household for parents who have children at some point in the sample. Specifically, this paper investigates how childbirth affects participation rates, (un)conditional earnings, hours worked, and hours spent on household activity.

We find that household time allocation mirrors the decrease in labor market activity for women after the arrival of children. This mirrored effect indicates intra-household substitution as opposed to a decrease in time use for women.

1.4 Non-public disability insurance and outflow to work

In the Netherlands, firms can opt out of public disability insurance (McVicar et al. (2022)). By choosing this option, firms are exempted from paying public partial disability (WIA) premiums. However, by opting out, they take on the responsibility for reintegrating workers who become disabled and for providing individual benefits to those workers.

The process of firms themselves re-integrating workers may create additional outflow channels. Specifically, firms have the option to request re-assessments for disabled workers, which serves as a means of reintegration not available to the Dutch Employee Insurance Agency (UWV), and these re-assessments are prioritized by UWV (Lammers et al. (2018)). To delve deeper into this phenomenon, my objective is to estimate whether non-publicly insured firms exhibit a higher rate of outflow from disability insurance, particularly in terms of outflow to work.

Outflow from disability insurance impacts the financial positions of disabled workers. If cessation of disability insurance results in work resumption, it can benefit workers by increasing their income and aid in consumption smoothing. However, terminating disability insurance benefits can worsen the financial challenges faced by disabled workers if they do not resume work. Balancing reintegration efforts with the need for long-term financial stability is crucial for preserving the consumption-smoothing effects of disability insurance.

The analysis uses administrative records from UWV spanning from 2006 to 2021. These records contain single-failure disability insurance (DI) admissions and re-assessments. Chapter 5 estimates duration models of outflow rates on the basis of insurance status, and additionally estimates - using (semi-)parametric duration models - whether re-assessments are more prevalent among non-publicly insured firms. The analysis estimates DI outflow rates by outflow reason while accounting for cumulative incidence.

We do not find differences in outflow to work on the basis of employer-insurance type. However, we do discover substantially higher outflow to full DI and slightly higher outflow out of DI, contrasted by lower outflow for reasons such as retirement. Overall, our findings indicate that although non-publicly insured firms generate more outflow from DI, they do not reintegrate workers more effectively than publicly insured firms.

Chapter 2

The Demand for Retirement Products: The Role of Withdrawal Flexibility and Administrative Burden

Abstract

Many people save too little for retirement. In the Netherlands this especially concerns self-employed workers. This chapter studies – using a stated choice experiment – whether increasing withdrawal flexibility and decreasing the administrative burden can increase the demand for retirement products. We find that the self-employed are willing to give up 8% of post-retirement benefit for a lower administrative burden. In addition, they are willing to give up 14% in order to have the option to withdraw money in case of low income or for mortgage payments. Contrasting this, the willingness to pay (WTP) for flexibility and a lower administrative burden is remarkably lower for employees. Employees are willing to give up only 4% for flexible retirement products, and are not willing to pay for a lower administrative burden. Our results suggest that increasing flexibility and lowering the administra-

This chapter was co-authored by Marike Knoef and Max van Lent. We are grateful to Mark Boumans, Sjoerd Brouwer, Tinka den Arend, Ian Koetsier, Emile Soetendal, and Floske Weehuizen, this project would not be possible without them. We thank Netspar for funding the experiment. We thank Albert Rutten and Johan Bonekamp for useful comments and suggestions. Finally, we thank seminar and conference participants at: Leiden University, Netspar, New Paper Sessions 2020, The Dutch Economist Week 2020, the 6th Maastricht Behavioral Economic Policy Symposium and the European Association of Labour Economists 2021 for useful comments and suggestions.

tive burden for pension products can increase demand, especially for self-employed workers.

2.1 Introduction

In western countries a substantial fraction of workers has little retirement savings. This is particularly the case among the self-employed (OECD (2019a)). Under-savings primarily manifest through a relatively low amount of savings through annuities. In the literature this is called the annuity puzzle (Benartzi et al. (2011)). A recent literature has pointed towards the administrative burden — i.e., the effort it takes to look up information and fill out forms — and a lack of withdrawal flexibility as key explanations for low annuity take-up rates (see e.g., Galiani et al. (2020); Lusardi and Mitchell (2007)).[1]

In this paper we study to what extent more flexible pension products and a lower administrative burden can help to increase the demand for pension annuities. We use a stated choice experiment which allows us to estimate the willingness to pay (WTP) for flexibility and a reduced administrative burden using a large sample of Dutch workers. We implement flexibility through early withdrawal options from one's pension fund. The administrative burden is lowered by reducing the amount of financial information one has to provide to purchase a fiscally attractive pension annuity.

Throughout this paper we particularly focus on self-employed workers' demand for pension products. The reason for this is twofold. First, self-employed workers typically have lower retirement savings and less often retirement savings products. Zwinkels et al. (2017) show that, in the Netherlands, 43% of the self-employed are not able to replace 70% of their current income after retirement, while this is 31% for employees. This is likely to be at least partially caused by institutional differences. For example, employees are much more often covered by mandatory pension schemes. Second, both a lack of flexibility and the administrative burden may have a bigger impact on self-employed workers. The preference for flexibility may be higher among the self-employed because of their higher income volatility. Furthermore, self-employed may be more aware of the administrative burden involved with tax facilitated pension products, and the administrative burden

[1] Also the COVID-19 pandemic has emphasized the interest for retirement products with withdrawal flexibility. Several countries have introduced (temporarily) additional flexibility in retirement products. For example, in France independent self-employed workers facing financial difficulties can take up (at most) 800 dollars from their retirement accounts.

may be higher for them than for employees because of their higher income volatility. Therefore, providing products that are both more flexible and reduce the administrative burden on the worker may particularly increase product take up for the self-employed, and may help restore inequality in the pension accumulation of self-employed workers and employees.

Theoretical papers have extensively provided evidence on demand for a lower administrative burden and more withdrawal flexibility during the accumulation phase. Flexibility can increase contributions for people who prefer liquidity. Amador et al. (2006), Davidoff et al. (2005a) and Horneff et al. (2015) show that offering liquidity can be optimal and increases annuity take-up. On the other hand, flexibility (in the accumulation phase) can reduce retirement wealth because people withdraw their savings before retirement. We build upon Amromin and Smith (2003) who highlight a demand for the willingness to cover liquidity shocks, Beshears et al. (2014) who show that early money withdrawal options as well as framing increase annuity take-up rates, and Beshears et al. (2020) who show that there is demand for commitment (i.e. products with withdrawal penalties).

As a (possible) consequence of the COVID-19 pandemic there have been several (temporary) options of flexible and early withdrawals from pension funds. Bateman et al. (2023) and Wang-Ly and Newell (2022) examine an Australian early pension wealth withdrawal access option in Australia during the COVID-19 pandemic, revealing that individuals primarily withdraw retirement savings to alleviate income and liquidity constraints. Most of the early money withdrawals were performed by present-biased individuals, see Hamilton et al. (2023). Similar patterns emerge from a scheme implemented in Chile, as highlighted by Fuentes et al. (2023) showing that early money withdrawal options are primarily taken up by low-income workers. Moreover, Lorca (2021), Fuentes et al. (2023), and Madeira (2022) demonstrate the adverse impact of such options on retirement wealth, prompting delayed retirements. They also observe that individuals with low pre-withdrawal pension wealth are more inclined to opt for early withdrawals, potentially exacerbating the decline in overall savings. While the earlier mentioned papers study the impact of (temporary) products with flexibility features, they don't study the desirability from a consumer's perspective. This is the first paper that estimates the WTP for flexible retirement products and products with reduced amounts of red tape. Note that offering pension products with a lower administrative burden and options for early money withdrawal — as we propose in our experiment — are currently not legally allowed in most developed countries including the Netherlands (Beshears et al. (2015)).

The key contributions of this paper to the literature are twofold. First, we estimate the willingness to pay for a lower administrative burden and the demand for flexibility in the accumulation phase, using a stated choice experiment. As far as we know, we are the first to distinguish between four types of withdrawal options: fined withdrawals for any reason (as in Beshears et al. (2020)), free withdrawals conditional on a low income level, withdrawals specifically for education and investment purposes, and withdrawals for mortgage down payments. These options have diverse characteristics: whereas retirement savings become liquid under the first option, mortgage down payments are still rather illiquid. Withdrawals in case of a low income help to smooth consumption over time, and education and investments may increase future income. Second, we distinguish between self-employed workers and employees, as the Netherlands are characterized by large institutional differences between these two groups (see also section 2 for an extensive discussion). We study heterogeneous results for different characteristics and circumstances of people to obtain a better understanding why flexibility and a low administrative burden are more important for some people than for others.

Our paper relates to a broad literature on the retirement-savings and annuity puzzle — which entails workers not annuitizing for retirement in spite of annuitization being optimal — and the potential solutions to this puzzle. One reason that people don't annuitize more is behavioral biases, see Benartzi and Thaler (2007a) and Thaler and Benartzi (2007a) for an overview. For instance, inertia Chetty et al. (2014); Bütler and Teppa (2007), procrastination Beshears et al. (2014), present bias (Linde (2019)), and a lack of skills Brown et al. (2017); Shu et al. (2016) and Galiani et al. (2020). In addition, workers with lower financial literacy annuitize less, see Lusardi and Mitchell (2007) and Hershey et al. (2017). Finally, preferences play a role. For instance (unanticipated) health shocks may be a reason for a low annuitization rate, see e.g. Peijnenburg et al. (2017a), a preference to retire later (Parker and Rougier (2007); García et al. (2019)), and a preference for lower income during retirement (see Selin (2012) and Joulfaian (2018)). Framing (the text of) pension products have been successfully used to increase annuitization, see e.g. Agnew et al. (2008); Beshears et al. (2014); Brown, Kling et al. (2008). We contribute to this literature by studying how attributes of pension products affect the demand for these products.

Our main findings are the following. Self-employed workers have a WTP of 8% of post retirement benefit for not having to provide fiscal information in order to purchase a pension product, while employees have a WTP close to zero. The WTP for flexibility is on average much larger. The self-employed are willing to pay up to 14% of post retirement benefit. The WTP for withdrawing money in case of low income and for mortgage payments is the highest. Employees are also willing to pay for increased withdrawal flexibility, although less. Employees have a WTP of at most 4%. This difference in WTP for flexibility may be explained by a difference in income uncertainty. Our results imply that policy design matters for annuity take up. Lowering the administrative burden and allowing certain types of withdrawal flexibility can help increase the take-up of pension annuities and consequently alleviate elderly from poverty.

The rest of this paper is organized as follows. The next section describes the Dutch retirement system. Section 3 describes our empirical methodology. In section 4 we explain our stated choice experiment design. Section 5 discusses the data, followed by section 6 presenting and discussing results. Finally, section 7 concludes.

2.2 Institutional setting

This section describes how the Dutch retirement system is organized and how the system differs between employees and the self-employed. One can categorize three sources of retirement contributions in the Netherlands, the three pillars. The first pillar entails state-funded flat-rate benefits on a pay-as-you-go basis. These benefits are equal to 50% to 100% of the net minimum wage, dependent on one's living situation (Dutch Law (2020)). Residents of the Netherlands accrue 2% of their state pension every year, for 50 years until reaching the statutory retirement age, irrespective of work history. Under current plans, the age of entitlement to state pension benefits will increase to 67 years in 2024, and thereafter it will be linked to the development of life expectancy.

The second pillar entails retirement benefits funded through one's employer. 90% of Dutch employees are mandatorily enrolled in such a pension plan. Most self-employed workers are not mandatorily enrolled in the second pillar. Though, they can save on a voluntary basis for at most ten years after quitting their job at their last employer. Most self-employed workers currently do not save in the second pillar. A consequence of the second pillar being employer-provided is that the self-employed typically do not have as many second pillar arrangements. One important feature of the second pillar is that contributions cannot be withdrawn before retirement.

The third pillar entails voluntary individual pension arrangements. As opposed to second pillar savings, third pillar pension contributions can be withdrawn after paying income tax and a 20% fine over the withdrawn amount. An exception to this fine exists when individuals become disabled. In this case, up to €40,000 can be withdrawn, with only income tax having to be paid over this amount.

The Dutch pension system attempts to encourage retirement savings in the second and third pillars by offering a tax deduction: Second and third pillar retirement contributions up to a certain threshold can be deducted from one's taxable income. Additionally, there is an unofficial fourth pillar. This fourth pillar consists of private possessions such as savings, stocks, and one's home.

The attributes of our products are embedded in the current Dutch system in the following way. Our administrative burden attribute simplifies the information that needs to be provided in order to purchase the annuity and receive a tax break from their respective second- and third pillar baselines. In the second pillar, the burden is reduced by not having to report one's income over the past three years. In the third pillar, workers have to compute their tax deductibility thresholds, which requires workers to look up their income over the past year. The flexibility component adds additional exemptions from paying the fine that comes with early money withdrawals in the third pillar.

2.3 Data

This section provides an overview of the data used for the analysis and shows descriptive statistics. We created a survey and targeted individuals who worked at least 28 hours a week and were between 25 and 60 years old[2]. The hour criterion is chosen to solely measure effects for workers who participate substantially on the labor market and whose main source of income is from (self-)employment. The survey was then administered to a sample of self-employed workers and a sample of employees of about the same size. These restrictions leave us with 1,741 respondents, 822 self-employed workers and 919 employees (note that the self-employed are oversampled). The survey was conducted between May 20, 2020, and June 8, 2020.

Table 2.1 presents demographic characteristics of respondents. Respondents are on average 43 years old, 38% of the sample is female, nearly three out of four own of house, and nearly everyone works more than 32 hours a week. We see some minor differences between the self-employed workers and employees in terms of homeownership, hours worked and education level. There is a sizable difference in the fraction of female workers between the self-employed and employees. Only 26% of employees in our sample are female, while half of the self-employed workers are female. However, this difference matches fairly closely with the gender distribution of employees and self-employed workers conditional on working 28 hours a week or more, as found by Torre et al. (2019). They show that conditional on working at least 4 days a week, 48% of the SE are female. Other demographic characteristics in our sample also fairly closely match those found in Torre et al. (2019). Worth noting, however, is that 54% of our self-employed sample is highly educated whereas (unconditionally on hours worked) 47% of Dutch SE are highly educated according to Torre et al. (2019).

[2]The survey was administered by Kien Wizard. We asked Kien Wizard to administer our survey to 800 self-employed workers and 800 employees. The sample matches population averages of self-employed workers and employees fairly closely CBS (2020), though some differences arise as a result of us selecting on the basis of the full-time workers.

	Full sample		Self-employed		Employees		Diff
	Mean	SD	Mean	SD	Mean	SD	P-value
Age	43.06	9.96	44.20	9.84	42.04	9.95	0.00***
Female	0.38	0.48	0.50	0.50	0.26	0.44	0.00***
Homeowner	0.73	0.45	0.71	0.45	0.74	0.44	0.10
Works 32 or more hours a week	0.99	0.12	0.97	0.18	1.00	0.00	0.00***
Works 28 to 32 hours a week	0.01	0.12	0.03	0.18	0.00	0.00	0.00***
Low education level	0.12	0.32	0.09	0.29	0.14	0.34	0.00***
Intermediate education level	0.40	0.49	0.37	0.48	0.43	0.49	0.01**
High education level	0.49	0.50	0.54	0.50	0.44	0.50	0.00***
Observations	1741		822		919		1741

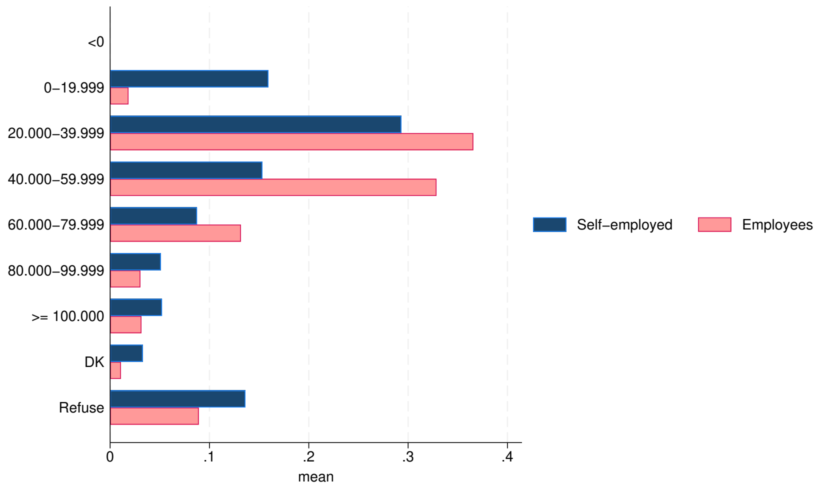
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.1: Demographic characteristics of respondents. Diff compares self-employed workers to employees.

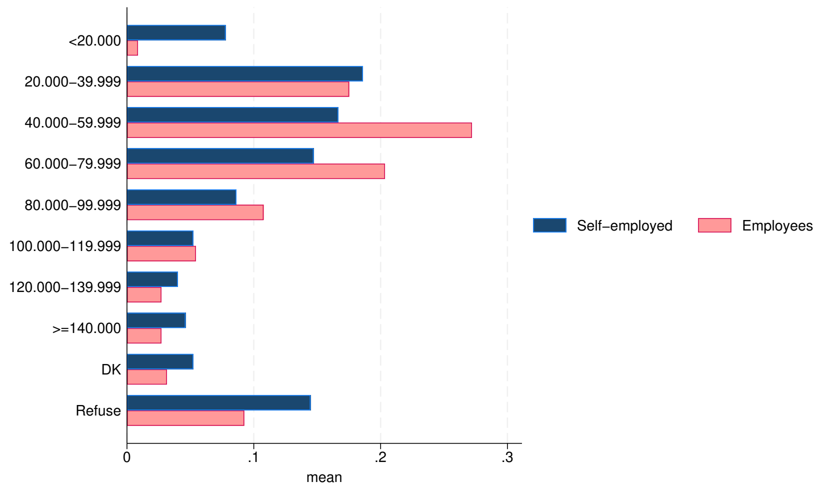
Figure 2.1 shows income and pension characteristics of our sample. Self-employed workers are over-represented in the tails of the income and liquidity distributions as compared to employees. Most self-employed respondents have personal incomes between €0 and €60.000, whereas most employees have incomes between €20,000 and €80,000. The pattern for household income is similar, though household incomes are somewhat larger than individual incomes, which indicates that most people also have a working spouse. Both groups have relatively few net liquid assets, see panel c. The self-employed also seem to not know and refuse to state their income and net liquid assets more often. This is in line with the fact that wages of the self-employed are typically more volatile and less predictable.

Table 2.2 shows anticipated income shocks of workers over the next 5 years as well as a result of the Covid-19 pandemic. As expected, self-employed workers anticipate much more income uncertainty than employees. Said income uncertainty may in turn lead this group to have a higher demand for liquidity.

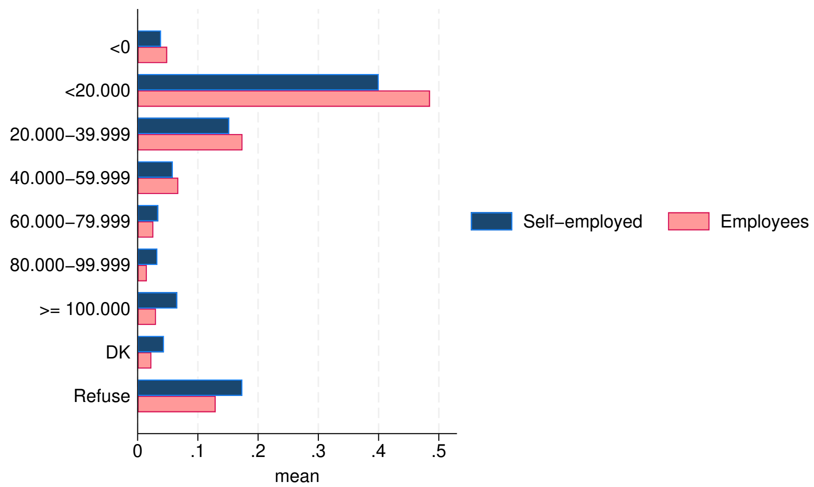
Table 2.2 also describes the preferences of workers for retirement savings. One fifth of our sample (strongly) wishes to save more for retirement than they are currently saving, this preference is similar for the self-employed workers and employees. Also roughly one in five reports to procrastinate the decision to save more for retirement.



(a) Personal income



(b) Household income



(c) Net liquid assets

Figure 2.1: The distribution of income and assets for self-employed and employees. DK denotes Don't know, refuse denotes respondent refused to answer the question.

	Full sample		Self-employed		Employees		Individual Diff	Joint Diff
	Mean	SD	Mean	SD	Mean	SD	P-value	P-value
Strongly wishes to save more for retirement	0.04	0.21	0.05	0.22	0.04	0.19	0.23	
Wishes to save more for retirement	0.15	0.36	0.12	0.33	0.18	0.39	0.00***	
Neutral with respect to saving more for retirement	0.33	0.47	0.30	0.46	0.36	0.48	0.02**	0.00***
Does not wish to save more for retirement	0.35	0.48	0.38	0.49	0.32	0.47	0.01**	
Strongly does not wish to save more for retirement	0.10	0.29	0.12	0.32	0.08	0.26	0.00***	
Does not know if wishes to save more for retirement	0.03	0.16	0.03	0.16	0.03	0.16	0.82	
Strongly disagrees with procrastinates retirement savings	0.06	0.23	0.06	0.23	0.04	0.19	0.48	
Disagrees with procrastinates retirement savings	0.13	0.33	0.12	0.33	0.14	0.35	0.66	
Neutral on procrastinates retirement savings	0.24	0.42	0.23	0.42	0.29	0.46	0.19	0.52
Agrees with procrastinates retirement savings	0.37	0.48	0.38	0.48	0.31	0.46	0.24	
Strongly agrees with procrastinates retirement savings	0.17	0.37	0.16	0.37	0.19	0.40	0.52	
Does not know if procrastinates retirement savings	0.05	0.22	0.05	0.22	0.03	0.16	0.30	
Strongly disagrees anticipated income fluctuations coming 5 years	0.16	0.37	0.04	0.20	0.28	0.45	0.00***	
Disagrees anticipated income fluctuations coming 5 years	0.32	0.47	0.21	0.41	0.42	0.49	0.00***	
Neutral anticipated income fluctuations coming 5 years	0.18	0.38	0.23	0.42	0.13	0.34	0.00***	0.00***
Agrees anticipated income fluctuations coming 5 years	0.23	0.42	0.35	0.48	0.12	0.33	0.00***	
Strongly agrees anticipated income fluctuations coming 5 years	0.09	0.29	0.14	0.35	0.05	0.21	0.00***	
No opinion on anticipated income fluctuations coming 5 years	0.01	0.11	0.02	0.14	0.01	0.07	0.00***	
Strongly disagrees income fluctuations due to covid	0.13	0.33	0.05	0.23	0.19	0.39	0.00***	
Disagrees income fluctuations due to covid	0.29	0.45	0.15	0.36	0.42	0.49	0.00***	
Neutral on income fluctuations due to covid	0.21	0.41	0.21	0.41	0.21	0.41	0.96	0.00***
Agrees income fluctuations due to covid	0.22	0.41	0.32	0.47	0.13	0.33	0.00***	
Strongly agrees income fluctuations due to covid	0.14	0.35	0.25	0.43	0.04	0.21	0.00***	
No opinion on income fluctuations due to covid	0.01	0.11	0.02	0.13	0.01	0.10	0.19	
Observations	1741		822		919		1741	1741

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.2: Descriptive statistics: preferences for retirement savings. Diff compares self-employed workers to employees.

Table 2.3 shows preferences of respondents.[3] We find that a majority of respondents are risk-averse. Risk preferences vary little between self-employed workers and employees, with the majority of respondents being risk-averse. These risk-aversion related results are somewhat surprising, as self-employed workers are typically found to be less risk-averse than employees (S. Brown et al. (2006); Masclet et al. (2009)). Respondents overall choose safer gambles than in Dave et al. (2010), which likely stems from the fact that our games have higher stakes. Present-bias and discount factors are estimated following Wang et al. (2016). Most workers are present-biased, but there is no difference in present bias and the long-term discount factor between self-employed workers and employees. Both the degree of present bias and the long-term discount factor are similar to those found in Wang et al. (2016) for a sample of Dutch students. Most respondents have a bequest motive. On average, they would spend €2200 themselves if they would receive €3000, and more than €5100 if they would receive €9000. The bequest motives of self-employed workers and employees do not differ. Self-employed workers seem to have a slightly higher

[3]The exact questions we asked respondents to measure risk preference, present-bias and bequest motives can be found in Appendix A2.3.

subjective life expectancy, but this difference is not statistically significant. Trust in pension funds and insurers is overall neutral to negative. Finally, both self-employed workers and employees consider themselves fairly financially literate. Moreover, around 80% of the sample provided the correct answer to the question regarding inflation as described in Lusardi and Mitchell (2007). However, less than half of the sample correctly answered what annual fiscal contribution room entails, with self-employed workers providing the correct answer relatively more often.

	Full sample		Self-employed		Employees		Individual diff	Joint diff
	Mean	SD	Mean	SD	Mean	SD	P-value	P-value
Risk Preference								
RRA coefficient larger than 3.46	0.41	0.49	0.42	0.49	0.40	0.49	0.31	
RRA coefficient between 1.16 and 3.46	0.19	0.39	0.17	0.38	0.20	0.40	0.16	
RRA coefficient between 0.71 and 1.16	0.17	0.37	0.16	0.37	0.17	0.37	0.70	0.28
RRA coefficient between 0.5 and 0.71	0.08	0.28	0.07	0.26	0.09	0.29	0.20	
RRA coefficient between 0 and 0.5	0.06	0.24	0.06	0.24	0.06	0.24	0.70	
RRA coefficient smaller than 0	0.09	0.29	0.10	0.30	0.08	0.27	0.12	
Time preference								
Present-bias	0.91	0.23	0.91	0.24	0.91	0.23	0.95	
Long-term discount factor	0.91	0.08	0.91	0.08	0.91	0.08	0.75	
Life expectancy								
Probability live to 70	0.72	0.23	0.72	0.24	0.72	0.22	0.85	
Probability live to 80	0.52	0.26	0.53	0.28	0.51	0.25	0.20	0.00*
Probability live to 90	0.30	0.26	0.33	0.28	0.28	0.23	0.00***	
Bequest motives								
Amount spent when 3000 euros available	2174.56	811.23	2161.05	838.52	2186.64	786.27	0.51	0.56
Amount spent when 9000 euros available	5121.00	2874.03	5126.23	2956.18	5116.32	2800.12	0.94	
Financial literacy								
Perceived financial literacy (Score out of 10)	7.55	1.45	7.52	1.52	7.57	1.38	0.47	
Correct answer to financial literacy question	0.79	0.41	0.77	0.42	0.81	0.39	0.02**	
Correct answer annual contribution question	0.46	0.50	0.52	0.50	0.40	0.49	0.00***	
Pension funds and insurers								
Strongly distrusts pension funds and insurers	0.15	0.36	0.17	0.38	0.13	0.34	0.03**	
Distrusts pension funds and insurers	0.28	0.45	0.29	0.45	0.28	0.45	0.55	
Does not trust or distrust pension funds and insurers	0.37	0.48	0.36	0.48	0.37	0.48	0.82	0.00
Trusts pension funds and insurers	0.16	0.36	0.11	0.32	0.19	0.39	0.00***	
Strongly trusts pension funds and insurers	0.03	0.16	0.04	0.19	0.02	0.13	0.01***	
No opinion on trust in pension funds and insurers	0.02	0.14	0.03	0.17	0.01	0.12	0.04**	
Observations	1741		822		919		1741	1741

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2.3: Preferences and expectations of respondents. 1-year and 10-year discount rates are winsorized at the 5th and 95th percentile. Diff compares self-employed workers to employees.

Respondents are asked to fill in the probability that they buy a given product in each vignette. Some respondents are rounding all their answers by 5 or 10 percent. This rounding behavior is shown in Appendix A2.2. Table A2.2 shows the rounding patterns in our data. 24% of answers by self-employed workers are multiples of 5, 16% are multiples of 10, and 11% are multiples of 50. These percentages are lower among employees, at 20%, 11%, and 7%, respectively.

A potential concern is that respondents wish to purchase neither of the retirement products in our first set of vignettes and that we hence falsely attribute the choice for either of the products to the willingness to purchase a product. To rule this concern out, table A2.2 shows — using the second set of vignettes — that only a small fraction indicates that they are not interested in purchasing an annuity.[4] This indicates that respondents are not averse to the hypothetical retirement products and may be interested in purchasing said products in practice.

2.4 Methodology

In this section, we describe how we estimate the WTP for more flexible retirement products with a lower administrative burden. Our estimation method closely follows Koşar et al. (2021). We assume that utility can be described with the following equation:

$$U_{ij} = B_j\alpha + F_j\beta + A_j\gamma + \xi_{ij} \tag{2.1}$$

Where U_{ij} denotes the utility of individual i for alternative j. B_j is a row vector with dummy variables describing the administrative burden of option j, F_j is a row vector with dummy variables describing the flexibility of option j, and A_j reflects the yearly annuity. α and β are vectors of preference parameters, and ξ_j is an idiosyncratic preference shock.

Individual i chooses retirement product j after observing the attributes $B_1, ..., B_J$, $F_1, ..., F_J$, $A_1, ..., A_J$ and $\xi_{i1}, ..., \xi_{iK}$. We assume that the ξ_i's are distributed i.i.d. Type I extreme value conditional on the attributes.

Respondent i is asked to report a probability of hypothetically choosing product j over product k. This can be written as:

$$Pr(U_{ij} > U_{ik}) = \frac{\exp(B_j\alpha + F_j\beta + A_j\gamma)}{\exp(B_j\alpha + F_j\beta + A_j\gamma) + \exp(B_k\alpha + F_k\beta + A_k\gamma)} \tag{2.2}$$

[4]Removing these responses from the data yields similar estimates.

From (2.2) we derive the following log odds ratio for product j as compared to product k:

$$\ln(\frac{p_{ij}}{p_{ik}}) = (B_j - B_k)\alpha + (F_j - F_k)\beta + (A_j - A_k)\gamma \ \forall\ j \neq k \quad (2.3)$$

As noted in the literature, survey respondents often round their subjective probabilities to multiples of 5% and 10% (Kleinjans and Soest (2014) and Manski (2004)). To take this into account we follow the literature and introduce measurement error into the model and estimate preferences using the least absolute deviations (LAD) estimator (e.g., Koşar et al. (2021)). Formally, we introduce this rounding behavior by assuming that our observed probabilities are measured with error such that:

$$\ln(\frac{\tilde{p_{ij}}}{\tilde{p_{ik}}}) = (B_j - B_k)\alpha + (F_j - F_k)\beta + (A_j - A_k)\gamma + \eta_{ijk} \ \forall\ j \neq k \quad (2.4)$$

where η_{ijk} captures (the difference in) measurement errors. Assuming that the distribution of η_{ijk} (conditional on B, F, and A) has a median of 0, this leads to the following median regression:

$$M[\ln(\frac{\tilde{p_{ij}}}{\tilde{p_{ik}}})|B,F,A] = (B_j - B_k)\alpha + (F_j - F_k)\beta + (A_j - A_k)\gamma \ \forall\ j \neq k \quad (2.5)$$

Median regression is more robust to outliers, and this is helpful for reported probabilities close to 0, which are often rounded to 0%.[5]

Quantile regression performs poorly when there are many (rounding-induced) tied values (Wilcox and Clark (2013)). We follow Machado and Silva (2005), by jittering our data to break the aforementioned ties. We adjust the choice probabilities of respondents who round all of their choice probabilities to multiples of 5% by a uniform distribution between −2.5% and 2.5% and the probabilities of respondents who round all of their probabilities to multiples of 10% (but not 5%)

[5] In our data, 12% of the answers have a corner solution of 0% and 9% of the answers have a corner solution of 100%. For estimation of (2.5), we convert choice probabilities of 0 to 0.001 and choice probabilities of 1 to 0.999, as log odds ratios for these values are undefined without this conversion.

by a uniform distribution between -5% and 5%. Note that adding this uniform noise does not violate the key identifying assumption of our model. Furthermore, different degrees of uniform noise as well as OLS yield roughly the same results as those presented in the results section.

2.5 Vignette design

This section describes the trade-offs respondents have to make in our stated choice experiment. Using sixteen vignettes, respondents are offered hypothetical retirement products that replace any pending pension contributions. To this end, we show two sets of eight vignettes.

In the first set of vignettes respondents choose between two hypothetical annuities. We ask respondents to assign probabilities of buying each product (replacing one's existing pension contributions) that sum up to 100% in each vignette. We explicitly make clear that these products replace any current retirement products that the respondent may have.

In the second set of vignettes we offer one product and have respondents assign a probability of buying said annuity, again making it explicitly clear that these products replace any current retirement products that the respondent may have. This approach allows us to identify whether demand for our hypothetical retirement products is present.

We prefer this two-step procedure over a design in which one has three options per vignette, where the third option is buying no product. That is because our procedure allows us to estimate the preference over two products, even for respondents who prefer not to buy any product.

Our products vary on three attribute levels. The administrative effort that is needed to purchase a tax facilitated pension annuity, i.e., the administrative burden, the flexibility to withdraw (part of) the funds early, and the price of the pension product which is expressed in the form of a yearly retirement annuity. The vignettes are constructed such that products with a lesser administrative burden and/or more early withdrawal options entail a lower annuity. An example of a vignette can be found in Appendix A2.1.

2.5.1 Attribute levels

2.5.2 Administrative Burden

We base the attribute levels on the existing retirement system. Our administrative burden attribute entails the administrative duties that the purchaser has to fulfill in order to purchase the product with tax breaks. For our baseline attribute levels, individuals do not have to provide any fiscal information to buy a product. For our second alternative one has to provide their income history over the past three years to purchase the product in question. This attribute is based on the second pillar of the Dutch retirement system[6]. For our last alternative, which is based on the current Dutch third pillar retirement system, individuals have to compute their annual contribution limit; the maximum amount of pension contributions one can deduct from their taxable income (Lusardi and Mitchell (2007)).

2.5.3 Flexibility

For flexibility, we use the status quo of not being able to withdraw savings as the base level, which is based on the current (lack of) flexibility in the second pillar. For the other four attribute levels, we introduce situations in which individuals can withdraw part of their pension contributions. The first alternative allows individuals to withdraw as many retirement contributions as they wish, albeit with a 20% early withdrawal penalty[7]. We additionally introduce three alternatives in which respondents are allowed to withdraw their contributions without any penalty in specific situations. These specific situations are chosen to introduce a commitment mechanism (Beshears et al. (2020)). The alternatives and their conditions are as follows:

- The second alternative allows individuals to supplement their income up to the minimum wage when their income falls below the minimum wage over a three-month period by withdrawing

[6] Note that these attribute levels measure whether making it less difficult to purchase a product increases product demand. We do not alter the fiscal stimulus that is behind the current system.

[7] This alternative is the third pillar status quo.

pension savings. We add this attribute level because the self-employed typically have more variable income than employees in addition to having a lower degree of social insurance to soften the effects of income shocks. This option helps to smooth consumption over the life cycle.

- Contrasting this, the third alternative instead allows individuals to withdraw €15,000 of their retirement savings every 5 years for investments in education and training. Note that employees are often compensated for education and training whereas the self-employed are not.[8]
- Finally, the fourth alternative allows individuals to withdraw up to €15,000 of their retirement savings every 5 years to pay off their mortgage. We introduce this attribute as the self-employed tend to save for retirement through the fourth pillar (Zwinkels et al. (2017)). Over the last decades, Dutch households have seen a strong growth in both their pension savings and their mortgage debts. It has often been argued that these long balance sheets have an amplifying effect on the cyclicality of the Dutch economy (Parlevliet et al. (2015)). That is because the longer balance sheets have made households more vulnerable to fluctuations in interest rates and asset prices. Furthermore, the growth of the mortgage portfolio has increased the financial risks for banks. When individuals are allowed to withdraw part of their retirement wealth to pay off their mortgage, this would shorten individual's balance sheets and reduce vulnerability. On the other hand wealth becomes somewhat more liquid, as people can sell their house.

2.5.4 Price

For the price we first compute an annuity based on a one-time retirement contribution of €1000. This annuity is based on investments in a portfolio of 50% in stocks and 50% government bonds, said portfolio creating an annual rate of return of 3.5% (Dijsselbloem et al. (2019)). To construct the annuity, the total value of the investments

[8]Note that all workers, including the self-employed, will have the option to receive up to €1000 from the government for educative ends as of March 2022 (Dutch Central Government (2021)).

at retirement are then divided by the discounted life expectation post retirement, discounted at 1% per year. This annuity then has continuous deviations ranging from -7.5% to 7.5% of the baseline annuity. The consequent annuity closely resembles annuities presently offered by Dutch private pension providers. The vignettes show respondents the yearly pension benefit they receive upon turning 67 in exchange for a one-time €1,000 contribution now.

2.5.5 Design Characteristics

We use Ngene[9] to translate the attribute levels into vignettes. We use a Bayesian Efficient design to estimate the WTP for flexibility and a lower administrative burden with as few observations as necessary. To this end, we set positive Bayesian priors on the reduced administrative burden, withdrawal options, and the post-retirement benefit.

The experiment's design contains 3 blocks with 8 vignettes each. Our sample consisting of 1,741 workers — 822 self-employed workers and 919 employees — are separately randomized into blocks. Subsequently, respondents are shown the 8 vignettes within their block in a randomized order. In addition, the order of the attribute levels shown is randomized per respondent.

Our stated choice experiment first shows the aforementioned 8 vignettes to respondents, asking them to assign probabilities to two hypothetical retirement products that must add up to 100%. The price is annuitized to an annual retirement benefit. This annuitization distributes the total discounted value of pension contributions over post-retirement life, conditional on survival probability. This is to say that the expected benefit payout equals the total retirement buildup when working. For post-retirement life, we discount using a 1% discount rate per year.

We also use eight randomly drawn (out of each set of products) hypothetical products from our vignettes. Respondents then assign a probability to buying these products as opposed to not buying a retirement product at all. This allows us to test whether respondents who stated to prefer a product would also actually consider buying that product.

[9] *ChoiceMetrics (2012) Ngene 1.1.1 User Manual Reference Guide, Australia* (2019)

Prior to showing the vignettes, we ask questions on background characteristics, financial literacy and whether respondents are presently building up retirement funds. After the vignettes, we ask questions regarding respondents' preferences. Respondents are asked how many hours a week they work, how long they have been self-employed, in which sector they work, individual and household income, their net liquid assets and whether they buy or rent their house.

We elicit risk and time preferences in addition to subjective life expectancy to tie into our flexibility attribute. We ask respondents for their choice in Dave et al. (2010)'s Eckel-Grossman gamble, albeit with the payouts multiplied by 10[10]. Subsequently, respondents are asked to choose which payout in 1 or 10 years they want, and makes them indifferent between said payout and receiving €1000 in the present. For time preference, we follow Wang et al. (2016) by giving respondents a hypothetical choice between €1.000 now and €X in 1 and 10 years respectively, asking how large X should be such that respondents are indifferent between these two choices in both cases. From this, we compute both a long-term discount rate and present bias[11]. We ask for bequest motives by letting respondents allocate 3000 and 9000 euros respectively between themselves and their inheritance. Finally, we take the financial literacy questions from Lusardi and Mitchell (2007).[12]

2.6 Results

2.6.1 Main results

This section presents estimates of our LAD model. Table 2.4 shows that the demand for retirement products increases when the administrative burden is lower. The size of the effect is similar for both types of administrative burden. Notably, self-employed workers drive the entire effect. The self-employed have a demand that is 5% higher

[10]The Eckel-Grossman model assumes constant relative risk aversion.

[11]We compute long-term discount rates by showing respondents two hypothetical scenarios in which they receive €1000 now and €X in 1 and 10 years, respectively. with the resulting question of what value of X would make respondents indifferent between the two. From X, we compute both the 1-year and 10-year discount rates. We subsequently compute present bias by dividing the 10-year discount rate (scaled to its 1-year equivalent) by our observed 1-year discount rates. If the resulting number is smaller than 1, this indicates present bias.

[12]Note that we have translated all the questions and administered the entire survey in Dutch.

when not having to calculate their annual contribution limit and not having to provide their income history. The employees on the other hand do not have a demand for a lower administrative burden. This can be explained by the higher complexity of the calculations needed for the self-employed workers. In other words, the current administrative burden is larger for the self-employed. Taking this burden away increases the demand for retirement products.

We see a strong demand for flexibility. Being able to withdraw funds in case of a below minimum wage income and in to pay off a mortgage increases demand strongly. This holds for both the group of self-employed workers and employees. Self-employed workers also reveal a sizeable demand for the opportunity to withdraw funds for investment in e.g., schooling. Workers do not have such a demand. A possible explanation for this is that most training and education taken up by workers is paid for by their employer. Neither self-employed workers nor employees show any demand for the option to withdraw income with a fiscal penalty. Finally, as expected, demand increases when benefits are higher conditional on the administrative burden and flexibility attribute levels.

Our results contrast Thaler and Benartzi (2007a): Only self-employed workers, for whom it is typically much more difficult to compute their fiscal information, have a WTP for administrative burden. In addition to being in line with earlier literature on the demand for early money withdrawal options as in Amromin and Smith (2003) and Beshears et al. (2014), our results highlight that workers with uncertain fiscal positions are willing to give up a substantial amount of their retirement benefits for early money withdrawal options. Furthermore, the aversion towards withdrawing with a fiscal penalty provides further evidence for a desire to commitment, as found by Beshears et al. (2020). Withdrawal penalties make respondents less likely to buy retirement products: Respondents prefer products that do not entail fiscal penalties but are tied to certain conditions for early money withdrawal instead.

Although the jittering procedure applied to the data should not affect estimates in expectation, a concern nonetheless remains regarding altering the data. To test whether our results are robust to the jittering applied to the choice probabilities, we repeat our LAD estimates with different amounts of noise applied to the choice probabilities. Appendix A2.5 shows that halving or doubling the amount of noise we jitter the data with does not change the sign or the rough order of magnitude of our estimates.

	(1)	(2)	(3)
VARIABLES	Full sample	Self-employed	Employees
Benefit deviation %	0.0251***	0.00671***	0.0504***
	(0.00250)	(0.00176)	(0.00454)
Compute annual contribution limit	-0.0612***	-0.0545***	-0.00768
	(0.0121)	(0.0159)	(0.0265)
Provide income history	-0.0638***	-0.0511***	-0.0309
	(0.0144)	(0.0164)	(0.0293)
Withdraw with penalty	-0.0146	-0.000695	-0.180***
	(0.0174)	(0.0224)	(0.0434)
Withdraw low income	0.168***	0.0943***	0.174***
	(0.0217)	(0.0227)	(0.0368)
Withdraw for investments	0.0993***	0.0793***	-0.0866**
	(0.0176)	(0.0193)	(0.0344)
Withdraw for mortgage	0.228***	0.0947***	0.236***
	(0.0236)	(0.0243)	(0.0463)
Constant	0.00919	0.00531	0.0564***
	(0.00851)	(0.0103)	(0.0200)
Observations	27,856	13,152	14,704
R-squared	0.019	0.015	0.039

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.4: LAD estimates. Standard errors are clustered at the individual level and in parentheses. Reference categories are a benefit deviation of 0, not having to provide any financial information, and not being able to withdraw contributions.

Table 2.5 shows the WTPs for product attributes as compared to a baseline of not having to provide fiscal information (the smallest administrative burden) and not being able to withdraw contributions respectively. We find an overall WTP of -2.5% of the post-retirement annuity for having to compute one's annual contribution limit and having to provide one's income history in the full sample. This means that respondents are willing to give up 2.5% of their post-retirement benefit in order to avoid having to compute one's own annual contribution limit or to provide one's income history. Dividing estimates based on whether respondents are self-employed shows that the entire effect is driven by the self-employed. self-employed workers have a WTP of 8%, while the WTP for employees is a quite precisely estimated 0%.

A likely explanation for this finding is that it is more difficult for self-employed workers to find out their past income than for employees. Payrolls are often stored by the employer for employees whereas self-employed workers typically have more uncertain incomes from multiple sources.

We find positive WTPs for flexibility, with the exception of the option to withdraw money with a fiscal penalty, for which the WTP is not significantly different from zero. For employees WTP estimates for early money withdrawal options with a fiscal penalty are even negative. This discovery reveals that some employees like commitment more than flexibility in the form of options to withdraw money with a penalty. This result is in line with Beshears et al. (2020), who find — using an online experiment — that some people prefer saving accounts with high withdrawal penalties over accounts with lower withdrawal penalties. This indicates that part of their respondents are partially or fully sophisticated present biased agents.

Among self-employed workers, a WTP of approximately 14% of the annuity is found for the option to withdraw when income is low and for mortgage payments. Likewise, self-employed workers are willing to give up 12% of their retirement benefits for the option to withdraw for investments. Among employees, WTPs of 3.5% and 4.5% of one's post-retirement annuity is found for the option to withdraw money when income is low and to withdraw money for mortgage payments, respectively. As such, both self-employed workers and employees have demand for more flexible retirement products, but the effect is much more pronounced for self-employed workers.

These results with respect to flexibility may be driven by self-employed workers facing larger income shocks than employees. As such, the option to supplement income or reduce one's mortgage is likely more valuable for self-employed respondents. This explanation is further compounded by the self-employed workers in our sample generally being risk-averse. For investments, self-employed workers being responsible for their own training may explain why self-employed workers have a positive WTP for investment-related withdrawals whereas employees do not.

	(1) Full sample	(2) Self-employed	(3) Employees
Compute annual contribution limit	-2.443***	-8.121**	-0.152
	(0.545)	(2.972)	(0.526)
Provide income history	-2.545***	-7.625**	-0.612
	(0.576)	(2.785)	(0.580)
Withdraw with penalty	-0.583	-0.104	-3.579***
	(0.711)	(3.348)	(0.904)
Withdraw low income	6.721***	14.06***	3.456***
	(0.689)	(4.236)	(0.711)
Withdraw for investments	3.962***	11.82**	-1.717*
	(0.619)	(3.747)	(0.707)
Withdraw for mortgage	9.101***	14.12***	4.681***
	(0.788)	(4.050)	(0.829)
Observations	27856	13152	14704

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.5: WTP estimates measured as a percentage of the post-retirement annuity. Standard errors are clustered at the individual level and in parentheses. WTPs are relative to a pension annuity which does not require any fiscal information to purchase and has no early money withdrawal options.

2.6.2 Heterogeneity

In order to better understand what drives the differences in demand for retirement products — in particular, between the self-employed and workers — we now estimate heterogeneous effects with respect to demographic characteristics, financial position, and preferences.

Table 2.6 shows the WTPs separated by demographic characteristics. Younger and older workers exhibit similar WTPs for all product attributes. Comparing men and women shows that women have much larger WTPs for reducing the administrative burden, withdrawing when income is low and withdrawing for mortgage payments. These effects may be driven by our self-employed respondents having a larger share of women.

Finally, renters have much larger WTPs for not having to provide fiscal information as well as low-income withdrawals and mortgage payment withdrawals than homeowners, though WTPs for renters are imprecisely estimated. Renters in our sample generally have less income, are less financially literate, and have fewer net liquid assets. With the increasing housing prices it has become difficult for renters to buy a house. Early withdrawal options may help renters purchase a house.

Table 2.7 shows the WTPs separated by respondents' financial position. As expected, low-income respondents have a much higher WTP for the option to withdraw money when income is low than high-income respondents. Low-income respondents also have a stronger distaste for having to provide fiscal information and a more pronounced taste for investment-related withdrawals. A similar pattern with respect to the flexibility attributes holds when comparing low and-high liquidity workers. A potential explanation for these findings is that low income and-liquidity workers are more affected by financial shocks. Workers who saved for retirement in 2019 have higher WTPs for all attributes except withdrawing with a fiscal penalty. Workers who want to save more for retirement have more pronounced WTPs than workers who do not. Surprisingly, WTPs for flexibility among workers who anticipate income fluctuations do not significantly differ from those for workers who do not anticipate income fluctuations. Moreover, respondents who are uncertain about their income as a result of Covid-19 for early money withdrawal options have larger WTPs than those who are not.

Table 2.8 shows the WTPs separated by preferences of respondents. Risk-averse workers have a higher WTP to reduce investment-related withdrawals than workers with low risk aversion, whereas other WTPs are similar. Present-biased respondents as well as respondents with a high discount rate have stronger distastes for having to provide fiscal information and exhibit higher WTPs for low-income, investment-related and mortgage-related withdrawals. Respondents with a self-assessed probability to live to 80 or older are more interested in investment-related withdrawals, but otherwise do not differ substantially from those with a low perceived probability of living to 80 or older. Workers who distrust pension funds have a higher WTP for investment-related withdrawals. Finally, estimates on the basis of annual contribution room are too imprecisely estimated to conclude any differences between the groups.

	(1) Younger	(2) Older	(3) Male	(4) Female	(5) Renter	(6) Home owner
Compute annual contribution limit	-2.311**	-2.481	-1.155*	-8.498	-12.80*	-2.954***
	(0.814)	(1.825)	(0.532)	(4.605)	(5.102)	(0.713)
Provide income history	-1.986**	-2.987	-1.618***	-7.310	-7.883*	-1.919**
	(0.723)	(1.726)	(0.487)	(4.067)	(3.682)	(0.602)
Withdraw with penalty	-1.891	-2.675	-1.703*	-6.851	-0.616	-1.705*
	(1.026)	(2.631)	(0.757)	(5.661)	(4.533)	(0.836)
Withdraw low income	5.170***	7.014**	3.622***	14.66*	19.75**	5.354***
	(0.943)	(2.484)	(0.678)	(6.451)	(6.286)	(0.721)
Withdraw for investments	3.856***	2.476	1.151	9.782	12.61*	1.897**
	(0.950)	(2.087)	(0.777)	(5.430)	(5.156)	(0.723)
Withdraw for mortgage	8.229***	7.779**	5.525***	14.31*	15.72**	8.803***
	(1.146)	(2.584)	(0.828)	(5.999)	(5.347)	(0.887)
Observations	11888	15968	17392	10464	7296	20272

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6: WTP estimates measured as a percentage of the post-retirement annuity separated by demographic characteristics. Standard errors are clustered at the individual level and in parentheses. Younger and older defined as age between 25 and 40 and age between 41 and 60 respectively. WTPs are relative to a pension annuity which does not require any fiscal information to purchase and has no early money withdrawal options.

	(1) Lowincome	(2) Highincome	(3) Low liquidity	(4) High liquidity	(5) Nosave	(6) Save	(7) Nosavemore	(8) Savemore	(9) Nocovid	(10) Covid	(11) Incomefluc	(12) Noincomefluc
Compute annual contribution limit	-3.050*	-1.853**	-2.295**	-1.290	-0.991	-10.73*	-2.887*	-3.098***	-1.806**	-2.879	-5.179*	-4.255
	(1.260)	(0.713)	(0.859)	(0.665)	(0.554)	(4.687)	(1.142)	(0.750)	(0.669)	(2.002)	(2.244)	(2.779)
Provide income history	-4.580***	-0.617	-1.513	-1.745**	-1.045*	-7.943*	-0.997	-2.867***	-1.269	-3.588	-6.198**	-6.281*
	(1.332)	(0.617)	(0.819)	(0.665)	(0.515)	(3.904)	(1.267)	(0.651)	(0.715)	(1.990)	(2.090)	(3.040)
Withdraw with penalty	-1.269	-2.676**	-1.451	-1.803	-2.336**	-5.259	-4.825*	-0.901	-3.868***	-4.229	0.509	-0.0802
	(1.827)	(0.851)	(1.047)	(0.978)	(0.762)	(5.075)	(2.096)	(0.885)	(1.050)	(2.999)	(2.814)	(3.880)
Withdraw low income	8.350***	3.494***	7.638***	3.425***	3.622***	16.70**	3.389**	6.956***	3.438***	7.896**	11.31***	11.44**
	(1.815)	(0.787)	(1.087)	(0.835)	(0.727)	(6.335)	(1.244)	(0.818)	(0.801)	(2.837)	(3.158)	(4.257)
Withdraw for investments	4.569**	1.174	4.277***	1.506	-0.0555	11.77*	-1.691	5.081***	-1.179	6.116*	11.09***	9.723*
	(1.588)	(0.718)	(0.961)	(0.777)	(0.774)	(5.140)	(1.467)	(0.888)	(0.811)	(2.537)	(3.156)	(3.999)
Withdraw for mortgage	8.396***	6.905***	9.564***	6.209***	5.361***	16.10**	6.317***	9.564***	6.571***	9.101**	11.48***	11.09**
	(1.840)	(1.005)	(1.219)	(1.062)	(0.862)	(5.882)	(1.461)	(0.974)	(0.923)	(2.917)	(3.226)	(3.987)
Observations	12368	10928	13632	10032	17792	10064	5504	21632	11616	15872	9360	14320

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.7: WTP estimates measured as a percentage of the post-retirement annuity separated by income and pension characteristics. Standard errors are clustered at the individual level and in parentheses. Low (High) income is defined as household income being less than (equal to or more than €60,000. Low (High) liquidity defined as less than (equal to or more than) €20,000. (No)Save defined as whether someone saved (did not save) for retirement in 2019. (No)savemore defined as answering neutrally or positively (negatively) to whether respondent wants to save more for retirement. (Dis)Trustspensionfunds defined as answering neutrally or positively (negatively) to question whether one trusts pension funds. (No)Covid defined as answering neutrally or positively (negatively) whether the Covid-19 pandemic makes the respondent's income uncertain. (No)Incomefluc defined as answering neutrally or positively (negatively) whether the respondent anticipates income fluctuations over the next 5 years. WTPs are relative to a pension annuity which does not require any fiscal information to purchase and has no early money withdrawal options.

	(1) Lowrisk	(2) Highrisk	(3) Timeconsistent	(4) Presentbiased	(5) Lowdiscount	(6) Highdiscount	(7) Lowprob80	(8) Highprob80	(9) Trust	(10) Distrust	(11) AnnualContribution	(12) NoAnnualContribution
Compute annual contribution limit	-2.500***	-2.054	-1.650**	-2.982*	-1.225*	-4.384**	-1.572	-2.936***	-0.981	-3.203***	-0.651	-4.175
	(0.711)	(1.926)	(0.613)	(1.185)	(0.568)	(1.370)	(2.116)	(0.731)	(1.721)	(0.761)	(0.553)	(3.454)
Provide income history	-1.649*	-3.912*	-1.358*	-4.721***	-1.067	-4.350***	-2.681	-1.258	-3.421	-1.960**	-1.353	-3.539
	(0.665)	(1.852)	(0.640)	(1.193)	(0.650)	(1.267)	(1.904)	(0.754)	(1.812)	(0.654)	(0.700)	(3.007)
Withdraw with penalty	-2.312**	1.188	-1.705	-1.510	-1.946*	-3.626*	-3.321	-2.546*	-19.69***	0.703	-1.902*	-3.626
	(0.897)	(2.713)	(0.903)	(1.560)	(0.916)	(1.731)	(3.007)	(1.087)	(5.901)	(0.750)	(0.905)	(4.485)
Withdraw low income	6.153***	7.217**	5.354***	10.01***	5.348***	7.684***	7.312*	5.510***	8.828***	5.145***	5.970***	5.872
	(0.774)	(2.529)	(0.801)	(1.637)	(0.824)	(1.510)	(3.024)	(0.840)	(2.625)	(0.723)	(0.793)	(4.358)
Withdraw for investments	3.935***	3.338	1.703*	5.756***	1.220	7.774***	2.134	4.277***	1.686	4.277***	1.677*	5.622
	(0.744)	(2.140)	(0.789)	(1.347)	(0.767)	(1.569)	(2.457)	(0.804)	(2.019)	(0.830)	(0.717)	(3.902)
Withdraw for mortgage	8.832***	8.865***	6.892***	11.44***	6.458***	11.71***	8.048**	8.923***	8.286**	9.092***	6.687***	10.37*
	(0.903)	(2.573)	(1.020)	(1.696)	(0.986)	(1.745)	(3.091)	(0.989)	(2.529)	(1.021)	(0.901)	(4.826)
Observations	21280	6576	18560	9296	18112	9744	14736	13120	12048	15296	12832	15024

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.8: WTP estimates measured as a percentage of the post-retirement annuity separated by personal preferences. Standard errors are clustered at the individual level and in parentheses. Low risk defined as a relative risk aversion coefficient between 1.16 and 3.46 ($-\infty$ and 1.16). Presentbiased (Timeconsistent) defined as a hyperbolic discounting parameter of less than (more than) 0.9. Lowdiscount (Highdiscount) defined as a discount factor of more than (less than) 0.9. Lowprob80 (Highprob80) defined as 50% or less (more than 50%) perceived probability of living to 80. Trust in pension funds measured for in both pension funds and private insurers. AnnualContribution defined as answering correctly when asked what the annual contribution limit is, NoAnnualContribution defined as answering incorrectly when asked what the annual contribution limit is. WTPs are relative to a pension annuity which does not require any fiscal information to purchase and has no early money withdrawal options.

In order to investigate whether the role of demographic characteristics, preferences, and financial situation in the preferences for flexibility and a lower administrative burden differs for the self-employed as compared to employees, we do the heterogeneity analysis also for the group of self-employed workers separately. Tables A2.3, A2.4, A2.7 show that roughly similar heterogeneity patterns hold among the subsample of self-employed workers, albeit with higher standard errors: Present bias and high discount rates remain major factors in the demand for a lower administrative burden, whereas income fluctuations and liquidity remain major factors in the demand for early withdrawal options.

All in all, our results suggest certain groups have substantial WTPs for having to reduce the administrative burden. WTPs are more pronounced for self-employed workers but negligible for employees. These results may be driven by self-employed workers not having an employer-based income administration and several workplaces, as such making it more difficult for self-employed workers to provide fiscal information. For the option to withdraw retirement savings early, we find sizable effects for both self-employed workers, though the WTP is more pronounced for self-employed workers. Options to withdraw when income is low and to withdraw for mortgages are especially associated with large WTPs.

2.7 Conclusion

In this paper we study whether increasing flexibility in the accumulation phase and lowering the administrative burden can help increase the demand for pension annuities. Using a stated choice experiment, we compute the WTP for early withdrawal flexibility options and a lower administrative burden when purchasing retirement products. We focus on self-employed workers and compare their demand with a representative group of employees. To account for individual uncertainty in individuals' choices, we follow (Manski (2004)) in eliciting choice probabilities as opposed to purely discrete choice. To this end, we estimate the median WTP of respondents while accounting for tied values that may arise as a result of rounding. Furthermore, we offer single retirement products as opposed to choices between two retirement products in half of our vignettes, as to take into account whether respondents are willing to buy retirement products in the first place.

We find that there is significant demand to lower the administrative burden for self-employed workers. Self-employed workers demand an 8% higher post-retirement benefit in exchange for having to provide fiscal information, be it having to compute one's tax-deductible retirement contribution or one's three-year income history whereas employees do not exhibit effects fairly close to zero with respect to having to provide fiscal information. To the contrary, employees are not willing to give up a higher post-retirement benefit in exchange for a lower administrative burden.

The WTP for flexibility attributes is more striking. Both the option to withdraw money contributions for one's mortgage and withdraw when income is low show significant and precisely estimated WTPs. For the option to withdraw money when income is low, WTP estimates range from 3% for employees to 14% of the post-retirement annuity for self-employed workers. For mortgage payments, these WTPs range from 5% to 14% of one's post-retirement annuity. For investments, a WTP of 12% of the post-retirement annuity is found for self-employed workers with small positive WTPs. A negative WTP for withdrawing with a penalty is found among employees, indicating that these workers wish for withdrawing to be tied to a condition rather than being free to do so.

There is a substantial heterogeneity in WTPs among other groups. Workers who distrust pension funds, as well as workers who are present biased and/or have high discount rates have high WTPs for a lower administrative burden. Respondents with few savings, younger respondents and homeowners in particular have a strong demand for liquidity. The WTP to withdraw when income is low is strongly heterogeneous, with workers who have low incomes valuing this option most. Finally, present-biased respondents and respondents with high discount rates have much higher WTPs for early money withdrawal options than those who are not present-biased and have a low discount rate. One concern is that early money withdrawal options facilitate suboptimal choices especially for the former group. This concern is exacerbated by empirical evidence (e.g., Hamilton et al. (2023)) highlighting similar patterns.

In summary, this paper's results provide grounds to both lower the administrative burden required for saving for retirement and offer early money withdrawal options in exchange for a lower annuity. Both employees and self-employed workers stand to benefit from products that offer these characteristics, but effects are particularly pronounced for self-employed workers. Furthermore, our heterogeneity analysis can be used to inform policymakers how to increase retirement savings through annuities, especially for groups that need it the most. Specifically, since the self-employed and the lower income workers have a high WTP to reduce the administrative burden, one policy recommendation could be to abolish the need to provide financial information needed to purchase annuities for up to a certain amount per year. It is worth noting, however, that retirement savings also present tax deduction opportunities. Any reductions in red tape should be carefully designed to be compatible with these opportunities.

A2 Appendices

A2.1 Example of a vignette

Product	**A**	**B**
For every 1000-euro gross contribution you will receive this benefit from age 67 until you pass away:	€161 before taxes a year	€177 before taxes a year
To contribute money to this product:	You have to provide your taxable income over the past three years.	You do not need to provide any fiscal information.
Flexibility: some products allow for early money withdrawal.	You may withdraw up to 15.000 euros every 5 years for mortgage payments.	If your gross income over the past three months equals less than 5.000 euros, then you may supplement your gross income up to 5.000 euros by withdrawing pension contributions.

Figure A2.1: Example of a vignette

A2.2 Rounding and zero probabilities

	Self-employed Mean	Employees Mean
All probabilities multiples of 5%	0.24	0.20
All probabilities multiples of 10%	0.16	0.11
All probabilities multiples of 50%	0.11	0.07
Observations	13152	14704

Table A2.1: Rounding behavior of respondents

	Self-employed Mean	Employees Mean
Probability of zero to buy product A	0.13	0.09
Observations	6576	7352

Table A2.2: Probabilities of zero in second set of vignettes (before rounding adjustments)

A2.3 Risk preference, present-bias and bequest motive questions

Financial Literacy

On a scale from 1 to 10, how financially literate do you believe yourself to be?

- (Input integer ranging from 1 to 10)

Risk preference

Suppose we toss up a coin and you receive money depending on whether the coin lands on heads or tails.

	Game 1	Game 2	Game 3	Game 4	Game 5	Game 6
Payout if heads	€280	€240	€200	€160	€120	€20
Payout if tails	€280	€360	€440	€520	€600	€700

Welk game would you choose?

o Game 1

Game 1
€280
€280

o Game 2

Game 2
€240
€360

o Game 3

Game 3
€200
€440

o Game 4

Game 4
€160
€520

o Game 5

Game 5
€120
€600

o Game 6

Game 6
€20
€700

Time preference

Enter the amount for which option A and option B are equally appealing. Assume prices will not change from today's prices (no inflation)

- You receive €1,000 now
- You receive €[input] in 1 year

Enter the amount for which option A and option B are equally appealing. Assume prices will not change from today's prices (no inflation)

- You receive €1,000 now
- You receive €[input] in 10 years

Bequest motives

You will never face the following choices in real life. We still believe it interesting to know what you would do.

Suppose you're 80 years old. You are healthy and do not have any healthcare costs. You know you will suddenly die in one year.

Suppose you have a net income of €3,000 per month in your final year of life. Assume you have no other income sources or assets.

How much of this €3,000 would you spend yourself, and how much would you leave for inheritance every month?

- Spend: €[input] per month
- Leave for inheritance: €[input] per month

You will never face the following choices in real life. We still believe it interesting to know what you would do.

Suppose you're 80 years old. You are healthy and do not have any healthcare costs. You know you will suddenly die in one year.

you have a net income of €9,000 per month in your final year of life. Assume you have no other income sources or assets.

How much of this €9,000 would you spend yourself, and how much would you leave for inheritance every month?

- Spend: €[input] per month
- Leave for inheritance: €[input] per month

Trust in pension funds and insurers

Indicate to which degree you agree with the following statement:

I trust pension funds and insurers

- Strongly agree
- Agree
- Neutral
- Disagree
- Strongly disagree
- Don't know / no opinion

A2.4 Heterogeneity among self-employed workers

	(1) Younger	(2) Older	(3) Male	(4) Female	(5) Renter	(6) Home owner
Compute annual contribution limit	-5.976	-8.497	-4.185*	-12.87	-28.48	-5.356**
	(3.219)	(5.679)	(1.667)	(9.878)	(20.83)	(1.805)
Provide income history	-8.001*	-7.417	-5.759**	-8.803	-14.63	-5.639***
	(3.137)	(5.072)	(1.801)	(7.527)	(11.60)	(1.647)
Withdraw with penalty	-4.963	0.914	1.046	-9.921	-5.434	-0.449
	(4.065)	(6.147)	(2.457)	(11.30)	(10.35)	(2.298)
Withdraw low income	11.68**	15.09	9.052***	19.71	54.42	8.748***
	(4.289)	(8.139)	(2.521)	(13.27)	(35.39)	(2.373)
Withdraw for investments	11.56**	10.60	8.720***	13.97	28.67	7.798***
	(4.077)	(6.584)	(2.333)	(10.29)	(19.97)	(2.147)
Withdraw for mortgage	14.76**	13.42	11.93***	16.60	21.38	13.08***
	(4.680)	(7.377)	(2.717)	(11.19)	(15.06)	(2.752)
Observations	5104	8048	6544	6608	3680	9328

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.3: Heterogeneity in demographic characteristics among self-employed workers.

	(1) Lowincome	(2) Highincome	(3) Low liquidity	(4) High liquidity	(5) Nosave	(6) Save	(7) Nosavemore	(8) Savemore	(9) Nocovid	(10) Covid	(11) Incomefluc	(12) Noincomefluc
Compute annual contribution limit	-11.47**	-3.273*	-12.38***	-3.769	-3.331	-13.87*	-11.38*	-8.857***	-6.854	-7.517*	-11.18***	-8.900
	(3.955)	(1.525)	(3.211)	(2.602)	(1.949)	(6.489)	(5.373)	(2.260)	(3.806)	(3.455)	(3.014)	(4.655)
Provide income history	-9.266**	-2.982*	-6.879**	-5.399*	-4.628*	-8.629	-4.737	-8.187***	-5.064	-7.947*	-9.844***	-9.330*
	(3.108)	(1.292)	(2.264)	(2.538)	(2.010)	(4.671)	(4.056)	(1.925)	(3.478)	(3.421)	(2.370)	(4.738)
Withdraw with penalty	1.846	0.0245	2.315	-0.641	0.628	-4.890	-9.488	0.161	-3.555	0.840	-0.885	-0.381
	(3.416)	(1.957)	(2.503)	(3.541)	(2.498)	(6.175)	(7.199)	(2.326)	(5.188)	(4.033)	(2.884)	(5.304)
Withdraw low income	19.25***	7.398***	20.32***	7.025*	7.217**	20.57*	1.328	15.09***	8.063	14.16**	14.52***	15.09*
	(5.495)	(1.986)	(4.898)	(3.295)	(2.638)	(8.642)	(4.189)	(2.974)	(4.888)	(5.068)	(3.531)	(6.706)
Withdraw for investments	13.25**	7.774***	14.27***	6.933*	7.495***	14.12*	0.502	12.66***	7.940	12.45**	14.40***	13.37*
	(4.376)	(1.869)	(3.573)	(3.208)	(2.260)	(6.683)	(4.319)	(2.613)	(4.214)	(4.657)	(3.390)	(6.185)
Withdraw for mortgage	16.60***	10.48***	18.86***	10.60**	9.564**	19.21*	8.894	14.94***	11.77*	13.82**	13.19***	14.27*
	(4.863)	(2.577)	(4.472)	(3.833)	(2.977)	(7.688)	(5.230)	(2.838)	(5.497)	(4.796)	(3.437)	(6.146)
Observations	5664	4720	5776	5088	4336	8816	2256	10544	2672	10256	6800	9840

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.4: Heterogeneity in pension characteristics among self-employed workers.

	(1) Lowrisk	(2) Highrisk	(3) Rational	(4) Naïve	(5) Lowdiscount	(6) Highdiscount	(7) Lowprob80	(8) Highprob80	(9) Trust	(10) Distrust	(11) AnnualContribution	(12) NoAnnualContribution
Compute annual contribution limit	-9.603***	-4.175	-7.261	-9.659***	-3.139	-12.04**	-7.521	-6.506**	-9.294	-5.810	-3.303*	-9.782
	(2.286)	(33.43)	(5.888)	(2.896)	(3.611)	(4.370)	(9.444)	(2.266)	(5.451)	(3.461)	(1.461)	(12.25)
Provide income history	-8.475***	-4.195	-6.163	-8.936***	-4.661	-10.13**	-8.187	-6.433**	-8.670	-5.824	-3.494**	-10.21
	(1.816)	(35.35)	(5.305)	(2.500)	(3.595)	(3.518)	(8.413)	(2.137)	(5.366)	(3.152)	(1.280)	(11.54)
Withdraw with penalty	-0.0242	-0.185	-0.0907	1.109	-0.652	0.672	1.285	-5.893	-30.39	2.623	-5.511*	2.623
	(2.123)	(53.38)	(6.897)	(2.927)	(5.468)	(3.210)	(10.39)	(3.194)	(17.56)	(3.699)	(2.361)	(11.06)
Withdraw low income	14.56***	11.44	8.802	19.70***	10.07	20.72**	14.52	11.67***	21.48*	9.608*	7.097***	18.21
	(2.825)	(53.61)	(7.353)	(4.588)	(5.552)	(6.403)	(14.31)	(2.709)	(9.795)	(4.291)	(1.698)	(18.07)
Withdraw for investments	12.60***	8.511	11.31	14.41***	7.473	15.76**	11.35	11.82***	12.15	10.36*	6.148***	15.14
	(2.610)	(43.08)	(7.734)	(3.688)	(4.889)	(5.026)	(12.32)	(2.614)	(7.167)	(4.323)	(1.551)	(15.99)
Withdraw for mortgage	14.59***	13.10	13.17	17.40***	9.736	20.06***	13.82	13.43***	17.90*	12.84**	9.383***	16.51
	(2.719)	(52.81)	(8.380)	(3.755)	(5.374)	(5.718)	(13.26)	(2.984)	(8.288)	(4.609)	(1.987)	(16.16)
Observations	9984	3168	8784	4368	8560	4592	6944	6208	6032	6848	6880	6272

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.5: Heterogeneity in demographic characteristics among self-employed workers.

A2.5 LAD estimates with varying uniform noise applied

VARIABLES	(1) Full sample	(2) Self-employed	(3) Employees
Benefit deviation %	0.0238***	0.00339**	0.0491***
	(0.00318)	(0.00168)	(0.00455)
Compute annual contribution limit	-0.0574***	-0.0277*	-0.00371
	(0.0123)	(0.0156)	(0.0256)
Provide income history	-0.0593***	-0.0260	-0.0334
	(0.0161)	(0.0162)	(0.0287)
Withdraw with penalty	-0.0141	-0.000751	-0.179***
	(0.0177)	(0.0224)	(0.0440)
Withdraw low income	0.162***	0.0478**	0.178***
	(0.0246)	(0.0220)	(0.0371)
Withdraw for investments	0.0936***	0.0401**	-0.0900***
	(0.0213)	(0.0186)	(0.0338)
Withdraw for mortgage	0.217***	0.0480**	0.238***
	(0.0274)	(0.0236)	(0.0450)
Constant	0.00792	0.00278	0.0606***
	(0.00881)	(0.0102)	(0.0197)
Observations	27,856	13,152	14,704
R-squared	0.019	0.015	0.038

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2.6: LAD estimates with half the uniform noise applied to rounders.

VARIABLES	(1) Full sample	(2) Self-employed	(3) Employees
Benefit deviation %	0.0282***	0.0119***	0.0500***
	(0.00243)	(0.00222)	(0.00437)
Compute annual contribution limit	-0.0665***	-0.0890***	-0.0137
	(0.0154)	(0.0204)	(0.0264)
Provide income history	-0.0704***	-0.0878***	-0.0314
	(0.0152)	(0.0188)	(0.0290)
Withdraw with penalty	-0.0355*	-0.00549	-0.191***
	(0.0203)	(0.0255)	(0.0419)
Withdraw low income	0.194***	0.166***	0.165***
	(0.0220)	(0.0270)	(0.0361)
Withdraw for investments	0.109***	0.141***	-0.0894***
	(0.0186)	(0.0256)	(0.0334)
Withdraw for mortgage	0.257***	0.164***	0.248***
	(0.0242)	(0.0293)	(0.0450)
Constant	0.00794	0.00578	0.0595***
	(0.00947)	(0.0115)	(0.0192)
Observations	27,856	13,152	14,704
R-squared	0.020	0.015	0.039

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2.7: LAD estimates with double the uniform noise applied to rounders.

Chapter 3

The Impact of Retirement on Income and Spending: Causal Evidence from Transaction Data

Abstract

This chapter contributes to the literature that studies the impact of retirement on household finances and financial behavior, often using survey or yearly administrative data. We use high-quality Dutch transaction data to estimate the causal effect of retirement on households' financial outcomes. We use the discontinuity imposed by Statutory Retirement Age (SRA) and variation in the SRA in order to measure causal effects. The monthly data allow us to estimate the direct short-run impact using RD and DiD designs. Our findings show a positive spike in net flow balance at retirement, which financially constrained households use to pay off debts. Debts decline especially for low income, low wealth, blue collar workers, and social insurance recipients.

This chapter was co-authored by Max van Lent and Marike Knoef. This chapter is based on anonymized data from customers of ING Netherlands. Data was treated in strict compliance with the General Data Protection Regulation. The report has been prepared by the authors for the TFI long-term research track. The views and opinions expressed in this chapter are solely those of the authors and do not necessarily reflect the official policy or position of the Think Forward Initiative – TFI – or any of its partners. Responsibility for the data analyses and content in this chapter lies entirely with the authors. The primary purpose of the TFI Research Programme is to inspire practical research insights in the financial decision-making domain. It does not constitute any financial advice or service offer. The data used in this study are confidential and cannot be shared publicly. We are grateful to ING, Stefan van Woelderen, Dominic Keyzer. Additionally, Ruben Lageweg prepared the data. This project would not be possible without them. We thank seminar participants at Leiden University for useful comments and suggestions. We thank Jim Been and Jordy Meekes for useful comments and suggestions.

In addition, we see a gradual increase in the end-of-month balance over time, that is not directly caused by retirement itself.

Keywords: Retirement, Personal Finance, Panel Data, Instrumental Variables, Regression Discontinuity, Difference-in-Difference, Transaction data

JEL Codes: C23, C24, C26, D14, D91, G21, H55, J26

3.1 Introduction

There is a sizable literature that studies the impact of retirement on individual and household finances and financial behavior. Several seminal papers on the life cycle model such as Ando and Modigliani (1963); Heckman (1976); Modigliani and Brumberg (1954) predict smoothing of consumption over the lifetime and a decumulation of wealth over the course of retirement. The empirical evidence on the impact of retirement on consumption is mixed.[1] Furthermore, recent literature finds that wealth for most households remains constant or increases after retirement, contradicting the standard life cycle model.[2]

Current research typically suffers from at least one of the following two issues. The data quality is limited; these papers use either surveys that ask people about their financial position in hindsight or coarse administrative data that suffer from imprecision and/or insufficient frequency. Second, most of the older research suffers from endogeneity bias in the decision to retire. For many people the decision to retire may be related to their finances pre-retirement and/or their plans after retirement. This results in biased estimates of the impact of retirement itself on households finances and financial behavior. Recent literature — exacerbated by the Covid-19 Pandemic — uses transaction data to investigate household and firm behavior (Baker and Kueng (2022); Buda et al. (2023); Carvalho et al. (2021); Floccari et al. (2023); Kapetanios et al. (2022)), highlighting the aforementioned biases.

In this chapter we estimate the impact of retirement on the retiree's finances. Throughout this chapter we focus on the net flow balance (i.e. the total inflow minus the total outflow from accounts), the end-of-month balance (i.e., the sum of the accounts at the end of the month), and whether the person is in debt (i.e., a negative balance). We use variation before and after reaching the statutory retirement age, as well as cohort differences in the statutory retirement age – which strongly affect the actual retirement age – as instruments for

[1] For instance Agarwal et al. (2015); Aguila et al. (2011); Been and Goudswaard (2020); Battistin et al. (2009); Luengo-Prado and Sevilla (2013a,b) find no change in consumption patterns around retirement, while Banks et al. (1998a); Bernheim et al. (2001); Lührmann (2010) discover decreases in consumption around retirement.

[2] See for instance Kieren and Weber (2022); Love et al. (2009); Olafsson and Pagel (2018); Poterba et al. (2011).

retirement. Specifically, by using cohort differences, we identify on the basis age values for which one cohort has reached the statutory retirement age whereas the other has not. Consequently, we estimate the impact of retirement on the net flow balance, the end-of-month balance, and on whether the person is in debt.

Our data originate from ING Netherlands, a large Dutch retail bank. We obtain a sample of around 12,000 individuals born between April 1952 and August 1953 (of which half the sample has 66 and the other half has $66\frac{1}{3}$ as SRA). Everyone in our sample transitions from work to retirement during our sample period. Our data contain monthly total inflows and outflows, and transfers to and from specific (anonymized) bank accounts in the period 2017 to 2021. In order to identify the exact month of retirement, we use the start of repeated inflows from second-pillar pension funds.[3] This implies that our sample consists only of workers who have worked at an employer at some point in their lifetime.

We find no discrete change in the net flow balance or end-of-month balance around the months of retirement. Instead we see a gradual and significant increase in the end-of-month balance with age. This result implies that on average people are building up wealth during retirement, but that this is not caused by the exact timing of retirement. However, we do find that retirement leads to a strongly significant reduction in the probability to be in debt. This is driven by low income, low wealth, blue collar workers, and social insurance recipients.

Our chapter relates to research on consumption around retirement. This research finds mixed effects. Battistin et al. (2009) and Hori and Murata (2019) discover, using survey data from respectively the US and Japan, that consumption decreases after retirement. Alaudin et al. (2019) find – using Malaysian survey data – that this is driven to a large extent by work-related expenses. Aguila et al. (2011) find that food consumption decreases after retirement, and Li et al. (2015) conclude a decrease in the consumption of non-durable goods. On the other hand, Been and Goudswaard (2020) find – using Dutch survey data – no significant impact of retirement on consumption. We contribute to this literature by using detailed transaction data, and using

[3]Second-pillar pension funds are funds build up by a worker's employer during their working life, and these are automatically paid out monthly to the worker when the worker retires. This date can be, and often is, different from the statutory retirement age.

a causal identification strategy.

There is some research that studies the distributional impact of retirement. Fisher and Marchand (2014) find that the drop in consumption following retirement is primarily concentrated in high-consumption household. Hurd and Rohwedder (2013) on the other hand, discover decreases in consumption primarily for low-wealth households. We contribute to this literature by adding a range of short-run heterogeneity tests, with more detailed and more frequent data (on, for instance, spending patterns before retirement).

Our findings are also of interest for policymakers. Many papers have found decreases in consumption and attribute this to inadequate pension savings as a result of poverty prior to retirement. Our data – for the Netherlands – contrast the finding that on average consumption decreases. We do not observe a decrease in inflow, if anything balances increase following retirement in the short run. This leads to healthier finances after retirement, and contradicts the notion that decreased consumption is caused by a lack of finances. Our heterogeneity analyses show that in particular people with a worse financial position prior to retirement improve their financial position as a consequence of retirement.

The rest of this chapter is organized as follows. Section 2 discusses the key aspects of the Dutch retirement system and how these aspects factor into our analysis. Section 3 explain the identification strategy, and section 4 covers the data used. We show and discuss results in section 5. Finally, section 6 concludes.

3.2 Institutional setting

This section provides an overview of how the Dutch retirement system is organized.

As in many countries, the Dutch pension system consists of three pillars. The first pillar operates on a pay-as-you-go basis and entails a uniform public pension provision for all Dutch inhabitants starting from the statutory retirement age. The public pension amount is linked to the net minimum wage and depends on the duration of a person's residency in the Netherlands. Couples residing in the Netherlands in the 40 years preceding their statutory retirement age receive

50% of the minimum wage, while single pensioners receive 70% of the minimum wage. To ensure a minimum standard of living, for individuals with an incomplete public pension, limited pension income, and minimal assets, the public pension is topped up with social assistance (e.g., for immigrants who lived only part of their life in the Netherlands).

In many countries, individuals can access their public pension earlier or later, although with a reduction or additional benefits.[4] In the Netherlands, this is not possible. People cannot access their public pension flexibly. The payout starts at the statutory retirement age for everyone. From its introduction in 1956 until 2013, the statutory retirement age was 65 for both men and women. As from 2013 the statutory retirement age has gradually increased (OECD (2019a)). For the cohorts considered in this study (individuals born between April 1952 and August 1953), the statutory retirement age is 66 or 66 and 4 months (see Table 3.1). This was established in a law passed in June 2015 that allowed individuals to adjust to the new situation. In 2019, a new agreement reduced the pace of the increase in the retirement age, but this did not impact the individuals born between April 1952 and August 1953.

Cohort	Statutory retirement age	Year of statutory retirement	Range of birth dates
1	66	2018	April 1, 1952 – December 31, 1952
2	66 and 4 months	2019	January 1, 1953 – August 31, 1953

Table 3.1: Retirement ages and birth years of the cohorts included in our analysis.

The Dutch second pension pillar comprises employer- and employee-funded occupational pensions. Occupational pensions in the Netherlands are capital-funded and have a mandatory nature, with approximately 90% of all employees having a pension scheme associated with their employer. Primarily, occupational pensions are structured as defined-benefit pension plans. Before the onset of the 21st century,

[4]For example, in the US, benefits can be claimed from the age of 62 and until the age of 70 (Duggan et al. (2007)).

the majority of pension plans aimed to supplement public pensions to achieve a combined pension income totaling 70% of the final gross wage, if an employee had worked full-time for at least 40 years, starting from the age of 65. However, since 2003, pension funds have revised their objectives, now targeting a payout equivalent to 70% of the average career salary (including public pension benefits). Furthermore, with the increase in the statutory retirement age, also the official age used for the accrual of occupational pensions increased. This all results in less generous pension incomes for younger cohorts. People can choose the age at which they access their occupational pension, with a reduction for early withdrawal and a compensation for later withdrawal. Usually, at the statutory retirement age job contracts are terminated. It is possible to work after the statutory retirement age. In such cases, earnings are received in addition to pension benefits. After the statutory retirement age, workers have a lower tax burden as they no longer have to pay pension contributions and employee insurance premiums. Contrariwise, it is possible to retire prior to the statutory retirement age, albeit subject to a lower occupational pension benefit.

Finally, the third pillar comprises private individual pension products, like life annuities, and other private savings. Third-pillar pensions are typically accumulated by self-employed workers, who mostly have an own responsibility to save for their pension, see e.g., Knoef et al. (2016). Throughout this chapter, we primarily focus on first- and second-pillar pensions, as our data contains transactional records detailing the inflow of both public pension and occupational pension on a monthly basis.

3.3 Data

For our analysis, we use transaction data from ING Netherlands, the largest retail bank in the Netherlands serving roughly 9 million Dutch clients. Our analysis starts with a random sample of approximately 20,000 individuals who receive a monthly inflow of at least €800 into their ING accounts and who start receiving occupational pension benefits between January 2016 and January 2021. By selecting individuals with a minimum monthly inflow of €800, we increase the likelihood that ING serves as their primary bank. Individuals included in the study are required to have received occupational pension benefits for at least two consecutive months. Typically, the first receipt of occupational pension benefits coincides with the moment of actual retirement.

Our data include information on the inflow of statutory retirement benefits from September 2017 to January 2021. We select individuals who start receiving statutory retirement benefits at any point during this period. This means that they did not yet receive these benefits in September 2017, but did start receiving these benefits later on. This selection leaves us with approximately 12,000 individuals.

The data encompass all accounts held by the individuals, including shared accounts.[5] Finally, we exclude the top 1% and bottom 1% inflow and outflow observations to prevent outliers from skewing the results.

3.3.1 Variable definitions and descriptive statistics

Since we observe individuals who start receiving their statutory retirement between September 2017–January 2021, and because we have bank data until May 2021, our analyses run from September 2017 until May 2021. For these months we observe account balances and monthly cash flows. Account balances are recorded at the end of each month.

Cash flow data include details on occupational and statutory retirement benefits, as well as total inflow and outflow each month for individual and shared accounts. We compute total inflow and outflow by aggregating the individual and shared accounts. For shared accounts, flows are divided by the number of adults in the household. Since inflow and outflow measures might be affected by transfers between accounts, our primary interest lies in the net flow balance. This measure is calculated each month as the difference between total inflow and total outflow, providing a clear picture of financial movements. In the remainder of this study, we will focus on net flow balance and present results on total inflow and outflow in Appendix A3.1. Fur-

[5]Although our selection criteria ensure a monthly inflow of at least €800, it is possible that clients maintain accounts with other banks as well. This could pose an issue, particularly if the use of these alternative accounts changes post-retirement. However, according to the household survey of the Dutch central bank ((CentERdata (2022))), the majority of ING clients do not have a current account at another bank.

thermore, we compute a variable "In debt" on a monthly basis by generating a binary indicator. This indicator equals 0 when the total balance amount is zero or positive, and equals 1 when the total balance amount at the end of the month is negative.

To take into account inflation, we adjust our financial data with the Consumer Price Index provided by Statistics Netherlands, using April 2021 as the base month. This adjustment ensures that our results reflect real balances and flows.

Finally, individual characteristics such as age, gender, household size, and homeownership status are assessed in May 2021, based on the administrative data of the bank. In the Netherlands, pension funds are predominantly organized by sector, allowing us to categorize individuals into blue-collar and white-collar sectors based on their pension fund affiliations. We categorize blue-collar and white-collar sector workers on the basis of pension flows on individual accounts. This method prevents pension flows from other household members from influencing our classification. However, it also means that individuals who choose to receive their occupational pensions in a shared account cannot be classified into either group. Consequently, these individuals are categorized as neither, resulting in the sum of blue-collar and white-collar sector workers not equaling one. Note that working in a blue or white-collar sector does not necessarily mean being a blue or white-collar worker. For example, in the construction sector (considered blue-collar), there are also individuals with white-collar jobs (e.g., an administrative job).

	Cohort 1			Cohort 2			
	Mean	Median	SD	Mean	Median	SD	P-value equal means
Age	67.81	68	0.52	67.21	67	0.60	0.000
Female	0.45	0	0.50	0.46	0	0.50	0.000
Household size	1.76	2	0.65	1.78	2	0.64	0.000
Blue-collar sector	0.30	0	0.46	0.29	0	0.46	0.000
White-collar sector	0.31	0	0.46	0.34	0	0.47	0.000
UI/DI recipient	0.20	0	0.40	0.17	0	0.38	0.000
Individuals		5734			6392		

Table 3.2: Characteristics of the individuals included in the sample, as observed in May 2021. UI/DI recipient is defined as receiving a UI or a DI benefit in the month prior to reaching the statutory retirement age.

Table 3.2 describes the characteristics of the sample, separating the two cohorts defined in Table 3.1. Cohort 1 has a statutory retirement age of 66, while cohort 2 has a statutory retirement age of 66 years and 4 months. The two cohorts have a roughly equal number of observations. By construction, cohort 1 is older than cohort 2, with averages of 67.81 and 67.21 years, respectively. Other characteristics are very similar between the two cohorts. About 45% of the individuals are female, the average household size is 1.77, for approximately one-third of the sample we know that they work(ed) in a blue-collar sector, and for another one-third we know that they work(ed) in a white-collar sector. Although the differences between cohort 1 and 2 are statistically significant, they are not meaningful economically (e.g., a less than one percentage point difference in the fraction of females, and a 0.01 difference in household size). Finally, approximately 20% of cohort 1 and cohort 2 receives UI/DI benefits in the month prior to reaching the SRA, these are likely older workers having a relatively large distance from the labor market.

To investigate household finances around retirement, we introduce several key variables. First, we define a dummy variable to indicate whether an individual receives occupational pension income. Figure 3.1a illustrates the percentage of occupational pension recipients by age for cohorts 1 and 2. About 30-35% of the individuals receives occupational pension income at the age of 64. This gradually increases to 55%, after which there is a jump of 35%-points to around 90% upon reaching the retirement age. The remarkable increase in occupational pension recipients at the retirement age can be attributed to the typical termination of labor contracts at the statutory retirement age. In addition, the statutory retirement age may serve as a reference point for individuals in their decision to retire. Seibold (2021), for example, shows that in Germany financial incentives alone cannot explain retirement patterns, but there is a large direct effect of statutory retirement ages. Nearly all remaining individuals in the sample start receiving pension benefits within the two years after the statutory retirement age.

Interestingly, we also see a small jump at the age of 65. This also suggests the presence of a social norm effect. Until 2013, the Dutch statutory retirement age was 65. This appears to remain a crucial benchmark for workers, despite subsequent increases in the statutory retirement age. This observation aligns with Behaghel and Blau (2012), who identified a similar pattern in the U.S., indicating that earlier societal norms around retirement ages continue to affect retirement decisions.

Figure 3.1b shows the average unconditional pension inflow by age and cohort. Here we see a spike at the statutory retirement age, which seems to be caused by the one-time payout of small pensions. After this spike, unconditional pension inflow slowly increases, in line with the slow increase in the number of people receiving occupational pension income (Figure 3.1a).

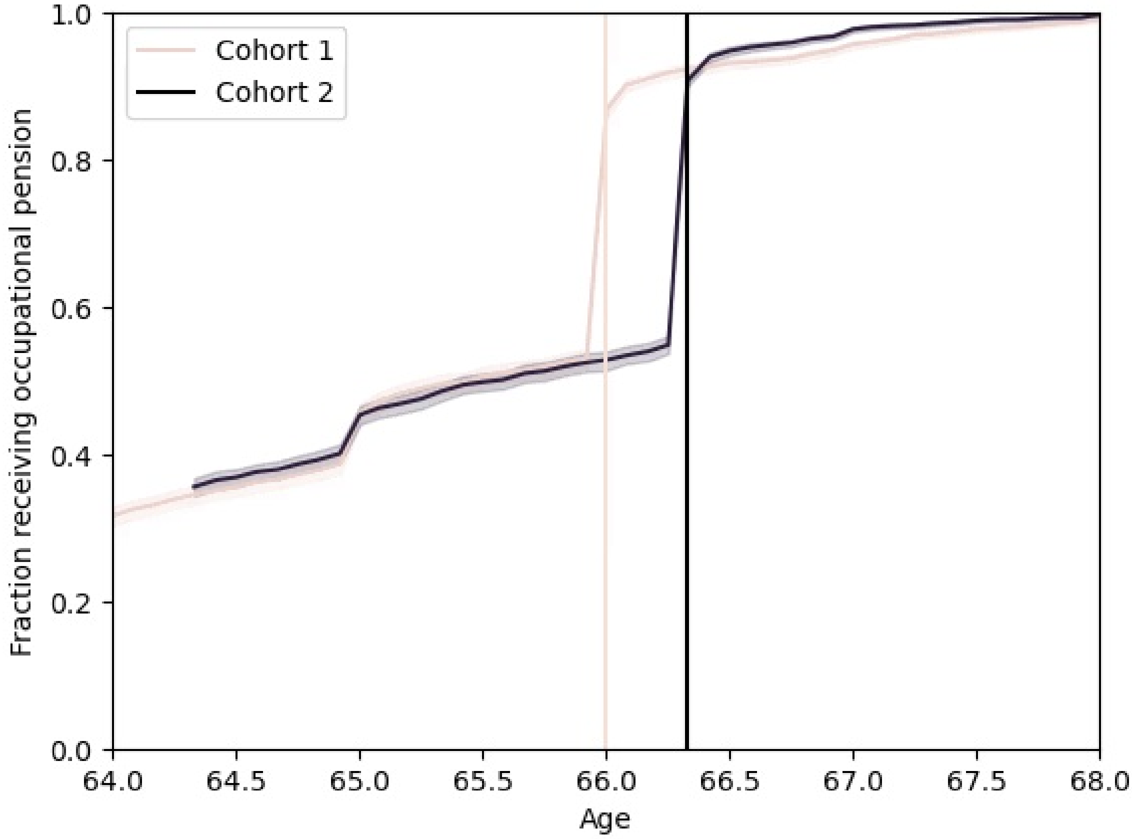


Figure 3.1a: Fraction receiving occupational pension by cohort and age.

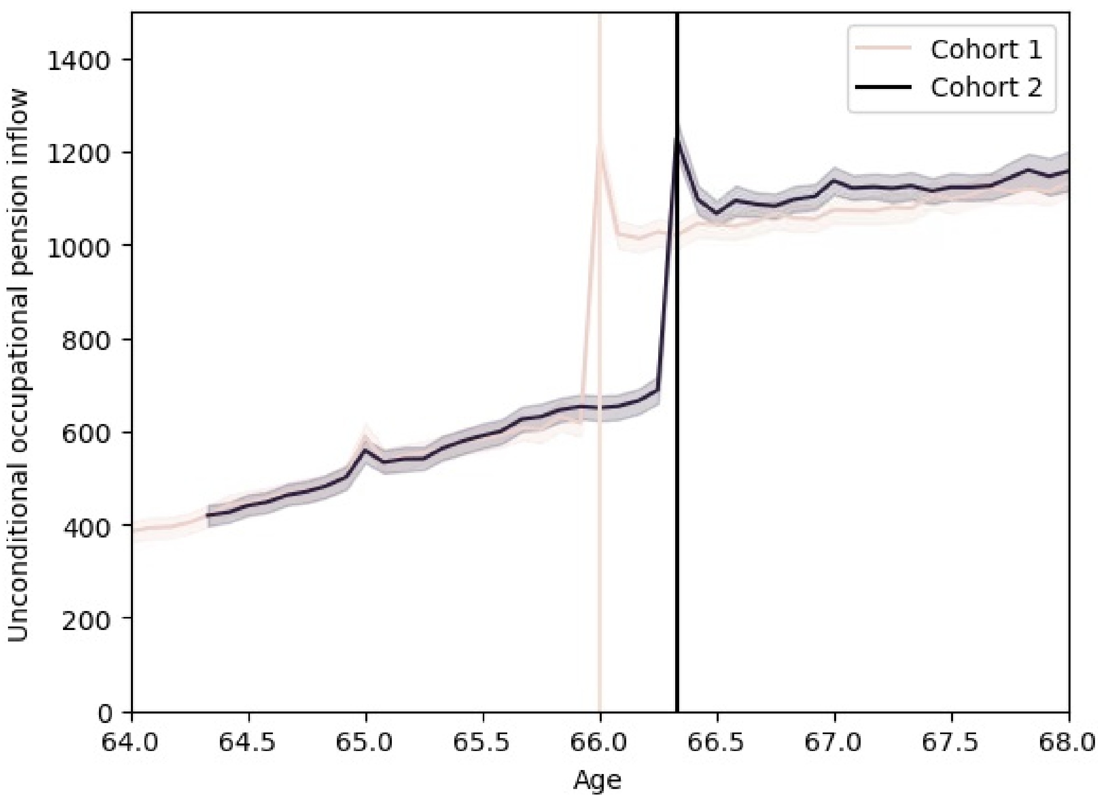


Figure 3.1b: Average occupational pension income by cohort and age.

Figure 3.1: Descriptives of pension recipiency

3.3.2 Financial data

Table 3.3 presents descriptive statistics of the financial variables in our data. Net flow balance is on average €48 for cohort 1 and €57 for cohort 2. The medians are €12 and €16, respectively, showing that for most people savings are low. The standard deviation, however is high with about 1800 euros for both cohorts.

The end-of-month balance, consisting of assets in individuals' ING accounts, is on average 32 thousand euros[6]. Also here the median is smaller at about 11 thousand euros. This discrepancy indicates skewness in the distribution, suggesting that some accounts hold much higher balances. The standard deviations are large, reaching nearly 79 thousand euros for cohort 1 and approaching 66 thousand euros for cohort 2. 4% of cohort 1 is in debt, as compared to 3% of cohort 2.

[6]This fairly closely matches the population average (CBS (2021c)).

The remainder of the variables in Table 3.3 are related to pensions. For about half of the observations in cohort 1, individuals receive a public pension. For the younger cohort 2 this is 44%. The public pension inflow (conditional on receiving public pension), is on average €700. This is in line with the full public pension being about €690 per individual for couples and €998 for singles in 2018 (depending also on a tax credit determined by the amount of other income). Occupational pension inflow, conditional on receiving occupational pensions, is quite similar for both cohorts: on average €919 for cohort 1 and €925 for cohort 2, with medians of 712 and 725 for cohort 1 and 2, respectively. Finally, total pension inflow, equaling the sum of public and occupational pensions, is on average €1411 for cohort 1 and €1345 for cohort 2. Also total pensions are right-skewed, with medians of 1217 and 1184 for cohort 1 and 2, respectively.

	Cohort 1			Cohort 2			
	Mean	Median	SD	Mean	Median	SD	P-value equal means
Net flow balance	48	12	1800	57	16	1774	0.0582
End-of-month balance	31954	10752	78916	32394	11980	65510	0.0277
In debt (binary)	0.04	0	0.19	0.03	0	0.16	0.0000
Fraction receiving public pension	0.52	1	0.50	0.44	0	0.50	0.0000
Public pension inflow (conditional)	700	677	370	701	682	372	0.0000
Fraction receiving occupational pension	0.70	1	0.46	0.69	1	0.46	0.0000
Occupational pension inflow (conditonal)	919	712	889	925	725	884	0.0000
Fraction receiving any pension	0.72	1	0.45	0.72	1	0.46	0.0000
Total pension inflow (conditional)	1411	1217	980	1345	1184	985	0.0000
Observations		270296			261884		
Individuals		5734			6392		

Table 3.3: Descriptive statistics of financial variables. Net flow balance is defined as the total inflow minus the total outflow in a given month. Total pension inflow is defined as the sum of public and occupational pensions.

Figure 3.2 shows the development of net flow balance, end-of-month balance and 'in debt' before and after retirement. The graphs on the left present age on the horizontal axis. The graphs in the middle and the right show the distance to the statutory and the actual retirement age on the horizontal axis, respectively.

Focusing first on the graphs in the top row, we find that net flow balance is predominantly positive and has pronounced spikes at the statutory retirement age. An explanation for these spikes is that a lot of labor contracts are terminated at the statutory retirement age, and

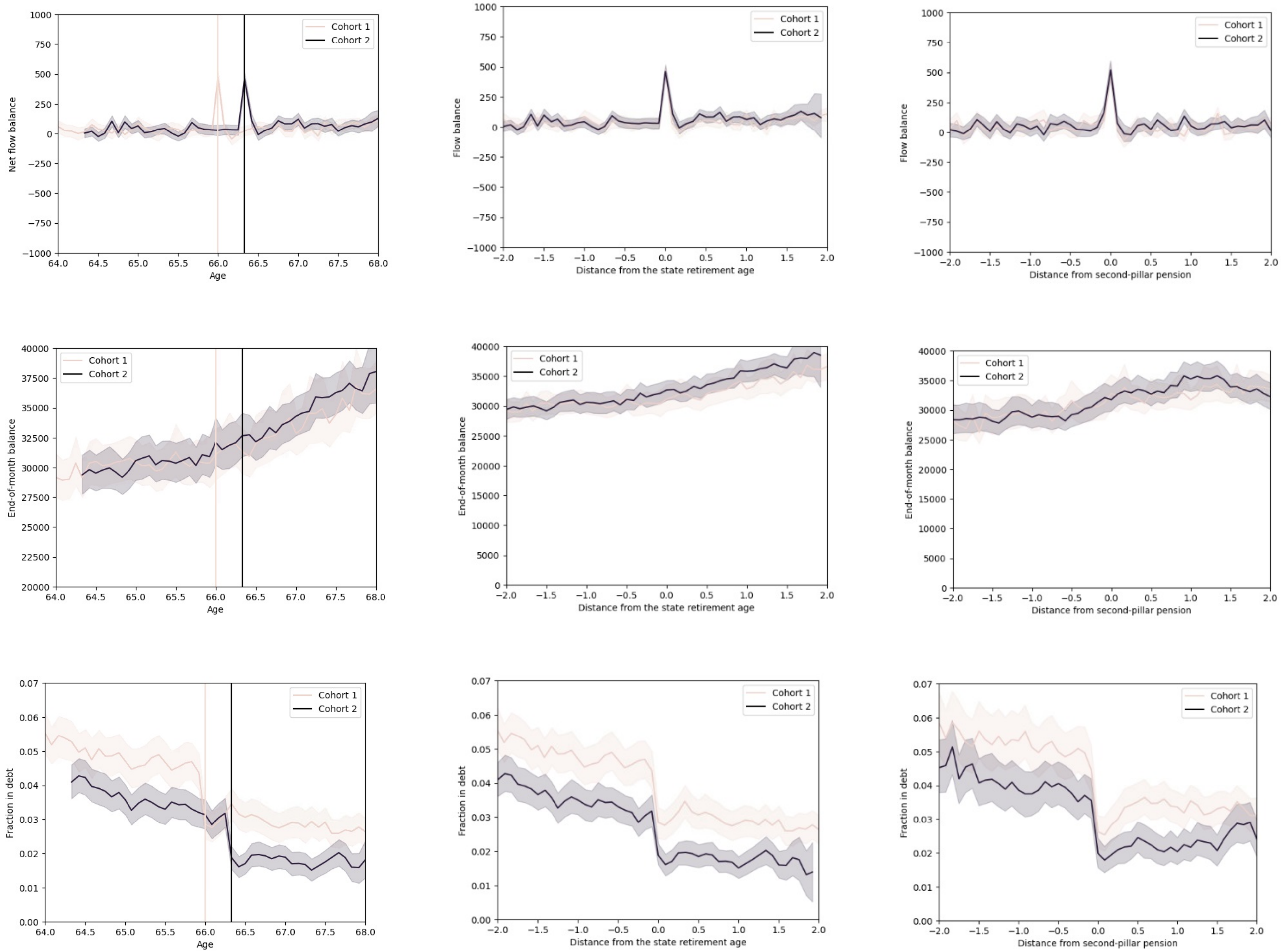


Figure 3.2: Net flow balance, end-of-month balance, and debt by age, as well as distances to the statutory and actual retirement age

that accumulated holiday pay and year-end bonuses are paid out at that moment. Also, at the statutory retirement age some small pensions are paid out as a lump-sum (which we also saw in Figure 3.1b). As detailed in Appendix A3.1, both inflow and outflow peak at the statutory retirement age, with inflow exceeding outflow. When comparing the periods before and after the statutory retirement age, we find that the net flow balance is quite similar.

Looking at the graphs in the middle row, we observe that the end-of-month balance increases with age. In the middle-right figure, however, we do not observe the clear upward trajectory in end-of-month balance. This can be explained by the fact that the end-of-month balance (wealth) is likely to influence the timing of actual retirement. More affluent individuals tend to retire relatively early, resulting in a more stable end-of-month balance around the actual retirement age compared to around the statutory retirement age.

The left-bottom graph shows the fraction in debt by age. In line with Table 3.3, cohort 1 is more often in debt than cohort 2. The percentage of individuals in debt decreases with age, and particularly around the statutory and actual retirement age debts are often paid off.

3.4 Methodology

To assess the effect of retirement on financial outcomes, we estimate several models. We will start with a description of a fixed effects model (Section 3.4.1), then a regression discontinuity design (Section 3.4.2), and finally a difference-in-differences approach in Section 3.4.3.

3.4.1 Fixed effects model

We start with the following Fixed Effects models:

$$F_{it} = \alpha_0 + \alpha_1 R_{it} + X'_{it}\alpha_3 + \eta_i + u_{it} \tag{3.1}$$

Where F_{it} denotes a financial outcome of individual i in month t (that is, net flow balance, end-of-month balance, or in debt). R_{it} denotes a dummy for whether individual i is actually retired in month

t (measured by the inflow of second-pillar pensions), and X_{it} denotes a vector of controls. We control for household size, gender, cohort, and calendar month fixed effects (to capture seasonality). Finally, η_i captures individual-fixed effects, and u_{it} are time-varying error terms. The assumption that the errors u_{it} are identically and independently distributed and independent of R_{it} and X_{it}, and that η_i are not necessarily independent of R_{it} and X_{it} leads to the Fixed Effects model. We will also estimate equation (3.1) without individual-fixed effects using OLS.

The parameter of interest in these regressions is α_1, which describes the association between retirement and the financial outcomes. Although the Fixed Effects model controls for time-invariant individual heterogeneity that is related to selection into retirement, α_1 may not reflect the causal effect of retirement on financial outcomes. That is because there may also be unobserved shocks that affect both retirement behavior and financial outcomes. For example, a health shock or the receipt of an unexpected bequest (or other windfall gains or financial setbacks) may influence both the retirement decision and financial outcomes. In this case, the error terms u_{it} are not independent of R_{it}. Therefore, next we describe a regression discontinuity approach and a difference-in-differences approach where we exploit the discontinuity induced by the SRA and the increase in the SRA to estimate the causal effects of retirement on financial outcomes.

3.4.2 Regression Discontinuity approach

The statutory retirement age creates a discontinuity in the probability of retirement that enables us to apply a regression discontinuity (RD) framework, with age minus the statutory retirement age as the running variable. Elaborating on Stancanelli and Van Soest (2012) and Been and Goudswaard (2020), we use a "fuzzy" regression discontinuity design, because the jump in the probability of retirement at the statutory retirement age is between zero and one.[7] Our fuzzy RD model is specified as follows:

[7] Stancanelli and Van Soest (2012) use a fuzzy RD design to identify the causal effect of retirement on home production. Been and Goudswaard (2020) use this design to estimate the causal effect of retirement on spending and time use decisions using survey data.

$$F_{it} = \beta_0 + \beta_1 R_{it} + \beta_2 f(A_{it}) + \beta_3 PUB_{it} f(A_{it}) + X'_{it}\beta_4 + v_{1it} \quad (3.2)$$

$$R_{it} = \gamma_0 + \gamma_1 PUB_{it} + \gamma_2 f(A_{it}) + \gamma_3 PUB_{it} f(A_{it}) + X'_{it}\gamma_4 + v_{2it} \quad (3.3)$$

Where F_{it}, R_{it}, and X_{it} are specified before, and A_{it} denotes age minus the individual's statutory retirement age (i.e., the distance to the statutory retirement age). PUB_{it} is a dummy variable that indicates whether an individual has reached the statutory retirement age. The discontinuity introduced by the statutory retirement age is the instrument in the analysis. $f(A_{it})$ is a polynomial centered around the statutory retirement age. The term $PUB_{it}f(A_{it})$ allows the slope to be flexible on each side of the statutory retirement age. Finally, the v's are zero-mean errors, and the correlation between the elements of v_1 and v_2 are presumably nonzero. The crucial condition for the instrument PUB_{it} to be valid, is that it is correlated with actual retirement R_{it}, but that it is uncorrelated with v_1. The parameter of interest in the regression is β_1, which provides us with an estimate of the causal effect of retirement on the financial outcomes.

In the baseline we use a first-degree order polynomial for $f(.)$, but we also estimate second-degree order polynomials as a robustness check. Furthermore, our baseline RD specification is a donut RD, where we exclude the month in which the statutory retirement age is reached, as well as the months just before and after the statutory retirement age. We thus exclude the spikes in Figure 3.2, to prevent our findings to be affected by payments, bonuses and/or expenses related to the date of retirement. In Appendix A3.2, we also show the estimation results using the complete dataset (without the donut). We cluster standard errors at the individual level.

Note that the RD model estimates local average treatment effects around the statutory retirement age. The estimates do not necessarily apply to individuals further away from the statutory retirement age. Furthermore, by using the statutory retirement age as an instrument, we estimate a complier average causal effect, where the compliers are those who retire at the statutory retirement age.

3.4.3 Difference-in-Differences approach

Whereas the regression discontinuity design exploits the discontinuity induced by the statutory retirement age, in the difference-in-differences approach we will exploit the increase in the statutory retirement age between cohort 1 and cohort 2. Due to the gradual phase-in of the increase in the statutory retirement age, cohort 2 had to wait an additional 4 months before receiving public pension benefits compared to cohort 1. That is, they became eligible at the age of 66 years and 4 months, while cohort 1 became eligible at the age of 66. On this basis, we can compare cohort 2 with cohort 1, which was not affected by a 4-month delay in receiving public pension benefits. Our reduced form is in line with the models of Staubli and Zweimüller (2013) and Rabaté et al. (2024):

$$F_{it} = \eta_0 + \eta_1 COH2_i + \eta_2 AGE_{it} + \eta_3 PUB_{it} + X_{it}\eta_4 + \iota_{it} \tag{3.4}$$

Where $COH2_i$ denotes a dummy variable indicating whether individual i belongs to cohort 2. AGE_{it} represents the age of the individual. PUB_{it} is one if an individual's age in month t is below the statutory retirement age, and zero otherwise. X_{it} represents a vector of demographic controls, and ι_{it} is the error term.

Since we are interested in the effect of retirement on financial outcomes, we estimate the following instrumented difference in differences model:

$$F_{it} = \delta_0 + \delta_1 COH2_i + \delta_2 AGE_{it} + \delta_3 R_{it} + X_{it}\delta_4 + w_{1it} \tag{3.5}$$

$$R_{it} = \zeta_0 + \zeta_1 COH2_i + \zeta_2 AGE_{it} + \zeta_3 PUB_{it} + X_{it}\zeta_4 + w_{2it} \tag{3.6}$$

The identifying assumption is that, if the statutory retirement age had not been increased, the development of financial outcomes over the life cycle would have been similar between cohort groups not yet qualified for retirement benefits ($COH2$, the treatment group) and those already eligible ($COH1$, the comparison group), after controlling for background characteristics. Under this assumption, δ_3 measures the average causal impact of an increased statutory retirement age on F_{it}, using variation over time. A possible concern is that trends in financial outcomes may alter across age groups over time for reasons not related to the increase in the statutory retirement age.

Our identification stems from age ranges in which one cohort has reached the statutory retirement age whereas others have not yet. We cluster standard errors at the individual level.

3.5 Results

This section shows and interprets the results of our estimations and robustness checks.

3.5.1 OLS and FE results

Table 3.4 presents OLS and Fixed Effects (FE) estimates examining the impact of retirement on net flow balance, end-of-month balance, and the fraction in debt. The OLS results including control variables suggest a slight decrease in net flow balance after retirement, a weakly significant increase in end-of-month balance of about €2000, and a significant decline of 1 percentage point in the fraction of individuals with debt. These findings, however, may be driven by a composition effect. For instance, those possessing substantial pension wealth and savings are more likely to retire early. Such individuals can also afford to save less or dissave after retirement, leading to a negative net flow balance.

Next, we take into account individual fixed effects. The FE estimates including control variables show a slight increase in net flow balance, indicating that individuals have on average €9 per month more left at the end of the month after retirement compared to before retirement. Furthermore, the results show that individuals have on average a significant €1000 more in their bank accounts after retirement than they had before. Finally, debts decrease significantly post-retirement relative to the pre-retirement period, but this is only a decline of -0.6%-points.

In the baseline estimations we exclude the month of retirement, and the months just before and just after retirement (the peak in net flow balance at retirement). Appendix A3.2 shows the OLS and FE results including these months. The conclusions are very similar.

The difference between the OLS and FE results underscores the importance of accounting for individual fixed effects when analyzing the financial implications of retirement. However, there may also be time-variant factors that affect both retirement and financial outcomes. For example, a health shock could act as a third factor, influencing early retirement and simultaneously affecting individuals' financial situation. Additionally, the retirement decision is an outcome of one's financial situation (Burkhalter et al. (2022)). To address this endogeneity, we proceed to estimate models where we exploit discontinuities in the statutory retirement age using a Regression Discontinuity (RD) and Difference-in-Differences (DiD) approach.

Dependent variable	OLS	OLS	FE	FE
Net flow balance	2.73	-22.03***	6.59	9.09**
	(4.57)	(5.36)	(3.98)	(4.12)
End-of-month balance	3995.67***	2011.50*	3189.52***	1053.64***
	(722.64)	(1035.08)	(205.81)	(182.91)
In debt (binary)	-0.020***	-0.013***	-0.015***	-0.006***
	(0.002)	(0.003)	(0.001)	(0.001)
Controls	x	✓	x	✓
Observations	497271	497271	497271	497271
Individuals	12125	12125	12125	12125

Clustered Standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.4: OLS and Fixed Effects (FE) results on the relationship between retirement and several financial outcome measures. FE models estimate individual-fixed effects. Control variables are gender, household size, age, a cohort 2 dummy, and a set of calendar month dummies.

3.5.2 Fuzzy donut RD results

Table 3.5 presents the results of the fuzzy donut RD model. We exclude the month in which the statutory retirement age is reached, as well as the months immediately preceding and following the statutory retirement age. Throughout we choose a bandwidth of 2 years (left and right of the statutory retirement age), based on the results of the optimal bandwidth selection strategy outlined by Imbens and Kalyanaraman (2012). The results of the first stage can be found in

Appendix A3.5. The instrument (that is, the binary indicator of receiving statutory retirement benefits) has a highly significant positive coefficient of 0.36, in line with the fraction of people who retire at the statutory retirement age in Figure 3.1.

Model (2) shows that the results with regard to net flow balance and end-of-month balance are roughly similar to the FE estimates, but less significant. Retirement increases the monthly net flow balance with €16 (similar to the FE estimate of €9, but not significant anymore). The coefficient for end-of-month balance has the same order of magnitude as the FE estimates (€890 compared to €1000), but is no longer significant. Both the FE and fuzzy RD estimates show that the fraction of individuals in debt declines significantly when people retire. The fuzzy RD estimates show a decline of 3.1%-points (compared to an average fraction in debt of 4% in cohort 1 and 3% in cohort 2).

Appendix Table A3.6 presents the full list of coefficients. Interestingly and as expected, we observe a substantial and highly significant positive coefficient for net flow balance in the month of May, which is the month when typically holiday payments are received. Accordingly, the end-of-month-balance is €1529 euros higher than in January (the reference month). Holiday payments are (partly) used to pay off debts, as evidenced by the decline in the fraction of individuals in debt by 1.3%-points during the same month. In the months that follow, the net flow balance turns negative (e.g. holiday spending). Furthermore, in December the end-of-month balance is relatively low compared to January (the reference month). On average, the end-of-month balance is €798 lower in December compared to January. The net flow balance is €99 lower in December compared to the reference month of January.

Appendix Table A3.6 shows the results including the donut observations. That is, including the month in which the statutory retirement age is reached, and the months immediately preceding and following the statutory retirement age. While the results for the end-of-month-balance and 'in debt' remain similar, the coefficient for net flow balance shifts to a substantial and significant value of €239. This shift is likely caused by the high peak in net flow balance observed in the retirement month.

Robustness checks in Appendix A3.3 show that the coefficient for 'in debt' is robust for varying bandwidth sizes around the statutory retirement age. The coefficients for end-of-month balance are similar for bandwidths of 1.5, 2, 2.5, and 3 years, but are more volatile for smaller bandwidths of 0.5 an 1 year. Net flow balance is only significant for larger bandwidths of 2.5 and 3 years. We also estimate the model with a second-order polynomial instead of a first-order polynomial. This leads to conclusions similar to the baseline results. Finally, Table A3.10 shows the reduced form results. These results align with the baseline results, indicating a consistent pattern across the models. However, the coefficients are smaller in the reduced form model as they reflect the intention to treat effects (focusing on the statutory retirement age, instead of actual retirement).

Dependent variable	Model 1	Model 2
Net flow balance	-20.28	16.33
	(24.79)	(25.07)
End-of-month balance	760.97	890.13
	(1043.53)	(1037.97)
In debt (binary)	-0.033***	-0.031***
	(0.004)	(0.004)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125
First-stage F-stat	23716	23017

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 3.5: Fuzzy donut RD estimates examining the effect of retirement on several financial outcome measures. Bandwidth of two years. Control variables are gender, household size, a cohort 2 dummy, and a set of calendar month dummies. Extended estimation results can be found in Appendix A3.6.

3.5.3 Instrumented difference in differences model

Whereas the RD in Table 3.5 exploits the discontinuity in retirement caused by the statutory retirement age, Table 3.6 shows the estimation results of the Instrumented difference-in-differences model, exploiting the increase in the statutory retirement age between cohort 1 and cohort 2. Figure 3.1a shows a clear parallel trend for cohorts 1 and 2 before the statutory retirement age. At the age of 66, cohort 1 reaches their statutory retirement age of 66. Upon examining the figure, it seems very likely that, in the absence of the increase in the retirement age, cohort 2 would have exhibited a similar pattern to cohort 1. Also with regard to the financial outcomes (Figure 3.2) we see clear parallel trends in the graphs in the left column.

The first-stage results show that the instrument (that is, the binary indicator of receiving occupational pension benefits) has a highly significant positive coefficient of 0.35 (Appendix A3.5). This is very similar to the coefficient found in the fuzzy donut RD model. Also the second stage results lead to similar conclusions. The coefficient for 'in debt' shows again a significant drop in the fraction of individuals in debt by 3.5%-points. The coefficients for net flow balance and end-of-month balance have the same order of magnitude and are not significantly different from zero. Appendix Table A3.6 presents the full list of coefficients and show similar dynamics across the year as we found in the fuzzy donut RD model (a relatively large net flow balance and end-of-month balance in May, and a relatively low end-of-month balance in December). In both models, the coefficients suggest that part of the holiday payments are used to pay off debt.

To make a clean comparison with the fuzzy RD donut model, in the instrumented diff-in-diff model we used the same estimation sample as in the fuzzy RD donut model. That is, we excluded the observations at the statutory retirement age, and the months immediately preceding and following the statutory retirement age. In Table A3.6 we estimate the same model including these observations. The results lead to the same conclusions as the fuzzy RD model (without the donut). Namely, the fraction in debt declines with 3.5%points, the coefficient of the end-of-month balance is not significant, and net flow balance is relatively large: €239. The reduced form results (Appendix Table A3.10) align again with the baseline results, and are smaller than the baseline results as they reflect the intention to treat effect.

Dependent variable	Model 1	Model 2
Net flow balance	-5.76	26.20
	(34.53)	(25.43)
End-of-month balance	1020.52	1436.00
	(1775.47)	(1053.85)
In debt (binary)	-0.029***	-0.031***
	(0.005)	(0.004)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125
First-stage F-stat	22429	22191

Clustered Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Instrumented DiD estimates examining the effects of retirement on financial outcome measures. Control variables are gender, household size, age, and month dummies. Extended estimation results can be found in Appendix A3.6.

3.5.4 Heterogeneity

Next, we are interested in heterogeneity analyses for several subsamples. We separate our estimates by gender, by blue/white collar sector, income, household size, wealth, and whether the individual received UI or DI benefits prior to receiving statutory retirement benefits.[8].

Table 3.7 shows RD estimates for the aforementioned subgroups. We find small but positive net flow balance effects for women, but no other gender-related differences. Net flow balances increase sharply for single-person households, and point estimates are significantly higher than for multi-person households. When comparing blue-collar workers to white-collar workers, UI/DI recipients to non-UI/DI recipients, and comparing high-inflow groups to low-inflow groups we find remarkable differences: Debt decreases more sharply for low-inflow groups, and end-of-month balances increase. The accumulation of savings observed in low-inflow and low-wealth households after retirement is particularly noteworthy, given their relatively limited savings prior to retirement.

[8]We define income and wealth as 'high' ('low') as the individual has an above (below) median income and wealth in 2016, as compared to the rest of the individuals in the sample. By selecting on flows and balances in 2016, we preclude income and wealth being an outcome of occupational pensions, as none of the individuals in the sample receive occupational pension benefits yet in this year. Group means of the dependent variables for these subgroups prior to receiving occupational pensions are shown in Table A3.17 of A3.6

The net flow balance and end-of-month balance estimates based on wealth initially appear counter-intuitive. The positive net flow balance estimate for the high-wealth group is attributable to high-wealth individuals with low inflows improving their net flow balance. Conversely, the absence of a positive net flow balance effect for the low-wealth group is because low-wealth individuals with high inflows worsen their net flow balances after retirement.

Comparing estimates to the base levels in Table A3.17, net flow balances increase sharply for financially constrained individuals, whereas debts decrease by over half of their mean value. For UI/DI recipients, Appendix A3.8 presents outcome measures based on age, distance from the statutory retirement age, and distance from receiving occupational pensions. This subgroup exhibits lower net flow balances and higher debt rates, which decrease more sharply after retirement than in the remainder of the sample.

Table 3.8 presents DiD estimates for the aforementioned subgroups. The results are similar to the findings in Table 3.7, albeit less precisely estimated. We no longer observe any significant net flow balance effects. Only end-of-month balance estimates for low-wealth individuals remain significantly positive. However, remarkable differences in debt remain intact. Contrasting white-collar and blue-collar workers, we find larger decreases in debt for blue-collar workers than for white-collar workers. As in Table 3.7, debt decreases more sharply for UI/DI recipients, low-inflow and low-wealth individuals, and blue collar workers than for the remainder of the sample. The decrease in accuracy is likely driven by our DiD model identifying based on a four-month difference in the SRA, whereas our RD model identifies based on a 2-year bandwidth around the SRA.

Dependent variable / subgroup	Men	Women	Single-person	Multi-person	DI/UI	No DI/UI
Net flow balance	6.52	23.25***	101.35***	-28.92	26.85***	13.03*
	(8.75)	(7.44)	(34.80)	(6.53)	(9.05)	(7.06)
End-of-month balance	-799.34	1738.02	1133.81	1234.66	2151.58*	805.57
	(1225.87)	(1098.49)	(2883.33)	(1631.46)	(1212.78)	(1051.05)
In debt (binary)	-0.031***	-0.032***	-0.032***	-0.031***	-0.044***	-0.028***
	(0.003)	(0.003)	(0.009)	(0.004)	(0.006)	(0.002)
Controls	✓	✓	✓	✓	✓	✓
Observations	265436	219710	162217	323372	92515	403074
Individuals	6569	5544	3897	8151	2211	9837

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable / subgroup	White-collar	Blue-collar	High-inflow	Low-inflow	High-wealth	Low-wealth
Net flow balance	4.23	69.78***	-6.68	34.05***	39.17***	-3.75
	(12.73)	(10.20)	(11.15)	(5.75)	(10.74)	(6.23)
End-of-month balance	-3148.53**	-1124.62	381.94	2106.76***	227.98	2360.75***
	(1516.77)	(1208.20)	(1819.63)	(573.37)	(1840.97)	(218.43)
In debt (binary)	-0.028***	-0.039***	-0.024***	-0.037***	-0.002**	-0.056***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)
Controls	✓	✓	✓	✓	✓	✓
Observations	158813	143579	247780	247809	247766	247843
Individuals	3991	3603	6002	6046	6042	6006

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Heterogeneity analyses for the instrumented donut fuzzy RD estimates, examining the effects of retirement on several financial outcome measures. Estimates control for gender, household size, a cohort 2 dummy, and a set of month dummies.

Dependent variable / subgroup	Men	Women	Single-adult	Multi-adult	UI/DI	No UI/DI
Net flow balance	10.69	27.10	109.95	-17.82	33.04	24.15
	(65.64)	(71.33)	(116.81)	(45.80)	(95.37)	(52.48)
End-of-month balance	-576.30	1903.96	1599.16	1754.26	2479.36	1342.85
	(3179.30)	(3038.71)	(4200.52)	(2083.77)	(3270.23)	(2126.04)
In debt (binary)	-0.031***	-0.032***	-0.033**	-0.031***	-0.045*	-0.028***
	(0.010)	(0.011)	(0.015)	(0.008)	(0.023)	(0.007)
Controls	✓	✓	✓	✓	✓	✓
Observations	265436	219710	162217	333372	92515	403074
Individuals	6569	5544	3897	8151	2211	9837

Dependent variable / subgroup	White-collar	Blue-collar	High-income	Low-income	High wealth	Low wealth
Net flow balance	11.55	72.00	8.84	39.97	51.52	4.62
	(122.19)	(106.30)	(72.95)	(53.87)	(78.91)	(50.88)
End-of-month balance	-2949.49	-1120.25	1147.78	2387.56	1071.83	2534.76**
	(6177.27)	(5861.48)	(3017.58)	(1839.27)	(3431.65)	(1289.69)
In debt (binary)	-0.028***	-0.039***	-0.024***	-0.037***	-0.003	-0.056***
	(0.015)	(0.016)	(0.008)	(0.012)	(0.002)	(0.014)
Controls	✓	✓	✓	✓	✓	✓
Observations	340554	143579	247780	247809	247766	247823
Individuals	3991	3603	6002	6046	6042	6006

Table 3.8: Heterogeneity analyses for the instrumented DiD estimates, examining the effects of retirement on several financial outcome measures. Control variables are gender, household size, age, and a set of month dummies

3.6 Conclusion

In this chapter, we investigate how retirement affects the financial behavior of retirees. Using the statutory retirement age as an instrument for actual retirement, we estimate how retirement affects cash flows and account balances using data from the largest retail bank in the Netherlands.

One of the contributions of our chapter lies in the use of high quality and high frequency bank account data. As compared to survey data, we have richer and more detailed data. This allows us to estimate a new support of income and spending behavior more accurately. For instance, we can identify effects in the exact month of retirement. This new support exhibits effects similar to those found in the existing survey data literature. With our identification strategy we estimate causal effects, as opposed to most of the existing retirement literature.

In the short-run, we find a spike in the net flow balance at the retirement age that is used to pay off debt, whereas account balances increase over time. As such, retirement spending behavior exhibits both an anticipatory effect and an immediate response, but exhibits smoothing effects in the longer run after retirement (positive age effects). These findings add a layer of depth to the retirement-consumption puzzle.

Additionally, our findings suggest that retirement in the Netherlands alleviates financial constraints. Specifically, for groups with lower educational levels, incomes, wealth, and UI/DI benefits. For them, we observe increases in both net flow balances and end-of-month balances after retirement. We do not observe decreases for individuals with higher education levels, incomes, and wealth, indicating that retirement does not impose financial constraints on wealthier individuals.

In general, our findings on the retirement-consumption puzzle may explain existing findings in the survey data literature of both papers that do (i.e., Hori and Murata (2019)) and do not (i.e., Been and Goudswaard (2020) show a drop in consumption after retirement. We discover positive net flow balances and wealth accumulation in the longer run, but no short-run effects of retirement apart from the spike at retirement. These findings indicate that capital accumulation is a long-run effect rather than a direct result of retirement.

This chapter informs policy in two ways. First, the absence of negative net flow balance effects suggests that the combination of first- and second-pillar pensions is, in general sufficient. The observed decreases in debt, particularly for low-income groups, indicates that retirement may help relatively vulnerable individuals pay off debts and accumulate wealth in the short run. Secondly, both the cash flow and end-of-month balance dynamics that we observe contribute to the discussion on the size as well as flexibility of retirement benefits over time.

For future research we emphasize the importance of more detailed and more frequent transaction data: Data at an even higher frequency, more detailed measures of flows and a broader range of individual characteristics may help further understand the nuances underlying the retirement-consumption puzzle.

A3 Appendices

A3.1 Inflow and outflow summary statistics and estimates

Table A3.1 presents summary statistics of inflow and outflow, separated on the basis of cohort. The average Inflow and outflow are slightly higher for cohort 1, as is the median. The difference in means is significantly different from zero.

	Cohort 1			Cohort 2			
	Mean	Median	SD	Mean	Median	SD	P-value equal means
Total inflow	3440	2427	3375	3353	2347	3352	0.0000
Total outflow	3392	2369	3253	3296	2287	3219	0.0000
Observations		270296			261884		
Individuals		5734			6392		

Table A3.1: Descriptive statistics of total inflow and total outflow.

Table A3.2 presents OLS and FE estimates for inflow and outflow. Inflow and outflow are estimated to slightly decrease after retirement in our OLS models. Controls reduce the estimated effect. FE estimates instead show increases in inflow and outflow of approximately €100. Our findings indicate that, as with our other outcome measures, the retirement decision is highly endogenous. OLS and FE models consequently cannot capture causal effects.

Dependent variable	OLS	OLS	FE	FE
Inflow	-218.96***	-160.48***	99.88***	106.19***
	(37.26)	(36.46)	(14.04)	(14.06)
Outflow	-207.27***	-147.13***	93.29***	101.44***
	(36.78)	(35.97)	(13.69)	(13.72)
Controls	x	✓	x	✓
Observations	487402	487402	487402	487402
Individuals	12102	12102	12102	12102

Clustered Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.2: OLS and FE estimates of inflow and outflow as a result of retirement. FE models include individual-fixed effects. Control variables are a cohort 2 dummy, household size, gender, age, and a set of month dummies.

Figure A3.1 illustrates inflow and outflow dynamics on the basis of distance from the statutory retirement age and occupational pension recipiency, respectively. Inflow and outflow spike after the receipt of statutory retirement benefits as well as the receipt of occupational benefits. Afterward, inflow and outflow decline, but remain slightly higher than before receiving statutory retirements and occupational pensions.

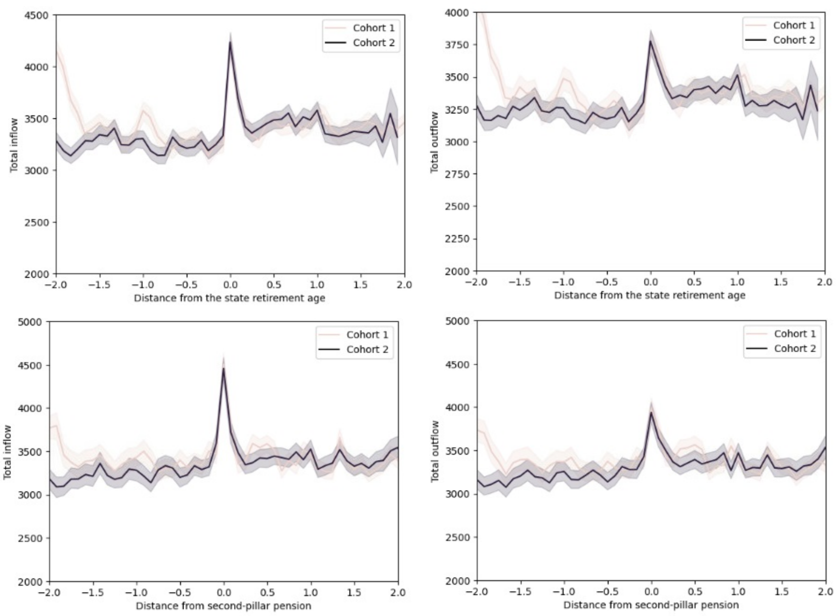


Figure A3.1: Inflow and outflow before and after the statutory retirement age and receiving occupational pension.

Tables A3.3 and A3.4 present RD and DiD estimates, respectively, for inflow and outflow. Inflow and outflow sharply increase for individuals who comply with the statutory retirement age, and the estimated effect intensifies after accounting for controls. However, we cannot verify whether these effects are due to increases in income and spending or whether they result from administrative processes such as transfers between bank accounts.

Dependent variable	Model 1	Model 2
Inflow	724.24***	855.75***
	(54.62)	(82.58)
Outflow	794.52***	839.42***
	(57.63)	(53.35)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125
First-stage F-stat	23716	23017

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.3: Instrumented RD estimates of inflow and outflow as a result of retirement. Control variables are a cohort 2 dummy, household size, gender, and a set of calendar month dummies.

Dependent variable		
Inflow	854.88***	904.86***
	(55.74)	(56.39)
Outflow	871.33***	878.66***
	(70.07)	(54.55)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125
First-stage F-stat	22429	22191

Clustered Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.4: Instrumented DiD estimates of inflow and outflow as a result of retirement. Control variables are age, household size, gender, and a set of calendar month dummies.

A3.2 Robustness checks: including the cutoff observations

The baseline models are estimated with a donut: we exclude the observation at the statutory retirement age, the observation just before the statutory retirement age, and the observation just after the statutory retirement age. In this way, we mitigate the effect of the peak in net flow balance at retirement. Since this effect is interesting in itself, and we are interested in the robustness of the results, we also estimated the models including these cutoff observations and show the results in this appendix.

Table A3.5 present OLS and Fixed Effects including the cutoff observations. Estimates are highly similar to those in 3.4: We find slight negative estimates for flow balance, positive effects for end-of-month balances in the fixed effects models, and strong negative effects on debt.

Dependent variable	OLS	OLS	FE	FE
Net flow balance	10.06***	-11.97**	21.87***	7.46*
	(4.51)	(5.28)	(3.92)	(4.07)
End-of-month Balance	3784.72***	1861.50*	3079.27***	1044.20***
	(723.41)	(1018.87)	(197.72)	(173.96)
In debt (binary)	-0.010***	-0.013***	-0.015***	-0.007***
	(0.002)	(0.003)	(0.001)	(0.001)
Controls	x	✓	x	✓
Observations	531277	531277	531277	531277
Individuals	12125	12125	12125	12125

Clustered Standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A3.5: OLS and FE estimates of cash flows as a result of retirement including the cutoff observations. FE models include individual-fixed effects. Control variables are gender, age, a cohort 2 dummy, household size, and a set of month dummies.

Tables A3.6 and A3.7 report estimates that include the threshold observations for the instrumented RD model and the instrumented DiD model, respectively. Net flow balances sharply increase as a result of the statutory retirement cutoff observations. As in our donut models, we find no short-run effect for end-of-month balances and observe a sharp decrease in debt.

Dependent variable	Model 1	Model 2:
Net flow balance	200.31***	238.78***
	(22.42)	(22.98)
End-of-month Balance	1067.46	1236.26
	(1514.98)	(1010.56)
Debt (Binary)	-0.035***	-0.034***
	(0.003)	(0.003)
Controls	x	✓
Observations	532177	532177
Individuals	12125	12125
First-stage F-stat	30042	29261

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.6: RD estimates of cash flows as a result of retirement including the cutoff observations. Control variables are gender, a cohort 2 dummy, household size, and a set of month dummies.

Dependent variable	Model 1	Model 2:
Net flow balance	200.08***	239.14***
	(22.35)	(23.09)
End-of-month balance	805.61	1881.29
	(1511.56)	(1272.59)
Debt (Binary)	-0.033***	-0.034***
	(0.004)	(0.004)
Controls	x	✓
Observations	532177	532177
Individuals	12125	12125
First-stage F-stat	29693	29313

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.7: DiD estimates of cash flows as a result of retirement including the cutoff observations. Control variables are gender, age, household size, and a set of month dummies.

A3.3 Robustness checks fuzzy donut RD model

To check the robustness of the fuzzy donut RD results, we estimate our models with different bandwidths and we estimate our model with a second-order polynomial instead of a first-order polynomial.

Table A3.8 exhibits RD estimates with varying bandwidths. As in the main results, roughly 35% of the sample complies with the statutory retirement age. We find effects close to 0 for net flow balances, only positive for the three-year bandwidth. End-of-month balance estimates likewise show no statistically significant effects, only being positive for the half-year bandwidth. Finally, debt decreases for all bandwidth sizes, with the magnitude of the decrease amplifying as the bandwidth size decreases.

Dependent variable	Bandwidth 3 yr	Bandwidth 2.5 yr	Bandwidth 1.5 yr	Bandwidth 1 yr	Bandwidth 0.5 yr
Occupational pension (binary)	0.378***	0.367***	0.355***	0.376***	0.380***
	(0.002)	(0.002)	(0.003)	(0.004)	(0.007)
Net flow balance	52.94***	37.49*	31.39	31.07	-74.56
	(19.52)	(21.21)	(29.67)	(41.18)	(79.51)
End-of-month balance	770.89	1007.84	900.22	565.50	3777.18*
	(1069.80)	(1007.34)	(1088.16)	(1233.61)	(1944.07)
In debt	-0.029***	-0.029***	-0.034***	-0.036***	-0.044***
	(0.003)	(0.004)	(0.004)	(0.004)	(0.008)
Controls	✓	✓	✓	✓	✓
Observations	652391	594220	385560	252228	115769
Individuals	12125	12125	12125	12124	12114

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.8: Donut RD estimates with varying bandwidth sizes between 0.5 and 3 years. Control variables are gender, a cohort 2 dummy, household size, and a set of month dummies.

Table A3.9 presents RD estimates using a second-order polynomial instead of a first-order polynomial. Compliance with the statutory retirement age closely aligns to the main results. Similarly, the estimates for our dependent variables are similar to those in 3.5: Net flow balances and end-of-month balances are not affected by receiving occupational pension, whereas debt sharply decreases.

Dependent variable	Model 1	Model 2
Occupational pension (binary)	0.360***	0.363***
	(0.004)	(0.004)
Net flow balance	-18.08	27.05
	(45.92)	(45.47)
End-of-month Balance	-179.56	650.34
	(1820.32)	(1412.72)
In debt (binary)	-0.032***	-0.039***
	(0.005)	(0.005)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125
First-stage F-stat	7751	7877

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A3.9: Donut RD estimates with a second-order polynomial instead of a first-order polynomial. Control variables are gender, a cohort 2 dummy, household size, and a set of month dummies.

A3.4 Reduced form results

Tables A3.10 and A3.11 present reduced form estimates of Tables 3.5 and 3.6, respectively. The estimates in Tables A3.10 and A3.11 are approximately a quarter of the estimates in the main results. These estimates closely match our main results when scaled by the first stage in Tables A3.15 and A3.16

Dependent variable	Model 1	Model 2
Net flow balance	-7.31	5.82
	(8.93)	(8.94)
End-of-month Balance	274.57	317.49
	(374.64)	(370.25)
In debt (binary)	-0.012***	-0.011***
	(0.001)	(0.001)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125

Clustered standard errors in parentheses.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A3.10: RD reduced form estimate on the basis of receiving statutory retirement benefits. Control variables are a cohort 2 dummy, household size, gender, and a set of month dummies.

Dependent variable	Model 1	Model 2
Net flow balance	-5.76	9.16
	(8.87)	(8.89)
End-of-month Balance	357.74	502.33
	(370.75)	(370.25)
In debt (binary)	-0.011***	-0.012***
	(0.001)	(0.001)
Controls	x	✓
Observations	497271	497271
Individuals	12125	12125

Clustered standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table A3.11: DiD reduced form estimate on the basis of receiving statutory retirement benefits. Control variables are age, household size, gender, and a set of month dummies.

A3.5 First stage results

Table A3.12 presents the full coefficient estimates of the first-stage models for the RD estimates and the DiD estimates, respectively. Individuals receive occupational pension benefits before the statutory retirement age at a rate of 12% per year, which decelerates to 4% per year after the statutory retirement age. Cohort 2 receives occupational pensions more often, whereas we find mixed effects for women. Larger households are less likely to receive occupational pensions, potentially due to financial constraints. Finally, individuals retire in January and September relatively often, and less often in April, May, and June.

Dependent variable:	Occupational pension	Occupational pension
Model:	RD approach	DiD approach
State pension	0.3567***	0.3498***
	(0.002)	(0.002)
Distance SRA	0.1206***	
	(0.001)	
State pension * Distance SRA	-0.0799***	
	(0.002)	
Cohort 2	0.0328***	0.0089***
	(0.001)	(0.001)
Age minus 66		0.0859***
		(0.001)
Female	-0.0239***	0.0240***
	(0.001)	(0.001)
Household size	-0.0200***	-0.0199***
	(0.001)	(0.001)

Month = 2	-0.0015	-0.0010
	(0.003)	(0.003)
Month = 3	-0.0040	-0.0040
	(0.003)	(0.003)
Month = 4	-0.0047*	-0.0066**
	(0.003)	(0.003)
Month = 5	-0.0049*	-0.0042
	(0.003)	(0.003)
Month = 6	-0.0056**	-0.0056**
	(0.003)	(0.003)
Month = 7	-0.0020	-0.0023
	(0.003)	(0.003)
Month = 8	-0.0009	-0.0014
	(0.003)	(0.003)
Month = 9	0.0014	0.0008
	(0.003)	(0.003)
Month = 10	-0.0002	-0.0010
	(0.003)	(0.003)
Month = 11	-0.0022	-0.0033
	(0.003)	(0.003)
Month = 12	-0.0032	-0.0041
	(0.003)	(0.003)
Constant	0.5809***	0.5413***
	(0.003)	(0.003)
Observations	497271	497271
Individuals	12125	12125

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3.12: Full parameter list of the first stage estimates of the models estimated in Table 3.5 and 3.6, respectively. Reference categories are the first month of the year (January) and male. Baseline age is equal to the statutory retirement age (66 for cohort 1 and 66.33 for cohort 2).

A3.6 Full estimation results

Tables A3.13 and A3.14 show the full estimation results of our OLS and FE models, respectively. In the OLS estimates there are minor differences on the basis of demographic characteristics. Total balance and debt respectively decrease with age. In months 5, we observe a spike in the net flow balance and total balance, and a drop in debt, these effects slowly return to 0 throughout months 6 to 8. The FE estimates reveal roughly similar, but far more precisely estimated differences than in the OLS models, revealing more small but significant monthly differences in household finance.

Dependent variable:	Net flow balance	End-of-month balance	In debt
Occupational pension	-22.231***	2011.50*	-0.013***
	(5.238)	(1035.08)	(0.003)
Female	22.275***	-1815.48	-0.003
	(4.048)	(1163.59)	(0.002)
Age minus 66	7.816	1511.45***	-0.005***
	(5.298)	(299.71)	(0.001)
Household size	-38.495***	265.28	0.004*
	(4.865)	(1020.19)	(0.002)
Cohort 2	20.884**	318.22	-0.011***
	(10.222)	(1146.28)	(0.002)
Month = 2	67.60***	674.17***	-0.002***
	(13.43)	(201.19)	(0.001)
Month = 3	38.512***	687.68***	-0.000
	(13.59)	(246.89)	(0.001)
Month = 4	2.47	891.22***	0.001
	(12.99)	(255.05)	(0.001)
Month = 5	338.51***	1522.04***	-0.012***
	(13.54)	(268.52)	(0.001)
Month = 6	-64.77	831.59***	-0.008***
	(13.95)	(252.32)	(0.001)
Month = 7	-181.40***	819.69***	-0.003***
	(13.05)	(269.66)	(0.001)
Month = 8	-49.56***	1021.22***	0.000
	(13.492)	(261.62)	(0.001)
Month = 9	4.29	905.69***	-0.002**
	(12.75)	(255.84)	(0.001)
Month = 10	-103.14***	610.74***	0.002***
	(12.55)	(233.28)	(0.001)
Month = 11	17.40	437.40*	0.001
	(12.95)	(249.04)	(0.001)
Month = 12	-98.91***	-770.38***	-0.001
	(15.70)	(288.52)	(0.001)
Constant	143.532***	30320***	0.044***
	(14.34)	(2383)	(0.005)
Observations	497,241	497,241	497,241
Individuals	12,125	12,125	12,125

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3.13: Full parameter list of the OLS model estimated in Table 3.4. FE models include individual-fixed effects. Reference categories are the first month of the year (January), cohort 1, and male.

Dependent variable:	Net flow balance	End-of-month balance	In debt
Occupational pension	-9.09	1053.64***	-0.006***
	(9.68)	(182.91)	(0.001)
Female	0.33***	-7.46	0.000*
	(0.14)	(8.15)	(0.000)
Age minus 66	14.38***	1579.17***	-0.007***
	(1.94)	(121.56)	(0.000)
Household size	0.10	47.53***	-0.000***
	(0.12)	(6.55)	(0.000)
Cohort 2	0.58**	-75.10***	0.000**
	(0.29)	(12.23)	(0.000)
Month = 2	65.75***	215.10***	-0.002***
	(13.35)	(68.27)	(0.001)
Month = 3	-68.46***	6.29	0.000
	(12.49)	(83.27)	(0.000)
Month = 4	0.92	235.26***	0.002***
	(12.87)	(83.52)	(0.001)
Month = 5	335.86***	730.73***	-0.012***
	(13.56)	(134.37)	(0.001)
Month = 6	-65.94***	796.14***	-0.008***
	(13.84)	(101.32)	(0.001)
Month = 7	-181.22***	589.62***	-0.003***
	(13.02)	(109.93)	(0.001)
Month = 8	-50.50***	542.99***	0.001
	(13.38)	(102.18)	(0.001)
Month = 9	3.53	573.79***	-0.001*
	(12.70)	(95.74)	(0.001)
Month = 10	-104.67***	275.18**	0.002***
	(12.51)	(109.44)	(0.001)
Month = 11	16.00	347.06***	0.001**
	(12.51)	(102.72)	(0.001)
Month = 12	-97.35***	-148.77*	-0.001
	(15.61)	(84.76)	(0.001)
Constant	17.70*	-1173.32***	0.006***
	(9.68)	(145.56)	(0.001)
Observations	497,241	497,241	497,241
Individuals	12,125	12,125	12,125

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3.14: Full parameter list of the FE model estimated in Table 3.4. Reference categories are the first month of the year (January), cohort 1, and male.

Tables A3.15 and A3.16 present the estimates shown in Table 3.5 and 3.6. In addition to the results shown in the main chapter, several differences are present. Net flow balance and end-of-month balance spike in month 5, likely due to workers getting their vacation payout. In month 12, flow balance and end-of-month balance decrease, likely due to expenses related to end-of-year holidays. and end-of-year payout in these months, respectively.

Dependent variable:	Net flow balance	End-of-month balance	In debt
Distance SRA	-9.40	718.04**	-0.001
	(7.22)	(278.83)	(0.001)
State pension * Distance SRA	40.22***	2224.53***	-0.000
	(7.98)	(406.90)	(0.001)
Occupational pension	16.33	890.13	-0.031***
	(25.07)	(1037.97)	(0.003)
Female	-23.17***	-1786.96	-0.003***
	(4.97)	(1161.06)	(0.001)
Household size	-37.68***	244.58	0.004***
	(4.13)	(1025.91)	(0.000)
Cohort 2	14.18***	985.93	-0.012***
	(5.08)	(1136.08)	(0.001)
Month = 2	68.21***	683.54***	-0.002**
	(13.43)	(202.43)	(0.001)
Month = 3	-65.77***	676.45***	0.000
	(12.56)	(246.42)	(0.001)
Month = 4	2.66	822.86***	0.001
	(12.99)	(248.16)	(0.001)
Month = 5	339.80***	1528.57***	-0.013***
	(13.56)	(268.82)	(0.001)
Month = 6	-63.90***	818.55***	-0.009***
	(13.95)	(248.76)	(0.001)
Month = 7	-181.06***	802.69***	-0.004***
	(13.05)	(268.96)	(0.001)
Month = 8	-49.53***	1002.80***	-0.001
	(13.39)	(261.89)	(0.001)
Month = 9	4.00	889.28***	-0.002**
	(12.75)	(255.14)	(0.001)
Month = 10	-103.59***	589.42**	0.002*
	(12.55)	(233.38)	(0.001)
Month = 11	16.78	406.88	0.001
	(12.95)	(248.24)	(0.001)
Month = 12	-99.38***	-797.68***	-0.001
	(15.70)	(287.05)	(0.001)
Constant	93.84***	29930***	0.058***
	(24.25)	(2518.08)	(0.003)
Observations	497271	497271	497271
Individuals	12125	12125	12125

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3.15: Full parameter list of the models estimated in Table 3.5. Reference categories are the first month of the year (January) and male. Baseline age equals 66 for cohort 1, and 66.33 for cohort 2.

Dependent variable:	Net flow balance	End-of-month balance	In debt
Occupational pension	26.20	1436.00	-0.031***
	(25.430)	(1053.85)	(0.004)
Cohort 2	9.19*	301.81	-0.011***
	(5.01)	(1139.03)	(0.002)
Age minus 66	7.23	1637.63***	-0.001
	(5.88)	(236.39)	(0.001)
Female	-23.43***	-1801.68	-0.003
	(4.98)	(1161.20)	(0.002)
Household size	-37.51***	253.57	0.004**
	(4.14)	(1025.89	(0.002)
Month = 2	67.96***	669.87***	-0.002*
	(13.43)	(201.97)	(0.001)
Month = 3	-65.74***	678.00***	-0.001
	(12.56)	(246.53)	(0.001)
Month = 4	3.64	877.24***	0.001
	(13.00)	(249.57)	(0.001)
Month = 5	339.47***	1510.56***	-0.013***
	(13.56)	(268.34)	(0.001)
Month = 6	-63.86***	820.68***	-0.009***
	(25.43)	(248.72)	(0.001)
Month = 7	-180.37**	813.35***	-0.004***
	(13.06)	(269.31)	(0.001)
Month = 8	-49.26***	1017.66***	-0.000
	(13.39)	(262.58)	(0.001)
Month = 9	4.29	905.63***	-0.002
	(12.75)	(255.85)	(0.001)
Month = 10	-103.20***	611.40***	0.002*
	(12.55)	(233.26)	(0.001)
Month = 11	17.34	438.09*	0.001
	(25.43)	(248.93)	(0.001)
Month = 12	-98.90***	-770.52***	0.001
	(15.70)	(288.55)	(0.001)
Constant	108.45***	30740***	0.058***
	(23.57)	(2513.49)	(0.003)
Observations	497271	497271	497271
Individuals	12125	12125	12125

Clustered standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A3.16: Full parameter list of the models estimated in Table 3.6. Reference categories are the first month of the year (January) and male. Baseline age equals 66.

A3.7 Means of dependent variables by subgroup

Table A3.17 presents the means of the dependent variables utilized in 3.7 and 3.8 for each subgroup. On average, net flow balances and end-of-month balances are positive for every subgroup, but they are higher for men, single-adult households, individuals who do not receive UI/DI benefits, white-collar individuals, and individuals with high inflows and wealth prior to retirement. Conversely, debt follows the opposite pattern.

Mean for:	Men	Women	Single-adult	Multi-adult	UI/DI	No UI/DI
Net flow balance	51.10	32.91	81.77	23.85	18.35	48.42
End-of-month Balance	33017.07	31209.03	31445.69	32479.22	23673.31	34084.44
In debt (binary)	0.035	0.031	0.037	0.032	0.053	0.029

Mean for:	White-collar	Blue-collar	High-inflow	Low-inflow	High-wealth	Low-wealth
Net flow balance	46.73	33.74	58.58	27.04	64.35	21.28
End-of-month Balance	34699.49	20590.07	43693.14	18262.30	57154.13	7133.48
In debt (binary)	0.030	0.041	0.023	0.044	0.001	0.066

Table A3.17: Means of dependent variables by subgroup.

A3.8 Cash flows and balances for UI/DI recipients

Figures A3.2 and A3.3 illustrate occupational pension recipiency on the basis of age, occupational pension inflow on the basis of distance from the statutory retirement age, and outcome measures on the basis of distance from the statutory retirement age and occupational pension recipiency, respectively. Pension recipiency is slightly higher prior to reaching the statutory retirement age, and the jump incurred by the statutory retirement age is larger than for the main sample. Occupational pension flows are lower than for the rest of the sample. Flow balances and total balances are higher for UI/DI recipients than for the rest of the sample. Finally, debt is higher among UI/DI recipients than for the rest of the sample.

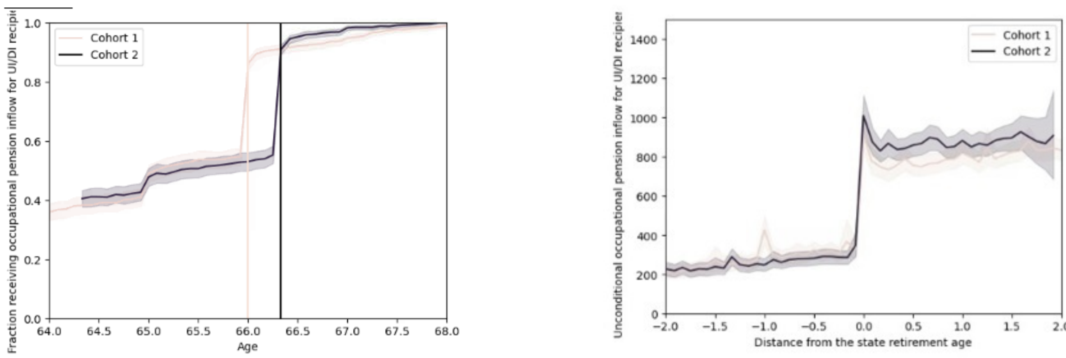


Figure A3.2: Pension recipiency and unconditional pension inflow for UI/DI recipients.

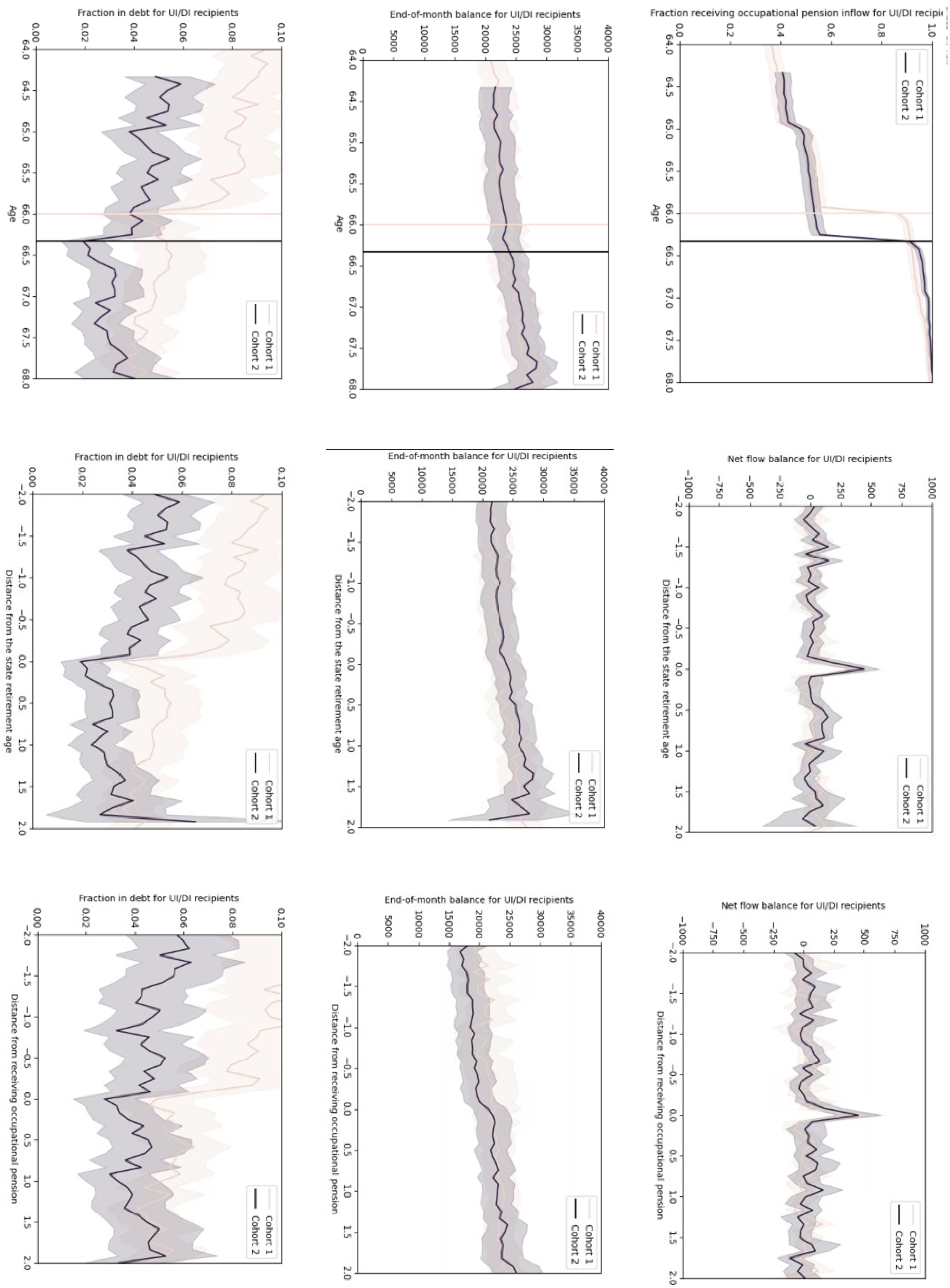


Figure A3.3: Outcome measures on the basis of age, distance from the statutory retirement age, and distance from occupational pension for UI/DI recipients.

Chapter 4

Child Penalties and the Gender Gap in Home Production and the Labor market

Abstract

The consequence of the arrival of children for the gender wage gap — known as the child penalty — is substantial and has been documented for many countries. Little is still known about the impact of having children beyond paid work in the labor market, such as home production. In this chapter we estimate — deploying an event study with Dutch survey data — the child penalty in both home production and the labor market. In line with the literature we find no labor market effects for men. For women, we observe a strong reduction in work hours and lower wages. However, we find an increase in home production for women roughly similar to the decline in paid work. Consequently, time allocated to the labor market plus home production is roughly equal across gender before and after the arrival of children. This result rejects the hypothesis that women substitute paid work for leisure after the arrival of children.

This chapter was co-authored by Max van Lent and Jim Been. We thank seminar and conference participants at Leiden University and the KVS new paper session 2023 for useful comments and suggestions.

4.1 Introduction

The last few decades have seen a remarkable convergence in the economic roles of men and women in society. Nevertheless, persistent gaps in the labor market remain: a gender gap has been reported in labor supply, earnings, and representation in top jobs (Cortés and Pan (2020)). The under-representation of women in the labor market and the existence of a gender gap is not only problematic for equity reasons, but it also entails welfare losses (Hsieh et al. (2019)). Traditionally, much of the gender gap is attributed to gender differences in education and labor market discrimination (Blau and Kahn (2017); Van Bavel et al. (2018)). More recent literature investigates alternative mechanisms underlying the gender pay gap. These mechanisms include differences in personality traits, preferences, and norms. See e.g. **?**, Bertrand et al. (2010), Blau and Kahn (2017), C. J. Flinn et al. (2018), and C. Flinn et al. (2021). And most recently, the arrival of children has been found a key catalyst for gender gaps in numerous countries [1]. Labor force participation rate penalties vary from 15% to 50% whereas earnings penalties range from 20% to 60% depending on the institutional setting.

The recent availability of longitudinal and administrative datasets enable the causal estimation of gender gaps through event studies. Existing literature (such as Kleven, Landais and Søgaard (2019)) provides causal evidence that the arrival of children increases the gender gaps in labor participation, earnings, and hourly wages. Two key mechanisms explain these gaps. First, gender gaps may be the result of occupational choice: mothers choose more flexible and lower paid jobs(Blau and Kahn (2017); Casarico and Lattanzio (2023)). Second, gender norms may explain child penalties: existing literature finds larger effects for mothers whose own mother did not work (Bedi et al. (2018); Kleven, Landais, Posch et al. (2019); Kleven (2022); Rabaté and Rellstab (2021); Rellstab (2023)).

The arrival of children increases the total amount of household work/activity e.g., taking care of children, cleaning the house, etc. Reduced paid work hours (due to either changes in occupation or

[1] See e.g., Andresen and Nix (2019), Cortés and Pan (2020), De Quinto et al. (2020), Kleven, Landais and Søgaard (2019), Kuziemko et al. (2018), Lundborg et al. (2017), Meurs and Pora (2019), Rabaté and Rellstab (2021), Sieppi and Pehkonen (2019).

gender norms) may therefore be a result of women taking up most of these additional household work, see Becker (1965). More recent empirical literature investigates the role of household time allocation in gender wage gaps Campaña et al. (2023); Cortés and Pan (2020); Erosa et al. (2022); Gimenez-Nadal and Alberto (2022). Household time allocation is found to be unequally divided across gender, with women on average performing less labor market activity being offset by women performing more household tasks than men. This literature, however, has not yet linked time use with childbirth.

Our chapter builds on the literature on the child penalty and gender gaps, in particular on the contributions by Kleven, Landais and Søgaard (2019) and Rabaté and Rellstab (2021). The aforementioned literature has exclusively focused on the formal labor market. However, there are large gender-based discrepancies in non-paid work at home (Aguiar et al. (2012); Bar and Leukhina (2011); Luengo-Prado and Sevilla (2013b); Sevilla-Sanz et al. (2010). As such, we add the role of childbirth to the time use literature. To our knowledge, we are the first to investigate how childbirth affects intra-household time allocation in the long run[2].

Our analysis bridges the gap between the literature on gender pay gaps and time use analyses by estimating how hours of work in the household change upon the arrival of children. Whereas existing literature on the one hand estimates the effect of childbirth on paid work and on the other hand shows an unequal distribution in household work, we provide some of the first long-run evidence on the role of children in the gender home production gap. To our knowledge, only one earlier paper estimates time use disparities as a result of childbirth (Kühhirt (2012)). Kühhirt (2012) use German survey data to estimate how the number of children affects time spent working and on home production. We add evidence from the Dutch institutional setting, a more recent sample time period, evidence on total household time allocation, and follow the methodology of Kleven, Landais and Søgaard (2019) to estimate the effects of childbirth. The Netherlands is particularly an interesting case since reduced paid working hours is institutionalized at employers.

[2]Short-run evidence exists (Aguilar-Gomez et al. (2019)), but only for the first year after childbirth. Additionally, the Dutch and Mexican institutional settings are not comparable.

Studying the impact of children on (the gender difference in) the time spent on work in the household is also important for policy-making. If women upon the arrival of children decrease their labor market activity in exchange for leisure, then financial incentives may increase female labor force participation, by an increased marginal utility of work as compared to leisure. However, if women exchange labor market hours only for work hours in the household, then the time constraint (from the inability to reduce time spent on household work) may be binding, and financial incentives will hardly increase labor market participation. Instead, policies that reduce the total amount of home production — such as increased access to child care — or reduces the share of home production that women take on (for instance via paternity leave) are then expected to be more successful.

The rest of this chapter is organized as follows. Section 2 discusses the Dutch institutional setting. Section 3 then describes our estimation strategy. Section 4 provides an overview of the data we use, followed by section 5 presenting the results of our analysis. Finally, section 6 concludes.

4.2 Institutional setting

This section describes Dutch institutional framework important for labor market decisions of parents and, particularly, how these differ from other countries and how this may affect the gender pay gap.

Part-time work is relatively common in the Netherlands, especially among women: Roughly 28% of all employed Dutch men and roughly 70% of all employed Dutch women work part-time (CBS (2021d)), as compared to the respective OECD averages of 10% and 25% (OECD (2023)). Additionally, roughly 76% of Dutch women are employed (OECD (2023)) as compared to the OECD average of 62% (OECD (2023)). Consequently, female labor market participation is high relative to other OECD countries, but primarily comprised of part-time work. It is worth noting that employers are legally obligated to offer part-time work options. As such, extensive margin child penalties are likely relatively small in the Netherlands.

The Netherlands has several schemes facilitating childcare by the parents. Options to reduce hours after the arrival of children exist for both men and women. The Netherlands has several flexible work arrangements and income support programs for parents, as Hartog and Salverda (2018) describe in an overview article up to 2016. In our sample, mothers and fathers are entitled to (unpaid) leave for up to 26 weeks after the birth of children (Plantenga and Remery (2009)). This leave can be taken up as is, but can also be taken up by parents working half of their usual hours for 52 weeks, allowing parents to combine taking care of their children with labor market activity. These support programs both make it relatively easy for parents to decrease their work hours and maintain their household income as a result of choosing to work less. As such, both men and women have many options to reduce their labor supply after childbirth relative to other OECD countries.

The Netherlands also have several formal childcare opportunities. Children enter primary school at the age of 4, before which they can (and often are) sent to daycare. Between the ages of 4 and 12, out-of-school childcare options are available. These childcare options are subsidized and means-tested: All parents are eligible for some degree of childcare subsidies, with effective subsidies being higher for low-income parents. Childcare opportunities make it relatively easy for mothers and fathers to maintain their labor market activity after having children, and are used for roughly 800,000 children on a yearly basis (Rijksoverheid (2021)). Additionally, informal childcare (i.e., by grandparents) is common in the Netherlands (Been et al. (2021))

Finally, the Netherlands has several measures of social insurances and protection in case of divorce or death of one's partner. In case of divorce, parents are legally obligated to make alimony payments on the basis of their income and the amount of time they spend taking care of their children [3]. This means low-earners are relatively well-insured against income losses and may increase intra-household specialization.

4.3 Methodology

We estimate how the arrival of children impacts the gender gaps in labor market participation, wages, and home production. We use

[3] Additionally, there are options to take up life insurance and remaining pensions are passed on to one's partner after death Rijksoverheid (2023).

a methodology similar to Kleven, Landais and Søgaard (2019). To measure home production, we investigate time use and the role of childbirth in time use. We measure the evolution of labor market outcomes and household allocation over time. To estimate the effects of childbirth, we perform an event study centered around the birth of one's first child. We define $t = 0$ as the year of birth of one's first child. We then separate the event by men and women. In this manner, we find out for men and women how outcomes diverge after birth of a first child.

Our estimation strategy requires several assumptions for causal inference. First, our event study assumes the exact timing of the arrival of children to be exogenous. Second, no confounding events are present. We observe neither observable anticipation effects before the arrival of children, nor effects on time use from placebo events[4]. This strengthens our belief that the two assumptions hold. The results of the F-tests are as follows:

Our bedrock specification closely follows that of Kleven, Landais and Søgaard (2019) and is as follows:

$$y^g_{ist} = \sum_j \alpha^g_j * I[j = t] + \sum_k \beta^g_k * I[k = age_{it}] + \sum_{e \neq -1} \gamma^g_e * I[e = s] + u^g_{ist}$$

Where i denotes the individual, s denotes event time, t denotes calendar time, g denotes gender, and X denotes a vector of control variables[5]. We estimate (1) with a set of controls consisting of education level dummies[6]. u is a residual term assumed to be normally distributed and uncorrelated with our regressors. Time, event time, and age are measured on a yearly basis. γ is the vector of interest for our analysis. We use the year prior to the arrival of children as our reference category. As such, γ measures effects relative to before childbirth.

[4]We test for the former through a joint F-test on the pre-childbirth coefficients by gender, the results of which can be found in table A4.1 in Appendix A4.1 We find no time use effects, and some labor market pre-trends that can be explained by career development. Additionally, we control for time-effects to limit confounding events.

[5]We obtain the same rough estimates without control variables.

[6]These controls are the same as in Kleven, Landais and Søgaard (2019).

4.4 Data

We use survey data from the Longitudinal Internet studies for the Social Sciences (LISS) panel for our analysis. The LISS panel is an online survey, administered by CenterData and Tilburg University, held among a representative sample of approximately 5,000 Dutch households. The LISS panel runs from 2007 to 2023 and covers a broad range of topics, including but not limited to work and schooling, family, and time use. LISS panel surveys are held monthly for demographic characteristics, and surveys asking for detailed information on topics such as work, schooling, and family are held yearly. The LISS panel is administered online among a representative group of individuals. Additionally, individuals without computers are provided one. As such, self-selection is precluded.

We supplement these data with the time use and consumption survey from the LISS panel. The time use and consumption survey is not part of the main panel, and as such is not observed every year in our sample. Instead, the time use and consumption surveys in our sample were held in 2009, 2011, 2013, 2016, 2018, 2019, 2020, and 2021. For a detailed overview of this data, we refer to Been and Goudswaard (2020).

Survey data has several key advantages over administrative data for our analysis. First, survey data allows us to measure outcomes that administrative data cannot, such as detailed time use measured as self-reported time spent on activities per week. Second, we can measure differences between actual time use and formal working hours.

In our analysis, we estimate child penalties with respect to the event time around childbirth. We estimate effects on labor market participation rates, monthly and hourly wages, total labor market activity, and home production. To isolate the effect of childbirth, we restrict our sample with respect to individuals having children. As such, we estimate our event solely for individuals we observe in the yearly LISS data the year before, the year of, and the year after childbirth[7]. Note that restricting our sample in this manner creates

[7]Restricting to the core sample implies that we do not necessarily observe time use in all of these years. Appendix A4.2 provides summary statistics and results that impute time use as the mean time use in the year before and after if time use is missing, potentially alleviating this concern.

additional unbalancedness in the panel: the resulting sample has a relatively large number of observations close to the birth of children, and a relatively small number of observations as the number of post-childbirth years increases. This implies that we sacrifice precision in our long-run estimates through this sample restriction.

	Male			Female			Diff	Joint diff
	Mean	SD	Obs	Mean	SD	Obs	P-value	P-value
Working	0.95	0.22	2018	0.85	0.36	2500	0.00***	
(Conditional) Monthly gross wage	3211.70	1761.19	1253	2624.90	1782.87	890	0.00***	
(Unconditional) Monthly earnings	3202.28	1522.74	1876	2121.83	1315.01	2343	0.00***	
Hours worked	33.64	17.89	1062	21.36	16.37	999	0.00***	
Hours working+commuting	37.24	20.27	559	23.80	19.05	641	0.00***	
Hours children	2.37	7.87	1708	7.35	19.01	1270	0.00***	
Hours chores+children	16.66	15.65	383	28.68	27.81	412	0.00***	
Hours total household	54.83	23.15	364	53.54	25.99	404	0.18	
Age respondent	31.95	6.41	2007	30.59	4.81	1840	0.00***	
Highest education: Primary School	0.02	0.15	2018	0.01	0.10	2493	0.00***	
Highest education: Junior High School	0.07	0.25	2018	0.06	0.23	2493	0.00***	
Highest education: Senior High School	0.06	0.23	2018	0.06	0.25	2493	0.00***	0.00***
Highest education: Community College	0.28	0.45	2018	0.27	0.44	2493	0.00***	
Highest education: College	0.36	0.48	2018	0.36	0.48	2493	0.74	
Highest education: University	0.22	0.41	2018	0.25	0.43	2493	0.00***	
Married	0.35	0.48	2976	0.48	0.50	2500	0.00***	
Number of children	1.32	0.52	348	1.35	0.49	382	0.48	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.1: Summary statistics by gender

Table 4.1 shows summary statistics for the variables used in the analysis. Our sample shows substantial gender differences. Men have a slightly higher participation rate, higher monthly gross wage and work more hours than women on average, whereas women perform more home production. Men are on average roughly a year older than the women in our sample. Women have slightly higher education levels, especially with respect to university degrees. Women are married more often than men. The majority of observations are unmarried, as these summary statistics include the four years prior to childbirth. Finally, most individuals in our sample have 1 or 2 children.

	Male			Female			Diff	Joint diff
	Mean	SD	Obs	Mean	SD	Obs	P-value	P-value
Working	0.96	0.20	212	0.89	0.31	266	0.01**	
(Conditional) Monthly gross wage	3092.78	1913.16	148	2899.26	2720.06	131	0.49	
(Unconditional) Monthly earnings	3060.95	1480.78	196	2327.85	1196.91	254	0.00***	
Hours worked	34.22	18.10	147	26.71	15.95	139	0.00***	
Hours working+commuting	39.07	20.94	79	30.04	18.06	96	0.00***	
Hours children	0.05	1.05	364	0.16	1.91	266	0.39	
Hours chores+children	9.40	8.54	66	9.16	6.88	73	0.85	
Hours total household	49.59	21.05	61	40.16	17.52	72	0.01***	
Age respondent	30.29	6.54	229	29.07	4.43	219	0.02***	
Highest education: Primary School	0.01	0.12	212	0.00	0.06	265	0.22	
Highest education: Junior High School	0.07	0.25	212	0.05	0.22	265	0.54	
Highest education: Senior High School	0.07	0.25	212	0.07	0.25	265	0.93	
Highest education: Community College	0.29	0.45	212	0.25	0.43	265	0.34	0.63
Highest education: College	0.32	0.47	212	0.34	0.47	265	0.73	
Highest education: University	0.25	0.43	212	0.29	0.45	265	0.27	
Married	0.25	0.43	364	0.36	0.48	266	0.00***	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.2: Balancing statistics of labor market outcomes and demographic characteristics in the year prior to childbirth.

Table 4.2 presents balancing statistics in the year prior to childbirth. Women participate on the labor market less than men even before childbirth and work fewer hours, and have fewer earnings. As compared to after childbirth, however, these differences are comparatively small. Part of the differences can likely be explained by age differences between men and women. These statistics overall indicate level differences in outcomes between men and women even prior to childbirth, but do not necessarily imply pre-trends.

We estimate event study models for hourly wages, monthly earnings, hours worked, commute time, hours spent on chores and children, and the sum of time spent on labor market and household activities. Figures 1–3 describe the outcome variables by event time (where the event is birth of the first child) and gender. As both our summary statistics and outcome variables are based on parents who have children at some point in the sample, all the plots over the event time have the same number of observations as in the summary statistics.

Figure 4.1: Means and confidence intervals of hourly and monthly earnings over the event time.

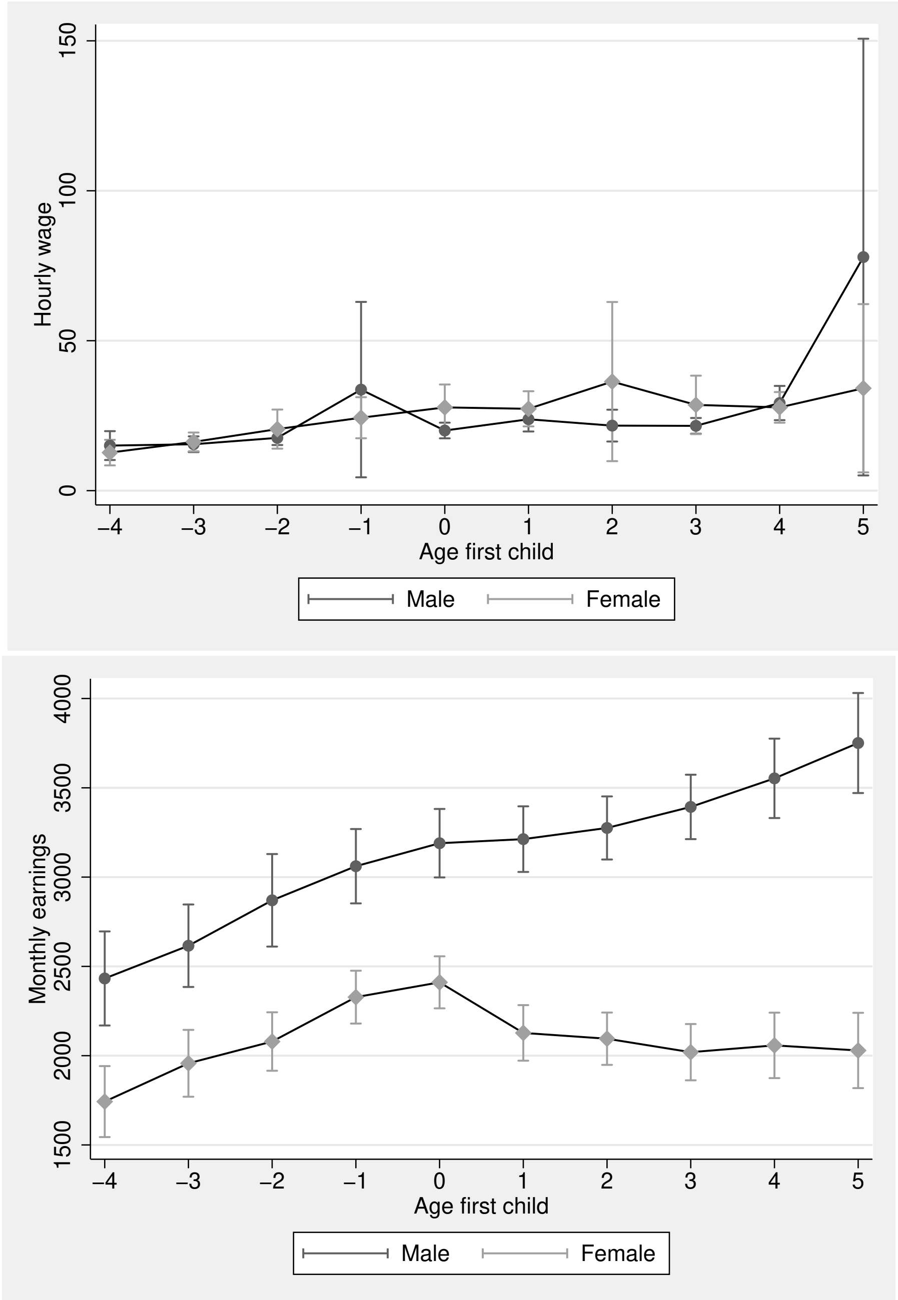


Figure 4.1 shows how childbirth, on average, affects earnings for men and women over (event) time. Men experience a steady income growth that does not seem to be affected by childbirth. On the other hand, women experience an income shock relative to men that they do not recover from. We see that nearly the entire drop of earnings for women in the first 5 years after childbirth come from the number of hours worked, not earnings per hour.

Figure 4.2: Means and confidence intervals of hours worked and commuted over the event time.

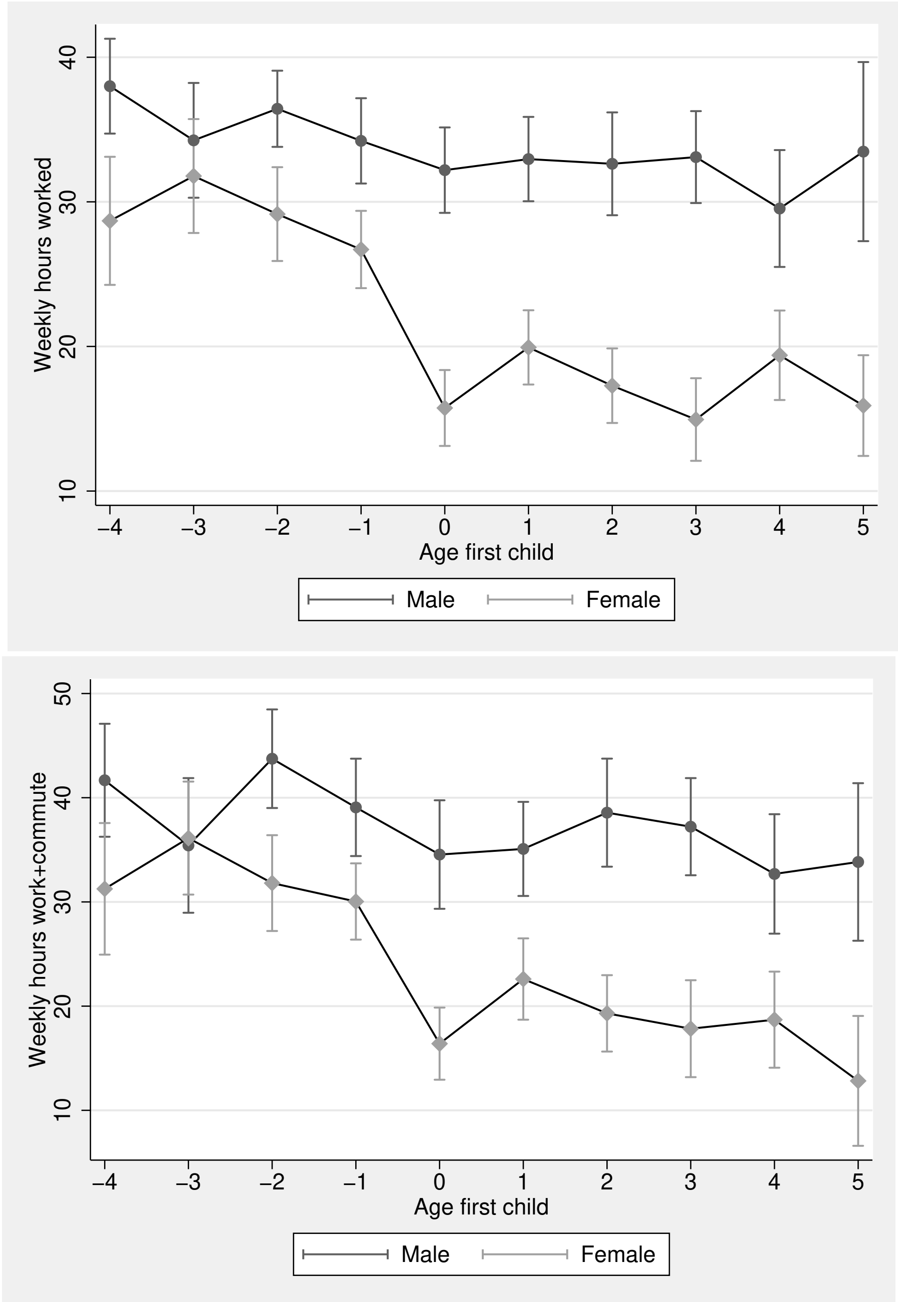


Figure 4.2 shows that childbirth affects time spent on labor market activity differently for women as compared to men. Men keep both their work and commute hours roughly constant over the years. Whereas women experience a sharp decline in labor market activity after childbirth, from approximately 30 work hours to 15–20 hours a week. We also observe differences in hours worked prior to childbirth, though these differences are comparatively small.

Figure 4.3: Means and confidence intervals of time spent on home production and the sum of labor market activity and home production over the event time.

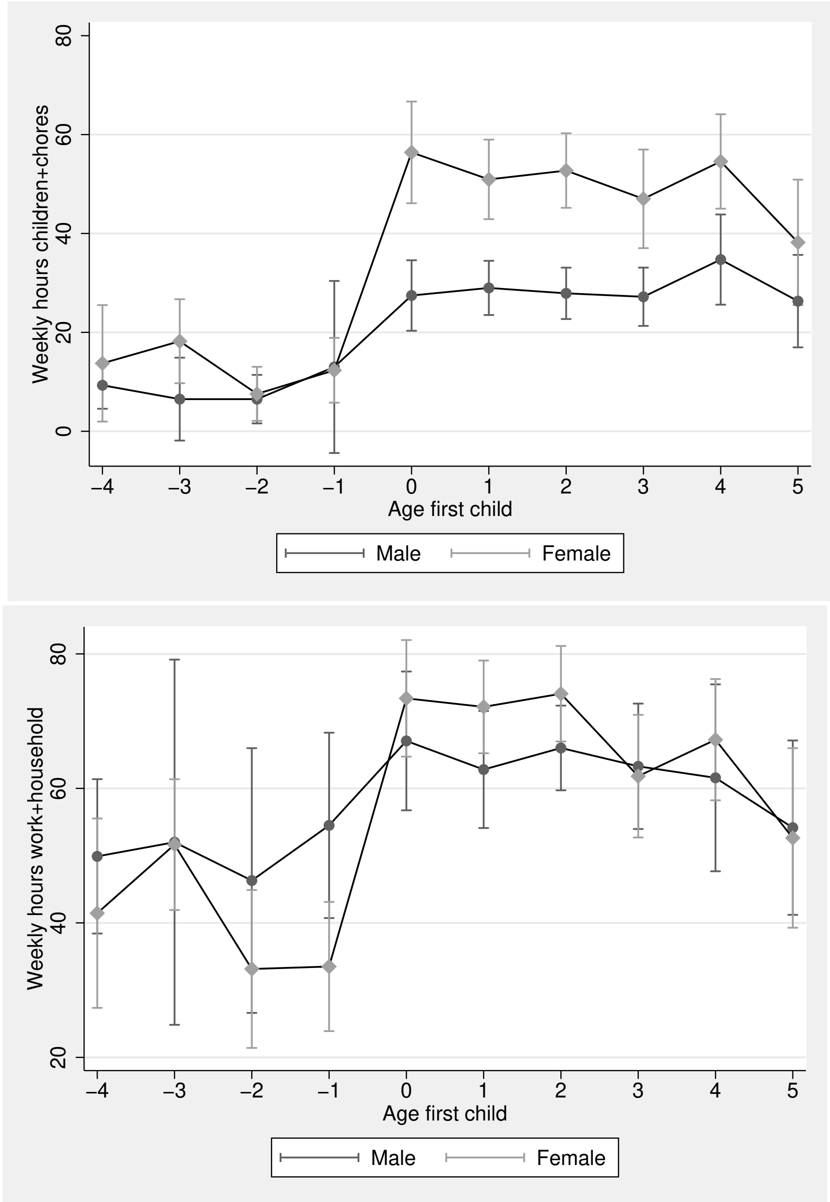
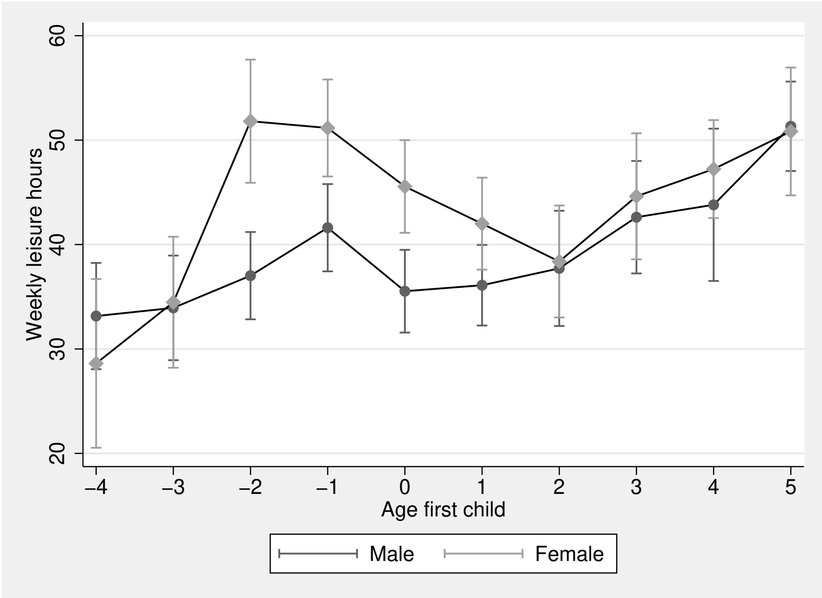


Figure 4.4: Means and confidence intervals of time spent on leisure over the event time.



Figures 4.3 and 4.4 show how childbirth affects time spent taking care of children and chores, the sum of labor market and home production, and leisure. Before the birth of children we see no difference in the amount of time spent on home production. After the birth of children both men and women both increase their home production. However, the increase is much more pronounced for women. Whereas men spend approximately 30 hours a week on home production after childbirth, women spend approximately 50 hours. No discernible differences between men and women are found when aggregating labor market activity and home production, measured in the sum of time spent working, commuting, taking care of children, and doing chores. Total activity is roughly similar between men and women, though activity does increase for women after childbirth. That is to say, the decrease in female labor market activity after childbirth is spent on household work instead of leisure.

Overall, we observe an increase in the gender gap in wage, labor force participation, and work hours after the birth of one's first child, with differences being primarily driven by a decrease in female labor market activity. These statistical patterns are similar to those observed in Kleven, Landais and Søgaard (2019) and Rabaté and Rellstab (2021). However, we observe the opposite effect when we look at time use, finding that women increase their home production more than men and after childbirth. Moreover, time spent on household chores roughly compensates for divergence in labor market activity.

4.5 Results

The previous section described the data and showed mean differences between men and women in their labor market activity and time spent on home production. However, men and women differ also in observable characteristics such as age and education level. We therefore estimate the effect of children using the framework laid out in the methodology, specifically equation (4.1)[8].

We use participation rates, wages, hours worked and commuted, hours spent on chores and children, and hours spent on total home production as dependent variables. We then plot the coefficients estimated based on equation (4.1) and their confidence bounds by event time for men and women separately. Figures 4–6 graphically show the event time coefficients. As our time use data have gaps, we re-estimate our time use models by imputing the mean time use values in the years before and after the gaps for the missing years. The results of estimates on the basis of imputed data can be found in Appendix A4.2.

[8]Due to the small number of observations, including individual-fixed effects does not leave enough variation in our estimates.

4.5.1 Labor market outcomes

Figure 4.5: Participation rate coefficients by gender. Based on 3350 observations, and 746 individuals.

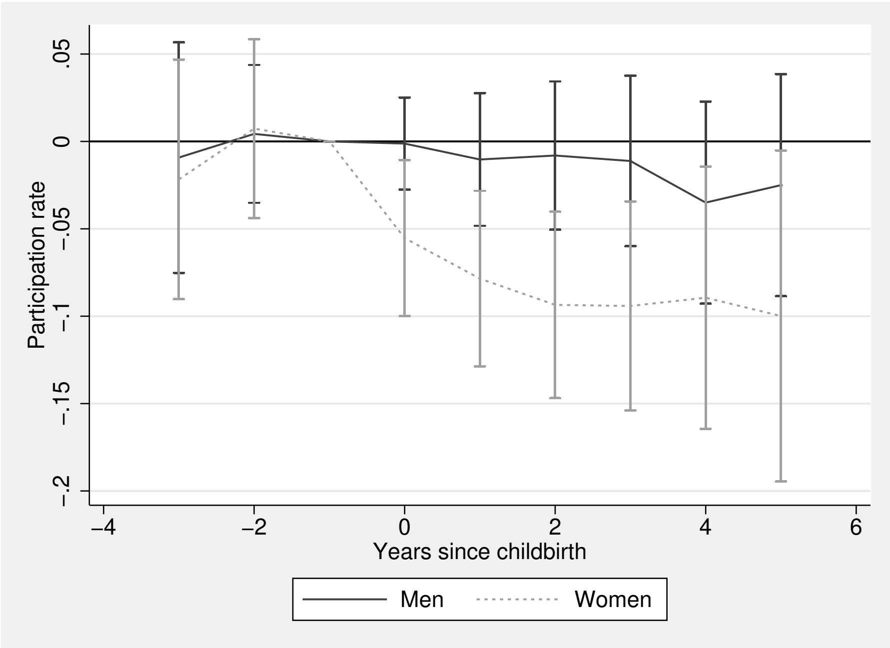


Figure 4.6: Monthly earnings coefficients by gender. Based on 3131 observations, and 712 individuals.

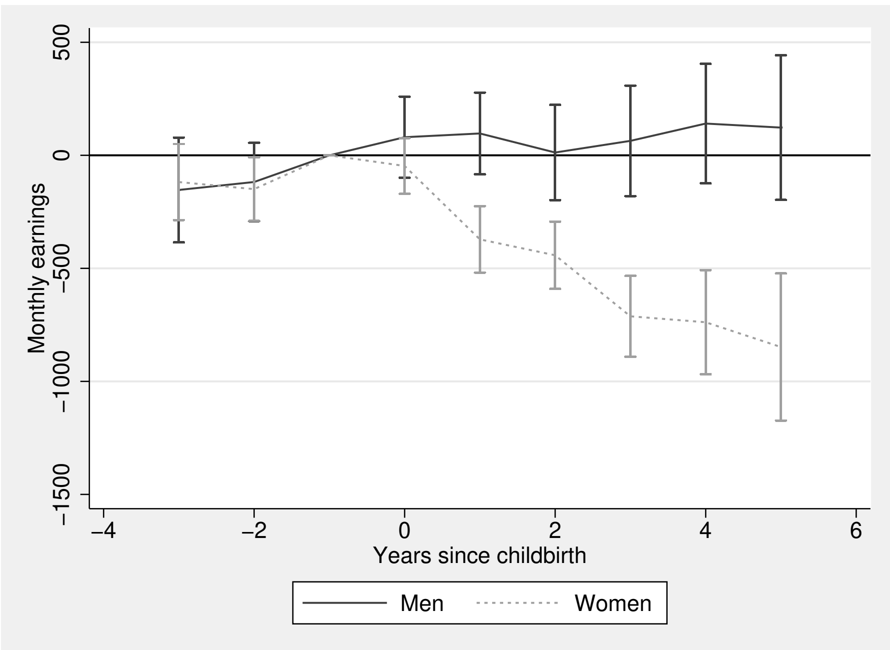
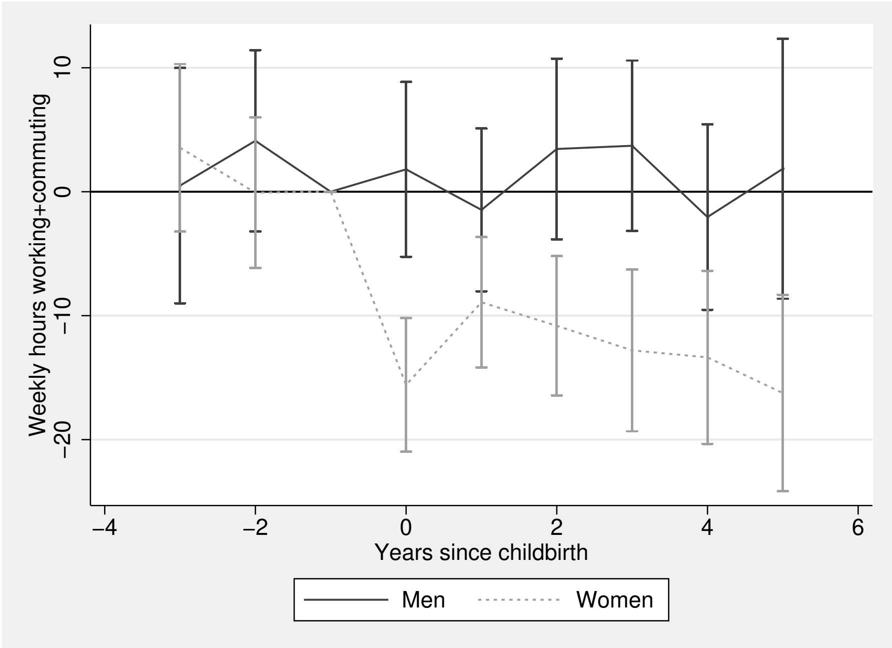


Figure 4.7: Weekly hours working and commuting coefficients by gender. Based on 1003 observations, and 564 individuals.



Figures 4.5, 4.6, and 4.7[9] show labor market effects and penalties as a result of childbirth. Men do not face labor market penalties as a result of childbirth, whereas women exhibit a strong negative effect on hours worked. We additionally observe some monthly wage penalties for women.

We find several labor market penalties for women. First, participation rates drop by 7 percentage points at childbirth, increasing to 10 percentage points in the longer run. However, these effects are not statistically different from those on men. Second, women experience a decrease in earnings of roughly €800 in the long run. Finally, the observed decrease in earning for working women is primarily driven by a decrease in hours spent on the labor market. As such, child penalties are driven by decreases in labor market activity both at the extensive and the intensive margin[10].

Overall, the decreases in earnings and labor market activity are also present in Rabaté and Rellstab (2021) and roughly similar in the long run, though our estimates are less precise.

[9] Figures A4.11, A4.12, A4.13 and A4.14 in Appendix 3 show supplementary estimates for hourly wages, log monthly wages, level monthly wages, and weekly hours worked, respectively.

[10] We find similar results when we restrict our estimates to the years for which we observe time use and consumption

4.5.2 Household time use

We extend existing child penalty estimates by also including time use in the household as an outcome measure. As is conventional in the literature, we measure effects relative to men one year before childbirth. Results for household time allocation are as follows:

Figure 4.8: Weekly hours spent on children and chores coefficients by gender. Based on 331 observations, and 255 individuals.

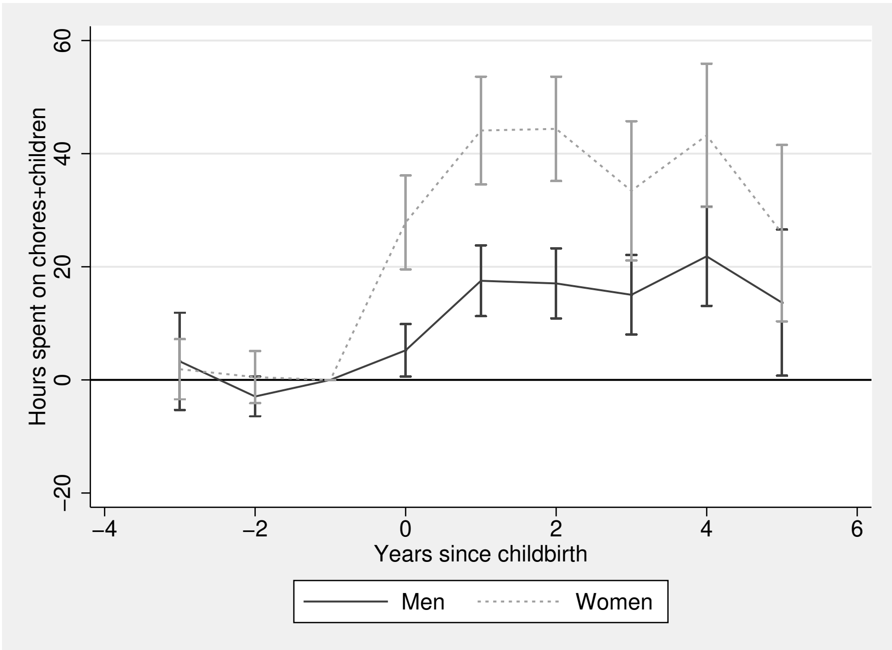
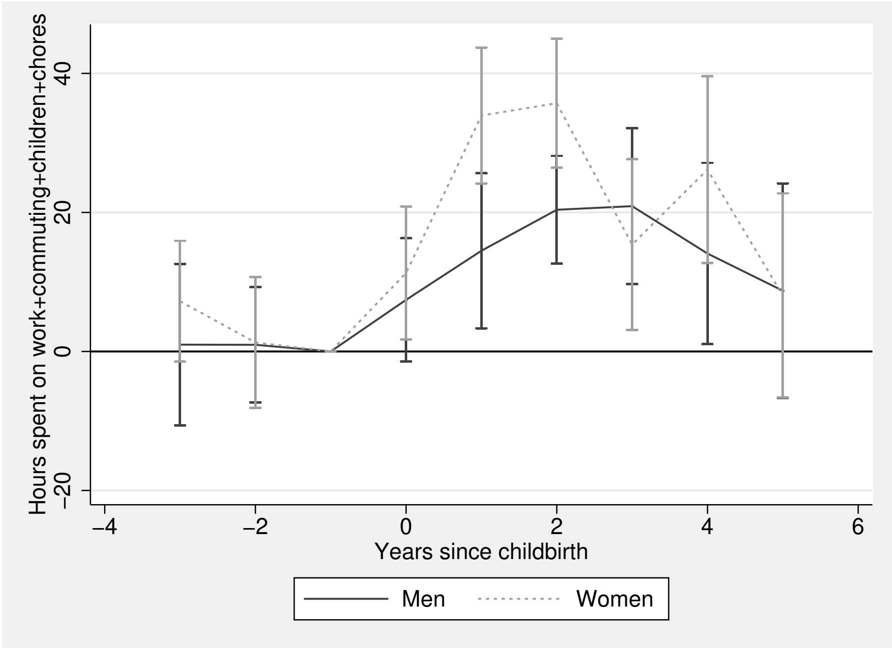


Figure 4.9: Weekly hours spent on working, commuting, children, and chores by gender. Based on 329 observations, and 255 individuals.



Figures 4.8, and 4.9[11] show how household tasks and total time use respond to childbirth. Both men and women increase their home production after childbirth, albeit women do so much more strongly than men: Whereas men increase their home production by approximately 10 to 20 hours a week after childbirth, women increase their home production by 40 hours a week. In the long-run, point estimates for home production remain higher for women than for men, though the differences are no longer statistically significant. The sum of labor market activity and household activity, meanwhile, is approximately equal between men and women. These results indicate substitution between labor market activity and household activity rather than a decrease in total activity.

[11]Figure A4.15 in Appendix A4.3 shows supplementary estimates for weekly hours spent on children.

Figure 4.10: Weekly hours spent on leisure by event time and gender. Based on 1093 observations, and 593 individuals.

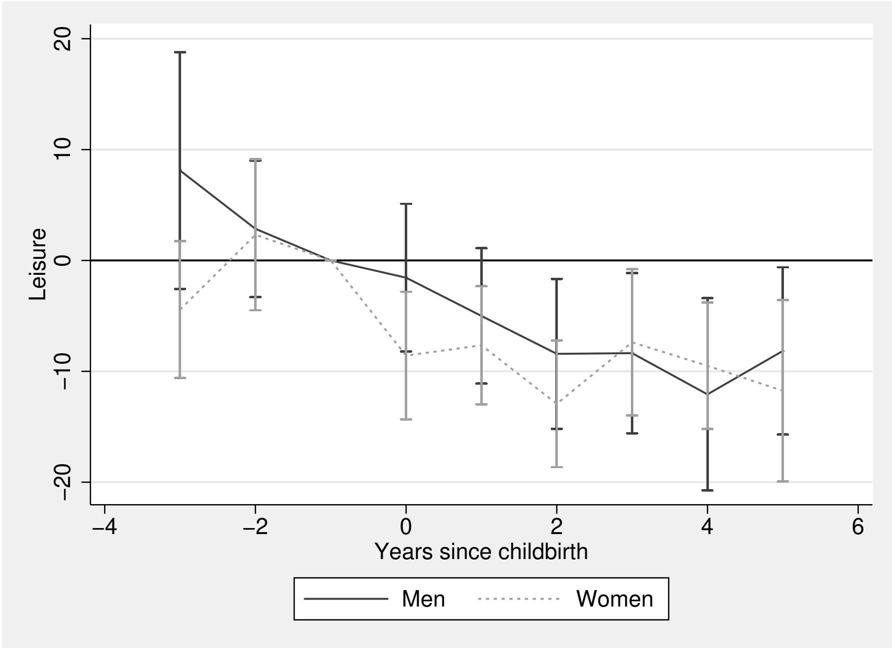


Figure 4.10 shows weekly hours spent on leisure. In line with total household activity increasing after childbirth, leisure hours decrease after childbirth. The decrease in leisure hours starts out relatively small, but accumulates to a decrease of 10 leisure hours by week. This decrease, however, is approximately the same for men as for women. Women therefore do not experience 'leisure advantages' as compared to men.

Outcome measure	Short-run penalty	Long-run penalty	Kleven et al. (2023) equivalent
Participation rate	-6.8%**	-7.5%	9%
	(3.2%)	(5.8%)	
Monthly (unconditional) earnings	€-468***	€-971***	34%
	(€119)	€(232)	
Monthly (conditional) wage	€-413	€-1492***	38%
	(€336)	(€488)	
Weekly hours working	-8.9***	-16.5***	52%
	(3.1)	(5.4)	
Weekly hours working and commuting	-7.5*	-18.1***	61%
	(4.3)	(6.7)	
Weekly hours taking care of children	14.0***	9.3	N/A
	(3.9)	(6.9)	
Weekly hours taking care of children and doing chores	26.6***	12.3	-25%
	(5.8)	(10.3)	
Weekly hours total household activity	19.4***	-0.7	-9%
	(4.6)	(10.8)	
Weekly hours total leisure	-2.6	-3.6	7%
	(4.1)	(5.7)	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.3: Baseline effects for women and the corresponding Kleven penalty by outcome measure relative to one year before the birth of one's first child. Short-run penalties are measured the year after childbirth (t=1). Long-run penalties are measured 5 years after the birth of one's first child (t=5). Short-run and long-run penalties measure effects for women relative to the year before childbirth, whereas the Kleven equivalent is defined in the same manner as in Kleven et al. (2023), and measure penalties for women relative to men. Penalties are measured by dividing the post-childbirth coefficients for men by the counterfactual outcome measure absent childbirth for women, then subtracting the post-childbirth coefficients for men divided by the mean counterfactual result for women.

Table 4.3 shows child penalties for women compared to men relative to before childbirth both immediately after childbirth and 5 years after childbirth, as well as their relative order of magnitude[12]. Women decrease participation by 9.9 percentage points immediately after childbirth, accumulating to 15.2 percentage points in the long run compared to men. Monthly earnings drop by approximately €1000 in the long run, although this is driven by a decrease in labor market participation on both the intensive and the extensive margin. Contrariwise, home production for women increases by 47 hours a week in the short run, declining to an increase of 24 hours in the long run. As a result, women spend more hours on total activity as compared to men immediately after childbirth, though this effect dissipates in the long run.[13]

[12]We cannot compute a relative penalty for time spent on children as there is no pre-childbirth time use to scale by.

[13]Note that all our findings are not affected by the Covid-19 pandemic: Restricting our sample

As compared to Rabaté and Rellstab (2021), we find very similar estimates, with at most a several percentage point difference in the point estimate. Adding to Rabaté and Rellstab (2021), household time allocation explains a substantial amount of the labor market divergence found in the literature thus far: Women have negative home production penalties, which both in relative and absolute terms can fully explain the decrease in female labor market activity as a result of childbirth.

All in all, we show that women suffer substantial child penalties on the labor market, though these penalties manifest due to women reducing their labor market activity. This decrease in labor market, however, is compensated for by an increase in home production, which increases much more sharply than for men.

4.6 Conclusion

In this chapter, we study the impact of the arrival of children on both gender gaps in the labor market and gaps in work in the household (home production). We find substantial gender gaps in the labor market that are exacerbated by the onset of children. These results are roughly in line with the existing gender gap literature, see e.g., Kleven et al. (2023) who show gender employment gaps throughout the world and find that child penalties are the main explanation in developed countries, like the Netherlands. Of particular note is that gender gaps are still present and substantial in the unique institutional setting of the Netherlands, with the Netherlands having relatively high freedom to reduce working hours and options to return to work after childbirth.

We find that gender gaps in labor market outcomes are bridged by time use in the household. We discover that women spend more time on household work and childcare, and that this difference (as compared to men) increases after childbirth. This finding also leads to total time use — i.e., the sum of labor market work and work in the household — being roughly equal between genders. This result suggests that in order to close the gender gap that is caused by the arrival of children, policy is needed that reduces the amount of work in the household that is done by women (both absolutely and relatively to

to end at 2019 yields very similar estimates.

men). Therefore, labor market policies alone are likely not successful in fully closing the gender labor market gaps. As such, policies that additionally target home production such as paternity leave (Kleven, Landais, Posch et al. (2019)) and childcare (Andresen and Nix (2019)) may play a role in reducing the gender pay gap.

A potential explanation for our findings is that women switch to more flexible jobs after the arrival of children. This explanation falls in line with existing literature: Norms are a potential explanation of gendered division of tasks after having children. As such, shifts in mothers' labor participation may be driven by household bargaining.

Although our data are unique in that they measure both labor market outcomes and time use in the household, they are limited by a lack of observations and individuals. First, few individuals in the sample have children within the data. Second, our time use data have few observations, to the point where we only observe most individuals once. Finally, our data lack detail in the type of time use. For future research, surveys specifically tailored to parents may be of interest.

A4 Appendices

A4.1 F-tests of pre-childbirth coefficients

Table A4.1 tests for pre-trends by showing joint F-tests of all pre-childbirth coefficients, by gender, for each of the outcome measures used. For men, we find some evidence of pre-trends for labor market participation, wages, and earnings. For women, we only observe pre-trends with respect to wages. These results indicate that while pre-trends are for the most part absent, some caution is warranted with respect to labor market penalties.

Table A4.1: P-values of F tests with respect to pre-childbirth coefficients

Group	Men	Women
Outcome measure	P-value	
Participation rate	0.04	0.66
Log wage	0.03	0.01
Monthly earnings	0.01	0.16
Hours worked	0.59	0.39
Hours work/commute	0.77	0.44
Hours children	0.46	0.77
Hours chores/children	0.24	0.62
Hours work/household	0.10	0.69

A4.2 Time use with imputed data

Figure A4.1: Weekly hours spent working by event time and gender with imputed time use.

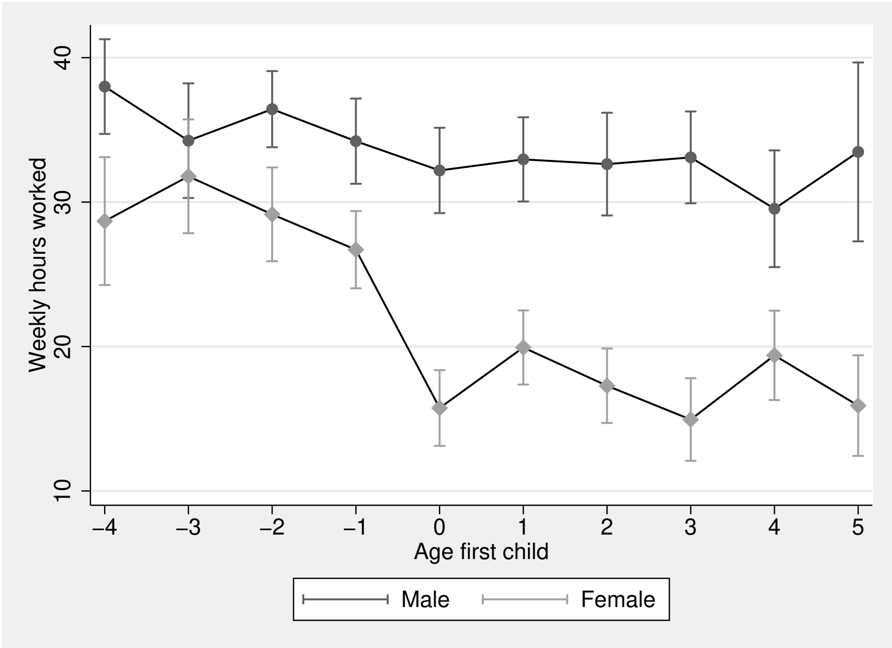


Figure A4.2: Weekly hours spent working and commuting by event time and gender with imputed time use.

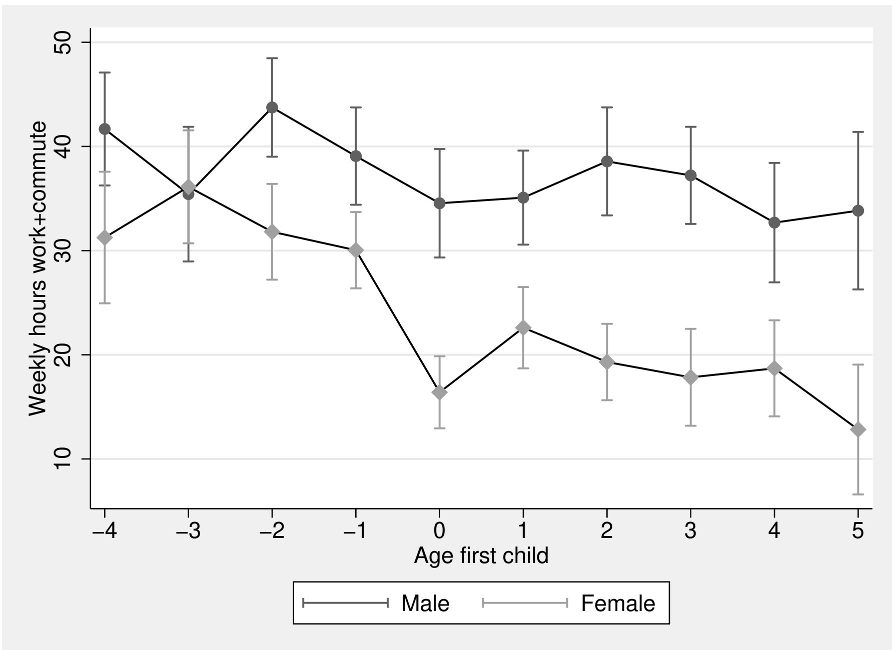


Figure A4.3: Weekly hours spent on children by event time and gender with imputed time use.

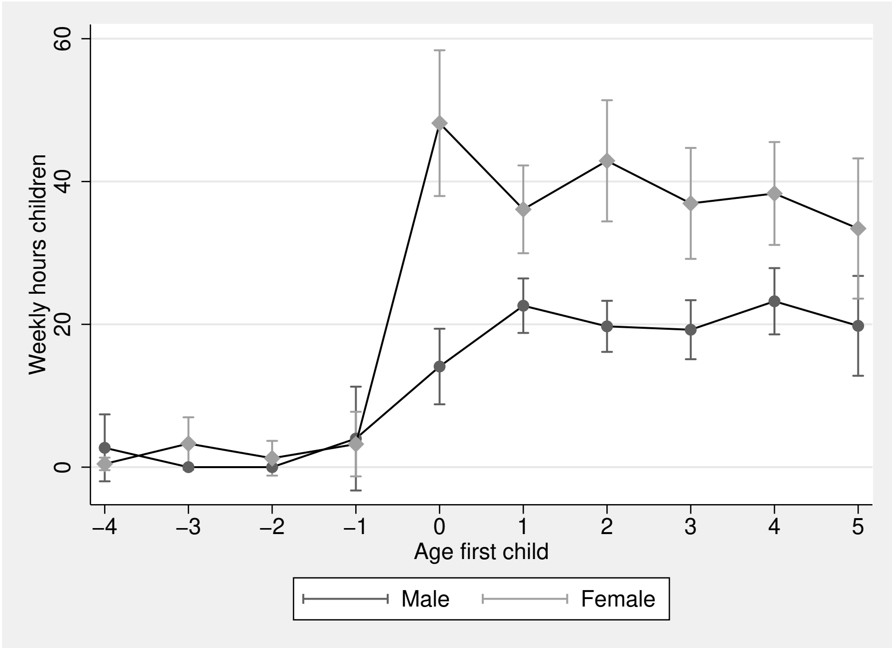


Figure A4.4: Weekly hours spent on chore and children by event time and gender with imputed time use.

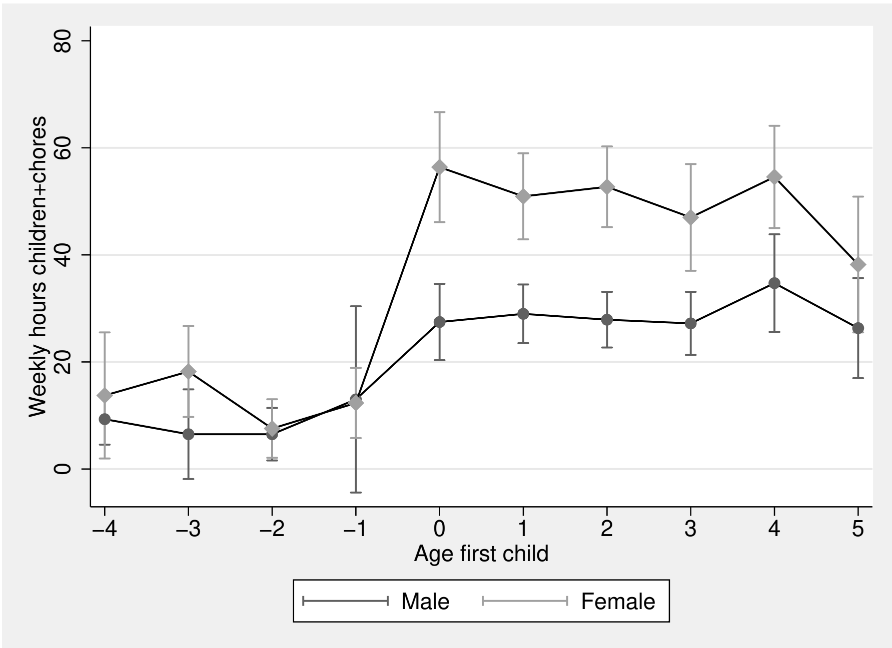


Figure A4.5: Weekly hours spent on total household activity by event time and gender with imputed time use.

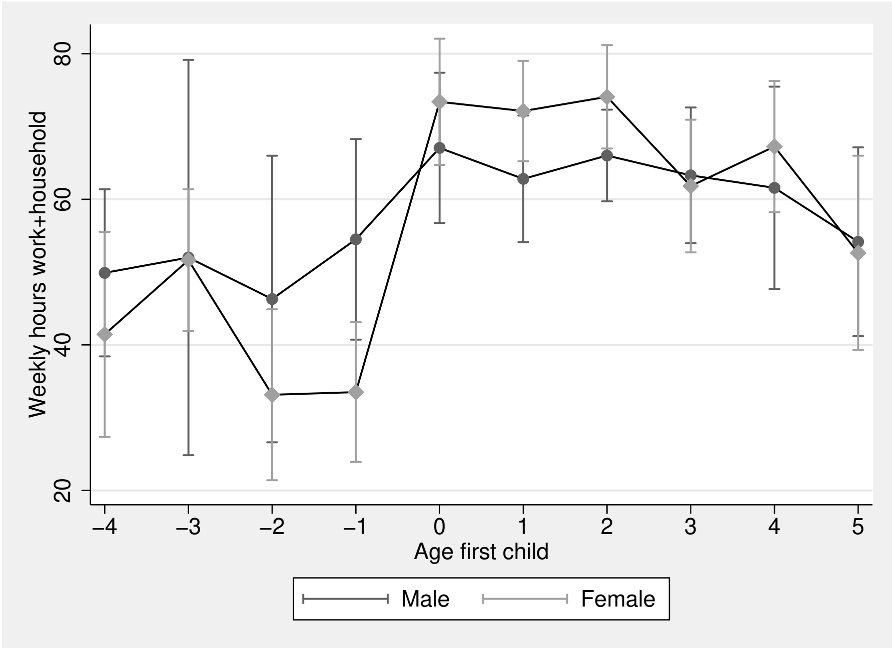


Figure A4.6: Effects of childbirth on hours spent working by event time and gender with imputed time use. Based on 1948 observations, and 666 individuals.

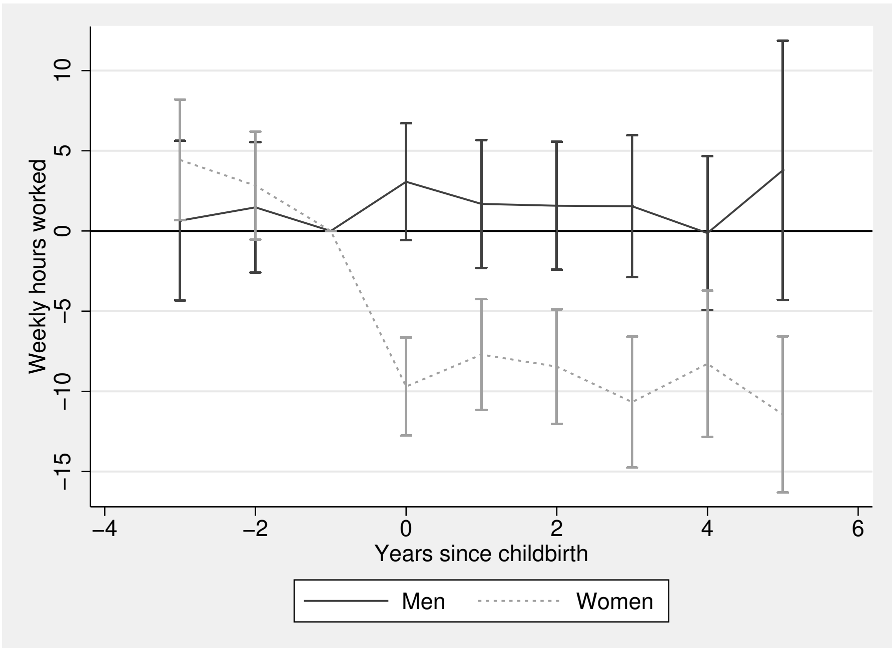


Figure A4.7: Effects of childbirth on hours spent working and commuting by event time and gender with imputed time use. Based on 1248 observations, and 565 individuals

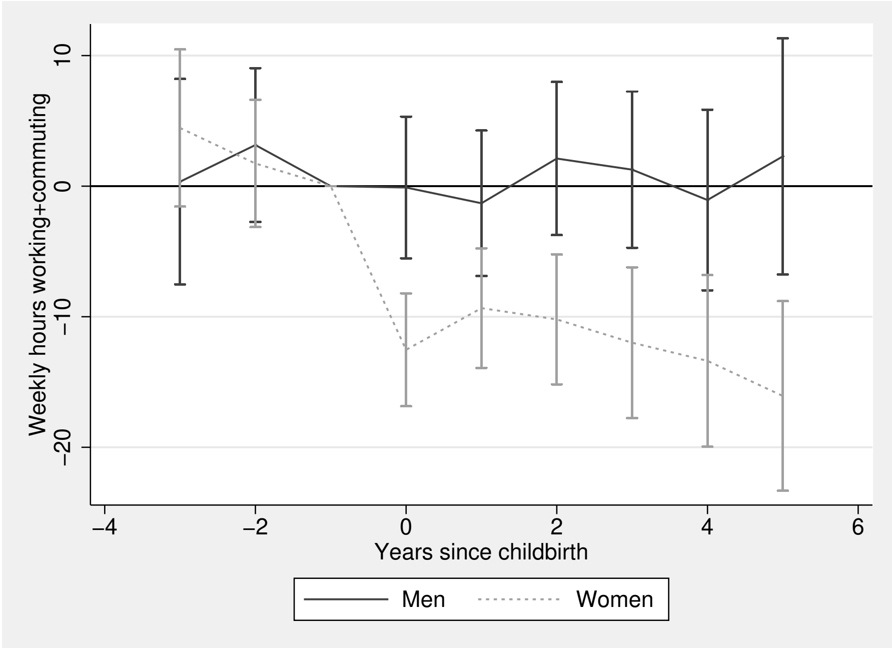


Figure A4.8: Effects of childbirth on hours spent on children by event time and gender with imputed time use. Based on 509 observations, and 312 individuals.

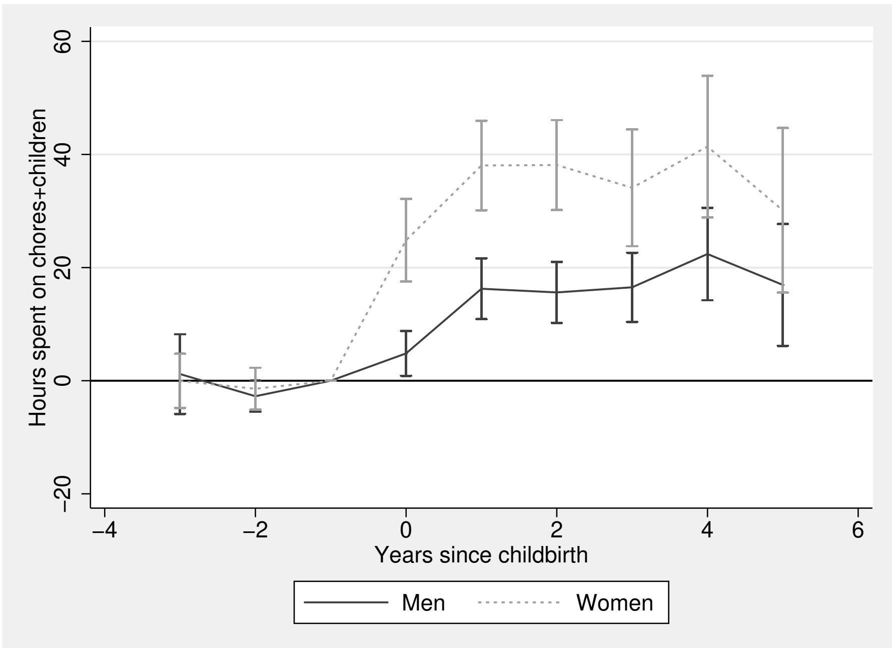


Figure A4.9: Effects of childbirth on hours spent on chores and children by event time and gender with imputed time use. Based on 387 observations, and 256 individuals.

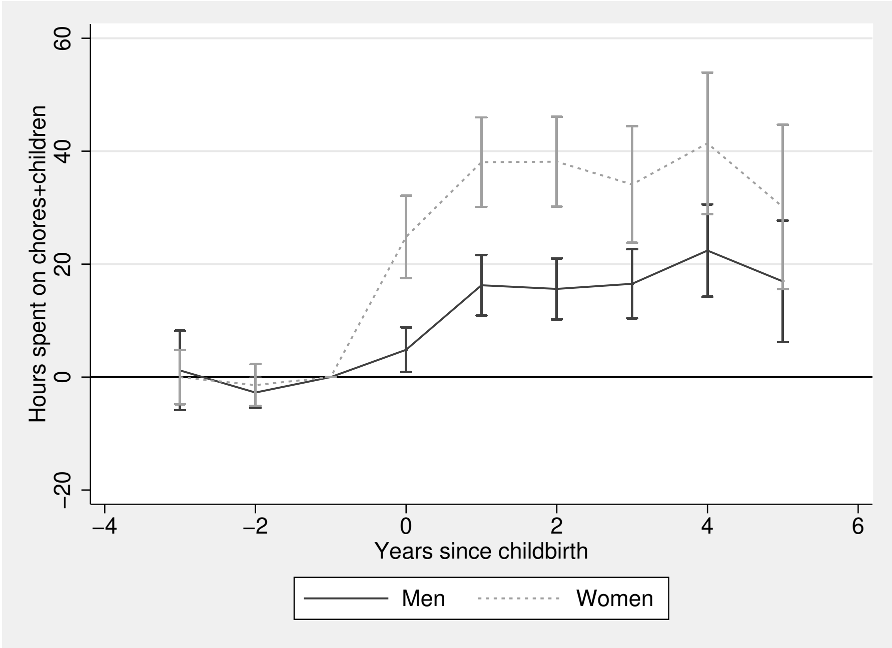
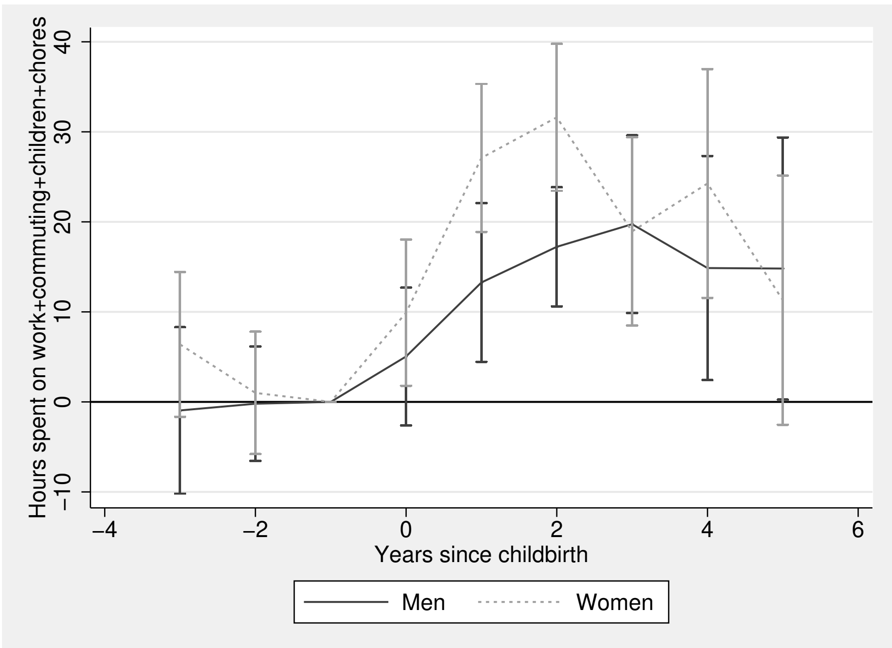


Figure A4.10: Effects of childbirth on hours spent on total household activity by event time and gender with imputed time use. Based on 385 observations, and 256 individuals.



A4.3 Additional outcome measures

Figure A4.11: Hourly wage estimates by event time and gender. Based on 747 observations, and 404 individuals.

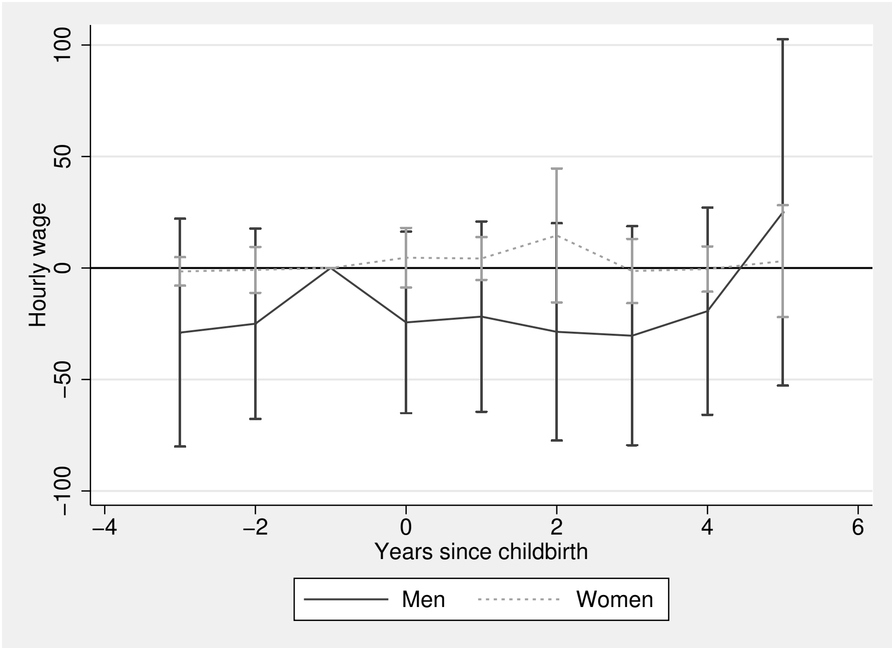


Figure A4.12: Log monthly wage estimates by event time and gender. Based on 1811 observations, and 565 individuals.

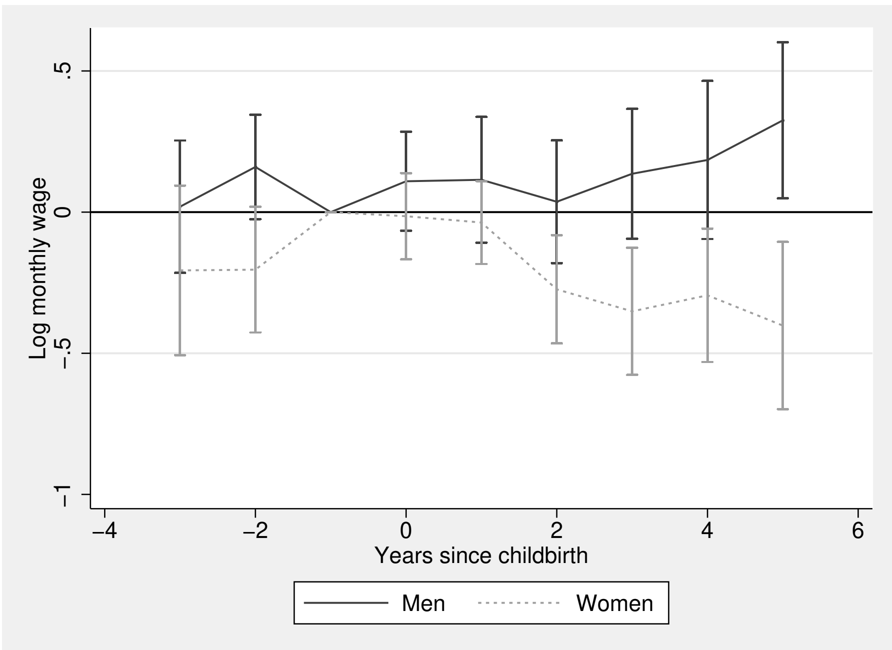


Figure A4.13: Level monthly wage estimates by event time and gender. Based on 1811 observations, and 565 individuals.

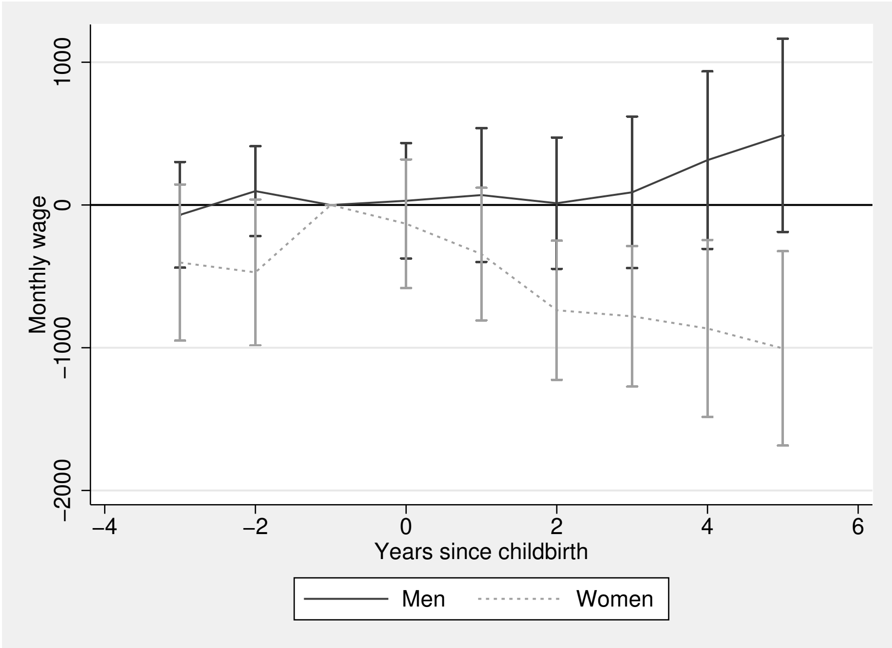


Figure A4.14: Weekly hours worked estimates by event time and gender. Based on 1573 observations, and 665 individuals.

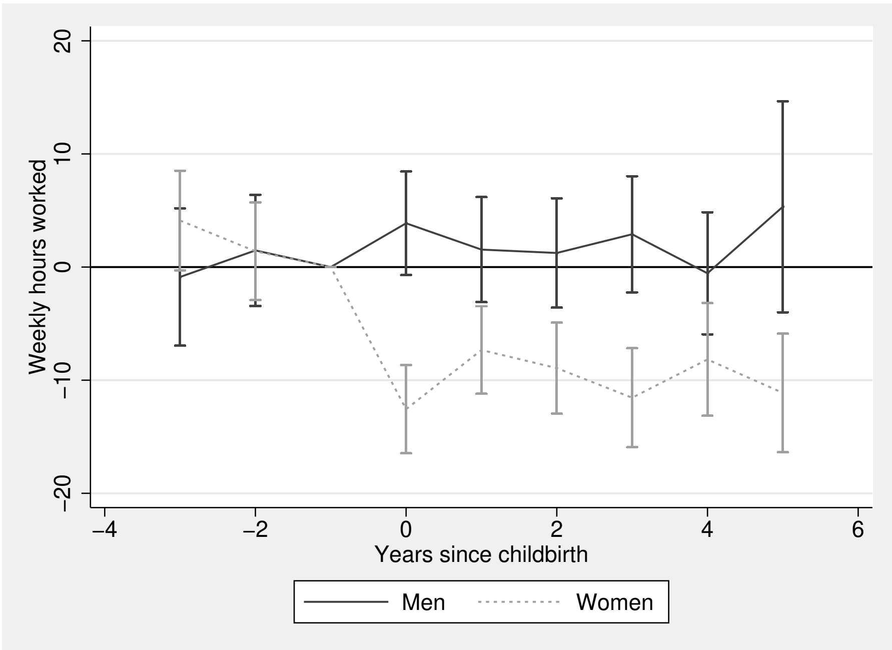
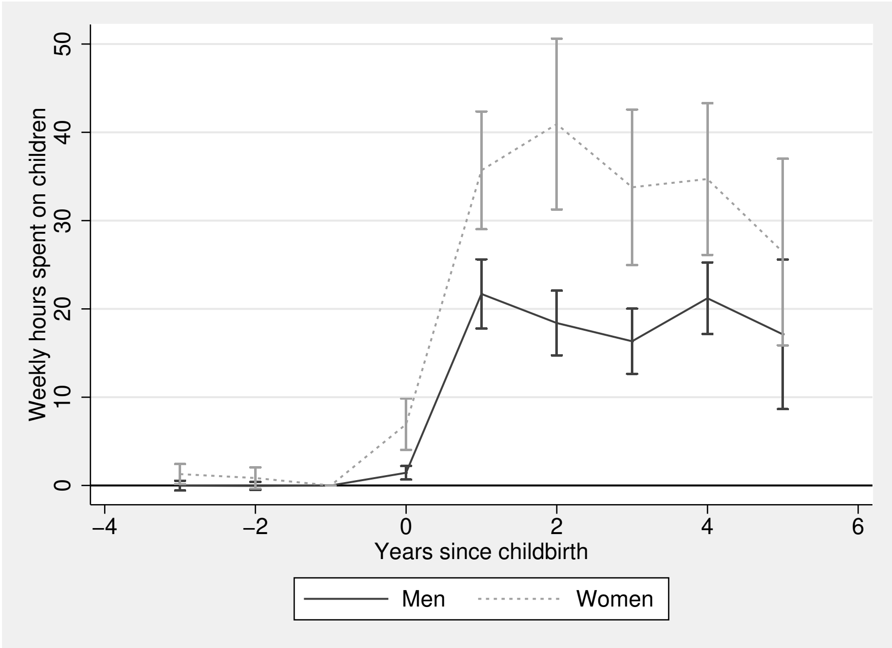


Figure A4.15: Weekly hours spent on children estimates by event time and gender. Based on 438 observations, and 312 individuals.



Chapter 5

Does Opting Out of Public Disability Insurance lead to more Outflow to Work? Evidence from the Netherlands

Abstract

In the Netherlands, firms can opt out of partial and temporary Disability Insurance (DI), becoming responsible for reintegrating disabled workers themselves. Opting out creates incentives for firms to reintegrate disabled workers. In this chapter, I provide the first evidence of whether opting out of disability insurance increases work-related exits from partial and temporary disability. I estimate duration models using administrative records from 2006 to 2022 containing all disability spells that started between 2006 and 2021, DI status, and the labor market status of temporarily disabled workers. I find that outflow to work is higher among non-publicly insured firms. These differences are a result of composition effects, related to both employee and employer characteristics. After controlling for differences in composition, no significant differences remain between firms that opt out versus those that are publicly insured. However, I find higher outflow to recov-

This chapter is single-authored. This chapter is based on anonymized data from DI recipients in the Netherlands, provided by the Dutch Employee Insurance Agency. The opinions expressed in this chapter are solely those of the author and do not necessarily reflect the official policy or position of the Dutch Employee Insurance Agency or any of its partners. Responsibility for the data analyses and content in this chapter lies entirely with the author. The data used in this study are confidential and cannot be shared publicly. I am grateful to the Dutch Employee Insurance Agency, Marloes Lammers, Carla van Deursen, and Frank Schreuder. This project would not be possible without them. I thank Marloes Lammers, Margaretha Buurman, and Miranda de Vries for useful comments and suggestions.

ery without work, and to full disability for those who opt out, and lower outflow for other reasons such as retirement, even after accounting for composition effects. Additionally, I find more re-assessments to a lower degree of disability among non-publicly insured workers. These findings are driven by the fact that firms who opt out are more active in requesting re-assessments for non-structurally disabled workers. My findings indicate that more active requesting of re-assessments leads to more efficient sorting of workers who regain (part of) their earning potential. However, re-assessments also increase outflow to structural disability. The results may have policy implications with respect to re-assessments: facilitating re-assessment requests can enhance DI outflow by swiftly capturing updates in disabled workers' health status.[1]

5.1 Introduction

Disability poses one of the greatest risks one can suffer in their career. In addition to losing one's current earnings capacity, disability disrupts human capital accumulation throughout the life cycle, thereby hindering one's earnings potential for the remainder of one's working life.

Most developed countries have Disability Insurance (DI) schemes to insure workers against this risk of disability. These disability schemes are usually government-provided, paying out a percentage of one's previous earnings capacity or a percentage of the minimum wage.

Some countries have non-public alternatives to DI, such as Canada, Switzerland, and the Netherlands (McVicar et al. (2022)). In these settings, private insurers are responsible for reintegrating and covering DI benefits of disabled workers themselves. This creates an incentive to re-integrate workers more actively than in the public system, as successful reintegration lowers DI premiums.

The Dutch DI system distinguishes between partial disability, temporary (full) disability, and structural full disability. Structural

[1]This chapter is based on anonymized data from DI recipients in the Netherlands, provided by the Dutch Employee Insurance Agency. The opinions expressed in this chapter are solely those of the author and do not necessarily reflect the official policy or position of the Dutch Employee Insurance Agency or any of its partners. Responsibility for the data analyses and content in this chapter lies entirely with the author. The data used in this study are confidential and cannot be shared publicly.

full disability benefits are always provided through public insurance, but employers can opt out of public insurance for partial and temporary (full) DI. In this case, employers can either reintegrate and pay DI benefits to disabled workers themselves, or re-insure through a private DI insurer.[2]

Partial and temporary (full) DI entitlement may cease due to various factors. First, workers can be re-assessed to having recovered from disability. Second, workers can be re-assessed to full DI, after which employers no longer have obligations to reintegrate. Finally, DI benefits can end for other reasons such as retirement.

In the Netherlands, opting out of public insurance results in several distinctions compared to employers who remain publicly insured. First, opting out augments the incentive and opportunity for employers to influence the reintegration of employees. Second, non-publicly insured employers (and/or the firm they insure at) can request re-assessments themselves instead of relying on the Dutch Employee Insurance Agency to do so. While re-assessments requested by non-publicly insured employers are still handled by the Dutch Employee Agency, non-publicly insured employers have an incentive to request re-assessments[3] of workers that the Dutch Employee Agency does not have: outflow from partial DI leads to lower DI premiums. Lowering premiums through re-assessments is achievable in two forms: First, re-assessments to a lower degree of disability results in lower benefit payments. Second, re-assessments that result in an employee moving out of DI or transitioning to structural DI relieve non-publicly insured employers from the obligation to pay the employee's insurance premiums.

Earlier literature has highlighted differences between non-publicly insured firms based on selection into non-public insurance (Hassink et al. (2018)), insurer effort (Koning and van Lent (2024)), and outflow out of DI (Lammers et al. (2018)). Notably, the latter study shows that both the total DI outflow and outflow to structural DI are significantly higher among non-publicly insured firms. However, no evidence yet exists on whether Dutch non-publicly insured firms reintegrate disabled workers more effectively.

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[2] The data do not differentiate between these two options.

[3] Firms are not charged any fees for re-assessments, but are required to fulfill administrative prerequisites.

In this chapter, I provide evidence on how non-public DI affects return-to-work in a unique institutional setting with a comprehensive set of Dutch administrative records. I observe a rich set of characteristics of both disabled workers and employers. Including but not limited to labor market earnings, degree of disability, employer traits, and re-assessments. This detailed data allows me to explore the mechanisms underlying non-publicly insured reintegration. I estimate duration models using Dutch administrative records. My contribution to the literature is twofold. First, I estimate how non-public disability insurance affects outflow to work. No existing Dutch literature has investigated this yet. The Dutch system is of particular interest as strong return-to-work incentives are present for employees in both public and non-public DI provision. Second, I enhance existing Dutch DI outflow estimates with more detailed data over a longer period of time than in existing literature, allowing for a higher degree of external validity than existing papers. The data contain a rich set of employee- and employer characteristics[4], allowing me to explore the mechanisms behind differences in publicly versus non-publicly insured DI.

My findings are as follows. First, I discover higher DI outflow to work among non-publicly insured employers. However, these differences are entirely driven by selection effects on the basis of employer- and employee characteristics: differences in outflow to work disappear when I study DI spells of employees that switch from public to non-public insurance as a result of the firm switching insurance status. However, I also observe a notable increase in the outflow to full Disability Insurance (DI) and recovery without resuming work. The impact on the former is particularly substantial, even among DI spells that switch insurance status. Finally, my findings reveal marginally higher yet statistically significant rates of re-assessments resulting in a reduction of the degree of disability for non-publicly insured workers. Differences in outflow to full DI are primarily driven by non-publicly insured firms having workers re-assessed more often. These findings suggest that while outflow to work may not increase as a result of non-public DI provision, non-public DI does lead to more outflow to full disability and thereby to less outflow for other reasons such as

[4]I.e., on the basis gender, age, sector, labor market area, degree of disability, and diagnosis category.

retirement.

This chapter is organized as follows. Section 2 lays out the Dutch DI setting. Section 3 discusses the existing literature on DI. Section 4 explains the data. Section 5 lays out the methodology. Section 6 presents results. Finally, section 7 concludes.

5.2 Institutional setting

This section describes the key elements of the Dutch DI system and the implementation of non-public disability insurance provision.

In the Netherlands, workers receive paid sick leave from their employer if they are unable to work as a result of sickness. Dutch workers can apply for DI after two years of sick leave. After filing for DI, Dutch workers are subject to an assessment of their degree of disability. Disability claims are assessed by a physician independent of the employer. Based on the input from the physician, a vocational expert determines the lost earnings capacity. The vocational expert uses a computer system (CBBS) as an input for his or her analysis. The computer system helps in determining which jobs workers could still perform, given their (remaining) abilities and capabilities. DI claims are admitted for workers who are assessed to have lost 35% or more of their earnings capacity. After a DI claim is admitted, workers receive a benefit on the basis of their lost earnings capacity relative to their wage before sick leave. Depending on the lost earnings capacity (or: degree of disability), workers are then categorized depending on their degree of disability and potential to recover.

Workers who have lost 35% of their earning capacity or less do not receive disability benefits. Workers with disability degrees between 35% and 80% receive partial DI (WGA 35-80) benefits, with strong return-to-work incentives in place since the benefits are only partial. For disability degrees of 80% or higher, the benefit awarded depends on whether the disability is assessed to be structural. When the disability cannot (yet) be considered structural, so-called temporary WGA DI benefits are awarded (WGA 80-100), equal to 70% of one's previous wage. These benefits are subject to reintegration efforts. I henceforth refer to both WGA benefits as temporary and partial DI. When the disability is considered structural, full DI benefits (IVA) equal to 75%

of one's previous wage are awarded, with no mandatory reintegration in place. Additionally, while both WGA schemes can be insured either publicly or non-publicly, IVA is always insured publicly.

As IVA benefits are never insured non-publicly, I estimate the models for DI recipients who start out in partial and temporary disability. In both WGA schemes, workers are first awarded a wage-related benefit (LGU) for between 3 and 24 months depending on how many years they worked prior to disability. The LGU amounts to a maximum of 75% of one's prior wage for the first two months, and 70% thereafter. It is worth noting that the LGU benefit level is the same as the Dutch unemployment benefit level. After the LGU ends, the benefit awarded increases with the degree to which workers use their remaining earning capacity.

Compared to DI systems in other countries, the Netherlands has highly stringent admission requirements, as well as strong incentives for employers to facilitate return-to-work for non-structurally disabled workers. However, the benefits paid out are high relative to other OECD countries, especially for workers who comply with the return-to-work incentives (McVicar et al. (2022)).

Disability in the Netherlands is publicly insured, for which employers pay an experience-rated premium based on how many sick and partially[5] disabled employees they have. This premium is bounded by between 0.16% and 2.64%[6] of wages for sick workers, and 0.21% and 3.48% of wages for disabled workers UWV (2023). Hassink et al. (2018) provide more details on public insurance premiums. Employers, however, can opt out of this public insurance for the partial and nonstructural DI scheme[7]. When firms opt out, assessments and DI premiums remain publicly set, but insurance payments and reintegration efforts are carried out by the employer instead of the public system. Additionally, DI benefits are no longer experience rated. Employers that opt out of public insurance have two options at hand:

1. Employers self-insure disabled workers. This means that they pay the individual DI benefits of employees themselves and become

[5]Structural full DI is not subject to experience-rating.

[6]The temporary work sector is an exception to this upper bound, instead having a maximum of 8,27%.

[7]Opting out requires a warranty declaration from the Dutch Tax Agency. This measure serves to prevent firms that cease to exist from shifting non-public DI spells to the public system.

responsible for reintegration.

2. Employers re-insure their DI risk at a private insurance company, in which case the insurance company (or another private party) handles reintegration and the employer pays the private company's DI premium, which large firms can negotiate.

Opting out creates a means to facilitate outflow not present for publicly insured firms: non-publicly insured employers, as opposed to the Employee Insurance Agency, have an incentive to monitor disabled workers and request re-assessments[8]. The Employee Insurance Agency (UWV) handles these re-assessments, and workers do not have the option to refuse this re-assessment. Second, non-publicly insured employers can design their own reintegration methods, including potential work accommodations and bonuses. Third, private insurers and firms may have their own reintegration and/or health professionals, allowing for potentially faster reintegration.

Due to the different incentives and reintegration mechanisms, employers tend to self-insure when they have relatively good prospects of reintegrating disabled workers. Non-publicly insured employers are over-represented in agriculture, construction, industry, retail, and transport. Furthermore, most non-publicly insured employers have a moderate (25-100) or very large (250+) number of employees (Cuelenaere et al. (2013)). This is driven by several factors. First, irrespective of insurance status, large firms face individual DI premiums as opposed to sector-based ones, creating a larger incentive to reduce their number of disabled workers. Second, large firms tend to have more developed human resource and personnel management options. Finally, large firms relatively often have the required solvency to self-insure and can diversify their DI inflow risk.

When employers self-insure, they no longer pay the aforementioned premiums, instead covering most individual DI benefits[9] and become responsible for reintegrating workers. The Dutch experience rating in the public system leads to DI premiums and the sum of individual benefits being roughly equal. As such, there are incentives

[8]Firms do not pay the costs of the aforementioned re-assessments, though some administrative red tape and requirements on behalf of the employer are present.

[9]Increased DI benefits as a result of using one's earning capacity are not borne by non-public insurers as to not disincentivize work resumption. Instead, employers pay the benefit the worker would have received in the absence of employment (UWV (2023)).

for the firm to decrease their total number of disabled workers, but not to switch to and from public insurance for a given number of disabled workers. Employers and other external parties (like insurers, or workers) can themselves request re-assessments by an insurance doctor, which are given a higher priority than re-assessments requested by insurance doctors themselves. However, even among non-publicly insured firms, re-assessments are always performed by the Dutch Employee Insurance agency with the assessor selected at random.

Incentives for non-publicly insured employers to have their employees re-assessed are twofold: First, having disabled workers re-assessed to a lower degree of disability results in non-publicly insured employers having to pay fewer DI benefits. Second, both employees and non-publicly insured employers have an incentive to transition to full disability: the employee then receives a 75% replacement rate without any reintegration obligations, while the non-publicly insured employer no longer has to pay the worker's disability benefits.

5.3 Literature review

Although the incentives underlying Dutch DI inflow and outflow have been extensively studied, there is very little direct evidence on the relative outflow rates between public and non-public DI insurance. Hassink et al. (2018) find no structural selection into non-public DI when DI outflow is low, but do discover selection in the two years prior to DI inflow when risks are temporarily low. Koning and van Lent (2024) show that non-public DI creates incentives to reduce moral hazard and increase outflow. Lammers et al. (2018) show that, as compared to public DI, non-public DI increases total outflow, outflow to recovery, and outflow to structural disability. However, to the best of my knowledge, no evidence on how non-public DI compared to public DI affects outflow to work exists yet. This chapter contributes to the scarce literature on non-public DI and is related to several strands of existing literature.

DI insures workers against disability risk over the life cycle. However, DI also creates several disincentives for disabled workers. First, DI decreases the relative return of working, and therefore discourages return-to-work efforts (French and Song (2014); Gelber et al.

(2017); Gruber (2000); Maestas et al. (2013); Ruh and Staubli (2019); Soika (2018); Spierdijk et al. (2009)). This phenomenon is known as moral hazard. Second, the presence of DI systems may lead to self-selection out of DI by individuals with low disability risk (Landais et al. (2021)), a phenomenon known as adverse selection. Adverse selection leads to conventional private DI markets failing, which makes public DI systems welfare enhancing (Chetty (2008)). These welfare gains then exhibit a trade-off between moral hazard and the insurance effect (Chetty (2008)).

Some countries offer private DI schemes in addition to public ones (McVicar et al. (2022)). International literature provides some evidence on the incentives created by non-public DI. Rehwald et al. (2017) discover, in an experiment, that non-public employment services offer more intensive job search assistance. This heightened intensity, however, does not result in higher job finding rates. Autor et al. (2014) find – using firm records in the United States, that non-public long-term disability exhibits higher return-to-work rates, particularly among low-liquidity and relatively healthy disability claimants, as these individuals have the most remaining work capacity and greater incentives to resume work. Seitz (2021) shows, using German private DI insurer data, that private DI increases moral hazard in the public DI system, as this allows workers to acquire additional benefits in addition to the public ones. However, the overall existence of public and private DI is not necessarily detrimental to welfare. Seibold et al. (2022) show German private DI take-up is primarily concentrated among low-risk and high-income individuals. This type of advantageous selection is primarily driven by these individuals having a lower risk preference than high-risk and low-income individuals.

DI design parameters may also affect outflow rates. Some Dutch evidence with respect to how non-public DI insurance affects inflow and outflow exists in the literature. Hassink et al. (2018) show that there is a selection mechanism in Dutch public disability insurance: Employers who anticipate relatively low DI inflow risk opt out of public insurance, arguing that differences in DI inflow rates are driven by selection mechanisms rather than caused by the services of private DI insurance. These selection mechanisms make it difficult to provide causal evidence of the effects of non-public DI. Garcia-Mandicó et al.

(2020) identify a 20% reduction in DI income and a €636 increase in labor market earnings for every €1000 reduction in DI benefits. These findings are primarily driven by workers with shorter claim durations and disabilities which are difficult to measure. De Groot and Koning (2016) estimate the effects of experience rating DI premium payments for small firms, finding a 7% decrease in DI inflow and a 12% increase in DI outflow. However, it is worth noting that the estimates in De Groot and Koning (2016) are primarily concentrated among the first year of sick leave and estimated prior to a DI reform that increased sick leave durations from 1 to 2 years. This reform means that the estimates likely do not hold for the current Dutch DI system. Koning and van Lent (2024) estimate the role of insurer effort in reintegrating disabled workers in the Netherlands. They estimate that insurer-based incentives to offer work accommodations and rewards for work resumption can offset moral hazard from DI recipiency. DI stringency also directly affects inflow and outflow rates. Garcia-Mandicó et al. (2020) estimate how the re-assessment of Dutch workers under a more stringent DI system affected disability receipt and earnings, and find increases in earnings as well as decreases in DI recipiency. Engström et al. (2017) estimate how early caseworker meeting with sick workers affects inflow to DI in Sweden, showing a 20% increase in DI inflow, with this inflow increase being potentially driven by sick workers signaling bad health in said intervention.

Related literature has investigated re-integration and preventative programs. Baert et al. (2018) use Dutch administrative data to estimate the effect of interventions by medical and occupational specialists on the sick leave of self-employed workers, finding that both intervention types adversely affect recovery rates. While no clear underlying mechanism is demonstrated, potential explanations include asymmetric information with respect to sickness and caseworkers being more focused on long-term recovery than on sick leave durations compared to self-employed workers. Viering et al. (2015) estimate the effect of individual placement and support on job finding rates for disability pensioners, showing that disability pensioners with individual placement and support were 20 percentage points more likely to find work than those who did not. Brongers et al. (2023) discover, using a randomized controlled trial in the Netherlands, that specialized care by labor market experts does not increase recovery for workers with multiple disabilities as compared to Dutch baseline reintegration systems. Peijen and Wilthagen (2022) show that company-based vocational education increases rehabilitation in the Netherlands, especially among workers with cognitive disabilities. Re-integration may therefore be most effective for workers with a single non-physical disability.

5.4 Data

I use administrative data from the Dutch Employee Insurance Agency from 2006 to 2022. The core sample is based on temporary and partial DI admissions between 2006 and 2021. I observe start- and end dates of DI spells and other spell-related characteristics such as sector, diagnosis type, degree of disability, employer size, and the age and gender of the employee.

I also observe monthly labor market records and monthly DI payments. These allow me to identify whether individuals work, at which employer DI recipients work, their earnings, the exact DI benefits handed out, and the insurance status of the employer. I merge labor market and DI payment records to the core sample such that I observe in each month whether DI recipients are working, and at which employer they work if so. From these data, I then construct indicators on whether DI recipients work during and/or after their DI spells.

Prior to the sample selection, I observe the start and end dates of all 195.000 DI spells that started in the sample period. In the data, I distinguish between three insurance types: publicly insured non-government employers, non-publicly insured non-government employers, and government employers. For the non-publicly insured, the data does not distinguish between either self-insuring or obtaining re-insurance at a private insurer. I distinguish between government and non-government for several reasons. First, government employers always self-insure. As such, no selection effects are present. Second, government employers are limited to only two specific sectors, whereas non-publicly insured non-government employers are active in a broader support of sectors. Third, government employers themselves have strongly different characteristics from non-publicly insured employers. Due to these differences, I do not include government employ-

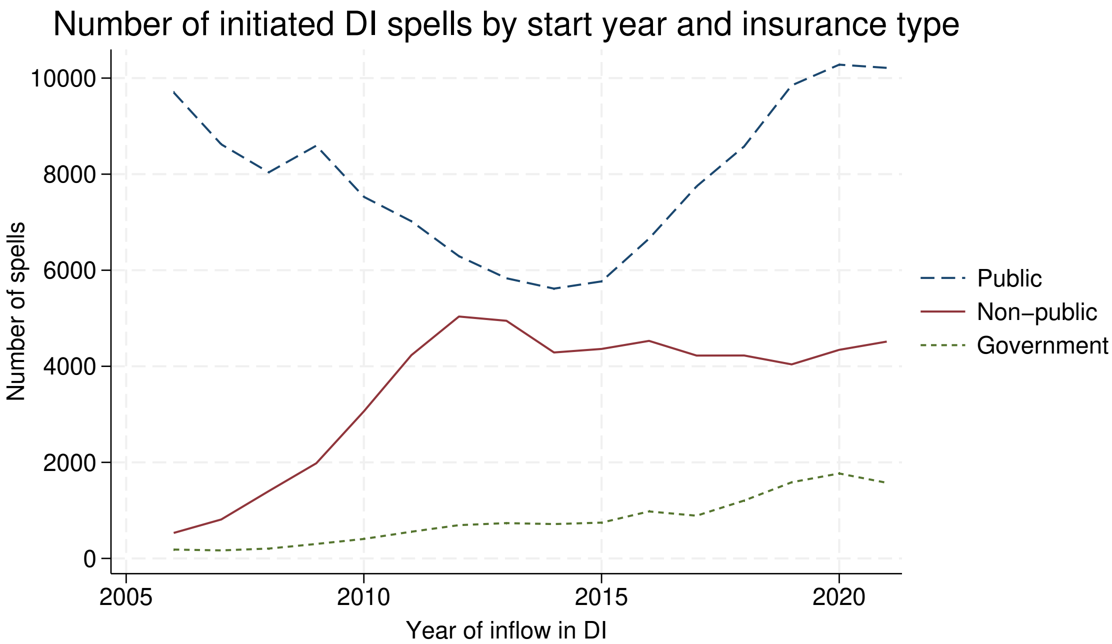

Figure 5.1: Total number of DI spells that started between 2006 and 2021, separated by start year and insurance status of the employee on the first day of DI entitlement. Government is defined as working directly for the government. Services provided to the government by private parties do not fall under the government category.

ers in the analysis in chapter 6. My selection criteria leave roughly 182 000 spells, of which roughly 13 000 result in outflow to work.

Figure 5.1 presents the number of starting DI spells by insurance type for every year in the sample. In the initial years of the observational period, a notable surge in the opt-out rate is discernible until 2013, followed by a decline in subsequent years. This trend is attributed to several contributing factors. Initially, an erroneous setting of public Disability Insurance (DI) premiums in 2009 resulted in them being set at levels that were too low (Cuelenaere et al. (2013)). Consequently, post-2009, there was an escalation in public DI premiums, prompting numerous employers to opt out of the public insurance scheme. Additionally, until 2013, non-publicly insured employers imposed lower DI premiums compared to those of the public insurance sector (UWV (2014)). After 2014, these public premiums were increased. The trend is supported by changes in the relative proportion of employers opting for non-public insurance coverage are in line with the changes in the premiums.

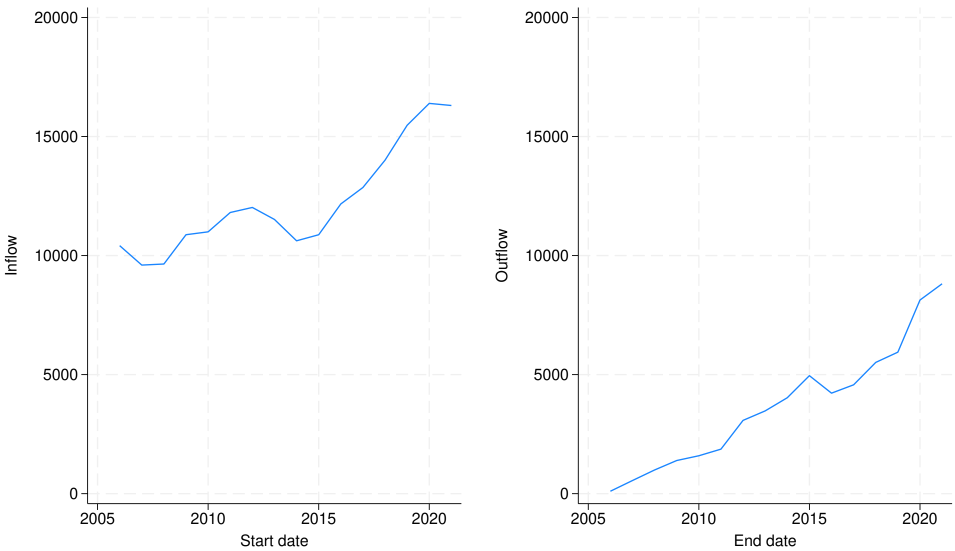

Figure 5.2: Number of governmental and non-governmental DI spells that started and ended in every year between 2006 and 2022.

Figure 5.2 illustrates DI inflow and outflow in the sample. Inflow starts at approximately 10,000 cases per year, and climbs upwards after 2015. Outflow starts at 0 cases per year, as I only investigate spells that started in 2006 or later, then steadily trends upwards to just under 10,000 cases a year by the end of the sample.

Characteristic	Public Insurance	Non-public Insurance	Government	Total
Male	44.6%	40.8%	40.4%	43.2%
Age	48.0	48.5	49.5	48.3
Diagnosis:				
Cancer	6.8%	6.7%	6.4%	6.7%
Cardiovascular disease	8.4%	9.5%	10.0%	9.1%
Psychological ailment	34.8%	34.2%	44.4%	35.3%
Musculoskeletal issues	26.2%	28.5%	14.7%	26.1%
Other/Unknown	23.8%	21.2%	24.5%	23.1%
Benefit (degree of disability) at inflow				
28	10.0%	10.6%	12.9%	10.3%
35	8.9%	9.1%	12.9%	9.2%
42	6.1%	6.6%	9.0%	6.4%
50,75	6.4%	6.9%	11.2%	6.9%
70	18.0%	6.2%	5.5%	13.8%
75	50.6%	60.7%	48.5%	53.4%
Employed at inflow	56.5%	67.3%	85.8%	61.5%
Employed at same employer at inflow	48.9%	58.9%	77.6%	53.6%
Employer sector:				
Agriculture and nutrition	4.4%	4.6%	0%	4.2%
Construction and wood	4.2%	4.0%	0%	3.8%
Industry	11.6%	13.0%	0%	11.2%
Retail	13.4%	14.6%	0%	12.9%
Transport	5.8%	7.7%	0%	6.0%
Financial services	11.3%	10.5%	0%	11.0%
Healthcare	19.6%	32.2%	0%	22.0%
Government (incl. education)	16.5%	12.9%	100%	17.2%
Other	13.2%	11.1%	0%	11.7%
Employer size:				
Small (0-10)	10.3%	8.0%	0.2%	9.0%
Medium (10-100)	18.9%	14.9%	0.7%	16.6%
Large (>100)	43.6%	62.4%	81.2%	51.5%
Unknown	27.2%	14.7%	17.9%	23.0%

Table 5.1: Individual characteristics of DI recipients separated by insurance status on the first day of DI entitlement. DI benefits in the Netherlands are assigned to categories, rather than an exact percentage. 'Government' is defined as working directly for the government. Services provided to the government by private parties do not fall under the government category.

Table 5.1 presents characteristics of DI recipients the moment they enter DI. Gender, age, diagnosis, and sector differences between publicly and non-publicly insured spells are small. Non-public DI spells have lower degrees of disability upon entry and are relatively likely to be employed upon entry.

On the employer-side, DI claimants at firms that opt out work in tertiary sector jobs relatively often, and are more often employed at large employers. Both of these phenomena are likely driven by firm size: Larger firms, on average, have a relatively stable number of DI recipients due to having individual public premiums instead of sector-based ones, and as such a greater incentive to self-insure. The healthcare sector, in addition to generally having large employers, have the expertise to reintegrate workers themselves, underlying the relatively large opt-out rate in this sector. No government employees are present in the healthcare sector, as healthcare provided by the government is classified as belonging to the government sector. Finally, non-government employees are present even in the government sector due to private contracts in this sector. These employees are classified as non-government. The data allow me to control for a wide set of employer characteristics, as well as the type of health issues DI claimants suffer from. This is particularly important because the differences in characteristics likely result in non-public DI spells ending more quickly.

Approximately a third of the DI spells in the data do not end within the sample, usually as a result of starting near the end of the sample. Additionally, for approximately one percent of DI spells I know that they ended within the sample, but not when. I know that these spells lasted for at least one month, but I cannot accurately determine their duration. As such, I set these spells as censored after the first month. Not accounting for spells ending past the point where I can observe them or have unknown durations leads to biased results. To prevent said bias, spells that do not end within the data or become censored are included in my estimates and implicitly subject to counterfactual prediction: On the basis of other spell end dates, my estimates predict when spells that did not end in the data would have ended later. As such, the number of estimated DI exits is larger than the number of DI exits I observe in the data.

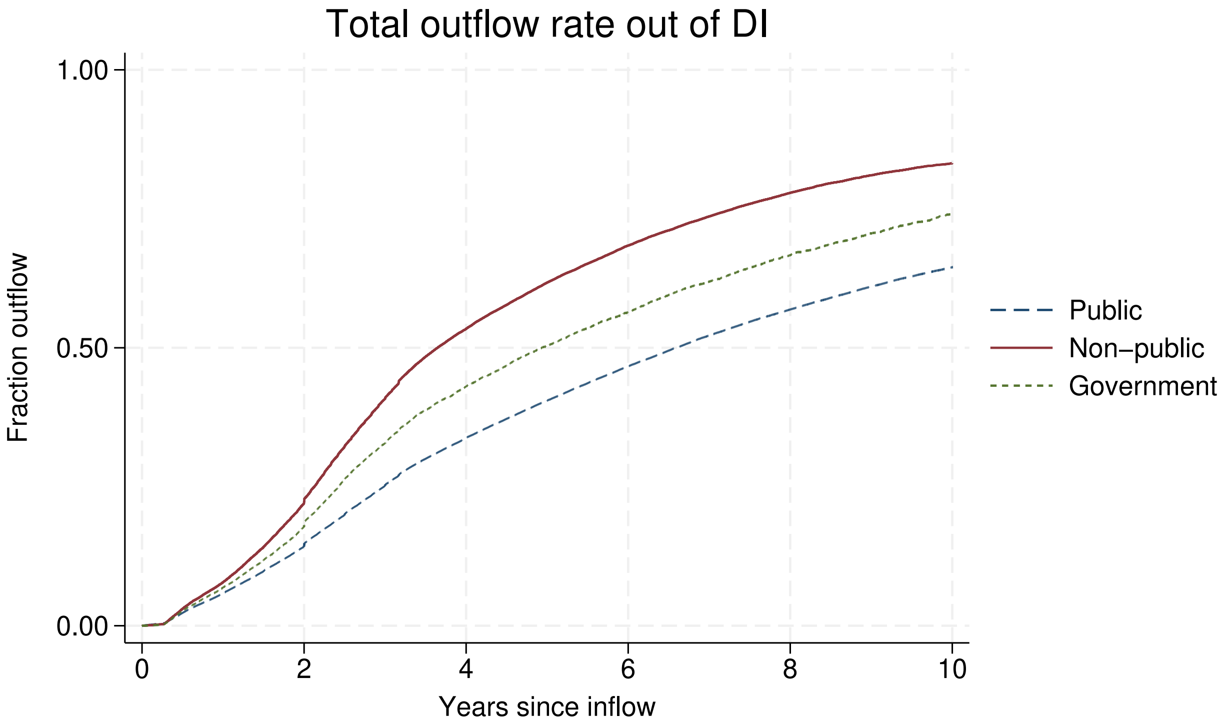

Figure 5.3: Fraction of spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement.

Figure 5.3 shows outflow from DI for any reason per insurance type, measured as the fraction of DI recipients in the sample that has left DI. In the first two years of DI benefits, outflow rates are relatively high, with small spike effects two years and thirty-eight months after inflow, and an acceleration of outflow afterwards. This phenomenon may be a result of disabled workers no longer being entitled to their initial benefit (LGU)[10] and transitioning to a (lower) benefit after this period. The two distinct peaks are driven by the maximum duration of the LGU decreasing within the sample: Before 2015, the maximum duration of the LGU was 38 months. From 2015 on, the LGU was phased out to last at most 2 years for new DI recipients. This phenomenon matches the spike effects found in Mesman et al. (2023).

Afterward, DI outflow accumulates at a decelerated rate. After 10 years, roughly two thirds of DI recipients stopped receiving benefits, with higher outflow rates among non-publicly insured workers, especially among those who do not work for the government. In the longer run, outflow continues to accumulate. The data continues af-

[10]Details on the LGU can be found in section 2.

ter the 10 years shown, but the additional outflow afterward is almost entirely for reasons such as retirement.

In addition to examining the characteristics of employers and employees, my main analysis focuses on employers that transition between public and non-public DI provision within the sample. Prior to 2014, when an employer opted out, either the employer or the insurer assumed responsibility for the rehabilitation of all disabled workers within the firm. Conversely, when a non-publicly insured firm elected to transition to public insurance, all existing DI cases remained with the firm or its insurer. Starting in 2014, employees at small firms continued to be covered by public insurance even if the firm opted out, and this policy was extended to large firms in 2017. This has the following implications for the analysis:

1. When firms opted out prior to 2014, all of their current DI spells became non-publicly insured.
2. From 2014 to 2016, the current spells of small firms stayed publicly insured when they switched insurance status.
3. From 2017, all current DI spell remained publicly insured when the employer opted out.

Firms having both publicly and non-publicly insured employees allows me to control for firm-specific and employee-specific characteristics that influence the speed of outflow from (partial and temporary) DI. Moreover, this also allows me to test for dynamic selection on the basis of outflow. To do so, and to prevent switching from being an outcome of long DI spells, I investigate spells of firms that switched from public to non-public insurance prior to 2017. I refer to these spells as switchers henceforth. If dynamic selection is present, I expect outflow rate effects to be present in the year prior to switching to non-public insurance. This chapter tests for this type of selection in the results.

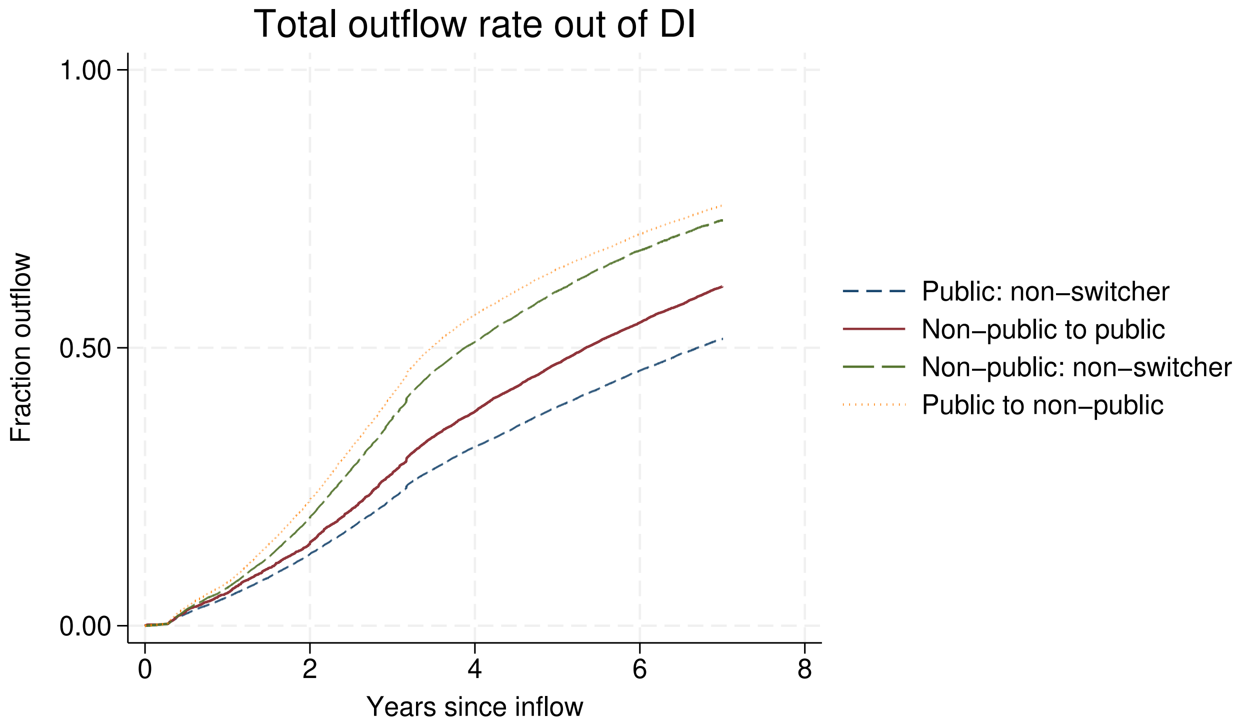

Figure 5.4: Fraction of DI spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement and whether the employee switched to or from public insurance during their disability spell.

Figure 5.4 shows DI outflow rates separated on the basis of both non-public and publicly insured firms in addition to a binary indicator of whether firms switch from public to non-public insurance during the employee's disability spell. As in figure 1, DI outflow is higher among non-publicly insured spells both among switchers and among non-switchers. Spells that switch, however, exhibit a slight increase in DI outflow as compared to spells that do not switch. This may be the result of dynamic selection, but may also indicate a causal mechanism being present. These differences may be the result of selection effects in non-public insurance. Note that figures 1 and 2 measure total outflow without accounting for competing risks. I also compute DI outflow separated by outflow type. Figure 4 shows Cumulative Incidence functions resulting from that exercise. Cumulative Incidence functions differ from Kaplan-Meier estimates in that they scale the incidence function by all other failure types, thus accounting for competing risks.

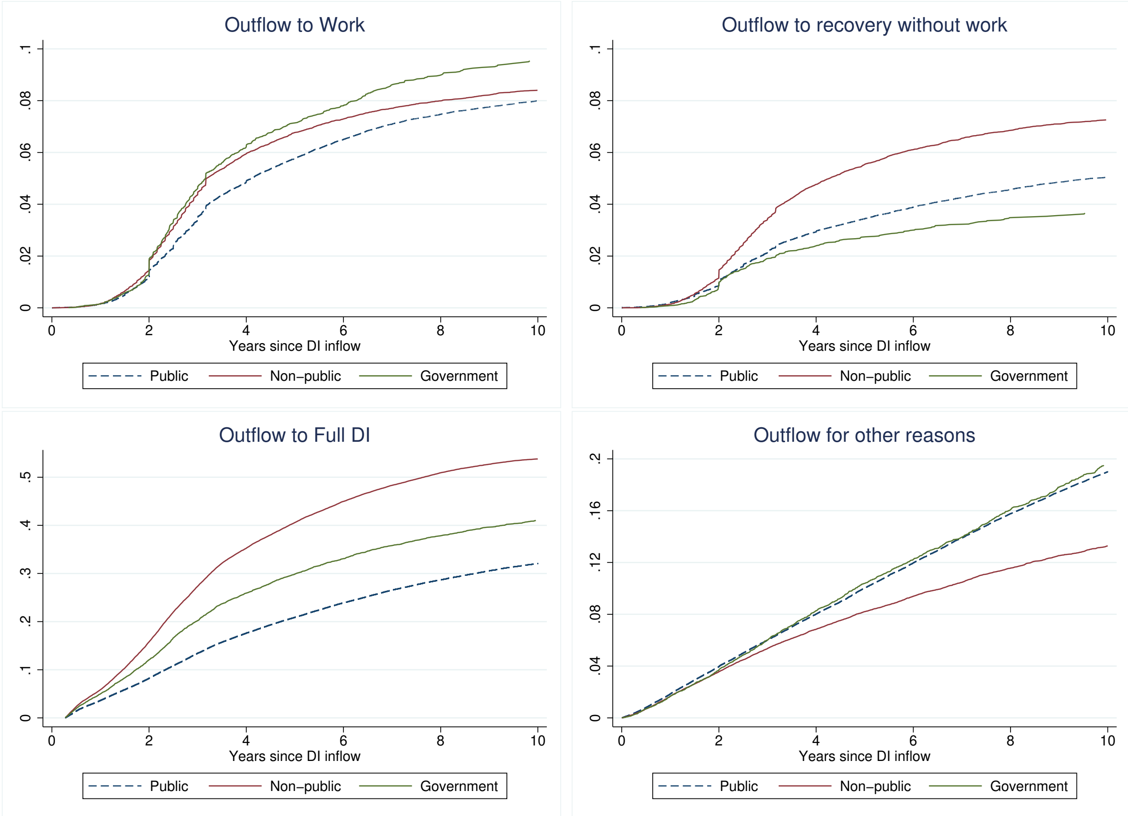

Figure 5.5: Fraction of DI spells that ended within a given number of years, separated by insurance status on the first day of DI entitlement and accounting for cumulative incidence of outflow reasons.

Figure 5.5 presents total DI outflow separated by outflow type. After 10 years, roughly 8% of DI spells end in outflow to work, with slightly higher figures among non-publicly insured DI recipients. Recovery without work is roughly 7% among non-publicly insured workers and roughly 4.5% among publicly insured workers. Figure 3 shows large outflow differences to full DI. Whereas roughly 30% of publicly insured workers flow out to full DI after 10 years, this figure is nearly twice as large for non-publicly insured workers. Most of the remaining DI outflow occurs as a result of retirement. Outflow for other reasons is notably smaller for non-publicly insured DI spells than for public and government DI spells. Additionally, outflow for other reasons becomes the main outflow reason in the long run, with the effect dominating earlier for public and government employers than for non-publicly insured employers.

Table 5.2: Percentage of spells in the sample that resulted in an approved re-assessment, separated by whether the spell is publicly or non-publicly insured.

	Public		Non-public	
	Mean (%)	**Standard error (%)**	**Mean (%)**	**Standard error (%)**
Partial DI to temporary full DI	4.05	0.06	5.66	0.09
Temporary full DI to partial DI	3.89	0.06	6.64	0.12
Temporary/partial DI to recovery	5.09	0.07	10.62	0.13
Partial DI to structural full DI	2.99	0.05	4.49	0.08
Temporary full DI to structural full DI	11.26	0.09	21.75	0.17
Total	27.28	0.15	49.16	0.25

I additionally investigate re-assessments in the data to identify a causal mechanism underlying outflow, as all spells that end in the data either end as a result of a re-assessment taking place or due to other reasons such as retirement. As I do not observe re-assessments prior to 2013, estimates of successful re-assessments are necessarily a lower bound. Table 5.2 shows the percentage of spells that were successfully re-assessed on the basis of whether the requester is public or non-public. Re-assessments from partial DI to temporary full DI are much less common among non-public requesters, likely due to this type of re-assessments increasing the DI payments they have to make. I also see slightly fewer approved re-assessments from partial DI to structural full DI. All other types of successful re-assessments, however, are more common among non-public requesters. Particularly re-assessments from non-structural full DI are relatively common. These differences are again incentive-driven: Re-assessments to full DI entail non-publicly insured firms no longer having to pay DI benefits or otherwise reintegrate the worker in question. As such, the descriptives indicate that non-public insurers specifically aim at the type of re-assessments that lead to lower DI premiums.

5.5 Methodology

This chapter aims to investigate how non-public DI (compared to public DI) affects DI outflow rates, focusing primarily on outflow to work. As the data exhibits censoring, I estimate exponential duration models. [11] I estimate (semi-)parametric hazard rates instead of propor-

[11] OLS and fixed effects models of outflow within a given timeframe yield similar conclusions. Appendix A5.1 shows these estimates.

tional ones as the data exhibit strategic outflow timing, in particular with respect to LGU benefits. Semi-(semi-)parametric and non-(semi-)parametric methods cannot capture this dynamic, as nonparametric methods omit the time horizon. The models estimated impose several assumptions. First, the hazard rate is assumed to be exponential conditional on the included covariates. Second, all dynamics underlying the relative risk of publicly and non-publicly insured employers are included. The second assumption is similar to the assumptions underlying least-square estimation. My baseline model is a hazard rate model in which the transition rate out of DI at duration t conditional on observed characteristics Z is specified as follows:

$$\theta(t|Z) = \lambda(t)\exp(Z'\beta) \tag{5.1}$$

where $\lambda(t)$ is a piecewise constant function representing the pattern of duration dependence (with coefficients for each year after inflow in disability).[12] β is a parameter vector, and Z is a vector containing: an indicator for non-public insurance at inflow, control variables, inflow-year, and calendar-year effects. In my basic analysis, I compare outflow on the basis of whether the employer is enrolled in public versus non-public DI insurance at DI inflow[13]. To eliminate potential composition effects between non-public and public insurance, I control for gender, age, sector, labor market area, degree of disability at inflow[14], diagnosis category, and a set of calendar- and inflow-year effects.

Additionally, I estimate models for the following four outflow reasons:

1. Recovery from disability with work

2. Recovery from disability without finding work

3. Outflow to full disability: Re-assessments can lead to full disability benefits. In this case, workers are no longer subject to reintegration obligations.

[12] I cannot estimate duration dependence for individual years after the 10th year of disability, as the data do not contain enough exits due to recovery after the 10th year of disability.

[13] I only show this parameter in the results section.

[14] Measured as the benefit percentage at inflow

4. Remaining outflow reasons such as retirement. I refer to this outflow type as outflow for ‘other’ reasons.

The individual outflow models do not account for the fact that the outflow reasons mentioned are mutually exclusive. Not accounting for this leads to upward-biased estimates. As such, after estimating individual exponential models, I compute total DI outflow by outflow reason while accounting for the risks competing with one another. To this end, I compute outflow rates by insurance status per outflow reason accounting for cumulative incidence. I bootstrap the sample and use Monte Carlo simulation with 100 repetitions to estimate the corresponding standard errors.

I also estimate one more set of equations in which I compare reassessment to a lower degree of disability without outflow. With this set of equations I test the hypothesis that firms and insurers act to reduce the degree of disability of disabled workers even if it does not lead to complete outflow, as this results in lower individual benefit payments.

At first glance adverse selection seems like a concern: firms may opt out as a result of having low DI risk instead of opting out itself lowering DI risk. However, this type of adverse selection is in practice mitigated by moderately-sized and large firms being subject to experience rating. This experience rating leads to long-run public DI premiums roughly equaling the sum of the employer’s DI benefit payments. While smaller employers are subject to a non-experience-rated sector premium, non-public DI insurance providers generally charge this same sector premium for small firms without differentiating with respect to individual DI risk (Cuelenaere et al. (2013)). As such, for a given number of DI recipients, opting out of public insurance will not reduce DI benefit payments in the long run, leaving little scope for adverse selection. Additionally, empirical evidence (i.e. Hassink et al. (2018)) does not find adverse selection on the basis of inflow as a result of this experience rating.

DI premiums can fluctuate and differ between public and non-public DI, creating a remaining threat to inference in the form of dynamic selection on outflow risk. Employers may opt out of public insurance in the short run when expected outflow is temporarily low. Existing literature (i.e. Hassink et al. (2018)) shows dynamic selection on the basis of transitory inflow risk. However, this is less likely to be an issue for outflow, as inflow can be predicted on the basis of sick workers, whereas no such method exists for outflow.

I estimate a second set of hazard rate models for spells during which large firms transitioned from public to non-public insurance before 2017: When large firms made this switch, all the firm's current DI spells also shifted to non-public insurance. Estimating whether outflow rates of spells accelerate after they switch to non-public insurance helps to eliminate adverse selection. In contrast, measuring outflow rates in the year prior to switching provides insight into dynamic selection, as this may capture firms acting based on anticipated short-run outflow rates.

The data allow me to include a wide range of employee- and employer characteristics. However, I cannot include fixed effects in my duration models as the number of firms in my data exceeds the number of exits to work. As such, some endogeneity likely remains. I additionally estimate duration models for employees who switch insurance status due to the reintegrating employer switching from public insurance to non-public insurance during the DI spells. This variation in insurance status eliminates most of the endogeneity in the opt-out decision, as this entails variation for workers and firms that were both publicly and non-publicly insured. I define recovery to work as being re-assessed to having recovered from disability and working at least two consecutive months in the timeframe from two months[15] before recovery to two months after recovery. As an extension, I estimate the same outcome for outflow to recovery with work at the employer at which the worker became disabled. This extension allows me to estimate whether outflow to work is the result of reintegration or of within-firm substitution.

Finally, firms and insurers have an incentive to reduce the degree of disability of disabled workers even if it does not entail outflow, as this results in lower individual benefit payments. To this end, I estimate one more set of cumulative incidence models in which I compare re-assessment to a lower degree of disability without outflow.

[15]Measuring work over a longer timespan roughly yields the same results

5.6 Results

This section presents results on outflow to work and computes the effects of non-public DI compared to public DI.

5.6.1 Outflow by outflow reason

I start by estimating the hazard rate for total outflow. Afterward, I estimate hazard rates for specific outflow reasons, treating outflow not for that particular reason as censored. These models thus estimate individual risks without accounting for competing risks.

Dependent variable: DI outflow to...	Total	Work	Recovery without work	Structural DI	Other reasons
Non-publicly insured at DI inflow	0.515***	0.271***	0.577***	0.734***	-0.0260*
	(0.0070)	(0.0201)	(0.0220)	(0.00932)	(0.0157)
Controls	yes	yes	yes	yes	yes
Number of spells	182,469	182,469	182,469	182,469	182,469
Number of exits	102,594	13,154	10,129	55,920	25,646

Table 5.3: Piecewise-constant estimates of outflow by outflow reason, not accounting for cumulative incidence. Publicly insured spells form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5.3 shows piecewise constant exponential estimates of outflow[16] on the basis of DI insurance types without modeling competing risks[17]. I find significantly positive effects for all outflow reasons except for outflow for other reasons, which has precisely estimated null effects. Estimates of outflow to work are positive, but estimates for recovery without work and especially estimates to structural DI are even higher than outflow to work. These results may be indicative of higher outflow to work among non-publicly insured employers compared to non-publicly insured employers, but may also be an overestimation of the true effects as exponential models do not consider cumulative incidence.

To take into account composition effects in non-public insurance as well as to test for dynamic selection on outflow risk[18], I re-estimate my model for employees that switch insurance status, which, prior to

16

17 I only present the effect of non-public insurance at DI inflow for conciseness. A5.2 contains a full coefficient list.

18 In appendix A5.3, I also do not find evidence of selection on the basis of inflow risk.

2017, can occur if firms switch insurance status during the employee's disability spell. To this end, I estimate four parameters: First, the baseline effect of non-public DI. Second, I estimate a time-varying dummy that equals 0 before the firm opts out, and 1 after the firm opts out. Third and fourth, I estimate the effect of the year before switching to/from non-public DI. In case dynamic selection is present, the year-prior effects are significantly nonzero.

Dependent variable: DI outflow to...	Total	Work	Recovery without work	Structural DI	Other reasons
Non-publicly insured at inflow	0.461***	0.139***	0.595***	0.648***	0.0351
	(0.0160)	(0.0428)	(0.0528)	(0.0215)	(0.0352)
Switch to non-public insurance	0.354***	0.104*	0.356***	0.527***	0.0846*
	(0.0221)	(0.0554)	(0.0775)	(0.0303)	(0.0449)
Year before switch to non-public insurance	0.157**	0.0238	0.145	0.338***	-0.259*
	(0.0632)	(0.137)	(0.247)	(0.0834)	(0.148)
Year before switch to public insurance	0.040	0.003	0.0377	0.041	0.110
	(0.0330)	(0.0935)	(0.0966)	(0.0408)	(0.0918)
Controls	yes	yes	yes	yes	yes
Number of spells	37,415	37,415	37,415	37,415	37,415
Number of exits	27,976	3,725	2,667	16,780	5,205

Table 5.4: Piecewise-constant estimates of dynamic selection, not accounting for cumulative incidence. Publicly insured spells that did not switch insurance status form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5.4 shows that DI outflow rates for non-publicly insured spells by enrollment are higher than for publicly insured DI spells. Outflow seems to increase at the moment of switching in model 1. Short-run outflow effects after switching are present for all outflow reasons. This may be driven by firms immediately becoming more active in requesting re-assessments after switching. I find evidence of dynamic selection for total outflow and outflow to full DI. This dynamic selection for total outflow is likely driven by dynamic selection in full DI, as full DI is the main outflow reason.

However, these estimates do not yet have a direct interpretation since they are measured as hazard rates and do not account for cumulative incidence. To provide a clean interpretation, I predict outflow rates by outflow reason on the basis of Table 5.4 while accounting for cumulative incidence. I utilize the 'switch to non-public insurance' parameter to identify causal effects, as employees who switch insurance status preclude adverse selection by the employee and employer. I compute outflow rates of non-public vs public insurance as the estimated outflow to work if all disabled workers switched from public to non-public insurance during their DI spell, as opposed to outflow rates if all spells are publicly insured, no dynamic selection is present, and none of the disabled workers switched from public to non-public insurance.[19]. I perform the same strategy for outflow to recovery without work, outflow to full disability, and outflow for other reasons. To compute single effects for every year since inflow, I compute the average predicted outflow rates for spells that started between 2006 and 2016[20], accounting for cumulative incidence.

[19]As a sensitivity analysis, I employ an auxiliary identification strategy based on the findings in 5.3. In this approach, I focus on firms that switch insurance status to or from public insurance during the employee's Disability Insurance (DI) spell. This selection aims to mitigate potential long-term adverse selection effects. I then compare two scenarios: one where all DI recipients are publicly insured upon entry versus one where all DI recipients are non-publicly insured upon entry. The predicted outflow rates resulting from this sensitivity analysis are provided in Appendix A5.4. This analysis also allows for the examination of non-switchers, allowing me to disentangle composition effects and causal mechanisms. To match the data used for the main outflow rates, I select on start years prior to 2017 and large firms.

[20]Start years after 2016 are not included as firms switching insurance status after 2016 no longer entails the employee switching insurance status as well.

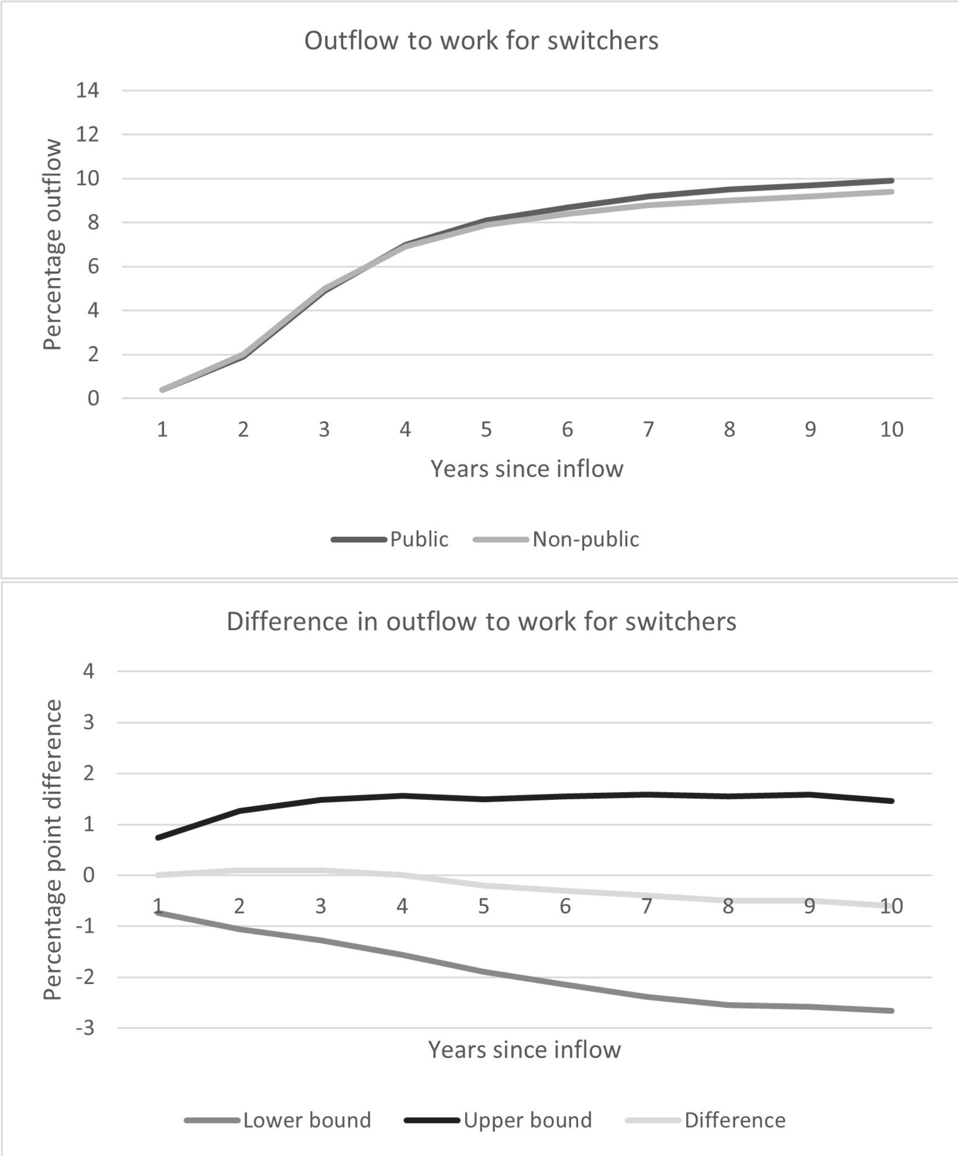

Figure 5.6: Estimated DI outflow to work within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

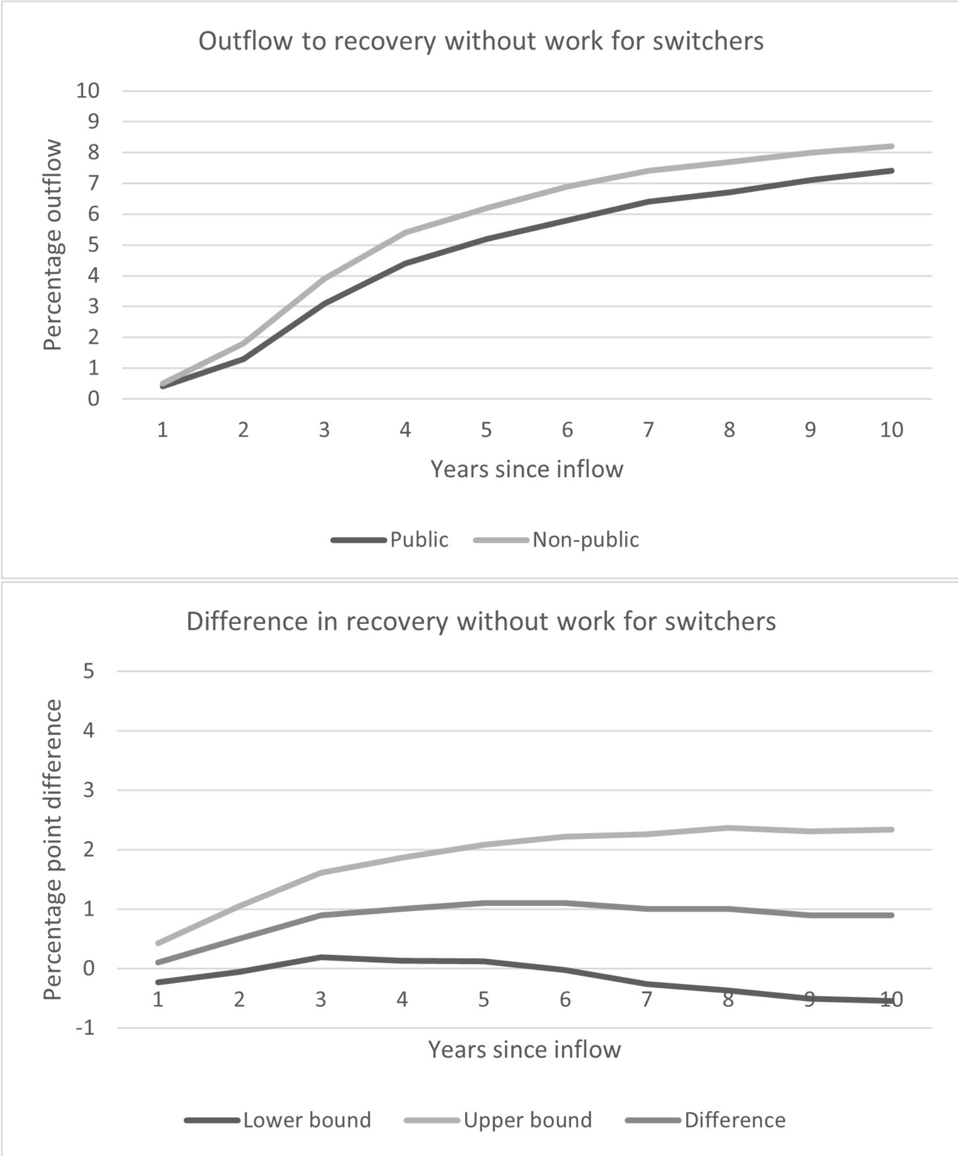

Figure 5.7: Estimated DI outflow to recovery without work within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

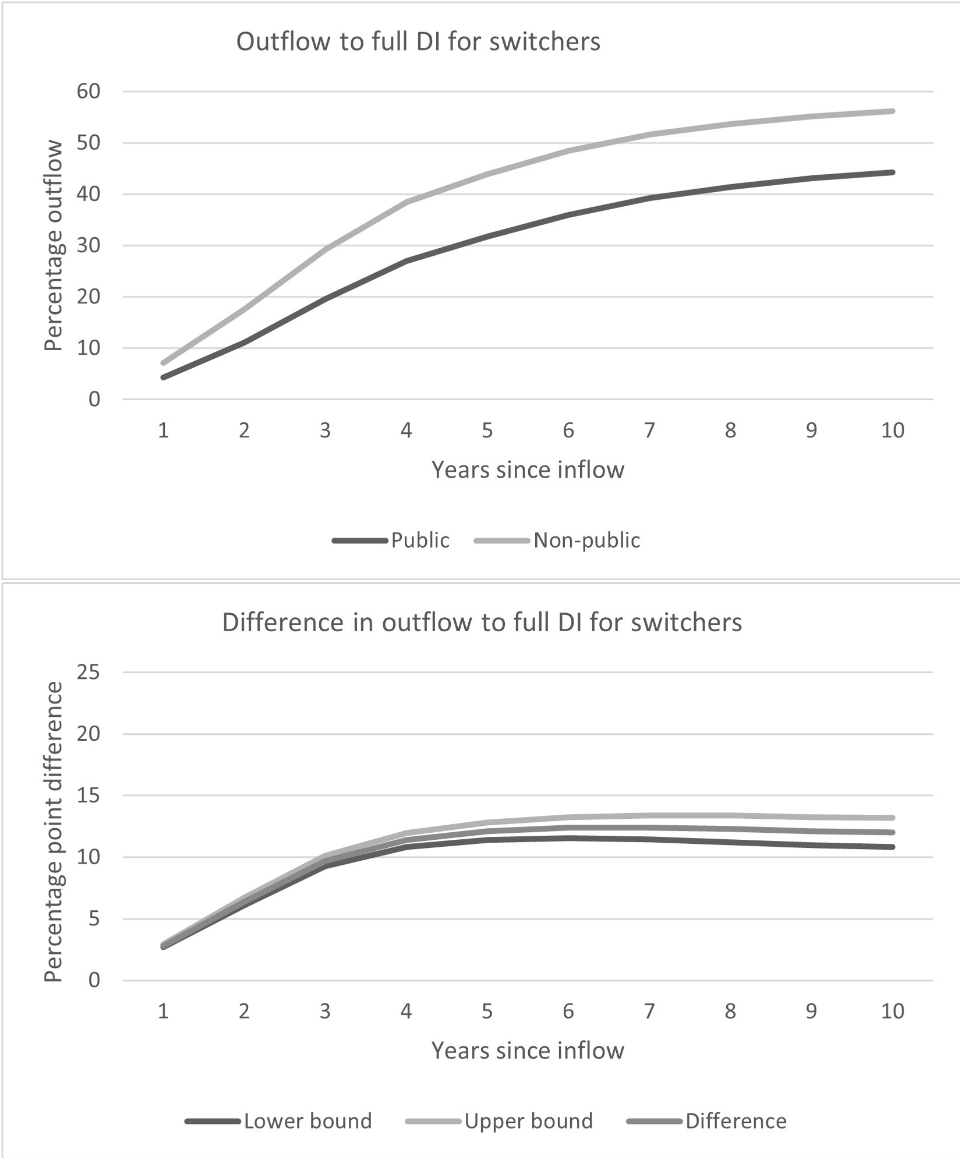

Figure 5.8: Estimated DI outflow to full DI within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

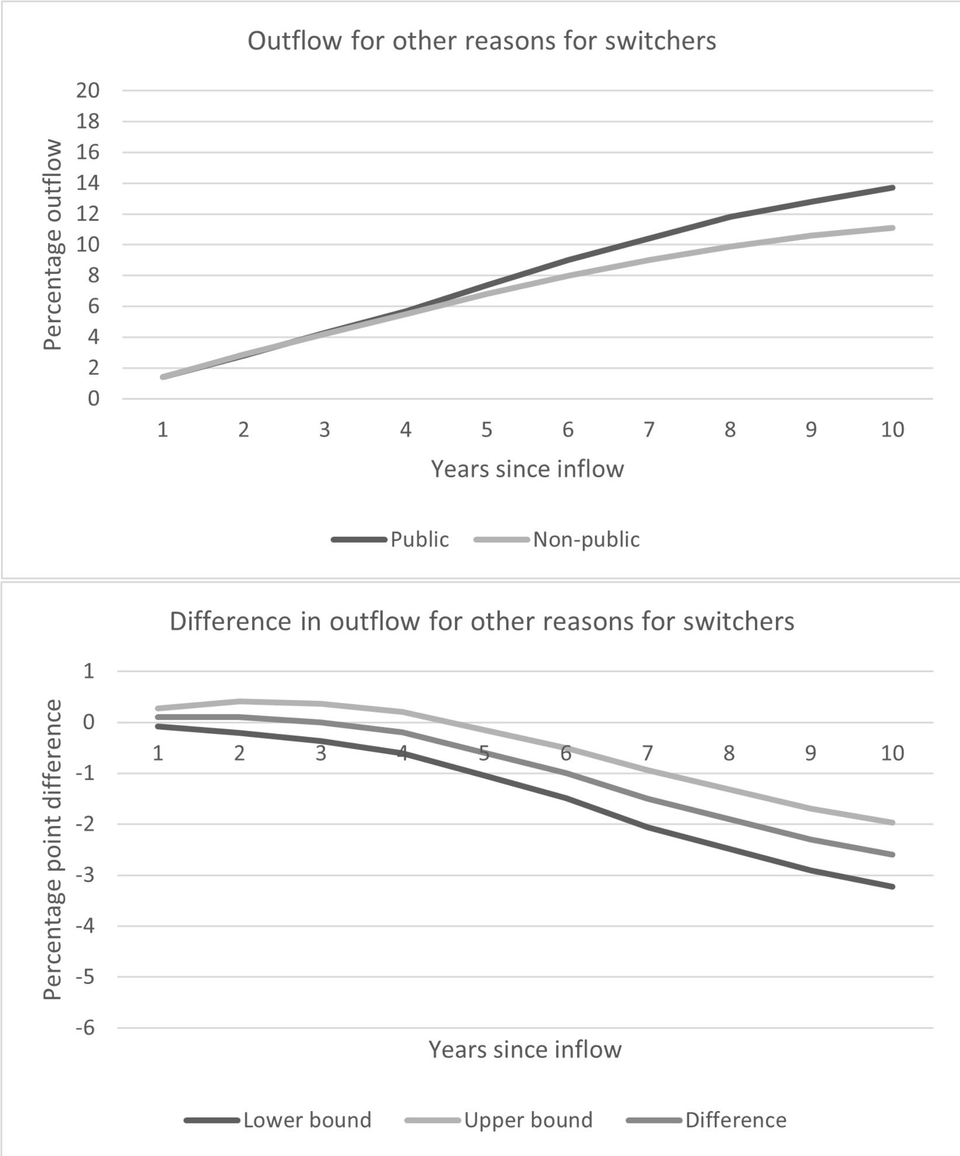

Figure 5.9: Estimated DI outflow for reasons such as retirement within a given number of years, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figures 5.6, 5.7, 5.8, and 5.9 depict estimates of outflow for each of the competing risks in the data. For each of these results, the estimates for switchers capture causal mechanisms, whereas the estimates for non-switchers capture both causal mechanisms and composition effects.

Regarding outflow to work, approximately 10% of spells for end in this manner. Differences on the basis of insurance status, insofar as they exist, are small and statistically insignificant. Consequently, there is no discernible causal evidence suggesting that non-public disability insurance leads to increased outflow to work. Rather, variations appear to be driven by composition effects at the employee and employer levels.

Examining outflow to recovery without work, 8% of non-publicly insured switchers and 7% of all publicly insured spells among switchers end in this manner. In the first three to five years of the disability spell, outflow to recovery without work for non-publicly insured spells is significantly higher than for publicly insured spells. However, these differences are no longer statistically significant after 5 years.

For outflow to full DI, 45% of publicly insured spells and roughly 55% of publicly insured switchers transition to full DI. These differences are large and statistically significant, suggesting that outflow to full DI is driven by a blend of causal mechanisms and composition effects.

Lastly, outflow for other reasons, such as retirement, accounts for 14% of publicly insured spells and 11% for non-publicly insured spells. Once more, the differences are significantly nonzero, indicating the presence of both causal mechanisms and composition effects. The difference in outflow for other reasons is likely driven by spells ending in full DI before individuals retire.

5.6.2 Outflow to Work by Current or Different Cmployer

Outflow to work can manifest in two forms. First, disabled workers can work elsewhere. Second, workers can find work at their existing employer. Non-publicly insured employers likely have more resources available to facilitate the latter outflow. To estimate this, I estimate a second set of duration models. I separate outflow to work based on whether the former DI recipient finds work with the employer they became disabled at and at a different employer from the one they became disabled at, respectively.

Dependent variable: DI outflow to...	Work at same employer	Work at different employer
Non-publicly insured at DI inflow	0.343***	0.227***
	(0.0313)	(0.0263)
Controls	yes	yes
Number of spells	182,469	182,469
Number of exits	5,497	7,657

Table 5.5: Piecewise-constant estimates of outflow by outflow reason, not accounting for cumulative incidence. Publicly insured spells form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.5 displays estimates of outflow to work on the basis of DI insurance types at the same employer. The number of exits in Table 5 is higher than in Table 4 as the outcome measure estimated now also includes work at a different employer. The outflow rate for employees returning to work at the same employer where they fell ill is higher among non-publicly insured employers than among publicly insured employers. These effects may be indicative of substitution: Firms that opt-out of public DI may create jobs for disabled workers within their own firms, as opposed to finding work elsewhere. Note that estimates for full DI and other reasons are identical to those reported in Table 5.3, as the spells in the first two columns of Table 5.3 sum up to the same spells as the spells in the first two columns of Table 5.5.

I re-estimate the dynamic selection model using outflow to work at the same employer as an outcome measure instead. Selection effects may differ here as employers may be able to create jobs within their own firms, creating a stronger basis for dynamic selection.

Dependent variable: DI outflow to...	Work at same employer	Work at different employer
Non-publicly insured at inflow	0.116*	0.160***
	(0.0600)	(0.0612)
Switch to non-public insurance	0.206***	-0.0488
	(0.0727)	(0.0859)
Year before switch to non-public insurance	0.216	-0.756**
	(0.151)	(0.342)
Year before switch to public insurance	0.0701	-0.0624
	(0.119)	(0.153)
Controls	yes	yes
Number of spells	37,415	37,415
Number of exits	2,006	1,719

Table 5.6: Piecewise-constant estimates of dynamic selection, not accounting for cumulative incidence. Publicly insured spells that did not switch insurance status form the reference category. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5.6 demonstrates higher outflow rates to work at the same employer among non-publicly insured DI spells than publicly insured DI spells. Furthermore, in the year of switching to non-public insurance, outflow to work at the same employer increases. In contrast, no effects of switching are present for work at different employers, and there is even evidence of negative dynamic selection on this type of outflow. These findings complement the estimates in Table 5.5 and Table 5.4. Not only is the effect of outflow to work at the same employer larger than the effect of outflow to work in general, there are also short-run effects after switching to non-public insurance in finding work at the same employer not present for work resumption in general. This may be incentive-driven: Non-publicly insured employers have incentives to themselves create employment opportunities for the workers they re-integrate. I do not observe any outflow-based anticipation effects when factoring in controls. The results suggest that while outflow among non-publicly insured employers is higher than among publicly insured employers. These differences are likely driven by non-publicly insured employers both creating jobs within their own firms and re-assessing disabled workers more actively immediately after (or even before) switching.

5.6.3 Re-assessments to a lower degree of disability

I also compute the percentage of spells re-assessed to a lower degree of disability, while accounting for cumulative incidence resulting from outflow from DI.

Figure 5.10: Estimated re-assessments to a lower degree of disability during the DI spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

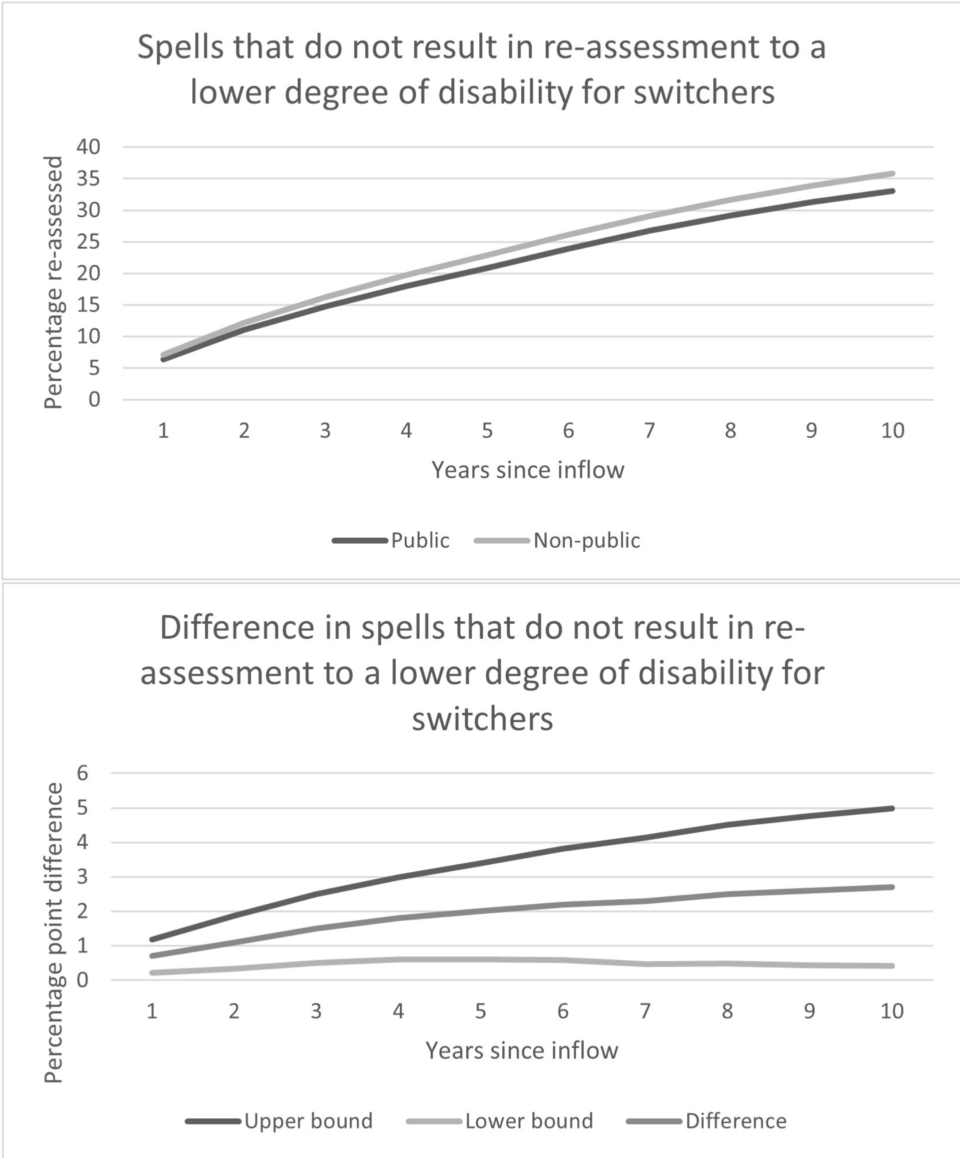

Figure 5.11: Estimated spells that do not result in re-assessments to a lower degree of disability during the DI spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figure 5.10 illustrates the predicted percentage of spells that result in re-assessments to a lower degree of disability, with spell that do not result in a re-assessment to a lower degree of disability as a competing risk. Figure 5.11 illustrates the opposite. I do not discover evidence that non-publicly insured spells are re-assessed to a lower degree of disability more often, but do find evidence of more non-publicly insured spells ending without a re-assessment to a lower degree of disability.

5.6.4 Re-assessments

The differences in outflow seem mainly driven by active re-assessment on behalf of non-public insurers. I test for this by estimating the number of re-assessments on the basis of insurance status. To this end, I estimate the number of reassessments by reassessment type that took place in the sample using both Ordinary Least Squares (OLS) and (firm) Fixed Effects (FE) models, using the same controls as in the previous estimates. These models use the number of re-assessments in the sample as the dependent variable.

	Dependent variable					
	Partial to Temp DI	Temp to Partial DI	Temp/Partial to Recovery	Partial to Struct DI	Temp to Struct DI	Total
OLS estimates						
Non-public DI	1,051***	3,766***	7,296***	1,605***	14,102***	27,820***
	(228)	(278)	(334)	(190)	(486)	(786)
Observations	180,253	180,253	180,253	180,253	180,253	180,253
Fixed Effects estimates						
Non-public DI	132	2,345***	5,098***	766**	8,949***	17,290***
	(402.2)	(499.6)	(550.7)	(342.7)	(828.6)	(1,246)
Number of firms	59,304	59,304	59,304	59,304	59,304	59,304
Observations	180,253	180,253	180,253	180,253	180,253	180,253

Table 5.7: Estimates of number of DI spells successfully Re-assessed in the sample, separated by re-assessment type. Estimated with OLS and FE. FE estimates include firm-fixed effects. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Partial to temporary measures re-assessments from partial DI to full DI. Temporary to partial measures re-assessments from temporary full DI to partial DI. Temporary/partial to recovery measures re-assessments from non-structural DI to recovery. Partial to structural DI measures re-assessments from partial DI to structural full DI. Temporary to structural measures re-assessments from temporary full DI to structural full DI. Firm-level clustered standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5.7 shows OLS and FE estimates of re-assessed individuals in the sample. I find no differences in re-assessments from partial DI to temporary full DI after controlling for fixed effects. However, I discover a higher number of re-assessments among the remaining four types, which are all forms of re-assessments that lower DI payments for non-publicly insured firms. Two mechanisms drive these results. First, there exists a higher degree of activity in re-assessments among non-publicly insured firms, and may indicate non-publicly insured firms more effectively monitoring and requesting re-assessments than publicly insured firms, leading to updates in disabled workers' health status being captured more swiftly. Second, re-assessments requested by private parties are prioritized by the Dutch Employee Insurance agency, and thus processed more swiftly.

All in all, I conclude few to no differences in outflow to work, but find large differences in outflow to full DI and a small decrease of one's degree of disability as a consequence of the opting-out decision of the employer. This is indicative that the higher outflow to work among non-publicly insured firms is a composition effect, but there may be causal effects present in the other outflow types. The outflow to full DI may be a result of spells with no prospect of recovery ending, but may also indicate incentives to shift benefit recipients to the public system. The causal differences in outflow between public and non-public differences are likely driven by more active re-assessments for non-publicly insured spells.

5.7 Conclusion

In this chapter, I estimate how non-public DI insurance affects outflow to work using a rich administrative dataset from the Dutch Employee Insurance Agency. In doing so, I estimate piecewise constant duration models, and predict outflow from DI by outflow reason while accounting for cumulative incidence. The competing risks here include recovery without work, outflow to full disability, and outflow for other reasons. The data allow me to control for a rich set of employee- and employer-characteristics, limiting the scope for endogeneity.

I find differences in outflow rates to work among non-publicly insured workers, but these differences are entirely driven by composition effects: after eliminating these composition effects, no statistically significant differences in outflow to work are present. However, I find non-selection-based differences in recovery without work. These results suggest that while non-public disability insurance does lead to re-assessments to lower degrees of disability, this does not entail more outflow to work.

There are also large differences in outflow to full disability and outflow for reasons such as retirement: Outflow to full disability is higher among non-publicly insured employers, whereas outflow for reasons such as retirement is lower among non-publicly insured employers. This effect is likely driven by spells ending in full DI before individuals reach the point of retirement.

Finally, I estimate re-assessments to a lower degree of disability in addition to the aforementioned outflow effects. I do not discover causal evidence of differences in the number of re-assessments to a lower degree of disability for non-public insurance. These findings may indicate stronger rehabilitation efforts on both the extensive margin as opposed to the intensive margin.

My findings indicate that outflow is primarily driven by direct exits from partial and partial and temporary disability rather than mechanical channels such as retirement. Non-public insurers may be more efficient at rehabilitating disabled workers, and may create jobs for disabled workers within their firms. However, most of the outflow in my findings is to structural disability.

The outflow results are primarily driven by differences in re-assessments: Re-assessments are more common in non-publicly insured firms. Particularly outflow to full disability exhibits differences between publicly and non-publicly insured firms in the form of re-assessments from temporary full disability to full disability. Smaller but nonetheless remarkable differences are also present in re-assessments from temporary full disability to partial disability. The types of re-assessments non-public parties realize are those that reduce DI premiums for themselves, indicating that incentives to request re-assessments may facilitate more effective reintegration. However, this may also lead to fewer re-assessments for workers who experience an increase in their non-structural degree of disability.

The results have several welfare implications. First, reintegration efforts among firms that opt out of public insurance are partially driven by composition effects, as the decision to opt out is an outcome of the firm's risk profile and options to reintegrate disabled workers. Second, the full DI outflow differences may be beneficial for disabled workers, as they no longer have to reintegrate and receive higher benefits. However, this also entails these workers exiting the labor market altogether. The additional outflow to full DI also entails higher overall public DI benefit payments. Differences in re-assessments to full disability create the follow-up question of whether workers re-assessed to full disability had any remaining recovery potential. However, I cannot compute welfare effects as I do not observe the counterfactual outcome in case of workers not being re-assessed, and I do not observe individuals after they exit DI.

For future research, investigating the mechanisms behind re-assessments may be of interest: While I observe more re-assessments and can to some degree correct for selection in this phenomenon, I cannot investigate the process that leads to these re-assessments being more common. Further investigating workers after they exit DI on the basis of insurance type may be of interest for future research.

All in all, I do not find causal evidence of outflow to work on the basis of non-public DI provision. However, I discover differences in other outflow types, with differences in outflow to full DI and outflow for reasons such as retirement being especially large, the latter due to spells ending as a result of re-assessments before the aforementioned individuals reach the retirement age. These findings are driven by non-public insurers requesting re-assessments more actively, leading to DI exits through both lower degrees of disability and higher degrees of disability, with the latter being most prevalent. This phenomenon primarily stems from non-public firms being more active in shifting workers with no recovery potential out of partial DI.

A5 Appendices

A5.1 OLS and FE estimates of outflow

Variables	Timeframe (years) 1	2	3	4	5	6
Outflow to Work						
OLS: Non-public insurance	23.44	44.71	349.5**	268.4	187.2	135.8
	(34.18)	(75.53)	(145.1)	(170.0)	(180.2)	(187.6)
OLS: Constant	-495.3	-2,866***	-4,947***	-5,262***	-5,367***	-5,329***
	(439.1)	(662.0)	(896.7)	(1,190)	(1,414)	(1,443)
FE: Non-public insurance	21.67	59.00	171.6	177.4	208.4	142.6
	(57.22)	(213.6)	(253.1)	(264.6)	(288.4)	(299.7)
FE: Constant	-180.7	-2,028***	-3,179**	-2,580*	-3,192**	-2,817*
	(635.6)	(784.4)	(1,242)	(1,538)	(1,579)	(1,626)
Outflow to Recovery without Work						
OLS: Non-public insurance	-22.02	311.1**	1,220***	1,400***	1,466***	1,495***
	(53.29)	(126.1)	(235.2)	(277.9)	(296.8)	(309.1)
OLS: Constant	-205.0	-3,758***	-6,108***	-6,968***	-7,746***	-7,639***
	(651.4)	(929.2)	(1,299)	(1,544)	(1,741)	(1,861)
FE: Non-public insurance	95.39	369.5*	1,053***	1,410***	1,615***	1,717***
	(84.11)	(224.0)	(288.5)	(319.8)	(348.9)	(361.6)
FE: Constant	471.1	-2,113*	-2,978*	-2,388	-3,242	-2,445
	(865.5)	(1,085)	(1,767)	(2,070)	(2,138)	(2,310)
Outflow to Full DI						
OLS: Non-public insurance	1,489***	4,912***	8,501***	10,087***	10,887***	11,391***
	(233.3)	(485.8)	(717.3)	(782.6)	(828.0)	(859.8)
OLS: Constant	6,768***	16,601***	22,149***	26,463***	28,327***	28,845***
	(1,256)	(2,010)	(2,444)	(2,746)	(2,886)	(2,973)
FE: Non-public insurance	1,262***	4,467***	7,003***	8,034***	8,606***	8,631***
	(218.3)	(387.3)	(477.0)	(521.3)	(564.6)	(599.9)
FE: Constant	8,918***	21,317***	27,861***	33,057***	35,513***	36,259***
	(1,506)	(2,288)	(2,537)	(2,596)	(2,745)	(2,856)
Outflow to Other						
OLS: Non-public insurance	-328.7***	-729.4***	-1,148***	-1,683***	-2,136***	-2,582***
	(81.26)	(120.4)	(150.8)	(178.5)	(203.1)	(221.6)
OLS: Constant	7,432***	16,060***	23,308***	31,215***	38,516***	42,651***
	(1,243)	(1,471)	(1,518)	(1,665)	(1,723)	(1,744)
FE: Non-public insurance	-82.35	-159.3	-248.5	-433.3*	-518.1**	-647.3**
	(123.4)	(182.2)	(208.1)	(227.9)	(243.0)	(258.1)
FE: Constant	6,553***	13,947***	20,559***	27,857***	34,511***	38,909***
	(1,412)	(1,602)	(1,715)	(1,897)	(1,947)	(1,991)

Table A5.1: OLS and Fixed effects estimates of outflow within a given timeframe. Clustered standard errors in parentheses. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender.

Table A5.1 shows OLS and Fixed Effects estimates of the number of individuals flowing out of DI within the sample by outflow reason within 1, 2, 3, 4, 5, and 6 years after inflow, using the same controls as in the rest of the paper. OLS and Fixed Effects estimates generally do not reveal additional outflow to work as a result of non-public DI. Recovery without work, meanwhile, is substantially higher among non-publicly insured spells than among publicly insured spells, with approximately 10000 more non-publicly insured spells ending in full DI for non-publicly insured as compared to publicly insured ones. Outflow for other reasons, meanwhile, is lower, though FE models find smaller differences. The estimates in Table A5.1 roughly match the effects I find in my cumulative incidence estimates.

A5.2 Full coefficient list of main estimates

Dependent variable: Outflow to:	Total	Work	Recovery without work	Full DI	Other
Non-public insurance	0.515***	0.271***	0.577***	0.734***	-0.0260*
	(0.00702)	(0.0201)	(0.0220)	(0.00932)	(0.0157)
Benefit %: 28	-0.359***	0.977***	0.0504	-0.976***	0.00681
	(0.0111)	(0.0251)	(0.0343)	(0.0180)	(0.0208)
Benefit %: 35	-0.397***	0.659***	-0.269***	-0.841***	0.0253
	(0.0118)	(0.0291)	(0.0415)	(0.0178)	(0.0210)
Benefit %: 42	-0.369***	0.567***	-0.286***	-0.715***	-0.0142
	(0.0138)	(0.0337)	(0.0480)	(0.0203)	(0.0253)
Benefit %: 50.75	-0.325***	0.483***	-0.371***	-0.587***	-0.0212
	(0.0134)	(0.0349)	(0.0510)	(0.0189)	(0.0248)
Benefit %: 70	-0.135***	0.117***	0.0631	-0.123***	-0.0895***
	(0.0134)	(0.0367)	(0.0392)	(0.0186)	(0.0257)
Labor market area: 00	0.0614**	-0.0546	0.232***	0.0748**	0.0554
	(0.0260)	(0.0689)	(0.0873)	(0.0351)	(0.0524)
Labor market area: 01	0.135***	0.0980	0.0802	0.211***	-0.0231
	(0.0324)	(0.0867)	(0.111)	(0.0429)	(0.0693)
Labor market area: 02	0.189***	0.317***	0.420***	0.184***	0.0711
	(0.0344)	(0.0888)	(0.114)	(0.0466)	(0.0711)
Labor market area: 03	0.0727**	-0.0649	-0.00200	0.178***	-0.0449
	(0.0341)	(0.0946)	(0.119)	(0.0447)	(0.0715)
Labor market area: 05	0.0982***	0.0341	0.162*	0.122***	0.00594
	(0.0291)	(0.0769)	(0.0968)	(0.0390)	(0.0604)
Labor market area: 06	0.0461	-0.0324	0.0718	0.0744*	0.00427
	(0.0289)	(0.0773)	(0.0973)	(0.0386)	(0.0594)
Labor market area: 07	0.0436	0.0547	0.0919	0.0444	0.0269

	(0.0320)	(0.0841)	(0.107)	(0.0431)	(0.0659)
Labor market area: 08	-0.0493	-0.0859	0.0131	-0.0207	-0.113
	(0.0355)	(0.0973)	(0.120)	(0.0473)	(0.0725)
Labor market area: 09	0.0126	0.0574	0.128	-0.0227	0.0269
	(0.0342)	(0.0917)	(0.114)	(0.0461)	(0.0690)
Labor market area: 10	-0.00169	0.00362	0.0907	-0.0297	0.00748
	(0.0386)	(0.104)	(0.130)	(0.0523)	(0.0784)
Labor market area: 11	-0.0443	0.0724	0.318***	-0.166***	0.0318
	(0.0310)	(0.0802)	(0.0988)	(0.0428)	(0.0617)
Labor market area: 12	-0.0685*	0.0882	0.244*	-0.232***	0.0150
	(0.0397)	(0.103)	(0.126)	(0.0564)	(0.0759)
Labor market area: 13	-0.134***	-0.103	0.00504	-0.201***	-0.0140
	(0.0288)	(0.0751)	(0.0949)	(0.0394)	(0.0575)
Labor market area: 14	-0.0898***	0.0461	0.199*	-0.227***	0.0931
	(0.0333)	(0.0847)	(0.105)	(0.0463)	(0.0658)
Labor market area: 15	0.0597**	-0.0130	-0.0435	0.0955**	0.0307
	(0.0288)	(0.0764)	(0.0992)	(0.0387)	(0.0591)
Labor market area: 17	0.0382	-0.0177	-0.0699	0.0891**	-0.0681
	(0.0312)	(0.0829)	(0.108)	(0.0417)	(0.0651)
Labor market area: 18	-0.222***	-0.288***	0.176**	-0.386***	0.0449
	(0.0272)	(0.0715)	(0.0885)	(0.0375)	(0.0536)
Labor market area: 19	-0.0500	-0.0237	0.210**	-0.175***	0.109*
	(0.0316)	(0.0837)	(0.103)	(0.0440)	(0.0615)
Labor market area: 20	-0.0821*	-0.0424	0.172	-0.219***	0.168*
	(0.0475)	(0.125)	(0.146)	(0.0678)	(0.0891)
Labor market area: 21	-0.120***	-0.211***	0.182**	-0.222***	0.0520
	(0.0283)	(0.0757)	(0.0924)	(0.0386)	(0.0562)
Labor market area: 22	-0.0343	-0.327***	0.164*	-0.0223	0.0113
	(0.0275)	(0.0748)	(0.0912)	(0.0371)	(0.0554)
Labor market area: 23	-0.0464	-0.261***	-0.193*	0.0330	-0.0496
	(0.0341)	(0.0940)	(0.116)	(0.0451)	(0.0711)
Labor market area: 24	0.0616*	-0.0152	0.0163	0.0859**	0.0525
	(0.0324)	(0.0848)	(0.110)	(0.0436)	(0.0666)
Labor market area: 25	0.0633**	-0.0710	0.294***	0.0790**	-0.0203
	(0.0294)	(0.0787)	(0.0958)	(0.0397)	(0.0605)
Labor market area: 26	-0.133***	-0.250***	0.261***	-0.264***	0.0494
	(0.0307)	(0.0834)	(0.0977)	(0.0425)	(0.0601)
Labor market area: 27	-0.0580*	-0.265***	0.114	-0.0684*	0.00569
	(0.0304)	(0.0836)	(0.0991)	(0.0411)	(0.0612)
Labor market area: 28	-0.129***	-0.216**	0.161	-0.202***	-0.0131
	(0.0312)	(0.0841)	(0.100)	(0.0427)	(0.0623)
Labor market area: 29	-0.193***	-0.243**	0.257**	-0.424***	0.0815
	(0.0367)	(0.103)	(0.114)	(0.0537)	(0.0668)
Labor market area: 30	0.0734***	-0.302***	-0.207**	0.224***	-0.0580
	(0.0279)	(0.0788)	(0.0971)	(0.0370)	(0.0579)
Labor market area: 32	-0.142***	-0.169*	0.0975	-0.243***	-0.0385
	(0.0353)	(0.0954)	(0.114)	(0.0490)	(0.0694)
Labor market area: 33	-0.0853***	-0.376***	-0.276**	-0.0400	0.0227
	(0.0328)	(0.0956)	(0.118)	(0.0436)	(0.0653)

Labor market area: 34	-0.0883***	-0.149*	0.0901	-0.161***	0.0869
	(0.0332)	(0.0892)	(0.110)	(0.0454)	(0.0643)
Labor market area: 35	-0.0454	-0.101	-0.00358	-0.0109	-0.0640
	(0.0674)	(0.183)	(0.209)	(0.0904)	(0.140)
Labor market area: 36	0.0639**	-0.0138	-0.00312	0.0806*	0.0174
	(0.0310)	(0.0823)	(0.107)	(0.0417)	(0.0632)
Labor market area: 37	-0.0918***	-0.134	0.0184	-0.152***	0.0212
	(0.0319)	(0.0856)	(0.106)	(0.0435)	(0.0634)
Sector: Agriculture/nutrition	0.112	0.671***	0.744***	-0.0752	-0.128
	(0.0736)	(0.247)	(0.272)	(0.0976)	(0.135)
Sector: Construction/wood	0.0672	0.501**	0.465*	-0.0785	-0.153
	(0.0737)	(0.247)	(0.274)	(0.0977)	(0.135)
Sector: Industry	0.107	0.782***	0.679**	-0.1000	-0.111
	(0.0727)	(0.244)	(0.269)	(0.0963)	(0.133)
Sector: Retail	0.144**	0.592**	0.781***	-0.0772	0.0135
	(0.0726)	(0.244)	(0.269)	(0.0963)	(0.133)
Sector: Transport	0.174**	0.964***	0.692**	-0.0623	-0.0315
	(0.0732)	(0.245)	(0.271)	(0.0970)	(0.134)
Sector: Healthcare	0.328***	1.023***	0.809***	0.171*	-0.0834
	(0.0725)	(0.244)	(0.269)	(0.0960)	(0.133)
Sector: Government education	0.146**	0.957***	0.397	-0.151	-0.0356
	(0.0736)	(0.246)	(0.274)	(0.0981)	(0.134)
Sector: Government, other	0.154**	1.010***	0.128	-0.0584	-0.0416
	(0.0733)	(0.245)	(0.274)	(0.0973)	(0.134)
Sector: Financial services	0.115	0.848***	0.563**	-0.120	-0.0445
	(0.0728)	(0.244)	(0.269)	(0.0965)	(0.133)
Sector: Temporary work	0.141*	0.498**	1.091***	-0.126	0.0124
	(0.0757)	(0.253)	(0.273)	(0.101)	(0.140)
Sector: Other	0.165**	0.326	0.941***	-0.0301	-0.0246
	(0.0728)	(0.246)	(0.269)	(0.0964)	(0.133)
Diagnosis type: Unknown	0.315***	1.129***	1.324***	-0.331***	0.710***
	(0.0155)	(0.0419)	(0.0458)	(0.0246)	(0.0277)
Diagnosis type: Heart disease	0.172***	0.697***	-0.00721	-0.0764***	0.507***
	(0.0122)	(0.0344)	(0.0565)	(0.0164)	(0.0234)
Diagnosis type: Cancer	-0.0122	-0.0550	-0.0445	-0.110***	0.160***
	(0.0135)	(0.0480)	(0.0639)	(0.0177)	(0.0250)
Diagnosis type: Psychological	-0.297***	0.0455*	0.356***	-0.524***	-0.0870***
	(0.00930)	(0.0264)	(0.0327)	(0.0121)	(0.0202)
Diagnosis type: Musculoskeletal	-0.134***	0.211***	0.491***	-0.315***	-0.0324
	(0.00942)	(0.0277)	(0.0343)	(0.0122)	(0.0201)
Age category: $\leq$ 30	-1.214***	2.348***	1.472***	-1.066***	-3.607***
	(0.0171)	(0.0727)	(0.0655)	(0.0281)	(0.0429)
Age category: 31 - 40	-1.217***	2.034***	1.285***	-0.813***	-3.649***

	(0.0123)	(0.0698)	(0.0616)	(0.0190)	(0.0289)
Age category: 41 - 50	-1.127***	1.625***	0.864***	-0.410***	-3.854***
	(0.0109)	(0.0694)	(0.0614)	(0.0164)	(0.0262)
Age category: 51-60	-0.796***	0.879***	0.310***	-0.105***	-2.263***
	(0.00979)	(0.0700)	(0.0621)	(0.0153)	(0.0166)
Male	0.114***	0.0320	-0.138***	0.124***	0.227***
	(0.00740)	(0.0210)	(0.0238)	(0.0101)	(0.0146)
Years disabled: <1	0.0549	-0.550***	0.314	1.446***	-1.781***
	(0.0625)	(0.196)	(0.214)	(0.0917)	(0.106)
Years disabled: 1-2	0.650***	0.886***	1.237***	1.883***	-1.467***
	(0.0575)	(0.178)	(0.198)	(0.0848)	(0.0978)
Years disabled: 2-3	1.056***	1.696***	1.890***	2.130***	-1.269***
	(0.0526)	(0.163)	(0.182)	(0.0778)	(0.0897)
Years disabled: 3-4	0.940***	1.412***	1.704***	1.956***	-1.036***
	(0.0480)	(0.149)	(0.167)	(0.0711)	(0.0816)
Years disabled: 4-5	0.757***	1.024***	1.328***	1.687***	-0.743***
	(0.0434)	(0.135)	(0.152)	(0.0646)	(0.0735)
Years disabled: 5-6	0.712***	0.836***	1.221***	1.569***	-0.458***
	(0.0389)	(0.122)	(0.137)	(0.0582)	(0.0656)
Years disabled: 6-7	0.633***	0.731***	1.033***	1.386***	-0.269***
	(0.0348)	(0.111)	(0.125)	(0.0524)	(0.0579)
Years disabled: 7-8	0.508***	0.499***	0.908***	1.142***	-0.186***
	(0.0314)	(0.103)	(0.114)	(0.0477)	(0.0509)
Years disabled: 8-9	0.391***	0.381***	0.824***	0.943***	-0.212***
	(0.0287)	(0.0982)	(0.105)	(0.0443)	(0.0457)
Years disabled: 9-10	0.238***	0.337***	0.506***	0.678***	-0.209***
	(0.0277)	(0.0965)	(0.106)	(0.0434)	(0.0423)
Inflow year: 2006	0.753***	-0.333	0.0995	1.287***	0.923***
	(0.0813)	(0.302)	(0.268)	(0.117)	(0.141)
Inflow year: 2007	0.801***	-0.342	0.140	1.454***	0.770***
	(0.0773)	(0.291)	(0.254)	(0.111)	(0.134)
Inflow year: 2008	0.669***	-0.170	0.203	1.199***	0.753***
	(0.0724)	(0.280)	(0.237)	(0.103)	(0.126)
Inflow year: 2009	0.583***	-0.259	0.106	1.072***	0.688***
	(0.0678)	(0.269)	(0.222)	(0.0963)	(0.119)
Inflow year: 2010	0.484***	-0.290	0.0683	0.895***	0.620***
	(0.0633)	(0.258)	(0.206)	(0.0895)	(0.112)
Inflow year: 2011	0.420***	-0.308	0.00256	0.806***	0.505***
	(0.0586)	(0.248)	(0.191)	(0.0825)	(0.104)
Inflow year: 2012	0.331***	-0.239	0.00704	0.661***	0.354***
	(0.0540)	(0.238)	(0.175)	(0.0757)	(0.0962)
Inflow year: 2013	0.256***	-0.213	-0.0316	0.513***	0.325***
	(0.0498)	(0.230)	(0.160)	(0.0693)	(0.0890)
Inflow year: 2014	0.179***	-0.170	-0.129	0.387***	0.239***
	(0.0459)	(0.222)	(0.147)	(0.0634)	(0.0831)
Inflow year: 2015	0.169***	-0.167	-0.0340	0.335***	0.193**
	(0.0421)	(0.215)	(0.134)	(0.0574)	(0.0775)
Inflow year: 2016	0.105***	-0.164	-0.145	0.243***	0.142**
	(0.0387)	(0.208)	(0.122)	(0.0520)	(0.0718)

Inflow year: 2017	0.0788**	-0.0342	-0.0700	0.175***	0.150**
	(0.0357)	(0.203)	(0.111)	(0.0473)	(0.0667)
Inflow year: 2018	0.0318	-0.00109	-0.0247	0.119***	0.0767
	(0.0332)	(0.199)	(0.101)	(0.0432)	(0.0629)
Inflow year: 2019	0.0610*	0.235	0.0210	0.0972**	0.129**
	(0.0313)	(0.196)	(0.0928)	(0.0402)	(0.0597)
Inflow year: 2020	0.0111	-0.0900	0.479***	-0.0468	0.0495
	(0.0313)	(0.205)	(0.0856)	(0.0399)	(0.0596)
Calendar year: 2007	-0.713***	3.385***	-0.105	-1.585***	-0.300**
	(0.0827)	(0.420)	(0.263)	(0.120)	(0.145)
Calendar year: 2008	-0.766***	4.865***	-0.990***	-1.652***	-0.304**
	(0.0756)	(0.376)	(0.247)	(0.110)	(0.134)
Calendar year: 2009	-0.866***	4.550***	-1.095***	-1.706***	-0.341***
	(0.0701)	(0.367)	(0.230)	(0.102)	(0.123)
Calendar year: 2010	-0.928***	4.435***	-1.505***	-1.659***	-0.399***
	(0.0650)	(0.359)	(0.215)	(0.0941)	(0.114)
Calendar year: 2011	-0.960***	4.289***	-1.528***	-1.665***	-0.321***
	(0.0599)	(0.351)	(0.198)	(0.0868)	(0.104)
Calendar year: 2012	-0.697***	4.952***	-1.240***	-1.477***	-0.260***
	(0.0543)	(0.342)	(0.179)	(0.0789)	(0.0949)
Calendar year: 2013	-0.535***	4.804***	-1.044***	-1.125***	-0.223***
	(0.0491)	(0.335)	(0.162)	(0.0710)	(0.0858)
Calendar year: 2014	-0.341***	4.703***	-0.844***	-0.789***	-0.142*
	(0.0440)	(0.329)	(0.144)	(0.0634)	(0.0770)
Calendar year: 2015	-0.114***	4.517***	-0.534***	-0.464***	0.0869
	(0.0389)	(0.323)	(0.127)	(0.0561)	(0.0681)
Calendar year: 2016	-0.106***	4.306***	-0.733***	-0.259***	-0.174***
	(0.0342)	(0.318)	(0.112)	(0.0491)	(0.0605)
Calendar year: 2017	-0.0494*	4.559***	-0.680***	-0.174***	-0.177***
	(0.0296)	(0.314)	(0.0959)	(0.0425)	(0.0524)
Calendar year: 2018	0.0667***	4.581***	-0.796***	0.0267	-0.0760*
	(0.0253)	(0.310)	(0.0822)	(0.0361)	(0.0451)
Calendar year: 2019	0.0986***	4.722***	-0.751***	0.0836***	-0.0895**
	(0.0217)	(0.307)	(0.0685)	(0.0306)	(0.0394)
Calendar year: 2020	0.121***	4.696***	-0.843***	-0.0126	0.252***
	(0.0185)	(0.305)	(0.0568)	(0.0264)	(0.0329)
Calendar year: 2021	0.103***	4.593***	-0.958***	-0.0455**	0.330***
	(0.0163)	(0.303)	(0.0480)	(0.0231)	(0.0291)
Constant	-2.148***	-12.27***	-7.042***	-3.913***	-0.829***
	(0.100)	(0.472)	(0.359)	(0.138)	(0.181)

Table A5.2: Full parameter list of the models estimated in Table 5.3. Baseline categories are a benefit percentage of 75, labor market area 38, sector 'unknown', diagnosis code 'other', age 61 years or older, female, disabled for 10 or more years, inflow year 2021, and calendar year 2022. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5.2 presents all coefficients of the models estimated in Table 5.3. Individuals with lower benefit percentages at inflow find work more often, and experience less outflow to full DI. Labor market area differences are present, but do not have a clean interpretation. Outflow to work is less prevalent in the 'other' category than in other sectors. Individuals with unknown disease types, heart disease, and psychological ailments experience different outflow patterns from other diagnosis categories. Younger individuals recover (irrespective of employment) more often, and experience less outflow to full DI and reasons such as retirement. Men experience more outflow to full DI and retirement. Strong duration dependence patterns are present: outflow to work is more common early in the DI spell, whereas outflow for full DI and reasons such as retirement becomes more common as spells last longer. Later inflow years results in less outflow to full DI and reasons such as retirement. Finally, earlier calendar years entail more outflow to work, but less outflow to full DI and reasons such as retirement.

A5.3 Selection on inflow risk

One remaining question entails whether there is dynamic selection based on inflow risk in DI. While this is not a threat to inference for my estimates, as I investigate outflow risk, selection on the basis of inflow risk may nonetheless inform policy. To this end, I construct the total number of spells starting in every year-month combination, and estimate how switching to and from non-public insurance affects inflow within one year with OLS and fixed effects models. These estimates are as follows:

Estimates of inflow-based dynamic selection				
Dependent variable:	Amount of new DI spells per month		Amount of new DI spells per month	
	Model 1	Model 2	Model 1	Model 2
	OLS		FE	
Non-publicly insured at inflow	9.879***	1.310***	38.19***	3.408***
	(1.782)	(0.480)	(3.351)	(0.905)
Within 1 year of switch to non-public	-64.91***	1.757	-21.99***	-0.574
	(2.334)	(1.530)	(2.223)	(1.380)
Within 1 year of switch to public	-8.511***	-3.335**	-0.773	-2.853*
	(2.800)	(1.614)	(2.631)	(1.647)
Constant	515.7***	452.3***	505.4***	688.7***
	(1.127)	(4.607)	(1.187)	(6.081)
Controls	no	yes	no	yes

Table A5.3: Estimates of inflow-based dynamic selection. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A5.3 shows estimates of dynamic selection based on DI inflow risk, with estimates within 1 year of switching indicating this type of selection, using the number of new spells as the dependent variable and using the same controls as in the rest of the paper. I do not find selection into non-public DI when inflow is low conditional on the controls used. Some weak evidence of lower inflow when switching to public insurance is present, but the order of the magnitude of these estimates are very small compared to baseline DI inflow, ranging from 3 to 9 fewer new DI spells per month. As such, I do not find meaningful inflow-based dynamic selection.

A5.4 Outflow rates on the basis of firm-switching

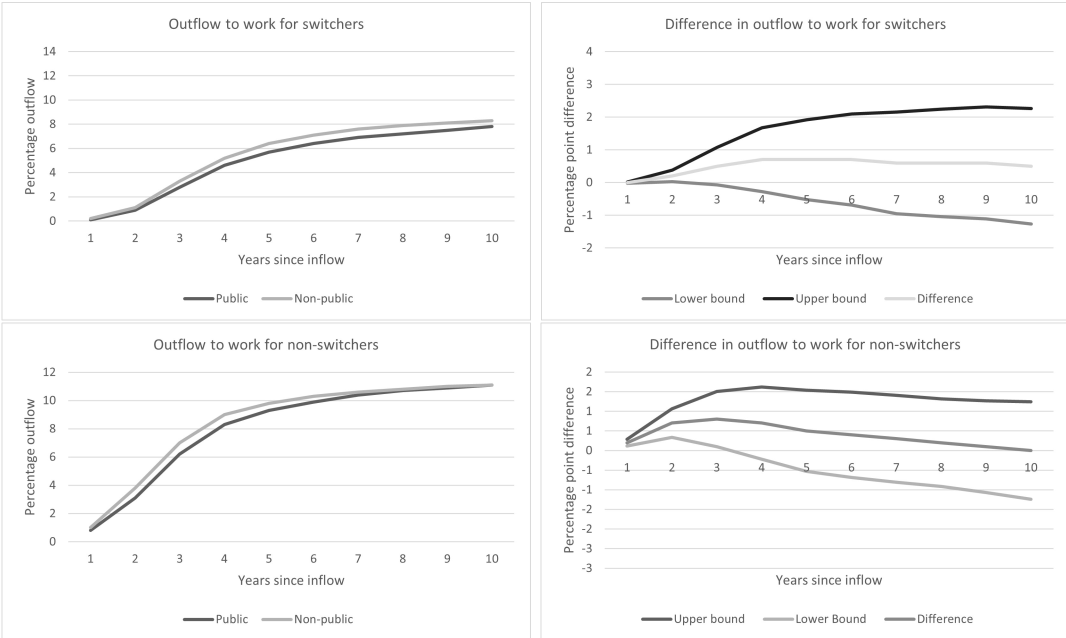

Figure A5.1: Estimated outflow to work separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

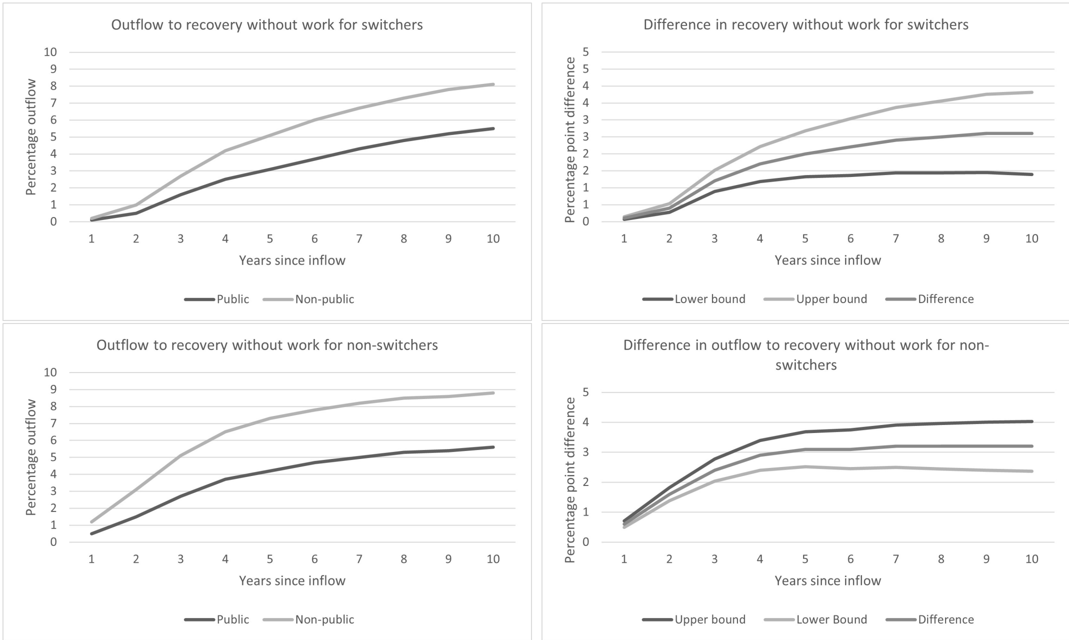

Figure A5.2: Estimated outflow to recovery without work separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

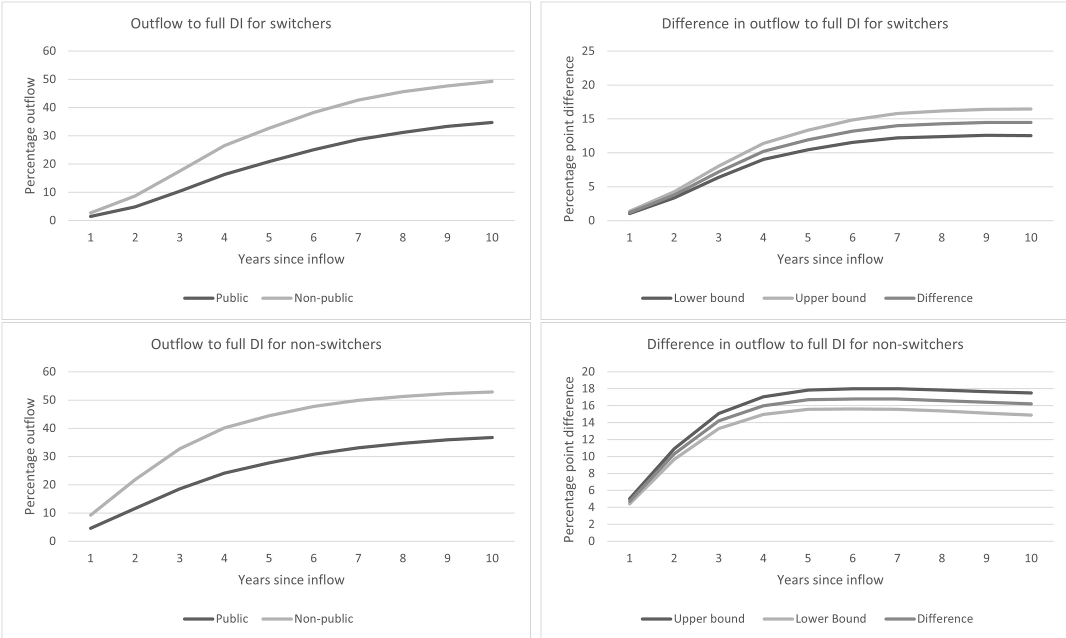

Figure A5.3: Estimated outflow to full DI separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

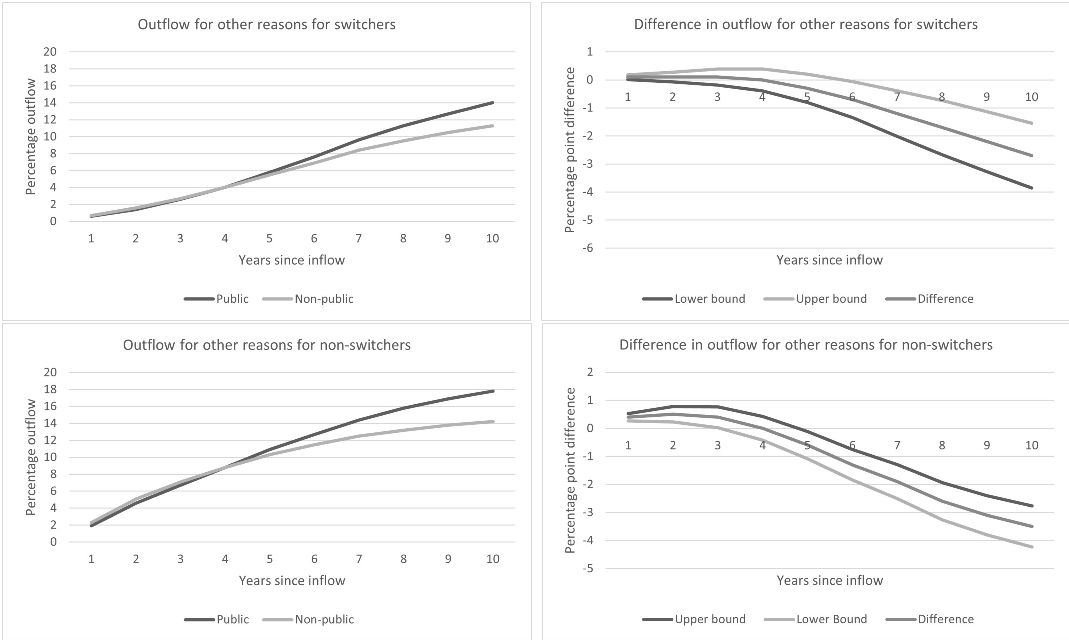

Figure A5.4: Estimated outflow for other reasons such as retirement separated by whether the firm switched insurance status during the spell, accounting for cumulative incidence of outflow reasons. Estimates control for duration dependence, inflow year, calendar year, degree of disability at inflow, region effects, industry, diagnosis category, age, and gender. Estimates compare estimated outflow in case all spells are publicly insured as compared to all spells being non-publicly insured.

Figures A5.1, A5.2, A5.3, and A5.4 illustrate outflow to work, recovery without work, full DI, and other reasons, respectively. After controlling for firm characteristics and accounting for cumulative incidence, I find no differences in outflow to work based on insurance status, although the outflow to work among non-switchers is higher than for switchers. Both groups - switchers and non-switchers - demonstrate higher outflow rates to recovery without work than switchers. Differences on the basis of insurance status are similar for switchers and non-switchers. Outflow to full DI is significantly higher for non-public firms compared to public firms, with the difference being roughly 15 percentage points. This difference is more pronounced for non-switchers, suggesting residual composition effects. Finally, outflow for other reasons such as retirement is lower for non-publicly insured firms for both switchers and non-switchers. However, the differences are again larger for non-switchers. In summary, examining firms that switch insurance status at some point in the sample as opposed to examining spells that switch to non-public insurance provides roughly equal, albeit more pronounced differences on the basis of insurance status.

As a result, differences in outflow to recovery without work are larger and significantly nonzero. Otherwise, estimates match those of figures 5.6, 5.7, 5.8, and 5.9.

Chapter 6

General Discussion

This thesis contributes to the literature on household finance and social insurance by addressing critical gaps in existing literature regarding the life cycle model and its deviations. Through empirical analysis, it offers insights into how individuals manage their finances over their lifetimes, with implications for policy design.

A central focus of this research is the examination of how households respond to financial shocks and risks. Understanding these responses can help policymakers refine social insurance programs to mitigate the adverse effects of income shocks.

Furthermore, this dissertation underscores the importance of understanding household financial management and risks associated with unexpected economic fluctuations. Understanding these dynamics can inform policymakers of the trade-offs interventions targeted at improving household finances entail.

This chapter discusses the findings of this dissertation. First, it covers the financial dynamics in relation to retirement savings, income, and expenditure (Sections 6.1 and 6.2). Second, this chapter discusses the mechanics underlying child penalties (Section 6.3). Third, this chapter discusses outflow from non-public disability insurance and the role of non-public insurance therein (Section 6.4). Fourth and finally, this chapter provides policy implications and suggestions for future research (Section 6.5)

6.1 Early money withdrawal options and reducing administrative burden for retirement

The demand for retirement products for self-employed workers can be increased by reducing the amount of administrative red tape and allowing for early money withdrawal options, particularly when income is low and to pay off mortgages.

There is a strong degree of heterogeneity between subgroups. The demand for early money withdrawal is the highest for self-employed workers, as well as low-income groups, and present-biased individuals. From employees, there is no demand for reduced administrative burden. However, there is a demand for early money withdrawal options when income is low and for mortgage payments, albeit to a lesser extent than for self-employed workers.

The demand for early money withdrawal options is subject to sophisticated present bias. Present bias refers to the tendency to prioritize immediate rewards over long-term benefits, while sophisticated present bias implies individuals are not only present biased but also aware of this bias within themselves. Retirement products that do not tie conditions to early money withdrawal options may therefore decrease their overall retirement savings rather than aid them. Particularly, withdrawal options that improve liquidity are in demand, for instance to supplement income or to pay off mortgages.

6.2 The impact of retirement on household finances

Our analysis relies on high-quality monthly transaction data that have not yet been utilized in the literature. These data allow us to capture the short-run dynamics of retirement on income and spending behavior, a phenomenon that has not been explored in existing research.

Our analysis reveals significant heterogeneity in financial effects of retirement. In the month of retirement, net flow balances (inflow minus outflow within a given month) spike. In the long run they stabilize. Chapter 3 reveals wealth accumulation effects for low-income groups, indicating that retirement helps alleviate income constraints for these groups. There are no observable wealth decumulation effects among high-income groups, suggesting that the replacement rates for high-income individuals are adequate. Finally, chapter 3 reveals that the fraction of individuals with debt decreases after retirement, particularly for low-income and low-wealth households, further indicating financial constraints being alleviated for these groups.

Our findings suggest that retirement savings are thus generally adequate and even beneficial to low-income and low-wealth groups, indicating redistributive effects of retirement. Moreover, retirement does not incur negative liquidity effects for high-income and high-wealth groups. As such, the replacement rates after retirement are generally sufficient. In addition, retirement serves as a channel of debt relief for groups with poor financial prospects.

The understanding of income and spending effects of retirement remains limited within existing literature. Particularly, the data used in existing research cannot accurately capture the dynamics underlying retirement. More frequent and more accurate data are yet needed to fully understand how retirement affects household finances. Additionally, transaction data in other institutional settings can further lay out the mechanisms underlying how retirement affects household finances

6.3 The child penalty in the Netherlands and the role of time use

Chapter 4 estimates to what extent time use in the household can explain child penalties. To do so, chapter 4 use detailed survey data from the LISS panel. These data contain not only self-reported labor market outcomes, but also self-reported time use in various forms of household and childcare activities.

Differences between men and women in the labor market are relatively small before they have children. However, after childbirth, a divergence in time use occurs: in line with existing literature, women experience a drop in earnings after childbirth that they never fully recover from. This drop is primarily driven by women reducing work hours. Time use in the household, on the other hand, is slightly higher for women prior to childbirth, and increases afterwards.

We find descriptive results that child penalties can be completely explained by the use of household time. Additionally, leisure decreases for women relative to men in the long run. These results reject the hypothesis that women trade labor market time for leisure after childbirth. As such, time use is subject to intra-household substitution.

Policies targeted at the household may be more effective in reducing child penalties than those focused on the labor market. For instance, childcare subsidies can help mothers resume their careers, thus mitigating the child penalties associated with maternal workforce absence. Similarly, parental leave for fathers can help divide household work, further alleviating child penalties. Additionally, policies can indirectly target the household. For example, implementing flexible working hours can help parents divide household work more effectively.

6.4 Outflow from non-public disability insurance

Chapter 5 examines how non-public disability insurance affects the outflow from temporary disability insurance, focusing specifically on return-to-work. The analyses use monthly disability insurance (DI) records from 2006 to 2022 from the Dutch Employee Insurance agency, supplemented with data on benefit payments, labor market activity, and re-assessments.

Initially, the data suggests a higher rate of individuals returning to work from non-publicly insured firms, even after adjusting for compositional differences between publicly and non-publicly insured cases. However, subsequent analyses that account for competing reasons for outflow (e.g., to structural disability) reveal that these differences disappear.

While the analysis does not reveal causal evidence of increased outflow to work among beneficiaries of non-public disability insurance, it does uncover effects in other types of outflow. Notably, non-publicly insured cases exhibit higher rates of outflow to recovery without work, and particularly to full disability insurance. Conversely, this group experiences a lower outflow attributed to retirement.

6.5 Policy and future research

This dissertation investigates the dynamics underlying the life-cycle model, particularly focusing on household finance and social insurance. Through a series of empirical investigations, this dissertation finds various income, spending, liquidity, labor market participation, and time use dynamics that the literature has not explored yet. These dynamics range from early money withdrawal options and reduced red tape allowing self-employed workers to save more for retirement, retirement having heterogeneous effects on financial outcomes, time use in the household descriptively explaining child penalties, and non-public disability insurance increasing outflow to full disability.

The examination of early money withdrawal options in Chapter 2 sheds light on their potential to incentivize self-employed workers to save more for retirement. Specifically, chapter 2's findings indicate that offering such options, especially during periods of low income or for mortgage repayment, can increase retirement savings. These results are compounded by the fact that early money withdrawal options are most in demand among low-income and low-wealth groups. Nevertheless, individuals with a strong preference for the present also exhibit a high demand for early money withdrawal options.

These dynamics underscore the importance of implementing means-tested conditions to early money withdrawal policies. Unrestricted access to early withdrawal may inadvertently diminish the replacement rates of certain self-employed workers instead of enhancing them. Therefore, policy interventions should carefully consider the implications of early withdrawal options and ensure that they are structured to align with long-term retirement savings goals.

Furthermore, reducing administrative red tape can potentially increase retirement savings of self-employed workers. To do so, reducing the number of legal rules may be beneficial. Specififcally, simplifying the rules with respect to one's annual contribution room may help increase self-employed workers save more for retirement. Additionally, tax authorities could provide information to annuity providers, after the self-employed worker has given permission for this. Finally, allowing individuals to purchase annuitities up to a given maximum without having to provide financial information may increase retirement savings.

However, it is essential to acknowledge that the estimates presented in Chapter 2 are based on hypothetical scenarios. To further understand the demand for retirement-related annuities, data from annuity providers may be of interest. Particularly, experiments with respect to offering early money withdrawal options in annuities and the degree to which these early money withdrawal options are used may inform both further policy.

Chapter 3 reveals the significant heterogeneity of household finance effects after retirement, revealing complexities beyond the scope of existing data. The estimates found can explain the results of papers that find evidence of a consumption drop after retirement, as well as those of papers that do not, since all of these papers utilize yearly data, which offer only a limited view of intertemporal dynamics. These findings emphasize the need for caution when formulating policy based on the current understanding of the retirement literature, as the substantial heterogeneity and short-run differences revealed in Chapter 3 are not adequately captured by existing evidence.

Savings dynamics, on average, are not affected by retirement. Only among low-income and low-wealth groups are positive end-of-month balance effects present. These findings indicate that retirement savings for employees are generally adequate to cover their living expenses, and in the case of individuals with liquidity constraints can even alleviate these constraints in the short run.

While Chapter 3 offers unprecedented monthly transaction data, it's important to acknowledge remaining data limitations. For future research, transaction data with a richer diversity of cash flows may be of interest. Likewise, more demographic characteristics can lay out more heterogeneity among groups of employees. Additionally, data that spans more years prior to and after retirement may help lay out the long-term dynamics of retirement.

Chapter 4 confirms existing evidence of child penalties in the labor market. Chapter 4 describes these penalties on the basis of time use in the household. After childbirth, women substitute labor by household time. Time use increases completely offset their reduced labor market activity. As such, women substitute time use instead of increasing leisure, indicating intra-household substitution driving child penalties. Policies targeted at household time division in addition to existing labor market policies may therefore be successful at reducing child penalties.

Nevertheless, Chapter 4 is subject to a limitation. The analysis relies on survey data, which are collected annually and offer limited insights into the nuances of time allocation. Moreover, self-reported time use data may introduce memory bias and subjective interpretation issues. A means of measuring actual time use may further deepen understanding of how time use can explain child penalties. More frequent datasets specifically tailored to parents, along with data that track couples over extended periods, may enhance the understanding of how time allocation evolves following childbirth.

Chapter 5 shows that non-public disability does not increase outflow to work, but does increase outflow to full disability and recovery without employment. On the other hand, it decreases outflow for reasons such as retirement. The underlying mechanism is likely that firms more actively act on incentives to have workers exit partial disability. However, lowering the degree of disability may not necessarily be beneficial for the employee. Additionally, firms have a disincentive to act on increases in the degree of workers' temporary degree of disability. This manifests through these types of re-assessments occurring less among non-publicly insured firms.

The findings in chapter 5 indicate that non-publicly insured firms act on the incentives they have, but these incentives do not increase work resumption. Instead, non-publicly insured firms demonstrate heightened responsiveness by proactively requesting reassessments for workers experiencing changes in their degree of disability.

For future research, exploring the dynamics surrounding disability reassessment could provide valuable insights for policy-making. Specifically, gaining more information on the parties initiating reassessments, the changes in disabled workers' statuses, and instances of rejected reassessment requests may enhance understanding of how non-publicly insured firms navigate the incentives inherent in private insurance.

In conclusion, the household finance and social insurance literature still contains numerous areas of ambiguity, both regarding the topics studied and the data utilized. This dissertation contributes to elucidating some of these dynamics by empirically investigating rich and detailed microdata. However, it also leaves many unanswered questions, signaling the need for further research. Thus, a deeper exploration of the household finance and social insurance literature is imperative.

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Nederlandse samenvatting

Studies over huishoudfinanciën en sociale zekerheid

Individuen ervaren tijdens hun levens veel financiële risico's en maken veel gebeurtenissen mee die van invloed zijn op de financiën van huishoudens. Deze risico's bestaan onder andere uit arbeidsongeschiktheid, ouderdom, werkloosheid en het krijgen van kinderen. Deze risico's en gebeurtenissen hebben een sterke invloed op de inkomsten en uitgaven en kunnen beleid rechtvaardigen dat individuen helpt hun consumptie over de levensloop te spreiden. Economen gebruiken vaak het levenscyclusmodel om lange-termijn consumptiegedrag te voorspellen. Volgens dit model spreiden individuen hun consumptie zelf in de tijd. Als het levenscyclusmodel correct is, anticipeert men op inkomensschokken gedurende de levenscyclus en past men consumptiepatronen hieraan aan. Het levenscyclusmodel is uitgebreid bestudeerd (Ando and Modigliani (1963); Heckman (1976); Modigliani and Brumberg (1954)). Empirische studies vinden echter herhaaldelijk afwijkingen van het levenscyclusmodel (Bikker et al. (2012); J. R. Brown et al. (2008); Deaton (1986); White (1978)). Deze afwijkingen vertalen zich vaak in consumptiegedrag dat sterk fluctueert over de tijd en kortetermijnplanning. Deze afwijkingen hebben aanzienlijke gevolgen voor de financiën van huishoudens, aangezien die de kans vergroten dat risico's gedurende de levenscyclus de huishoudfinanciën verstoren en tot liquiditeitsproblemen leiden.

Nederland heeft een uitgebreid stelsel van sociale zekerheid met veel financiële vangnetten (OECD (2021)). Dit helpt huishoudens om hun consumptie in de tijd te stabiliseren. Pensioenuitkeringen helpen bijvoorbeeld om de consumptie op latere leeftijd soepel te laten verlopen en op peil te houden. Subsidies voor zwangerschapsverlof en kinderopvang bieden inkomenssteun en mogelijkheden om terug te keren naar de arbeidsmarkt. Arbeidsongeschiktheidsverzekeringen dekken het risico van arbeidsongeschiktheid. Inzicht in de vraag of deze sociale regelingen het beoogde effect hebben, is van cruciaal belang voor het ontwerpen van deze regelingen.

Dit proefschrift onderzoekt empirisch de effecten van pensionering, het krijgen van kinderen en arbeidsongeschiktheidsverzekeringen op het inkomen, de uitgaven en de tijdsbesteding van huishoudens. Centrale vraag is hoe de beschikbaarheid en het gebruik van sociale verzekeringen de beslissingen van individuen beïnvloeden met betrekking tot inkomen, uitgaven, sparen, tijdsbesteding en arbeidsparticipatie. Om deze effecten te onderzoeken, maakt dit proefschrift gebruik van verschillende soorten microdata, variërend van enquêtegegevens tot banktransacties. Met name de banktransacties zijn (bijna) uniek in de literatuur. Door deze analyses uit te voeren, draagt dit proefschrift bij aan een beter begrip van hoe sociale vangnetten het economische gedrag en de welvaart van individuen bepalen. Deze informatie kan nuttig zijn bij het formuleren van beleid op deze terreinen.

Pensioensparen van Zelfstandigen Zonder Personeel: Vervroegd Opnemen en Verminderde Administratieve Lasten

De pensioenopbouw van Zelfstandigen Zonder Personeel (zzp'ers) varieert in Nederland aanzienlijk: ongeveer de helft van de zzp'ers ontvangt na pensionering minder dan 70% van hun inkomen voor pensionering (Knoef et al. (2016)). Deze lage 'replacement rate' van

zzp'ers komt voornamelijk doordat ze geen bedrijfspensioenen opbouwen en zelf niet genoeg sparen om dit te compenseren.
In een vignettenstudie presenteert hoofdstuk 2 hypothetische pensioenproducten met mogelijkheden om pensioeninleg vervroegd op te nemen en administratieve vereenvoudigingen. Deze producten kennen variërende uitkeringen na pensionering. Daarmee wordt de betalingsbereidheid geanalyseerd, uitgedrukt als een percentage van de pensioenuitkering na pensionering, voor verminderde administratieve lasten en mogelijkheden om vervroegd pensioen op te nemen. Er is een vragenlijst uitgezet onder ongeveer 800 zzp'ers en 800 werknemers. Voor beide groepen onderzoekt dit hoofdstuk de betalingsbereidheid.
We constateren dat zzp'ers bereid zijn 14% van hun pensioen op te geven om de mogelijkheid te hebben om geld op te nemen als het inkomen laag is of om een hypotheek (af) te betalen. Ze hebben 8% van hun uitkering na pensionering over voor het feit dat ze geen fiscale informatie hoeven te verstrekken. Bij werknemers blijkt een bereidheid om 4% van hun pensioen op te geven voor de mogelijkheid om vroegtijdig geld op te nemen, maar zij hebben geen betalingsbereidheid voor minder administratieve lasten.
We vinden echter ook bewijs van een 'sophisticated present bias': Individuen hebben een irrationele tijdsvoorkeur voor het heden (present bias), maar zijn zich ook bewust van deze tijdsvoorkeur ('sophisticated'). Dit maakt dat individuen zichzelf in bescherming nemen tegen mogelijkheden om vrij geld uit te nemen. De schattingen laten geen (zelfs een negatieve) bereidheid zien om te betalen voor opties voor vrije vroegtijdige geldopname als daarbij een fiscale boete wordt geheven. Bovendien constateren we dat er een grotere vraag is naar opties voor vroegtijdige geldopname bij personen met een sterke tijdsvoorkeur voor het heden of een hoge discontovoet. Bij het ontwerpen van beleidsinterventies is het

belangrijk om het vroegtijdig opnemen van geld afhankelijk te maken van de inkomens- en vermogensposities van individuen.

Het effect van Pensionering op de Financiën van Huishoudens: Onderzoek op basis van Transactiegegevens

Er is veel wetenschappelijke literatuur die onderzoekt hoe pensionering de persoonlijke financiën beïnvloedt. Effecten van pensionering op uitgaven wisselen van nuleffecten tot afnames (Agarwal et al. (2015); Aguila et al. (2011); Banks et al. (1998a); Battistin et al. (2009); Been and Goudswaard (2020); Bernheim et al. (2001); Luengo-Prado and Sevilla (2013a); Lührmann (2010); Luengo-Prado and Sevilla (2013b)). Verder blijkt dat mensen na pensionering (op de lange termijn) extra spaargeld opbouwen (Kieren and Weber (2022); Love et al. (2009); Olafsson and Pagel (2018); Poterba et al. (2011)). Deze bevindingen zijn in tegenspraak met de voorspellingen van het levenscyclusmodel. Deze afwijkingen schrijft de literatuur toe aan verschillende verklaringen, zoals de wens om geld te schenken of na te laten (Lockwood (2018a, 2012a)), irrationaliteit ('bounded rationality') (Olafsson and Pagel (2018)) en besparingen op uitgaven door 'home production' (zelf huishoudelijke taken verrichten in plaats van inkopen) (Been and Goudswaard (2020)).

Bestaande schattingen van de effecten van pensionering op de financiën van huishoudens zijn echter gebaseerd op jaarlijkse administratieve datasets of enquêtes. Deze gegevens zijn beperkt: Jaarlijkse data maken het niet mogelijk om kortetermijneffecten in kaart te brengen, en enquêtegegevens zijn vaak onnauwkeurig gemeten. Om deze problemen aan te pakken gebruikt hoofdstuk 3 maandelijkse transactiegegevens van ING. Hoofdstuk 3 kijkt concreet naar hoe pensionering de netto flow balance (instroom minus uitstroom van geld in een gegeven maand), banksaldi aan het einde

van de maand en het percentage mensen met schulden beïnvloedt.
In Nederland komen individuen in aanmerking voor zowel een staatspensioen als een bedrijfspensioen. Alle Nederlanders ontvangen eerste-pijler pensioenen (AOW) na het bereiken van een bepaalde leeftijdsgrens, afhankelijk van hun geboortejaar. Veel werknemers bouwen bedrijfspensioen op in loondienst. Bedrijfspensioenen kan men eerder of later dan de AOW-leeftijd opnemen, alhoewel de uitbetalingen lager (hoger) zijn als zij voor (na) het bereiken van de AOW-leeftijd ingaan. Omdat de AOW-leeftijd per geboortecohort vast staat en de gegevens meerdere geboortecohorten omvatten, gebruikt de analyse de AOW-leeftijd als instrumentele variabele voor bedrijfspensioenen. Hierdoor kan analyse causale schattingen maken van de effecten van pensionering op huishoudfinanciën.
De dataset bevat, na toepassing van selectiecriteria, ongeveer 12.000 personen die op een bepaald moment binnen de steekproefperiode een bedrijfspensioen hebben ontvangen, waardoor de data hun financiële gedrag zowel voor als na het ontvangen van bedrijfspensioenen bevat. Bovendien bevat de dataset details over geldstromen, rekeningsaldi en een beknopte reeks demografische kenmerken. De data zijn maandelijks gemeten van 2016 tot en met 2021.
Uit de analyse blijken opmerkelijke kortetermijneffecten in de bestedingsdynamiek na pensionering, met name in de vorm van een scherpe stijging van de netto flow balance (instroom op minus uitstroom van de bankrekening) in de maand van pensionering. Deze effecten nemen echter op de lange termijn af en lopen uiteindelijk terug tot 0. Dit duidt op een aanzienlijke variabiliteit in de dynamiek van de financiën van huishoudens op de korte termijn, die in de bestaande literatuur over pensioenbesparingen niet volledig tot uitdrukking komt.
We laten bovendien een grote mate van heterogeniteit zien tussen verschillende groepen gepensioneerde werknemers: de inkomens- en

uitgaveneffecten zijn groter (kleiner) voor groepen met hogere (lagere) inkomens, meer (minder) spaargeld en witte boorden beroepen.
Bovendien onthult de analyse een stijging van de spaartegoeden onder individuen met een laag inkomen en een laag vermogen. Omgekeerd blijkt uit hoofdstuk 3 geen daling van de besparingen onder de bovengenoemde hoge-inkomensgroepen. Daarnaast is er na pensionering een opmerkelijke afname van het aandeel huishoudens met een negatief spaarsaldo. Deze bevindingen geven aan dat pensionering liquiditeitsbeperkingen voor individuen met lagere inkomens verlicht, terwijl er geen effecten zijn gevonden voor groepen met hogere inkomens.

Kinderboetes en de rol van tijdsbesteding in het huishouden

De inkomenskloof tussen mannen en vrouwen is de afgelopen decennia afgenomen, maar blijft tot op de dag van vandaag bestaan (Cortés and Pan (2020)). De bestaande literatuur bestudeert deze verschillen uitgebreid en stelt vast dat inkomensverschillen tussen mannen en vrouwen met name het gevolg zijn van het krijgen van kinderen (Andresen and Nix (2019); Cortés and Pan (2020); De Quinto et al. (2020); Kleven, Landais and Søgaard (2019); Kuziemko et al. (2018); Lundborg et al. (2017); Meurs and Pora (2019); Rabaté and Rellstab (2021); Sieppi and Pehkonen (2019)): Voor de geboorte van kinderen zijn de inkomens van mannen en vrouwen relatief gelijk. Hierna lopen inkomens echter uiteen: vrouwen verminderen hun activiteit op de arbeidsmarkt zowel op de intensieve als op de extensieve marge, en het inkomen van vrouwen herstelt zelfs jaren na de bevalling nooit van deze afname. Dit fenomeen staat bekend als de kinderboete ('child penalty').
De literatuur schrijft kinderboetes doorgaans toe aan geslachtsnormen (Bedi et al. (2018); Kleven, Landais, Posch et al.

(2019); Kleven (2022); Rabaté and Rellstab (2021); Rellstab (2023)) en aan tijdsbesteding in het huishouden (Blau and Kahn (2017); Casarico and Lattanzio (2023)). Tot nu toe is er echter slechts één artikel dat (op de korte termijn) tijdsbesteding in het huishouden rechtstreeks in verband brengt met de kinderboete. Dit hoofdstuk biedt het eerste (beschrijvende) onderzoek naar de samenhang tussen tijdsbesteding in het huishouden en kinderboetes op lange termijn.
Om kinderboetes in Nederland te schatten, gebruikt dit onderzoek een 'event study' ontwerp zoals in Kleven, Landais and Søgaard (2019). De analyse schat de effecten van het krijgen van kinderen op zowel de arbeidsmarktresultaten als de tijdsbesteding van het huishouden. Hoofdstuk 4 doet dit voor de jaren voor, tijdens, en na het krijgen van kinderen. Hierbij worden gegevens gebruikt uit het LISS-panel van 2008 tot 2021.
Uit de analyse blijkt dat de afname van de arbeidsmarktactiviteit na het krijgen van kinderen gepaard gaat met een toename van activiteit in het huishouden. Dit betekent ook dat vrouwen niet meer vrije tijd krijgen na de bevalling. Integendeel, de totale tijdsbesteding door vrouwen aan werken in een baan en in het huishouden neemt toe in vergelijking met deze tijdsbesteding door mannen.
Uit de bevindingen blijkt dat kinderboetes voornamelijk voortkomen uit substitutie van taken binnen het huishouden. Dat wil zeggen dat vrouwen arbeidsmarktactiviteit ruilen voor huishoudelijke activiteit. In aanvulling op arbeidsmarktbeleid kan beleid gericht op het huishouden (bijvoorbeeld kinderopvang) effectief zijn in het verminderen van kinderboetes.

Niet-Publieke Arbeidsongeschiktheidsverzekering en Uitstroom naar Werk

In Nederland kunnen bedrijven zich uitschrijven uit de publieke arbeidsongeschiktheidsverzekering van gedeeltelijk of tijdelijk arbeidsongeschikte werknemers (WGA'ers)(McVicar et al. (2022)). Door voor deze optie te kiezen, betalen bedrijven niet langer publieke arbeidsongeschiktheidspremies, maar nemen zij de verantwoordelijkheid op voor de re-integratie en het betalen van de uitkeringen van WGA'ers.

De re-integratie van WGA'ers zelf regelen kan extra uitstroomkanalen creëren voor niet publiek verzekerde bedrijven. Concreet hebben bedrijven de mogelijkheid om herkeuringen voor WGA'ers aan te vragen, wat dient als een re-integratiemiddel dat niet beschikbaar is via het UWV (Lammers et al. (2018)). Bovendien geeft het UWV voorrang aan deze herkeuringsaanvragen. In dit hoofdstuk wordt geanalyseerd of niet publiek verzekerde bedrijven een hogere uitstroom uit de arbeidsongeschiktheidsverzekering vertonen, vooral in termen van uitstroom naar werk.

Uitstroom uit arbeidsongeschiktheidsverzekeringen heeft aanzienlijke gevolgen voor de financiële positie van WGA'ers. Als het stopzetten van de arbeidsongeschiktheidsverzekering gepaard gaat met hervatting van het werk of een andere baan, kan dit ten goede komen aan WGA'ers door hun inkomen te verhogen en te helpen bij het spreiden van de consumptie. Het beëindigen van arbeidsongeschiktheidsuitkeringen kan echter de financiële problemen van WGA'ers verergeren als zij niet aan het werk (kunnen) gaan. Evenwicht tussen re-integratie inspanningen en de behoefte aan financiële stabiliteit op de lange termijn is van cruciaal belang.

De analyse gebruikt administratiegegevens van UWV van 2006 tot en met 2021. Deze administratiegegevens bevatten het verloop van de WIA-trajecten en herbeoordelingen van gedeeltelijk arbeidsongeschikten. De analyse gebruikt duurmodellen van uitstroom op basis van de verzekeringsstatus.

Uit de analyse blijkt geen oorzakelijk verschil in uitstroom naar werk tussen niet publiek verzekerde bedrijven en publiek verzekerde

bedrijven. Er zijn echter wel andere verschillen in de uitstroom: de uitstroom naar herstel zonder werk, en vooral de uitstroom naar structurele volledige arbeidsongeschiktheid, is hoger bij niet publiek verzekerde bedrijven, terwijl de uitstroom om redenen als pensionering en overlijden lager is.
De bevindingen komen voort uit herbeoordelingen, waaruit blijkt dat niet publiek verzekerde bedrijven proactiever zijn in het aanvragen ervan dan het UWV. De uitstroom die door herkeuringen in gang wordt gezet, betreft voornamelijk herbeoordelingen die overgaan van een tijdelijke volledige arbeidsongeschiktheidsverzekering (WGA 80+) naar een structurele volledige arbeidsongeschiktheidsuitkering (IVA). Herbeoordelingen die leiden tot een lagere mate van arbeidsongeschiktheid of het beëindigen van de arbeidsongeschiktheid komen echter ook vaker voor bij niet publiek verzekerde bedrijven.
Uit hoofdstuk 5 blijkt al met al dat eigenrisicodragers een hogere uitstroom uit de WIA realiseren, maar dat er geen (causale) verschillen in uitstroom naar werk zijn. Dit wil zeggen dat eigenrisicodragers niet effectiever of minder effectief re-integreren dan publiek verzekerde bedrijven.

Overview of author contributions

According to the PhD regulations of the Faculty of Law at Leiden University, an overview of the author contributions is required. These are as follows:

Chapter 1: Introduction
Pim Koopmans wrote the manuscript.

Chapter 2
Pim Koopmans, Marike Knoef, and Max van Lent designed the study. Pim Koopmans cleaned the data, conducted the analyses, and took care of the data management. Pim Koopmans drafted the paper. Marike Knoef and Max van Lent provided revisions.

Chapter 3
Max van Lent initiated the project. Pim Koopmans, Max van Lent, and Marike Knoef designed the study. Pim Koopmans and Marike Knoef designed the econometric models. Pim Koopmans cleaned the data, conducted the analyses, and took care of the data management. Pim Koopmans drafted the paper. Max van Lent and Marike Knoef provided revisions.

Chapter 4
Pim Koopmans and Max van Lent designed the study. Pim Koop-

mans, Max van Lent, and Jim Been implemented the econometric model. Pim Koopmans arranged and cleaned the data, conducted the analyses, and took care of the data management. Pim Koopmans drafted the paper. Max van Lent and Jim Been provided revisions.

Chapter 5

Chapter 5 is single-authored.

Chapter 6: General discussion

Pim Koopmans wrote the manuscript.

CV

Pim Koopmans was born on May 16, 1995, in Zoetermeer, the Netherlands. He obtained a BSc in Economics and Business Economics from Vrije Universiteit Amsterdam, followed by an MSc in Economics with a specialization in labor market and health economics.

After completing his MSc, Pim undertook an internship at CPB Netherlands Bureau for Economic Policy Analysis. During this internship, he researched the elasticities of disability and unemployment insurance with respect to their benefit levels.

In 2019, following his internship, Pim began his PhD at the Department of Economics, Leiden Law School, Leiden University. His doctoral research was part of the "Self-reliance and Social Protection over the Lifecycle" research program, and received financial support from the Instituut GAK. Pim presented his work to various policy-related stakeholders, including during the Dutch Economist Week, Netspar, the Dutch Association of Insurers, and the Dutch Employee Insurance Agency (UWV).