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Retirement planning and financial incentives: The impact of announced cuts in pensions

Stefanos Tyros

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MASTER'S THESIS

Retirement planning and financial incentives: The impact of announced cuts in pensions

Author:
Stefanos TYROS

Supervisors:
Dr. Ralph STEVENS
Prof. dr. Casper VAN EWIJK

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Statement of Originality

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UNIVERSITEIT VAN AMSTERDAM

Abstract

Master's in Economics

Retirement planning and financial incentives: The impact of announced cuts in pensions

by Stefanos Tyros

We investigate the response of old age households in the Netherlands to the abolishment of the state pension partner supplement - a pension cut affecting households with a low income younger partner. The discontinuation of the supplement took place on April 2015, following a 20 year announcement period. The dataset available allows us to probe the households' response before being subjected to the income shock. Life-cycle theory predicts that upon announcement households readjust their consumption and labour supply paths in order to smoothen them. Behavioural extensions, as well as lack of information and liquidity constraints, can alter these predictions. Previous empirical estimates have found no or small wealth effect on retirement decisions. We investigate three channels of response: a labour supply response of the older partner through means of delaying retirement, a labour supply response of the younger partner and a response in the savings rate of the household. The setup of our analysis is unique for a number of reasons. First, we used microdata from tax returns that allows us to calculate effects with big accuracy. Second, the wealth shock is large (around 70,000€ for an average household with a young partner with very low or no income), it depends on the age difference of the partners and is targeted to households with a younger low income partner. Third, the sample is rather diverse, as the income of the older partner and the household wealth are unconstrained. Fourth, there was an active campaign in order to inform the public of the policy change. Fifth, the announcement period was large, allowing households to alter their behaviour well in advance of the shock and, sixth, the dataset allows us to probe a variety of response channels. For all these reasons, our analysis constitutes a test on the hypothesis of a zero marginal propensity to consume future income, under the mental accounting framework. We employ a differences-in-differences-in-differences setup and find no effect due to the supplement abolishment in any of the three channels. A negative effect on the labour supply of the younger partner and the household savings, present for households with a high-income younger partner as well, is attributed to a reform of the occupational pension funds enacted at the same date.

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Contents

Chapter 1

Introduction

Understanding how individuals respond to shocks in their future income is crucial, both in terms of economic theory as well as policy. This response is expected to be different compared to when individuals are subjected to shocks in their current income or wealth (Thaler, 1990). This work takes advantage of a unique setup, in order to probe the response of households to a large shock in their future income, with a substantial announcement period that allows them to reconfigure their consumption and labour supply paths.

In the mid 90's, the World Bank attempted to alarm national governments of the effects of an ageing population with fewer children on the national pension systems and on economic growth (World Bank, 1994). Since then, the Netherlands has undertaken major reforms, both in its state pension and the occupational pension system (Broeders et al., 2009 and Bovenberg and Nijman, 2018). As the discussion continues for further reforms (Van Vuuren, 2014), it is crucial to investigate how households respond to these reforms. We investigate the response of old age households to one of the changes in the Dutch pension system, leading to a sizeable negative income shock on the affected households, with both an income and a substitution effect.

The first pillar of the pension system in the Netherlands consists of the state pension (AOW) that is provided to everyone reaching the state pension age (SPA) and is independent of their labour history. The reform under investigation is the abolishment of the state pension partner supplement (AOW supplement). Until 2015, new pensioners with a younger partner that had little or no income received the AOW supplement, on top of their AOW pension, as soon as they reached SPA. This supplement was discontinued on April 1st 2015. Given the long horizon of retirement planning, the policy change was announced 20 years in advance. Our dataset allows us to probe the response of the

households up until the SPA of the older partner, that is, before they are affected by the shock.

Standard Life-Cycle models (Ando and Modigliani, 1963) predict that upon announcement of a future shock in the present value of their life-time wealth households respond by smoothing their consumption and labour supply for the remainder of their life cycle. Given that we investigate the response prior to the shock, standard theory predicts only a wealth effect on the households' behaviour. Within the framework of behavioural economics Thaler (1990) shows that mental accounting violations of the principle of fungibility predict a lack of response to a future income shock. This would lead to a small or zero wealth effect on the retirement planing of the households, as has been found empirically (De Mooij et al., 2009). Moreover, we consider the effects of lack of information and liquidity constraints on the households' responses.

Given these considerations, we investigate three possible response channels. The first is a labour supply response of the older partner. Given their status as the main breadearner of the household (as the younger partner has low or no income), we expect them to be time-constrained. We, therefore, probe their labour supply response through means of delaying retirement. The second channel is a labour supply response of the younger partner and the third an increase in the savings rate of the household.

The policy change provides us with a unique setup to investigate the response of households' retirement planning to a future income shock. First of all, we use microdata based on tax returns. This provides us with a large set of data on the Dutch population, allowing us to compute effects with large accuracy. Second, the total wealth shock is sizeable, with a drop in lifetime wealth of around 70,000€ for an average household with a younger partner with very low or no income. Moreover, the shock is dependent on the age difference of the partners and is targeted on a specific sample of the population - namely households with a young partner with low income. Third, the sample considered is rather diverse, as the supplement was not conditional on anything else other than the income of the younger partner. Therefore, the sample includes households with low income and/or wealth that could be liquidity constrained as well as high income/wealth households with small disutility from lowering consumption in order to smoothen it.

Fourth, there was an active campaign in order to inform the public. That included newspaper articles, advertisements and a letter sent in 2010 to those affected by the policy change. Fifth, the announcement period of the reform was rather large (20 years). That gave plenty of time to the households to readjust their expectations and act accordingly. Sixth, using the dataset we are able to investigate multiple channels of response, in order

to get a complete picture of the reaction of the households. Hence, for all the reason mentioned, our analysis constitutes a test on the hypothesis of a zero marginal propensity to consume future income, under the mental accounting framework.

We use a differences-in-differences-in-differences setup in order to disentangle the effect of the abolishment of the supplement from other effects. This allows us to investigate the effect per month of age difference of the partners, hence per euro of total future income loss. We find no response in terms of the older partner delaying their retirement. We do find a negative response in the partner's labour supply and the corresponding negative response in the households' wealth (i.e. their savings rate). This is attributed to the reform of the occupational pensions in the Netherlands, that was enacted simultaneously with the abolishment of the AOW supplement. That reform affected all households, not only the ones with a low-income younger partner. Hence, we observe no significant effect due to the abolishment of the supplement in any of the three channels.

The rest of the thesis is structured in the following way. Chapter 2 introduces the Dutch pension system and describes in detail the policy change. Chapter 3 sets the research questions and the theory behind them. Chapter 4 describes the dataset and Chapter 5 the methodology used in the analysis. Finally, Chapter 6 presents the results, Chapter 7 discusses them and Chapter 8 presents our conclusions.

Chapter 2

The Dutch Pension System

2.1 Overview

The Dutch pension system is a three pillar system. The first pillar consists of the state provided pension, the AOW (SVB, 2018), which is funded by the AOW premium (17.9% in the first two tax brackets) and from general tax revenues. Every registered citizen in the Netherlands accruals 2% of the state pension per year, during the 50 years prior to reaching the state pension age. The full benefit, after 50 years of residency, amounts to 100% of the minimum wage (€1,565.40 per month in May 2018[1]) for couples (50% each) and 70% for single individuals. Until 2012 the SPA was 65 years old. This has been gradually increasing, with SPA reaching 67 years by 2021 and being linked to life-expectancy thereafter (Belastingdienst, 2018).

Pillar one also includes a means-tested supplement (the AIO) to ensure subsistence level. This safety net is provided to those receiving the state pension and has strict income and wealth requirements. Up to 2015, pillar one also included a supplement for individuals with a younger low-income partner (the AOW supplement). This supplement and its discontinuation is presented in more detail in the next section.

The second pillar consists of occupational pensions (OECD, 2017). These pensions are funded by the contributions of employees and employers in the pension funds. The pillar consists of industry-wide pension funds (e.g. for civil servants), corporate pension funds and pension funds for independent professionals. If the social partners agree on a pension scheme for their employees (a majority of 50-60% of employers and employees is required), the government can make a pension scheme mandatory for the entire sector or profession.

[1]Source: https://www.government.nl/topics/minimum-wage/amount-of-the-minimum-wage, accessed 12/05/2018.

As a result, over 90% of the wage workers and professionals have a pillar two pension scheme. Until 2015, employees were able to draw income from their pillar two pension up to 8 years prior to SPA (depending on their scheme), without any essential reduction in their pension income. As of 2006 new rules have made pillar two pensions actuarially fair, followed by a transition period. On April 2015 the transition period ended, making it less financially attractive to retire early, leading to fewer early retirees.

Pillar two does not cover employees without a collective labour agreement or self-employed. Non-standard workers and self-employed are covered by the third pillar of the pension system. Participation in this pillar is not mandatory, leading to quite low participation rates.

2.2 The AOW Supplement

As mentioned earlier, for individuals in a couple the AOW pension amounts to 50% of the minimum wage for each partner. Until April 1st 2015, if at the time the older partner reached SPA the younger one had an income below a certain amount (€1,411.13 per month for 2015), then the first would receive an AOW supplement on top of his/her AOW pension (SVB, 2018). This was done automatically, without the older partner having to apply for the supplement. This supplement reached 50% of the minimum wage (i.e. around €780 per month) if the younger partner made less than €236.70/month. I.e. if the younger partner had very low or no income they would receive the full AOW pension for a couple as soon as the older partner reached SPA. For each euro of labour income above the threshold of €236.70/month the supplement would be reduced by two thirds of a euro. Hence, effectively the income of the younger partner between €236.70 and €1,411.13/month was taxed at a 66.7% rate, as soon as their older partner reached SPA. If this income was drawn from a pension the effective tax was 100%.

On April 1st 2015 two changes came into effect. First, and most important for our analysis, the supplement was discontinued for new pensioners. This means that if the older partner in a couple reached SPA after the 1st of April 2015 and their younger partner had a low income, they would not receive the supplement. This amounts to a maximium income loss of €782.70 per month, if the income of the younger partner was below €236.70/month. That results to a sizeable drop in income of more than €9,000/year, until the younger partner reached SPA as well. Given that the average age difference of the partners that receive the supplement is over 7 years, the abolishment

would result to a total shock of around €70,000 for a couple with the average age difference and a younger partner having very low or no income.

The second change was the discontinuation of the supplement for individuals that had reached SPA before April 1st 2015 but the younger partner had an income above the €1,411.13/month threshold. More precisely, if the partner had an income above this threshold and it dropped bellow only after April 1st 2015 the household would not start receiving the supplement. Hence, the law not only discontinued the supplement for new pensioners but also for those that did not meet the criteria at the day of the implementation of the law, thus eliminating any new supplement recipients. The response of the latter is not investigated in this work.

As stated above, the change in law could, potentially, lead to a substantial drop in income for some households. It also constituted an abolishment of a very high income tax on the younger partner. Hence, given that retirement plans have a rather long horizon, the Dutch government decided that it should provide a substantial announcement period before the law was enacted. As such, the law that was enacted in 2015 was legislated in 1996. This means that people were given a notice period of at least 20 years that they would not be receiving this supplement.

Chapter 3

Research Questions and Hypotheses

The core of the policy change described above is a state pension cut with an income change, a change in the marginal tax on the young partner's labour supply and a long notice period. This cut affected households where the younger partner had low or no income. The research question of this thesis, therefore, is: how do households react to such a drop in future income? Can households' behaviour be explained as forward looking or myopic?

We investigate differences in the behaviour of the cohorts that did not receive the supplement (post-1949 cohorts) with those that did (pre-1950 cohorts). We refer to this as the reaction of the post-1949 cohorts to the supplement abolishment from now on. As discussed below, in Chapter 4, our dataset allows us to probe this reaction up to (and including) the year that the older partner reaches SPA. We, therefore, investigate the reaction of the post-1949 cohorts directly *before* they experience the income shock. We do not probe their reaction during the income shock. We consider three alternative responses: an increase in their savings, their labour supply and a delay of early retirement.

3.1 The Standard Model

The basic underlying theoretical model that motivates us to discuss these responses is the classical Life-Cycle model. In such a model individuals maximise their lifetime utility, subject to some constraints, as seen below:

$$\begin{aligned} \max_{\{C_t L_t\}_{t=0}^{\infty}} V &= \sum_{t=1}^{\infty} p_t \beta^t U(C_t, L_t), \\ \text{s.t.} \quad W_{t+1} &= (W_t + G_t + I_t - C_t)(1 + r_t), \quad I_t = f(L_t). \end{aligned} \tag{3.1}$$

Here V is the present value of the individuals' lifetime utility. p_t is the probability of being alive in period t, β the time discount factor and $U(C_t, L_t)$ the utility in period t. V is maximised with respect to C_t and L_t, the consumption and leisure (i.e. $1-$labour supply) in period t. This is subject to the budget constraint in the second line of Equation 3.1, in which W_t is the wealth in period t, G_t the government transfers (including the state pension) and I_t the income, which is a function of leisure. r_t is the interest rate and W_{t+1} the wealth in period $t+1$. The abolishment of the AOW supplement constitutes a decrease in G_t from the SPA of the individual until the SPA of their younger partner. In that case, individuals need to alter the path of their consumption and labour supply, by reducing C_t and/or L_t, in order to satisfy the budget constraint.

Ando and Modigliani (1963) show that in a standard Life-Cycle setup individuals consume an amount proportional to their discounted total lifetime income in every period. Given that by consumption we indicate both the consumption of goods and services and of leisure, individuals smoothen their consumption and/or their labour supply over their lifetime. Unexpected shocks in the lifetime budget force individuals (in our case households) to alter their consumption and labour supply. This is done in order to achieve smoothening within the new budget. It is important to note that this analysis is dependent on the ability of household to freely save and borrow.

Under such conditions, forward looking households are expected to react on the day of the announcement of the shock. In the case of the abolishment of the AOW supplement, that would amount to smoothening a shock of €70,000 (for a household with a younger partner with low or not income and the average age difference of 7 years) over 35 years (20 years of announcement plus 15 years of expected pension life). That would result to a shock of around €2,000 per year, a substantial yet not large change.

Given that our analysis (see Chapter 5) uses other cohorts as controls in order to derive the effect on individuals with younger parters affected by the policy change, we use the following definition for simplicity. We refer to the partner that reaches their SPA around 2015 as "the individual" and the other partner as "the partner". Hence in the group we are interested in (the treatment group) the older partner is referred to as the individual. In the control groups we consider, individuals reaching SPA around 2015 can also be younger than their partner.

As we investigate the time period prior to the shock, under the Life-Cycle framework we probe the reaction of the households to the wealth effect of the abolishment. The abolishment of the effective tax, i.e. the substitution effect of the reform, is expected to have an effect only during the time of the shock. That assumes that the partner would be free to move their income to non-taxed periods via means of borrowing.

The first channel of response is the individual increasing their labour supply. Given that they already are the main bread-earner (as the partner has low or no income) we typically expect them to be time-constrained. Hence, we investigate their labour supply response through means of delaying retirement (extensive margin). The second channel is an increase of the labour supply of the young low-income partner (intensive margin if they had low income, extensive if they were not working). Finally, the third channel is an increase in the savings rate of the household. That would result to a reduction in the pre-shock consumption and a smoothening of the consumption of the household during the shock.

As mentioned in Chapter 2, pensioners with little or no income other than their AOW pension and little wealth (less than €12,040) are eligible for the AIO supplement. Hence, some of the households could substitute for the loss of the AOW supplement with the AIO supplement. Only a small part of the population affected by the AOW supplement abolishment would be eligible for the AIO supplement, though. This is supported by the data in Section 4.2 where we present the percent of house-ownership and the wealth quartiles and by the fact that given the individual's income, most of them would also be receiving a pillar two pension. Moreover, as found in Van Eekelen and Kridene-Lageman (2017) less than half of those affected by the abolishment that were eligible for the AIO supplement actually applied for it.

It has been shown that wealth shocks can alter significantly the labour supply of individuals. French (2017) uses data for lottery winners and finds a modest immediate and persistent drop in earnings. The wealth effect from pensions, though, on labour supply and retirement is, in general, found to be small or even zero (De Mooij et al., 2009). In the next section we elaborate on extensions of the standard model attempting to explain deviations from consumption and labour supply smoothening.

3.2 Extensions

There are several extensions to the standard model that could explain a different behaviour of the households than the one described above. I.e. households may not alter

their path at announcement, but later when the SPA of the individuals is approaching. They might also not react at all in the pre-shock period. We consider five such extensions. The first three are behavioural extensions, namely time-inconsistency, self-control costs and the violation of wealth fungibility. The third is lack of information and the fourth liquidity constraints.

Time inconsistent choices of individuals assign small discounting between future dates, but when the first of those dates arrives the relative discounting to the latter is much larger. One such model is discussed in Laibson (1998), where hyperbolic discounting (compared to exponential in the classical theory) is used to model the dynamical inconsistency of preferences. Such individuals might postpone the decision of increasing their savings rate until close before the shock, or even not do it at all.

Shefrin and Thaler (1988) enrich Life-Cycle models in order to include behavioural elements. These behavioural alternations on the utilities of individuals alter the basic model's results. Two of these alternations are of interest in our study. The first is self-control. The paper recognises that self-control can be costly for individuals. As evidence, they provide the existence of commitment devices, such as pensions funds, to help them postpone current consumption to the future. With regards to our analysis that would imply that there is an extra cost to households committing to saving more in order to smoothen consumption in the shock period. Hence, it might cause a delay of the saving response until closer to the SPA of the individuals, or even cause the households to not save extra at all.

The second is mental accounting. This results to households' economic planning that violates the principle of fungibility. That is, households set different value (beyond discounting) to different components of their lifetime budget. That violates the initial consumption smoothening assumptions, as households in our analysis would view a reduction of one euro in their post-SPA income differently than an equivalent reduction to their wealth. More precisely, Thaler (1990) separates wealth into three categories: current income, assets and future income. He discusses the variation of the marginal propensity to consume of these categories. For current income this is found to be close to 1, for assets between 0 and 1 and for future income close to zero. This explains why winning the lottery (a positive shock in the assets) results to a labour supply reduction, as the individuals consume part of the extra wealth (looking at leisure as a good to be consumed). It also explains why this effect can be very low for pension wealth, as such wealth is seen in terms of a future income. Hence, households with an older partner in the pre-1950 cohorts that expected to receive the supplement would not alter their consumption and

labour supply relative to not receiving it. That would result to no effect being detected in the pre-shock period from the abolishment of the supplement.

Next, we move to lack of information. The predictions of the standard model assume that individuals have perfect information and act upon it. Lack of such information will lead the individual to act according to their beliefs at the time. In terms of our analysis there are two relevant scenarios. The first is a lack of knowledge about the supplement in the pre-1950 cohort. I.e. that individuals that would receive the supplement when reaching SPA were unaware of the fact that they would receive this additional income. The second scenario is the post-1949 cohorts being aware of the supplement but not of its abolishment. In that case the households would be expecting to receive an extra income in the future that they actually wouldn't.

The Sociale Verzekeringsbank (SVB) that paid out the supplement notified the 1950-55 cohorts of its discontinuation in 2010, by means of a letter and a leaflet. The Dutch government undertook surveys in 2009 in order to establish the knowledge of the affect cohorts.[1] These showed that, at the time, only around 20% were aware of the supplement and its discontinuation. Moreover, less than 5% stated they had taken relevant actions. Under both scenarios mentioned above, the pre-1950 cohorts and post-1949 would have the same expectations in terms of their post-SPA cohort. That would result to no difference in the behaviour of the cohorts being identified. If these scenarios were partially true (as is indicated by the surveys) then any effects due to the households that were informed would be diluted by the rest of the population.

Last, we explore the Life-Cycle model under liquidity constraints. Under such a scenario individuals cannot (or can up to a limited extend) borrow in order to smoothen consumption. One such scenario would be the case where households expect to have a higher income after the individual reaches SPA, but are unable to borrow before in order to move consumption forward. In that case there would be a smaller savings response to the abolishment of the supplement, given that it would just balance out the pre and post-SPA income of the household. This is relatively unlikely, though, given that in most cases households experience a steep income drop at SPA.

A second scenario has to do with the effective tax that the younger parters were experiencing while their older partner received the supplement. Under liquidity constraints this substitution effect may have an effect on the labour supply even before the shock. The channel refers to the pre-1950 cohorts and specifically the younger partner's labour

[1] See (in Dutch) `https://www.tweedekamer.nl/kamerstukken/brieven_regering/detail?id=2009Z11477&did=2009D30191`, accessed 02/08/2018.

supply: assume that the the younger partner would decide to retire two years before the SPA of their older partner in the absence of the supplement. They would, then, start receiving their pillar two pension. In the presence of the supplement, though, as soon as the older partner reaches SPA the first €700 of the younger partner's pillar two pension would be effectively be taxed at a 100% rate, as they could have been receiving the supplement instead. The younger partner is, therefore, incentivised to not draw the pillar two pension and receive the supplement. That is, to start drawing the pillar two pensions when reaching their SPA. In the absence of any liquidity constraints the younger partner could fund the two years until the SPA of the older partner (as it has decided to stop working two years before the individual's SPA) by borrowing. If the household is liquidity constrained, the younger partner would have to fund these years by postponing the date they stop working until the SPA of the older partner. Hence, they work more in the presence of the supplement. That would lead to a (relative) negative effect of the supplement abolishment on the labour supply of the younger partner.

To summarise, according to the standard model households are expected to alter their consumption and labour supply paths at the moment of the announcement of the discontinuation of the supplement. That could materialise through a delay in early retirement of the older partner, an increase in the labour supply of the younger partner and/or an increase in the savings rate of the household. Behavioural effects, lack of information and liquidity constraints can alter this behaviour. Possible alternations include a delay or complete lack of a savings response and a lack of retirement response. Moreover, possible results are the lack or delay of the increase of the labour supply of the partner, or even a labour supply decrease. It is worth keeping in mind that a combination of those effects can occur within the population of interest. In that case, the effect would be diluted by the portion of the population that did not alter their behaviour.

Chapter 4

Data Description

4.1 Sample Building

The data used in our analysis consists of tax microdata from Statistics Netherlands (CBS). This means that we are able to use the whole population of interest and not just a subsample. This has two main advantages over survey data, namely: high explanatory power, given the large sample size, and no potential sample selection effects. Therefore, we base our analysis on years for which tax data are available, hence from 2006 until 2017.

We use four main types of data. The first is data on individuals and couples and the second is their income. Third, we use data on the date individuals start drawing income from their pillar two or three pension scheme. Fourth, we use data on household wealth. Note that the latter two data sources are only available up to 2016 and 2015 respectively. More specifically we use the following data sets from CBS:

- Data on individuals and couples:

    1. GBAPERSOONTAB: contains all individuals who have lived in the Netherlands at some time between 1995 and 2017. It associates each individual with an identification number (rinpersoon) that is used to identify them when merging various data sets. Moreover, it contains additional information on the individuals, such as sex, date of birth and migrant background.
    2. GBAHUISHOUDENSBUS: contains all the households that have existed in the Netherlands from 1995 until the end of 2016. A new household is considered whenever an individual enters or leaves the current household. Hence, an

individual living alone consists of a household, if they find a partner they are considered as a new household and for every child they have thereafter they are once again considered to live in a new household. The data set also contains information on the start and end dates of the households, along with the individuals in it (identified by rinpersoon) and their position in the household (partner, child, etc).

- Data on Income:

  3. POLISBUS & SPOLISBUS: these two data sets combined contain information about the wage income of all individuals in the Netherlands from 2006 to 2017 (the former from 2006 to 2009 and the latter from 2010 to 2017). They include the start and end dates of all wage contracts, along with the income acquired by each labour contract and various other characteristics of these contracts (SPOLISBUS contains some additional variables, but uses the same definitions to identify the wage income of individuals).
  4. ZELFSTANDIGENTAB: contains the income from self-employment of individuals from 1995 to 2015.

- Data on retirement dates:

  5 PENSOVPERSOONBUS: contains the date individuals start drawing income from their pillar two or three pension scheme. More specifically, it indicates whether an individual is receiving a war pension or some other pillar two or three pension and the corresponding starting date of the pension (and the end date in case an individual has passed away). Starting dates are included up to December 31st 2016.

- Data on household wealth:

  6. VEHTAB: contains data on the wealth of households from 2006 until 2016. It provides information about assets and liabilities of the households on January 1st of every year. It includes various form of assets and liabilities, such as house wealth, bank savings, stocks, etc.

These data sets were combined in order to create the sample of our interest. The process, along with our assumptions, was the following:

- GBAHUISHOUDENSBUS was used to identify individuals, along with their partners, included in the households. Merging these households, we acquired a database

of all couples that existed in the Netherlands since 1995, along with their start and end dates.

- GBAPERSOONTAB was used to identify individuals in the birth cohorts of interest. Since the law was enacted in 2015, the year in which the 1950 cohort retired (partially), we identified all individuals born between 1945 and 1955. This was merged with the couples database in order to create a database of all couples where at least one person in the household was born in the years 1945-1955 (the individual), along with variables such as sex and immigrant background. This was, then, merged again with GBAPERSOONTAB in order to integrate the same variables (i.e. birth data, gender, etc.) for the partner as well.

- We restricted our sample to couples who have been continuously together from 1996 (the year of the announcement of the law change) until 2017. In this way we ensured that couples have been continuously planning their finances as partners, both in case they respond early (at announcement) or later, near the SPA of the individual.

- Next the database was merged with POLISBUS and SPOLIBUS in order to include the wage income of all individuals and their partners from 2007 until 2017. The 2006 data was not used, in order to avoid incomplete data, given that it was the first year these microdata were collected.

- The date that individuals reach SPA was calculated, given their date of birth and the corresponding SPA from Belastingdienst (2018). Then, we constructed variables for the wage income of all individuals and their partners t^* years from the date the individual reaches SPA, where t^* run from 5 calendar years before SPA until 5 calendar years after. This was done in order to be able to compare different birth cohorts around SPA.

- ZELFSTANDIGENTAB was then merged to the database, in order to include the income from self-employment of all individuals and their partners from 2007 until 2015. We excluded households where at least one of the partners had a sizeable self-employed income, for two reasons. First, 2016 and 2017 data were not available for the self-employment income. Second, we wished to avoid the volatility of income from self-employment over the crisis period, as it would include multiple loss-making individuals who would skew the results. Hence, we excluded all households where at least one of the partners earned more from self-employment than wages in any year from 2007 to 2015 (this reduced the sample by 14.6%). This choice was made in order to avoid dropping couples where income from self-employment was only a

small part of the overall income and, hence, not where their main response would be visible.

- PENSOVPERSOONBUS was, then, merged in order to calculate the retirement dates of individuals and their partners. This data included the dates in which individuals starting drawing income from their pillar two or three pension scheme, up to and including the end of 2016. We use these dates to define their retirement. Hence, we would be unable to have retirement dates for those with SPA after 2016 and for these that continued working after reaching SPA. Therefore, we defined retirement to be capped at SPA, and when investigating the retirement response we will be looking at the response in terms of a reduction in early retirement.[1] For those without retirement data (around a quarter of the sample) the majority were people who had not accrued any pension rights as they had not been active in the labour market. We classified individuals without pension accumulation as the ones with a wage income smaller than €5000/year for all years 2007-2017. For the rest (around 1% of the sample), we defined their retirement year as the last year (capped at SPA) in which they earned more than €5000 (taking into account the case were they make less than that due to retirement in the middle of the year). Finally, we drop households where at least one of the partners had a war pension since birth (1.4% of the sample), mostly due to a lost relative during WWII. We also drop individuals from the Dutch colonies who received a pension on January 1960 (3.2% of the sample), and, finally, all those that retired before 1996 (2.3% of the sample), as the policy change was announced later on.

- Finally, we merge the VEHTAB data in order to include the wealth of households over the years 2006 to 2016. More specifically, we merge the liquid wealth (savings and stock) along with whether the couple owns their main residency. A small percentage of household has no wealth data (might not file tax returns) or the two partners have different wealth (they file separate tax returns). We remove these individuals from the sample.

The resulting database includes all couples that have been continuously together since 1996, where the individual is born between 1945 and 1955, and the main source of income of the household are wages. In this database we have wages, house-ownership and household liquid wealth around the year where the individual reaches SPA, along with the retirement year and month of individuals and their partners.

[1] A rather small number of people works after reaching SPA. We have checked this for the cohort 1945, for which we have retirement data.

4.2 Summary Statistics

We present the summary statistics of our final sample. Our analysis was based mostly on data of the cohorts 1948-1952, in order to be able to probe the response of the household up to and including the individuals' SPA. Hence, we present the relevant statistics. The first table summarises the number of individuals per cohort.

TABLE 4.1: Number of individuals per cohort

Cohort	1948	1949	1950	1951	1952
N	32,940	30,412	30,838	30,013	30,781

The sample consists of 50.7% male individuals, with no large deviations across cohorts. Next, we present in Figure 4.1 a histogram describing the age difference distribution of the couples in our full sample. Age difference is defined, in months, as the distance between the date the individual reaches SPA minus the date the partner does (so that it includes changes in the SPA). Hence, a positive age difference denotes an individual with a younger partner. Slight differences occur in the various cohorts, but most striking is

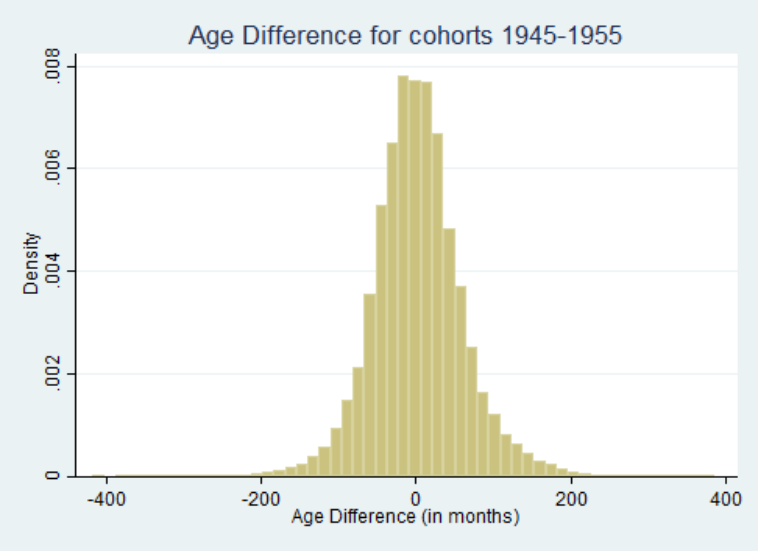

FIGURE 4.1: The distribution of the age difference (in months) within couples, in the cohorts 1945-1955

the difference in the distribution between male and female individuals. This is shown in Figure 4.2. Note that in slightly above 80% of the households the older partner is a male. Moreover, we are able to distinguish immigrant from non-immigrant individuals and find that around 7.2% of the individuals are first generation immigrants and 5.1% are second generation immigrants.

(A) Female individuals (B) Male individuals

FIGURE 4.2: The distribution of the Age Difference (in months) for the cohort 1951 for Female (panel A) and Male (panel B) individuals

With regards to the households' finances, around 75% of the individuals live in a house they own 5 years before reaching SPA. Looking at the liquid wealth[2] of households, Table 4.2 displays in the upper panel the quartiles for the 1949 and 1951 cohorts. The latter have slightly lower liquid wealth levels, 5 years prior to the individual's SPA. However, at SPA (see middle panel in Table 4.2) the liquid wealth level for the 1951 cohort is larger than for the 1949.

TABLE 4.2: Wealth and income quartiles

	25^{th} percentile	50^{th} percentile	75^{th} percentile
	Household liquid wealth (€) 5 years prior to individual's SPA		
1949	6,494	25,614	69,113
1951	5,014	24,564	63,119
	Household liquid wealth (€) at individual's SPA		
1949	4,378	23,471	66,105
1951	6,546	27,580	67,372
	Wage income (€/year) of individual 5 years prior to SPA		
1949	0	6,283	31,041
1951	0	13,801	35,453

Notes: This table presents the household liquid wealth quartiles (in euros) 5 years prior (upper panel) and at the individual's SPA (middle panel) and the individual's wage income quartiles 5 years before SPA (lower panel).

The reversal in the liquid wealth levels for the 1949 and 1951 cohorts can be explained by the labour income in the years prior to SPA, as explained below. The lower panel of Table 4.2 shows the percentiles for the wage income of the individuals 5 years prior to their SPA. This is clearly larger for the 1951 cohort compared to the 1949 one. The reason

[2]In Chapter 5 we define liquid wealth and we discuss the reasons we focus on it.

for this is the pillar two policy changes mentioned in Chapter 2. These changes induced individuals to have, on average, longer working lives and, hence, higher incomes the last years prior to reaching SPA (as wage income is on average higher than early retirement income). As a result, they were able to accumulate extra wealth, part of which was saved as liquid wealth.

It is interesting for our later analysis to take a deeper look into how the pillar two policy change affected early retirement. Figure 4.3 shows the distribution of early retirement for

(A) Female individuals 1949

(B) Male individuals 1949

(C) Female individuals 1951

(D) Male individuals 1951

FIGURE 4.3: The distribution of early retirement, in months to SPA, for male and female individuals in the 1949 (panel A & B) and 1951 (panel C & D) cohorts.

male and female individuals in the 1949 and 1951 cohorts. These descriptive statistics indicate that, for the 1949 cohort, the first sizeable wave of early retirement is 60 months prior to SPA (mostly for female, but also to a lesser extend for male individuals), i.e. at 60 years of age. This is due to the fact that pillar two schemes had a default early retirement, some at as early as age 60. This probability mass disappears in the 1951 distribution, both for male as well as for female individuals.

Finally, there are two very interesting points to note in these histograms. The first is that female individuals seem to respond more to the policy change. This can be seen in the

1951 figures, where early retirement essentially disappears in the female distribution, but is only reduced in the male case. The second is for the 1949 cohort. The probability mass at 60 months before SPA is much stronger for females, with males retiring early in a more uniform fashion. One explanation for this is that couples try to retire simultaneously, as shown in Gustman and Steinmeier (2010). As such, given that males are usually the older partners, they postpone retirement until their female partner reaches the age of 60 (or the earlier their pension scheme allowed them to retire), at which point they both retire. This gives rise to a lot of female individuals retiring 60 months prior to SPA and males delaying early retirement. This might have been facilitated through, for example, the pension fund of the health care sector (PfZW) that allowed for a flex-pension starting at age 60.[3]

[3]See example at https://www.pfzw.nl/Documents/brochures-werkgevers/pensioenreglement-en-statuten-2006.pdf.

Chapter 5

Methodology

Our analysis focuses on the effects of a policy change that is enforced at a specific time. In such analyses Differences-in-Differences (diff-in-diff) is a commonly used strategy. The basic assumption of diff-in-diff is that one can recognise two groups, one affected by the policy change and one that was not, that was the policy change not to have taken place, would follow a common trend in the variable of interest. Then, these two groups are compared between the time before and after the policy change, resulting the effect on the treatment group.

Such an analysis is carried out in Lammers et al. (2013). The paper is investigating the effects of a policy change enacted in 2004 in the Netherlands. The reform affected the employment of individuals older than 57.5 years of age. They used this identification and compared individuals just below the threshold age (55.5 to 57.5) and individuals just above (57.5 to 59.5) in the year 2001 (before the policy change) and 2004 (after the policy change). As explained, the fundamental assumption was that individuals just below the threshold age and individuals just above would follow a similar (counterfactual) trend in their employment, was the policy change not to have taken place. Bloemen et al. (2013) use a similar model to investigate the effect of early retirement on mortality. They use a policy change and set up a slightly more complex model than the previous paper. This model uses a triple interaction term (i.e. a differences-in-differences-in-differences) allowing them to control for one more dimension in their treatment effect, something that we also use in our analysis. Below, we describe our methodology per channel investigated.

5.1 Retirement Response

First, we look at the retirement response of the households. As noted in the previous chapter, we have data available on retirement up to and including 2016, hence we investigate the response in terms of cutting short early retirement. That would correspond in equation 3.1 to postponing the period in which L_t of the individual becomes equal to 1. Given that younger partners reach SPA much later than 2016, we are unable to look at their response in the same way, which we do expect to be minimal, though, as explained in Chapter 3. We define early retirement, R, as the number of months prior to reaching SPA that the individual retired, defined as a negative number reaching 0 when the individual retires at SPA or later.

A diff-in-diff-in-diff approach is used, where couples not affected by the change will be used as control. That includes households with individuals in the pre-1950 cohorts and households with individuals in the post-1949 cohorts who have an older partner. Early retirement, R, is taken as the dependent variable. The age difference, A_i, between the two partners (in months) will be the variable indicating whether the treatment is "turned on" or not. When the age difference is negative the partner is older and, therefore, the individual is not affected by the change in the law. For individuals with a younger partner we will observe the effect of the policy change per month of age difference (i.e. per around €750 lost, if their partner's income is very low/zero). Cohort (indicated by t, which is fixed given the individual i) fixed effects are included along with interaction terms. This results to the following regression:

$$\begin{aligned} R_{it} = \sum_{\tau=t_1}^{t_2} \lambda_\tau \delta_{\tau t} + \gamma_A A_i + \gamma_\theta \theta(A_i) + \gamma_{A\theta} A_i \theta(A_i) + \sum_{\tau=t_1+1}^{t_2} \gamma_{\delta_{\tau t}\theta} \delta_{\tau t} \theta(A_i) \\ + \sum_{\tau=t_1+1}^{t_2} \gamma_{\delta_{\tau t} A} \delta_{\tau t} A_i + \sum_{\tau=t_1+1}^{t_2} \alpha_\tau \delta_{\tau t} A_i \theta(A_i) + \beta X_{it} + \epsilon_{it}, \end{aligned} \tag{5.1}$$

where t_1 is the first control cohort we are considering (the base control cohort) and t_2 the last treatment cohort. These can be chosen accordingly in order to perform various robustness checks. A_i is the age difference of the partners, positive for individuals with a younger partner. $\theta(.)$ is the Heaviside step function, which is equal to 1 when the age difference is positive and 0 when it is negative (i.e. it indicates whether the partner is younger or older). $\delta_{\tau t}$ is the Kronecker delta, which is equal to 1 when $\tau = t$ and 0 otherwise (i.e. it indicates whether the individual is in the cohort τ). λ_τ are the cohort fixed effects for cohort τ, γ_A is the age difference fixed effect and α_τ are the treatment effects per cohort, relative to t_1. The γ_θ and $\gamma_{A\theta}$ terms are included, so that the treatment

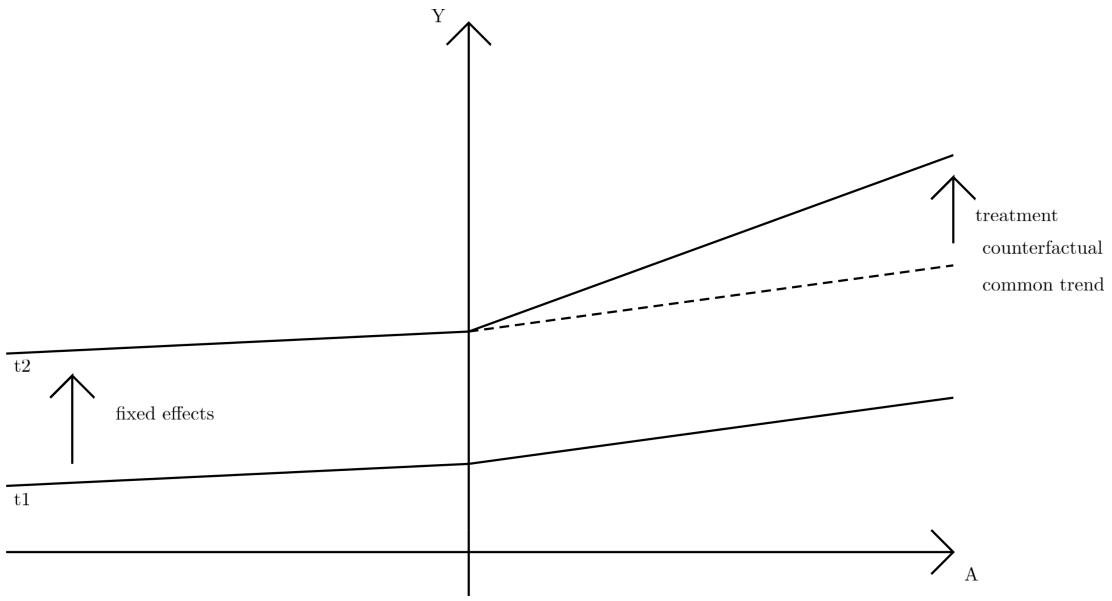

FIGURE 5.1: A graphical representation of the regression (5.1), indicating the difference in cohort fixed effects and the treatment effect. *Notes:* The representation assumes positive γ_A, $\gamma_{A\theta}$, α_{t_2} and zero $\gamma_{\delta_\tau t\theta}$, $\gamma_{\delta_\tau tA}$ and γ_θ.

term is not biased. Individual controls are denoted by X_{it} and ϵ_{it} is the error term. A graphical representation of the two cohort regression, with positive slopes, can be seen in Figure 5.1.

A positive α_τ will signify a postponement of retirement for those affected by the supplement abolishment, compared to the control cohort t_1. As we do not expect high income young partners to respond to the policy change (as, if gross income is more than 1400 euro/month they would not receive a supplement), we restrict the sample to partners that would earn less than €1,400/month. In doing so, we avoid averaging out the real effect over the whole population, as only the subsample of individuals with a low income younger partner would react to the treatment. How should the cutoff be applied depends on whether we expect households to react early, i.e. in the years for which we don't have income data. In our main analysis we regard individuals as not affected by the policy change if 4 and 5 years before they reach SPA their partner has labour income above €20,000/year. As seen in our results in Chapter 6, the lack of early (pre-2007) income reaction ensures that our cutoff does not give rise to a selection bias. The income cap could give rise to an issue with regards to older partners, as they might have already retired, resulting to a low wage income. We, therefore, do a robustness check by explicitly excluding partners that are retired 5 years before the individual's SPA.

Having multiple pre-1950 cohorts amounts to having multiple control groups. The first would serve as the base control cohorts relative to which all coefficients would be calculated. The extra control cohorts would serve as placebo treatment cohorts. We expect

that for all those cohorts α_τ is zero, hence including them serves as a robustness check to our setup and to the base control cohort. Moreover, using multiple post-1949 cohorts allows us to probe the evolution of the response for the various treatment cohorts. These could be different if behavioural responses come into play (assuming all other diff-in-diff assumptions to hold). For example, cohorts that reach SPA soon after the implementation of the law could react in a different manner. In order to avoid this we do not use the 1950 cohort in our analysis.

With regards to extra control variables, it must be taken into account that the supplement was means-tested and lasted until the younger partner reached pension age. Hence, we use the income of the younger partner as a control to account for the means tested aspect. We, also, control for household wealth, as rich couples are expected to react less, given that the wealth shock would be a small proportion of their accumulated wealth. Other demographic indicators can also be used, such as the gender of the individual and the partner, migrant background, etc. Note that these controls would not alter the results, as long as the treatment and control groups are not different in their composition with respect to these controls. However, including them does reduce the variance of the error/residual term, thereby enhancing parameter significance.

Comparing this analysis to the papers discussed above, we use a slightly different interpretation of the diff-in-diff strategy, taking advantage of its symmetry in the identification of the control groups. The standard reading of the diff-in-diff would have been that we take as control group individuals with older partners and as treatment group individuals with younger partner, and that we compare them using cohorts before the policy change and after. In that reading, the assumption would be that these two groups have a common trend in time. These are rather different groups, though, and such assumption would be hard to justify. Hence, we take advantage of the dual interpretation of the diff-in-diff-in-diff and use the age difference of the partners as the relevant variable. We, therefore, use the pre-1950 cohorts as control groups and the post-1950 cohorts as treatment groups. Under such a set-up, it is more intuitive to see why the common trend assumption would hold. In Chapter 6 this intuition comes under scrutiny in a specific case, as the pillar two reform affects the common trend assumption. We investigate that case in Section 6.4.

As a final note, we elaborate a bit further on why age difference is taken as the variable to define the treatment group. A more natural control group would be individuals with a high income partner. The downside with this approach is two-faced. First, we would have to use two or three partner income cutoffs, in order to define the groups, instead of one as in our approach. This could in principle contaminate our results more with early, unobserved responses. Second, in this case we would have been comparing two groups

that seem rather different, i.e. high-income partners vs low-income partners. These results could be affected by time-trends in inequality or the business cycle.

5.2 Savings Responce

As has been discussed in Chapter 3, households are expected to react by increasing their liquid wealth, in order to smoothen consumption during the time of the lost supplement. That would correspond to a reduction of consumption, C_t in equation 3.1, for the periods prior the SPA of the individual. A past savings response will be visible in the stock of wealth later on. Therefore, we can probe a forward looking savings decision in 1996 by looking at wealth data starting at 2006 (the years for which we have wealth data available).

Our assumption is that households use the accumulated wealth in order to finance their consumption during the negative income shock. A house (or other illiquid assets), even though part of the household's wealth, cannot (easily) be used for the purpose of consumption smoothing. Therefore, we chose W^{t^*} to represent the net liquid wealth of the household, i.e. cash, savings and stocks minus non-mortgage loans. House ownership, though, reduces the monthly expenditures. Hence, we also use a control variable indicating home ownership.

We investigate the wealth that an affected household has accumulated in a specific year. More precisely, we look at the wealth a certain number of years before the individual reaches SPA, so that we compare cohorts at the same point of their lives. In doing so, we run an identical regression to (5.1) with the wealth of the household, t^* years before the individual's SPA, W^{t^*}, as the dependent variable.

α_τ will signify the extra wealth accumulated by cohort τ compared to cohort t_1 due to the policy change, t^* years before the respective individual's SPA. We start by choosing t^* being as large as the data allows (e.g. 5 years). The same procedure will be applied for later years (e.g. 4, 3, and 2 years before reaching SPA). The difference of these α_τ over a year indicates the additional savings of the treatment group, relative to the control group. As we look at the wealth near SPA, myopic household could also react. Consumption smoothening, though, predicts a constant savings rate, assuming no other circumstances change. Hence, we should be able to disentangle the two effects, by looking at the second difference of the α_τ for each cohort. If the savings rate increases near SPA, it would mean that households try to save in a short-sighted manner, right before SPA.

5.3 Partner Labour Supply Response

In case the labour supply of the younger partners was increased in the aftermath of the announcement of the abolishment of the AOW supplement, in order to secure an extra, long term, income, that should be visible in their labour supply in the later years. In terms of equation 3.1 that would correspond to an decrease of L_t for the partner in the years after the announcement of the policy change. We use labour income as a proxy of labour supply.

In order to analyse the income data of the partner we set up a different regression model than (5.1). This is due to the fact that the income level includes a two-step decision. The first is whether to work or not, the second is how much to work. If one runs a regression model as for the other variables, there will most likely be a selection bias. That is due to the fact that those not working are most likely of lower capabilities than those that do, on average, hence if they are forced to work they will most likely receive lower income.

We, therefore, set up a Heckman regression (Heckman, 1979). This includes a probit regression on whether one is working or not (i.e. whether the labour income is larger than zero or not) and a linear regression on the income of those that do work, including the inverse Mill's ratio as a dependent variable. The first step, referred to as selection, needs to be non-collinear with the second. Hence, we shall use the liquid wealth of the household as a control in the first step but not in the second. Other than that, the variables used are exactly the same as in Equation (5.1).

The dependent variable will be the wage income of the partner t^* years before the individual reaches SPA. Setting t^* to be equal to 5, 4, 3, etc (as well as negative for those who chose or managed to respond only after the SPA of the individual) we can probe their reaction. In that case, the treatment effect should give us the total (both early and late) response to the change in the law in labour supply. More specifically, the selection regression will give the effect of the policy change in the probability of working, i.e. the employment rate. The linear regression gives the effect on the income received, conditional on being employed.

Chapter 6

Results

In this chapter we present the results of our analysis. First, we present the results on our analysis of the individual's retirement response, discussed in Section 5.1. Second, we present the results for the income response of the younger partner (see Section 5.3) and, third, the results on the savings response of the household (see Section 5.2). We find no significant response in any of the three channels. We also discuss the channel that causes a spurious negative effect to appear in the partner income and savings channels, in Section 6.4.

6.1 Retirement Responce

First, we discuss the results for the response of the households with respect to the individual delaying early retirement. As mentioned in Chapter 4 the retirement data available extend until the end of 2016. This allows us to probe the response of the 1951 cohort, for individuals born in the first six months. We use the 1948 and 1949 cohorts as controls. Initially we use the regression (5.1) without controls. From the sample we exclude individuals who have never worked and those whose partner earned more than €20,000/year 5 and 4 years before the individual reached SPA. We present the results when using 1949 as the base control cohort, then the 1948 as base control group and then using both the 1948 as the base control group and the 1949 cohort as a placebo treatment cohort.

In Table 6.1, columns (1)-(3), quote the variables from Equation (5.1) next to the value of its coefficient given by running the regression, without any extra control variables (X_{it}). Hence, the numbers quoted for δ_{1948t} are the fixed effects on early retirement for the 1948 cohort, and similarly for the other two cohorts. The numbers quoted next to A_i are the

coefficients of early retirement with respect to age difference and similarly for the other variables. The value of interest for us (presented on the top line of the regression tables) is the coefficient of the interaction term $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$, as it encodes the response of the 1951 cohort per month of age difference when the individual has a younger partner, i.e. the response per month of lost supplement. Despite the large sample, we find insignificant results.

The first thing to note from this table is the effect of the pillar-two policy change. This can be seen in the cohort fixed effects, which are given in absolute terms (i.e. there is no constant in the regression). We observe that the fixed effect for 1951 (the coefficient of δ_{1951t}) is much less negative than the corresponding fixed effects for the 1949 (the coefficient of δ_{1949t}) and the 1948 (the coefficient of δ_{1948t}) cohorts. This signifies that the individuals in our sample in the 1951 cohort postpone retirement by an average of 16 months (the difference of the 1951 and 1949 fixed effects). Note that the fixed effects for the 1948 and 1949 cohorts are virtually the same, as no policy change affected them.

The effect of additional control variables

It is important, though, to consider extra control variables for two reasons. First, if a variable is not similarly distributed within the cohorts compared, failing to control for it could be a source of bias in the regression. Second, controlling for them tends to reduce the variation in the residual, which in turn could increase the precision of the results. Columns (4)-(7) in Table 6.1 provide the results of the regression when controls are included. For simplicity, we present the results using only the 1949 cohort as control group, given that they remain largely the same when using the 1948 cohort as well. We run the regression separately for female and male individuals and for male non-homeowners, which are more liquidity constrained. Comparing column (4), that includes controls, to column (1), that does not, we note that the explanatory power of the extra controls is limited, judging by the small increase in R-squared.

Here, Wage^{p}_{b4} signifies the effect of the wage of the partner 4 years before the individual reaches SPA (we do not look further close as they might retire by then). Similarly, Wage_{b5} is the wage of the individual 5 years prior to reaching SPA and Wealth_{b4} the wealth of the household 4 years before SPA. OwnHouse_{b4} is a dummy variable that signifies whether the couple owns the house they stay, 4 years before SPA of the individual. "SPA Apr", etc. are fixed effects of the month the individual reaches SPA, relative to reaching SPA on March. Finally, "First gen." and "Second gen." are fixed effects of the migration status of the individual, relative to not being an immigrant.

First, our variable of interest, $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$, is not significant, with the exception of

TABLE 6.1: Early Retirement Responce with no controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Control group	1949	1948	1948&1949	1949	1949	1949	1949
Population		Analysis sample		Analysis sample	Females	Males	Males non-homeowners
diff-in-diff							
$\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$	0.0401	0.00898	0.00898	0.0613**	-0.00319	0.0154	0.0339
	(1.43)	(0.33)	(0.33)	(2.16)	(-0.03)	(0.33)	(0.42)
δ_{1948t}		-41.93***	-41.93***				
		(-54.49)	(-54.49)				
δ_{1949t}	-39.21***		-39.21***	−41.37***	−30.74***	−40.16***	−41.43***
	(-47.56)		(-47.56)	(-30.07)	(-15.20)	(-23.33)	(-11.65)
δ_{1951t}	-23.53***	-23.53***	-23.53***	−26.22***	−8.055***	−32.05***	−29.81***
	(-27.88)	(-27.88)	(-27.88)	(-15.90)	(-3.21)	(-15.79)	(-7.63)
A_i	0.0114	-0.0144	-0.0144	0.0615***	0.0514**	-0.0190	-0.0148
	(0.65)	(-0.86)	(-0.86)	(3.36)	(2.34)	(-0.54)	(-0.22)
$\delta_{1949t} \cdot A_i$			0.0257				
			(1.06)				
$\delta_{1951t} \cdot A_i$	-0.0469**	-0.0212	-0.0212	−0.0666***	-0.0216	-0.0189	-0.0577
	(-2.02)	(-0.94)	(-0.94)	(-2.81)	(-0.75)	(-0.43)	(-0.77)
$\theta(A_i)$	-1.634	0.480	0.480	0.838	2.436	0.178	0.558
	(-1.51)	(0.47)	(0.47)	(0.76)	(0.90)	(0.13)	(0.18)
$\delta_{1949t} \cdot \theta(A_i)$			-2.115				
			(-1.41)				
$\delta_{1951t} \cdot \theta(A_i)$	-2.988*	-5.103***	-5.103***	-2.553	-4.773	3.360*	2.577
	(-1.88)	(-3.29)	(-3.29)	(-1.62)	(-1.11)	(1.66)	(0.60)
$\theta(A_i) \cdot A_i$	0.0388*	0.0699***	0.0699***	-0.0129	−0.147***	0.0701*	0.0836
	(1.91)	(3.52)	(3.52)	(-0.62)	(-2.68)	(1.92)	(1.21)
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$			-0.0311				
			(-1.09)				
other controls							
Wage^{p}_{b4}(in €1000s)				0.130***	0.262***	0.0933*	-0.117
				(3.24)	(4.14)	(1.87)	(-1.06)
Wealth_{b4}(in €1000s)				−0.00490***	−0.00247*	−0.00566***	−0.0413***
				(-4.61)	(-1.65)	(-3.92)	(-5.00)
OwnHouse_{b4}				−7.817***	−5.521***	−9.081***	
				(-12.85)	(-5.52)	(-11.84)	
SPA Apr				−4.454***	−4.892**	−4.388***	-2.325
				(-3.59)	(-2.03)	(-3.05)	(-0.83)
SPA May				−2.835**	-1.982	−3.206**	-1.247
				(-2.41)	(-0.91)	(-2.30)	(-0.45)
SPA June				−4.672***	−4.632**	−4.695***	-3.847
				(-3.80)	(-1.98)	(-3.26)	(-1.38)
SPA July				−3.379***	−5.046**	−2.743**	-2.583
				(-2.98)	(-2.39)	(-2.05)	(-0.98)
SPA Aug				−4.362***	−5.567***	−3.867***	-3.654
				(-3.75)	(-2.62)	(-2.79)	(-1.31)
SPA Sept				−4.403***	−5.164**	−4.288**	−6.297*
				(-3.01)	(-2.04)	(-2.38)	(-1.74)
SPA Oct				−4.843***	−8.402***	-2.792	-5.923
				(-3.27)	(-3.18)	(-1.56)	(-1.62)
SPA Nov				−3.056**	−4.389*	-2.393	-3.560
				(-2.10)	(-1.76)	(-1.32)	(-0.97)
SPA Dec				−7.700***	−11.06***	−5.967***	−7.227*
				(-5.00)	(-4.04)	(-3.21)	(-1.84)
Female				13.57***			
				(15.72)			
Wage_{b5}(in €1000s)				0.273***	0.184***	0.289***	0.313***
				(15.29)	(6.15)	(13.97)	(6.87)
First gen.				4.354***	2.092	6.036***	8.023***
				(4.27)	(1.19)	(4.83)	(4.63)
Second gen.				-1.153	-2.283	-0.542	1.056
				(-0.96)	(-1.13)	(-0.36)	(0.36)
N	19,261	20,320	29,975	18,513	6,232	12,281	2,982
r2	0.468	0.478	0.504	0.496	0.430	0.529	0.454

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The dependent variable is the individual's early retirement, R_{it}. Column (1) uses 1949 as the base control, regression (2) uses 1948 and (3) uses 1948 as the base cohort and 1949 as a placebo treatment cohort. Columns (4)-(7) include extra controls. Column (4) uses the whole sample, (5) the females, (6) the males and (7) the male non-homeowners. *Notes:* Base month = March, Base immigration status = not immigrant.

the regression in column (4), which results to a positive and significant effect. Looking at columns (5) and (6), though, it is clear that this is due to some non-linear relation between early retirement and the gender dummy. If that was not the case the results in columns (5) and (6) would be similar to column (4), with the exception of lower significance due to the smaller size sample. This is not the case as, for example, can be seen for the coefficients of δ_{1949t} and δ_{1951t} in columns (5) and (6).

The non-linear relation between the dependent variable and the gender dummy variable takes place both through the different response of male and female individual to the pillar two reform, as well as their responsiveness to financial incentives, as discussed below. When we disentangle them, by splitting the sample in terms of gender, we see no effect. Hence, from now on we will present results separately for male and female individuals and will not focus on the results for the whole analysis sample (as in columns (1)-(4)). This means that our analysis, so far, signifies that individuals do not respond to the loss of the supplement by prolonging their working lives. Using 1948 as the control group results to the same outcome.

From Table 6.1 we notice that the cohort fixed effects differ between men and women. Female individuals (column (5)) reduce early retirement a lot more than men do. So, women in our sample respond a lot more to the pillar-two policy change, as we had also observed in our summary statistics in Chapter 4. Next, we consider the parameter estimates of the other control variables in columns (4)-(7). This is not the focus of our analysis, but sensible results indicate the robustness of our regression setup. The coefficient of Wage_{b5} is positive and significant for all regressions. This suggests that people with higher wages extend their working lives. Next, we observe that the coefficient of Wealth_{b4} is negative and significant for all regressions. This, again, logically suggests that individuals with higher wealth retire earlier, as (for example) they feel less financial need to work longer, or save more in order to retire earlier. A similar result is that the coefficient of OwnHouse_{b4} is negative and significant, as owning a house is another form of wealth. The positive effect of Wage^{p}_{b4} probably signifies some attributes that make an individual postpone their retirement and is correlated with their partner's income and we do not control for. That could be education level, for example. It is worth noting, that men seem to be more responsive to financial incentives, as the point estimates for Wage_{b5}, Wealth_{b4}, OwnHouse_{b4} and Wage^{p}_{b4} are larger, in absolute terms, in column (6) compared to column (5).

The months fixed effects are most likely a combination of birth month fixed effects and the decision of individuals to retire later or earlier given the month that is most convenient. Finally, first generation immigrants tend to postpone retirement, most likely due to the

fact that they have spend fewer years in the Netherlands and, hence, have accumulated both less state pension rights due to the residency requirement and less pillar two pension rights due to fewer years worked. Second generation migrants typically have spent their whole life in the Netherlands, explaining why no effect is observed for them.

Robustness analysis

We, finally, explore the effect of choosing the control and treatment groups in various ways. For example, it could be the case that only households where the partner has even lower income than €20,000/year react. Then the response would be visible if we constraint their income in the sample even further. Similarly, we can constraint the wealth of the household, as rich households would have a relatively smaller incentive to save. Given the different retirement behaviour of the cohorts and the fact that we choose our sample by looking at the wage of the partner 5 years before the SPA of the individual we can, also, explicitly exclude partners that have already retired.

Table 6.2 presents the coefficients of the $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ term (i.e. the policy change effect) for various treatment and control group specifications. "Placebo 1948-49" runs a placebo regression where the control group 1948 is compared to the 1949 cohort as a placebo treatment group (i.e. formally the variable of interest is $\delta_{\mathbf{1949t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$). "Not Retired Partner" refers to the sample where the partners, in both the treatment and control groups, have to explicitly not been retired when we select them based on their income, so that selection bias is avoided. "Not high wealth" caps the wealth of the household at €100,000 5 years prior to the individual's SPA, and "Low wealth" caps it at €50,000. "Low partner wage" uses the €100,000 cap on wealth and, moreover, caps the partner income 4 and 5 years before the individual's SPA to €15,000/year. "Placebo high partner wage" uses the same wealth cap, but considers only individuals with partners with income higher that €20,000 - in this case individuals were not affected by the policy change. Finally, "Small Age Difference" constraints the sample to couples with a maximum of 5 years age difference. In this way we minimise any possible non-linear age difference effects.

First, we observe significant results in column (I) for most samples. As discussed before, though, this is just due to the non-linear dependence on the gender dummy variable. Crucially, the coefficient is not significant for the 1948-1949 placebo regression. This re-enforces our earlier explanation that the significance on column (I) is due to the different response of male and female individuals to the pillar-two policy change. Given that no such change occurred between the 1948 and 1949 cohorts, we observe no significant result. Columns (II) and (III) confirm our earlier results, namely the absence of any response in the retirement of the individual. The high partner wage placebo does indicate a significant

TABLE 6.2: Early Retirement Response with various control and treatment specifications

	(I)	(II)	(III)
Population	Analysis sample	Females	Males
Placebo 1948-49	-0.0128	0.0317	0.00120
	(-0.40)	(0.31)	(0.02)
Not Retired Partner	0.146*	-0.0138	-0.0482
	(1.71)	(-0.09)	(-0.29)
Not high wealth	0.0648**	-0.0773	0.0268
	(2.09)	(-0.64)	(0.51)
Low wealth	0.0741**	-0.0559	0.0485
	(2.26)	(-0.44)	(0.88)
Low partner wage	0.0637**	-0.0361	0.0289
	(2.01)	(-0.31)	(0.54)
Placebo high partner wage	-0.216**	-0.395***	0.298
	(-2.02)	(-3.29)	(0.62)
Small Age Difference	0.106	0.372	-0.104
	(1.36)	(1.31)	(-0.88)

t statistics in parentheses

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

R_{it}, the early retirement of the individual, is regressed using the extra controls. The coefficient of $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ is displayed for the various sample specifications. Estimates in column (I) use the whole sample, (II) only female individuals and (III) only male.

effect, but this is most likely due to the small sample of this specification. It includes only around 200 female individuals with younger partner.

Summarising, our results indicate that individuals do not respond to the policy change by postponing early retirement. This is confirmed by using both 1948 and 1949 as the base control cohort, as well as using different controls and treatment group specifications with regards to wage and wealth levels. The next subsection explores whether households adjust to the income shock by means of the partner increasing their labour supply.

6.2 Partner Income Response

Next, we present the results for the income response of the partner. We use the 1948 and 1949 cohorts as control groups (the first as the base control cohort and the second

as a placebo treatment cohort) and 1951 and 1952 as treatment groups. As discussed in the methodology, we use a Heckman two-step regression, that has the same set-up for the two steps as is Equation 5.1. The sample excludes individuals whose partner earned more than €20,000 5 and 4 years before they reached SPA. We also exclude households with wealth larger than €100,000 5 years before the individual's SPA, but the results are largely the same when we do not make this selection.

First, we present in Table 6.3 the results for the income of the partner in the calendar year the individual reaches SPA. Given the established non-linearity of the gender dummy we do not present the full sample results. Column (1) displays the results for the female sample, (2) for the male, (3) for the male sample that own their house and (4) for those male individuals that do not. The separation on homeownership is not presented for female individuals, mainly because the sample is not large enough (not many female individuals have a younger partner) and therefore most of the results are insignificant.

The full results for this regression are presented, in order to scrutinise them, as in the previous case. We present the results for both stages of the Heckman two-stage regression. The names for the coefficients follow closely those of Table 6.1. The coefficients of $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ are insignificant in both stages.

Columns (1)-(4) of Table 6.3 display the results of stage one of the Heckman two-step regression, i.e. the selection of whether the partner is working or not. This is a probit regression on the employment of the partners. The positive coefficient of $\theta(A_i)$ indicates that younger partners have a higher probability of working. This is expected, as older partners at the SPA of the individual are already retired.

Columns (5)-(8) display the results of stage two. This is a regression on the level of the income of the partners who work. With respect to the control variables, we observe the following. As expected, owning a house has a negative effect on the income of the partner (seems positive for male individuals, but only due to non-linearity of house-ownership as seen by (7) and (8)). First generation immigrants have a higher income, more likely due to the fact that they postpone retirement as seen in the previous section, but are less likely to work. Finally, we note that the inverse Mill's ratio is significant in columns (5) and (6), indicating that minor selection effects are indeed present.

Partner employment response before the individual reaches SPA

When running the same regression for the income of the partner in the years before the individual's SPA we observe significant results. More specifically we observe this result for male individuals, mainly in the first step of the Heckman regression and to a lesser

TABLE 6.3: Wage of Partner at SPA of individual.

	Employment				Income level			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population	Females	Males	Males homeowners	Males non-homeowners	Females	Males	Males homeowners	Males non-homeowners
diff-in-diff								
$\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$	0.000738	-0.00428	-0.00420	-0.00398	49.56	-18.45	1.680	30.97
	(0.35)	(-1.53)	(-1.24)	(-0.80)	(0.42)	(-0.44)	(0.03)	(0.46)
δ_{1948t}	−1.031***	−1.549***	−1.324***	−1.635***	89192.4***	-12487.2	-733.3	12345.3
	(-15.99)	(-21.93)	(-10.00)	(-12.36)	(3.41)	(-1.28)	(-0.08)	(0.85)
δ_{1949t}	−1.083***	−1.587***	−1.353***	−1.658***	89555.7***	-13332.5	-1681.0	11027.8
	(-15.29)	(-21.15)	(-9.92)	(-11.74)	(3.32)	(-1.35)	(-0.19)	(0.75)
δ_{1951t}	−1.168***	−1.383***	−1.138***	−1.561***	93074.5***	-11151.2	166.3	11069.8
	(-16.06)	(-18.18)	(-8.46)	(-10.01)	(3.32)	(-1.23)	(0.02)	(0.79)
A_i	0.00552***	0.00700***	0.00776***	0.00583*	−190.3**	82.24**	68.87	-7.443
	(8.69)	(3.55)	(3.16)	(1.77)	(-2.22)	(1.98)	(1.42)	(-0.13)
$\delta_{1949t} \cdot A_i$	0.000449	-0.00244	-0.000944	-0.00402	-48.94	-36.41	-67.18	35.74
	(0.47)	(-0.92)	(-0.27)	(-0.94)	(-0.86)	(-0.93)	(-1.33)	(0.62)
$\delta_{1951t} \cdot A_i$	-0.00110	0.00308	0.00297	0.00240	14.34	9.492	-6.685	-34.15
	(-1.23)	(1.12)	(0.90)	(0.49)	(0.26)	(0.24)	(-0.14)	(-0.53)
$\theta(A_i)$	0.133**	0.388***	0.350***	0.434***	-235.2	5302.8***	3506.0**	310.5
	(2.02)	(6.91)	(5.26)	(4.05)	(-0.06)	(2.87)	(1.99)	(0.10)
$\delta_{1949t} \cdot \theta(A_i)$	-0.0354	0.106	0.0856	0.120	2012.3	1551.7	1528.4	1349.4
	(-0.36)	(1.30)	(0.88)	(0.76)	(0.36)	(1.30)	(1.16)	(0.66)
$\delta_{1951t} \cdot \theta(A_i)$	0.153	0.00766	0.00651	0.0613	-7941.2	642.1	454.2	2381.6
	(1.48)	(0.09)	(0.07)	(0.36)	(-1.28)	(0.58)	(0.38)	(1.12)
$\theta(A_i) \cdot A_i$	-0.00121	-0.000319	-0.000325	0.0000321	57.92	-12.31	-14.27	13.33
	(-0.86)	(-0.16)	(-0.13)	(0.01)	(0.72)	(-0.42)	(-0.38)	(0.32)
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	0.00147	0.00146	0.000317	0.00226	-8.778	28.71	60.68	-30.86
	(0.71)	(0.54)	(0.09)	(0.52)	(-0.08)	(0.74)	(1.19)	(-0.57)
other controls								
OwnHouse_{b5}	0.0928***	0.211***	0.0175	0.0177	−3385.9*	1754.8**	-1193.3	-413.0
	(4.16)	(11.51)	(0.17)	(0.24)	(-1.84)	(2.18)	(-1.10)	(-0.55)
SPA Feb	-0.0683	−0.119**	−0.194***	0.0478	2978.5	-264.4	-596.0	1684.1
	(-0.94)	(-1.99)	(-2.70)	(0.43)	(0.68)	(-0.30)	(-0.57)	(1.41)
SPA Mar	-0.0186	-0.0370	-0.0626	0.0167	2391.6	629.5	150.8	2111.5*
	(-0.27)	(-0.66)	(-0.94)	(0.16)	(0.59)	(0.89)	(0.20)	(1.86)
SPA Apr	0.00425	-0.0409	-0.0860	0.0817	755.8	-185.4	-568.1	804.9
	(0.06)	(-0.73)	(-1.30)	(0.78)	(0.19)	(-0.26)	(-0.74)	(0.68)
SPA May	-0.0116	0.00584	-0.0476	0.138	2776.6	564.2	25.75	967.0
	(-0.17)	(0.10)	(-0.72)	(1.30)	(0.69)	(0.82)	(0.04)	(0.73)
SPA Jun	0.0258	-0.0104	-0.0748	0.148	342.9	558.7	-150.7	1714.5
	(0.37)	(-0.19)	(-1.13)	(1.41)	(0.08)	(0.81)	(-0.20)	(1.27)
SPA Jul	0.0550	0.0288	-0.00629	0.117	5.660	935.2	371.0	1269.7
	(0.83)	(0.53)	(-0.10)	(1.13)	(0.00)	(1.38)	(0.54)	(1.01)
SPA Aug	0.0296	0.0352	0.0398	0.0247	-148.8	1182.1*	429.3	2726.1**
	(0.44)	(0.64)	(0.61)	(0.24)	(-0.04)	(1.73)	(0.61)	(2.48)
SPA Sep	0.0629	0.0451	0.0338	0.0813	-1827.8	653.8	283.1	463.0
	(0.94)	(0.83)	(0.53)	(0.78)	(-0.46)	(0.95)	(0.41)	(0.40)
SPA Oct	0.0348	0.0736	0.000827	0.256**	2392.1	1365.8*	479.1	1316.0
	(0.50)	(1.32)	(0.01)	(2.43)	(0.59)	(1.86)	(0.68)	(0.76)
SPA Nov	0.0571	0.0495	-0.00179	0.164	-635.0	1082.4	407.2	1184.2
	(0.83)	(0.89)	(-0.03)	(1.56)	(-0.16)	(1.53)	(0.58)	(0.83)
SPA Dec	0.0254	0.132**	0.0875	0.228**	1756.7	1741.8**	831.4	1027.4
	(0.36)	(2.36)	(1.32)	(2.13)	(0.43)	(2.09)	(1.09)	(0.63)
Wage_0(in €1000s)	0.00925***	0.00455***	0.00376***	0.00917***	-132	68.9***	53.4***	-2.17
	(5.12)	(8.94)	(6.88)	(6.26)	(-0.79)	(4.09)	(3.90)	(-0.04)
First gen.	−0.176***	−0.232***	0.00545	−0.379***	8462.6**	-788.3	1072.6**	1614.0
	(-4.21)	(-7.52)	(0.12)	(-8.45)	(2.46)	(-0.85)	(2.41)	(0.76)
Second gen.	-0.0391	-0.0237	-0.0212	-0.0316	2480.9	612.8	608.7	1002.4
	(-0.84)	(-0.65)	(-0.49)	(-0.45)	(0.89)	(1.35)	(1.32)	(1.35)
Wealth_{b5}(in €1000s)	−0.000282***	0.000126**	0.000109**	0.00110*				
	(-4.96)	(2.41)	(2.17)	(1.87)				
Mills lambda					−52664.0***	9347.3*	4644.6	-3176.0
					(-3.15)	(1.88)	(0.95)	(-0.45)
N	26,984	32,758	22,921	9,837	26,984	32,758	22,921	9,837

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The dependent variable is the income of the partner. The table displays the results for the two steps of the Heckman regression for the wage of the partner at the individual's SPA. Regressions (1) & (5) use the female sample and (2) & (6) the male. (3) & (7) use male individuals that own their house and (4) & (8) male individuals that do not. *Notes:* Base month = March, Base immigration status = not immigrant.

extend in the second. The result is driven by male individuals that own their home, so we will focus on them.

In order to elaborate, we present the evolution of the effect on the probability of working (i.e. the selection of the Heckman process) over time, for male homeowners (i.e. for their mostly female partners). Figure 6.1 is a cohort-time graph (see Bosch et al., 2010). It plots the coefficients of $\delta_{\tau \mathbf{t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ for cohorts 1949, 1951 and 1952 over time, with the zero of the x-axis at the SPA of the individual. It, therefore, gives the effect of the policy relative to the 1948 base cohort. For the 1952 cohort, data is available only up to one year before SPA. The coefficients are seen as scatter-points with the corresponding error bars that represent the 95% confidence level.

As expected, the 1949 cohort gives insignificant effects, as no policy change took place between 1948 and 1949. For the other two cohorts we see a negative effect, that grows and becomes significant two and one year before the SPA of the individual, before becoming insignificant again at SPA. The fact that both 1951 and 1952 cohorts give very similar point estimates makes the evidence stronger. Therefore, the data suggests that the probability of the younger partner having an income, just before the individual reaches SPA, is significantly reduced by the policy. One explanation for this effect could be liquidity constraints, as discussed in Chapter 3. We check this hypothesis by exploring the regression results given another construction of the control and treatment groups

Our assumption was that the term would probe the reaction of the households due to the discontinuation of the AOW supplement. That is due to the fact that it would only affect individuals, born after 1949, with younger partners. Moreover, the effect would be dependent on the age difference of the two, as that would determine the lost wealth to the household. As discussed, though, in Chapter 2, the second pillar of the pension system was reformed, effective on the same date as the AOW supplement reform for the 1950 cohort. Although this reform did not have any explicit dependence on the age difference on the partners, the reaction of the households is rather complex, as seen in Figure 4.3. Hence, the term $\delta_{\tau \mathbf{t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ could, in principle, pick up part of that reaction as well.

The crucial difference, though, is that the pillar two reform affected all individuals, whereas the AOW supplement abolishment only those with a low-income younger partner. We, therefore, investigate to what extend the effect is visible in households with high-income younger partners as well. Table 6.4 presents the coefficients of the relevant terms, for the years around the SPA of the individual. The first three terms are the ones plotted in Figure 6.1, i.e. calculated using a sample where the income of the partner is capped at 20,000 a year. The second part of the table was calculated using a sample with

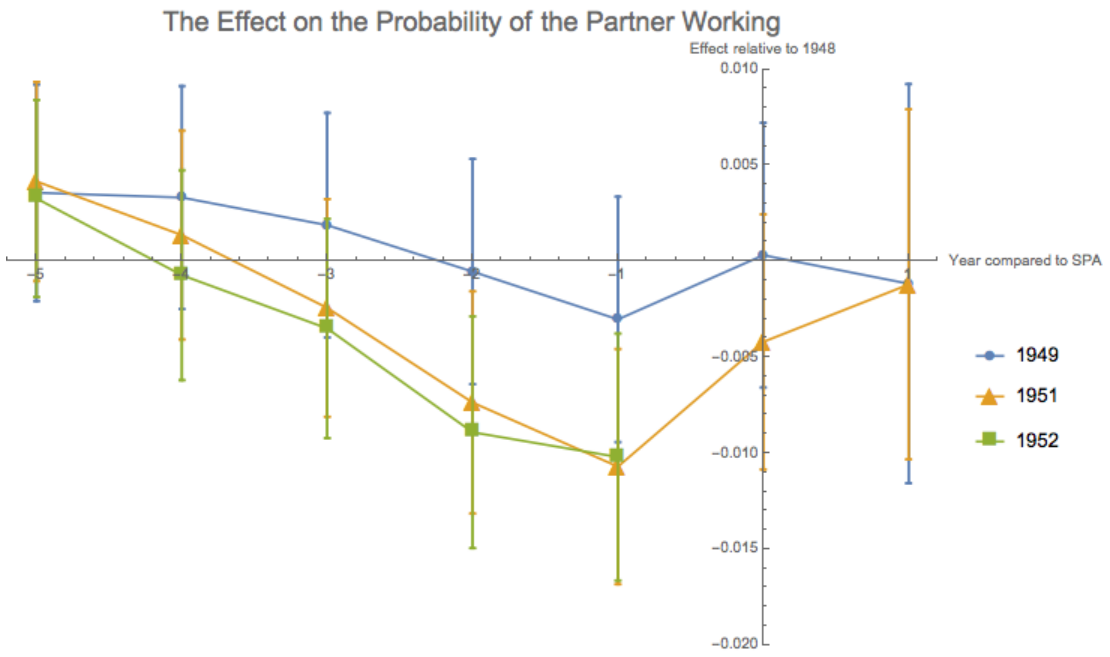

FIGURE 6.1: A graphical representation of the coefficients of the term $\delta_{\tau \mathbf{t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$, relative to 1948, for the probability of the partner working around the individual's SPA. The blue dotted curve corresponds to the cohort 1949, the orange triangle curve to 1951 and the green squared curve to 1952. The error bars represent the 95% confidence level. *Note:* The sample used is male individual homeowners, i.e. the effect is for the mostly female partners of male homeowners. Similar results are found for the level of the income, but with lower significance.

no constraint on the partner's income. The point estimates are essentially the same for the two samples. The significance increases in the second sample due to the sample size; it consists of roughly 30% more observations.

This indicates that the identified effect is present to the same extend in households with high-income young partners as well. The lack of any difference in the estimates between the low income sample and all-income sample strongly indicates that there is no effect to the employment nor the income level of the partner due to the AOW supplement abolishment. Otherwise, it would have created a differential in the two estimates. We, finally, note that similar results arise when undertaking robustness checks such as the ones presented in Table 6.2. Considering households with only high wealth, the results remain largely the same, providing evidence that the effect is not due to liquidity constraints. After presenting the results for the savings channel, in which we observe a similar negative effect, we provide more evidence on its pillar two reform origin in Section 6.4.

Summarising, we find no response to the employment and the income level of the younger partner, in addition to the lack of response in the retirement of the individual, as a results of the abolishment of the AOW supplement. In the next subsection, we investigate whether there is a savings response to the reform.

TABLE 6.4: Effect on probability of work for low and any income partner

	(1) SPA-3	(2) SPA-2	(3) SPA-1	(4) SPA	(5) SPA+1
			Low income partner		
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	0.00188	-0.000539	-0.00303	0.000317	-0.00117
	(0.63)	(-0.18)	(-0.93)	(0.09)	(-0.22)
$\boldsymbol{\delta_{1951t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$	-0.00243	-0.00736**	-0.0107***	-0.00420	-0.00121
	(-0.84)	(-2.49)	(-3.42)	(-1.24)	(-0.26)
$\boldsymbol{\delta_{1952t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$	-0.00350	-0.00891***	-0.0102***		
	(-1.20)	(-2.89)	(-3.11)		
			Any income partner		
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	0.00160	-0.000590	-0.00311	0.00123	-0.00221
	(0.54)	(-0.20)	(-0.96)	(0.35)	(-0.43)
$\boldsymbol{\delta_{1951t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$	-0.00137	-0.00782***	-0.0114***	-0.00494	-0.00133
	(-0.50)	(-2.74)	(-3.76)	(-1.47)	(-0.29)
$\boldsymbol{\delta_{1952t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$	-0.00424	-0.0102***	-0.0133***		
	(-1.53)	(-3.46)	(-4.18)		

t statistics in parentheses

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

The coefficients of $\boldsymbol{\delta_{\tau t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$ for the cohorts 1949, 1951 & 1952 are given, from three years prior to SPA up to one year after. The top panel displays the results for the sample in which the partner income 5 and 4 years before SPA has been capped to €20,000/year. The bottom panel displays the results for the sample with no constraint on the partner's income. *Note:* The sample used is male individual homeowners, i.e. the effect is for the mostly female partners of male homeowners.

6.3 Savings Response

We present the results for the savings response of the household. As mentioned, we do so by looking at the response of the households through their stock of liquid wealth. We use a regression similar to Equation 5.1 but with the liquid wealth of the household t^* years prior to the individual's SPA, W^{t^*}, as the dependent variable. Similarly to Table 6.3, Table 6.5 presents the full regression results for the wealth of the household at the individual's SPA. The results are given for female individuals in column (1), for male individuals in (2), and for male individual homeowners and non-homeowners in (3) and (4) respectively.

In Table 6.5 we observe a clear increase in the stock of wealth for the cohort 1951 (seen by comparing δ_{1951t} with δ_{1948t} and δ_{1949t}). This is, at least partly, due to the extension of

TABLE 6.5: Regression on the household Wealth at the individual's SPA.

	(1)	(2)	(3)	(4)
Population	Females	Males homeowners	Males	Males non-homeowners
diff-in-diff				
$\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$	-172.3	−410.4**	-525.4	−199.9*
	(-0.99)	(-2.08)	(-1.63)	(-1.88)
δ_{1948t}	8968.2	20696.7***	23288.0**	13516.0***
	(1.16)	(2.92)	(2.30)	(4.10)
δ_{1949t}	8209.9	11016.1	8283.1	15767.2***
	(0.87)	(0.98)	(0.55)	(4.42)
δ_{1951t}	10870.5	32970.7***	41102.7***	14595.8***
	(1.04)	(2.86)	(2.63)	(3.57)
A_i	-59.57	−181.7**	−233.9*	-110.9
	(-1.08)	(-1.96)	(-1.71)	(-1.16)
$\delta_{1949t} \cdot A_i$	-197.0	-62.39	-199.4	131.3
	(-0.79)	(-0.25)	(-0.50)	(1.26)
$\delta_{1951t} \cdot A_i$	-0.696	347.1**	457.6*	177.8*
	(-0.01)	(2.20)	(1.77)	(1.72)
$\theta(A_i)$	−28286.7**	6614.4	9440.3	2782.1
	(-2.14)	(1.12)	(1.11)	(1.00)
$\delta_{1949t} \cdot \theta(A_i)$	21787.1	5349.3	9175.1	-4445.8
	(1.54)	(0.38)	(0.46)	(-0.95)
$\delta_{1951t} \cdot \theta(A_i)$	35677.4**	-4104.9	-6576.4	-865.2
	(2.38)	(-0.39)	(-0.45)	(-0.21)
$\theta(A_i) \cdot A_i$	260.9*	42.39	26.02	86.89
	(1.85)	(0.36)	(0.14)	(0.89)
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	87.28	98.06	283.2	-164.0
	(0.31)	(0.37)	(0.66)	(-1.52)
other controls				
OwnHouse_0	8161.5	207.9		
	(1.35)	(0.08)		
Wage_{b5}	0.0822	-0.148	-0.203	0.115
	(0.42)	(-0.68)	(-0.78)	(1.12)
SPA Mar	1538.1	-1569.4	-2286.9	-144.9
	(0.16)	(-0.31)	(-0.31)	(-0.06)
SPA Apr	46428.2	-7892.4	-12297.1	1256.2
	(1.64)	(-1.00)	(-1.09)	(0.56)
SPA May	-3277.0	-2997.5	-5358.9	1547.2
	(-0.34)	(-0.55)	(-0.68)	(0.68)
SPA June	3304.4	8150.5	10971.6	511.8
	(0.34)	(1.22)	(1.11)	(0.24)
SPA July	3567.9	-3123.8	-5351.2	281.7
	(0.37)	(-0.54)	(-0.65)	(0.11)
SPA Aug	97.81	-391.2	-1939.2	2152.1
	(0.01)	(-0.06)	(-0.20)	(0.77)
SPA Sept	7357.5	-16724.5	-25426.6	5082.6
	(0.64)	(-1.01)	(-1.12)	(1.26)
SPA Oct	-911.4	-11835.9	-19059.8	3988.8
	(-0.08)	(-0.81)	(-0.94)	(0.96)
SPA Nov	8155.4	3399.9	3583.3	1409.2
	(0.72)	(0.43)	(0.32)	(0.41)
SPA Dec	-1904.0	3646.8	1775.9	5380.7
	(-0.15)	(0.46)	(0.16)	(1.36)
First gen.	−20238.9***	901.7	9586.7*	−6899.0***
	(-3.43)	(0.32)	(1.91)	(-4.04)
Second gen.	-5840.1	5331.5	8175.6*	-1194.1
	(-1.12)	(1.49)	(1.66)	(-0.51)
N	13,658	16,801	11,788	5,013
r2	0.00507	0.00659	0.00586	0.122

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The wealth of the household at the individual's SPA, W^0, is the dependent variable. Regression (1) uses the sample with female individuals and (2) with male. (3) includes only male homeowners and (4) male non-homeowners. *Notes:* Base month = March, Base immigration status = not immigrant.

their working lives, as a result to the pillar two reform. In column (2) (at 95% confidence level) and in column (4) (at 90% confidence level) we note a negative and significant effect in the coefficient of the $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ term. This seems to follow the previous results on the labour participation of the parters. This time the significant result arises for non-homeowners (column (4)). For homeowners (column (3)) the significance is slightly below 90%, lower than in the partner income case, possibly due to the higher volatility of wealth compared to income. As in the partner income case we focus on male homeowners, due to their higher point estimates.

In order to investigate the evolution of the effect on liquid wealth we create a year-cohort graph in Figure 6.2, similar to Figure 6.1. We plot the effect on the liquid wealth of the households, for male homeowners, for the cohorts 1949, 1951 and 1952. There's slightly less data available for the wealth of the household compared to wages, hence we can only explore the effect up to SPA of the individual.

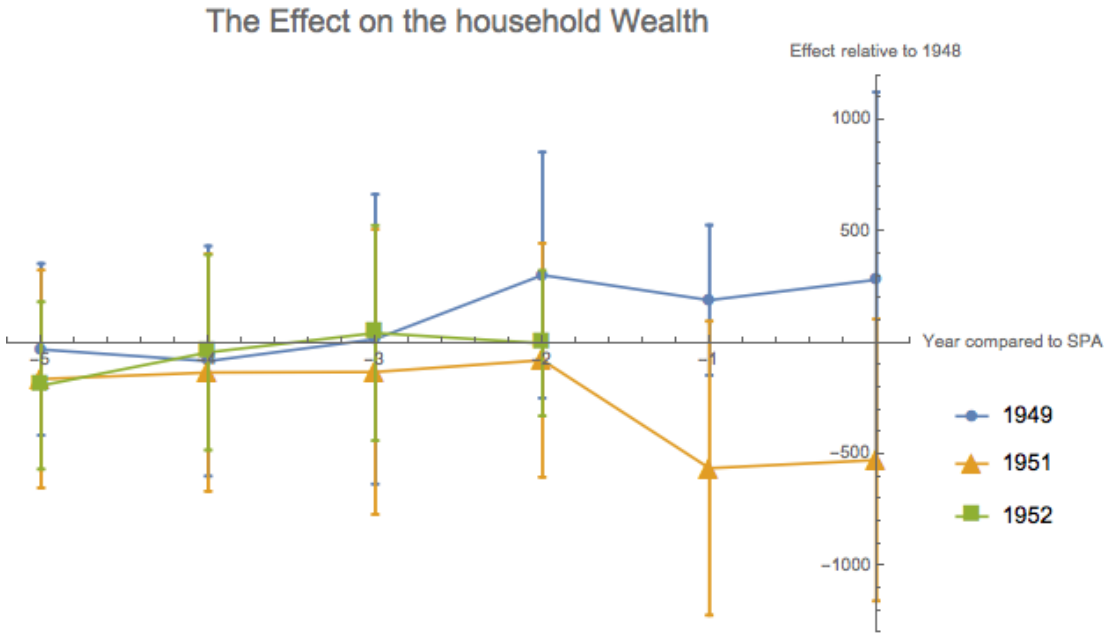

FIGURE 6.2: A graphical representation of the coefficients of the term $\delta_{\tau \mathbf{t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$, relative to 1948, for the liquid wealth of the households around the individual's SPA. The blue dotted curve corresponds to the cohort 1949, the orange triangle curve to 1951 and the green squared curve to 1952. The error bars represent the 95% confidence level. *Note:* The sample used is male individual homeowners, i.e. the effect is for the mostly female partners of male homeowners.

It is clear from the graph that the effect on the households' wealth is significantly negative, per month of age difference, with a level of confidence of around 90% compared to the 1948 cohort and around 95% compared to the 1949. The point estimate of around €500 per month of age difference, i.e. around €42,000 for the median age difference of 7 years

is quite large. The uncertainty of this point estimate is substantial, though, given the larger variance of wealth compared to income. What is striking is that this seems to be a direct result, or at least connected to, the labour response noted in the previous section. Comparing Figures 6.2 and 6.1 we note that the wealth effect is rather similar to the partner wage effect, with a lag of one year (most likely as the relative loss of wage would show up in next year's wealth stock).

We, therefore, create a table similar to Table 6.4, Table 6.6, which shows, once again, how the partner wage cutoff is irrelevant for the effects observed. Hence, using the same argument we conclude that the response in terms of saving, due to the supplement abolishment, is also insignificant.

TABLE 6.6: Effect on the household wealth

	SPA-3	SPA-2	SPA-1	SPA
	Low income partner			
	(1)	(2)	(3)	(4)
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	16.56	303.8	191.5	283.2
	(0.05)	(1.08)	(0.80)	(0.66)
$\delta_{1951t} \cdot \theta(A_i) \cdot A_i$	-130.4	-77.47	-561.9*	-525.4
	(-0.40)	(-0.29)	(-1.67)	(-1.63)
	Any income partner			
	(5)	(6)	(7)	(8)
$\delta_{1949t} \cdot \theta(A_i) \cdot A_i$	-35.35	264.6	182.9	224.0
	(-0.12)	(1.00)	(0.82)	(0.56)
$\delta_{1951t} \cdot \theta(A_i) \cdot A_i$	-199.2	-129.0	-452.4	-458.1
	(-0.68)	(-0.51)	(-1.50)	(-1.64)

t statistics in parentheses

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

W^{t^*} is regressed. The coefficients of $\delta_{\tau \mathbf{t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ for the cohorts 1949 & 1951 are given, from three years prior to SPA up to SPA. The top panel coefficients ((1)-(4)) are for the sample in which partner income 5 and 4 years before SPA has been capped below 20,000 euros. The bottom panel ((5)-(8)) refers to the sample with no constraint on partner income.

Summarising, we find no significant response in the liquid wealth of the households due to the AOW abolishment. Hence, we have investigated three possible response channels, namely the retirement of the individual, the income of the partner and the wealth of the household. Our estimates indicate that the households do not respond through any of these channels. In the last section of this chapter we provide a concrete interpretation of the significant negative effect displayed both in the income of the partner, as well as in the wealth of the household.

6.4 A Pillar Two Reform Effect

Now, we turn our attention to interpreting the negative effect noted in the income of the partner and the wealth of the households. As already noted, this effect is not due to the AOW supplement change, as it affects households were the younger partner has high income as well. In Chapter 4 we discussed that it has been shown that partners try to coordinate their retirement (Gustman and Steinmeier, 2010). Given that at the same time with the abolition of the AOW supplement the pillar two reform took place, complicated responses might arise for partners in different pillar two systems. An individual in the post-1949 cohorts will retire in the same pillar two system as their younger partner. If the partner is older, though, they might be born before 1950 and, therefore, retire under the old system.

TABLE 6.7: Effect on the partner employment at SPA-2

	(1) Male	(2) Male homeowners
$\delta_{1951t} \cdot A_i$	0.00419*	0.00591**
	(1.67)	(2.11)
$\delta_{1952t} \cdot A_i$	0.00561**	0.00956***
	(2.24)	(3.28)
$\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$	-0.00586**	-0.00782***
	(-2.31)	(-2.74)
$\delta_{\mathbf{1952t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$	-0.00681***	-0.0102***
	(-2.68)	(-3.46)
N	42758	30136

t statistics in parentheses

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

Selected coefficients of the regression on the partner employment two years before the individual's SPA are displyed. Regression (1) uses the male sample & (2) the male homeowners.

Table 6.7 displays selected coefficients on the probability of the partner working (i.e. the employment probit) two years before the individual's SPA. No cap has been put on the partner's income, given that we have established that the effect appears for partners of any income. The results are presented for male individuals (column (1)) and male individuals with homeownership (column (2)). As we have already noticed, we observe a negative effect for $\delta_{\mathbf{1951t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ and $\delta_{\mathbf{1952t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$. It is worth remembering, though, that this effect is not exactly the response of younger partners with an individual in the 1951 cohort compared to an individual in the 1949 cohort. It is the effect compared

to the 1949 cohort and compared to the individuals with an older partner in the 1951 cohort; as we run a differences-in-differences-in-differences. We use the individuals with an older partner to set the slope for the 1951 cohort, above which we measure the effect of the policy.

Given the hypothesis that individuals with an older partner are affected by the pillar two reform, we can observe the regression considering individuals with a younger partner setting the cohort trend instead. The effect on individuals with a younger partner in the 1951 cohort compared to the 1949 cohort (and not compared to individuals with an older partner) is given by the sum of $\delta_{1951t} \cdot A_i$ and $\boldsymbol{\delta_{1951t} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}}$. These two coefficients cancel out almost perfectly to give a net effect of zero, relative to the 1949 cohort (given the errors). The same holds for the 1952 cohort, for regressions on the income a different number of years away from the individual's SPA, as well as for regressions on the household wealth. This indicates no effect for individuals with younger partners (i.e. no reaction to the AOW supplement abolition), and a positive effect for individuals with an older partner.

This positive effect means that, relatively to older partners of individuals in the 1949 cohort, the larger the age difference the more likely is the partner to work. Given that age difference is negative for individuals with an older partner, this means that the younger the older partner is the more likely they are to work (compared to the control group). The reason for that is that these partners have an incentive to delay retirement, despite the fact that they are in the old pillar two system. The incentive is that their younger partner is unable to retire early, as they are in the new pillar two system, hence they need to delay retirement in order to retire together. And the reason this effect is a per age difference effect is that the younger the partner is the more able they are to postpone retirement. This hypothesis can, also, explain why the effect goes to zero at the individual's SPA. The reason is that at that time the individual can retire and hence the older partner does so as well. We have focused on male individuals (i.e. the mostly female partners' response), The reason that the result is stronger for homeowners is due to the fact that they are less liquidity constrained, hence they retired earlier to start with.

Finally, this hypothesis will become testable in the future, when data becomes available for later cohorts, e.g. 1955. These individuals, if they have a younger partner, will, relative to the 1949 cohort, be affected by the AOW supplement abolishment. But, for those with an older partner, most of their partners will be born after 1949 and, hence, will be in the new pillar two system as well. Running the same regression should, assuming that these cohorts are still comparable in every other (uncontrolled) way, result to insignificant

results for the coefficients of $\delta_{\mathbf{1955t}} \cdot \theta(\mathbf{A_i}) \cdot \mathbf{A_i}$ and $\delta_{1955t} \cdot A_i$. We, now, turn to the discussion section, where we bring together our results and attempt to interpret them.

Chapter 7

Discussion

Our results indicate that households did not react to the discontinuation of the AOW supplement, neither in a forward looking manner nor in a short-sighted one. More precisely, they did not react in the years prior to being affected by the change, as would be expected according to consumption smoothening. Our data does not reach the years after the individual reaches SPA, except for the income of the 1951 cohort that reaches until SPA+1, but results to an insignificant effect. Below, we discuss possible interpretations of our results. The first attributes the lack of significant responses to behavioural effects. Then, we discuss various scenarios of lack of information. Finally, we point out the implications of our results on policy decisions.

We find no effect on the households' retirement planning, despite the size of the shock. For a household where the partners have the average age difference of around 7 years, the total lifetime wealth shock due to the lost income would accumulate to around €70,000, if the younger partner has very low or no income. One explanation, discussed in Section 3.1, could be that pensioners cover the wealth loss with the AIO supplement.[1] This supplement ensures that couples live above the minimum subsistence level, equal to the minimum wage for a couple. However, in order for the partners to receive this supplement, they would be required to have little or no income other than the AOW of the older partner, and a maximum of €12,040 in assets. Therefore, only a very small part of our sample would be eligible, and, as it is discussed below, a majority of the eligible pensioners is unaware of the supplement itself.

Hence, the lack of response is seen to be due to behavioural responses, such as those discussed in Section 3.2, or due to lack of information (or both). One interpretation of the lack of a savings response is time-inconsistency, leading households to be extremely short

[1] See https://www.svb.nl/int/nl/aio/hoeveel_bijstand/hoeveel_eigen_vermogen/index.jsp.

sighted and not saving extra due to the supplement abolishment. A second interpretation is in terms of the mental accounting framework (Thaler, 1990). As discussed in Section 3.2, under this framework wealth is non-fungible. This leads households to consider future income wealth as a different kind of wealth compared to current income and assets. As such, the marginal propensity to consume was considered by Thaler to be close to zero. That would imply a zero wealth effect on both consumption on goods and services (i.e. savings) and leisure (i.e. labour supply). Our results can be seen as a positive test of this hypothesis, as the abolishment of the supplement is a future income shock on the households' budgets that is found to have no effect on the households' consumption and labour supply.

A final interpretation of the results is lack of information, in various forms. The most obvious is that either the individuals in the cohorts prior to 1950 were not fully aware of the existence of supplement, or the post-1949 cohorts were not aware of its discontinuation. In any of the two cases cohorts would have had similar expectations about their future income. That would lead to no effect being observed, as found in our analysis, as any response would have been fully dependent on these expectations. Note that, it is not required that individuals are aware of the discontinuation itself. As long as both cohorts have informed views of their (different) future income they are expected make different retirement decisions.

The lack of knowledge about the policy change was noted by the institutions responsible for implementing it - e.g. the Sociale Verzekeringsbank (SVB). Individuals in the post-1949 cohorts received a letter from the SVB in 2010 notifying them of the policy change. The main goal of that action (that was clearly stated in the letter) was to incentivise the younger partners to work. It did not make explicit the loss in income, hence it is unclear if it was informative in terms of reshaping their expectations on their future income. Moreover, it has already being shown in Van Eekelen and Kridene-Lageman (2017) that pensioners fail in large numbers to claim their benefits, even in cases where they are below subsistence level. More precisely, households that received a full AOW pension and the AOW supplement would reach the minimum subsistence level. When the supplement was abolished, couples with little or no additional income fell below this level, hence they were eligible to apply for the AIO supplement. It is shown, though, that less than half of those eligible actually applied for the AIO. Beyond the poverty issues that this might have created to thousands of households, it also reinforces the scenario that individuals are largely unaware of the supplements that they are eligible for. Hence, the pre-1950 cohorts were, possibly, not aware of the supplement they would automatically receive.

Another form of lack of information could have been due to the fact that the Dutch

public believes, in general, that the pension system is rather adequate. In that sense, they might feel unmotivated to act, even if they are fully aware of the AOW supplement abolishment. Similarly, they might not have the skills or not being used to acting towards their retirement plan. Even if they did, though, there is limited additional retirement savings products and tax incentives.

Finally, it is pointed out that the post-1949 cohorts were subjected to the pillar two reform as well. This reform was largely present in the public sphere and must have occupied large part of the planning of those nearing retirement. We have already seen from our analysis that individuals were essentially forced to work an extra 3-5 years. Hence, it is highly likely that the AOW abolishment was overshadowed in terms of the attention payed by the households in planning their retirement years. Moreover, as they have worked more years, they have, on average, a higher wealth stock as well. That could lead them feel comfortable enough to not consider any effects due to the AOW supplement abolishment.

All these forms of lack of information are expected, though, to be experienced by part and not the whole of the population. This is supported by the government surveys that show that even 6 years before the policy change 20% of the population was aware of the supplement abolishment. Therefore, information lack would dilute any possible effects of those that were informed, but not completely diminish them. Moreover, time inconsistency is able to explain only the lack of reaction in the savings channel. Hence, the zero wealth effect on all three channels points to the existence of mental accounting in the households' planning. That would, as found in our analysis, result to no behaviour difference between the households that received the supplement and those that did not.

The robustness of our results has been discussed already in Chapter 6. We have used various cohorts as control groups and used various control and treatment group specifications, all indicating the same results. It was shown that the significant negative results in the income of the partner and, as a result, in the household wealth, were not due to the AOW supplement. These were interpreted as a response of older partners of individuals in the post-1950 cohorts, due to the pillar two reform.

We have, now, discussed various extensions of this work. We conclude that in order to complete our picture of the households' response we will need data from future years, in order to eliminate the spurious negative effects we observed, as well as to investigate the response of the households after they are actually affected by the supplement abolishment. At that point, they will be forced to respond, as they will be faced with a negative income shock and not only its expectation.

As a final point, we note the implications for policy work. The main focus of the letters sent to the individuals affected by the supplement abolishment was to incentivise their younger partners to increase their labour supply. The large announcement period was intended to give enough time to the households to become informed and reshape their expectations. It is clear from our results that households did not respond to the announcement. More specifically, the younger partners did not increase their labour supply neither at announcement nor after they were informed by the letter in 2010. The latter is not surprising, given that many of the partners had not worked their whole life an by that time they were reaching the age of 60. Hence, it can be concluded that the long announcement period of the abolishment did not play any role. Either due to miscommunication with the households, or due to behavioural effects that shrink the horizons over which they view their finances, the announcement period had no effect. Given, therefore, that it played no role in helping households maximise their utility by altering their path of consumption, no benefits to those affected are observed. On the other hand, the costs are obvious in terms of government finances. This work, therefore, indicates that this long announcement period, in this case, was unnecessary.

Whether the pre-1950 cohorts were unaware to the supplement or the post-1949 unaware or did not respond to it's abolishment is irrelevant in our analysis, but it is largely relevant in terms of policy. The first could lead to inefficient allocation of resources (as households save more than they need to for retirement) and the second to households in poverty. The latter is aggravated by the fact that many of those households that dropped below the subsistence level did not apply for the AIO supplement. It is unclear, at this point, whether post-SPA data would help differentiate the two scenarions. A survey on the knowledge of individuals on the existence of the supplement could be useful. It is not clear, though, how precise it could be, given that individuals would have to respond on what they were aware of, years before. In the next chapter we conclude our analysis.

Chapter 8

Conclusions

This work investigates the effect of an announced pension cut on households' consumption and labour supply. More specifically, we probe their response to the abolishment of the Dutch state pension partner supplement. This was given to individuals that when reaching state pension age had a low-income younger partner. It was abolished in 2015, with the announcement of the abolishment 20 years earlier. The abolishment resulted in a negative shock in the households' future income. Given our dataset, we explore the reaction of the households before they are hit by the income shock, i.e. how they respond in anticipation to the abolishment.

Standard Life-Cycle models predict that given an expected negative shock in their income households would react accordingly to smoothen consumption. I.e. that they alter their consumption and labour supply path. We investigate three possible response channels, a delay in retirement of the older partner, an increase in the labour supply of the younger partner and an increase in the households' savings rate. We employ a differences-in-differences-in-differences regression, using cohorts retiring before 2015 as controls and age difference between the partners as the treatment variable. This allows us to investigate the effect per month of age difference of the partners, hence per euro of total future income loss. We find no significant results in all channels. A spurious negative effect in the income of the partner and the household wealth is attributed to complex responses due to the pillar two reforms. We, therefore, conclude that households did not respond to the abolishment before being affected by the shock.

The setup of this analysis is unique in a number of ways. First, we use microdata based on tax returns, which allows us to compute the effect with large accuracy. Second, the total wealth shock considered is sizeable, equal to around €70,000 for a couple with the average age difference and a young partner with very low or no income. This shock is

dependent on the age difference pf the partners and is targeted to a specific group of the population - namely individuals with low-income younger partners. Third, as beyond the income requirement for the younger partner, the supplement had no other restrictions, the sample considered is rather diverse, as it includes households with various income and wealth levels. Fourth, an active campaign to inform the public about the policy change took place and, fifth, a long announcement period would allow for households to alter their behaviour early enough. Sixth, we investigate various channels of response, allowing us to reach a complete picture of the households' reaction.

The lack of a savings response can be attributed to time-inconsistency in the decisions of the households. Moreover, any wealth effect of the abolishment on all three channels would be diluted by the lack of information in part of the population. The complete lack of response in all channels, though, is attributed to mental accounting. Under this framework households are considered to have a zero marginal propensity to consume future income. Given that the abolishment was a large shock in the future income of households with no effect on their consumption and labour supply, our result constitutes a positive test to the mental accounting hypothesis.

In terms of policy, our results deem the long announcement period unnecessary. Households did not take advantage of it in order to maximise their utility (hence no benefit from the long announcement period), but there were costs in terms of continued government expenditure. That has implications to other policy changes that alter the future income of households. Our analysis suggests that individuals will not alter their behaviour during the announcement period, hence policy makers would be able to can them short without any negative effect on the utility of the affected households.

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