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Preface

In this dissertation, I explore questions in labor economics related to aging and migration. I investigate the effects of public policies on aging populations and of migration decisions, and how they can impact multiple generations. I use empirical, reduced-form methods, combined with administrative and survey data sources from different countries. The dissertation consists of three self-contained chapters.

Chapter 1 is titled *Should I Care or Should I Work? Multigenerational Effects of Long-Term Care*, and is coauthored with Katja Kaufmann. In this chapter, we analyze the spillover effects of long-term care arrangements (LTC) within families in the Netherlands across three generations. We exploit a quasi-experimental setting provided by a 2015 reform to public LTC. This tightened the access requirements for residential care and incentivized aging-in-place, delaying elders' entry into nursing homes. Under the new legislation, only elders in need of full-time supervision are granted residential care services, while those with milder conditions are assigned to home care services. Because virtually all LTC is publicly funded, and co-payments are relatively low, a restriction to access publicly financed residential care implies an actual restriction to residential LTC use. Using administrative data on the full Dutch population, we compare families with elders who lost access with those who remained eligible post-reform, in a Difference-in-Differences design. We look at the short- to long-run effects on labor supply and savings of younger generations.

We find that the reform led to a decrease in the likelihood of moving into a nursing home, and increased the likelihood of using home care services. We estimate a corresponding increase in children's labor supply, and a negative effect on their savings. On the one hand, children increase monthly work hours on average by 6.1 hours/month, in the three years following their parents' rejection to residential LTC. This corresponds to a 5.2% increase compared to the pre-treatment mean. On the other hand, they decrease their savings, by 8,549€ three years after the rejection (a 5.9% decrease). We further explore spillover effects of the reform by looking at the reaction of adult grandchildren of those in need of care. We do not find average effects on their labor supply.

We explore differential responses by gender, age, family composition, opportunity cost, socioeconomic status, and location. This allows us to identify important determinants of family's behavior and individual preferences. Elders' pre-existing financial resources, children' opportunity costs, and gender play a key role in shaping children responses. Elders resources, family size, household composition, and gender shape the involvement of the youngest generation. Other characteristics, such us geographical location, children' household composition, and elders' age and living arrangements do not seem to play as important of a role.

While public expenditure for LTC reduced in the years following the implementation of the reform, our analysis of the effects on younger generations suggest that losing eligibility caused a tightening of the family's financial constraints. Publicly provided financial support for home care services, provided as an alternative to residential care, does not sufficiently compensate families for the loss of nursing home placement. Furthermore, heterogeneity analysis indicates some groups are more affected than others by the more stringent financial constraint, which could have important implications for inequality.

Chapter 2 is titled *Live Longer and Healthier: Impact of Pension Income for Low-Income Retirees*, and is coauthored with Han Ye. In this chapter, we investigate how a permanent increase in pension income affects the mortality and health outcomes of low-income pensioners. We exploit two eligibility criteria of a 1992 German pension subsidy program to estimate the causal impact of additional pension income. Because Germany has universal health care, the implications of pension income is not tied to access to (subsidized) health care.

Our analysis is based on novel administrative data covering the universe of German pensioners who died between 1994 and 2018. Using a difference-in-differences method, we find that eligibility for the pension subsidy increases pension income by 57.9€/month, and improves age at death (censored at age 75) by 1.6 months (around a 0.2% increase). We find no significant effects on the age at claiming pension, on average. To better understand the mechanisms, we examine the responses in health outcomes using survey data. We find that the additional pension income improves both mental and physical health. In addition, individuals feel less financially constrained and are more optimistic about their future. Despite the fact that men receive a smaller subsidy on average, the heterogeneity analysis suggest that both the mortality and health responses are driven by men. Instead, we estimate women anticipate retirement by about 2 months.

The policy implication of our findings is that the pension subsidy for low-income workers in Germany have beneficial effects on life expectancy and health. The cost-benefit analysis suggests that this program is a cost-effective policy to increase the life expectancy of pensioners. A simple back-of-the-envelope calculation suggests that a subsidy, targeted at people with low pension entitlements, would help to flatten the income-mortality gradient and reduce the gap in life expectancy at age 65 between the top and bottom income deciles in Germany by 3%.

Chapter 3 is titled *Intergenerational Returns to Migration: Evidence from Italian Migrants Worldwide*, and is coauthored with Guido Neidhöfer. In this Chapter, we estimate the causal impact of parental migration and choice of destination country on their children's future outcomes, and analyze whether the parents' initial migration decision was influenced by the expectations of these long-term returns.

We use unique administrative data on the universe of Italians living abroad and cross-country survey data to compare the educational and labor market outcomes of second-generation Italian immigrants worldwide to their peers living in Italy. Since self-selection of (first generation) migrants is a known identification threat in the study their performance, selection in the parents' generation might affect their children' performance via parental investments and intergenerational transmission of human capital. Our dataset is unique in that it provides information on several background characteristics, including parents' education, age, Italian place of origin and exact destination in the host country. We use these information to abstract from self-selection on observable characteristics. Then, as migrants might be also selected on unobservable characteristics, we estimate a multinomial selection bias correction model, to take also this dimension of selection into account. This identification strategy allows us to abstract not only from selection into the migration decision, but also into the choice of destination country. In the estimation, we rely on the exogenous variation of two proxies for push and pull factors influencing migration choices.

Our findings show that the intergenerational returns to migration of Italian migrants are strongly heterogeneous by destination country, gender, and parental socioeconomic background. Returns in terms of educational attainment are not always positive, reflecting differences in education systems and incentives in different countries. However, on average the children of Italian migrants in most destination countries are outperforming their peers in Italy in terms of employment status and predicted income. Gender and parental socioeconomic background play an important role as well in shaping the children' performance.

Finally, applying the framework of a random utility model, we test whether the expectation of better future opportunities for children influenced their parents' initial migration choice. We find that parents who migrated after having their first child tend to prioritize their children's future opportunities over their own income gains when choosing a destination country.

Chapter 1

Should I Care or Should I Work? Multigenerational Effects of Long-Term Care

Joint with Katja Kaufmann

1.1 Introduction

Worldwide, populations are aging rapidly, with critical implications for pension and long-term care (LTC) systems. As of 2024, 1 in 10 people globally are above age 65. By 2050, the proportion of the world's population over 65 will increase up to 16% (United Nations, 2021). In the Netherlands, the country under study in this paper, the share of the population over age 65 has grown from 12% in 1990 to 22% in 2020, and is expected to be at 25% by 2050 (Bakx, Doorslaer, and Wouterse, 2023). This increase is accompanied by a corresponding increase of LTC demand: in 2020, 24% of the 65+ and 72% of the 85+ Dutch population used some form of LTC. Conversely, 63% of informal caregivers were below age 60, and 14% below age 40. As the elderly population share and demand for LTC increase, analyzing how younger generations are affected is of key interest.[1]

The previous literature has predominately focused on how care arrangements affect recipients (see, e.g. Barnay and Juin, 2016; Bakx et al., 2018) or informal

[1] The importance of this topic is mirrored in a surge of papers on this topic, see, e.g., a recent list of NBER working papers surveying LTC systems around the world (see, e.g., Bakx, Doorslaer, and Wouterse (2023) for the Netherlands, Banks, McCauley, and French (2023) for England, Brugiavini, Carrino, and Pasini (2023) for Italy, Costa-i-Font et al. (2023) on Spain, Geyer et al. (2023) for Germany, Gørtz, Christensen, and Gupta (2023) for Denmark, Gruber and McGarry (2023) for the United States, Fu, Iizuka, and Noguchi (2023) for Japan, and Gruber, McGarry, and Hanzel (2024) for a survey on LTC worldwide).

caregivers more generally (see, e.g., Lilly, Laporte, and Coyte, 2010; Schmitz and Westphal, 2015, 2017; Zhu, Jin, and Lee, 2022), but less is known about the specific impact on spouses and adult children (notable exceptions are Hiedemann, Sovinsky, and Stern (2018), Bergeot and Soest (2021), Chen and Lin (2022) and Massner and Wikström (2023)), and even less is known about spillover effects beyond the second generation. This paper aims to fill this gap by looking at how LTC arrangements affect up to three generations of the extended family. We exploit a quasi-experimental setting provided by a 2015 Dutch reform to public LTC, tightening the access requirements for residential care and incentivizing aging-in-place. We look at the short-, medium-, and long-run effects on a broad range of outcomes and for different subgroups.

To limit public spending on nursing home (or residential) care, in 2015 the Dutch LTC system underwent a major reform, aimed at saving costs and keeping people self-sufficient for as long as possible. Under the new legislation, applicants for residential care undergo an assessment procedure, with new and stricter standards. Only individuals in need of full-time supervision are admitted to nursing homes, while those with milder conditions are assigned to home care services. Elders admitted to nursing home facilities (i.e., residential care) before the 2015 reform are entitled to continued benefits from residential care services, even when they do not meet the new, stricter requirements (Ginneken and Groenewegen, 2015; Maarse and Jeurissen, 2016).

Notably, virtually all LTC is publicly funded, and co-payments are relatively low (Bakx, Schut, and Wouterse, 2020). In 2023, out-of-pocket expenditures for LTC account for only 6.3% of total LTC financing, including residential care co-payments, home care out-of-pocket expenses, as well as expenditures from voluntary insurance schemes (CBS, Health expenditure; functions and financing).[2] Therefore, the Netherlands constitutes a particularly interesting setting, in which a restriction to access to publicly financed residential care corresponds to an actual restriction to the possibility of moving into a nursing home.

Using administrative data on the universe of the Dutch population, we investigate the spillover effects of this reform on the labor supply and financial outcomes of

[2] Co-payments are a function of household income and a portion of wealth, which ensures their affordability for the care recipient. Co-payments for residential care are computed based on the sum of household earnings and 8% of wealth, defined as any financial assets and real estate, excluding the net value of the own house. They rely on a income exemption and maximum threshold, and additionally depend on the type and intensity of care received. Co-payments for home care, instead, are computed based on a fixed hourly rate, and must lie within a set minimum and maximum threshold. These thresholds depend on income, wealth, age, and household composition. In 2016, the median co-payment was, respectively, 33% and 2% of annual income for permanent nursing home residents and home care recipients (Bakx et al., 2020).

the children and grandchildren of the affected elderly, in a Difference-in-Differences (DiD) setting. We focus on first-time applicants to public LTC services between 2014 and 2015, above age 65 in 2014. Then, we identify two groups: medium care needs (MCN) and high care needs (HCN) applicants. MCN applicants were entitled to nursing home care prior to the reform, but are no longer eligible post-reform. HCN applicants are unaffected by the reform, because they are in relatively worse health and hence are always eligible for residential care.

We compare changes in labor supply and savings of children and grandchildren of MCN applicants to HCN applicants' descendants. Our identification strategy relies on the assumption that, in absence of the reform, the labor supply and savings of descendants of MCN applicants would have changed in a similar way as those of HCN applicants. Both MCN and HCN applicants would have been entitled to enter nursing homes, and the elder-care burden would not have fallen on the affected younger generations. We test for this parallel trend assumption and provide evidence for absence of pre-trends in care take-up, labor supply, and savings measures. Then, we explore short-, medium- and long-run spillover effects onto younger generations, from three years before to three years after their affected family member's first application date. We estimate labor supply effects both in terms of changes with respect to their values one year before the application date. The additional individual level difference helps to further abstract from pre-determined level differences, and focuses the analysis on the changes induced by the treatment.

Our DiD estimates show that the reform led to a decrease in the likelihood of moving into a nursing home of 24.0 percentage points, and increased the likelihood of using home care services by 68.8 percentage points. Our analysis of the effects on younger generations suggest that losing eligibility caused a tightening of the family's financial constraints. Publicly provided financial support for home care services, provided as an alternative to residential care (i.e. nursing home access), does not sufficiently compensate families for the loss of nursing home placement. We find an increase in children's labor supply, and a negative effect on their savings in the medium run. Starting from one year after the application, children experience an average 5.2% increase in monthly work hours, compared to the pre-treatment mean one year before the application. Three years after the application, children's wealth decreased by 5.9%. Our results indicate that, on average, children adjust labor supply and exploit their savings to face the increased financial burden.

We explore differential responses by gender, age, family composition, opportunity cost, socioeconomic status, and location. This allows us to identify important determinants of family's behavior and individual preferences. We find that elders' pre-existing financial resources and children' opportunity costs, combined with children' gender play a key role in shaping responses.

We find families reaction varies, depending on the income and wealth of elders. In wealthier families, sons' wealth decreases, by 4.1% three years after the application, while labor supply remains unchanged. Instead, in less wealthy families, sons increase labor supply, working, on average, an additional 9.5 hours/month (a 7.7% increase).

If there is a trade-off between informal care provision and privately hired professional care at home, the hourly wage constitutes the best proxy for the opportunity cost of informal care. Using the cutoff proposed by Bakx, Doorslaer, and Wouterse (2023) for the Netherlands, we investigate differential responses by looking at differences in pre-determined hourly wages. We find opportunity cost to be the key determinant of daughters' labor supply adjustments, both at the extensive and intensive margin. On the one hand, daughters with higher hourly wage increase labor supply. Monthly work hours increase by 8.1 hour/month (10.6%) in the two years following the application date. On the other hand, daughters with lower hourly wage decrease labor supply in the short-run, by 4.8 hours/month (6.3%), an effect that fades out in the medium-run. We estimate similar effects on the number of months employed per year, and no impact on daughters' savings.

Other characteristics, such us geographical location, family size and composition (i.e. number of siblings, presence of cohabiting partners or young children), and elders' age does not seem to play as important of a role in determining children' responses.

We further explore spillover effects of the reform by looking at the reaction of adult grandchildren of those in need of care. At the time of application, they are 25 years old, on average. They might therefore be directly involved in the care of their grandparents, and hence adjust their labor supply accordingly. We find that elders' financial resources are key in determining responses also of their youngest male descendants. However, contrary to their fathers, grandsons from wealthier families decrease their work hours by 25.4 hours/month in the years following the application, a 26.0% decrease with respect to their pre-treatment average. Furthermore, family size and household composition play an important role in determining grandchildren' responses. In smaller families, with a more limited supply of potential informal caregivers, grandsons decrease their work hours by 29.7%. Granddaughters, on the other hand, decrease working hours if they do not live with their parents, by 17.2% one year after the application date.

A number of papers in the literature aims at identifying the relationship between unpaid caregiving and labor supply, assuming exogeneity conditional on observable controls, or by instrumenting the caregiving decision in a 2SLS strategy with, for example, parental age and health, number of siblings or sick people in the household, or age of close friends (see, e.g., Lilly, Laporte, and Coyte, 2010; Schmitz and Westphal, 2015, 2017; Zhu, Jin, and Lee, 2022). We contribute to the existing literature by making use of a reform of the LTC system, which increased the hurdles to institutional care and thereby shifted elders with MCN to an aging-in-place model. Other recent papers making use of reforms in the LTC sector to identify effects on adult children's labor supply are Massner and Wikström (2023), who exploit a reform in Sweden that decreased the fee for formal elderly care which led to reduced healthcare utilization among the elderly and increased labor supply of their children, and Chen and Lin (2022), who use a 2012 reform in Taiwan that allowed more international caregivers, thereby increasing children's labor supply. In comparison to these works, we provide novel evidence on the extended family's reaction to a *restriction* in residential care access, and highlight important asymmetries with respect to the expansion case.

Previous work has investigated the effects of caregiving on unpaid (female) caregivers more generally, without distinguishing between spouses, adult children, and other relatives or friends (see, e.g., Lilly, Laporte, and Coyte, 2010; Schmitz and Westphal, 2015, 2017; Zhu, Jin, and Lee, 2022), we study how the reform specifically affected adult children, their partners, and grandchildren. Also, we aim to add to the literature investigating not only short-run, but also medium- and longer-run effects on the family and potential caregivers, up to several years after the initial health shocks (notable exceptions investigating dynamic/longer-run effects are Schmitz and Westphal (2017), Hiedemann, Sovinsky, and Stern (2018), Bergeot and Soest (2021), and Zhu, Jin, and Lee (2022) and Maestas, Messel, and Truskinovsky (2024)). Moreover, rich administrative data allow us to investigate effects not only on the extended family network, but also in terms of a wide range of outcomes, ranging from labor supply, to financial and health outcomes, and to living arrangements. Lastly, the data allow us to investigate heterogeneities not only by type of caregiver (spouse, adult child, grandchild, etc.), but also by gender, opportunity costs, distance to the affected elderly family member, and socioeconomic status. This allows us to disentangle important determinants of family choices and individual behavior.

The remainder of this paper is organized as follows. Section 1.2 describes the 2015 reform and the Dutch institutional background. Section 1.3 provides an overview of the data and describes the main estimation sample. Section 1.4 discusses the empirical strategy. Section 1.5 presents and discusses the main results. Finally, Section 1.6 concludes.

1.2 Institutional Background

1.2.1 The Dutch LTC System before 2015

The Netherlands was the first country to implement a universal public LTC insurance scheme in 1968, the Exceptional Medical Expenses Act (Algemene Wert Bijzondere Ziektekosten, AWBZ). Initially, the AWBZ covered nursing home care and institutionalized care for the mentally handicapped, but it gradually expanded to include home health care (1980), and residential care for the elderly (1997) (Alders and Schut, 2019). The resulting public financing scheme for LTC mainly served the elderly population, with three quarters of patients above age 65, and was substantially more generous and comprehensive than most other European countries. Coverage included accommodation costs in nursing homes and home help for domestic activities. Contributions and co-payments were income-related, and relatively low in comparison to other OECD countries, amounting to around 10% of costs (Colombo et al., 2011). The remaining funding of the system came from social security contributions (60%) and taxes (30%).

Because of its relative generosity and the increased number of users, the LTC system became an increasingly important component of public healthcare expenditures. Between 2000 and 2014, the average annual growth rate of public expenditure on LTC was 4.3% in real terms (Alders and Schut, 2019). In 2014, the Netherlands were spending 3% of GDP on LTC, which corresponded to almost half of the total health care expenditures, and was the second-highest share among OECD countries (OECD, 2024).

In 2014, the AWBZ scheme covered most aspects of LTC, such as residential care, home nursing care, and social care. Eligibility for residential and home nursing care was evaluated by an independent Care Assessment Center (CIZ). The center decided on eligibility for care in an institution (for example, a nursing home) or at home, the amount of care that patients are entitled to, as well as the duration of care. The decision was based on the information provided in the application form and the information about previous use of LTC. The application assessor decides which information to verify and which information is missing. In either case, they might contact the patient, their household, family members, health insurers, and health care providers, mostly through phone calls. The final decision is based on health status, functional limitations, living conditions, social environment, psychiatric and social functioning, as well as on any other professional services and informal care they are already receiving (CIZ, 2013). After the assessment, patients can decide whether to receive in-kind care or a cash benefit (*personal budget*), equivalent to 75% of the cost of in-kind care.[3] Social care was instead under the Social Support Act (*Wet Maatschappelijke Ondersteuning*,WMO). The WMO is a tax-funded scheme, managed by municipalities, covering services of housekeeping, transport, meal, house adjustment (Maarse and Jeurissen, 2016).

1.2.2 The 2015 Reform

In January 2015, the LTC system underwent a major restructuring, aimed cutting public expenditure by keeping elders self-sufficient for as long as possible. On the one hand, the reform shifted the focus of the public system support from residential to decentralized home care. On the other hand, it incentivized individual and social responsibility (Maarse and Jeurissen, 2016). Three main legislative changes were implemented.

First, the introduction of the Long Term Care Act (*Wet Langdurige Zorg*, WLZ), to replace the AWBZ Act. The WLZ regulates residential care and home care for people in need of full-time supervision. Eligibility for residential care is still assessed by the CIZ, which also decides on the amount of care and its duration. Decision criteria now more heavily reliant on health status and the functional limitations of the applicant. Co-payments were set at 9.65% of care costs, up to a maximum of €3241 per year in 2015 (Ginneken and Groenewegen, 2015).[4] As before, elders eligible for residential care that prefer to stay at home can apply for a personal budget, and receive full-time care at home. People already in residential care, but who do not meet the new, stricter requirements, are allowed to keep their entitlements for WLZ care for the rest of their lives.

Second, the 2006 Health Insurance Act (*Zorgverzekeringswet*, ZVW) was reformed to make health insurers fully responsible for community nursing (e.g. administration of medicines, wound care, and injections) and body-related personal care (e.g., support washing, dressing, and shaving).

Third, the 2006 WMO Act was reformed to account for all other non-residential care. Municipalities receive a state budget to perform their new role,[5] and are ultimately responsible for the assessment and assignment of care services at home. Specifically, the WMO 2015 gives applicants a right to publicly funded support if they cannot run a household on their own or participate in social life. However, each municipality independently determines its own eligibility criteria. Municipali-

[3] In 2013, cash benefits accounted for 11% of total expenditure, after having grown by 20% annually since 2002 (Schut, Sorbe, and Høj, 2013)

[4] Thereby lowering the contribution rate compared to the previous AWBZ Act.

[5] However, domestic care funding in 2015 was cut by 30% compared to the corresponding amount spent under the AWBZ Act (Ginneken and Groenewegen, 2015)

ties have discretion as to the type and the extent of assistance to be delivered. Assessments are mostly carried out by employees of the municipality or by social district teams, and are aimed at first exploring the options for support from the elder's social network (Kroneman M, Groenewegen P, and Ginneken E, 2016).

The three legislative changes combined imply, on the one hand, that applicants with mild health conditions are no longer eligible for residential care. On the other hand, the provision of all non-residential care is moved either to health insurers or municipalities. In addition, large expenditure cuts were imposed on all parts of the new LTC system, resulting in a total government budget cut by 28.6%. More precisely, residential care expenses were cut by 12.0%, while home care expenses where cut by 82.2% (CBS, 2024a), saving about 0.1% of the Netherlands GDP in 2015.

Overall, the 2015 reform led to a substantial shift of clients from residential to home care, and an increased incentive for family members to be involved in elders' care. The shift of patients from residential to home care is best illustrated by the drop in applications to the CIZ after 2015, depicted in Figure 1.A.1. After 2015, the CIZ only assesses applicants to residential care. Elders with milder conditions are instead redirected toward out-patient services via the WMO, and/or to professional nursing at home via ZVW.

1.3 Data

The analysis is based on Dutch administrative data maintained by Statistics Netherlands (Centraal Bureu voor de Statistiek, CBS), covering the entire Dutch population and including information that allows us to follow families across generations and over time. Birth and marriage records allow us to link several generations and create extended family networks. Information on public LTC services provides detailed information on applications for LTC services, both those provided in a residential care facility and those to be delivered at home (see Appendix 1.B for more details on the specific datasets and variables used).

We focus on individuals applying to LTC services between 2014 and 2015, that is we restrict time of application symmetrically around the date of the reform. We restrict the sample to individuals born before 1949, so that they are at least 65 in 2014, because the focus of the paper is on LTC of the elderly. This ensures that LTC applicants are already out of the labor force, and excludes younger individuals in the LTC system due to poor health. Then, we link elders with their children. In each family, we refer to the first person in the eldest generation applying to LTC services between 2013 and 2016 as *first generation* or 1G, and to their children as *second* *generation* or 2G.[6] Because we are interested in the labor market outcomes of 2G, we exclude families in which the second generation is born after 1996, i.e. is above age 18 in 2014.[7] We refer to the so-constructed eldest generation sample as Sample 1G, and to the sample of their adult children as Sample 2G. Finally, we link the second generation with their children, the *third generation* or 3G. To explore effects on the labor supply of adult grandchildren, we also restrict the youngest generation to those over 18 in 2014.[8] We refer to the sample with this additional restriction as Sample 3G, and only use this in our estimates regarding the third generation. We keep Sample 2G and Sample 3G separate, to make sure our estimates for the first and second generation are not biased by fertility-related selection.

We link the second and third generations with their labor market information at the monthly level, and construct outcome measures from three years before to three years after the application date. We average each measure over the time periods of 12-months legnth, e.g., information three years before application are an average value between 24 and 13 months before the application. This allows for more precise estimates, and abstracts from seasonality and temporary periods of unemployment or reduced employment. Then, we link each generation with socioeconomic status (SES) and geographical measures, observed one year before the application date. This ensures the variables are pre-determined, and not affected by the reform. We link each generation to their location of residence, information on their household's composition, personal income, household income, and household wealth.

1.3.1 Definition of Care Needs

The 2015 reform imposed stricter eligibility criteria for accessing publicly financed nursing homes. Before the reform, individuals with relatively milder health conditions were granted access. After, only those in need of full-time supervision were eligible to move into a residential care facility. To best identify the group of applicants affected by the reform, we split applicants by level of required care into three groups: *low care needs* (LCN), *medium care needs* (MCN), and *high care needs* (HCN).

[6] In the data, we exclude individuals with more than two legal parents or with no parent alive in 2014. This creates three cases when identifying which member of the eldest generation is 1G: (i) both parents are alive in 2014, and neither previously applied for LTC; (ii) both parents are alive, and one of them applied for LTC prior to 2014; (iii) one parent is not alive in 2014. In the first case, 1G is the elder applying first for LTC. In the second case, 1G is the elder applying after 2014. In the third case, 1G is the elder alive in 2014. If neither parent applies for LTC between 2014 and 2015, we exclude the family from the analysis.

[7] This drops about 0.01% of the sample.

[8] This restriction drops about 30% of the Sample 3G.

LCN applicants were never eligible for nursing home care. MCN applicants were eligible before the reform, but not afterward. HCN applicants are those that remain eligible despite the stricter criteria. The definition of these three groups is challenging for two reasons. First, we do not have information on the exact evaluation score assigned to each applicant, but only on the care services the applicant eventually receives. However, because virtually all applicants receive some level of service, we can use the assigned services as a proxy for the relative level of care need of each applicant. Second, composition of care services changed with the reform, at home and in nursing homes. This makes it harder to compare applicants before and after the reform at a granular level, but does not preclude the definition of broader care needs categories. Because of the changes in care packages composition, we vary the assignment rule before and after 2015.

Our baseline definition of care needs proceeds as follows. Before 2014, we define any applicant assigned to home care services as LCN. Then, we use the description of the assigned care services to discern between MCN and HCN applicants. Care services are split by disability type, and their labels reflect the intensity of care they provide. We assign applicants to the MCN group if they are assigned to services labeled as less than intensive in each disability group. HCN applicants are, consequently, those assigned to intensive or very intensive services. After 2015, the description of care services at home is less clear on the intensity of assigned care services. We assign to the LCN group those applicants who, after 2015, are only receiving household help services under the WMO. Those receiving any other type of care at home, or those rejected from nursing home care, constitute the MCN group. Finally, anyone admitted to nursing homes after 2015 is assigned to the HCN group. Appendix 1.C includes the full list of care packages for each care needs group.

An alternative way of defining the three care needs group is to exploit information on pre-determined applicants' health status. Unfortunately, available information on the health status of the elderly are limited. Our data include information on medical specialist physical care. We use this information to predict the level of care needs for applicants before and after the reform. Specifically, we adopt two approaches in measuring health status. The first uses diagnosis category fixed effects, so as to compare 1G with health conditions within the same diagnosis specialization (e.g., cardiology, urology, geriatrics, etc.). The second approach constructs an estimate of the *Charsol comorbidity index* (Charlson et al., 2022), based on observed diagnosis by a medical specialist. This is a weighted measure of comorbid diseases, taking into account both their number and the seriousness, and is used in the medical literature to predict 10-year survival rate. Appendix 1.D lists the diagnosis categories used in the first approach, and the diagnosis-treatment combination categories used in the estimation of the Charsol index. Details of the prediction procedure are included in Section 1.4, while Section 1.5 shows robustness of results when care needs are predicted via health status.

Our treatment is the introduction of the 2015 WLZ Act, restricting access to nursing homes. That is, our treatment is *losing access to in-patient care*. Accordingly, LCN applicants are always treated, MCN applicants are the treated, and HCN applicants are never treated. Our baseline specification considers the MCN applicants as the treated group, and HCN applicants as the control. We exploit the discontinuity the reform created on the right side of the health distribution, by excluding those with challenging health conditions who not yet in need of full-time care. Because HCN individuals were always eligible for nursing home access, and because the reform did not change explicitly the provision of care within nursing home, we argue HCN constitute an appropriate control group. In our robustness checks, we also implement a research design that includes LCN in the control.

1.3.2 Descriptive Statistics

Table 1.A.1 shows 1G's descriptive statistics, one year before applying to LTC. We report mean and standard deviation of key variables for the baseline sample, and by care needs, according to our main definition. We observe that applicants with higher care needs (HCN) are slightly older (by less than two years) and are more often male. One year before application, they are less likely to live alone than medium care needs applicants (MCN). We do not find important differences in the number of children nor in the share or the likelihood of having daughters. Further, HCN elderly are living around 3.3km further away from their closest child than the MCN group. As for socioeconomic status (SES), we consider a measure combining both individual's income and wealth, expressed percentiles of the national level distribution in a given year. HCN applicants report to be wealthier (SES is 3.9 percentiles higher). Indeed, they have higher household income (by about 2,999€/year) and wealth (by 68,779 €), and are more likely to live in a family-owned house (by 0.108 percentage points) compared to MCN applicants. By construction, all HCN applicants, and MCN applicants before 2015 are admitted to nursing homes. About 78% and 86%, respectively, of the MCN and HCN group before 2015 actually move to a nursing home, while approximately 45.1% of the HCN group after 2015 does so. While only 9.2% and 4.9% of MCN and HCN applicants use home care before 2015, almost the full MCN sample and 41% of the HCN sample after 2015 receives home care. Conditional on not being admitted to a nursing home, MCN applicants are, on average, admitted 1.6 years after their first application. Further, we observe that the

MCN group is more likely than the HCN group to be alive two and three years after the application date.

Table 1.A.2 shows descriptive statistics for 2G and 3G one year before the application. The second generation (Panel A) mirrors the age patterns of their parents, with 2G MCN about one year younger than their HCN counterparts. Gender, and number of siblings are evenly split across care groups, while MCNs are approximately 1 and 2 percentage points more likely to have a partner, and to have children in the household, in comparison to the HCN group. Additionally, 2G HCN live about 2km further away from their parents on average than 2G MCN, but 48.3% of them live less than 5km away. This suggests the distribution of residence locations is similar across care groups, one year before application. We do not observe strong differences across the two groups in terms of household SES or income. However, 2G HCNs are somewhat wealthier than MCNs (by 41,523€). Labor supply varies mostly on the intensive margin, with higher care needs group relying on about 3 working hours hours less. Gross and hourly wages are similar across the two groups. The third generation (Panel B) mirrors, similarly to their parents, the age patterns of their grandparents, and shows an evenly split gender distribution. Notably, because of the additional sample restriction (i.e. 3G to be above 18 in 2014), 1G are mechanically older than in the baseline 1G sample. Differences between MCN and HCN groups remain similar. MCN 3Gs live somewhat closer to their grandparents (by about 3.8km) and are more likely to live with their parents. Labor supply is similar across the two groups at both the intensive and extensive margin. 3G HCN perceive slightly higher gross and hourly wages, respectively by 173€/month and 1.3€/hour. To summarize, differences between the two groups (MCN and HCN) are generally also small for 2G and 3G, particularly in terms of labor supply.

1.4 Empirical Strategy

Estimating the causal effect of aging-in-place on family members is challenging because of endogeneity in the choice between nursing home care and home care. Unobserved preferences, attitudes toward risk, and life style choices as transmitted across generations might impact both LTC arrangements for the elderly, and the labor supply of younger generations. This paper exploits a reform of the LTC sector to estimate the causal effect of aging-in-place on the labor market and savings evolution of younger generations. We study the average treatment effect of the reform using a Difference-in-Differences (DiD) design.

We use changes in eligibility rules for admission into nursing home care to obtain a DiD estimate. The reform implies that applicants with MCN are no longer eligible for publicly financed residential care. For each family in the sample, we focus on the first member of 1G applying for LTC services between 2014 and 2015, and define their level of care needs based on the outcome of their first application. In the baseline specification, the treatment group consists of MCN individuals with medium care needs. The control group consists of HCN individuals.[9] With the DiD design, we measure the change in the difference between treatment and control group before versus after January 2015. We thus compare (own and family) outcomes of MCN applicants as opposed to HCN applicants before the reform (January to December 2014) versus after the reform (January to December 2015).

As a first step, we explore the effect of the reform on the likelihood of moving into a nursing home or using care services at home. The goal is to verify that the reform with its stricter criteria introduced in 2015 had the intended effects on the take-up of residential care and home care. We estimate the following equation:

$$Y_{ift}^{1G} = \beta_0 + \kappa D_{if}^{1G} \times After_{if}^{1G} + \beta_1 D_{if}^{1G} + \beta_2 After_{if}^{1G} + \beta_3 P_{ift}^{1G} + \beta_4 S_{if\tau-1}^{1G} + \nu_{ift} \tag{1.1}$$

where Y_{ift}^{1G} is an outcome for individual i in the first generation (1G) and family f measured at time t. The treatment group indicator D_{if}^{1G} is defined as $D_{if}^{1G} = I(MCN_{if}^{1G})$, an indicator variable taking value one if 1G is MCN, and 0 if 1G is HCN. $After_{if}^{1G}$ is a dummy that takes value one, if 1G in f applied to LTC services for the first time between January 2015 and December 2015, and 0 if 1G's first application was between January 2014 and December 2014. κ is our main coefficient of interest, and estimates the average treatment effect of not being eligible for nursing home access. P_{ift}^{1G} contains pre-determined demographic characteristics at time of observation of the outcome: a female indicator, 5-year cohort group fixed effects, indicators for having one, two, or three or more adult children, an indicator for having at least one daughter, and a continuous control for the age of the youngest child and its square. $S_{if\tau-1}^{1G}$, with τ indicating the application date, contains information 1G's household composition, socioeconomic status, location, and health status measured one year before the application date ($\tau - 1$). In particular, $S_{if\tau-1}^{1G}$ includes: an indicator for living alone, an indicator for the closest child living less than 5.75km away (sample median), indicators for SES being within first, second, or higher than third quartile of the national SES distribution, an indicator for a member of the household owning the home i lives in[10], municipality of residence fixed effects, medical

[9] We exclude LCN 1Gs from the baseline sample.
[10] That is, this indicator takes value 1 if i themselves or a member of their household, such as their partner, owns their home.

expenses for specialist care, and Charsol Index at $\tau-1$. We measure these variables before application, as their evolution might be affected by treatment.

In a second step, we aim to identify the effect of the reform on 1G's direct descendants by estimating the following equation:

$$\Delta Y_{ift}^{jG} = \gamma_0 + \theta D_{if}^{1G} \times After_{if}^{1G} + \gamma_1 D_{if}^{1G} + \gamma_2 After_{if}^{1G} + \gamma_3 P_{ift} + \gamma_5 S_{if\tau-1} + \varepsilon_{ift} \quad \forall j \in \{2,3\} \tag{1.2}$$

where $\Delta Y_{ift}^{jG} = Y_{ift}^{jG} - Y_{if\tau-1}^{jG}$ is the difference between t and $\tau-1$ (i.e. one year before the application date) of an outcome for an individual i in the jth generation (jG) of family f. The additional difference allows us to abstract from initial individual level differences in outcomes, providing a clearer picture of how outcomes have differentially evolved following the deterioration in health status of the first generation. When looking at labor supply variables, we measure outcomes at t and $\tau-1$ as an average over the 12 months following t and $\tau-1$, respectively. This reduces noise in the labor market data and abstracts from temporary and short-term adjustments in labor supply. Income and wealth are, instead, observed at the yearly level. D_{if}^{1G} and $After_{if}^{1G}$ are defined as in Equation 1.1. θ is the main coefficient of interest and estimates the average treatment effect of the reform across generations. Similarly to Equation 1.1, P_{ift} includes demographic information, and $S_{if\tau-1}$ other characteristics measured one year before application. Because Equation 1.2 focuses on the descendant generations, we adapt the controls as follows. P_{ift} includes a 1G female indicator, 1G 5-years cohort group fixed effects, jG continuous measure of age and its square, jG female indicator, and indicators for jG size (one, two, or three or more members)[11]. $S_{if\tau-1}$ includes an indicator for 1G living alone, an indicator for jG living less than 5.75km away from 1G, 1G's municipality of residence fixed effects, an indicator for a member of 1G's household owning their home, 1G and jG's household SES quartiles fixed effects, jG's sector of occupation fixed effects, 1G's medical expenses for specialist care, and 1G's Charsol Index, all measured in $\tau-1$. For 2G, $S_{if\tau-1}$ additionally includes an indicator for 2G having a cohabiting partner, and an indicator for living with children in $\tau-1$. For 3G, we instead include an indicator for living with own parents, and its interaction with an indicator for having a cohabiting partner or living with children. Appendix 1.B provides additional details on the outcomes and the controls included in each regression.

[11] For 2G, this corresponds to the number of siblings born from 1G. For 3G, it is the number of cousins descending from 1G.

Furthermore, we look at outcomes for the whole family. We are not only interested in effects on individual members of 2G, but also at how each generation and the family reacts together to the elders' increased need for care. We estimate the following variation of 1.2:

$$\Delta Y_{ft} = \alpha_0 + \pi D_f^{1G} \times After_f^{1G} + \alpha_1 D_f^{1G} + \alpha_2 After_f^{1G} + \alpha_3 P_{ft}^{1G} + \alpha_5 S_{f\tau-1}^{1G} + \omega_{ft} \quad (1.3)$$

where ΔY_{ft} indicates the change in the outcome Y measured for all members of the entire family or a particular generation within the family, instead of at the individual level. For labor supply, this is -for example– the total number of work hours across all members of generation 2Gs, divided by the number of 2G members in each family. For savings, we instead use as outcomes the sum of wealth across all individuals (and their households) within one family, divided by the sum of all family members. This includes wealth of 1G, 2G, and their cohabiting partners.[12] Controls are the same as in Equation 1.3, and π estimates the treatment effect.

Estimating Equations 1.2 and 1.3 separately for each t allow us to flexibly estimate the treatment effect on jG at different times around the application date, rather than providing an average effect over the years after τ.[13] The estimation of separate equations per period t is particularly important in our analysis, as 1G's situation evolves rapidly over the years following the application date, and we expect family's behavior to adapt accordingly. By nature of the policy, entrance to nursing homes is not denied indefinitely, but only as long as 1G's health is good enough for them to remain in their own homes. Once the elders require full-time supervision because of a deterioration of their health, they will be admitted to residential care. Their descendants' reaction will, therefore, evolve accordingly. Indeed, we expect our treatment to have an effect only between the date of the rejection to residential care and the date of eventual admission, which is, on average, 1.6 years later. Thus, we want to distinguish between year 0, 1, 2, and 3, to make sure we fully capture the evolution of families' behavior following the rejection, and can distinguish its determinants.

1.4.1 Predicting Care Needs

An important concern of our identification is that, for $\hat{\theta}$ to be unbiased, the evaluation criteria to be considered MCN or HCN do not change with the reform. That is,

[12] Unfortunately, wealth information are only available at household and not individual level.
[13] As it would happen by estimating Equations 1.1-1.3 in a panel setting with individual and time fixed effects.

that the pre-application health status of MCN and HCN applicants is the same before and after the reform. This might not be the case if evaluators become less (or more) lenient after 2015, for instance because they receive guidelines to be more stringent in the application to the new evaluation criteria (or, on the contrary, if they decide to be more generous in their evaluations). We partially address this concern in Equation 1.2, by controlling for medical expenses and estimated Charsol Index based on received specialist care in the year preceding the application. However, the inclusion of these controls might not fully capture a change in evaluators' behavior. To address this concern, we predict, as a robustness check, treatment status using two alternative measures of health status at $\tau - 1$, based on received specialist care.

We implement a two-step procedure to predict care needs, based on pre-determined health status. We use information on specialist care diagnosis as an objective measure of health. The 2015 WLZ Act imposed that only those in need of full-time supervision are eligible to access nursing homes. Therefore, applicants admitted to nursing home care after 2015 should be in relatively worse health than those admitted before. As in our baseline specification, we assign individuals admitted to nursing homes after 2015 to the HCN group. First, focusing on the sample of first-time applicants in 2015, we estimate the following equation:

$$\begin{aligned} HCN^{1G}_{if\tau} \;=\; & \delta_0 + \delta_1 Fem^{1G}_{if} \times H^{1G}_{if\tau-1} + \delta_2 H^{1G}_{if\tau-1} + \delta_3 Fem^{1G}_{if} + \delta_4 S^{1G}_{if\tau-1} \\ & + c^{1G}_{if} + \xi_{if\tau} \\ & if\ \tau \;\in\; [Jan\ 2015,\ Dec\ 2015] \end{aligned} \tag{1.4}$$

where $HCN^{1G}_{if\tau}$ is an indicator for individual i of generation 1G in family f being in the HCN group at time of application τ, $H^{1G}_{if\tau-1}$ is a measure of health during the year prior to application, and Fem^{1G}_{if} is an indicator of 1G's sex. Our health measures are constructed based on medical specialists' diagnoses of physical and mental health. Because the incidence of different health conditions varies strongly by gender, we interact the chosen health measure with a gender dummy. $H^{1G}_{if\tau-1}$, pre-determined health, is either measured by the estimated Charsol Index or via diagnosis category fixed effects. $S^{1G}_{if\tau-1}$ includes the same variables as in Equation (1.1), and c^{1G}_{if} are 5-year birth cohort groups fixed effects (also included in our main equations).

Then, we use Equation (1.4) to predict HCN status in the *sample 1G* applying in 2014. We assign 1G applying before the reform to HCN, if 1G was admitted to residential care, and the predicted HCN value is above the median of its distribution in the predicted sample ($\widetilde{HCN}$). Applicants before the reform, admitted to residential care and not $\widetilde{HCN}$, are assigned to the MCN group ($\widetilde{MCN}$).The remainder of the applicants, in particular those not admitted to nursing home prior to the reform are the LCN group, as in our baseline specification.

In a second step, we focus on the sample of $\widetilde{MCN}$ and LCN applicants prior to the reform, and estimate the following:

$$\begin{aligned}\widetilde{MCN}^{1G}_{if\tau} = {} & \lambda_0 + \lambda_1 Fem^{1G}_{if} \times H^{1G}_{if\tau-1} + \lambda_2 H^{1G}_{if\tau-1} + \lambda_3 Fem^{1G}_{if} + \lambda_4 S^{1G}_{if\tau-1} \\ & + c^{1G}_{if} + \zeta_{if\tau} \\ & if\ \tau \in [Jan\ 2014,\ Dec\ 2014]\end{aligned} \tag{1.5}$$

where $\widetilde{MCN}^{1G}_{if\tau}$ is an indicator for being in the predicted MCN group, and other controls are as in Equation (1.4). Similar to the first step, we predict, via Equation 1.5, MCN status among applicants that are not in the HCN group in 2015 (i.e., not admitted to nursing homes). Then, applicants in 2015 are $\widetilde{MCN}$ if: (i) they are not admitted into nursing home care; (ii) their predicted MCN status is above the distribution's median.

We then consider those in the $\widetilde{MCN}_{if\tau}$ group to be the treatment group, and use the HCN (post-reform) or $\widetilde{HCN}_{if\tau}$ (pre-reform) group as controls. This approach has two main advantages. First, it ensures treatment and control group have similar health status before the application. Second, it addresses concerns related to evaluators who decide on eligibility for nursing homes access, and who may change their evaluation criteria with the reform. This is because we use the characteristics of HCN applicants after the reform to predict HCN status before the reform, conditional on admission to nursing homes. However, because we unfortunately only have access to information on medical specialist care, our measure of health status are limited to certain types of health conditions. As health care is firstly administered by general practitioners (GPs) in the Netherlands, measuring health status only through specialist care might exclude relevant elders from MCN or HCN groups. Because of these drawbacks, we consider Equation 1.2, with controls for health status as mentioned in Section 1.4 to be our main specification.

1.5 Results

In this section, we present DiD estimation results. We first show that the reform had a direct effect on LTC arrangements, by increasing the likelihood of using home care services and decreasing the likelihood of moving into a nursing home. Then, we show the effect of the reform on the labor supply and savings of 2G and 3G.

We further explore heterogeneity in effects to shed some light on the underlying mechanisms, and perform a number of robustness tests.

1.5.1 First Generation

1.5.1.1 First Stage: Effects on LTC arrangements

First, we establish the direct effect of the reform on LTC arrangements for the Sample 1G.

Table 1.1. First Stage (DiD)

	(1)	(2)	(3)	(4)	(5)	Mean (before)
Nursing home	-0.212***	-0.199***	-0.195***	-0.193***	-0.240***	0.78
	(0.034)	(0.035)	(0.035)	(0.035)	(0.058)	(0.011)
Home care	0.748***	0.755***	0.752***	0.752***	0.688***	0.092
	(0.018)	(0.018)	(0.018)	(0.018)	(0.035)	(0.008)
Obs.	7,237	7,237	7,237	7,237	7,237	481
Bio&Family		✓	✓	✓	✓	-
Geo			✓	✓	✓	-
SES				✓	✓	-
Health					✓	-

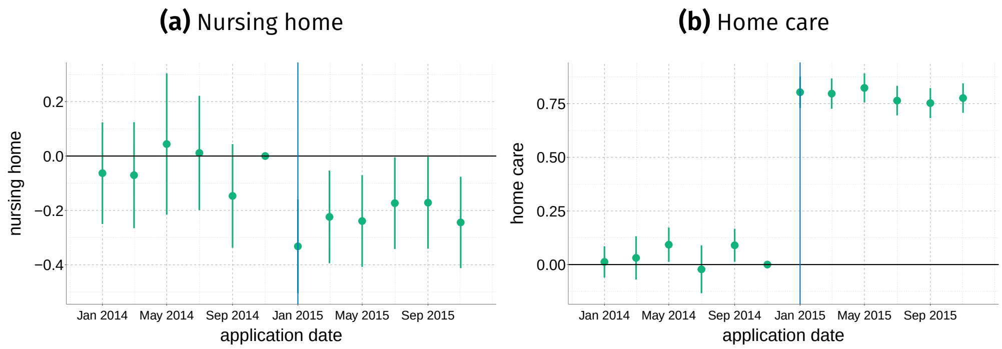
Notes: Effect on 1G's LTC arrangements. Treatment *HCN*: MCN are treated, HCN are control. Sample: 1G born in the Netherlands before 1949; applied for the first time to LTC services between 2014 and 2015; have children born before 1996. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Table 1.1 reports DiD estimates of the effect of requesting LTC services on the likelihood of moving into a nursing home or using home care for our main treatment definition (*HCN*). Column (1) reports estimates without any control variables, and Columns (2) to (5) progressively include controls. In Column (2) we include gender and 5-year grouped birth cohort fixed effects, as well as information on family and household composition (number of adult children, indicator for at least one daughter, age of the youngest child, and an indicator for living alone at $\tau-1$). Column (3) further controls for information about the location of residence, such as 1G's municipality fixed effects and an indicator variable for living less than 5.75km away (median distance) from the closest adult child at $\tau-1$. Column (4) adds 1G SES quartile fixed effects and an indicator for a member of 1G's household owning their home. Finally, Column (5) controls for the Charsol Index and medical expenses from specialist somatic care. The last column reports mean outcomes for the MCN applicants before 2015.

When including all controls, we estimate a decrease in the likelihood of moving into a nursing home of 24 percentage points (significant at the one percent level), and an increase in the likelihood of using home care services, of 68.8 percentage points (again significant at the one percent level). This effect is robust to the inclusion of demographic and socioeconomic controls. Table 1.A.3 reports DiD coefficients for alternative treatment definitions. We estimate comparable increases in home care usage across specifications, and decreases in nursing home care usage with Treatment $\widehat{MCN}_{He}$ (Panel B) and $\widehat{MCN}_{Ch}$ (Panel C). With Treatment $HCN + LCN$ (Panel A) we estimate a sharper decrease in nursing home care use, by 84.4 percentage points.

Figure 1.1 plots event study coefficients by application date in 2-months bins, relative to the last period before the reform (i.e., November-December 2014), of the likelihood of nursing home care (Panel (a)) and home care (Panel (b)) for our main treatment definition.

Figure 1.1. First Stage.



Notes: Figure 1.1 displays the estimated effect being in the MCN group on using nursing home (Panel a) or home care (Panel b) by application period (2-months bins). All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

We estimate insignificant or very small effects of our treatment on LTC use outcomes before January 2015. Afterwards, we estimate a sharp decrease in the likelihood of moving into a nursing homeand a corresponding sharp increase in the likelihood of using home care services. The change remains roughly constant across application periods. Point estimates are reported in Table 1.A.4, columns (1) and (2). Columns (3)-(8) of Table 1.A.4 report point estimates by application period for each alternative treatment definition. We estimate some significant pre-trends for treatment $HCN + LCN$, and a stronger increase (decrease) in the likelihood of using home (nursing home) care. Predicted treatments show, instead, a picture very close to our baseline specification. Overall, this lends empirical support to the validity of the parallel trends assumption in the uptake rate of home and nursing home care, for most of our treatment definitions.

Effects on longevity. Before looking at the second and third generation, we estimate the effect of losing access to residential care on survival of the first generation. We look at number of survival years after the date of the rejection, as well as likelihood of surviving one, two, or three years after the application date.[14] Figure 1.A.2 depicts main treatment effect on survival days and being alive three years later by period of application. Coefficients are not significant (at 95% confidence level) and mostly close to zero independently on the application period. Reduced form coefficients are reported in Table 1.A.5, for different outcomes and treatment definitions. Results on other survival measures with our main treatment definition are also either not or only marginally significant (at 10% level). Looking at alternative treatments, we estimate a positive significant effect on survival years, but no significant (or marginally significant) effect on probabilities of surviving one, two or three years after rejection. Our evidence indicates the reform did not significantly affect elders' survival.

1.5.2 Second Generation

In this section, we investigate the effects of the aging-in-place reform on the labor supply of the second generation. That is, the adult children of those in need of care. We estimate outcomes at individual level, through Equation 1.2, or at family level, through Equation 1.3. Labor supply outcomes at family level are a summarizing measure across all members of the second generation. Savings outcomes at family level summarize savings for both generations and, because of data limitations, include in-laws. All outcomes are measured as the difference between the value in the year they are observed and their value one year before application. This allows to observe their evolution over time and compared to a pre-care need period.

Table 1.2 reports the estimated DiD coefficients on the main outcomes of interest, measured one year following the application. Panel A focuses on outcomes at individual level, and Panel B on outcomes at family level. We measure labor supply as change in work hours per month, and savings as change in household wealth diminished by the value of the own home. Column (1) reports DiD estimates without

[14] To anybody surviving after the end of 2019, we impute survival years to be equal to the difference between January 2020 and the date of application. This might introduce a downward bias to the number of survival years, as some of the elders might live longer than the imputed values. Hence, we rely on likelihood of surviving one, two, or three years after the application date as robust outcomes for the estimation of the treatment effect on survival.

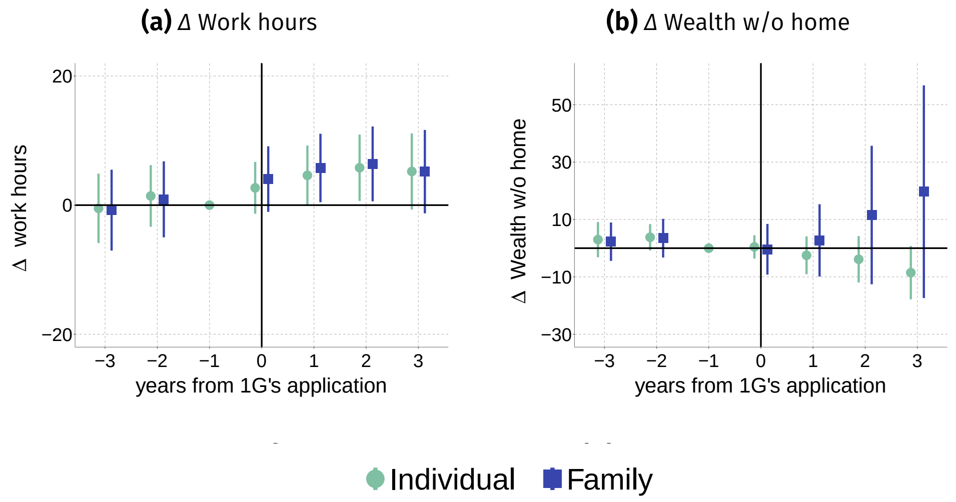
Table 1.2. Impact on 2G at $\tau + 1$ (DiD)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2G						
Δ Work hours	5.819**	4.726**	4.465*	4.287*	4.551*	4.613*
	(2.353)	(2.356)	(2.361)	(2.357)	(2.359)	(2.358)
Δ Wealth w/o home	-4.191	-3.090	-3.150	-2.562	-2.530	-2.489
	(3.381)	(3.372)	(3.381)	(3.361)	(3.361)	(3.358)
Panel B: Family						
Δ Work hours	7.036**	6.042**	5.858**	5.706**	-	5.755**
	(2.749)	(2.713)	(2.714)	(2.706)	-	(2.705)
Δ Wealth w/o home	1.149	1.913	2.716	2.716	-	2.724
	(6.419)	(6.458)	(6.385)	(6.385)	-	(6.424)
Obs. (2G)	15,911	15,911	15,911	15,911	15,911	15,911
Obs. (Family)	5,529	5,529	5,529	5,529	-	5,529
Bio&Family		✓	✓	✓	✓	✓
Geo			✓	✓	✓	✓
SES				✓	✓	✓
Occupation					✓	✓
Health						✓

Notes: Effect on 2G's labor supply and savings one in $\tau + 1$. Treatment *HCN*. Treatment *HCN*: MCN are treated, HCN are control. Outcomes are differences between their value in $\tau + 1$ and in $\tau - 1$. Monetary values are expressed in 2015 euro. Sample 2G: children of Sample 1G. Panel A: one member of 2G is one observation; controls as in Equation 1.2. Panel B: one family is one observation; controls as in Equation 1.3; labor supply per 2G; savings per family member. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

any control. Columns (2) through (6) progressively include control variables. Column (2) controls for gender and age, as well as household composition for both generations one year before application. Column (3) additionally includes information about the place of residence of 1G, as well as the geographical distance between the two generations, one year before application. Column (4) adds SES controls, as well as an indicator for 1G's owning their home one year before application. Column (5) adds 2-digit sector of occupation fixed effects (excluded for family level outcomes). Finally, Column (6) controls for 1G's Charsol Index and expenses from specialist care one year before application. The exact list of controls for Panel A and B specifications can be found in Section 1.4. Estimated results are robust to the addition of controls. At individual level (Panel A), we estimate a marginally significant (at 10% level) increase in work hours, by 4.6 hours/month, and no estimate significant effect on savings. At family level (Panel B), we estimate a significant (at 5% level) effect on work hours per 2G, by 5.8 hours/month. We do not detect any significant

Figure 1.2. Effect on 2G around application time (DiD).



Notes: Figure 1.2 displays estimated DiD coefficients on 2G labor supply and savings around the application date for our main treatment. Outcomes are changes in total work hours per month (Panel a) and in household wealth diminished by the own home value, expressed in thousands 2015 euro per year (Panel b). Green dots use the individual level 2G sample (one observation=one 2G), blue squares use the family level sample (one observation=one family). On the x-axis, time indicates when the outcome is observed and it is expressed with respect to the application date. All controls are included. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

effect on family wealth. Figure 1.A.3 depicts estimated effect on main outcomes one year after the application, by application date. We do not estimate significant pre-trends for any outcomes. When looking at the change in family's work hours, we estimate significant increases in the number of work hours per month (at 10% level) after January 2015. Other outcomes are not significantly changed by the treatment.

To explore how families' react at different times around the application date τ, we estimate treatment effect on change in outcomes from three years before to after τ. Results on main outcomes at individual and family level are depicted in Figure 1.2. This *pseudo-event studies* vary the time of observation of the outcome (t) on the left hand side of Equations 1.2 and 1.3, rather than expanding the indicator $After_f^{1G}$ into time of application indicators (as in Figure 1.A.3). This allows us to explore the evolution of our main outcomes around the time of the application and provide additional insights on how family react to the increased need for care of their elders. We estimate a significant increase in labor supply starting from the first year after the application date. Work hours increase significantly by 4.6, 5.8, and 5.2 hours/month (at 10%, 5%, and 10% significance level, respectively) from one to three years after the application date. Our point estimates indicate an average increase of 4.6 hours/month between τ and $\tau + 3$, or a 4.6% increase of the treatment group pre-treatment average at $\tau - 1$ (approximately 99.9 hours/month).

As for savings, we estimate a decrease in wealth net of home value in the third year after the application date, by 8,549€(significant at 10% level) or 5.9% compared to the pre-treatment average at $\tau - 1$. While decreases in the years before are not significant, point estimates are negative and decreasing from the first year after the application. Labor supply effects are similar at family level: work hours per 2G significantly increase by 5.8 and 6.4 hours/months in the first and second year after the application (significant at 5% level). We do not estimate any significant effect on family's savings. Point estimates suggest an increasing pattern, but are not significantly different from the individual level estimates.

Table 1.A.6 reports point estimates for individual-level outcomes, and includes some alternative measures of both labor supply and savings. In particular, we find similar effects on labor supply when looking at changes in number of months employed per year: we estimate a significant increase (at 5% level) in this variable measured at $\tau + 2$ and $\tau + 3$, by 0.5 months/per year. When looking at irregular work hours per year (the combination of overtime and hours from secondary jobs), estimates are positive but not significant. Furthermore, we estimate marginally significant decreases in labor income, measured either through earnings or main income (i.e. monetary amount received per year from the main source of personal income), by, respectively, 658€/year in τ and 1,378€/year in $\tau + 3$. For savings, we do not estimate significant effects in any alternative measure. Point estimates suggest that the decrease in wealth without home might be driven by decreases in more liquid wealth, such as bank or financial assets. Table 1.A.7 reports effects around τ at family level, for main and alternative measures of labor supply and savings. Results on alternative outcomes are similar to those on the two main variables, and follow the same pattern estimated at individual level.

Figure 1.A.4 depicts results on the main outcomes around the application date, using alternative treatment definitions. Overall, results on wealth both at individual and family level are robust across treatment definitions. Results on labor supply loose significance with other treatments.

1.5.2.1 Heterogeneity

To investigate whether such zero effects hide differences by demographic or socioeconomic groups, we split the sample along several theoretically important dimensions. Table 1.3 reports results for the main sample splits on 2G's labor supply and savings in the years following the application date. We focus on the individual-level Sample 2G, considering each 2G as a separate observation, and on two main outcomes: total number of work hours per month, and household wealth net of home

Table 1.3. Heterogeneous impact on 2G (DiD)

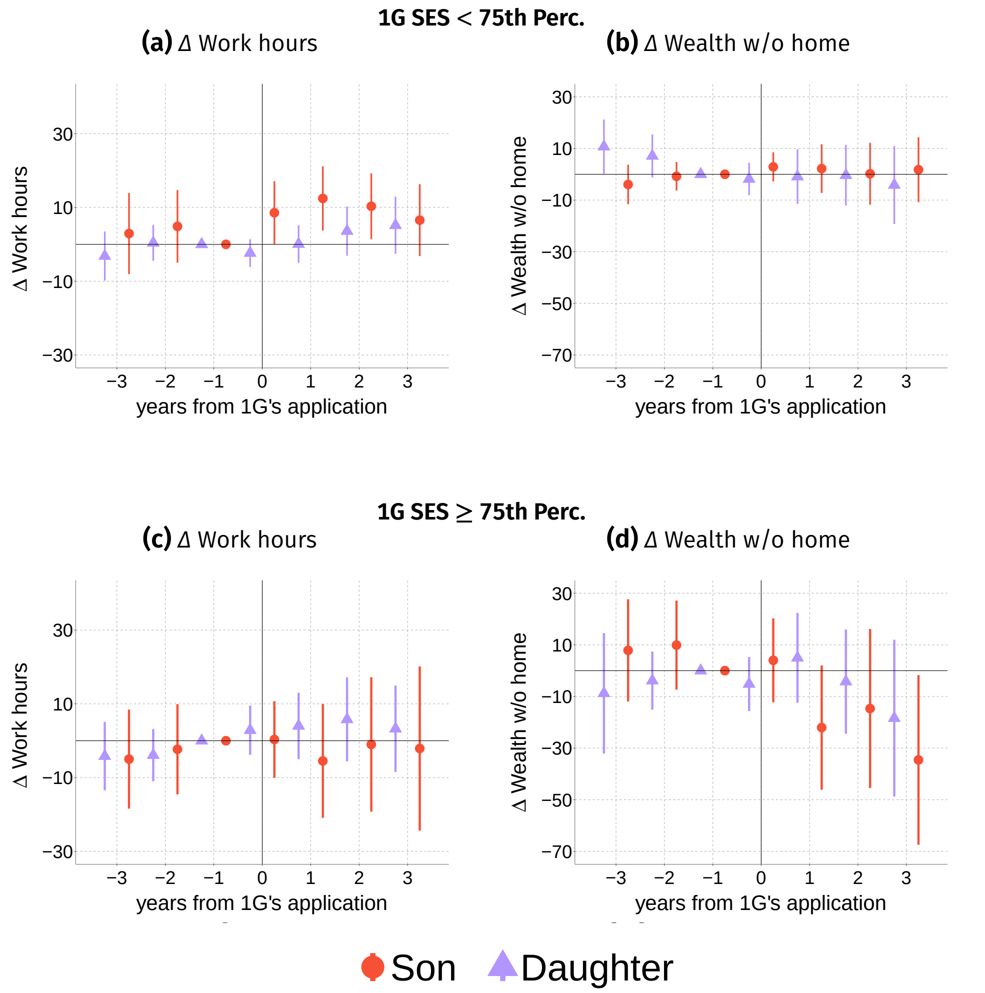
	Baseline		**1G SES<75th**				**Hourly Wage**			
			Yes		**No**		**Low**		**High**	
	D (1)	S (2)	D (3)	S (4)	D (5)	S (6)	D (7)	S (8)	D (9)	S (10)
Panel A: $\tau - 3$										
Δ Work hours	-3.676	1.086	-3.173	2.937	-4.174	-4.982	-3.875	9.371	-4.111	-4.734
	(2.821)	(4.447)	(3.394)	(5.613)	(4.717)	(6.845)	(3.633)	(8.617)	(3.585)	(3.994)
Δ Wealth w/o home	6.121	-0.312	10.696**	-3.967	-8.783	7.863	7.803	2.961	0.676	-3.027
	(4.964)	(3.992)	(5.352)	(3.907)	(11.890)	(10.087)	(5.192)	(4.275)	(11.423)	(6.378)
Panel B: $\tau - 2$										
Δ Work hours	-0.903	2.868	0.395	4.873	-3.905	-2.318	-1.160	13.071*	-1.781	-4.875
	(2.094)	(4.015)	(2.486)	(5.021)	(3.611)	(6.233)	(2.791)	(7.695)	(2.510)	(3.646)
Δ Wealth w/o home	4.788	2.337	7.105*	-0.791	-3.875	9.899	5.958	0.975	1.851	3.337
	(3.460)	(3.223)	(4.243)	(2.817)	(5.741)	(8.795)	(4.451)	(3.164)	(4.859)	(5.227)
Panel C: τ										
Δ Work hours	-1.452	5.489	-2.359	8.563*	2.848	0.343	-4.849**	8.398	6.969**	4.002
	(1.648)	(3.474)	(1.917)	(4.375)	(3.394)	(5.290)	(1.827)	(7.285)	(3.517)	(2.474)
Δ Wealth w/o home	-3.478	3.584	-1.827	2.866	-5.178	4.014	-3.233	5.169	-3.596	2.267
	(2.730)	(3.100)	(3.207)	(2.876)	(5.337)	(8.291)	(2.976)	(3.327)	(6.018)	(4.856)
Panel D: $\tau + 1$										
Δ Work hours	0.629	7.063*	0.072	12.445**	4.013	-5.479	-3.546	12.541*	9.276*	3.626
	(2.230)	(3.872)	(2.599)	(4.448)	(4.595)	(7.867)	(2.385)	(7.355)	(4.871)	(3.753)
Δ Wealth w/o home	-0.457	-3.976	-0.903	2.191	4.972	-22.037*	3.846	2.588	-10.542	-9.242
	(4.536)	(4.848)	(5.395)	(4.822)	(8.865)	(12.277)	(5.104)	(5.329)	(9.747)	(7.569)
Panel E: $\tau + 2$										
Δ Work hours	3.261	6.751	3.601	10.325**	5.787	-1.005	1.003	13.825*	6.671	1.969
	(2.920)	(4.200)	(3.402)	(4.553)	(5.802)	(9.294)	(3.269)	(7.134)	(6.008)	(5.028)
Δ Wealth w/o home	-3.120	-3.236	-0.380	0.198	-4.246	-14.671	0.026	-3.558	-10.170	-3.345
	(5.075)	(6.216)	(5.984)	(6.123)	(10.300)	(15.711)	(5.775)	(7.374)	(10.664)	(9.434)
Panel F: $\tau + 3$										
Δ Work hours	3.907	4.164	5.168	6.564	3.258	-2.104	2.582	19.480**	5.050	-6.463
	(3.275)	(4.824)	(3.950)	(4.973)	(5.970)	(11.354)	(3.705)	(7.198)	(6.425)	(6.355)
Δ Wealth w/o home	-9.074	-7.074	-4.167	1.780	-18.395	-34.567**	-7.921	0.670	-13.837	-11.768
	(6.831)	(6.538)	(7.696)	(6.415)	(15.488)	(16.747)	(7.787)	(7.254)	(14.040)	(10.268)
Obs.	7,833	8,101	6,075	6,261	1,758	1,840	5,397	4,105	2,436	3,996

Notes: Heterogenous treatment effect on 2G after the application date τ. Sample: one 2G is one observation. Treatment *HCN*. Outcomes are changes, compared to $\tau - 1$, in total work hours per month and household wealth net of home value in thousand euro per year. All controls are included. Columns (1)-(2) look at baseline estimates. Columns (3)-(6) by 1G's SES at $\tau - 1$. Columns (7)-(10) by 2G's hourly wage below 19.88€/hour at $\tau - 1$. Odd columns consider daughters (D), even columns sons (S). Monetary values are expressed in 2015 euro. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source:* Authors' calculations from the CBS data.

value. For each characteristic, we divide the sample in two or four sub-samples, and estimate Equation 1.2 with each one.

Gender. Previous work on spillover effects of LTC highlights gender differences in the propensity to provide informal care: daughters are often more likely than sons to care for their elderly parents. Following this stylized fact, we split the 2G sample by gender. Results around τ are reported in Columns (1)-(2) of Table 1.3. While point estimates are only marginally significant (at 10% level in $\tau + 1$) we observe an increasing pattern in sons' work hours between τ and $\tau + 2$. We do not estimate significant effects on daughters' labor supply, and differences by gender are significant in τ and $\tau + 1$. We do estimate negative but non-significant changes in savings for both daughters and sons.

Figure 1.3. Effect on 2G by gender and 1G's SES (DiD).



Notes: Figure 1.3 displays estimated DiD coefficients on 2G labor supply and savings around the application date by 1G's SES at $\tau - 1$. SES is measured as percentile of the income and wealth distribution, cutoff is the 75th percentile. Sample: one 2G is one observation. Outcomes are changes in total work hours per month (a and c) and in household wealth net home value, expressed in thousands euro per year (b and d). Monetary values are in 2015 euro. On the x-axis, time indicates when the outcome is observed with respect to the application date. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

1G's SES. Families might decide to firstly use 1G's economic means to compensate for the loss in access to nursing homes. We split our baseline sample by wether 1G's SES (i.e. income and wealth) is above or below the 75th percentile of its distribution at $\tau - 1$. Richest 1Gs are more likely to be able to fully carry the financial burden posted by their increased need for care, so that their children are less likely to be affected by the policy change. Figure 1.3 shows how 2G's labor supply and savings evolve, depending on these characteristics. Point estimated are also reported in Columns (3)-(6) of Table 1.3. We do not estimate significant effects for daughters. Sons' response margins depend, instead, on their parents' SES. In wealthier families, sons' wealth deteriorates starting from $\tau+1$. We estimate a negative effect by 34,567€ in $\tau+3$, a 4.1% decrease compared to the pre-treatment average in $\tau-1$. Instead, sons from lower SES increase their work hours by 8.6, 12.4, and 10.3 hours per month in τ, $\tau+1$, and $\tau+2$, respectively (significant at 10% and 5% level). This averages to an increase by 9.5 hours per month in the years after τ, or a 7.7% increase.

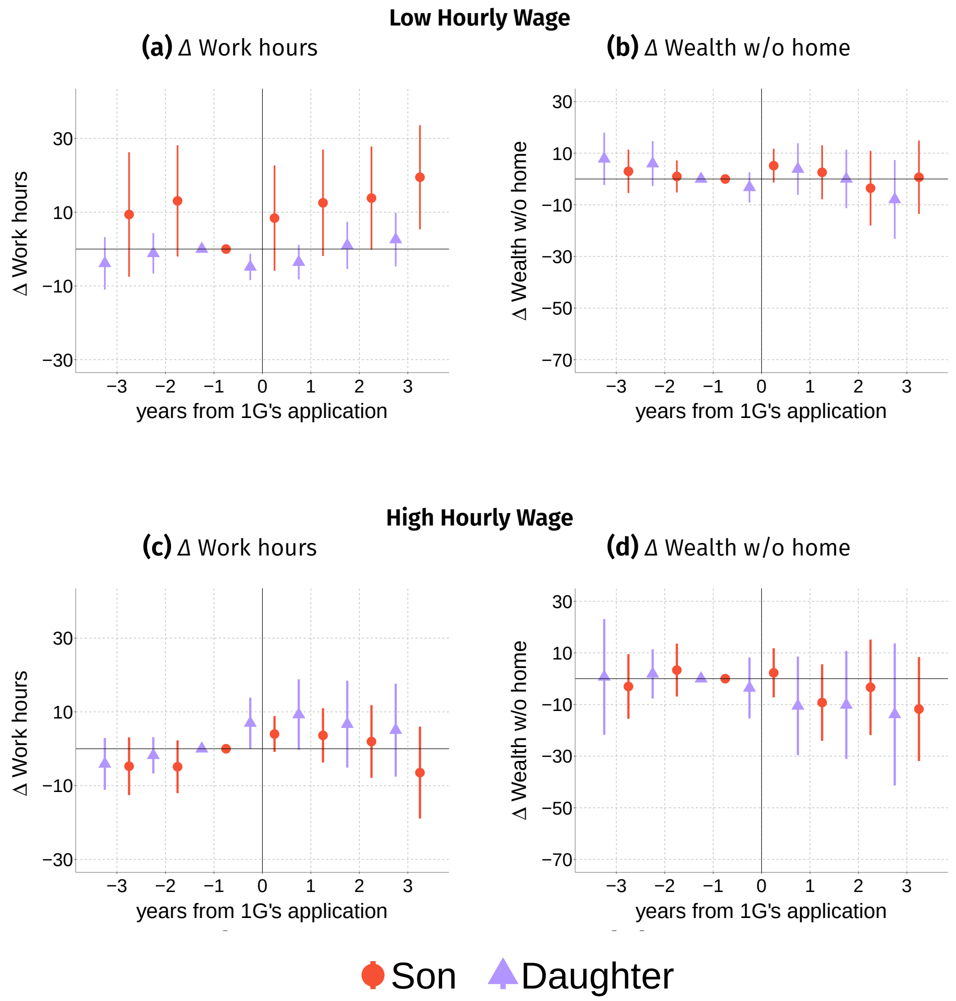
Opportunity Cost. Columns (7)-(10) of Table 1.3 and Figure 1.4 look at differences by hourly wage in $\tau-1$. The rationale behind this division is to use the hourly wage as a measure of the opportunity cost of providing informal care against hiring a professional caregiver. Following Bakx, Doorslaer, and Wouterse (2023), which provide estimates for this measure in the Netherlands, we use 19.88 (2015)€/hour as cutoff.[15] Results show daughters' adjust their labor supply in opposite ways if their are above or below such cutoff. On the one hand, daughters with lower hourly wage significantly decrease their work hours in τ, by 4.8 hours/month (significant at 5% level) or 6.3% compared to daughters' pre-treatment mean. The decrease is temporary, and limited to the months immediately following the application date. On the other hand, daughters with higher hourly wage significantly increase their work hours in τ and $\tau+1$, by 7.0 and 9.3 hours per month (or by 9.1% and 12.1%), respectively. We do not estimate any significant effect on daughters' wealth. Sons, on the other hand, do not change their labor supply above the cutoff, and increase work hours in $\tau+1$ and $\tau+2$ if they perceive a lower hourly wage, by 12.5 and 13.8 hours per month, respectively. We do not find significant effects on sons' wealth.

Other dimensions. We explore a number of additional characteristics of both 1G and 2G that might play a role in shaping families' responses to the elders' increased care need. Figure 1.A.5 summarizes them, by showing the estimated treatment effect across sample splits at on outcomes measured at $\tau+1$. Tables 1.A.8 and 1.A.9 report estimated effects around application time for daughters and sons, respectively.

First, we explore wether family size plays a role, particularly if size of 2G's peer group within the family (siblings and partners) is relevant. We do not estimate differential effects depending 2G having siblings or having a cohabiting partner. We additionally look at heterogeneity by 1G household's composition, and split by wether 1G lives alone at $\tau-1$. We do not estimate significant effects on 2Gs if their parents live alone. However, we estimate a significant increase in sons' labor supply, if their

[15] This converts the 20.86 (2019)€/month in Bakx, Doorslaer, and Wouterse (2023) to 2015 euros.

Figure 1.4. Effect on 2G by gender and hourly wage (DiD).



Notes: Figure 1.4 displays estimated DiD coefficients on 2G labor supply and savings around the application date by 2G's hourly wage at $\tau - 1$. Hourly wage cutoff is 19.88€/hour. Sample: one 2G is one observation. Outcomes are changes in total work hours per month (a and c) and in household wealth net home value, expressed in thousands euro per year (b and d). Monetary values are in 2015 euro. On the x-axis, time indicates when the outcome is observed with respect to the application date. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

parents do not live alone. This might suggest 1G's partner might be providing some hours of informal care, that allows 2G to work some more hours per month.

Furthermore, 2G's reaction might depend on their own household needs, if they have children, and elders' care needs might add or overlap with childcare ones. We split the 2G sample by gender and having children in their household. We do not estimate a significant treatment effect for 2Gs with children. Instead, we estimate a significant increase in sons' labor supply, by 15.9 hours/month in $\tau + 1$ (significant at 10%).

Previous literature highlighted that geographical distance is an important factor in the decision of who should be the main care provided, among siblings, for their elderly parents. We do not find distance playing a role for daughters. However, we estimate a significant (at 10% level) increase in son's work hours, if they live closer to the elder in need, by about 14 hours/month at $\tau + 1$.

Then, we investigate differences by 1G's age at time of application. We estimate treatment effects on the restricted sample 2G, whose 1Gs are above age 75. We do not estimate significantly different effects of this sample from the baseline.

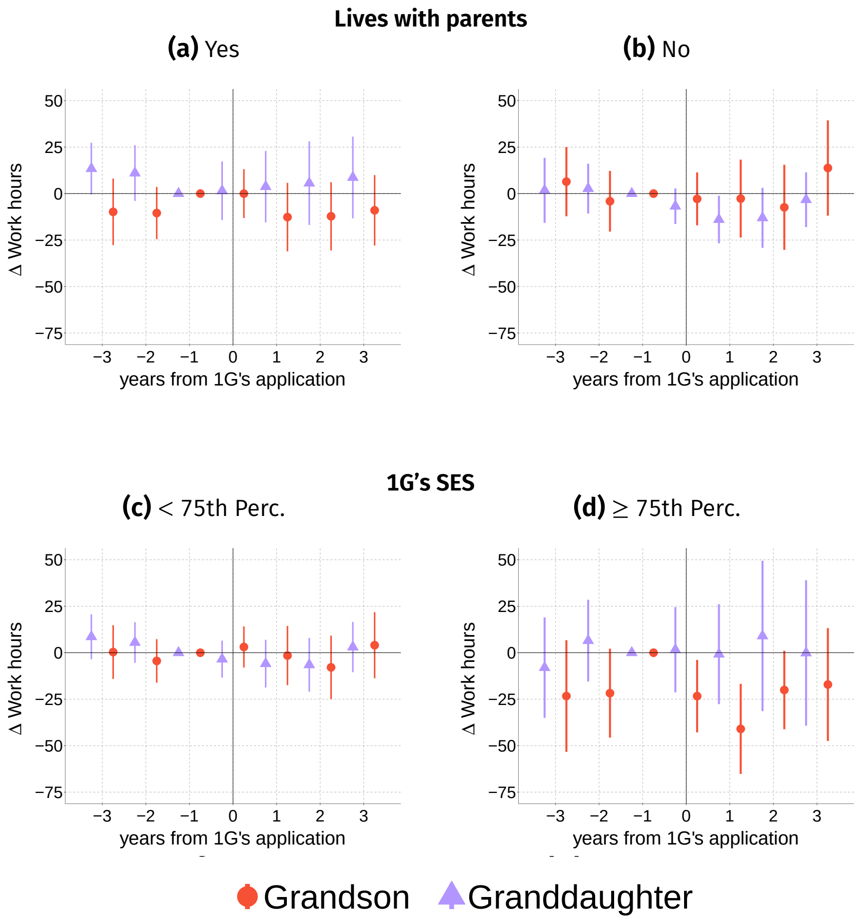
Family heterogeneity. Finally, we estimate heterogeneous effects on overall family's labor supply and wealth. We split the family-level sample depending on 1G's SES, 2G's siblings, and distance between 1G and the closest 2G and estimate Equation 1.3 on each subsample. Table 1.A.10 reports estimated coefficients on work hours per 2G and wealth net of home value per family members. Results are in line to those at individual level.

1.5.3 Third Generation: Effects on Labor supply

We further explore spillover effects of the reform by looking at the adult grandchildren of those in need of care. At the time of application, they are, on average, 24 years old. Therefore, they may also be directly involved in the care of their grandparents, and this might influence their labor supply. We focus on effects on total work hours per month and explore differences by gender, distance to their grandparents, dimension of their family, household composition, and socioeconomic status. In the following, we look only at the individual-level 3G sample, that considers each 3G as a separate observation, and estimate Equation 1.2.

We identify two important dimensions of heterogeneity for 3G: their household composition, and specifically if they do or do not live with their parents, and their grandparents' SES, used as proxy for their grandparents financial independence and their family's overall SES. Figure 1.5 depicts reduced form coefficients around the application date across these sample splits. Panels (a) and (b) distinguish the 3G sample by wether they live in the same household as their parents at $\tau - 1$, and by 3G's gender within each sub-sample. If they do not live with their parents, we estimate a significant decrease in granddaughters' working hours, by approximately 14 hours/month or 17.0% in $\tau + 1$, significant at 5% level. Additionally, we estimate a non-significant decrease of similar magnitude in $\tau + 2$. We do not estimate significant effects for grandsons. Differences by gender are, however, never significant. Panels (c) and (d) distinguish 3G by 1G's SES at $\tau - 1$. We estimate a significant decrease in work hours for grandsons in richer families, on average by

Figure 1.5. Effect on 3G by gender, household composition, and 1G's SES (DiD).



Notes: Figure 1.5 displays estimated DiD coefficients on 3G labor supply around the application date τ by 3G's gender and living with 2G or 1G's SES at $\tau - 1$. Sample: one 3G is one observation. Panels (a) and (b) split the sample by wether 3G lives with their parents (2G) at $\tau - 1$. Panels (c) and (d) by 1G's SES below or above the 75th percentile of its distribution at $\tau - 1$. All subfigures additionally split the sample by 3G's gender. Outcome is the change in total work hours per month. On the x-axis, time indicates when the outcome is observed with respect to τ. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

25.4 hours/month (or 26.0%) after τ We do not estimate significant changes for granddaughters. Furthermore, gender differences are significant, in this instance. Columns (2)-(3) and (8)-(9) of Table 1.A.11 reports point estimates for both analysis.

Table 1.A.11 reports estimated coefficients on 3G's labor supply across additional heterogeneity dimensions. Column (1) reports baseline estimates by 3G's gender, Columns (4)-(5) distinguish by family size, proxied by an indicator for 3G having any cousins, and Columns (6)-(7) distinguish by distance of 3G and 2G at $\tau - 1$. We estimate a decrease in grandsons' labor supply, by more than 31 hours/month

from $\tau + 1$ through $\tau + 3$. The effects are significant at 10% or 5% level. We find suggestive evidence of distance playing a role for grandchildren: closer granddaughters increase labor supply, while those living more than 2km away decrease it. Similarly, grandsons living more than 2km away also significantly decrease their labor supply.

Overall, we find evidence of grandchildren adapting their labor supply to contribute to their grandparents' care, either financially or through their time. Our results suggest this might be the case for granddaughters living outside of their parents' household, that decrease their work time. Similarly, grandsons from richer households (with less stringent financial constraints) or smaller families (where the supply of potential informal caregivers is lower) reduce work hours, and potentially spend more time with or caring for their grandparents instead.

1.6 Conclusions

This paper estimates spillover effects of restricting access to nursing homes across three generations. It exploits the quasi-experimental setting provided by a 2015 Dutch reform to the LTC system, restricting access to nursing home and incentivizing elders to stay self-sufficient for as long as possible. In a difference-in-differences setting, we use the reform to abstract from endogeneity in care choices. We compare elders losing eligibility for nursing homes to those that remain eligible, and look at spillover effects on their adult children and grandchildren.

Using administrative data on the full Dutch population, and focusing on first-time LTC applicants between 2014 and 2015, we provide evidence of how an exogenous change of LTC options available and an increase in families' time and financial constraints can affect multiple generations. We find restricting access to nursing homes, without fully compensating for the additional costs of taking care of the elderly at home, increases children labor supply in the two years following the application date, while decreasing their wealth three years later. We do not find aggregate effects on grandchildren' labor supply.

Our findings highlight that there are important heterogeneities in how families adapt to the new LTC system. Elders' financial resources mainly influence their male descendants reaction. Women, who typically exhibit a more elastic labor supply, adapt their work hours depending on the opportunity cost of providing informal care themselves. Family size and household composition shape the involvement of youngest descendants.

On the aggregate level, the 2015 reform lead a sizable reduction of government expenditure. Keeping constant the share of population above 65 to its 2014 level, government expenses in LTC decreased, on average, by almost one billion euro per year between 2015 and 2019 (0.13% of GDP)[16]. Our estimates on 2G indicate a decrease in liquid wealth by 8,549€. While 2G increases the work hours, we do not estimate an change in their earnings or personal income. Applying the marginal tax rates on savings[17], our estimates imply, at most, a loss in government revenues by 6 million euro. Thus, the reform was successful in containing governments' budget. However, our results indicate some groups are more affected than others by the more stringent financial constraint. While wealth losses seem to be concentrated among wealthier families, work hours reduction also affect women with low hourly wage, as well as young women living outside of their parents home, and young men in smaller families. Future research should be devoted to better understand if the reform lead to an increase in gender inequality, or had a long-lasting effects on younger generation labor market trajectories.

Our analysis has an important limitation, in that we can only observe time adjustments linked to labor supply. Because we do not have information on actual time use, we cannot rule out that, when family members do not increase labor supply, they are also not reducing leisure or home-related activities time, to provide care to their elders. Furthermore, while we do not find an effect on elders' survival, we also do not explore wether the reform had an impact on their health, potentially increasing public health care expenses. Additional research should be devoted to investigating both channels, that would provide a fuller picture of family's organization regarding elder care arrangements, and a more overview of the reform's effect on public expenditure.

Overall, our findings reveal aging-in-place policies can have important and long-lasting effects on younger generations, and caution policy maker to take these effects into account in policy design. Without adequate compensation for the increased financial burden, families might have to erode their savings or work longer hours to sustain their elders, and this, in turn, might have broader implications for their well being.

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[16] Own calculations from OECD (2024).
[17] Between 2.9% and 4.6%, depending on the source of wealth

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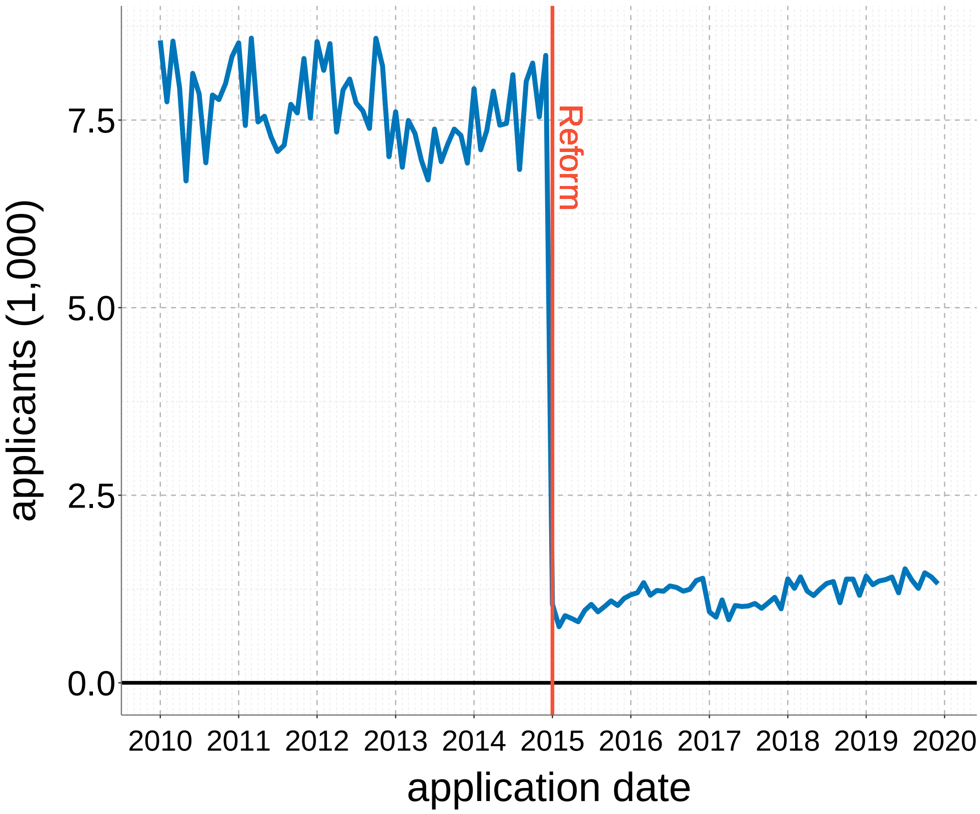
Appendices to Chapter 1

Appendix 1.A Additional Figures and Tables

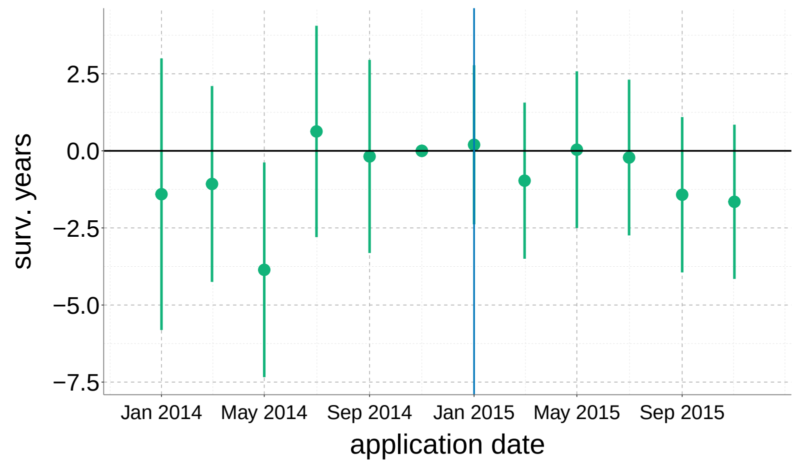
Figure 1.A.1. Applications to CIZ.



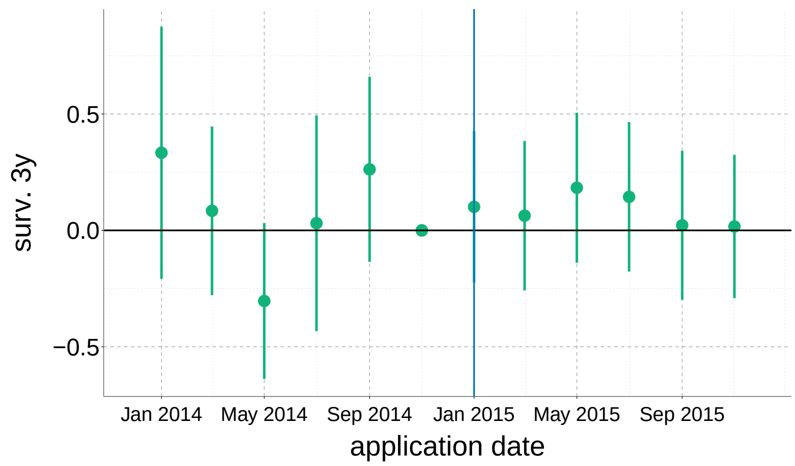
Notes: Figure 1.A.1 displays the number of first-time applications for LTC evaluated by the Care Assessment Center (CIZ) between January 2010 and December 2019, by month of application. The CIZ evaluates applications to any LTC service (home and residential care) through December 2014, and only receives applications to residential LTC after January 2015. Y-axis indicates number of first-time applicants, in thousands. X-axis indicates the start of each year between 2010 and 2019. One point indicates thousands first-time applicants in one month. Months of applications on the x-axis are omitted for simplicity. *Source*: Authors' calculations from the CBS data.

Figure 1.A.2. Survival of 1G.

(a) Survival years

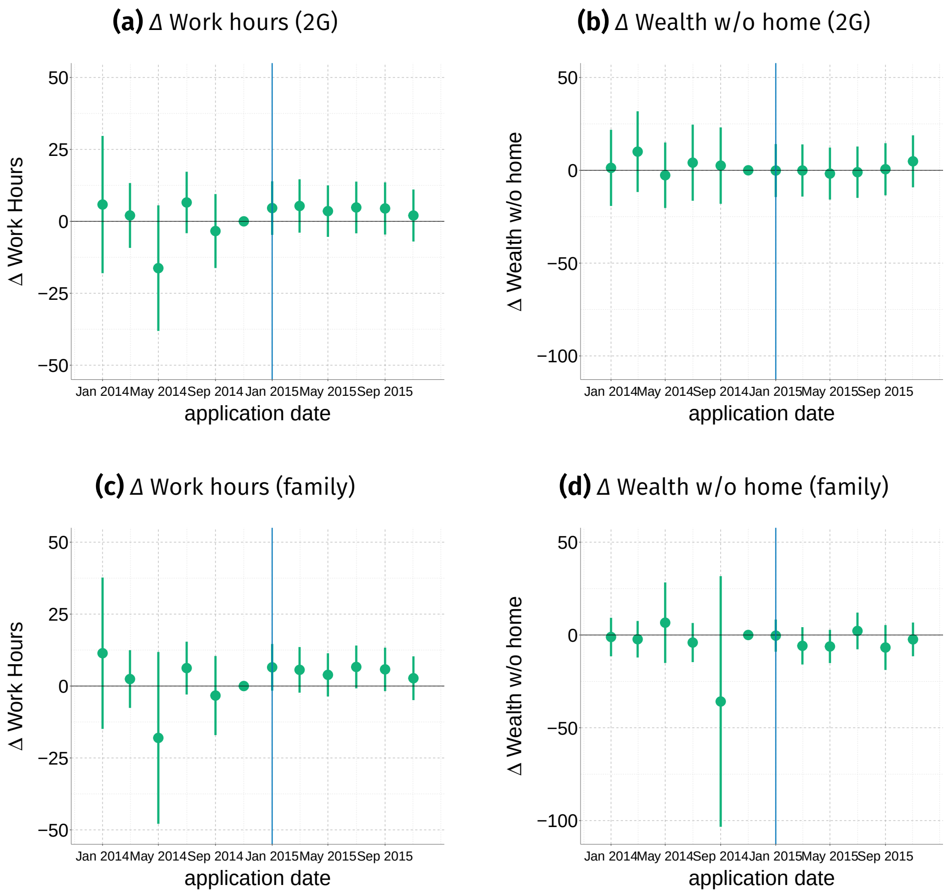


(b) Surv. 3Y



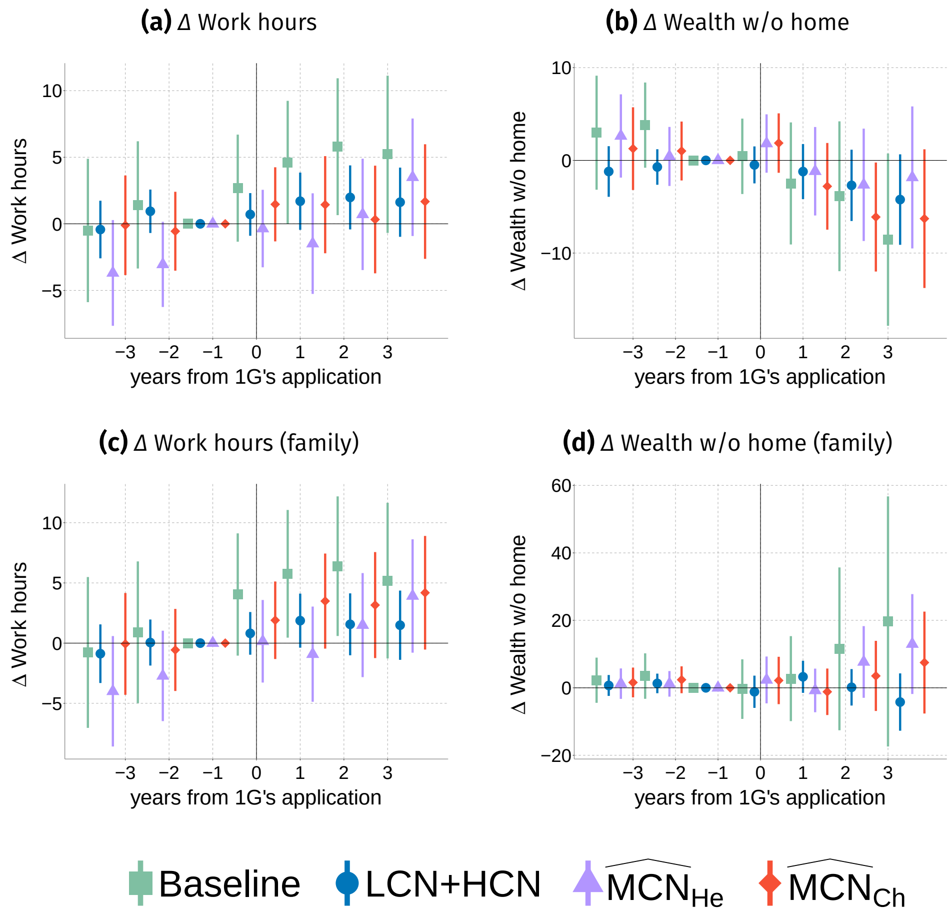
Notes: Figure 1.A.2 displays the estimated effect being in the MCN group (treatment *HCN*) on survival years (Panel a) and likelihood of surviving three years after the application (Panel b) by application period (2-months bins). Figure plots 95 percent CIs. *Source*: Authors' calculations from the CBS data.

Figure 1.A.3. Effect on 2G (event study).



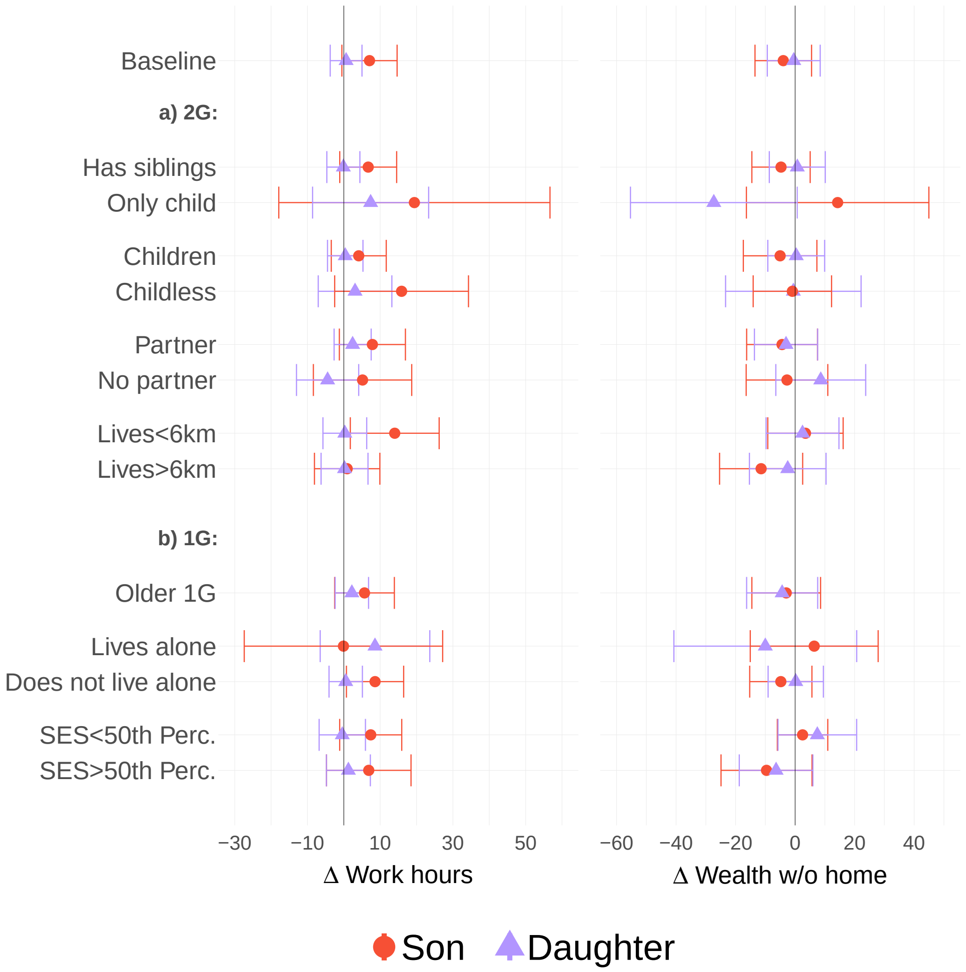
Notes: Figure 1.A.3 displays estimated coefficients on 2G main labor supply and savings one year after the application by application date. Outcomes are changes in total work hours per month (a and c) and in household wealth net home value, expressed in thousands euro per year (b and d). Monetary values are in 2015 euro. Panels (a-b) use sample at individual level (one observation=one 2G), (c-d) at family level (one observation=one family). All controls are included. On the x-axis, application date in 2-months time bins is indicated. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

Figure 1.A.4. Alternative treatments: effect on 2G around application time (DiD).



Notes: Figure 1.A.4 displays estimated DiD coefficients on 2G labor supply and savings from three years before to three years after the application date when treatment is estimated via alternative definitions. *HCN* indicates our baseline treatment; *HCN* + *LCN* includes *LCN* elders and their families in the control group; $\widehat{MCN}_{He}$ and $\widehat{MCN}_{Ch}$ predict treatment using medical specialist diagnosis categories or corresponding Charsol Index. Outcomes are changes in monthly work hours (a and c) and wealth net home value (b and d). Panels (a-b) measure outcomes at individual level, (c-d) at family level. All controls are included. On the x-axis, time is expressed with respect to the application date. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

Figure 1.A.5. Other heterogenous effects on 2G (DiD).



Notes: Figure 1.A.5 displays heterogenous treatment effect on 2G's labor supply and savings one year after the application. Sample: one 2G is one observation. Treatment *HCN*. Outcomes, indicated on the x-axis, are the change (from $\tau - 1$) either in total work hours per month (left panel), or in household wealth net home value (right panel), expressed in thousands euro per year. All controls are included. Y-axis indicates the characteristics of 2G (part a) or 1G (part b) used to split the sample. Red dots indicate results for sons, purple triangles for daughters. All subfigures plot the 95 percent CIs. *Source*: Authors' calculations from the CBS data.

Table 1.A.1. Summary statistics, 1G.

	Baseline		MCN		HCN	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
1) Biography						
Birth year	1937.557	6.000	1938.049	5.912	1936.326	6.043
Female	0.472	0.499	0.487	0.500	0.435	0.496
Adult children	2.265	0.681	2.275	0.678	2.238	0.688
Has a daughter	0.773	0.419	0.775	0.417	0.767	0.423
Dist. to 2G (km)	11.785	27.010	10.842	25.435	14.144	30.471
Lives with 2G	0.041	0.197	0.038	0.192	0.046	0.210
1G lives alone	0.100	0.300	0.098	0.297	0.106	0.308
2) Household's SES, income, and savings						
SES (percentile)	52.920	26.058	51.213	25.489	55.156	26.622
Income	30.774	11.371	29.917	10.768	32.916	12.502
Wealth	160.505	223.994	140.851	201.271	209.637	266.384
Wealth w/o home	66.610	141.534	55.717	123.125	93.840	176.572
Owns home	0.485	0.500	0.454	0.498	0.562	0.496
3) LTC						
Admitted (pre)	1.000	0.000	1.000	0.000	1.000	0.000
Admitted (post)	0.297	0.457	0.000	0.000	1.000	0.000
Nursing home (pre)	0.793	0.405	0.780	0.415	0.864	0.343
Nursing home (post)	0.234	0.423	0.142	0.349	0.451	0.498
Home care (pre)	0.085	0.280	0.092	0.289	0.049	0.216
Home care (post)	0.759	0.427	0.988	0.110	0.413	0.218
Days to admission (post)	299.235	428.978	575.984	441.377	0.000	0.000
3) Longevity						
Surv. 1Y	0.099	0.299	0.093	0.290	0.116	0.320
Surv 2Y	0.720	0.449	0.818	0.386	0.474	0.499
Surv. 3Y	0.624	0.484	0.723	0.447	0.377	0.485
Surv. years	5.555	4.409	6.539	4.301	3.095	3.649
Obs.	7,237	7,237	5,169	5,169	2,068	2,068

Notes: Table 1.A.1 reports descriptive statistics for 1G in the baseline sample and by treatment status, one year before applying to LTC. “MCN" stands for Medium Care Need, “HCN" for High Care Need. “S.D." stands for standard deviation. “Admitted" is an indicator for being admitted to nursing home care. “Pre" and “post" indicate wether the variable is measured only for applicants before or after January 2015. Monetary values are expressed in 1,000 (2015)€. *Source*: Authors' calculations from the CBS data.

Table 1.A.2. Summary statistics, 2G and 3G.

	Baseline		MCN		HCN	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Panel A: 2G						
1) Biography						
Age	49.162	6.250	48.807	6.174	49.628	6.318
Female	0.496	0.500	0.496	0.500	0.496	0.500
Siblings	1.794	0.917	1.805	0.914	1.779	0.921
Has a partner	0.789	0.408	0.793	0.405	0.784	0.411
Has children	0.785	0.411	0.790	0.407	0.777	0.416
Lives with 1G	0.018	0.133	0.016	0.127	0.020	0.141
Dist. to 1G (km)	25.610	41.143	24.614	40.284	26.913	42.208
1G <5km	0.495	0.500	0.504	0.500	0.483	0.500
2) Household's SES, income, and savings						
SES (percentile)	64.798	25.142	64.395	24.934	65.324	25.402
Income	56.536	86.040	56.499	105.419	56.584	50.482
Wealth	225.887	896.018	207.901	896.738	249.424	894.576
Wealth w/o home	164.964	876.467	150.246	885.352	184.224	864.371
3) Labor supply						
Months employed (year)	8.662	5.214	8.774	5.157	8.516	5.284
Work hours (month)	101.303	73.780	102.530	73.746	99.697	73.798
Irregular work hours (year)	67.213	273.718	71.209	284.592	61.985	258.720
Gross wage (month)	2.748	3.600	2.727	3.440	2.775	3.799
Hourly wage	18.862	22.098	18.709	20.663	19.063	23.845
Panel B: 3G						
1) Biography						
Age	25.314	5.255	25.108	5.205	25.721	5.329
Female	0.490	0.500	0.493	0.500	0.486	0.500
1G Birth year	1932.251	5.620	1932.545	5.670	1931.672	5.474
Dist. to 1G (km)	29.055	42.948	27.788	41.955	31.554	44.742
Lives with parents	0.415	0.493	0.431	0.495	0.384	0.486
3) Labor supply						
Months employed (year)	9.006	4.556	8.981	4.561	9.056	4.547
Work hours (month)	94.328	69.694	93.201	69.783	96.552	69.469
Irregular work hours (year)	99.550	273.235	99.505	278.095	99.639	263.398
Gross wage (month)	1.314	1.618	1.256	1.576	1.429	1.692
Hourly wage	10.104	10.526	9.679	9.273	10.942	12.599
Obs. (2G)	13,908	13,908	12,332	12,332	9,424	9,424
Obs. (3G)	13,605	13,605	7,711	7,711	5,894	5,894

Notes: Table 1.A.2 reports descriptive statistics for 2G and 3G in their respective baseline sample and by treatment status, one year before 1G applies to LTC. MCN stands for Medium Care Need, HCN for High Care Need. Monetary values are expressed in 2015€. Except hourly wage, they are also expressed in thousands. *Source*: Authors' calculations from the CBS data.

Table 1.A.3. First stage, alternative treatments (DiD)

	(1)	(2)	(3)	(4)	(5)	Mean (before)
Panel A: Treatment *LCN* + *HCN*						
Nursing home	-0.864***	-0.861***	-0.867***	-0.866***	-0.844***	0.78
	(0.018)	(0.018)	(0.018)	(0.019)	(0.026)	(0.011)
Home care	0.754***	0.769***	0.786***	0.785***	0.643***	0.092
	(0.014)	(0.014)	(0.015)	(0.015)	(0.021)	(0.008)
Obs.	13,496	13,496	13,496	13,496	13,496	1,444
Panel B: Treatment $\widehat{MCN}_{He}$						
Nursing home	-0.257***	-0.259***	-0.259***	-0.257***		0.737
	(0.031)	(0.032)	(0.032)	(0.032)		(0.017)
Home care	0.763***	0.762***	0.761***	0.760***		0.109
	(0.019)	(0.019)	(0.019)	(0.019)		(0.012)
Obs.	7,013	7,013	7,013	7,013		695
Panel C: Treatment $\widehat{MCN}_{Ch}$						
Nursing home	-0.386***	-0.392***	-0.392***	-0.389***		0.803
	(0.030)	(0.030)	(0.030)	(0.030)		(0.022)
Home care	0.806***	0.801***	0.800***	0.796***		0.057
	(0.018)	(0.018)	(0.018)	(0.018)		(0.013)
Obs.	7,255	7,255	7,255	7,255		315
Cohort&Gender		✓	✓	✓	✓	-
Family		✓	✓	✓	✓	-
Geo			✓	✓	✓	-
SES				✓	✓	-
Health					✓	-

Notes: This table shows alternative treatment effects on 1G's LTC arrangements. Treatment *HCN* + *LCN*: MCN are treated, HCN and LCN are control. Treatment $\widehat{MCN}_{He}$: predicted MCN and HCN based on specialist diagnosis. Treatment $\widehat{MCN}_{Ch}$: predicted MCN and HCN based on Charsol Index. Sample 1G: born in the Netherlands before 1949; applied for the first time to LTC services between 2014 and 2015; have children born before 1996. *Cohort&Gender* includes 1G's 5-years grouped birth cohort fixed effects and female indicator. *Family* includes an indicator for 1G living alone one year before application ($\tau - 1$), an indicator for 1G having at least a female child, and age of 1G's youngest child. *Geo* includes an indicator for at least one child living less than 5.75km from 1G, and 1G's municipality of residence fixed effects at $\tau - 1$. *SES* includes 1G's SES quartiles fixed effects and an indicator for a member of the 1G's household owning a house at $\tau - 1$. *Health* includes expenses for specialist somatic care and the estimated Charsol Index at $\tau - 1$. Last column reports means for MCN applying before the reform. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Table 1.A.4. First stage by period of application (Event Study)

	HCN		*HCN + LCN*		$\widehat{MCN}_{He}$		$\widehat{MCN}_{Ch}$	
	Home care	**Nursing home**	**Home care**	**Nursing home**	**Home care**	**Nursing home**	**Home care**	**Nursing home**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Jan 2014	0.012	-0.063	-0.297***	0.212***	-0.008	-0.181	-0.031	-0.053
	(0.037)	(0.095)	(0.042)	(0.060)	(0.037)	(0.125)	(0.039)	(0.136)
Mar 2014	0.031	-0.071	-0.162***	0.129**	0.078	-0.136	-0.046	0.125
	(0.051)	(0.099)	(0.037)	(0.052)	(0.050)	(0.093)	(0.050)	(0.094)
May 2014	0.093**	0.044	-0.216***	0.149**	0.052	-0.241**	-0.058	0.153
	(0.041)	(0.133)	(0.040)	(0.052)	(0.059)	(0.102)	(0.055)	(0.097)
Jul 2014	-0.022	0.011	-0.237***	0.169***	0.040	-0.155*	-0.019	0.016
	(0.057)	(0.107)	(0.034)	(0.050)	(0.047)	(0.093)	(0.048)	(0.094)
Sep 2014	0.090**	-0.147	-0.167***	0.100*	0.017	-0.187*	-0.045	0.177*
	(0.039)	(0.097)	(0.037)	(0.053)	(0.052)	(0.097)	(0.053)	(0.095)
Nov 2014	-	-	-	-	-	-	-	-
	-	-	-	-	-	-	-	-
Jan 2015	0.804***	-0.332***	0.574***	-0.782***	0.821***	-0.487***	0.794***	-0.408***
	(0.037)	(0.088)	(0.030)	(0.045)	(0.035)	(0.076)	(0.038)	(0.079)
Mar 2015	0.797***	-0.224**	0.641***	-0.758***	0.821***	-0.370***	0.792***	-0.289***
	(0.036)	(0.087)	(0.029)	(0.044)	(0.033)	(0.075)	(0.037)	(0.078)
May 2015	0.824***	-0.239**	0.623***	-0.755***	0.832***	-0.395***	0.803***	-0.302***
	(0.035)	(0.086)	(0.027)	(0.043)	(0.033)	(0.074)	(0.036)	(0.077)
Jul 2015	0.764***	-0.173**	0.626***	-0.719***	0.786***	-0.360***	0.753***	-0.268***
	(0.035)	(0.086)	(0.027)	(0.043)	(0.033)	(0.073)	(0.036)	(0.076)
Sep 2015	0.753***	-0.171**	0.645***	-0.730***	0.760***	-0.352***	0.739***	-0.258***
	(0.036)	(0.086)	(0.028)	(0.044)	(0.033)	(0.073)	(0.037)	(0.077)
Nov 2015	0.776***	-0.244**	0.669***	-0.780***	0.779***	-0.410***	0.754***	-0.313***
	(0.035)	(0.086)	(0.027)	(0.043)	(0.033)	(0.073)	(0.036)	(0.077)
Cohort&Gender	✓	✓	✓	✓	✓	✓	✓	✓
Family	✓	✓	✓	✓	✓	✓	✓	✓
Geo	✓	✓	✓	✓	✓	✓	✓	✓
SES	✓	✓	✓	✓	✓	✓	✓	✓
Health	✓	✓	✓	✓				

Notes: This table shows the treatment effect on 1G's LTC arrangements by application period (2-months groups). Base period is November 2014. Outcome is indicated in each column title. Columns (1)-(2) consider the main treatment definition, with only HCN as control group. Columns (3)-(4) use both HCN and LCN as control group (HCN+LCN). Columns (5)-(6) use predicted treatment via specialist care diagnosis category ($\widehat{MCN}_{He}$). Columns (7)-(8) use predicted treatment via Charsol Index ($\widehat{MCN}_{Ch}$). Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculations from the CBS data.

Table 1.A.5. Survival of 1G (DiD)

	Treatment:			
	HCN	$HCN + LCN$	$\widehat{MCN}_{He}$	$\widehat{MCN}_{Ch}$
	(1)	(2)	(3)	(4)
Surv. years	0.304	0.470**	1.063**	1.414**
	(0.357)	(0.164)	(0.430)	(0.428)
Surv. 1Y	-0.057*	-0.023*	-0.032	-0.018
	(0.032)	(0.013)	(0.034)	(0.035)
Surv. 2Y	-0.010	0.001	-0.048	-0.074*
	(0.035)	(0.015)	(0.037)	(0.038)
Surv. 3Y	0.074*	0.024	0.076	0.087*
	(0.043)	(0.018)	(0.047)	(0.047)
Obs.	7,237	13,496	7,013	7,255
Cohort&Gender	✓	✓	✓	✓
Family	✓	✓	✓	✓
Geo	✓	✓	✓	✓
SES	✓	✓	✓	✓
Health	✓	✓		

Notes: This table shows the treatment effects on 1G's survival. Treatment HCN: MCN are treated, HCN are control. Treatment $HCN + LCN$: MCN are treated, HCN and LCN are control. Treatment $\widehat{MCN}_{He}$: predicted MCN and HCN based on specialist diagnosis. Treatment $\widehat{MCN}_{Ch}$: predicted MCN and HCN based on Charsol Index. Survival years are measured as number of days between the application date and either the date of death or January 1, 2020, divided by 365. Surv. 1Y, 2Y, 3Y measure the likelihood of being alive one, two, or three years after the application date. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *Source*: Authors' calculations from the CBS data.

Table 1.A.6. Impact on 2G's labor supply and savings over time (DiD)

	Years from application date:					
	-3	**-2**	**0**	**1**	**2**	**3**
Panel A: Labor Supply						
Δ Months employed	-0.186	-0.091	-0.019	0.227	0.477**	0.465**
	(0.180)	(0.150)	(0.129)	(0.151)	(0.186)	(0.215)
Δ Work hours	-0.497	1.423	2.679	4.613*	5.792**	5.222*
	(2.746)	(2.437)	(2.051)	(2.358)	(2.622)	(3.012)
Δ Irregular work hours	9.205	10.292	25.362	25.165	12.422	2.325
	(19.735)	(18.687)	(16.928)	(17.995)	(13.372)	(11.886)
Δ Earnings	-0.675	-0.385	-0.658*	0.474	0.641	0.435
	(0.544)	(0.439)	(0.383)	(0.578)	(0.617)	(0.747)
Δ Income (main source)	-0.402	-0.705	-0.201	0.395	-0.314	-1.378*
	(0.565)	(0.491)	(0.460)	(0.583)	(0.718)	(0.813)
Panel B: Savings						
Δ Wealth	-3.594	4.322	-2.593	-1.239	1.192	1.701
	(4.424)	(3.608)	(3.268)	(4.669)	(6.101)	(7.397)
Δ Wealth w/o home	2.985	3.801	0.437	-2.489	-3.871	-8.549*
	(3.137)	(2.344)	(2.075)	(3.358)	(4.122)	(4.741)
Δ Bank assets	0.133	1.314	0.222	-0.237	-1.848	-2.810
	(1.635)	(1.215)	(1.189)	(1.547)	(1.841)	(2.233)
Δ Financial assets	-0.128	1.861	0.282	-0.807	-1.863	-3.096
	(1.770)	(1.293)	(1.279)	(1.837)	(2.170)	(2.659)
Obs.	15,977	15,970	15,934	15,911	15,822	15,769
Gender&Age	✓	✓	✓	✓	✓	✓
Family	✓	✓	✓	✓	✓	✓
Geo	✓	✓	✓	✓	✓	✓
SES	✓	✓	✓	✓	✓	✓
Occupation	✓	✓	✓	✓	✓	✓
Health	✓	✓	✓	✓	✓	✓

Notes: This table shows the treatment effect on 2G's labor supply and savings between three years before and 3 years after the application date. Columns indicate the number of years the outcome is observed from the application date. Outcomes are measured as differences with respect to their value in $\tau - 1$, Column $\tau - 1$ is omitted. Treatment *HCN*: MCN are treated, HCN are control. Sample: 1G born in the Netherlands before 1949; 1G applied for the first time to LTC services between 2014 and 2015; 2G born before 1996; one 2G is one observation. Controls in Equation 1.2. Panel A focuses on labor supply: changes in month employed per year, total work hours per month, irregular work hours per year, gross earnings per year, gross income from main personal income source per year. Panel B focuses on savings, observed at household level per year. Monetary values are expressed in thousand 2015 euro. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Table 1.A.7. Impact on families' labor supply and savings over time (DiD)

	Years from application date:					
	-3	**-2**	**0**	**1**	**2**	**3**
Panel A: Labor Supply (per 2G)						
Δ Months employed	-0.299*	-0.151	0.086	0.272*	0.465**	0.452*
	(0.177)	(0.155)	(0.140)	(0.159)	(0.196)	(0.236)
Δ Work hours	-0.775	0.897	4.038	5.755**	6.389**	5.190
	(3.195)	(3.004)	(2.590)	(2.705)	(2.957)	(3.297)
Δ Irregular work hours	18.199	12.257	24.249	26.448	13.459	1.209
	(27.086)	(26.373)	(24.118)	(24.325)	(16.466)	(11.663)
Panel B: Savings (per family member)						
Δ Wealth	0.162	0.430	-0.461	2.748	8.273	11.872
	(4.135)	(4.121)	(4.762)	(7.547)	(10.531)	(15.638)
Δ Wealth w/o home	2.252	3.480	-0.384	2.724	11.550	19.676
	(3.415)	(3.436)	(4.504)	(6.424)	(12.309)	(18.902)
Δ Bank assets	-0.302	0.687	-0.536	0.551	-0.680	-0.832
	(0.569)	(0.424)	(0.528)	(0.803)	(1.167)	(2.097)
Δ Financial assets	0.514	0.784	0.278	0.245	-0.224	1.128
	(0.854)	(0.566)	(0.605)	(0.991)	(1.517)	(3.801)
Δ Other Assets	-0.060	0.012	-0.428**	0.038	0.268	0.345
	(0.184)	(0.063)	(0.193)	(0.372)	(0.449)	(0.596)
Obs.	5,529	5,529	5,529	5,529	5,529	5,529
Gender&Age	✓	✓	✓	✓	✓	✓
Family	✓	✓	✓	✓	✓	✓
Geo	✓	✓	✓	✓	✓	✓
SES	✓	✓	✓	✓	✓	✓
Health	✓	✓	✓	✓	✓	✓

Notes: This table shows the treatment effect on 2G's labor supply and savings between three years before and 3 years after the application date. Columns indicate the number of years the outcome is observed from the application date. Outcomes are measured as differences with respect to their value in $\tau-1$, Column $\tau-1$ is omitted. Treatment *HCN*: MCN are treated, HCN are control. Sample: 1G born in the Netherlands before 1949; 1G applied for the first time to LTC services between 2014 and 2015; 2G born before 1996; one family is one observation. Controls as in Equation 1.3. Panel A focuses on labor supply measured as per capita across all 2Gs in the family. Panel B focuses on savings, measured yearly and per family member (including 1G, 2G, and in-laws). Monetary values are expressed in thousand 2015 euro. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Table 1.A.8. Other heterogeneous effects on daughters (DiD)

		1G			*2G*							
	Baseline	**Older**	**Lives alone**		**Has siblings**		**Children**		**Partner**		**<5.75km**	
			Yes	No	No	Yes	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: $\tau - 3$												
Δ Work hours	-3.676	-6.835*	19.046*	-6.493**	-6.080	-3.730	-5.548**	4.203	-4.044	-5.695	-2.013	-5.460
	(2.821)	(3.775)	(10.344)	(2.860)	(6.346)	(3.009)	(2.752)	(8.091)	(2.800)	(7.505)	(3.934)	(4.098)
Δ Wealth w/o home	6.121	9.607	13.738	6.014	37.738*	6.058	3.985	14.607	6.792	2.739	3.713	6.923
	(4.964)	(6.999)	(14.378)	(5.317)	(22.509)	(5.140)	(5.449)	(12.479)	(6.071)	(7.778)	(5.379)	(8.012)
Panel B: $\tau - 2$												
Δ Work hours	-0.903	-1.930	11.581**	-2.447	-2.898	-1.110	-3.561*	10.237*	-3.480	4.963	2.093	-3.953
	(2.094)	(2.702)	(4.769)	(2.304)	(5.205)	(2.232)	(2.139)	(5.762)	(2.214)	(4.849)	(2.883)	(2.965)
Δ Wealth w/o home	4.788	3.187	8.555	4.640	19.413	4.532	5.158	5.030	5.804	3.559	10.280**	-0.616
	(3.460)	(4.854)	(7.946)	(3.801)	(13.300)	(3.583)	(4.255)	(4.247)	(4.533)	(2.915)	(4.446)	(5.192)
Panel C: τ												
Δ Work hours	-1.452	-0.616	1.498	-0.953	-4.001	-1.483	-2.231	1.890	-0.452	-4.844	-1.025	-2.330
	(1.648)	(1.815)	(6.351)	(1.667)	(6.303)	(1.724)	(1.856)	(3.964)	(1.837)	(3.610)	(2.140)	(2.535)
Δ Wealth w/o home	-3.478	-6.674**	-13.643*	-1.894	-0.943	-2.693	-3.004	-5.513	-1.765	-7.757	-0.554	-6.811*
	(2.730)	(3.296)	(7.048)	(2.985)	(12.313)	(2.751)	(3.082)	(5.966)	(3.250)	(4.865)	(3.528)	(4.108)
Panel D: $\tau + 1$												
Δ Work hours	0.629	2.216	8.575	0.522	7.375	-0.122	0.395	3.089	2.426	-4.456	0.279	0.198
	(2.230)	(2.343)	(7.676)	(2.341)	(8.120)	(2.318)	(2.486)	(5.161)	(2.600)	(4.360)	(3.069)	(3.291)
Δ Wealth w/o home	-0.457	-4.353	-10.019	0.214	-27.319*	0.734	0.378	-0.595	-3.050	8.621	2.496	-2.480
	(4.536)	(6.091)	(15.644)	(4.738)	(14.258)	(4.797)	(4.872)	(11.611)	(5.419)	(7.699)	(6.244)	(6.567)
Panel E: $\tau + 2$												
Δ Work hours	3.261	1.673	13.780	2.588	6.204	2.783	3.077	2.643	4.731	0.151	4.897	1.125
	(2.920)	(2.660)	(9.274)	(3.061)	(9.099)	(3.076)	(3.264)	(5.699)	(3.450)	(5.277)	(4.616)	(3.746)
Δ Wealth w/o home	-3.120	-7.281	13.851	-5.149	-7.035	-2.788	-5.206	8.592	-7.880	13.362	2.656	-5.440
	(5.075)	(6.533)	(18.877)	(5.289)	(21.050)	(5.335)	(5.575)	(12.447)	(6.082)	(8.784)	(6.743)	(7.611)
Panel F: $\tau + 3$												
Δ Work hours	3.907	2.029	26.908**	1.948	7.988	3.170	3.768	4.214	3.439	7.276	7.761*	0.364
	(3.275)	(3.687)	(11.188)	(3.354)	(10.618)	(3.435)	(3.139)	(9.910)	(3.771)	(6.661)	(4.555)	(4.687)
Δ Wealth w/o home	-9.074	-11.157	30.138	-13.372*	8.105	-10.280	-12.378	13.842	-18.095**	21.345	-8.117	-7.549
	(6.831)	(9.400)	(21.296)	(7.228)	(28.517)	(7.256)	(7.801)	(14.278)	(7.875)	(13.230)	(7.945)	(10.884)
Obs.	7,833	5,459	749	7,084	509	7,324	6,376	1,457	6,073	1,760	3,996	3,837

Notes: This table shows heterogenous treatment effect on daughters (female 2G) around the application date τ. Sample: female 2G, one 2G is one observation. Treatment *HCN*. Outcomes are changes, compared to one year before the application date, in total work hours per month and household wealth net of home value in thousand euro per year. All controls are included. Each Panel splits 2G by gender. Column (1) looks at baseline estimates. Column (2) on families with 1G above age 75. Columns (3)-(4) by 1G living alone. Columns (5)-(6) by 2G having siblings. Columns (7)-(8) by 2G having children in the household. Columns (9)-(10) by having a cohabiting partner. Columns (11)-(12) by 2G living less than 5.75km away from 1G. Monetary values are expressed in 2015 euro. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Table 1.A.9. Other heterogeneous effects on sons (DiD)

		1G			*2G*							
	Baseline	**Older**	**Lives alone**		**Has siblings**		**Children**		**Partner**		**<5.75km**	
			Yes	No	No	Yes	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: $\tau - 3$												
Δ Work hours	1.086	-0.577	-8.899	1.824	2.189	0.801	-5.552	17.741	1.574	0.237	8.271	-6.150
	(4.447)	(4.878)	(15.626)	(4.666)	(25.219)	(4.520)	(4.215)	(10.838)	(5.233)	(8.436)	(7.518)	(4.855)
Δ Wealth w/o home	-0.312	-2.604	6.384	-0.678	0.721	-0.633	-0.241	-1.548	-3.193	9.541*	-2.092	1.629
	(3.992)	(4.714)	(11.104)	(4.346)	(13.941)	(4.127)	(5.073)	(5.930)	(4.976)	(5.537)	(6.136)	(5.329)
Panel B: $\tau - 2$												
Δ Work hours	2.868	3.426	-7.380	3.934	1.654	3.053	-3.965	19.304*	1.555	6.832	10.574	-4.697
	(4.015)	(4.161)	(12.922)	(4.215)	(22.728)	(4.068)	(3.413)	(10.393)	(4.640)	(7.821)	(7.211)	(3.752)
Δ Wealth w/o home	2.337	-1.634	7.473	1.645	18.151	1.532	2.483	0.295	1.910	2.886	2.151	3.043
	(3.223)	(3.289)	(8.364)	(3.530)	(12.232)	(3.329)	(4.104)	(4.909)	(3.950)	(4.980)	(4.979)	(4.154)
Panel C: τ												
Δ Work hours	5.489	3.214	-0.837	6.591*	27.751	4.943	5.858*	6.037	8.921**	-5.821	9.715	1.243
	(3.474)	(3.583)	(12.237)	(3.533)	(21.448)	(3.509)	(3.020)	(9.474)	(4.072)	(6.627)	(5.997)	(3.713)
Δ Wealth w/o home	3.584	2.213	1.242	3.537	21.199*	2.507	4.446	2.368	4.934	0.084	2.177	5.189
	(3.100)	(3.823)	(6.313)	(3.439)	(12.486)	(3.171)	(3.888)	(4.868)	(3.864)	(4.456)	(4.127)	(4.614)
Panel D: $\tau + 1$												
Δ Work hours	7.063*	5.692	-0.098	8.591**	19.406	6.709*	4.107	15.888*	7.857*	5.151	13.998**	0.922
	(3.872)	(4.185)	(13.894)	(4.014)	(18.975)	(3.991)	(3.856)	(9.381)	(4.634)	(6.895)	(6.232)	(4.587)
Δ Wealth w/o home	-3.976	-2.988	6.421	-4.801	14.292	-4.733	-5.038	-0.922	-4.388	-2.728	3.486	-11.423
	(4.848)	(5.889)	(10.945)	(5.334)	(15.602)	(4.999)	(6.306)	(6.715)	(6.069)	(6.993)	(6.459)	(7.122)
Panel E: $\tau + 2$												
Δ Work hours	6.751	6.017	8.801	7.307*	13.334	6.663	3.609	16.183*	7.899	3.951	9.715	4.383
	(4.200)	(5.066)	(16.613)	(4.328)	(30.890)	(4.215)	(4.794)	(8.771)	(4.872)	(8.629)	(6.921)	(5.059)
Δ Wealth w/o home	-3.236	-2.730	6.081	-5.295	51.304	-5.513	-3.254	-1.991	-2.083	-3.674	-0.296	-5.451
	(6.216)	(7.621)	(21.020)	(6.629)	(32.751)	(6.280)	(7.932)	(9.057)	(7.676)	(9.227)	(9.582)	(8.155)
Panel F: $\tau + 3$												
Δ Work hours	4.164	4.702	22.839	2.573	5.659	4.445	2.998	8.595	4.402	5.094	13.694*	-3.309
	(4.824)	(6.314)	(15.259)	(5.116)	(27.133)	(4.938)	(5.839)	(8.891)	(5.587)	(9.959)	(7.723)	(5.911)
Δ Wealth w/o home	-7.074	-3.701	10.716	-9.502	30.522	-8.705	-8.344	-3.204	-9.240	0.818	-9.185	-4.351
	(6.538)	(8.178)	(18.752)	(7.024)	(37.531)	(6.565)	(8.662)	(8.190)	(8.159)	(9.169)	(10.022)	(8.741)
Obs.	8,101	5,676	706	7,395	523	7,578	5,819	2,282	6,187	1,914	4,140	3,961

Notes: This table shows heterogenous treatment effect on sons (male 2G) around the application date τ. Sample: male 2G, one 2G is one observation. Treatment *HCN*. Outcomes are changes, compared to one year before the application date, in total work hours per month and household wealth net of home value in thousand euro per year. All controls are included. Each Panel splits 2G by gender. Column (1) looks at baseline estimates. Column (2) on families with 1G above age 75. Columns (3)-(4) by 1G living alone. Columns (5)-(6) by 2G having siblings. Columns (7)-(8) by 2G having children in the household. Columns (9)-(10) by having a cohabiting partner. Columns (11)-(12) by 2G living less than 5.75km away from 1G. Monetary values are expressed in 2015 euro. Robust standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *Source*: Authors' calculations from the CBS data.

Table 1.A.10. Heterogeneous impact on family outcomes (DiD)

	Baseline	1G SES<75th		2G has siblings		2G<5.75km	
		Yes	No	No	Yes	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: $\tau - 3$							
Δ Work Hours	-0.775	0.663	-4.231	-0.424	-0.620	3.825	-10.712**
	(3.195)	(4.166)	(3.834)	(9.889)	(3.381)	(4.423)	(3.569)
Δ Wealth w/o home value	2.252	3.306	-1.935	4.156	1.635	5.098	-5.187
	(3.415)	(4.234)	(6.348)	(3.986)	(4.081)	(4.376)	(4.452)
Panel B: $\tau - 2$							
Δ Work Hours	0.897	2.692	-3.190	-1.658	1.289	5.605	-9.400**
	(3.004)	(3.954)	(3.344)	(9.864)	(3.113)	(4.152)	(3.437)
Δ Wealth w/o home value	3.480	5.755	-1.364	5.267*	2.831	6.159	-1.275
	(3.436)	(4.437)	(5.104)	(3.130)	(4.192)	(4.715)	(4.158)
Panel C: τ							
Δ Work Hours	4.038	4.773	2.274	5.887	3.847	7.135**	-1.763
	(2.590)	(3.398)	(2.847)	(8.784)	(2.681)	(3.633)	(2.704)
Δ Wealth w/o home value	-0.384	3.502	-11.982*	2.322	-1.254	3.446	-10.969*
	(4.504)	(5.866)	(6.706)	(2.408)	(5.633)	(5.683)	(6.199)
Panel D: $\tau + 1$							
Δ Work Hours	5.755**	7.414**	2.244	11.107	5.118*	9.590**	-1.237
	(2.705)	(3.398)	(4.050)	(7.932)	(2.881)	(3.670)	(3.280)
Δ Wealth w/o home value	2.724	7.284	-6.562	-3.490	3.924	8.434	-13.061**
	(6.424)	(8.319)	(9.273)	(4.745)	(7.789)	(8.482)	(6.606)
Panel E: $\tau + 2$							
Δ Work Hours	6.389**	8.133**	2.932	9.038	6.183**	10.338**	-0.505
	(2.957)	(3.519)	(5.389)	(11.973)	(2.919)	(3.820)	(4.527)
Δ Wealth w/o home value	11.550	22.993	-17.935	63.925	2.504	11.528	6.769
	(12.309)	(17.146)	(12.062)	(58.370)	(8.796)	(10.109)	(29.040)
Panel F: $\tau + 3$							
Δ Work Hours	5.190	6.494*	2.492	9.420	4.934	10.209**	-4.411
	(3.297)	(3.554)	(7.263)	(11.685)	(3.412)	(4.284)	(4.927)
Δ Wealth w/o home value	19.676	40.999	-27.752	108.849	4.648	20.138	28.814
	(18.902)	(28.093)	(18.734)	(90.364)	(13.792)	(14.705)	(56.698)
Obs.	7,236	5,584	1,652	1,078	6,158	5,041	2,195

Notes: This table shows heterogenous treatment effect on families around the application date τ. Outcomes are changes, compared to one year before the application date, in total work hours per month and household wealth net of home value in thousand euro per year. All controls are included. Column (1) reports baseline family estimates. Columns (2)-(3) split families by 1G's SES at $\tau - 1$. Columns (4)-(5) by number of 2Gs. Columns (6)-(7) by distance of 1G's residence to the closest 2G at $\tau - 1$. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: Authors' calculations from the CBS data.

Table 1.A.11. Heterogeneous impact on 3G's labor supply (DiD)

	Baseline	Lives with 2G		Small Family		<2km from 1G		1G SES<75th	
		Yes	No	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Granddaughters									
$\tau-3$	6.566	13.408*	1.723	5.531	7.230	15.102	3.057	8.587	-8.025
	(5.736)	(7.097)	(8.917)	(14.398)	(6.210)	(10.416)	(6.941)	(6.163)	(13.773)
$\tau-2$	6.496	11.030	2.668	8.440	5.507	11.105	4.764	5.520	6.514
	(4.986)	(7.637)	(6.826)	(8.560)	(5.886)	(10.816)	(5.658)	(5.569)	(11.196)
τ	-2.684	1.499	-6.800	-4.401	-1.521	-0.544	-4.380	-3.442	1.618
	(4.639)	(8.038)	(4.875)	(6.669)	(5.629)	(7.682)	(5.710)	(5.076)	(11.682)
$\tau+1$	-4.724	3.752	-13.967**	-7.110	-4.019	12.157	-10.269	-5.889	-0.797
	(5.876)	(9.792)	(6.524)	(11.667)	(6.808)	(9.609)	(7.071)	(6.551)	(13.699)
$\tau+2$	-3.328	5.620	-13.061	-6.630	-0.883	21.981*	-13.124*	-6.516	9.024
	(7.054)	(11.475)	(8.233)	(13.414)	(8.134)	(13.008)	(7.920)	(7.367)	(20.615)
$\tau+3$	3.537	8.679	-3.292	-4.049	6.862	20.223	-2.438	3.024	-0.116
	(6.622)	(11.202)	(7.507)	(14.057)	(7.580)	(14.664)	(7.204)	(6.892)	(19.937)
Panel B: Grandsons									
$\tau-3$	-2.677	-9.835	6.414	-41.140**	2.286	-11.408	-1.530	0.348	-23.312
	(6.751)	(9.130)	(9.496)	(16.202)	(7.298)	(11.720)	(8.343)	(7.384)	(15.305)
$\tau-2$	-7.589	-10.472	-4.131	-13.600	-7.224	-5.078	-9.317	-4.417	-21.760*
	(5.455)	(7.165)	(8.327)	(18.491)	(5.524)	(11.960)	(6.070)	(5.974)	(12.196)
τ	-2.411	-0.033	-2.856	-20.906	0.571	9.922	-5.769	3.089	-23.351**
	(5.000)	(6.705)	(7.253)	(13.665)	(5.266)	(11.374)	(5.385)	(5.648)	(9.927)
$\tau+1$	-9.969	-12.670	-2.699	-31.171*	-6.371	1.108	-13.317*	-1.520	-40.981***
	(7.074)	(9.401)	(10.680)	(18.720)	(7.538)	(16.064)	(7.700)	(8.129)	(12.349)
$\tau+2$	-11.942*	-12.221	-7.407	-32.588**	-8.980	-1.515	-15.832*	-7.889	-20.054*
	(7.227)	(9.358)	(11.671)	(15.315)	(8.000)	(14.457)	(8.432)	(8.716)	(10.756)
$\tau+3$	-1.355	-9.011	13.760	-31.188*	3.032	-0.438	-2.503	4.045	-17.117
	(7.865)	(9.666)	(13.085)	(17.342)	(8.560)	(18.140)	(8.539)	(9.067)	(15.466)
Obs. (GD)	6,697	2,475	4,222	781	5,916	1,358	5,339	5,684	1,013
Obs. (GS)	6,908	3,450	3,458	817	6,091	1,654	5,254	5,915	993

Notes: This table shows heterogenous treatment effect on 3G' work hours around the application date τ. Sample: one 3G is one observation. Treatment *HCN*. Outcome is the change, compared to $\tau-1$, in total work hours per month. All controls are included. Panel A shows results for granddaughters (GD), Panel B for grandsons (GD). Column (1) looks at baseline estimates. Columns (2)-(3) split the 3G sample by wether they live with their parents in $\tau-1$. Columns (4)-(5) by family size at $\tau-1$: small families are such that 3G does not have cousins. Columns (6)-(7) by 3G's location with with respect to 1G in $\tau-1$. Columns (8)-(9) by 1G's SES in $\tau-1$. Monetary values are expressed in 2015 euro. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the CBS data.

Appendix 1.B Additional Details on Data and Variables

Below we provide additional information on data sources and variables used in the analysis. All datasets used are non-public microdata from Statistics Netherlands (Statistics Netherlands, 2020). Documentation for each of the files below can be found at the embedded link. Please note that these are only available in Dutch.

Personal background information, death and birth dates, and intergenerational linkages are combined using gbapersoontab, ogbaoverlijdentab, and kindoudertab. Linkages within households and information on the residence location come from gbahuishoudenbus, gbaadresobjectubus, and vslgwbtab. Labor market histories, wealth, and income data are extracted from secmbus, spolibus, vehtab,inpatab, inhatab. Long-term care eligibility are from indicawbztab, indicwlztab. Information on long-term care use are in zorgmvtab, gebzzvtab,gebwlztab, zvwwvptab, and gebwmotab. Information on specialist care are from mszsubtrajectentab.

Table 1.B.1 provides an overview of the definitions of the variables used in the analysis.

Table 1.B.1. Variables definitions.

Label	Definition
Panel A: LTC and Survival	
admitted	indicator for being eligible for nursing home care .
nursing home	indicator for going to a nursing home .
home care	indicator for using any kind of home care services.
Surv. years	number of years lived since the application date τ.
Surv. 1Y	indicator for being alive in $\tau + 1$.
Surv. 2Y	indicator for being alive in $\tau + 2$.
Surv. 3Y	indicator for being alive in $\tau + 3$.
Panel B: Labor Supply and Income	
months employed	months employed per year.
work hours	total work hours per month, average over one year .
irregular work hours	paid overtime and hours from secondary jobs per year.
gross wage	real gross wage per month, average over one year .
hourly wage	*gross wage* divided by *work hours*.
earnings	gross earnings per year.
income	gross income per year.
income (main source)	gross income from main source of personal income per year.
Panel C: Financial variables	
wealth	household's total assets minus liabilities; assets: bank and savings balances, securities, bond, shares, value of owner-occupied home, business assets, and value of other real estate; liabilities: mortgage debt on own home, study debts, and other debts, e.g. for consumption purposes, to finance acquisition of shares, bonds, real estate.
wealth w/o home	*wealth* diminished by the value of owner-occupied home and mortgage debt on it.
financial assets	value of household's deposits in accounts with (savings) banks, bonds, and shares, excluding shares of substantial interest (≥5% of a company's issued share capital).
bank assets	value of household's deposits in accounts with (savings) banks.
other assets	value of household's assets in the form of cash, movable property, trust assets, share in undivided estate, assets encumbered by usufruct or restricted property.
home value	value of household's owned dwelling used as main residence.
other real estate	value of household's owned property minus home value.
Panel D: Other variables (controls)	
female	indicator for being female.
adult children	indicators for having one, two, or three or more adult children.
daughter	indicator for having at least one daughter.
2G size	indicators for having one, two, or three or more siblings.
3G size	indicators for having one, two, or three or more cousins.
1G<5km	indicator for living less than 5.75km from 1G.
Partner	indicator for 2G having a cohabiting partner.
Children	indicator for 2G living with children.
Lives with parents	indicator for 3G living with their parents.
SES	household income and wealth, expressed in percentile of its distribution per year.

Appendix 1.C Additional Details on Care Needs

Section 1.3.1 splits the sample into low, medium, and high care need elders, based on the care services they receive. This are assigned when they apply to LTC, and are bundles of hours of nursing, personal, or other type of care. If they include residential care services, they are called *Zorgzwaartepakket* (*ZZP*, care package). The labels and definition of care packages change with the 2015 reform. We include here the detailed list of care packages included in each care need group before and after the reform. The exact content of each package can be found in the codebooks for indicawbztab, indicwlztab, and gebwmotab.

For applicants between January and December 2014, home and residential care services are assigned by the same institution under AWBZ legislation. We distinguish care needs as follows:

(1) low care need (LCN): not assigned to any ZZP.
(2) medium care need (MCN): assigned to ZZPs 20010, 20020, 20030, 20040, 30270, 30290, 30310, 30330, 30430, 30440,30450,30490, 30510, 30530, 30550, 30630, 30650, 30670, 30730, 30750, 40810, 40830, 40850, 40950, 40970, 40990.
(3) high care need (HCN): 20050, 20060, 20070, 20080, 20090, 20091, 20092, 200100, 30310, 30330, 30350, 30370, 30390, 30410, 30460, 30470, 30480, 30550, 30570, 30590, 30610, 30690, 30710, 30770, 30790, 40870, 40890, 40910, 40930, 41010, 41030, 41050.

For applicants between January and December 2015, residential care services are assigned by a centralized institution under WLZ legislation, while home care services are assigned by municipalities under WMO legislation. We distinguish care needs as follows:

(1) low care need (LCN): receiving WMO support packages with codes 007, or 100 through 107.
(2) medium care need (MCN): applied and rejected from residential care, or receiving WMO support packages (MWV) with code above 300.
(3) high care need (HCN): eligible for residential care.

Appendix 1.D Health Information

The health information we use rely on data on received specialist medical care, included in mszsubtrajectentab. We use them to extrapolate information on the market price of the specialist care received one year before the application to LTC (variable MSZSTRVerkoopprijsDBC), to compute the Charsol Index, or to use medical diagnosis-treatment specialty categories to predict care needs. See section Section 1.3.1 for details on the use of each variable.

The Charsol Comorbidity Index, proposed by Charlson et al. (1987), and more recently updated by Quan et al. (2011) and Radovanovic et al. (2014), is a widely used method in the medical literature to classify medical conditions that might alter the risk of mortality. We compute it for sample 1G, based on the evidence provided by diagnosis-treatment sub-trajectory pathways of received specialist care (variable MSZSTRSpecialismeDiagnoseCombinatie). The computation goes as follows. First, we add one point for each pathway addressing: myocardial infarction; CHF, i.e. exertional or paroxysmal nocturnal dyspnea, responsive to digitalis, diuretics, or afterload reducing agents; peripheral vascular disease; CVA or TIA, i.e. history of a cerebrovascular accident with minor or no residua and transient ischemic attacks; dementia; chronic pulmonary disease; connective tissue disease; peptic ulcer disease. Second, we add: for history of liver diseases, one point if mild or three if moderate to severe[18]; for history of diabetes mellitus, one point if uncomplicated, two points if leading to end-organ damage. Third, we add two points for each pathway addressing: hemiplegia, moderate to severe CKD[19]; leukemia; lymphoma; localized solid tumor. Fourth, we add six points for metastatic solid tumors and AIDS. Finally, we add one point for being 70-79 years old, and 2 points for being above 80.

As an alternative to the use of the Charsol Index to predict care needs, we use fixed effects for categories of medical specialist diagnosis, interacted with gender. We consider the following diagnosis categories, as recorded in the mszsubtrajectentab (variable MSZSTRBehandelendSpecialisme): ophthalmology; ear, nose an throat surgery; surgery; orthopedics; urology; neurosurgery; internal medicine; gastroenterology; cardiology; pulmonary diseases; rheumatology; rehabilitation; cardio-thoracic surgery; psychiatry; neurology; geriatrics; radiotherapy; radiology; audiology centers; specialist in geriatrics. We exclude the following categories, as unrelated to elders' care needs: plastic surgery; obstetrics and gynecology; dermatology; pediatrics; allergology; anaesthesiology, clinical genetics.

[18] Severe = cirrhosis and portal hypertension (ph) with variceal bleeding (vb) history; moderate = cirrhosis and ph without vb history; mild = chronic hepatitis or cirrhosis without ph.

[19] Severe = on dialysis, post kidney transplant, uremia; moderate = creatinine >3 mg/dL.

Chapter 2

Live Longer and Healthier: Impact of Pension Income for Low-Income Retirees

Joint with Han Ye

2.1 Introduction

Old-age poverty has become an important policy concern in light of diminishing public pension generosity and increased longevity (Sarfati, 2017; Börsch-Supan and Coile, 2018). In particular, the trend of transitioning from a defined benefit to a defined contribution pension system has left a growing number of lower-income workers vulnerable to old-age poverty (ILO, 2014). Many governments have provided safety nets for pensioners with low benefits, however, relatively little is known about how pension income affects mortality and health[1], although this is an important indicator of the social value of old-age income support programs. Moreover, whether people live longer and healthier lives due to additional pension income can also help understand the persistent and widening socioeconomic disparities in old-age mortality in many developed countries (Currie and Schwandt, 2016; Wenau, Grigoriev, and Shkolnikov, 2019; Haan, Kemptner, and Lüthen, 2020), although mortality has improved for the population as a whole. Therefore, the answer to this question can have considerable policy relevance, as old-age poverty is a growing and pervasive problem around the world.

[1] There is a small but growing literature studies the labor supply responses to the generosity of public pension (e.g., Stock and Wise (1990), Krueger and Pischke (1992), Snyder and Evans (2006), Gelber, Isen, and Song (2017), and Ye (2022)).

This question remains understudied, in part due to the difficulty of isolating exogenous variations in the parameters of the public pension system, such as benefit levels, pension eligibility age, penalties for claiming pensions early, etc. Existing papers on the mortality response to pension reforms mostly focus on reforms that raised or lowered the pension eligibility age (see, e.g., Hernaes et al. (2013), Shai (2018), and Belles, Jiménez, and Ye (2022)) or reforms that bundle changes in parameters (see, e.g., Bozio, Garrouste, and Perdrix (2021) and Saporta-Eksten, Shurtz, and Weisburd (2021)).

In this paper, we investigate how a permanent increase in pension income affects the mortality and health outcomes of low-income pensioners by examining a German pension subsidy program. Several features of this program make it an ideal natural experiment to study the effects of additional pension income. First, the subsidy is determined on the basis of contributions made before the announcement of the program. Second, individuals are eligible for the subsidy only if they fulfill two conditions at retirement: at least 35 contribution years and average monthly earnings points from full-value contribution years below a certain threshold. These eligibility criteria allow us to estimate the causal impact of additional pension income. Third, the additional benefits from this subsidy program occur without any changes to other pension system parameters, such as the statutory retirement age. This enables us to isolate the causal impact of additional pension income from other characteristics of pension systems. Also, as Germany has universal health care, the implications of pension income is not tied to access to (subsidized) health care, as in the U.S. for example (Ayyagari, 2019). This feature allows us to decouple the effect of additional income on mortality from the effect of losing access to health care. Fourth, enrolment is automatic, as the subsidy is added directly to the pension of eligible individuals without any application process. This ensures that the subsidy reaches those who might not have enrolled due to incomplete information or transaction costs (Bertrand, Mullainathan, and Shafir, 2004; Finkelstein and Notowidigdo, 2019), whom are often the people most in need of support.

Our analysis is based on novel administrative data covering the universe of German pensioners who died between 1994 and 2018. The baseline sample consists of West German old-age pensioners who were born between 1932 and 1942. Using a difference-in-differences (DID) method, we find that eligibility for the pension subsidy increases pension income by 57.9€/month (around an 8% increase).[2]

After establishing a sizable impact on pension income, we turn to the impact on mortality. We find that eligibility for the pension subsidy improves age at death

[2] All monetary values are CPI adjusted and expressed in 2015 euros.

(censored at age 75) by 1.6 months (around a 0.2% increase). Specifically, eligibility reduces the probability of dying before age 65, and 70 by 0.8 percentage points (14.5%), 1.5 percentage points (5.9%), respectively. We, therefore, estimate intent-to-treat pension income-mortality elasticities of -1.8, -0.72. We find no significant effects on the age at claiming pension. Despite the fact that men receive a smaller subsidy on average, the heterogeneity analysis suggest that the mortality responses are mainly driven by men. The estimates are robust to several robustness tests that vary the sample restrictions and the set of controls. Moreover, we verify that there are no mortality effects when using placebo eligibility conditions in ineligible samples. To better quantify and scale the effects, and investigate the importance of pension income on mortality and health, we also employ a instrumental variable method. In particular, we use the two eligibility criteria as instruments for the pension income. We quantify that a permanent increase in monthly pension income of 100€ (about a 14% increase) increases the age at death (censored at age 75) by around 2.8 months.

To better understand the mechanisms, we examine the responses in health outcomes using the SHARE-RV dataset, which links information from the *Survey on Health and Retirement in Europe* with active pension records from the *German Pension Register*. The survey sample contains a similar population of pensioners to the administrative sample. However, the questions were asked when they were alive. We find that increases in pension income improve both mental and physical health. For example, we find that additional pension income leads to reductions in depression, the number of chronic diseases, the incidence of chronic lung disease and high blood pressure, and difficulties with activities of daily living. In addition, feeling less financially constrained and feeling more optimistic about the future appear to be relevant drivers of improved health. We also find a reduction in both alcohol and cigarette consumption among men, which may be related to a reduction in stress. Again, we find stronger effects for men than for women.

The policy implication of our findings is that the pension subsidy for low-income workers in Germany have beneficial effects on life expectancy and health. In particular, male recipients live longer and healthier lives. We show that a stable increase in cash flow during retirement, despite being a relatively small amount, can have substantial improvement on health and life expectancy for poor retirees in a developed country with a universal healthcare system. Additional pension income can make individuals feel less stressed, less financially constrained and reduce their alcohol and cigarette consumption, which improve quality of life and ultimately decrease mortality. The cost-benefit analysis suggests that this program is a cost-effective policy to increase the life expectancy of pensioners. The monetary benefits of the life expectancy gain of 100€ additional pension income per month is around 183,785€ for male recipients. Finally, a simple back-of-the-envelope calculation suggests that a subsidy, targeted at people with low pension entitlements, would help to flatten the income-mortality gradient and reduce the gap in life expectancy at age 65 between the top and bottom income deciles in Germany by 3%.

We contribute to the relatively small but growing literature on the causal impact of pension income on mortality. Most of the evidence is for developing countries (Case, 2004; Jensen and Richter, 2004; Barham and Rowberry, 2013; Huang and Zhang, 2021; Miglino et al., 2023) by exploring either non-contributory pension programs or conditional cash transfer programs. For example, Miglino et al. (2023) study the effect of income on mortality by exploring the eligibility condition for the non-contributory pension program in Chile. They find the basic pension increases income by 72% and reduces four-year mortality by 28%. Huang and Zhang (2021) examine the implementation of China's New Rural Pension Scheme, which targeted at vulnerable elderly in rural areas. They find that the pension scheme increased the household income by 18% and result in a reduction in one-year mortality by 2.2 percentage points. Consistent with our findings, they find that addition pension income saves lives.

However, the implication of additional pension income in developed countries might not necessary apply to developed countries. To the best of our knowledge, there are only two papers studying the impact of pension income on mortality and health in the context of developed countries (Snyder and Evans, 2006; Johnsen and Willén, 2022). While Johnsen and Willén (2022) show that negative shocks to pension income had no impact on both employment and health care utilisation of pensioners in Sweden, Snyder and Evans (2006) find that lower pension income leads to reduced mortality by examining a cut in social security wealth for the U.S. “notch" cohort. However, the effects of higher and lower pension income on mortality are not necessarily symmetric. These estimates may not be generalizable to policies aimed at ensuring income support for older people at risk of poverty. In fact, in contrast to their findings, we show that higher pension income leads to lower mortality. Another important distinction is the indirect employment response. Pension income differs from other types of income in that it could affect mortality directly by improving physical and mental health and indirectly by influencing retirement choices. While higher income typically improves life expectancy, it can also induce earlier retirement, thereby increasing mortality (e.g., Fitzpatrick and Moore, 2018; Kuhn et al., 2020) or decreasing mortality (e.g., Hernaes et al., 2013; Hagen, 2018; Belles, Jiménez, and Ye, 2022), depending on the sub-population affected. The employment effect may offset or amplify the wealth effect on mortality. For example, the reduced mortality due to lower pension income in Snyder and Evans (2006) is explained by the beneficial effects of employment. Our paper studies a pension subsidy program that has a relatively small effect on retirement age (Ye, 2022), which helps to pinpoint a pure wealth effect of additional pension income on mortality.

Moreover, our paper links to the broader literature examining the impact of income on mortality and health outcomes for older people by examining other social insurance programs (e.g., Bailey and Goodman-Bacon, 2015; Eli, 2015; Gelber et al., 2023; Becker et al., 2024; Black et al., 2024).[3] In particular, Gelber et al. (2023) study the impact of more generous Disability Insurance benefits on mortality for low-income DI beneficiaries, who are vulnerable population similar as in our setting. They show that $1,000 more in annual disability insurance payment in the U.S. reduces mortality of low-income beneficiaries by 0.18 to 0.35 percentage points.

Previous studies have also investigated the pure wealth effect on mortality and health by exploring financial shocks, such as lotteries (e.g., Lindahl, 2005; Cesarini et al., 2016; Lindqvist, Östling, and Cesarini, 2020) and stock market fluctuations (e.g., McInerney, Mellor, and Nicholas, 2013; Schwandt, 2018). Our paper differs from these studies in two important aspects: the population studied and the income variation. We focus on low-income pensioners, the population most affected by recent pension reforms. In addition, we examine a permanent increase in pension benefits, which provides a steady higher income stream, as opposed to a one-off windfall or transitory income fluctuations.

The rest of the paper proceeds as follows. Section 2.2 explains the main elements of the German Pension System and of the subsidy program. Section 2.3 describes the data and Section 2.4 delineates the empirical strategies. Section 2.5 reports the DID results, IV estimates and also provides some evidence on the mechanisms driving our results. Finally, Section 2.6 discusses and Section 2.7 concludes.

2.2 Institutional Setting

German Public Pension System The German Public Pension System is an earnings-related points system financed on a pay-as-you-go basis.[4] Participation is mandatory,

[3] A large literature examine the health and mortality effects of income by examining transfer program and social insurance such as cash transfers(e.g., Aizer et al., 2016; Aizer, Eli, and Lleras-Muney, 2020), the Earned Income Tax Credit (e.g., Evans and Garthwaite, 2014; Dow et al., 2020), health insurance (e.g., Bitler, Gelbach, and Hoynes, 2005; Ziebarth, 2018).

[4] The pension system is mainly financed via mandatory contribution payments, which are normally shared equally by employers and employees. In 2021, the total mandatory contribution rate was 18.6%.

except for civil servants and the self-employed. On average, the public pension replaces around 50% of pre-retirement wage, net of income, and payroll tax. As of the end of 2021, the average monthly pension benefit of the insured was around 1,163 euros for men and 860 euros for women.

The statutory retirement age for a regular old-age pension remained 65 years old for the cohorts in our baseline sample; the only prerequisite being at least five years of contributions. Several alternate pathways make retiring before 65 years of age possible.[5] For example, eligible workers born before 1946 can claim their pension at the earliest via the old-age pension due to unemployment, at age 60. Women have another option to claim the pension as early as age 60 via the old-age pension for women. Almost all female recipients of the subsidy program born before 1952 are eligible for this pathway.[6]

In Germany, pension benefit levels are closely tied to lifetime wages. The main determinant of pension benefits is the sum of the individually accumulated earnings points (Entgeltpunkte, (EP)). Essentially, for each year of contributions, a worker accumulates some earnings points, which are determined by the individual wage in that year relative to the average wage of all the insured. For example, a worker whose wage is half of the average wage will accumulate 0.5 points in that year.[7] Aside from a few exceptions, workers with few contribution years or low relative wages are more likely to face old-age poverty. This is one of the reasons that the majority of the subsidy recipients are women, as they have short employment periods and a lower wage over their life cycle. Pensioners can work while claiming their pensions, however, they face a stringent earnings test.

Pension Subsidies for Low-wage Workers The pension subsidy program studied in this paper was introduced during the German pension reform in 1992.[8] The primary policy consideration of this subsidy program is to ensure adequate old-age income, which credits additional earnings points to eligible individuals. The target recipients are workers with low lifetime pension contributions.[9]

[5] Starting from 2012, the statutory retirement age for cohorts born after 1947 began increasing from 65, and this will reach age 67 for cohorts born after 1964. There are four main early retirement pathways: old-age pensions for long-term insured, old-age pensions for women, old-age pensions due to unemployment (and, later, part-time work); and old-age pensions for severely disabled persons Börsch-Supan, Wilke, et al. (2004).

[6] The eligibility requirements for the women's pension pathway were: 1) at least 15-years of pension insurance contributions; and 2) at least 10 of the 15 years of pension insurance contributions need to have been acquired after age 40.

[7] See Appendix 2.B.1 for more details on the pension benefit calculation.

[8] See Appendix 2.B.2 for a summary of other reforms implemented in 1992.

[9] The German name of this subsidy program is "Mindestentgeltpunkte bei geringem Arbeitsentgelt". See German Social Law, vol. 6 clause 262 (SGB VI § 262) for the exact definition.

This subsidy program ensures an adequate pension for people with two characteristics: individuals with a long pension contribution history and workers with low wages. Specifically, individuals need to fulfill two criteria to become eligible for this subsidy program. First, a worker should have at least 35 contribution years. Second, the average monthly EPs from full-value contribution years at the time of retirement are below 0.75. This criterion means that only individuals in the bottom 37.5 percentile of the income distribution at the time of retirement are eligible. It guarantees that workers are not only poor before 1992 but also at the time of retirement.[10] According to the statistics from the Research Data Center of the German Pension Insurance, in December 2015, 14% of old-age pensioners — 4% of all male pensioners and 26% of all female pensioners — were recipients of this subsidy program.

Eligible pensioners do not need to apply for this subsidy. The amount is computed by applying a built-in formula and is added directly to the recipients' pension account by the pension office. The subsidy size is predetermined. The determinants of subsidy size are total contributions made before 1992 and the average relative wage (average earning points) prior to 1992 (aep^{92}). The subsidy size has a kinked relationship with pre-1992 average earning points. [11] Recipients receive, on average, around 85 euro per month in our baseline sample, which corresponds to an increase in pension income of 11%. In 2015, the total payments for this subsidy program were approximately 3 billion euros.

[10] Full value contribution periods are typically periods with gainful employment. See Online Appendix 2.B.4 for more details of the composition of creditable years, contribution periods and consideration periods.

[11] In particular, subsidy size is determined as:

$$Subsidy_i = min\left(0.5 \times \sum_{\tau<92} EP_{i\tau}, 0.75 \times T_i^{92} - \sum_{\tau<92} EP_{i\tau}\right) \quad where\ EP_{i\tau} = \frac{\omega_{i\tau}}{\overline{\omega}_\tau} \tag{2.1}$$

where, for each individual i and each year of contribution τ, $EP_{i\tau}$ indicates accumulated earnings points, T_i^{92} indicates years contributed before 1992, $\omega_{i\tau}$ indicates earned wage τ and $\overline{\omega}_\tau$ indicates (West or East) German average wage in τ. The formula implies the subsidy size has a kinked relationship with pre-1992 average earning points, and Figure 2.A.1 depicts this relationship in the case of an individual who contributed 19 years to the pension system prior to 1992. Once subsidy size in terms of EPs is determined, this is added to the accumulated lifetime EPs of individual i, which are then used to compute pension income. See Ye (2022) for more details and Appendix 2.B.3 for examples illustrating the calculation of the subsidy amounts.

2.3 Data

2.3.1 Main Data and Sample

The analysis is based on a novel administrative dataset covering the universe of retirees who left the German public pension system between 1994 and 2018, provided by the German State Pension Fund (FDZ-RV). The dataset is a non-public version of the Discontinued Pension Records (RTWF, Rentenwegfälle), which contains the universe of individuals who were active in the German public pension system at some point in their lives (workers and pensioners) and who left the pension system (mostly due to death) at the time of data collection. The main dataset is assembled from 24 years of cross-sectional waves (1994 to 2018). The dataset includes time-invariant information (such as accumulative pension points, gender, birth month, number of children, and age at claiming pension), at the time when they fall out of the pension system. We refer to this sample as the RTWF sample throughout the paper.

Several important advantages of the data are worth noting. First, this data contains accurate information on average pension points from full-value contribution and contribution years, which are necessary for us to determine the treatment status. Moreover, the data provides an accurate measure of the amount of pension subsidy and pension income, which are crucial for testing the relevance of the instruments. Third, we observe the exact dates of their birth and death and the universe of the German inactive pension accounts, which help us measure the mortality responses accurately. Lastly, our data includes information on an individual's marital status, which was specially provided by the German pension data center. Unfortunately, other potentially useful information is lacking due to the cross-sectional nature of the data set; for example, biographical information such as pension points accumulated before 1992.[12] In addition, occupation is not accurately measured and therefore cannot be used.

For our baseline sample, we restrict the analysis to those individuals who left the pension system due to death. We further restrict to German nationals residing in West Germany. By doing so, we abstract from migration patterns and German reunification effects. Moreover, East Germans face different pension rules that are not comparable to those who worked in West Germany. We keep retirees who claimed old-age pension because the pension subsidy is a part of the old-age pension benefit. We restrict the sample to cohorts born between 1932 and 1942. The lower bound

[12] Because we do not observe the pension points accumulated before 1992, we cannot perform a regression kink design as in Ye (2022), which using aep_{92} as a running variable.

(1932) is chosen to include individuals who could potentially retire after 1992 (age 60 in 1992) after the introduction of the reform. The upper bound (1942) is chosen to include people who are at least 75 years old in 2018 to have an uncensored measure of probability of dying. Finally, we keep those who contributed within the bandwidth of 24 months around the 35-year contribution eligibility, and those with *aep* between 0.45 and 1.05 (approx. between 1200 and 3000 euro of gross monthly wage). In the robustness analysis, we vary the choice of sample selection around the *aep* and the contribution year eligibility conditions. Our results remain the same. The final sample contains 149,053 individuals, of whom 65% are women, and 35% satisfy both conditions with *aep* below 0.75 and more than 35 contribution years.[13]

Table 2.A.1 reports the summary statistics for West Germans who claimed an old-age pension between 1994 and 2018 (*West German pensioners*), for those born between 1932 and 1942 in the *West German pensioners* sample (*1932-1942 sample*), and finally for our *baseline sample*, i.e. with the 33-36 contribution years and 0.45-1.05 *aep* restrictions. In the baseline sample, age at death (censored at age 75) is around 72.2.[14] Their average probability of dying before age 65, 70, and 75 are 5%, 25%, and 51% , respectively. They have a pension income of 706€/month, and become a subsidy recipient with a 30% likelihood. Conditional on being a recipient, they receive a subsidy of about 85€/month. Of the baseline sample, 35% are male, and 60% are married. Female pensioners have on average 2.2 children. On average, these pensioners claim their current pension at age 63. The baseline sample is comparable to the West German Pensioners and 1932-1942 samples, except for the share of women. The baseline sample is 65% female, while the *West German Pensioners* and *1932-1942* samples have 42% and 39% women, respectively. This is likely due to women having lower wages, thus lower *aep*, and women are more likely to have contribution years between 33 and 36 as they are granted a generous amount of contribution years devoted to childcare (Table 2.A.2). Table 2.A.2 shows the characteristics of pensioners in our sample by gender. Overall, men are more likely to die before the age of 70, with a probability of 31 % compared to

[13] The majority of the subsidy recipients are female workers. Out of all treated individuals in our baseline sample, 79.5% are women. The higher share of women can be explained by two characteristics of women: lower wages and more child-raising periods. On the one hand women, on average, have lower wages than men; therefore, their *aep* is more likely to be below 0.75. On the other hand, because child-raising periods count as contribution years, it is relatively easier for women to reach the 35 contribution years cutoff. In particular, the time of raising a child up to age 10 counts in the consideration period. The package is 10 years for one child, 15 years for two children and 20 years for more than two children.

[14] As we only observe deaths that occurred between 1994 and 2018, our baseline cohorts were at least 75 years old in 2018. For this reason, we examine the impact on the probability of dying before age 75 and age at death censored at 75 throughout the paper. When we refer to age at death in the following, we refer to age at death censored at age 75.

22% for women. Age at death (censored at age 75) is around 72.5 for women, and 71.7 for men. Men receive about 829€/month of pension income, while women receive 640€/month. Women are also less likely overall to have more than 35 years of contributions, while they are more likely to have *aep* below 0.75.

We verify that being in the sample is not affected by eligibility conditions. Table 2.A.3 shows that the impact of eligibility conditions on being included in the baseline sample (column 1) and by gender (columns 2 and 3). We find no significant effect on being selected into our sample.[15]

2.3.2 Survey Data on Health Outcomes

To provide suggestive evidence on the impact of additional pension income on health and to better understand the mechanisms behind the reduction in mortality, we examine an auxiliary dataset: SHARE-RV. This dataset links the German subsample of the Survey on Health Ageing and Retirement in Europe (SHARE) with administrative pension records provided by FDZ-RV.[16] SHARE collects data on a representative sample of individuals aged 50 and over. We take the following waves: wave 1 (interview years 2004 and 2005), wave 2 (2006 and 2007), wave 4 (2011 and 2012), wave 5 (2013), wave 6 (2015), and wave 7 (2017).[17]

Our SHARE-RV sample contains West German old-age pensioners who were born after 1931.[18] To ensure a reasonable sample size for the analysis, we take a larger bandwidth around the 35-year contribution eligibility and the 0.75 *aep* cutoff than the RTWF sample. The SHARE-RV sample contains people who contributed between 15 and 55 years and had an *aep* between 0.25 and 1.25. We end up with 2,328 observations, of which 44% are women and 37% are eligible for the subsidy.

The SHARE-RV sample allows us to gain insights into how health conditions, financial constraints, and psychological feelings are affected by additional pension

[15] In Table 2.A.16, we further show robustness by varying the cohort restrictions and varying the *aep* bandwidth choice. Because age at death is censored at age 75, we also show that the probability of dying before 60 and the probability of dying after 75 are not affected by the eligibility conditions for the pension subsidy by using younger and older cohorts (Table 2.A.4). See Appendix 2.C for further discussion.

[16] Specifically, SHARE-RV links SHARE with *Versichertenkostenstichtprobe* (VSKT) and *Versichertenrentenbestand* (RTBN). VSKT is a longitudinal dataset and contains monthly information on respondents' employment histories. RTBN is a cross-sectional dataset that summarizes respondents' benefits accumulated during retirement and information on the amount of paid pensions. SHARE-RV is based on direct linkage, meaning that the records of the same SHARE respondents were linked using the respondents' social security number as a unique identifier. See SHARE-RV website and Börsch-Supan et al. (2020) for more information on SHARE-RV.

[17] See SHARE website for further information on SHARE. We do not use wave 3 because it is a retrospective survey and has a different structure from the other waves.

[18] We do not set an upper bound (1942) as we did in the mortality data sample, because health variables are not subject to censor biases and include more cohorts increase the sample size.

income. In particular, we consider the following overall health measures: an indicator of overall well-being (*CASP*), a self-reported indicator of health, the number of diagnosed chronic diseases, and a measure of depression symptoms. Moreover, we use a set of variables measuring physiological feelings. We use "how often the individual felt money stopped them from participating in generally defined activities" as an indicator for perceived financial constraints. We also exploit self-reported measures of optimism, particularly measuring how often the individuals feel their life is full of opportunities and how often they feel that their future looks good. Table 2.A.5 gives an overview of how these variables are constructed and their scale.

Table 2.A.6 shows that the baseline SHARE-RV sample is generally comparable to the West German Pensioners sample, except for the amount of pension income without subsidy, being slightly unhealthier and having fewer pension income.[19] Table 2.A.7 further shows the characteristics of pensioners in the baseline survey sample by gender. The pattern is similar as to the administrative data set. Women are overall healthier, except for the depression index. Women's pension income is on average a smaller share of the household income.

2.4 Empirical Strategy

Estimating the causal effects of income on mortality is challenging because of the endogeneity of income. Unobserved factors might affect both pension income and mortality. This paper exploits the eligibility conditions for an exogenous pension subsidy program to estimate the causal effect of pension income on mortality. First, we study the intent-to-treat effect of the pension subsidy program on mortality using a Difference-in-Differences (DID) method. Second, we use an instrumental variable (IV) approach to report the causal effect of pension income on mortality and health outcomes.

2.4.1 Difference-in-Differences Method

We use the two eligibility criteria of the subsidy program to obtain a DID estimate. The first difference is having *aep* at retirement below 0.75, and the second is having more than 35 years of contributions. We measure the change in the differences between treatment and control group before and after 35 contribution years. The treatment group consists of individuals with *aep* at retirement below 0.75. The con-

[19] We also compare the baseline sample with a restricted sample if we impose the same restrictions as in the mortality data sample. Note that the sample size drops to 205 when we make this restriction. They are generally comparable too.

trol group consists of individuals with *aep* at retirement above 0.75. Table 2.A.2 reports the summary statistics for the treatment and control groups for men and women, respectively. The two groups present similar characteristics except for the control groups have higher pension benefits without subsidy. The average amount of pension benefits without subsidy differ by approximately 236€ per month for women and 318€ per month for men between the two groups.

Theoretically, individuals with *aep* below 0.75 and more than 35 contribution years could still receive no subsidy if their wages were high before 1992 (aep_{92} being higher than 0.75 renders the amount of subsidy zero). Because the RTWF data does not provide information on average earning points before 1992, our DID estimator measures an Intent-To-Treat (ITT) effect. In practice, most individuals who fulfill the two conditions receive a positive subsidy for two reasons. First, eligible individuals do not need to apply for the subsidy. The pension office automatically adds the amount to their pension account. Second, 81% of pensioners fulfill the eligible conditions received a positive amount of subsidy (Table 2.A.14).

The estimation equation for the DID design is the following:

$$Y_i \;=\; \alpha + \theta D_i \times Above35_i + \delta_1 D_i + \beta X_i + \tau + \lambda + \epsilon_i \tag{2.2}$$

Y_i represents the outcome variable of individual i with *aep*. The treatment indicator D_i is defined as $D = \mathbb{1}(aep_i < 0.75)$. τ indicates contribution years fixed effects. $Above35_i$ is a dummy that takes the value one for individuals with 35 or more contribution years, and zero for those with less than 35 contribution years. θ measures the reduced-form effect of being eligible for pension subsidy on pension income and mortality.

X_i contains the demographic characteristics, such as a male indicator, being married, not having any health insurance, having children[20] and pension benefits without subsidy. λ is the birth cohort fixed effect. τ is the contribution year fixed effect. The standard errors are clustered at the birth year level [21]. Because we have a small number of clusters, we also report the bootstrap p-values in brackets in all tables.

The DID analysis identifies the impact of additional pension income under the assumption that, had the subsidy program do not exist, the evolution of mortality over the contribution year for people with *aep* between 0.45 and 0.75 (pension

[20] This variable is based on whether the individual has claimed child benefit. As it is usually the women who do this, this variable is a poor measure of the number of children for men. Instead, for men this variable is a proxy for being a man who is more involved in caring for children at home.

[21] It is crucial to include cohort fixed effects because there has been a series of pension reforms in Germany during the sample periods. The cohort fixed effects account for the incentive changes caused by raising the statutory retirement age, which was implemented gradually by cohorts.

benefits between 500 and 850€/month) would have been similar to trends in outcomes for pensioners with *aep* between 0.75 and 1.05 (pension benefits between 700 and 1200€/month). Because we look at people very close to the 0.75 earnings threshold and compare them across a narrow window of two years around the 35 contribution years, it is more plausible that our identification assumption will be fulfilled. Moreover, the event-study analysis in section 2.5.2 provides support for the common-trend assumption. Moreover, the placebo analysis using placebo samples consist of people of aep_i higher than 0.75 in section 2.5.4 further support causal interpretation of our estimates.

Manipulation into Treatment. If the existence of the subsidy and the knowledge of its eligibility conditions were to induce individuals to manipulate either one of the parameters determining eligibility, our estimates would be biased. In the following, we show that such manipulation is rather unlikely and not supported by empirical evidence.

First of all, because the subsidy amount is computed from aep^{92}, which in turn is fully determined by full-value contribution periods and wages prior to 1992, the subsidy size is as good as exogenous. To receive a positive subsidy amount, one must in practice fulfill three conditions: (1) have more than 35 years of contributions, (2) have $aep < 0.75$, and (3) have $aep^{92} < 0.75$ (otherwise the amount of subsidy is zero). Since aep^{92} cannot be manipulated, selective behaviour could only come from manipulating *aep* or changing labor supply decisions.

Second, we discuss the possibility of selection into the *aep* condition. After 1992, those with aep^{92} below 0.75 might closely monitor their *aep* to ensure they do not lose the subsidy entitlement. In practice, *aep* is highly correlated with aep^{92} (Ye, 2022). The higher the number of contribution periods before 1992, the closer will aep^{92} be to *aep*, i.e. average earning points at retirement. Consequently, the only plausible instance in which manipulation might be profitable is for somebody with aep^{92} below but close to 0.75 and *aep* above but close to 0.75. Only a small share of pensioners fall into this group.[22] Moreover, the kinked subsidy schedule suggests that such an individual would receive a relatively low monthly subsidy, approximately lower than 20€/month. That is, the monthly subsidy would be less than 4% of their pension income and less than 2% of their pre-retirement wage. This makes it unlikely to be profitable for people to lower their wages to manipulate subsidy eligibility. It is also worth noting that for manipulation to be possible, people would

[22] Using the VSKT sample, we find that only 6% of pensioners with *aep* at retirement higher than 0.75 have aep^{92} lower than 0.75.

need to know about the subsidy well in advance and fully understand the complicated formula by which the subsidy is allocated and calculated, which is likely to be a strong requirement. Finally, if individuals were to accept lower wages at the end of their careers in order to qualify for the subsidy, we should observe bunching of individuals with more than 35 years of contributions around the 0.75 *aep* cutoff. Figure 2.A.2 shows the density of *aep* distinguishing between those with more (red bars) and less (blue bars) than 35 years of contributions in the baseline sample (panel (a)) and the differences between these two densities (panel (b), above minus below 35 group). Panels (c) and (d) show the distribution for women and men. Overall we observe a rather smooth density around the cutoff for both groups and, if anything, a higher concentration of individuals with less than 35 years of contributions at $aep = 0.75$. Therefore, we rule out the possibility of strategic behaviour around the 0.75 cutoff.

Finally, we discuss the possibility of selection into the 35 years of contributions condition. Individuals with aep^{92} and *aep* below 0.75 might be tempted to postpone retirement and reach 35 years of contributions in order to receive the subsidy. If this were the case, we would observe bunching at 35 contribution years in the density of individuals with $aep < 0.75$ in our baseline sample. Figure 2.A.3 plots the distribution of contribution periods by *aep* group for the baseline sample (panel (a)) and the difference in densities between the two groups (panel (b), below 0.75 minus above 0.75). Although we observe bunching at 35 years of contributions, this sharp bunching exists for both groups and similar for both gender. One possible explanation is that 35 years of contributions is also the eligibility requirement for the old-age pension for the long-term insured. The long-term insured path allows people to retire at 63 instead of waiting until the statutory retirement age.

Figure 2.A.3 (b) shows that the $aep < 0.75$ group bunch more than the $aep \geq 0.75$ group. This could be problematic. However, when we further examine the distribution of contribution years, we can see that compared with the $aep \geq 0.75$ group, the excess mass for the $aep < 0.75$ group seems to primarily come from people at the top of the distribution. Figure 2.A.3 (b) shows that while the $aep \leq 0.75$ group has a large degree of bunching, this group also has a smaller mass between 35 and 37 years of contributions, compared with the $aep < 0.75$ group. This suggests that relatively poorer individuals are retiring earlier (reducing the years of contributions) than they would have otherwise. This is most likely driven by the incentives to retire via the long-term pension once they reach 35 years of contributions threshold. Panels (c) and (d) show the distribution for women and men, and we can see that this shift is slightly more pronounced for men, who are more likely to use the old-age for the long-term insured. This seems reasonable in light of the intuition that poorer

people are more likely to be blue-collar workers with a more physically demanding job, which may give them a greater incentive to retire as early as possible. In the robustness test, we show that our results remain unchanged when we drop those retire exactly at 35 years of contribution (Table 2.A.16).

2.4.2 Instrumental Variables Strategy

The purpose of the instrumental variable approach is twofold. First, it helps us to investigate the broader question: what is the effect of pension income on mortality? Second, it facilitates the investigation of health outcomes. Because of the sample size limitations of the SHARE-RV data, we need to rely on the IV analysis to explore the health consequences of having more pension income. We use the interaction between the two subsidy eligibility conditions as an instrument for pension income (PB_i). The first-stage and second-stage equations are as follows:

$$PB_i = \gamma_0 + \gamma_1(D_i \times Above35_i) + \gamma_2 Above35_i + \gamma_3 D_i + \beta X_i + \lambda + \tau + \mu_i \qquad (2.3)$$

$$Y_i = \pi_0 + \pi_1 \widehat{PB}_i + \pi_2 Above35_i + \pi_3 D_i + \theta X_i + \lambda + \tau + \epsilon_i \qquad (2.4)$$

PB_i indicates the amount of total pension income received by individual i. γ_1 measures the average treatment effect of the eligibility conditions on pension income. If the RTWF sample is used, X contains the demographic characteristics, including gender, being married, having children, not having health insurance, pension income without subsidy, receiving an unemployment pension, receiving a women's pension, receiving disability pension. When using the SHARE-RV sample, X contains gender, being married, having children, being in contact with at least one of their children at least once a week, an indicator for their children being employed, pension income without subsidy, years of schooling, and socioeconomic status before retirement[23]. We also control for age at claiming pension, the contribution years fixed effect τ and birth cohort fixed effect λ. Using the predicted value of pension income ($\widehat{PB}_i$), we obtain the causal effect of pension income on mortality or health outcomes (π_1).

There are three conditions necessary to interpret the two-stage least squares IV estimates. First, the interaction of these two eligibility conditions is independent of

[23] SES before retirement is measured using the following variables: no information, unpaid care or incapacity to work or illness, unemployed or marginally employed, gainfully employed and obligated to pay social insurance, other as supplementary period, pension provision from own insurance.

unobserved characteristics that affect pension income and mortality. Further, pension income must be strongly associated with the two eligibility conditions. The DID results in Section 2.5.1 confirm the exogeneity and relevance of the instruments.

Second, the exclusion restriction requires that the interaction of the two eligibility conditions affect mortality outcomes only through changes in pension income. One concern would be the indirect impact of pension subsidy program on age at claiming pension. We have shown that eligibility for the subsidy has economically small impacts on retirement choices. Nonetheless, by controlling for age at claiming pension in our regressions, we address this concern on second-order effects of the subsidy program. Throughout the paper, we always show the IV estimates with and without controlling for age at claiming pension. The results are similar.

Third, the monotonicity condition requires that satisfying both eligibility conditions will not cause a reduction in pension income. This condition is readily satisfied because of the nature of the subsidy program which aims to increase pension income.

2.5 Results

In this section, we first present graphical evidence and estimation results under the DID framework. We show both the pension income and mortality responses to eligibility for the subsidy program. We further estimate heterogeneous results and robustness and placebo tests. Then, we show the impact of additional pension income on mortality using the IV method. Finally, we explore the impact on health outcomes, financial constraints, psychological feelings, and risky behaviours to better understand the mechanisms.

2.5.1 Effects on Pension Income

First, we examine graphically the impact on the probability of receiving the subsidy and the amount of the subsidy received. Figure 2.A.5(a) plots average subsidy size against years of contributions for the control ($aep \geq 0.75$) and treatment ($aep < 0.75$) groups. We observe a sharp increase in the subsidy received after 35 years of contributions for the treatment group. In contrast, no change is observed for the control group. The empirical pattern is similar to the policy schedule depicted in Figure 2.A.4. The similar pattern is observed when we look at the probability of receiving subsidies (Figure 2.A.5(b)).

Furthermore, Figure 2.A.6(a) shows pension benefits without subsidies by number of contribution years, for the treated and control groups. In absence of the subsidy, pension benefits increase approximately linearly with the number of contribu-

Table 2.1. Impact of subsidy eligibility (DID estimates)

	(1)	(2)	(3)	(4)	Mean
First stage					
Recipient	0.701***	0.700***	0.693***	0.693***	0.297
	(0.010)	(0.010)	(0.008)	(0.008)	(0.457)
	[0.000]	[0.000]	[0.000]	[0.000]	
Subsidy	0.588***	0.587***	0.580***	0.579***	0.235
	(0.020)	(0.020)	(0.018)	(0.018)	(0.472)
	[0.000]	[0.000]	[0.000]	[0.000]	
Pension income	0.241**	0.238***	0.292***	0.579***	7.060
	(0.053)	(0.052)	(0.036)	(0.018)	(254)
	[0.001]	[0.001]	[0.000]	[0.000]	
Impact on mortality					
Age at death	0.203***	0.191***	0.155**	0.136**	72.241
	(0.031)	(0.034)	(0.035)	(0.036)	(3.716)
	[0.000]	[0.001]	[0.003]	[0.009]	
Dying before 65	-0.010***	-0.010***	-0.010***	-0.008***	0.055
	(0.002)	(0.002)	(0.002)	(0.002)	(0.227)
	[0.000]	[0.000]	[0.000]	[0.000]	
Dying before 70	-0.022***	-0.021**	-0.017**	-0.015**	0.253
	(0.003)	(0.004)	(0.004)	(0.004)	(0.434)
	[0.001]	[0.002]	[0.003]	[0.004]	
Dying before 75	-0.018**	-0.015**	-0.010	-0.008	0.512
	(0.006)	(0.006)	(0.006)	(0.006)	(0.500)
	[0.017]	[0.037]	[0.128]	[0.186]	
Impact on labour supply					
Age at claiming pension	-0.036	-0.031	0.020	-0.041	62.96
	(0.041)	(0.040)	(0.034)	(0.035)	(2.349)
	[0.417]	[0.459]	[0.555]	[0.295]	
Obs	149,053	149,053	149,053	149,053	149,053
Contribution year FE	✓	✓	✓	✓	-
Birth cohort FE		✓	✓	✓	-
Controls			✓	✓	-
PI without subsidy				✓	-

Notes: This table shows the impact of being eligible for the pension subsidy on a list of first-stage, mortality and labour supply outcomes. Sample: RTWF baseline sample. Columns 1, 2, 3 and 4 show the results with contribution year fixed effects, adding birth cohort fixed effects, adding controls, adding income control, respectively. Control includes an indicator for male, for having at least one child, and not having health insurance. PI without subsidy stands for monthly pension income without subsidy. Sample means are reported in Column 5. "Age at claiming pension" refers to the age at which the individual started to claim the pension they are currently receiving. Monetary values are expressed in hundred 2015 euro. Standard errors clustered at birth cohort level are in parentheses, bootstrapped p-values are in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

tion years. We do not see a change in the gap after the 35 years cutoff. In contrast, Figure 2.A.6 (d) shows that the pension benefits with subsidies entail a permanent upward shift of the trajectory, decreasing the difference in pension benefits between the two groups. The patterns are similar when we look at men and women separately.

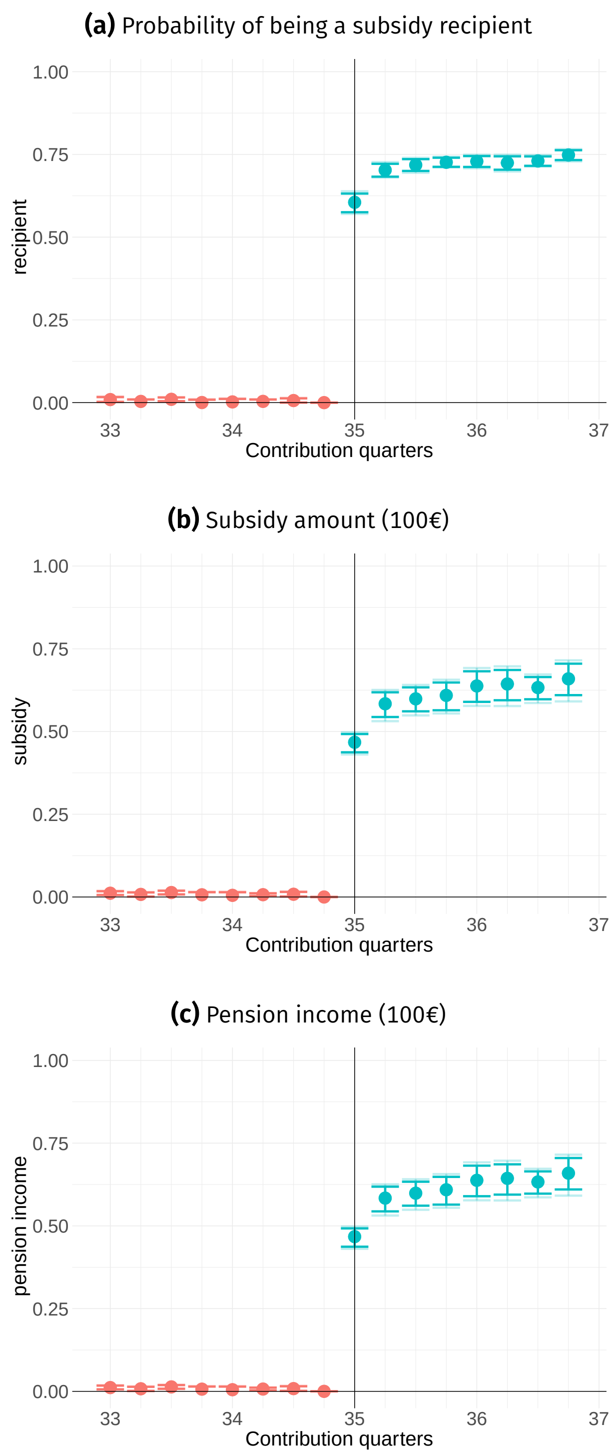
Panel A of Table 2.1 shows the first-stage DID estimates, i.e., the impact on the probability of receiving a subsidy, subsidy amount, and total pension income. We progressively control for contribution years fixed effects (column 1), birth cohort fixed effects (column 2), demographic, pension-related variables (column 3), and finally pension income without subsidy (column 4). We find that eligibility for the subsidy increases the probability of receiving a subsidy by 69%, increases the size of the subsidy received by 58€/month, and increases pension income by the same amount. While the estimates on the probability of being a recipient and subsidy amount are not sensitive to varying controls, these matter for the estimated effect on pension income. This is not surprising as the two groups differ in their lifetime income. Therefore, it is crucial to add pension income without subsidy as a control variable. Figure 2.1 and panel A of Table 2.A.8 show the event-study figure and results. We observe non-significant or precisely zero estimates before the 35 contribution years cutoff and a sharp increase in subsidy amount, pension income and probability of being a recipient afterwards, supporting the parallel trend assumption.

2.5.2 Effects on Mortality

We now examine the effect of subsidy eligibility on mortality outcomes. The graphical evidence and regression analysis show that additional pension income reduces mortality. Figure 2.A.7 plots the mean mortality outcomes for for the control ($aep \geq 0.75$) and treatment ($aep < 0.75$) groups by number of contribution years.

We identify two patterns. First, the probability of dying before 65 notably increases after 35 years of contributions, particularly among men (Figure 2.A.7). This pattern emerges probably because 35 years of contributions qualify individuals for an early retirement option —the old-age pension for the long-term insured, which allows earlier retirement at age 63. This option is less utilized by women, who could retire at age 60 through a specific women's pension. The discontinuous increase in mortality is likely due to the negative association between retirement and mortality, which has been shown in the U.S. context (Fitzpatrick and Moore, 2018). Second, although the probability of dying before age 65 spikes after reaching 35 years of contribution for both groups, the gap widens after the cutoff. Similarly, we observe

Figure 2.1. Event study coefficients in the baseline sample, first stage.



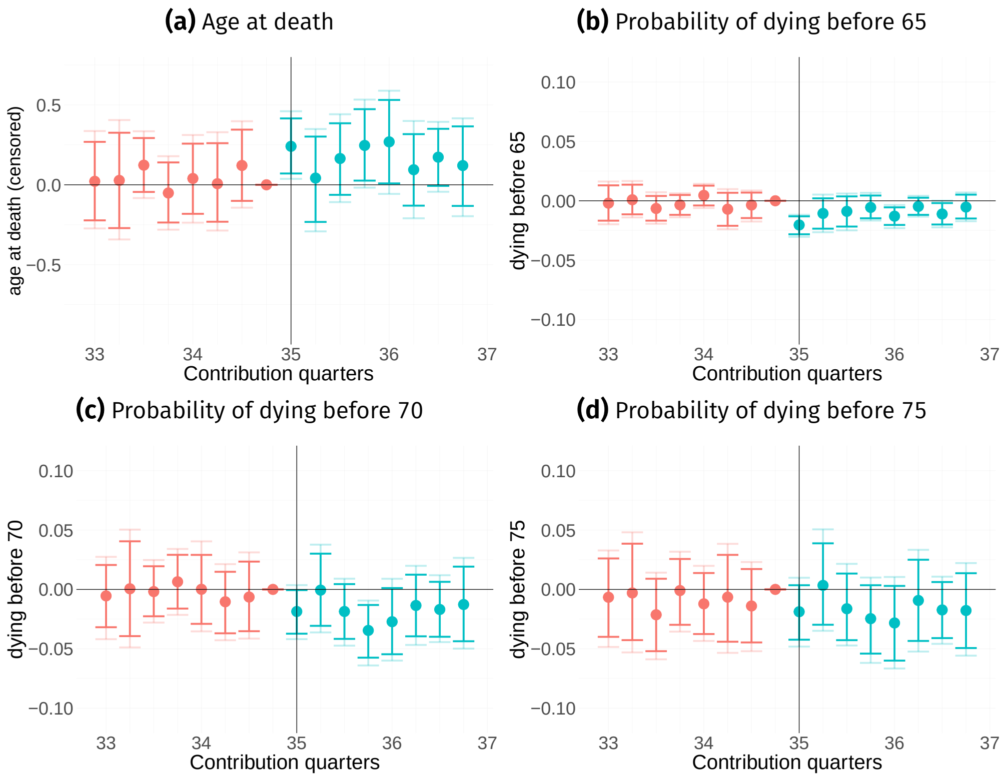
Notes: Figure 2.1 displays the event study coefficients for first-stage outcomes in the baseline sample. Time dimension are contribution quarters. All subfigures plot the 95 percent CI (shadowed line) and 90 percent CI (solid line).

a similar evolution of mortality outcomes before the 35 year cutoff and a change in the trajectory in favour of the poorer group, likely due to additional pension income. These patterns suggest that the improvement in life expectancy for the poorer group partly comes from offsetting the harmful impact of retirement. As income and consumption drop at retirement (e.g., Hurst (2008) and Battistin et al. (2009)), additional pension income could help low-income retirees better cope with the transition from work to retirement. However, these are raw scatter plots that do not control for important covariates. For example, notably, without controlling for birth cohorts nor pension income without subsidy, the mortality rates of the poorer group are lower than those of the richer group. Therefore, we will turn to the regression results, which allow for a more precise analysis.

Panel B of Table 2.1 shows the estimated effects of being eligible for additional pension on mortality. We find significant decreases in the probability of dying before 65, and 70 by 0.8, and 1.5 percentage points, respectively. These correspond to relative decreases of 15.7%, and 5.8% with respect to the sample average. The estimates are not sensitive to varying controls. The event study results are depicted in Figure 2.2 and reported in Panel B of Table 2.A.8. For each outcome, we find non-significant and close to zero point estimates before the 35 year cutoff, supporting the parallel trends assumption. We show the 95 percent CI (shaded line) and the 90 percent CI (solid line) in Figure 2.2.

When we use alternative measures of mortality, the results are similar. Table 2.A.9 show the impacts on the probability of dying between age 62 and 69, the probability of dying between 70 and 75, and the probability of dying within 4, 8, and 12 years of claiming an old-age pension (hence also 4, 8 and 12 years after receiving the subsidies, respectively). Eligibility for the subsidy decreases the probability of dying between 62 and 69, and the probability of dying within 8 and 12 years from retirement by 1.5, 0.9 and 1.2 percentage points, respectively. Figure 2.A.8 further plots the impact on the probability of dying at each year after retirement (left column), which is also the time when the subsidies are disbursed, and the impact on the cumulative probability of dying by each year (right column). We can see that the additional income yield improvement effects starting from the second year for men. Moreover, the improvement in life expectancy is driven by the responses in the first 5 years after the subsidies are received.

Figure 2.2. Event study coefficients in the baseline sample.



Notes: Figure 2.2 displays the event study coefficients for main mortality outcomes in the baseline sample. All subfigures plot the 95 percent CI (shadowed line) and 90 percent CI (solid line).

2.5.2.1 Heterogeneous Mortality Effects

Table 2.2 reports the DID estimates by gender, marital status, income distribution and type of health insurance. Table 2.A.12 shows the p-values testing the hypothesis that the coefficients by subgroup are equal.

Table 2.2. Heterogeneous effects (DID estimates)

		Gender		Marital status		Subsidy size		Health insurance	
	Baseline	Women	Men	Married	Not married	High	Low	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
First stage									
Recipient	0.693***	0.705***	0.440***	0.655***	0.756***	0.758***	0.629***	0.728***	0.382***
	(0.008)	(0.009)	(0.007)	(0.008)	(0.009)	(0.012)	(0.007)	(0.007)	(0.013)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Subsidy	0.579***	0.632***	0.267***	0.559***	0.570***	0.874***	0.292***	0.618***	0.267***
	(0.018)	(0.015)	(0.009)	(0.015)	(0.023)	(0.034)	(0.007)	(0.018)	(0.010)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pension income	0.579***	0.632***	0.267***	0.559***	0.570***	0.874***	0.292***	0.618***	0.267***
	(0.018)	(0.015)	(0.009)	(0.015)	(0.023)	(0.034)	(0.007)	(0.018)	(0.010)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Impact on mortality									
Age at death	0.136**	0.028	0.234**	0.135**	0.121	0.163**	0.108**	0.121**	0.136
	(0.036)	(0.048)	(0.068)	(0.044)	(0.082)	(0.043)	(0.037)	(0.043)	(0.105)
	[0.009]	[0.575]	[0.009]	[0.019]	[0.191]	[0.006]	[0.021]	[0.028]	[0.223]
Dying before 65	-0.008***	-0.001	-0.005	-0.004	-0.014**	-0.008***	-0.009**	-0.007**	-0.014
	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.008)
	[0.000]	[0.821]	[0.150]	[0.266]	[0.006]	[0.000]	[0.001]	[0.002]	[0.122]
Dying before 70	-0.015**	-0.004	-0.030**	-0.016**	-0.012	-0.018***	-0.012**	-0.014**	-0.013
	(0.004)	(0.004)	(0.010)	(0.005)	(0.009)	(0.005)	(0.004)	(0.005)	(0.016)
	[0.004]	[0.355]	[0.012]	[0.011]	[0.223]	[0.000]	[0.016]	[0.030]	[0.419]
Dying before 75	-0.008	-0.002	-0.023**	-0.012*	-0.001	-0.014*	-0.003	-0.009	0.006
	(0.006)	(0.008)	(0.010)	(0.006)	(0.012)	(0.006)	(0.006)	(0.007)	(0.018)
	[0.186]	[0.829]	[0.049]	[0.056]	[0.928]	[0.067]	[0.644]	[0.219]	[0.798]
Dying before 80	-0.003	-0.001	-0.008	-0.005	-0.002	-0.005	-0.002	-0.003	0.001
	(0.003)	(0.004)	(0.006)	(0.005)	(0.003)	(0.004)	(0.003)	(0.003)	(0.019)
	[0.271]	[0.828]	[0.230]	[0.281]	[0.490]	[0.147]	[0.578]	[0.299]	[0.952]
Impact on labour supply									
Age at claiming pension	-0.041	-0.172**	0.037	-0.053	-0.006	-0.120**	0.035	-0.070*	0.123
	(0.035)	(0.044)	(0.036)	(0.039)	(0.044)	(0.036)	(0.050)	(0.035)	(0.079)
	[0.295]	[0.005]	[0.350]	[0.211]	[0.892]	[0.014]	[0.493]	[0.084]	[0.151]
Obs	149,053	96,820	52,233	89,169	51,548	123,300	122,615	129,920	13,464
Contribution year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table shows heterogeneous effects of being eligible for pension subsides. Column 1 shows the impact for the baseline sample. Columns 2 and 3 show the results by gender. Columns 4 and 5 show the results by marital status.Columns 6 and 7 compare the impacts on individuals with ape between 0.45 and 0.61 with the impacts on individuals with aep between 0.61 and 0.75. Columns 8 and 9 show results by health insurance status. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *Source*: Authors' calculations from the RTWF data.

By Gender. Columns (2) and (3) of Table 2.2 show the DID estimates by gender. Unsurprisingly, women in our sample receive more than twice the amount of subsidy than men. This is because, on average, women have a lower lifetime earnings profile than men and are therefore more likely to be entitled to a higher level of subsidy.[24] However, despite a larger first-stage impact on pension income, we find stronger effects on mortality for men than women. Men's probability of dying before 70 and 75 decreases by 3 and 2.3 percentage points, respectively, while the effects for women are close to zero and insignificant. Age at death (censored at 75) increases by 0.23 years (2.8 months) for men, while there is no effect for women. On the other hand, we find that eligible women respond by retiring earlier. They start to claim a pension about 2 months earlier, which could partly contribute to the absence of mortality effects. In the following paragraph, we explore whether marital status and children lead to the gender difference. Section 2.6.1 provides a in-depth discussion of gender differences, combining evidence on health outcomes with the survey data.

By Marital Status and Children. One possible explanation for the lack of impact on women could be related to the actual standard of living of the treated women. It is possible that the program subsidises women in relatively well-off households, since the subsidy is a function of individual rather than household income. However, their actual standard of living may be determined not only by their own income but also by the income of their husbands or children. Moreover, because of the possibility of accumulating contribution years from years spent caring for children, low-income women with shorter careers can also be eligible for the subsidy. Therefore, the additional income from the subsidy may have a limited impact on their longevity. To explore this possibility, we perform some heterogeneity analyses on women by their marital status and whether they have children.

We would expect single women to benefit more from the subsidy, as they are the sole earners in the household. However, Table 2.A.10 shows that neither married nor not married women seem to experience mortality (columns (1) and (3)), despite similar first-stage estimates. Table 2.A.12 shows the p-values testing the hypothesis that the coefficients by subgroups within the female sample are equal.[25] If household composition explains the absence of mortality effects for women, we would also expect eligible women without children to benefit more from the subsidy as they need to have worked more years to reach the 35 contributions years threshold compared to women with children. Yet again, we do not find significantly

[24] Women's aep^{92} are distributed centering 0.5, which grants the highest amount of pension subsidy, all else equal.

[25] Marital status is recorded at the time of pension claim application. The singles include widower, divorcees and the ones who have never married. There are around 9% with missing marital status, which we do not include in this heterogeneity analysis. We suspect that those ones who with missing status are widows.

different effects by parental status (columns (4) and (5)). However, the impact on pension income is slightly higher for non-mothers. Marital status and children do not seem to help explain why women's mortality outcomes are less responsive. Section 2.6.1 provides additional discussion of the gender differences.

Columns (5) and (6) of Table 2.A.10 show heterogeneous effects by marital status for men. Here, we estimate a bigger effect on subsidy size for unmarried men. Nonetheless, mortality results are driven by married men, for whom we estimate similar effects as for the full sample of men.

By Subsidy Size. Although pensioners in our treatment group are all poor, with below average monthly wage, it would be interesting to see if very poor and relative poor pensioners react differently. Columns (6) and (7) of Table 2.2 split the treatment group into ones with lower and higher *aep*, while keep the control group unchanged. Because the subsidy size is correlated with *aep*, essentially we compare individuals with varying subsidy size. Those with *aep* between 0.45 and 0.61 on average receive 87 euro additional monthly pension income, while those with *aep* between 0.61 and 0.75 receive 29 euro additional pension income per month. We find that those with more subsidy gain 0.16 years of life expectancy compared with 0.11 years of life expectancy for those with less subsidy. The estimated changes in the probability of dying before 65, 70 and 75 are also larger for the former group. Table 2.A.12 shows that the differences in age at death and the probability of dying before 65 are statistically significant at the 10% and 5% levels.

In Table 2.A.11, we further present the heterogeneous impacts by subsidy size for men and women separately. This exercise also help us to see if the improvement in life expectancy is driven by the ones receive more subsidies. Additionally, we show the estimated impacts on individuals with *aep* between 0.25 and 0.45, who have even lower average lifetime wage, despite receiving similar amount of pension subsidy due to the kinked subsidy schedule. This helps us to see whether the poorest pensioners' life expectancy benefit more.

We derive two main insights. First, the mortality impacts on men are driven by men with more subsidy, around 40 to 60 €per month of subsidy. We do not find any changes in mortality for men with *aep* between 0.61 and 0.75, who receive 18 €per month of subsidy. Second, the poorest women actually experience an improvement in life expectancy from additional pension income. Eligible women with *aep* between 0.25 and 0.45 receive 89 €additional monthly pension benefit, similar as the women with *aep* between 0.45 and 0.61. Yet, they have significantly lower probabilities of dying before age 75 (1.7 percentage points lower). These results suggest that subsidy size matters, especially for those retirees with very low income.

By Health Insurance Status. Finally, we investigate the heterogeneous effects by health insurance status. In Germany, the majority of people have public health insurance. Only people with a higher labor income (or who are a dependent of a private health insurance policy holders) or the self-employed can enroll in private health insurance.[26] As those with private health insurance have higher household incomes and better healthcare coverage, we expect that additional pension income has little impact on their well-being. We find that the first-stage estimates are smaller for those with private health insurance. People with private health insurance received a smaller subsidy, on average, mainly because they are more likely to be men. The age at death is postponed by 1.5 months (significant at the 10 percent level) for those with public health insurance, while it increases by 1.6 months for those with private health insurance, but the effect is insignificant. Taking into account the different magnitudes of the first-stage estimates, the results for mortality are similar, with the exception of the probability of dying before age 75.

2.5.2.2 IV to Scale the Effect

To quantify the impact of an additional 100€of pension income on mortality and to compare it with the health outcomes using Survey data, we also the impacts in an IV framework. Table 2.3 reports the effect of having more pension income on mortality and the age at which the pension is claimed. We show the estimates for the overall sample (columns (1) and (2)) and by gender (columns (3)-(6)). Odd columns do not control for age at claiming pension, while even columns do. One concern of the exclusion restriction is that eligibility for the subsidy also affects retirement choices. Our preferred specification is to control for age at claiming pension (including pension pathways) to abstract from possible labor supply effects.

Panel A shows the first-stage estimates. Eligibility for a pension subsidy increases pension income by around 58€/month (63€/month for women, and 27€/month for men, on average). F-statistics are above the rule-of-thumb threshold of 10 in all specifications.

[26] Although the majority of the German population is covered by generous statutory health insurance, out-of-pocket payment for healthcare services remain and account for 13% of total healthcare expenditure in Germany (WHO, 2023). Bock et al. (2014) shows that the top three highest amount of out-of-pocket payment for elderly German public health insurance beneficiaries are medical supplies, dental prostheses and payments for pharmaceuticals (co-payments for prescribed drugs, and non-prescribed drugs). This is mainly driven by the fact that costs for glasses, medical devices that went beyond pure medical necessity, such as electrical wheelchair, dental prostheses, and non-prescribed drugs are not covered by health insurance. See here for a detailed description of the co-payment regulations of the statutory health insurance in Germany. Privately insured individuals could have better coverage and incur less out-of-pocket payment. Unfortunately, we can't test the impact on out-of-pocket payments due to sample size limitations.

Table 2.3. Impact of pension income on mortality (IV estimates)

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Pension income	0.579***	0.579***	0.632***	0.628***	0.267***	0.267***
(instr.=eligible)	(0.017)	(0.017)	(0.014)	(0.014)	(0.009)	(0.009)
Panel B: IV						
Impact on mortality						
Age at death	0.235***	0.251**	0.037	0.082	0.652***	0.608***
	(0.060)	(0.067)	(0.068)	(0.071)	(0.177)	(0.169)
	[0.008]	[0.010]	[0.617]	[0.301]	[0.009]	[0.008]
Dying before 65	-0.014***	-0.016***	-0.001	-0.007	-0.008	-0.004
	(0.003)	(0.003)	(0.003)	(0.004)	(0.008)	(0.009)
	[0.000]	[0.000]	[0.687]	[0.135]	[0.348]	[0.674]
Dying before 70	-0.026***	-0.028***	-0.007	-0.010	-0.071**	-0.067**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.024)	(0.023)
	[0.005]	[0.005]	[0.343]	[0.186]	[0.019]	[0.022]
Dying before 75	-0.015	-0.015	-0.001	-0.002	-0.072**	-0.070**
	(0.010)	(0.010)	(0.012)	(0.012)	(0.028)	(0.028)
	[0.187]	[0.179]	[0.901]	[0.848]	[0.042]	[0.046]
Impact on labour supply						
Age at claiming pension	-0.071	-	-0.275***	-	0.130	-
	(0.057)	-	(0.057)	-	(0.097)	-
	[0.279]	-	[0.001]	-	[0.225]	-
First stage F-stat	22,000	22,000	16,000	16,000	6,058.48	6,070.35
Obs	149,053	149,053	116,515	116,515	68,914	68,914
Contribution years FE	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓
Age at claiming pension		✓		✓		✓

Notes: This table shows the effect on mortality of an increase in pension income of 100 euros per month. Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator of eligibility for pension subsidy. Pension income is calculated based on total earning points at retirement. In addition to a list of controls, pension income without subsidy, birth cohort fixed effects and contribution year fixed effects in the odd columns, the even columns control for age at claiming pensions. Columns 1 and 2 show the results for the baseline sample. Columns 3 to 6 show the results for women and men respectively. Monetary values are expressed 2015 in hundred euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *Source*: Authors' calculations from the RTWF data.

The IV estimates in Panel B of Table 2.3 indicate that more pension income has a statistically significant positive effect on age at death and a significant negative effect on the probability of dying before 65, and 70. These estimates are in line with DID results. We find that 100€ per month of additional pension income causes a decrease of 1.4, and 2.6 percentage points in the probability of dying before 65, and 70, respectively, in the full sample. The age at death censored at 75 increases by 2.8 months. Similar to the DID estimates, results on mortality are predominantly driven by men, who experience substantially larger improvements in mortality outcomes than women. We estimate a more than 6-months increase in age at death, and a reduction of the probability of dying before ages 70 and 75 by 7.1 and 7.2 percentage points, respectively. Women, on the other hand, do not experience any longevity gains, but claim pension about 3 months earlier. Moreover, the inclusion of a control for the age at which a pension was claimed does not affect the magnitude of the estimates.

To interpret the IV results, it is important to understand who the compliers are. In our setting, the compliers are those individuals whose pension income increases when they fulfill the two eligibility conditions. Because the pension subsidy amount is computed by applying a built-in formula and is credited directly to the recipient's pension account by the pension office, almost all eligible individuals are compliers. The only exception is people with a zero subsidy amount because their aep^{92} is above 0.75, therefore according to the subsidy formula, they receive a zero subsidy amount even though they fulfill both eligibility conditions. In practice, there are very few never-takers. These are people with higher average wages before 1992 but lower average wages when they retire. Table 2.A.14 compares the characteristics of individuals in the baseline sample with those of the eligible individuals, the compliers (subsidy recipients in the eligible group, 78% of all eligible individuals) and the never-takers (individuals who received no subsidy despite of general subsidy eligibility, 22% of all eligible individuals). Compared to the never-takers, the compliers are less likely to be male, married and more likely to be covered by public health insurance. They also have children at an earlier age and have fewer years of schooling. Compliers are poorer overall — they have lower pension incomes without the subsidy and are less likely to own a home.

2.5.3 Effects on Pension Claiming

Additional pension income can also change pension claiming behaviors. Panel C of Table 2.1 reports the impact on age when the pension is claimed. We do not find any significant effects for the full sample. When we split the sample by gender, we find that an increase by 63€/month in pension income induces women to claim their pensions 2.1 months earlier (Column (2) of Table 2.2). The response is smaller but in the same ballpark as the claiming responses estimated in Ye (2022) for female recipients. [27]

Table 2.A.13 further explore the impact of different early retirement pathways. We find that subsidy eligibility leads to an increase in the probability of claiming pension for the unemployed and claiming pension for the long-term insured and a decrease in claiming disability pension. This is reasonable given the strict requirements for claiming disability insurance (rigorous medical examinations) and the greater stigma associated with being labeled as disabled (Celhay, Meyer, and Mittag, 2025). Pensioners with additional income can now afford to opt for other early retirement pathways, although claiming disability insurance at the same age entails less financial penalties.[28] When examining the impact by gender, we see a different pattern. Eligible women are more likely to use women's pension and the pension for long-term insured as early retirement pathways, rather than relying on the unemployed pathway. For eligible men, they substitute disability pension pathway with pension for the unemployed and pension for the long-term insured when additional income become available.

We also examine the heterogeneous impact on age at claiming by martial status, subsidy size and health insurance. Overall, we find that the decline in age at claiming pension is driven by women, pensioners eligible for a larger amount of subsidy and those with public health insurance.

2.5.4 Placebo Tests and Robustness Checks

Placebo Tests. First, we take the two placebo samples consist of people of aep_i higher than 0.75 : $aep_i \in [0.8, 1.25]$ and $[1, 1.4]$ (columns (2)and (3) of Table 2.A.15). These individuals are not eligible for the subsidies. We take a hypothetical cutoff in *aep* in these placebo checks. Even though we find some positive effect on pension income, the size is an order of magnitude smaller (around 2 euro more per month). We do not find any significant impact on mortality. Estimated event study coefficients for the $aep_i \in [0.8, 1.25]$ sample are depicted in Figure 2.A.9. This exercise also helps to rule out the possibility that the estimated mortality impact is

[27] Ye (2022) shows that a 100 euro increase in the monthly subsidy induces female recipients to claim their pensions six months earlier. Our sample includes both recipients and non/recipients and we measure the intend to treat effect. This is probably why we find a smaller effect on age at claim for women in this paper. Note that we cannot use the same regression kink method as in Ye (2022) to examine the impact on recipients because we don't observe aep_{92} in our data.
[28] This is because disability insurance has a lower normal retirement age.

driven by differences in mortality responses to early retirement across income levels.

Second, we consider two placebo samples, consists of people with lower or higher number of contribution years than in the baseline sample: $CY \in [29, 32]$, $CY \in [37, 40]$ (columns (3) and (4) of Table 2.A.15). These individuals are not eligible for the subsidies. We take a hypothetical cutoff in CY (30 and 39, respectively) in these placebo checks. Again, we find none or small positive effects on pension income (less than 5€/month). We do not find significant effects on mortality. These tests rule out the possibility that other confounding factors are driving our reduced-form estimates.

Robustness Checks. Several exercises further establish the robustness of the estimates as we vary sample selection (Table 2.A.16). First, we present that the results are robust to the exclusion of individuals who retired after exactly 420 months (35 years). Second, the estimates are similar in magnitude when we narrow the bandwidth of *aep* be even loser to the cutoff. The results are also robust when we expand the *aep* bandwidth and expand the contribution year bandwidth to between 30 and 40 years (column 5 in Table 2.A.16). Third, our estimates are also robust to the inclusion of younger cohorts, i.e. individuals born 1943 to 1948 and when restricting the analysis to cohorts born between 1932 and 1937, i.e. the cohorts born before the Second World War. For more detailed discussion, see Appendix 2.D.

2.5.5 Survey Evidence: Health Outcomes

To better understand the mechanisms behind the reduction in mortality, we explore the changes in health outcomes by exploiting the SHARE-RV data. First, we show the impact on health measures, including measurements of overall health, self-perceived health, number of chronic diseases, and depression index.[29] We then unpack overall response of better physical health by looking at specific types of chronic diseases. Moreover, to probe deeper into the connection between income and mental health, we look at a measure of optimism and measures of perceived financial constraints. We also look at changes in risky behaviours. We present the IV estimates in this section.

It is worth noting that we can only observe the survey responses of surviving individuals. Since we find a reduction in mortality due to higher income using the administrative dataset, older pensioners who survive to participate in the survey without the subsidy may be healthier than those with the subsidy. Therefore, the

[29] See Section 2.3.2 and Table 2.A.5 for descriptions of these outcome variables.

survey sample in the treatment group may be less healthy compared to the control group. In this case, our health estimates likely provide a lower bound on the health impact of additional pension income.

Table 2.4. Impact of pension income on health outcomes (IV estimates)

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Pension income	0.414***	0.440***	0.416***	0.431***	0.573***	0.580***
	(0.037)	(0.037)	(0.044)	(0.044)	(0.077)	(0.076)
Panel B: IV						
CASP	0.693***	0.644***	-0.121	-0.094	1.461***	1.413***
	(0.239)	(0.226)	(0.274)	(0.266)	(0.422)	(0.415)
Self-reported heatlh	0.654***	0.658***	-0.289	-0.252	2.076***	2.063***
	(0.248)	(0.235)	(0.285)	(0.275)	(0.506)	(0.499)
Depression index	-0.532**	-0.506**	0.339	0.323	-0.774**	-0.753**
	(0.237)	(0.223)	(0.291)	(0.280)	(0.343)	(0.337)
Number of chronic diseases	-0.825***	-0.796***	-0.071	-0.092	-2.330***	-2.279***
	(0.257)	(0.244)	(0.278)	(0.271)	(0.596)	(0.583)
Difficulties with ADLAs	-0.147	-0.253	0.606*	0.525	-1.013***	-1.058***
	(0.266)	(0.260)	(0.350)	(0.337)	(0.390)	(0.398)
Difficulties with IADLAs	-0.472**	-0.482**	0.099	0.088	-0.628*	-0.630*
	(0.221)	(0.220)	(0.222)	(0.220)	(0.358)	(0.361)
Length hospital stay (nights)	-0.920	-0.996	2.630	2.540	-6.462	-6.525
	(2.062)	(1.947)	(1.875)	(1.813)	(5.715)	(5.658)
Long hospital stay (≥14)	-0.036	-0.035	0.018	0.019	-0.039	-0.043
	(0.051)	(0.048)	(0.054)	(0.052)	(0.096)	(0.094)
Number hospital stays	0.136	0.115	0.266**	0.253**	-0.169	-0.176
	(0.102)	(0.097)	(0.124)	(0.120)	(0.180)	(0.179)
First stage F-stat	124.30	141.51	87.53	96.23	55.86	58.70
Obs	2,328	2,328	1,365	1,365	963	963
Contribution year FE	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓
Retirement age		✓		✓		✓

Notes: This table shows the effect on health outcomes of an increase in pension income of 100 euros per month. Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator for eligibility for the pension subsidy. Pension income is calculated based on total earning points at retirement. Estimates are based on standardised outcomes and thus measure effects in percent of the standard deviation from mean. In addition to a list of controls (number of schooling years, married, having children, interaction between having children and being in contact with them at least once a week, interaction between having children and all children having a job, SES indicators, being a house owner, male), pension income without subsidy, birth cohort fixed effects and contribution year fixed effects in the odd columns, the even columns control for age at claiming pensions. Columns 1 and 2 show the results for the baseline sample. Columns 3 to 6 show the results for women and men respectively. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the SHARE-RV data.

Overall Health. Table 2.4 shows the effect of additional pension income on overall health. Columns (1) and (2) show results for the baseline sample, (3) and (4) for women, and (5) and (6) for men. The estimated coefficients are reported in terms of standard deviations from the mean. Even columns control for age at claiming pension. Generally, adding the age control does not substantially affect the estimates. In the following, we focus on the results when controlling for age at claiming pension. Panel A shows that all estimated first-stage coefficients are positive and highly significant and F-statistics are above 10. Different to the RTWF sample, eligibility for the subsidy program increases the pension income by 43€/month for women, and 58€/month for men in the SHARE-RV sample.[30]

Table 2.4 Panel B reports the IV estimates. For the full sample, we find a positive impact on overall well-being, as measured by CASP (an indicator of overall well-being), and a reduction in the number of diagnosed chronic diseases. An additional 100€ monthly pension income increases the CASP by 65 percent of a standard deviation (significant at the 1 percent level) and the number of chronic diseases decreases by 80 percent of a standard deviation (significant at the 1 percent level). When distinguishing between genders, we find the improvements in health measures are driven by men, which is consistent with the finding that men drive the improvement in mortality outcomes in Table 2.3. We also see a significant improvement in the self-perceived health and depression index. An additional 100€ monthly pension income improves men's overall well-being (CASP) by 1.4 standard deviations, self-perceived health by 2.1 standard deviations and reduces the depression index by 0.8 standard deviations and number of diagnosed chronic diseases by 2.3 standard deviations. The results are consistent with existing evidence, which has shown that the number of chronic diseases and depression symptoms are strongly correlated with a worse quality of life and excess mortality (Adamson et al., 2005; Kahneman and Krueger, 2006). For women, we find non-significant effects of additional pension income.

Long-term Care Dependency. We also examine the impact of two health measures which are linked to long-term care dependency: difficulties with Activities of Daily Living (ADLs, including dressing, bathing, going to bed, eating, walking across a room), difficulties with Instrumental Activities of Daily Living (IADLs, including shopping, preparing meals, taking medication, managing money, using the telephone). These two measures are also good indicators of cognitive decline, which can have a negative impact on financial decision-making (Mazzonna and Peracchi,

[30] This is because the treated men are poorer than treated women in the SHARE-RV sample because we include a wider range of *aep* and contribution years in the survey sample. The treated men in the SHARE-RV sample have a pension income of around 715€/month without subsidy, while treated women have 760€/month of pension income without subsidy.

2020), in addition to the need for long-term care services (Li et al., 2023). We find an additional 100€/month of pension income reduces men's difficulties with ADLs by about 1.1 standard deviation at the 1 percent significant level. For difficulties with IADLs, we find an overall decline of 0.6 standard deviations (significant at 10% level), again driven by men. These findings suggest that additional pension income may reduce pensioners' long-term care dependency, which can alleviate considerable financial burdens on the healthcare system. For women, surprisingly, we find an increase in difficulties with ADLs by 0.6 standard deviations at a 10 percent significance level.

Chronic Diseases. Table 2.5 investigates the effects on specific types of chronic disease, including whether the individual has had a stroke, has chronic lung disease, has cataracts, and has high blood pressure. We again observe substantially stronger effects for men than for women. Women experience marginal reductions in the probability of chronic lung disease and cataracts (non-significant effects). On the other hand, men experience sizable reductions in the probability of being currently diagnosed with a chronic lung disease, cataracts, or high blood pressure.

Table 2.A.17 examines the impact on diseases for which the incidence is less likely to be affected by changes in income, such as cancer, Parkinson's disease, fractures of the hip, and the incidence of diabetes. Indeed, we find no effect of additional pension income on the probability of these occurring.[31]

Future Outlook. In addition, we explore the impacts on perceived financial constraints and optimism in Table 2.5. Both measures can be underlying causes of stress, depression, and poor mental health, consequently affecting mortality (Mendes de Leon, Rapp, and Kasl, 1994; Gardner and Oswald, 2004; Ridley et al., 2020). All coefficients are expressed in standard deviations from the mean. For the full sample, we find significant improvements in "feel full of opportunities" and "future looks good". Again, men drive the results. We also see a significant reduction in feeling a "lack of money stops them from participating in activities" for men. These factors could contribute to the estimated decrease in depression and improvement in self-perceived health for men.

[31] Potentially, higher pension incomes may affect the incidence of diabetes by allowing pensioners to afford healthier diets. In addition, less stress about money could also reduce the likelihood of obesity. However, diabetes is caused by genetic predisposition and obesity. These factors take many years to influence the onset of diabetes, and we find no significant effect of additional pension income on the likelihood of having diabetes. Note that we cannot distinguish between type I and type II diagnosis in the data.

Table 2.5. Impact of pension income on other outcomes (IV estimates)

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Pension income (100€)	0.414***	0.440***	0.416***	0.431***	0.573***	0.580***
	(0.037)	(0.037)	(0.044)	(0.044)	(0.077)	(0.076)
Panel B: IV						
(I) Chronic diseases						
Stroke	-0.018	-0.020	0.016	0.014	0.041	0.038
	(0.040)	(0.040)	(0.047)	(0.047)	(0.073)	(0.073)
Chronical lung disease	-0.147**	-0.151**	-0.019	-0.029	-0.416***	-0.418***
	(0.062)	(0.059)	(0.069)	(0.067)	(0.146)	(0.144)
Cataracts	-0.202***	-0.191**	-0.141	-0.140	-0.260**	-0.248**
	(0.078)	(0.075)	(0.098)	(0.096)	(0.119)	(0.117)
High blood pressure	-0.068	-0.050	0.233	0.233	-0.755***	-0.728***
	(0.118)	(0.112)	(0.147)	(0.143)	(0.215)	(0.210)
(II) Financial constraints and optimism						
Lack of money stops	-0.375	-0.335	-0.212	-0.216	-1.106**	-1.043**
	(0.244)	(0.232)	(0.288)	(0.280)	(0.438)	(0.431)
Feel full of opportunities	0.758***	0.665***	-0.020	-0.012	1.507***	1.436***
	(0.243)	(0.228)	(0.283)	(0.274)	(0.433)	(0.424)
Future looks good	0.658***	0.608***	0.186	0.183	1.097**	1.070**
	(0.245)	(0.231)	(0.288)	(0.279)	(0.426)	(0.420)
(III) Risky behaviours†						
Days/week alcohol (last 6 months)	-0.141	-0.045	-0.099	-0.065	-0.864*	-0.820*
	(0.289)	(0.276)	(0.334)	(0.324)	(0.465)	(0.455)
Currently smoking	-0.362***	-0.332***	-0.283**	-0.257*	-0.261	-0.255
	(0.118)	(0.111)	(0.139)	(0.133)	(0.185)	(0.184)
Ever smoked daily	-0.114	-0.115	-0.082	-0.063	0.075	0.056
	(0.113)	(0.106)	(0.135)	(0.130)	(0.192)	(0.189)
First stage F-stat	124.30	141.60	87.53	96.23	55.86	58.70
Obs	2,328	2,328	1,365	1,365	963	963
Contribution year FE	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓
Retirement age		✓		✓		✓

Notes: This table shows the effect on chronic diseases of an increase in pension income of 100 euros per month. Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator for eligibility for the pension subsidy. In addition to a list of controls, pension income without subsidy, birth cohort fixed effects and contribution year fixed effects in the odd columns, the even columns control for age at claiming pensions. Columns 1 and 2 show the results for the baseline sample. Columns 3 to 6 show the results for women and men respectively. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the SHARE-RV data.
† Sample size for these outputs is 1,426 (840 women). First stage F-statistics above 39.

Risky Behaviours. Finally, we examine risky behaviours, such as smoking and alcohol consumption, which are important risks that can lead to poorer health. Table 2.5 shows that an additional 100€ of monthly pension income reduces the days of alcohol consumption in the last 6 months by 82% of a standard deviation for men (significant at 10% level).[32] The overall probability of smoking at the time of the interview is reduced by about 3.3 percentage points for both men and women. We don't find any effect on the probability of ever having smoked on a daily basis. The reduction in drinking and smoking could be related to the lower stress levels resulting from the extra income.

All in all, these findings suggest that the reduction in mortality is driven by an improvement in overall health. In particular, more pension income leads to a reduction in the incidence of chronic diseases, including cataracts and high blood pressure, and an improvement in the ability to perform daily activities. We also show that better health outcomes may also partly be due to a better mental health status, as indicated by a reduction in the depression index, reduced stress about money, a more optimistic view of the future and a reduction in frequent alcohol consumption.

2.6 Discussion

2.6.1 Gender Differences

We find stark difference in mortality and health responses to additional pension income by gender. Men benefit from having additional pension income, while women are not affected. The analysis in Section 2.5.2.1 suggests that marital status and number of children do not explain the different gender responses. One explanation is that the composition of eligible women is more heterogeneous than that of men, even after controlling for marital status and number of children. Women with more than 35 contribution years and low average earnings can include, on the one hand, mothers with low attachment to the labor market but who are rewarded more pension contribution years due to childcare needs and, on the other hand, women who have worked for many years in low-paid jobs.

While Table 2.A.11 shows that when we examine women with very low wage (those with *aep* between 0.25 and 0.45), we indeed find some beneficial impacts of additional pension income on life expectancy. However, since men are the primary

[32] These variables are unfortunately only available for some respondents in the SHARE-RV baseline sample. Therefore, we report the minimum number of observations and value of first-stage F-statistics.

earners in most West German family, women with a low pension entitlement and low wage can either come from truly low-income families or from well-off families because the household income is high.

We use two measures to proxy for truly low-income families: a higher share of pension income in household income and non-homeowners households.[33] Table 2.A.18 shows the heterogeneous effects by the share of pension income in household income. We compare the results for people with a share of pension income above and below 50% of total household income.[34] Although suggestive, we find that women whose pension income makes up a higher proportion of overall household income are more likely to respond to the following health measures: feeling life is full of opportunities and future looks good.

Table 2.A.19 shows the heterogeneous effects on the list of health outcomes by home ownership status, which is an indicator of household wealth. Home ownership is defined by whether any household member owns at least one house/apartment. We find that men who do not own a home respond more strongly to additional pension income on many dimensions of health. We do not find that women who are not homeowners benefit more from additional pension income, except for feeling better about the future. This suggests that while reliance on pension income may partly explain men's health responses to additional income, it can't explain the different gender responses.

One alternative explanation for the gender difference in responses may stem from different working conditions across the lifespan, leading to varying pre-existing health conditions in men and women. Table 2.A.20 compares the mortality and health outcomes in the absence of the subsidy by gender. We proxy the outcomes in the absence of the subsides by looking at the pensioners who have more than 35 years of contributions but with *aep* higher than 0.75. Indeed, we find that potentially treated men are overall less healthy than women.

To further test the differences in health between eligible men and women, we utilise the scientific use file of the Insurance Account Sample (VSKT, administrative data from the German Pension insurance) 2002, 2003-2006 waves, which contains biographical information on a random sample of individuals with an active public

[33] The correlations between these two measures and marital status are not very high, suggesting that marital status does not capture these characteristics. The correlation between the share of pension income in total household income and being married is -0.24. And the correlation between one of the household members being a home owner and being married is 0.04. These two measures also capture different families, as the correlation between one of the household members being a home owner and the share of pension income is -0.17.

[34] Note that the sample size is almost halved as we only observe household income for some of the respondents. If we further divide it by income share and by gender, the sample size becomes even smaller, with the result that the F-statistic for men is below 10.

pension insurance account in Germany in 2002, 2003 to 2006. We make the same sample restrictions as our baseline sample. We examine whether those eligible individuals are healthier or unhealthier before claiming the subsidy and whether the difference varies by gender. Table 2.A.21 shows the effect of eligibility on the duration of sickness and the probability of experiencing any sickness before age 50 (and age 55). The estimates are positive and significant. On average, individuals eligible for the subsidy claim sickness leave benefits for one month more and they are 0.07% more likely to experience any sickness leave before age 50. When we divide the sample by gender, we can see that men drive the results. Eligible men claim 5 months more sickness leave before age 50, while eligible women claim a half-month more sickness leave before age 50. The results on sickness before age 55 are similar. This finding implies that eligible individuals are less healthy, which is not surprising given that the subsidy program is targeted at poorer retirees. Moreover, eligible men are in much worse health than eligible women. This could be the reason why we see a large mortality reduction in our context, because the subsidy program targets predominantly low-income and poor health beneficiaries, similar to the disability insurance recipients in the U.S. (Gelber et al., 2023) and low-income pensioners in rural China (Cheng et al., 2018; Huang and Zhang, 2021).

2.6.2 Comparisons with Existing Literature

In Table 2.1, we show that eligibility for the pension subsidy increases pension income by 8.2% (58/706) and reduces the probability of dying before age 65, and 70 by 14.5% (0.008/0.055), and 5.9% (0.015/0.253), respectively. Therefore, we estimate ITT pension income-mortality elasticities of -1.77, and -0.72, which represent the percentage change in the probability of dying before age 65, and 70 due to a 1% increase in pension income.[35]

To understand the estimated mortality and health responses, we compare our results with the existing literature. The effect of an increase in pension income on mortality is not necessarily symmetric with the effect of a decrease in pension income, therefore, we make separate comparisons between studies which focus on studying increases or decreases in pension income.

First, we compare our estimates with evidence on mortality responses to an increase in pension income (Case, 2004; Barham and Rowberry, 2013; Huang

[35] The IV estimates in Table 2.3 show monthly pension income increase by 100 euro (around 14 percent increase) leads to a decrease in the probability of dying before age 65, and 70 of 25.5% (0.014/0.055), and 10.3% (0.026/0.253), respectively. The pension income-mortality elasticities calculated using the IV estimates are similar: -1.79, -0.73.

and Zhang, 2021; Miglino et al., 2023), the majority of which explore the non-contributory pension in developing countries. Our pension income-mortality elasticity of -0.72 (probability of dying before age 70) is at the higher end compared to these studies. For example, Barham and Rowberry (2013) study the phasing-in of the Mexican conditional cash transfer program, Progresa, between 1997 and 2000, which led to an increase in average beneficiary income levels of 22% in rural areas. They find a 4% decline in average municipality-level mortality for people aged 65 and above. This translates to an income-mortality elasticity of -0.18. Miglino et al. (2023) study the effect of income on mortality by exploring the eligibility condition for the non-contributory pension program in Chile. They find the basic pension increases income by 72% and reduces four-year mortality by 28%, leading to an income–mortality elasticity of -0.386. In comparison, our estimated pension income-mortality elasticity is similar to the findings of Huang and Zhang (2021), which find an income-mortality elasticity of -0.67, similar to our estimate. Huang and Zhang (2021) examine the implementation of China's New Rural Pension Scheme, which increased the household income by 18%. They find that the pension scheme reduced one-year mortality by 2.2 percentage points (12%) among the treated group.

Second, we compare our estimates with evidence on mortality responses to decreases in pension income (Jensen and Richter, 2004; Snyder and Evans, 2006; Johnsen and Willén, 2022). While Jensen and Richter (2004) find lower income leads to higher mortality by exploring a crisis in Russia in 1996, during which many pensioners were not paid for an extended period, Snyder and Evans (2006) show lower pension income leads to reduced mortality by examining a variation in social security wealth for the U.S. “notch" cohort. In addition, Johnsen and Willén (2022) shows negative income shocks had no impact on both employment and health care utilisation of pensioners in Sweden. Specifically, Jensen and Richter (2004) finds an income-mortality elasticity of -0.20. They find that the crisis decreased household income by 24% for these pensioners and they were 5% more likely to die in the two years following the crisis. Snyder and Evans (2006) find an income-mortality elasticity of 0.5, and that lower pension income leads to reduced mortality, which they attribute to beneficial effects of employment. They find a 4% drop in income leads to a reduction in five-year mortality by 2%.

The relatively large income-mortality elasticities suggested by our findings are likely due to the fact we study the impact on low-income retirees, who are likely the most vulnerable among the population with limited resources. For example, Gelber et al. (2023) study the impact of more generous Disability Insurance (DI) benefits on mortality and find a large impact on low-income DI beneficiaries. They show

$1,000 more in annual disability insurance payment in the U.S. reduces mortality of low-income beneficiaries by 0.18 to 0.35 percentage points, implying an elasticity of mortality with respect to DI income of around -0.6 to -1.0. The magnitude of their elasticity is similar to ours.

Interestingly, Becker et al. (2024) find no mortality effects of increased DI benefits in Germany for DI recipients below age 60. However, the populations at study are clearly different. Becker et al. (2024) focus on mortality effects of younger people with server health conditions, while we focus on older people with low income.

Lastly, comparing our findings to studies in the medical literature, our results are unsurprising. Compared with studies on patients cutting back high-value drugs (e.g., statins, antihypertensives for cardiovascular and steroids, inhalers for respiratory) (Brot-Goldberg et al., 2017; Chandra, Flack, and Obermeyer, 2021), our estimates show a similar size of the impacts on mortality. For example, Chandra, Flack, and Obermeyer (2021) find that an exogenous 100$/month decrease in Medicare's drug coverage, a 24.4% change) causes mortality to increase by 0.016 percentage points per month (13.9%). Although the majority of the German population is covered by statutory health insurance, out-of-pocket payments for healthcare services remain and account for 13% of total healthcare expenditure in Germany (WHO, 2023).[36] One possible channel of our results could be that additional income allows recipients to pay out-of-pocket for additional drugs and medical supplies. Unfortunately, the out-of-pocket payments is poorly recorded in SHARE, we can not test this possibility empirically. Moreover, when compared with studies on correlation between different causes of mortality risk and the switch from a sedentary to a moderately active lifestyle, our estimated effects on mortality rates imply similar benefits to regularly engaging in moderately intensity physical activity.[37]

2.6.3 Policy Implications

Over the past few decades, there is a large and widening gap in life expectancy across income groups in many countries, including Germany (Tarkiainen et al.,

[36] Bock et al. (2014) shows that the top three highest amount of out-of-pocket payments for elderly German public health insurance beneficiaries are medical supplies (such as electrical wheelchair), dental prostheses and payments for pharmaceuticals. See here for a detailed description of the co-payment regulations of the statutory health insurance in Germany.

[37] Various studies have shown a correlation between different causes of mortality risk and the switch from a sedentary to a moderately active lifestyle. For example, Richardson et al. (2004) show that regular physical activity can reduce overall mortality of U.S. adults aged between 51 and 61 by 38% compared with sedentary individuals. Baade et al. (2011) find that colorectal cancer patients engaging in some level of physical activity after the diagnosis had a 25% to 28% lower risk of all-cause mortality within five years of diagnosis than sedentary participants in Australia.

2012; Wenau, Grigoriev, and Shkolnikov, 2019; Haan, Kemptner, and Lüthen, 2020). The improvement in mortality is the largest in the high-income group and the smallest in the low income group. Haan, Kemptner, and Lüthen (2020) shows that for West German men born in 1932-1934, the gap in life expectancy at age 65 between the top and bottom of the earnings decile is 4 years; while for cohorts born in 1941-1943, this gap increased to close to 7 years [38]. At the same time, younger cohorts receive less subsidy as this subsidy program is gradually being phased out.[39] From a policy perspective, it would be interesting to know how this life expectancy gap would change if the subsidy level remain at a high level. Our IV estimates imply that if men born between 1941-1943 had received the equivalent subsidy as those born between 1932-1934, their life expectancy at age 65 would have increased by one month. This adjustment would consequently narrow the gap by one month, constituting approximately 3% of the overall disparity. This simple exercise suggests that providing additional pension income to low-income pensioners would help to flatten the income-mortality gradient.

We also perform a simple cost-benefit analysis by computing the associated increase in the value of a statistical life when receiving an additional 100€ pension benefits per month. Using the value of a statistical life year at age 60 implied by Aldy and Viscusi (2007) and life tables for the average German (Destatis, 2023), we show that for each 100€ subsidy, the mortality improvements for men are worth 183,785€.[40] The fiscal cost of providing the subsidy for men is about 31,224€ per male subsidy recipient (assuming they retire at age 60). Compared with the fiscal cost of providing the subsidy, we show that the pension subsidy program was cost-effective in increasing the life expectancy of male recipients.[41] See Appendix 2.E for more details.

[38] Figure 2 of Haan, Kemptner, and Lüthen (2020)

[39] This is because low-income workers who never contributed to the pension system before 1992 will not benefit from this subsidy program. Eligible West German men born in 1932-1934 received, on average 90€/month, of pension subsidy, while this number is reduced to 43€/month for younger cohorts.

[40] Our calculation of improvement in life expectancy at age 60 is a lower bound because the gain in life expectancy is benchmarked to the life tables for an average German, rather than the poorest German pensioners, who likely experience higher-than-average mortality rates.

[41] We also perform a similar cost-benefit analysis for women. We take a pension income without subsidy of 623€/month (average value for eligible women) and a life-expectancy improvement of 1.9 months (based on non-significant 1 percentage point decrease in the probability of dying before 70). Life expectancy at age 60 is 25.41 years (305 months) for women. Additionally, we estimate eligible women retire about 3 months earlier. Thus, the cost of providing a retired woman with an additional 100€/month would be 34,249.8€. This implies a net monetary gain of about 7,479€ per subsidy recipient. However, given the effects on women are not statistically significant, one should be cautious when concluding that such a policy would be or not be cost-effective for women.

2.7 Conclusion

This paper estimates the impact of pension income on the mortality and health for low-income pensioners by exploiting a German pension subsidy program. The specific feature of the program allows us to identify the effect of additional pension benefits on mortality in an environment where the statutory pension eligibility ages remain unchanged and also the retirement timing responses are limited.

By utilising a novel administrative data covering the universe of retirees who died between 1994 and 2018, we find that eligibility leads to a permanent increase in monthly pension income of 8.2% (about 58€) and a 2-month delay in age at death (censored at 75). The IV analysis shows that a 100€ increase in monthly pension income (about 14% increase) reduces the probability of dying before age 65, and 70 by 25.5%, and 10.3%, respectively. The heterogeneity analysis suggests that the mortality responses are driven by men. The analysis using survey data suggests that additional pension income also leads people to live healthier lives. Again, we find that men's health improves while women's health does not. We find significant improvements in both mental and physical health for men. Feeling less financially constrained, feeling more optimistic about the future and life chances, and reducing alcohol and cigarette consumption are possible drivers of improved health.

The external validity of the results could be questioned given that the subsidy recipients consist of pensioners with lower-than-average earnings in Germany. However, the recent trend of lowered public pension generosity to incentivize later retirement has left a growing number of lower-income workers vulnerable to old-age poverty risk in many developed countries (see e.g., Engelhardt and Gruber (2004), Cribb and Emmerson (2019), and Morris (2022)). People with lower incomes have greater health risks and are the ones most in need of income support. Our findings can be used to consider the beneficial effects of providing safety nets for low-income pensioners in countries with similar contexts.

The main policy implication is that additional pension income improves life expectancy and leads to better physical and mental health for low-income pensioners. The findings further support income support programs for the elderly, as the social value is greater than the fiscal costs. Moreover, additional pension income for low-income retirees could flatten the income-mortality gradient and narrow the socioeconomic disparities in old-age mortality.

An interesting extension to this paper will be to further unpack the gender differences in responses to additional pension income. A caveat of this paper is that we cannot link household members and further explore the differential gender responses in mortality and health, which may be a fruitful avenue for future research.

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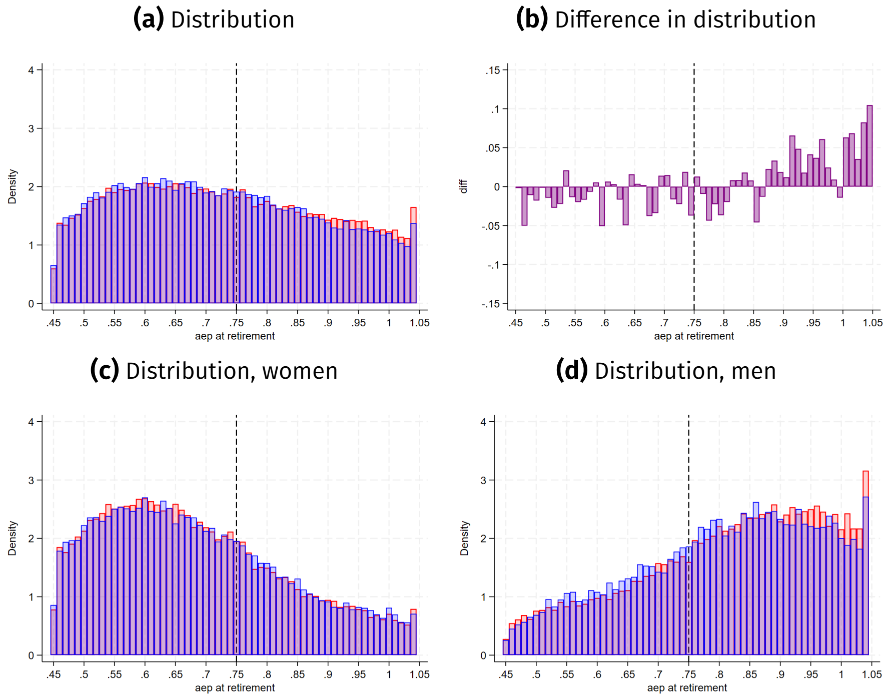
Appendices to Chapter 2

Appendix 2.A Additional Figures and Tables

Figure 2.A.1. Relationship between subsidy size and aep^{92}.

Notes: Figure 2.A.1 displays the relationship between subsidy size and average earning points before 1992, in the case of an individual that contributed 19 years to the pension system before 1992. Horizontal axis indicates aep^{92} and corresponding monthly wage in parenthesis, while vertical axis indicates the additional earning points the individual is entitled to from the subsidy program, and the corresponding monetary subsidy amount (in 2015€) in parenthesis.
Source: Figure 1 in Ye (2022).

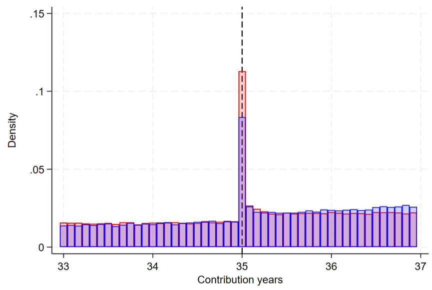
Figure 2.A.2. Distribution of *aep* by contribution years above / below 35, full sample and by gender



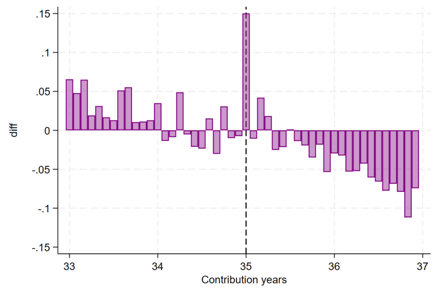
Notes: Figure 2.A.2 (a) displays the distribution of *aep* for groups with contribution years above ("above 35", red) and below 35 ("below 35", blue) in the baseline sample. Figure 2.A.2 (b) depicts the difference in density between the "above 35" and the "below 35" group. Figure 2.A.2 (c) and (d) display the distribution for women and men.

Figure 2.A.3. Distribution of contribution years by *aep* below and above 0.75, full sample and by gender

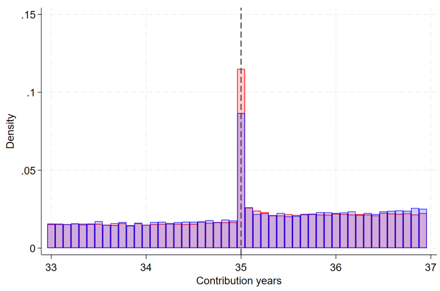
(a) Distribution



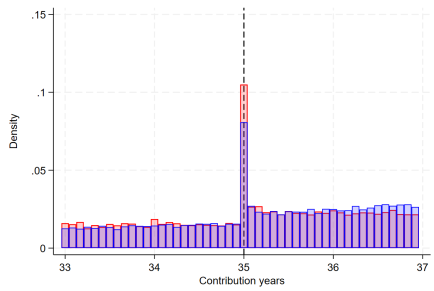
(b) Difference in distribution



(c) Distribution, women

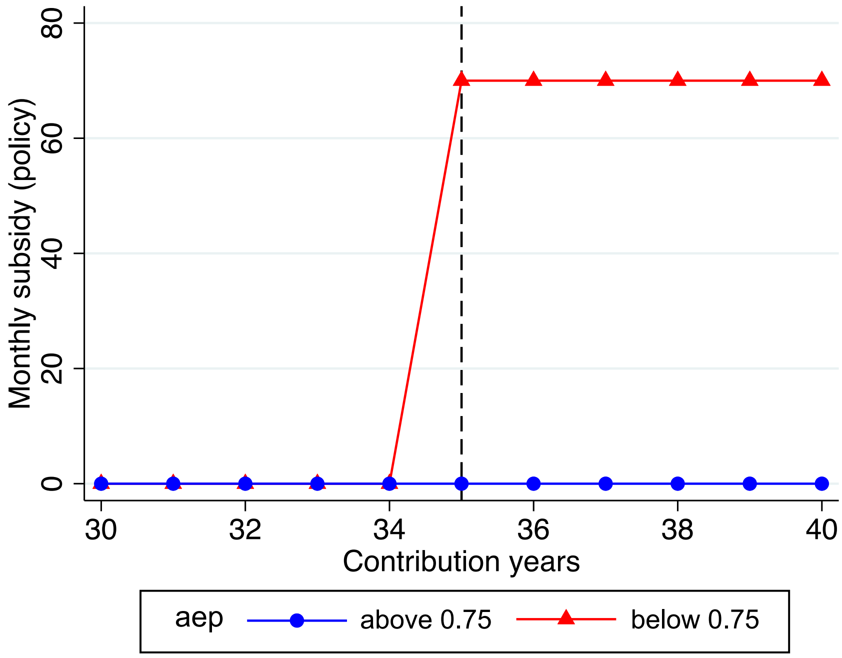


(d) Distribution, men



Notes: Figure 2.A.3 (a) displays the distribution of contribution years for groups with *aep* above (blue) and below 0.75 (red) in the baseline sample. Figure 2.A.3 (b) shows the difference in distribution between below 0.75 and above 0.75 groups. Figures (c) and (d) display the distribution for women and men.

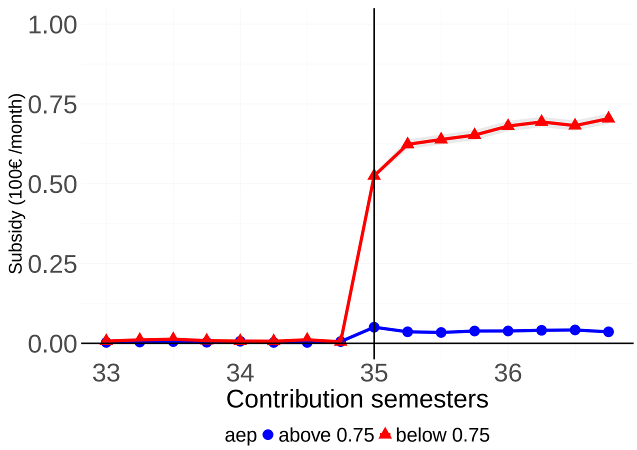
Figure 2.A.4. Policy schedule of subsidy size by contribution years and by treatment status



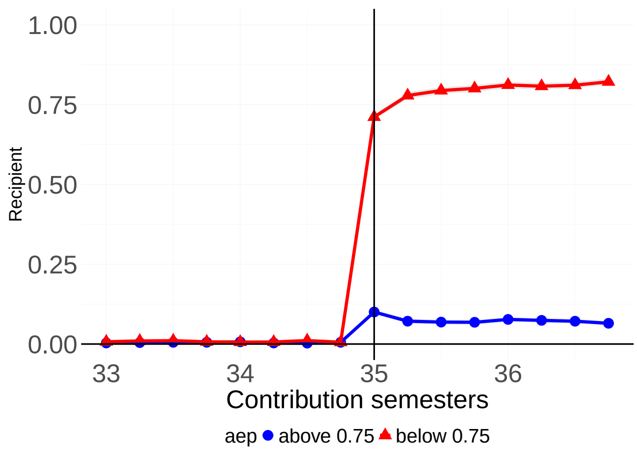
Notes: Figure 2.A.4 displays the pension subsidy schedule by contribution years and by treatment status according to the policy. The control group (blue dots) consists of pensioners with average earnings points at retirement higher than 0.75 and lower than 1.05, while the treatment group (red triangles) consists of pensioners with average earnings points at retirement between 0.45 and 0.75. The average monthly subsidy of 65 euro is the sample mean for treated pensioners with 35 to 40 years of contributions.

Figure 2.A.5. First stage: mean probability of being a recipient and amount of pension subsidy by contribution years.

(a) Pension subsidy (100€)

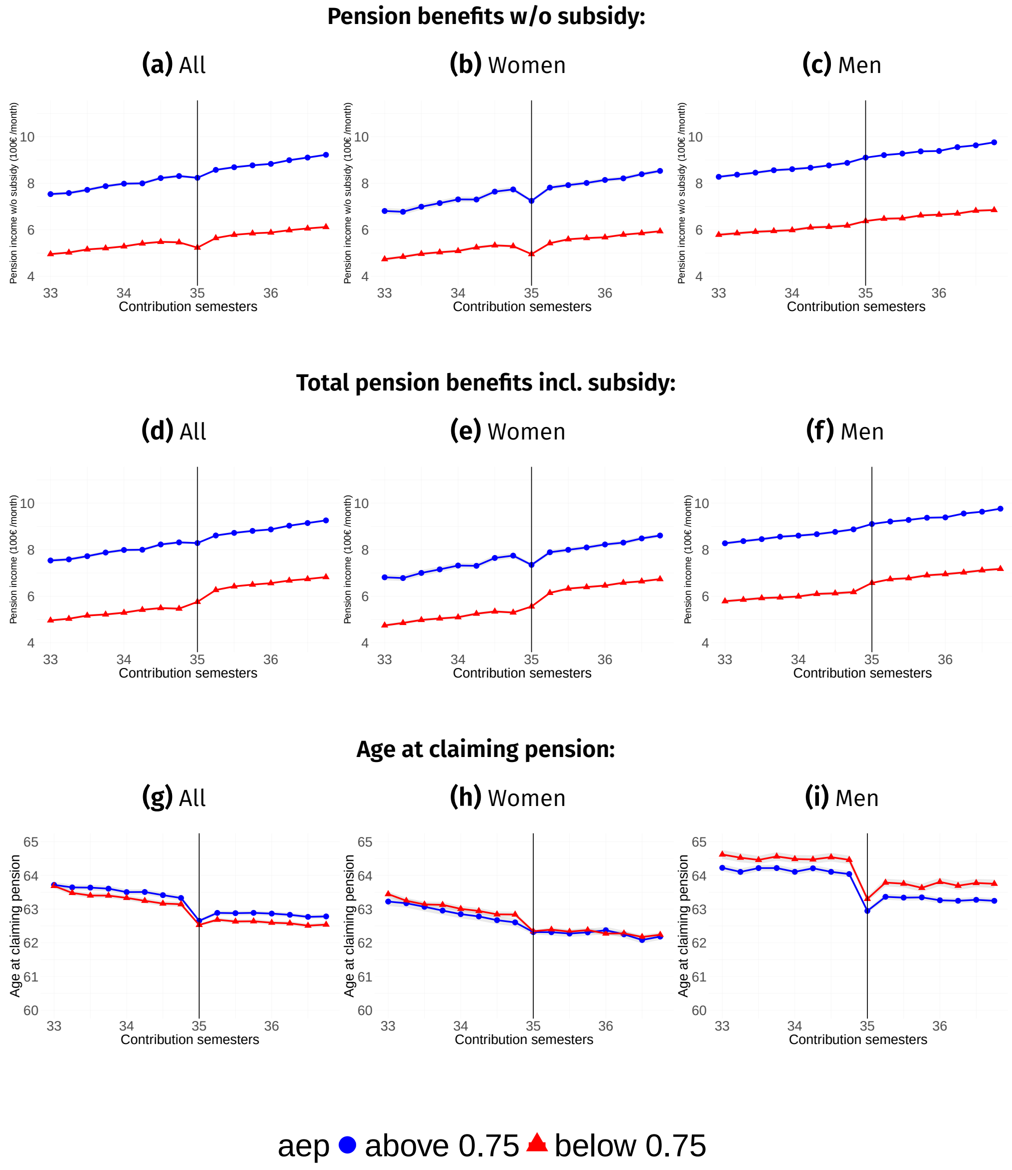


(b) Probability of being a recipient



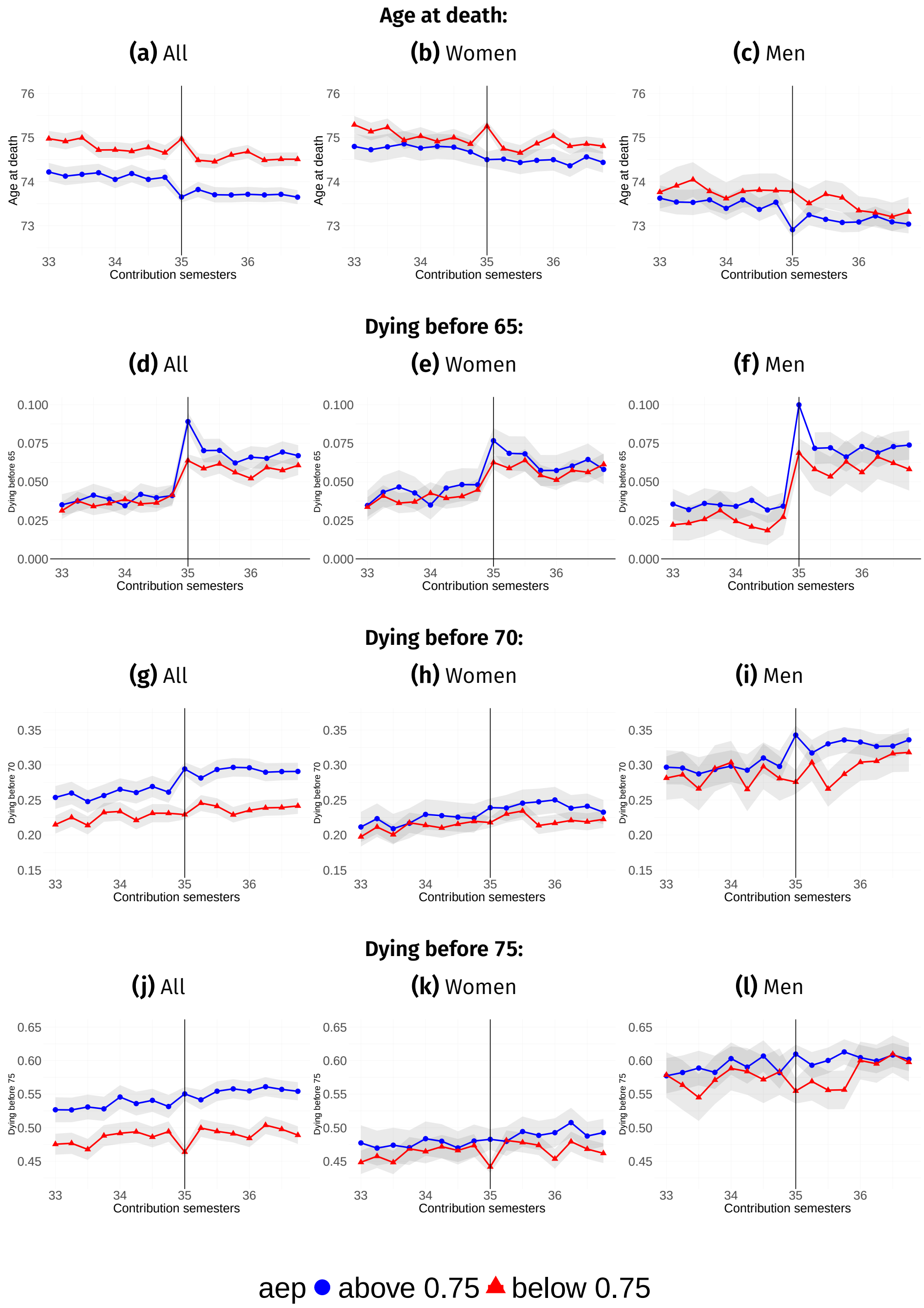
Notes: Figure 2.A.5 displays the mean amount of pension subsidy (panel (a)) and the mean probability of being a subsidy recipient (panel (b)) by number of contribution years. Within the baseline sample, “$aep \geq 0.75$" (blue circles) indicates individuals with *aep* between 0.75 and 1.05 (pension benefits between 1050 and 1500€/month) while “$aep < 0.75$" (red triangles) indicates individuals with *aep* between 0.45 and 0.75 (pension benefits between 600 and 1050€/month). Monetary values are expressed in hundred 2015 euro. The shadowed areas indicate the normally distributed 95% confidence interval.

Figure 2.A.6. Scatter plot of pension income and age at claiming over contribution years by treatment status by gender



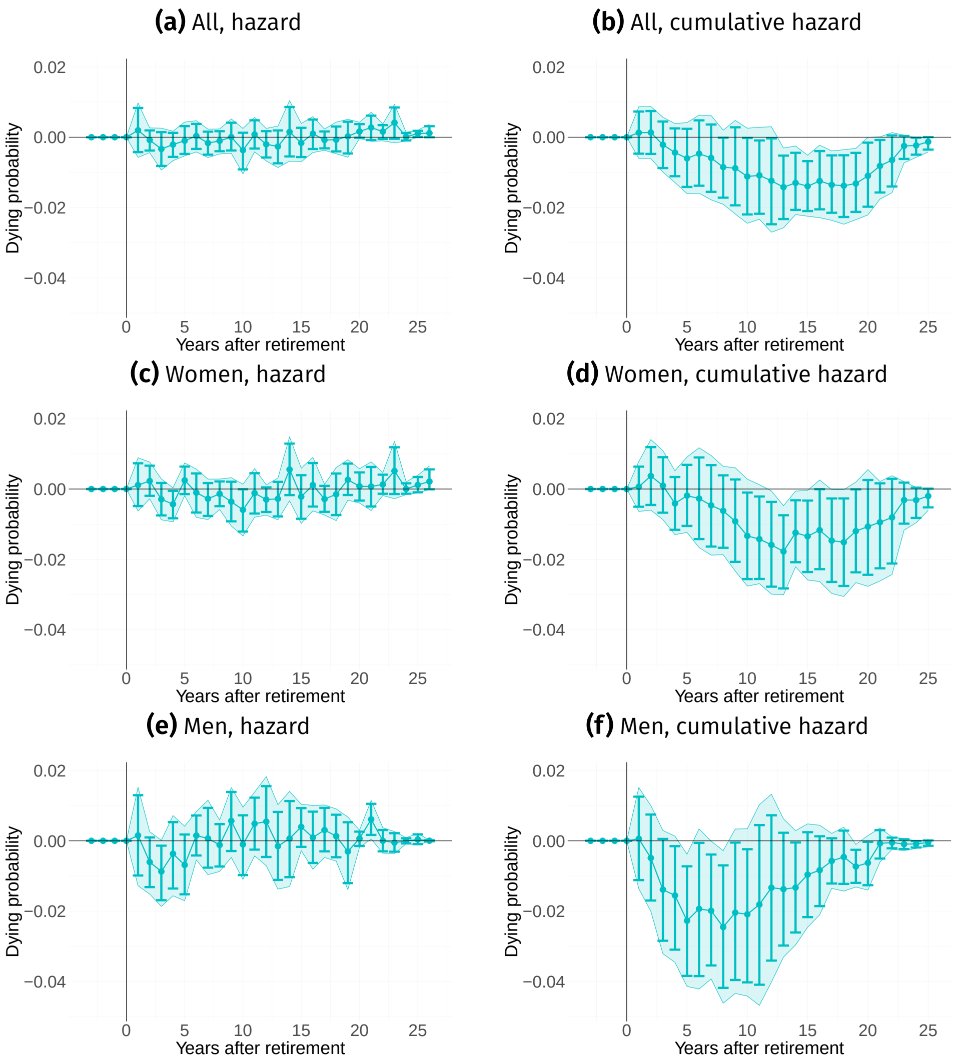
Notes: Figure 2.A.6 displays the pension benefits with and without subsidy and age at claiming pension by treatment over contribution years by gender. The blue circles indicate individuals with *aep* between 0.75 and 1.05 (pension benefits between 700 and 1200€/month), while the red triangles indicate individuals with *aep* between 0.45 and 0.75 (pension benefits between 500 and 850€/month). Monetary values are expressed in hundred 2015 euro. The shadowed areas indicate the normally distributed 95% confidence interval.

Figure 2.A.7. Scatter plot of mortality outcomes over contribution years by treatment status by gender



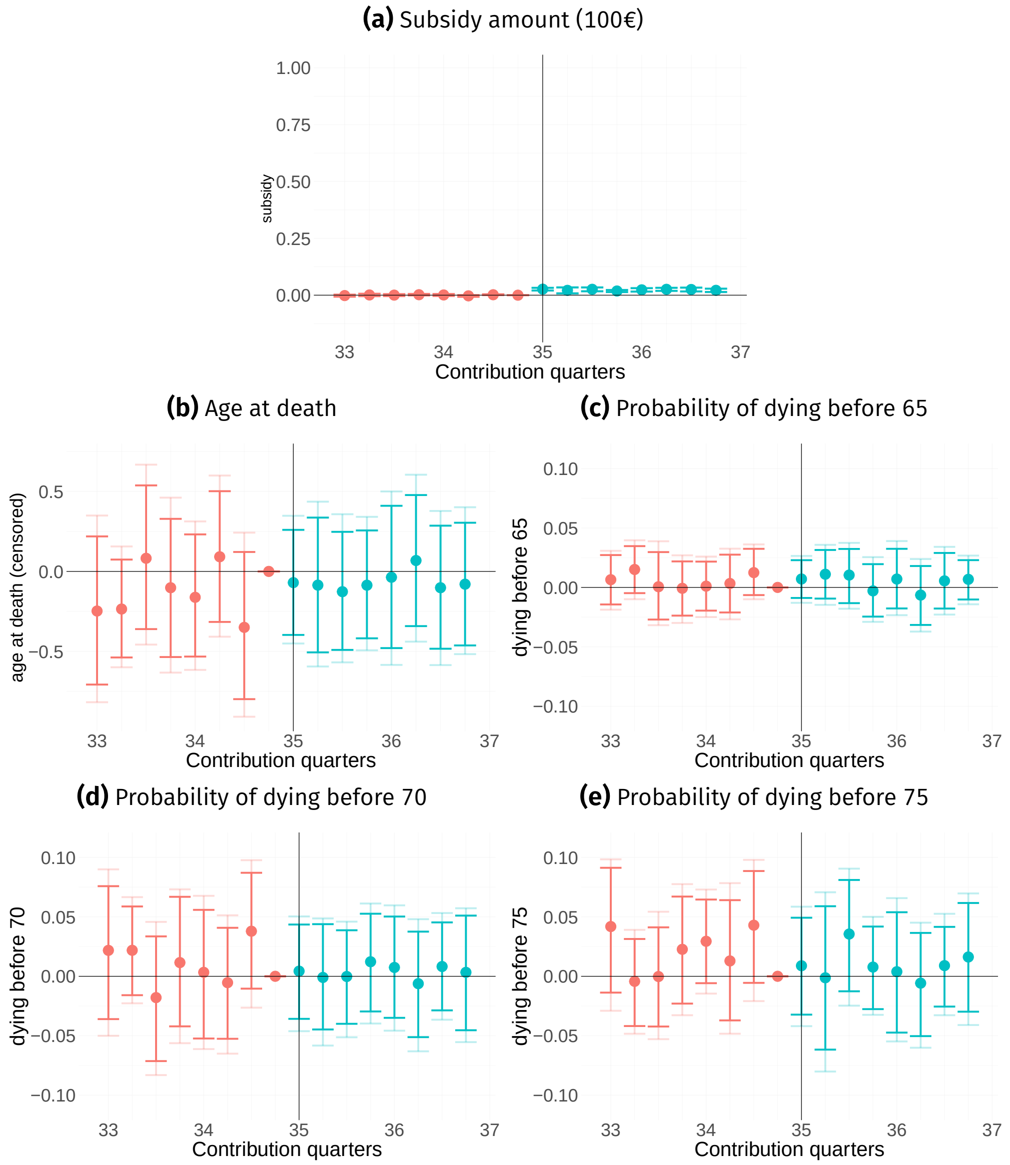
Notes: Figure 2.A.7 displays the mean mortality outcomes over contribution years by *aep* group by gender. The shadowed areas indicate the normal 95% confidence interval.

Figure 2.A.8. Effect of eligibility on the probability of dying after retirement



Notes: The left-hand column of Figure 2.A.8 shows the reduced-form effects of eligibility for the a subsidy on the probability of dying in each year after retirement. That is, the impacts on the probability of dying within one year after retirement, between 1 and 2 years after retirement, 2 and 3 years, etc. The right-hand column of Figure 2.A.8 shows the reduced-form impact of eligibility for the a subsidy on the probability of dying at each year after retirement. That is, the effects on the probability of dying 1 year, 2 years, ... and 15 years after retirement. All figures show the 95 percent CI (shaded area) and the 90 percent CI (solid line).

Figure 2.A.9. Placebo checks: event study coefficients in the *aep* ∈ (0.8-1.25) placebo sample.



Notes: Figure 2.A.9 displays the event study coefficients for pension income (first stage) and mortality outcomes in the *aep* ∈ (0.8-1.25) placebo sample. Placebo cut-off at *aep* = 1. All subfigures plot the 95 percent CI (shadowed line) and 90 percent CI (solid line).

Table 2.A.1. Summary statistics (RTWF)

	West German Pensioners			Cohorts 1932 - 1942			Baseline Sample		
	Mean	Std.Dev.	N	Mean	Std.Dev.	N	Mean	Std.Dev.	N
Mortality outcomes									
Age at death	75.10	6.93	4,442,649	74.34	5.76	2,612,036	74.33	5.82	149,053
Age at death	72.17	3.75	4,442,832	72.27	3.69	2,612,035	72.24	3.72	149,053
Dying before 60	0.00	0.00	4,442,649	0.00	0.00	2,612,036	0.00	0.00	149,053
Dying before 65	0.05	0.23	4,442,649	0.05	0.23	2,612,036	0.05	0.23	149,053
Dying before 70	0.27	0.44	4,442,649	0.25	0.43	2,612,036	0.25	0.43	149,053
Dying before 75	0.50	0.50	4,442,649	0.51	0.50	2,612,036	0.51	0.50	149,053
Pension income and subsidy related variables									
Pension income (PI, 100€)	9.67	5.84	4,442,832	10.37	5.77	2,612,035	7.06	2.25	149,053
Subsidy (100€)†	0.14	0.47	4,442,510	0.14	0.46	2,612,035	0.878	0.60	149,053
Subsidy recipient	0.13	0.33	4,442,649	0.13	0.33	2,612,036	0.30	0.46	149,053
PI w/o subsidy (100€)	9.53	5.90	4,442,510	10.22	5.84	2,612,035	6.83	2.32	149,053
Pension related characteristics									
CY	35.45	11.15	4,442,649	36.48	10.72	2,612,036	34.73	1.06	149,053
CY>35	0.64	0.48	4,442,649	0.69	0.46	2,612,036	0.63	0.48	149,053
aep	0.91	0.32	4,439,960	0.94	0.32	2,610,792	0.73	0.16	149,053
aep <0.75	0.34	0.47	4,442,649	0.30	0.46	2,612,036	0.56	0.50	149,053
Age at claiming pension	63.86	3.08	4,442,548	63.11	2.54	2,612,035	62.96	2.35	149,053
% claim disability pension	0.13	0.33	4,442,649	0.14	0.35	2,612,036	0.14	0.35	149,053
% claim unemployment pension	0.12	0.33	4,442,649	0.17	0.38	2,612,036	0.08	0.27	149,053
% claim women pension	0.09	0.29	4,442,649	0.13	0.33	2,612,036	0.25	0.43	149,053
Individual characteristics									
Birth year	1935.36	6.15	4,442,649	1936.41	2.97	2,612,036	1936.29	2.98	149,053
% male	0.58	0.49	4,442,649	0.61	0.49	2,612,036	0.35	0.48	149,053
% married	0.61	0.49	4,442,649	0.68	0.47	2,612,036	0.60	0.49	149,053
Number of children*	2.04	1.46	1,874,487	2.03	0.001	1,021,029	2.19	1.47	96,820
% private health insurance	0.11	0.31	4,442,649	0.11	0.31	2,612,036	0.09	0.29	149,053
% public health insurance	0.84	0.37	4,442,649	0.84	0.36	2,612,036	0.87	0.33	149,053

Notes: Table 2.A.1 reports descriptive statistics for the *West German Pensioners* sample, the *1932-1942* sample and the *baseline* sample. *West German Pensioners* sample restricts to those who died between 1994 and 2018 and were residing in West Germany, holding German citizenship and claiming old-age pension at time of death. *1932-1942* sample further restricts to individuals born between 1932 and 1942. *Baseline* sample further adds *aep* (average earning points from full contribution periods) and contribution years (CY) restrictions, respectively to the bandwidths of 0.45 - 1.05 and 33 - 36. Number of children is imputed from child-benefits claims. † conditional on being a recipient. * only reported for women in the sample. *Source*: Authors' calculations from the RTWF data.

Table 2.A.2. Summary statistics by gender by treatment status (RTWF)

	Women						*Men*					
	Baseline		**Treatment** ***aep*** **∈ [0.45, 0.75)**		**Control** ***aep*** **∈ [0.75, 1.05)**		**Baseline**		**Treatment** ***aep*** **∈ [0.45, 0.75)**		**Control** ***aep*** **∈ [0.75, 1.05)**	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Mortality outcomes												
Age at death	72.53	3.63	72.52	3.68	72.53	3.59	71.71	3.82	71.83	3.82	71.68	3.82
Dying before 65	0.05	0.22	0.06	0.24	0.05	0.22	0.06	0.23	0.06	0.24	0.06	0.23
Dying before 70	0.22	0.42	0.22	0.42	0.22	0.42	0.31	0.46	0.29	0.46	0.31	0.46
Dying before 75	0.47	0.50	0.46	0.50	0.47	0.50	0.59	0.49	0.58	0.49	0.60	0.49
Pension income and subsidy related variables												
Pension income (PI, 100€)	6.40	2.21	6.23	1.79	6.53	2.47	8.29	1.77	6.86	1.07	8.65	1.73
Subsidy (100€)	0.33	0.53	0.72	0.59	0.04	0.18	0.06	0.24	0.27	0.48	0.00	0.03
Subsidy recipient	0.40	0.49	0.86	0.34	0.06	0.24	0.10	0.30	0.45	0.50	0.01	0.09
PI w/o subsidy (100€)	6.07	2.21	5.51	1.69	6.49	2.44	8.23	1.81	6.59	1.02	8.65	1.73
Pension related characteristics												
CY	34.70	1.06	35.42	0.49	34.16	1.05	34.79	1.05	35.43	0.49	34.62	1.09
CY>35	0.62	0.49	1.00	0.00	0.33	0.47	0.65	0.48	1.00	0.00	0.56	0.50
aep	0.68	0.15	0.60	0.08	0.75	0.16	0.81	0.15	0.63	0.08	0.86	0.13
aep <0.75	0.69	0.46	1.00	0.00	0.46	0.50	0.32	0.47	1.00	0.00	0.15	0.36
Age at claiming pension	62.57	2.37	62.31	2.26	62.77	2.43	63.68	2.13	63.63	2.08	63.69	2.15
% disability pension	0.13	0.33	0.19	0.40	0.08	0.27	0.17	0.37	0.23	0.42	0.15	0.36
% unemployment pension	0.03	0.18	0.03	0.17	0.04	0.19	0.15	0.36	0.09	0.29	0.17	0.38
% women's pension	0.38	0.49	0.36	0.48	0.40	0.49	0.00	0.00	0.00	0.00	0.00	0.00
Individual characteristics												
Birth year	1936.29	2.98	1936.04	2.97	1936.47	2.98	1936.31	2.97	1936.64	2.95	1936.22	2.96
% married	0.59	0.49	0.61	0.49	0.58	0.49	0.61	0.49	0.62	0.49	0.60	0.49
Number of children†	2.19	1.47	2.31	1.46	2.10	1.48	0.11	0.55	0.09	0.51	0.11	0.57
% private health insurance	0.06	0.23	0.05	0.23	0.06	0.24	0.15	0.36	0.22	0.41	0.14	0.34
% public health insurance	0.91	0.29	0.91	0.28	0.91	0.29	0.80	0.40	0.73	0.44	0.82	0.39
Observations	96,820		41,472		55,348		52,233		10,719		41,514	

Notes: Table 2.A.2 reports descriptive statistics for *women* and *men* in the *baseline* sample, treatment and control groups. *Baseline* sample restricts to those who died between 1994 and 2018 and were residing in West Germany, holding German citizenship and claiming old-age pension at time of death, born between 1932 and 1942 and *aep* (average earning points from full contribution periods) between 0.45 and 1.05 and contribution years (CY) between 33 and 36. Treatment group is defined as those with $aep < 0.75$ while control group are those with $aep \geq 0.75$. *Source*: Authors' calculations from the RTWF data.

†Number of children is imputed from child-benefits claims.

Table 2.A.3. Sample selection.

	From full RTWF sample to baseline		
	All	Women	Men
	(1)	(2)	(3)
Eligibility ($D \times Above35$)	-0.008	-0.012	-0.002
	(0.008)	(0.008)	(0.006)
	[0.391]	[0.266]	[0.719]
aep$<$ 0.75	0.004	-0.000	0.006
	(0.006)	(0.005)	(0.007)
	[0.531]	[0.922]	[0.459]
Obs	9,484,551	3,080,889	6,403,662
Adj. R-sqr	0.362	0.472	0.263
Contribution year FE	✓	✓	✓
Birth cohort FE	✓	✓	✓
Controls	✓	✓	✓
PI without subsidy	✓	✓	✓

Notes: Baseline sample defined as: residing in West Germany, holding German nationality, perceiving old-age pension, cohorts 1932 - 1942, aep_i between 0.45 and 1.05, contribution years between 33 and 36. Full sample includes anyone who died after age 60 while claiming or contributing to any pension, and born after 1900. Controls include indicators for having children, marital status, and not having health insurance. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values are in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.4. Impact on mortality before 60 and after 75 (DID estimates)

Birth cohorts	1945-1955	1932-1937	1922-1931
	(1)	(2)	(3)
Panel A: All			
Dying between 50-60	0.019		
	(0.015)		
	[0.227]		
Dying between 75-80		0.009	
		(0.008)	
		[0.406]	
Dying between 80-85			-0.003
			(0.003)
			[0.431]
Panel B: Women			
Dying between 50-60	0.016		
	(0.028)		
	[0.582]		
Dying between 75-80		0.005	
		(0.014)	
		[0.938]	
Dying between 80-85			0.014
			(0.008)
			[0.156]
Panel C: Men			
Dying between 50-60	0.000		
	(0.016)		
	[0.984]		
Dying between 75-80		0.027	
		(0.013)	
		[0.094]	
Dying between 80-85			-0.005
			(0.009)
			[0.590]
Obs (all)	29,805	87,409	100,631
Obs (women)	15,334	55,128	60,144
Obs (men)	14,471	32,281	40,487
Contribution year FE	✓	✓	✓
Birth cohort FE	✓	✓	✓
Controls	✓	✓	✓
PI without subsidy	✓	✓	✓

Notes: Column (1) shows the impact on the probability of dying between 50 and 60 by using cohorts born between 1945 and 1955, for which we observe the complete death between ages 50 and 60. Because some of these individuals died before claiming pension, we assume they would have retired at age 63 had they not died. Column (2) shows the impact on the probability of dying between 75 and 80 by using cohorts born between 1932 and 1937, for which we observe the complete death counts between ages 75 and 80. Column (3) shows the impact on the probability of dying between 80 and 85 by cohorts born between 1922 and 1931, for which we observe the complete death counts between ages 80 and 85. All regressions restrict to individuals with 33 - 36 contribution years and *aep* between 0.45 and 1.05. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.5. Definition of health, financial constraints and optimism variables in SHARE-RV data

	Definition	Scale
CASP	Quality-of-life scale for early old-age individuals considering both mental and physical health.	0-57, the higher the better
Self-reported health	Self-reported evaluation of own's health.	0 (Poor) - 4 (Excellent)
Depression index	EURO-D Measure of Depressive Symptoms in the Aging Population, measured as number of reported symptoms of depression.	0 - 12
Number of chronic diseases	Number of chronic diseases currently treated for: heart attack, high blood pressure, high blood cholesterol, stroke, diabetes, chronic lung disease, cancer, ulcer, parkinson, cataracts, hip femoral fracture.	0 - 11
Difficulties with ADLs	Difficulties with Activities of Daily Living.	0 - 5
Difficulties with IADLs	Difficulties with Instrumental Activities of Daily Living.	0 - 3
Stroke	Currently treated for stroke or cerebral vascular disease.	0 - 1
Chronic lung disease	Currently treated for chronic lung disease.	0 - 1
Cataracts	Currently treated for cataracts.	0 - 1
High blood pressure	Currently treated for high blood pressure.	0 - 1
Low money stops	How often does money stop from doing things.	0 (never) - 3 (often)
Full of opportunities	How often feels life is full of opportunities.	0 (never) - 3 (often)
Future looks good	How often future looks good.	0 (never) - 3 (often)
How often consumed alcohol	How often consumed alcohol in the past six months.	1 (not at all) - 7 (almost every day)
Is currently smoking	Regular smoker at the time of the interview.	0 - 1
Ever smoked daily	Has ever smoked on a daily basis.	0 - 1

Notes: Table 2.A.5 describes main output variables used from the SHARE-RV. *Source*: SHARE data documentation.

Table 2.A.6. Summary statistics (SHARE-RV)

	West German Pensioners		Baseline Sample		Restricted Sample	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Outcome variables						
Health measures						
CASP	39.47	5.33	39.04	5.41	38.02	5.85
Self-reported heatlh	1.71	0.96	1.63	0.95	1.60	0.97
Depression index	2.18	1.93	2.30	1.97	2.48	2.20
Number of chronic diseases	1.44	1.28	1.46	1.31	1.54	1.32
ADLA	0.18	0.65	0.17	0.61	0.23	0.74
Professional care at home	0.04	0.19	0.04	0.19	0.09	0.28
Hospital overnight stays	0.21	0.41	0.21	0.41	0.22	0.41
Stroke	0.05	0.21	0.04	0.20	0.04	0.19
Chronical lung disease	0.09	0.28	0.11	0.31	0.10	0.30
Cataracts	0.13	0.34	0.12	0.33	0.12	0.33
High blood pressure	0.50	0.50	0.51	0.50	0.49	0.50
Feelings measures						
Low money stops	1.02	1.05	1.12	1.09	1.12	1.13
Life full of opportunities	2.26	0.83	2.19	0.86	2.06	0.90
Future looks good	2.24	0.84	2.18	0.86	2.06	0.85
Risky behaviours						
How often consumed alcohol	3.77	2.09	3.59	2.05	3.31	1.94
Smoke currently	0.16	0.37	0.19	0.39	0.13	0.33
Ever smoked daily	0.48	0.50	0.50	0.50	0.31	0.46
Pension income and subsidy						
Pension income per month (PI, 100€)	10.93	6.37	9.89	4.35	9.61	2.17
Subsidy per month (100€)	0.12	0.37	0.19	0.45	0.42	0.64
Subsidy recipient	0.13	0.34	0.21	0.41	0.38	0.49
PI without subsidy per month (100€)	10.46	6.36	9.40	4.29	8.01	1.97
Pension related characteristics						
CY	35.01	13.70	37.34	10.07	35.54	2.99
CY>35	0.63	0.48	0.66	0.47	0.60	0.49
aep	0.99	0.53	0.83	0.25	0.72	0.17
aep<0.75	0.33	0.47	0.41	0.49	0.59	0.49
Age at claiming pension	63.19	2.32	62.83	2.10	62.18	2.05
Self-reported retirement age*	62.81	2.54	62.61	2.53	61.22	3.59
Individual and household characteristics						
Birth year	1942.78	6.11	1943.69	5.71	1939.07	2.77
% Male	0.54	0.50	0.46	0.50	0.30	0.46
% Married	0.79	0.41	0.78	0.41	0.71	0.45
Household size	1.96	0.66	1.96	0.67	1.91	0.75
Number of children	1.02	1.42	1.16	1.45	1.69	1.79
Age at first child	24.37	4.62	24.45	4.75	23.98	4.90
Age at last child	29.36	5.43	29.14	5.39	29.01	5.33
% all children employed	0.55	0.50	0.54	0.50	0.53	0.50
% contact children ≥1/week	0.51	0.50	0.49	0.50	0.50	0.50
Months unemployed before 1992	3.90	10.42	5.36	12.45	7.21	15.49
Years of schooling	12.19	3.27	11.67	2.88	11.31	2.62
Owns a house	0.48	0.50	0.44	0.50	0.36	0.48
Household income per month (100€)	32.07	37.58	29.60	32.95	24.97	17.01
Pension/household income share	0.40	0.26	0.40	0.24	0.41	0.21
Observations	3,775		2,328		205	

Notes: Table 2.A.6 reports descriptive statistics for the SHARE-RV sample. *West German Pensioners* sample includes old-age German retirees residing in West Germany. *Baseline* sample further restricts to those born after 1931, with 15 to 55 contribution years (CY) and average earning points at retirement (*aep*) between 0.25 and 1.25. SHARE-RV *Restricted* sample uses the same restrictions as the RTWF baseline sample. * only available for 944, 609 and 32 observations for each sample. *Source*: Authors' calculations from the SHARE-RV data.

Table 2.A.7. Summary statistics by gender (SHARE-RV sample)

	All		Women		Men	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
Outcome variables						
Health measures						
CASP	39.04	5.41	39.23	5.15	38.81	5.70
Self-reported heatlh	1.63	0.95	1.68	0.95	1.56	0.95
Depression index	2.30	1.97	2.52	1.99	2.04	1.91
Number of chronic diseases	1.46	1.31	1.37	1.28	1.58	1.35
ADLA	0.17	0.61	0.15	0.60	0.19	0.62
Professional home care	0.04	0.19	0.03	0.18	0.04	0.20
Hospital overnight stays	0.21	0.41	0.19	0.39	0.24	0.43
Stroke	0.04	0.20	0.03	0.18	0.05	0.22
Chronical lung disease	0.11	0.31	0.10	0.31	0.11	0.32
Cataracts	0.12	0.33	0.13	0.34	0.11	0.31
High blood pressure	0.51	0.50	0.49	0.50	0.53	0.50
Feelings measures						
Low money stops	1.12	1.09	1.09	1.07	1.15	1.11
Life full of opportunities	2.19	0.86	2.23	0.84	2.15	0.87
Future looks good	2.18	0.86	2.21	0.83	2.15	0.89
Risky behaviours						
Consumed alcohol (days/week)	3.59	2.05	3.20	1.88	4.06	2.15
Smoke currently	0.19	0.39	0.17	0.38	0.20	0.40
Ever smoked daily	0.50	0.50	0.39	0.49	0.63	0.48
Pension income and subsidy						
Pension income per month (PI, 100€)	9.89	4.35	8.17	3.44	11.94	4.44
Subsidy per month (100€)	0.19	0.45	0.29	0.52	0.06	0.32
Subsidy recipient	0.21	0.41	0.34	0.48	0.05	0.21
PI without subsidy per month (100€)	9.40	4.29	7.48	3.46	11.68	4.07
Pension related characteristics						
CY	37.34	10.07	33.61	9.70	41.78	8.59
CY>35	0.66	0.47	0.51	0.50	0.84	0.37
aep	0.83	0.25	0.72	0.21	0.96	0.22
aep<0.75	0.41	0.49	0.63	0.48	0.16	0.37
Age at claiming pension	62.83	2.10	62.51	2.17	63.21	1.94
Self-reported retirement age*	62.61	2.53	62.29	2.67	63.01	2.30
Individual and household characteristics						
Birth year	1943.69	5.71	1944.28	5.64	1942.99	5.73
Married	0.78	0.41	0.75	0.43	0.81	0.39
Household size	1.96	0.67	1.90	0.63	2.02	0.71
Number of children	1.16	1.45	2.06	1.37	0.08	0.49
Age at first child	24.45	4.75	24.32	4.68	28.30	5.32
Age at last child	29.14	5.39	29.00	5.35	33.15	5.10
% all children employed	0.54	0.50	0.54	0.50	0.53	0.50
% contact children ≥1/week	0.49	0.50	0.52	0.50	0.46	0.50
Months unemployed bf 1992	5.36	12.45	5.33	11.60	5.39	13.40
Years of schooling	11.67	2.88	11.43	2.85	11.95	2.88
Own a house	0.44	0.50	0.44	0.50	0.44	0.50
Household income per month (100€)	29.60	32.95	30.22	32.47	28.85	33.52
Pension/household income share	0.40	0.24	0.33	0.21	0.50	0.25
Observations	2,328		1,365		963	

Notes: Table 2.A.7 reports descriptive statistics for the baseline SHARE-RV sample. Sample includes old-age German retirees residing in West Germany, born after 1931, with 15 to 55 contribution years (CY) and average earning points at retirement (*aep*) between 0.25 and 1.25. * only available for 609 observations. *Source*: Authors' calculations from the SHARE-RV data.

Table 2.A.8. Event study estimates in baseline sample.

	(aep<0.75) × quarters of contribution														
	33Q1	33Q2	33Q3	33Q4	34Q1	34Q2	34Q3	35Q1	35Q2	35Q3	35Q4	36Q1	36Q2	36Q3	36Q4
Panel A: First stage															
Recipient	0.009**	0.004	0.010**	0.000	0.002	0.004	0.006*	0.605***	0.703***	0.718***	0.726***	0.729***	0.724***	0.730***	0.748***
	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)	(0.004)	(0.015)	(0.011)	(0.010)	(0.007)	(0.009)	(0.011)	(0.008)	(0.008)
	[0.027]	[0.263]	[0.011]	[0.947]	[0.657]	[0.236]	[0.080]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Subsidy	0.011**	0.008*	0.013**	0.007	0.005	0.007**	0.008**	0.468***	0.584***	0.599***	0.609***	0.638***	0.644***	0.633***	0.659***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.004)	(0.014)	(0.020)	(0.019)	(0.022)	(0.025)	(0.025)	(0.019)	(0.026)
	[0.005]	[0.053]	[0.007]	[0.148]	[0.327]	[0.011]	[0.041]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pension income	0.011**	0.008*	0.013**	0.007	0.005	0.007**	0.008**	0.468***	0.584***	0.599***	0.609***	0.638***	0.644***	0.633***	0.659***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.004)	(0.014)	(0.020)	(0.019)	(0.022)	(0.025)	(0.025)	(0.019)	(0.026)
	[0.005]	[0.053]	[0.007]	[0.148]	[0.327]	[0.011]	[0.041]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Panel B: Impact on mortality															
Age at death	0.022	0.027	0.123	-0.051	0.039	0.007	0.121	0.241**	0.042	0.164	0.246*	0.269*	0.094	0.173	0.120
	(0.134)	(0.161)	(0.092)	(0.105)	(0.120)	(0.132)	(0.123)	(0.098)	(0.148)	(0.121)	(0.123)	(0.145)	(0.123)	(0.097)	(0.136)
	[0.870]	[0.866]	[0.209]	[0.616]	[0.744]	[0.955]	[0.354]	[0.023]	[0.796]	[0.219]	[0.071]	[0.093]	[0.446]	[0.105]	[0.398]
Dying before 65	-0.002	0.001	-0.006	-0.003	0.005	-0.007	-0.004	-0.020***	-0.011	-0.009	-0.006	-0.013**	-0.005	-0.011*	-0.005
	(0.008)	(0.007)	(0.006)	(0.005)	(0.005)	(0.008)	(0.006)	(0.004)	(0.007)	(0.007)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
	[0.806]	[0.900]	[0.271]	[0.460]	[0.345]	[0.369]	[0.577]	[0.000]	[0.163]	[0.262]	[0.345]	[0.017]	[0.256]	[0.051]	[0.370]
Dying before 70	-0.005	0.001	-0.002	0.006	0.000	-0.010	-0.006	-0.019*	-0.001	-0.019	-0.035**	-0.027	-0.014	-0.017	-0.013
	(0.014)	(0.022)	(0.011)	(0.013)	(0.016)	(0.014)	(0.017)	(0.010)	(0.017)	(0.013)	(0.013)	(0.015)	(0.014)	(0.012)	(0.017)
	[0.703]	[0.970]	[0.870]	[0.612]	[0.995]	[0.451]	[0.702]	[0.091]	[0.975]	[0.177]	[0.007]	[0.110]	[0.354]	[0.203]	[0.464]
Dying before 75	-0.007	-0.003	-0.021	-0.001	-0.012	-0.007	-0.014	-0.019	0.003	-0.016	-0.025	-0.028	-0.009	-0.017	-0.018
	(0.018)	(0.022)	(0.016)	(0.015)	(0.014)	(0.020)	(0.017)	(0.013)	(0.019)	(0.016)	(0.015)	(0.017)	(0.019)	(0.013)	(0.018)
	[0.722]	[0.904]	[0.221]	[0.964]	[0.404]	[0.747]	[0.432]	[0.182]	[0.898]	[0.342]	[0.145]	[0.127]	[0.623]	[0.198]	[0.337]
Panel C: Impact on labour supply															
Age at claiming pension	0.061	0.018	-0.097	-0.038	-0.046	-0.065	-0.058	-0.020	-0.025	-0.038	-0.054	-0.087	-0.023	-0.055	-0.037
	(0.059)	(0.083)	(0.084)	(0.082)	(0.059)	(0.077)	(0.071)	(0.065)	(0.063)	(0.090)	(0.073)	(0.089)	(0.083)	(0.071)	(0.088)
	[0.328]	[0.833]	[0.274]	[0.647]	[0.470]	[0.458]	[0.484]	[0.771]	[0.695]	[0.722]	[0.512]	[0.367]	[0.787]	[0.479]	[0.683]
Obs	149,053														
Contribution year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Estimates for baseline sample. Restrictions: 1932-1942 birth cohorts, 33-36 contribution years, 0.45-1.05 *aep*. Controls include having children, not having health insurance, and male dummy. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.9. Impact of subsidy eligibility on other measures of mortality (DID estimates)

	All (1)	**Women** (2)	**Men** (3)
Dying between 62 - 69	-0.015***	-0.007	-0.029**
	(0.004)	(0.005)	(0.010)
	[0.000]	[0.202]	[0.011]
Dying between 70 - 75	0.007	0.003	0.006
	(0.004)	(0.005)	(0.009)
	[0.112]	[0.617]	[0.467]
Dying within 4 years	-0.004	-0.004	-0.016*
	(0.004)	(0.004)	(0.008)
	[0.256]	[0.340]	[0.072]
Dying within 8 years	-0.009*	-0.006	-0.025**
	(0.005)	(0.006)	(0.010)
	[0.094]	[0.288]	[0.031]
Dying within 12 years	-0.012*	-0.016**	-0.013
	(0.007)	(0.007)	(0.011)
	[0.098]	[0.044]	[0.264]
Obs	149,053	96,820	52,233
Contribution year FE	✓	✓	✓
Birth cohort FE	✓	✓	✓
Controls	✓	✓	✓
PI without subsidy	✓	✓	✓

Notes: This table shows the impact of eligibility for the pension subsidy on a list of alternative measures of mortality: probabilities of dying between age 62 and 69, probabilities of dying between age 70 and 75, probabilities of dying within 4, 8, and 12 years from the age at which they started claiming the current pension. Column 1 shows the impact for the baseline sample. Columns 2 and 3 show the results by gender. All specifications control for contribution year fixed effects, birth cohort fixed effects, a list of controls (having children, not having health insurance, male dummy) and monthly pension income without subsidy. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.10. Heterogeneity effects by marital status and gender (DID estimates)

	Women				*Men*	
	Marital status		**Children**		**Marital status**	
	Married	Not married	Yes	No	Married	Not married
	(1)	(2)	(3)	(4)	(5)	(6)
First stage						
Recipient	0.677***	0.753***	0.687***	0.789***	0.315***	0.645***
	(0.011)	(0.008)	(0.010)	(0.008)	(0.010)	(0.014)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Subsidy	0.611***	0.618***	0.612***	0.779***	0.205***	0.363***
	(0.015)	(0.020)	(0.015)	(0.028)	(0.010)	(0.019)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pension income	0.611***	0.618***	0.612***	0.779***	0.205***	0.363***
	(0.015)	(0.020)	(0.015)	(0.028)	(0.010)	(0.019)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Impact on mortality						
Age at death	0.038	0.030	0.012	0.131	0.285***	0.089
	(0.061)	(0.106)	(0.044)	(0.154)	(0.072)	(0.096)
	[0.545]	[0.805]	[0.796]	[0.420]	[0.000]	[0.396]
Dying before 65	0.001	-0.004	-0.002	0.004	-0.002	-0.011
	(0.003)	(0.005)	(0.003)	(0.010)	(0.005)	(0.006)
	[0.826]	[0.488]	[0.530]	[0.722]	[0.623]	[0.134]
Dying before 70	-0.006	-0.004	-0.001	-0.019	-0.034**	-0.013
	(0.005)	(0.011)	(0.004)	(0.015)	(0.012)	(0.010)
	[0.290]	[0.757]	[0.795]	[0.223]	[0.008]	[0.215]
Dying before 75	-0.005	0.001	0.003	-0.027	-0.031**	-0.004
	(0.009)	(0.017)	(0.006)	(0.027)	(0.009)	(0.014)
	[0.651]	[0.958]	[0.627]	[0.353]	[0.003]	[0.787]
Impact on labour supply						
Age at claiming pension	-0.105**	-0.214**	-0.121**	-0.191	-0.095*	0.241**
	(0.039)	(0.051)	(0.037)	(0.105)	(0.044)	(0.071)
	[0.025]	[0.003]	[0.009]	[0.112]	[0.052]	[0.011]
Obs	57,416	32,638	84,114	12,706	31,753	18,910
CY FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Income	✓	✓	✓	✓	✓	✓

Notes: This table shows the impact of eligibility for the pension subsides by subgroups and gender. Columns 1 and 2 show the results by marital status for women. Columns 3 and 4 show results by whether having a child or not for women. Columns 5 and 6 show the results by marital status for men. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. *Source*: Authors' calculations from the RTWF data.

Table 2.A.11. Heterogeneity effects by subsidy size and gender (DID estimates)

	Women				*Men*			
	Baseline	**Treatment heterogeneity samples**			**Baseline**	**Treatment heterogeneity samples**		
Treatment *aep* ∈	[0.45,0.75]	[0.25,0.45]	[0.45,0.61]	[0.61,0.75]	[0.45,0.75]	[0.25,0.45]	[0.45,0.61]	[0.61,0.75]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First stage								
Recipient	0.705***	0.728***	0.772***	0.629***	0.440***	0.603***	0.414***	0.459***
	(0.009)	(0.023)	(0.011)	(0.008)	(0.007)	(0.011)	(0.021)	(0.010)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Subsidy	0.632***	0.886***	0.937***	0.297***	0.267***	0.600***	0.405***	0.178***
	(0.015)	(0.026)	(0.028)	(0.006)	(0.009)	(0.008)	(0.024)	(0.005)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pension income	0.632***	0.886***	0.937***	0.297***	0.267***	0.600***	0.405***	0.178***
	(0.015)	(0.026)	(0.028)	(0.006)	(0.009)	(0.008)	(0.024)	(0.005)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Impact on mortality								
Age at death	0.028	0.054	0.025	0.028	0.234**	0.392**	0.496***	0.068
	(0.048)	(0.066)	(0.049)	(0.054)	(0.068)	(0.138)	(0.083)	(0.089)
	[0.575]	[0.421]	[0.604]	[0.646]	[0.009]	[0.023]	[0.000]	[0.412]
Dying before 65	-0.001	0.000	0.001	-0.002	-0.005	-0.006	-0.011**	-0.001
	(0.003)	(0.005)	(0.003)	(0.003)	(0.003)	(0.007)	(0.004)	(0.004)
	[0.821]	[0.946]	[0.818]	[0.538]	[0.150]	[0.423]	[0.014]	[0.767]
Dying before 70	-0.004	-0.006	-0.004	-0.004	-0.030**	-0.032	-0.057***	-0.012
	(0.004)	(0.008)	(0.005)	(0.005)	(0.010)	(0.018)	(0.011)	(0.011)
	[0.355]	[0.458]	[0.410]	[0.475]	[0.012]	[0.109]	[0.001]	[0.266]
Dying before 75	-0.002	-0.017*	-0.006	0.004	-0.023**	-0.055**	-0.049***	-0.007
	(0.008)	(0.009)	(0.009)	(0.008)	(0.010)	(0.019)	(0.013)	(0.013)
	[0.829]	[0.097]	[0.493]	[0.654]	[0.049]	[0.031]	[0.000]	[0.556]
Impact on labour supply								
Age at claiming pension	-0.172**	-0.190*	-0.265**	-0.070	0.037	-0.076	0.060	0.022
	(0.044)	(0.101)	(0.039)	(0.069)	(0.036)	(0.138)	(0.072)	(0.042)
	[0.005]	[0.085]	[0.002]	[0.394]	[0.350]	[0.600]	[0.435]	[0.600]
Obs	96,820	69,701	77,477	74,691	52,233	44,454	45,823	47,924
Contribution year FE	✓	✓	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table shows the impact of eligibility for the pension subsides by *aep* subgroups and gender. Columns 1 shows baseline results. Columns 2 and 6 keep the same control group as in baseline, but uses as treatment group individuals with $aep \in [0.25, 0.45]$, while keeping the same restrictions on birth cohorts and contribution years as in baseline. Columns 3 and 7 use individuals with $aep \in [0.45, 0.61)$ as treatment group. Columns 4 and 11 take individuals with $aep \in [0.61, 0.75)$ as treatment group. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.12. P-value on significance in difference of point estimates for heterogeneous effects (Table 2.2 and Table 2.A.10)

	Table 2.2				Table 2.A.10	
	Gender	Marital status	Subsidy size	Health insurance	Child	Marital status
	(1)	(2)	(3)	(4)	(5)	(6)
First stage						
Recipient	0.000	0.000	0.000	0.000	0.000	0.000
Subsidy	0.000	0.474	0.000	0.000	0.000	0.661
Pension income	0.474	0.920	0.000	0.000	0.000	0.661
Impact on mortality						
Age at death	0.031	0.888	0.144	0.927	0.454	0.955
Dying before 65	0.433	0.126	0.834	0.463	0.612	0.523
Dying before 70	0.029	0.709	0.245	0.932	0.269	0.879
Dying before 75	0.080	0.400	0.113	0.447	0.286	0.762
Impact on labour supply						
Age at claiming pension	0.001	0.298	0.926	0.056	0.460	0.068

Notes: This table shows the bootstrapped p-values on the significance of differences in point estimates in heterogeneous effects. The Null-Hypothesis is that the point estimates in subgroups (e.g. male vs. female) are identical. P-values higher than 0.1 indicate that we cannot reject the H0 with a probability higher than 90%. The null hypothesis (H0) is that the point estimates from the heterogeneous groups are significantly different. Columns (1) (2) (3) and (4) report differences by gender (women - men), by marital status (married - not married), by subsidy size (high - low) and by type of health insurance (public - private), corresponding to the estimates in Table 2.2. Columns (5) and (6) report the differences by having children (yes-no) and by marital status (married - not married) for women, corresponding to the estimates in Table 2.A.10. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.13. Impact of pension income on pension claiming pathways (DID estimates)

	All (1)	**Women** (2)	**Men** (3)
Disability pension	-0.027***	0.013	-0.034**
	(0.005)	(0.007)	(0.009)
	[0.000]	[0.104]	[0.000]
Pension for the unemployed	0.031***	-0.004**	0.035**
	(0.005)	(0.002)	(0.014)
	[0.000]	[0.004]	[0.047]
Women’s pension	0.002	0.022**	-
	(0.005)	(0.008)	-
	[0.716]	[0.016]	-
Pension for long-term insured	0.014**	0.022*	0.041**
	(0.006)	(0.011)	(0.012)
	[0.023]	[0.055]	[0.018]
Obs	149,053	96,820	52,233
Contribution year FE	✓	✓	✓
Birth cohort FE	✓	✓	✓
Controls	✓	✓	✓
PI without subsidy	✓	✓	✓

Notes: This table shows the impact of being eligible for the pension subsidy on pension claiming pathways. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors’ calculations from the RTWF data.

Table 2.A.14. Summary statistics for the compliers

	Baseline		Eligible group		Compliers		Never takers		C-NT
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	P-value
RTWF Sample									
Pension income and subsidy related variables									
Subsidy (100€)	0.23	0.47	0.63	0.60	0.81	0.56	0.00	0.00	0.000
Subsidy recipient	0.30	0.46	0.78	0.41	1.00	0.00	0.00	0.00	-
PI w/o subsidy (100€)	6.83	2.32	5.73	1.64	5.65	1.63	6.01	1.62	0.000
Individual characteristics									
Birth year	1936.29	2.98	1936.16	2.98	1936.08	2.94	1936.44	3.07	0.000
% male	0.35	0.48	0.21	0.40	0.12	0.32	0.51	0.50	0.000
% married	0.60	0.49	0.61	0.49	0.59	0.49	0.69	0.46	0.000
Number of children†	2.19	1.47	2.31	1.46	2.28	1.44	2.51	1.57	0.000
% private health insurance	0.09	0.29	0.09	0.28	0.05	0.22	0.22	0.41	0.000
% public health insurance	0.87	0.33	0.88	0.33	0.92	0.28	0.73	0.44	0.000
Obs.	149,053		52,191		40,681		11,510		
SHARE-RV Sample									
Pension income and subsidy related variables									
Subsidy (100€)	0.19	0.45	0.68	0.71	1.05	0.62	0.00	0.00	0.000
Subsidy recipient	0.21	0.41	0.65	0.48	1.00	0.00	0.00	0.00	-
PI without subsidy (100€)	9.40	4.29	7.50	1.72	7.30	1.79	7.86	1.52	0.000
Individual and household characteristics									
Birth year	1943.69	5.71	1945.87	5.15	1945.57	5.03	1946.42	5.32	0.000
% male	0.46	0.50	0.24	0.43	0.17	0.37	0.38	0.49	0.000
% married	0.78	0.41	0.81	0.40	0.76	0.43	0.89	0.32	0.585
Household size	1.96	0.67	1.98	0.63	1.97	0.69	2.01	0.52	0.782
Number of children	1.16	1.45	1.58	1.36	1.67	1.33	1.40	1.40	0.000
Age at first child	24.45	4.75	23.46	3.88	22.97	3.87	24.62	3.68	0.000
Age at last child	29.14	5.39	28.44	5.58	27.87	5.70	29.78	5.04	0.000
% all children employed	0.54	0.50	0.55	0.50	0.55	0.50	0.56	0.50	0.555
% contacts children ≥1/week	0.49	0.50	0.52	0.50	0.52	0.50	0.52	0.50	0.220
Months unemployed before 1992	5.36	12.45	7.89	17.21	6.80	15.82	9.92	19.43	0.004
Years of schooling	11.67	2.88	11.37	2.69	10.91	2.39	12.24	2.98	0.000
Own a house	0.44	0.50	0.39	0.49	0.33	0.47	0.50	0.50	0.000
Household income (100€)	29.60	32.95	28.47	28.18	25.52	30.18	34.01	23.08	0.037
Pension/household income	0.40	0.24	0.38	0.21	0.43	0.22	0.30	0.18	0.703
Observations	2328		493		322		171		

Notes: Table 2.A.14 reports descriptive statistics for the compliers and never takers. Eligible group consists of individuals fullfil both eligibility conditions. Compliers are subsidy recipients in eligible group and never takers are not subsidy recipient in the eligible group. Last column reports p-values of differences in means between compliers and never takers. *Source*: Authors' calculations from the RTWF and SHARE-RV data.

Table 2.A.15. Placebo checks (DID estimates)

	Baseline	**Placebo**			
aep ∈	[0.45,1.05]	[0.8,1.25]	[1.0,1.4]	[0.45,1.05]	[0.45,1.05]
CY	[33,36]	[33,36]	[33,36]	[29,32]	[37,40]
cutoff: *aep* <	0.75	1.0	1.2	0.75	0.75
cutoff: *CY* ≥	35	35	35	30	39
	(1)	(2)	(3)	(4)	(5)
Panel A: First stage					
Recipient	0.693***	0.031***	0.035**	-0.001	0.015**
	(0.008)	(0.004)	(0.006)	(0.001)	(0.003)
	[0.000]	[0.000]	[0.002]	[0.509]	[0.003]
Subsidy	0.579***	0.024***	0.023***	-0.001	0.049***
	(0.018)	(0.003)	(0.004)	(0.001)	(0.006)
	[0.000]	[0.000]	[0.001]	[0.378]	[0.000]
Pension income	0.579***	0.024***	0.023**	-0.001	0.049***
	(0.018)	(0.003)	(0.004)	(0.001)	(0.006)
	[0.000]	[0.000]	[0.001]	[0.378]	[0.000]
Panel B: Impact on mortality					
Age at death	0.136**	0.049	-0.254	-0.042	0.064*
	(0.036)	(0.033)	(0.173)	(0.051)	(0.032)
	[0.009]	[0.174]	[0.193]	[0.418]	[0.069]
Dying before 65	-0.008***	0.024**	0.001	-0.002	
	(0.002)	(0.002)	(0.009)	(0.002)	(0.002)
	[0.000]	[0.851]	[0.019]	[0.668]	[0.434]
Dying before 70	-0.015**	-0.005	0.031	0.007	-0.006
	(0.004)	(0.006)	(0.017)	(0.006)	(0.004)
	[0.004]	[0.409]	[0.114]	[0.288]	[0.117]
Dying before 75	-0.008	-0.009	0.010	0.000	-0.001
	(0.006)	(0.006)	(0.018)	(0.006)	(0.003)
	[0.186]	[0.186]	[0.603]	[0.941]	[0.733]
Panel C: Impact on labour supply					
Age at claiming pension	-0.041	-0.008	-0.060	-0.085**	-0.060**
	(0.035)	(0.028)	(0.091)	(0.027)	(0.025)
	[0.295]	[0.786]	[0.521]	[0.016]	[0.045]
Obs	149,053	71,534	71,534	83,165	172,755

Notes: This table shows the impact of eligibility for the pension subsidy for a list of placebo samples. Column (1) shows the results for the baseline sample. Columns (2) and (3) takes the same contribution year restriction but varying the *aep* restriction and cut-offs. Column (2) restricts *aep* to be between 0.8 and 1.25 with a placebo cut-off at 1.0, column (3) restricts *aep* to be between 1.0 and 1.4 with a placebo cut-off at 1.2. Columns (6) and (7) takes the same *aep* restriction but varying the contribution years restriction and cut-offs. Column (4) restricts contribution year to 29 and 32 years with a placebo cutoff at 30, column (5) restricts contribution year to be between 37 and 40 with placebo cut-off at 39. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.16. Robustness checks (DID estimates)

	Baseline	1932-1942			1932-1948	1932-1937
aep ∈	[0.45,1.05]	[0.45,1.05]	[0.6,0.9]	[0.25,1.25]	[0.45, 1.05]	[0.45, 1.05]
exactly at 35 CY	keep	drop	keep	keep	keep	keep
	(1)	(2)	(3)	(4)	(5)	(6)
First stage						
Recipient	0.730***	0.741***	0.667***	0.766***	0.722***	0.742**
	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)	(0.009)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.016]
Subsidy	0.646***	0.660***	0.354***	0.859***	0.610***	0.689***
	(0.021)	(0.023)	(0.010)	(0.030)	(0.026)	(0.015)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Pension income	0.646***	0.660***	0.354***	0.859***	0.610***	0.689***
	(0.021)	(0.023)	(0.010)	(0.030)	(0.026)	(0.015)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Impact on mortality						
Age at death	0.135**	0.125**	0.104**	0.203***	0.113**	0.148***
	(0.027)	(0.028)	(0.031)	(0.013)	(0.026)	(0.034)
	[0.002]	[0.002]	[0.006]	[0.000]	[0.004]	[0.000]
Dying before 65	-0.009***	-0.008***	-0.006***	-0.009**	-0.009***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
	[0.000]	[0.001]	[0.001]	[0.001]	[0.001]	[0.000]
Dying before 70	-0.014**	-0.014**	-0.013**	-0.021***	-0.011**	-0.015***
	(0.004)	(0.004)	(0.004)	(0.001)	(0.003)	(0.004)
	[0.004]	[0.007]	[0.010]	[0.000]	[0.011]	[0.000]
Dying before 75	-0.008*	-0.007	-0.005	-0.019**	-0.008**	-0.008
	(0.004)	(0.004)	(0.005)	(0.003)	(0.003)	(0.005)
	[0.088]	[0.102]	[0.355]	[0.001]	[0.045]	[0.219]
Impact on labour supply						
Age at claiming pension	0.010	0.016	-0.001	0.051	0.009	0.021
	(0.015)	(0.018)	(0.008)	(0.045)	(0.013)	(0.020)
	[0.547]	[0.451]	[0.875]	[0.463]	[0.535]	[0.344]
Obs	401,932	387,027	216,320	2,043,223	464,444	260,231
Contribution year FE	✓	✓	✓	✓	✓	✓
Birth cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
PI without subsidy	✓	✓	✓	✓	✓	✓

Notes: This table shows the robustness of the estimates by varying sample selection. Column (1) shows the results for the baseline sample. Column (2) excludes individuals who retired after exactly 35 years of contribution (420 months). Column (3) takes individuals with *aep* ∈ [0.6 – 0.9]. Column (4) takes individuals with *aep* ∈ [0.25 – 1.25] and with 20-50 contribution years, in line with the SHARE-RV baseline sample restrictions. Column (5) expands the cohorts restriction to 1932-1948. Column (6) restricts the baseline sample to those born between 1932 and 1937. All specifications control for contribution year fixed effects, birth cohort fixed effects, a list of controls (being married, having children, claiming unemployment, disability or women's pension, not having health insurance, male dummy) and monthly pension income without subsidy. Standard errors clustered by birth cohort are in parentheses, bootstrapped p-values in brackets. With respect to bootstrapped p-values: *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the RTWF data.

Table 2.A.17. Impact of pension income on other diseases (IV estimates)

	All		Women		Men	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Pension income (100€)	0.414***	0.440***	0.416***	0.431***	0.573***	0.580***
	(0.037)	(0.037)	(0.044)	(0.044)	(0.077)	(0.076)
Panel B: IV						
Cancer	0.032	0.037	-0.030	-0.019	0.045	0.040
	(0.063)	(0.060)	(0.069)	(0.067)	(0.112)	(0.110)
Parkinson	0.014	0.013	0.024*	0.024*	-0.003	-0.003
	(0.009)	(0.009)	(0.014)	(0.014)	(0.003)	(0.004)
Hip femoral fracture	-0.006	-0.009	-0.033	-0.034	-0.049	-0.053
	(0.034)	(0.032)	(0.030)	(0.031)	(0.081)	(0.080)
Diabetes	0.010	-0.014	0.136	0.105	-0.119	-0.110
	(0.095)	(0.091)	(0.106)	(0.103)	(0.185)	(0.183)
First stage F-stat	124.30	141.60	87.53	96.23	55.86	58.70
Obs	2,328	2,328	1,365	1,365	963	963
Contribution year FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Retirement age		✓		✓		✓

Notes: This table shows the effect on probability of having a list diseases of an increase in pension income of 100 euro per month. Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator for eligibility for pension subsidy. In addition to a list of controls, pension income without subsidy, birth cohort fixed effects and contribution year fixed effects in the odd columns, the even columns control for age at claiming pensions. Columns 1 and 2 show the results for the baseline sample. Columns 3 to 6 show the results for women and men respectively. Monetary values are expressed in hundred 2015 euro. Standard errors clustered by birth cohort are in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. † Smaller estimation sample of 1,753 observations (1,018 women and 735 men). First-stage F between 40 and 115 for all specifications. First stage estimated coefficients remain similar. *Source*: Authors' calculations from the SHARE-RV data.

Table 2.A.18. Heterogeneity by share of pension income over total household income (IV estimates)

	Share of pension income over total household income					
	Above 50%, "Poor"			Below 50% "Well-off"		
	All	Women	Men	All	Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: First stage						
Pension income	0.736***	0.908***	0.531***	0.239***	0.167**	0.455***
	(0.109)	(0.119)	(0.162)	(0.059)	(0.073)	(0.106)
Panel B: IV						
CASP	0.761**	0.015	3.796**	0.911	-0.186	1.944**
	(0.369)	(0.349)	(1.507)	(0.706)	(1.169)	(0.763)
Self-reported health	0.126	-1.064***	2.852**	1.439*	0.047	3.093***
	(0.338)	(0.366)	(1.216)	(0.797)	(1.278)	(1.095)
Depression index	-0.783**	0.157	-1.582*	-1.209	0.797	-2.119**
	(0.339)	(0.364)	(0.850)	(0.791)	(1.406)	(0.842)
Number of chronic diseases	-0.294	0.423	-2.238**	-2.876***	-2.577	-2.708***
	(0.380)	(0.396)	(1.037)	(0.961)	(1.651)	(0.916)
Difficulties with ADLAs	-0.048	0.783	0.710	-0.276	0.143	-0.955*
	(0.522)	(0.579)	(0.505)	(0.530)	(0.974)	(0.573)
Long hospital stay ($\geq$14)	0.150*	0.155**	0.448	-0.388**	-0.562*	-0.136
	(0.079)	(0.073)	(0.492)	(0.163)	(0.303)	(0.142)
Lack of money stops	-0.432	-0.279	-0.256	-1.264*	-1.924	-1.810**
	(0.335)	(0.357)	(0.795)	(0.740)	(1.408)	(0.787)
Feel full of opportunities	1.144***	0.673*	1.490	0.717	-0.842	2.058**
	(0.438)	(0.401)	(1.613)	(0.710)	(1.189)	(0.872)
Future looks good	1.074***	0.413	3.695**	0.688	-0.175	1.688**
	(0.384)	(0.390)	(1.495)	(0.716)	(1.272)	(0.851)
Days/week alcohol (last 6 months)	0.102	0.149	0.746	0.523	1.557	-1.281*
	(0.393)	(0.396)	(0.956)	(0.850)	(1.449)	(0.710)
Ever smoked daily	-0.668***	-0.763***	0.064	-0.029	0.988	-0.503
	(0.211)	(0.182)	(0.752)	(0.352)	(0.767)	(0.331)
Observations	487	199	288	676	470	206
First stage F-stat	41.76	47.31	9.52	15.66	4.79	15.29
Contribution year FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Retirement age	✓	✓	✓	✓	✓	✓

Notes: This table shows the heterogeneous effect on mortality of an increase in pension income of 100 euro per month by share of pension income (without subsidy) over total household income (without subsidy). Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator for eligibility for pension subsidy. Columns 1, 2 and 3 show the results for the subgroup with pension income share above 50% for all, women and men. Columns 4, 5 and 6 to 6 show the results for the subgroup with pension income share below 50% for all, women and men. Standard errors clustered by birth cohort are in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the SHARE-RV data.

Table 2.A.19. Heterogeneity by any household member owning a house (IV estimates)

	Home ownership			Without home ownership		
	All (1)	Women (2)	Men (3)	All (4)	Women (5)	Men (6)
Panel A: First stage						
Pension income	0.358***	0.350***	0.350***	0.528***	0.560***	0.586***
	(0.043)	(0.049)	(0.049)	(0.067)	(0.073)	(0.142)
Panel B: IV						
CASP	0.642**	-0.235	1.338*	0.566	-0.161	2.734***
	(0.315)	(0.394)	(0.698)	(0.356)	(0.345)	(0.962)
Self-reported heatlh	0.473	-0.850*	2.839***	0.713**	0.077	2.364**
	(0.344)	(0.445)	(0.986)	(0.356)	(0.343)	(0.963)
Depression index	-0.379	0.581	-0.642	-0.544	0.271	-2.567***
	(0.321)	(0.439)	(0.599)	(0.367)	(0.373)	(0.871)
Number of chronic diseases	-0.858***	-0.009	-3.926***	-0.271	0.122	-2.193***
	(0.327)	(0.362)	(1.218)	(0.345)	(0.378)	(0.830)
Difficulties with ADLAs	-0.538*	0.289	-2.402***	-0.023	0.337	-0.744
	(0.325)	(0.460)	(0.828)	(0.404)	(0.407)	(0.840)
Long hospital stay (≥14)	-0.051	0.018	-0.062	-0.030	-0.031	-0.183
	(0.067)	(0.082)	(0.149)	(0.074)	(0.062)	(0.188)
Lack of money stops	-0.345	-0.244	-0.485	-0.480	-0.174	-1.029
	(0.327)	(0.408)	(0.731)	(0.345)	(0.344)	(0.909)
Feel full of opportunities	0.995***	0.053	2.282***	0.316	0.029	2.027**
	(0.329)	(0.401)	(0.847)	(0.354)	(0.360)	(0.866)
Future looks good	0.214	-0.423	1.221*	1.206***	0.728**	3.295***
	(0.325)	(0.416)	(0.683)	(0.359)	(0.354)	(1.050)
Days/week alcohol (last 6 months)	0.086	-0.170	-0.902	-0.222	0.048	-1.342
	(0.412)	(0.486)	(0.893)	(0.419)	(0.452)	(1.293)
Ever smoked daily	-0.185	0.016	-0.137	-0.681***	-0.751***	-0.525
	(0.158)	(0.188)	(0.339)	(0.196)	(0.216)	(0.383)
Observations	1,468	852	616	860	513	347
First stage F-stat	68.60	51.60	17.95	62.94	58.44	17.10
Contribution year FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Retirement age	✓	✓	✓	✓	✓	✓

Notes: This table shows the heterogeneous effect on mortality of an increase in pension income of 100 euro per month by whether any household member of the individual owns a house. Panel A reports first-stage estimates and panel B reports the IV estimates. The instrument for pension income is an indicator for eligibility for pension subsidy. Columns 1, 2 and 3 show the results for the subgroup having assets for all, women and men. Columns 4, 5 and 6 to 6 show the results for the subgroup doesn't have any assets for all, women and men. Standard errors clustered by birth cohort are in parentheses, *** p<0.01, ** p<0.05, * p<0.1. *Source*: Authors' calculations from the SHARE-RV data.

Table 2.A.20. Summary statistics for people with *aep* > 0.75 and more than 35 years of contribution by gender

	Women (1)	Men (2)	P-value diff. (3)	Source (4)
Mortality measures				
Age at death	72.42	71.62	0.000	RTWF
Dying before 65	0.06	0.06	0.000	RTWF
Dying before 70	0.26	0.35	0.000	RTWF
Dying before 75	0.48	0.60	0.000	RTWF
Health measures				
CASP	39.62	38.58	0.007	SHARE-RV
Self-reported heatlh	1.88	1.53	0.000	SHARE-RV
Depression index	2.30	2.14	0.266	SHARE-RV
Number of chronic diseases	1.24	1.62	0.000	SHARE-RV
ADLA	0.12	0.22	0.034	SHARE-RV
Feelings measures				
Low money stops	1.09	1.14	0.541	SHARE-RV
Life full of opportunities	2.32	2.11	0.001	SHARE-RV
Future looks good	2.25	2.14	0.059	SHARE-RV
Risky behaviours				
Consumed alcohol (days/week)	3.41	4.12	0.000	SHARE-RV
Smoke currently	0.25	0.22	0.480	SHARE-RV
Ever smoked daily	0.45	0.65	0.000	SHARE-RV

Notes: Table 2.A.20 reports descriptive statistics for people with *aep* > 0.75 and contribution years above 35 by gender. Those individuals are not eligible for subsidy only because of having higher *aep*. Columns 1 and 2 shows the average values for women and men, column 3 the pvalue of the difference in means between the two groups, column 4 indicate the data source. *Source*: Authors' calculations from the RTWF and SHARE-RV data.

Table 2.A.21. Impacts of Eligibility on sickness leaves before age 50 (VSKT data)

	Full sample (1)	Women (2)	Men (3)
Duration of sickness before age 50	1.110***	0.681*	5.606***
	(0.401)	(0.365)	(1.823)
	[0.002]	[0.054]	[0.009]
Mean Dep. Variable	1.296	1.169	1.865
Prob (having sick leave before age 50)	0.074*	0.069	0.157
	(0.044)	(0.046)	(0.132)
	[0.074]	[0.143]	[0.241]
Mean Dep. Variable	0.218	0.207	0.271
Duration of sickness before age 55	1.907***	1.617***	6.005***
	(0.511)	(0.485)	(2.236)
	[0.000]	[0.000]	[0.009]
Mean Dep. Variable	1.798	1.635	2.530
Prob (having sick leave before age 55)	0.122***	0.114**	0.195
	(0.046)	(0.050)	(0.131)
	[0.012]	[0.028]	[0.130]
Mean Dep. Variable	0.256	0.245	0.308
Contribution year FE	✓	✓	✓
Birth Cohort FE	✓	✓	✓
Controls	✓	✓	✓
PI without subsidy	✓	✓	✓
Observations	2924	2517	407

Notes: Table 2.A.21 reports the impact of eligibility for the subsidy on health status before retirement proxies by duration of sickness before age 50 and probablity of taking up any sick leave before age 50. We show the impacts for the full sample, women and men, respectively. Sample restriction: West German pensioners born between 1932 and 1942 with 30 to 40 contribution years and average earning points at retirement between 0.45 and 1.05. Duration of sickness before age 50 is measures in months. The dummy of being sick before 50 takes value 1 if duration of sickness before age 50 is above zero. *Source*: Authors' calculations from the SUFVSKT 2002, 2004-2006.

Appendix 2.B Additional Details on Institution

2.B.1 Details on pension benefit formula

The main determinant of pension benefits is the sum of the individually accumulated earnings points (Entgeltpunkte, (EP)). Essentially, for each year τ of contributions, a worker i accumulates some earnings points $EP_{i\tau}$, which are determined by the individual wage $w_{i\tau}$ relative to the average wage of all the insured $\bar{w_{\tau}}$. For example, a worker whose wage is half of the average wage in the contribution year τ will accumulate 0.5 points in that year. Equation 1 shows the monthly pension benefits for individual i who retires in year t.

$$PB_{it} = \underbrace{(\sum_{\tau} EP_{i\tau} + \text{ Subsidy}_i)}_{\text{Personal Pension Base}} \times PV_t \text{ , where } EP_{i\tau} = \frac{w_{i\tau}}{\bar{w_{\tau}}} \tag{2.B.1}$$

The amount of pension benefit PB_{it} is the personal pension base multiplied by the pension value.

This benefit level will also be adjusted by an adjustment factor AF_{it}. The adjustment factor penalizes early claims. Benefit levels decrease by 0.3% for each month before the full retirement age is reached. However, the deductions of 3.6% per year of delayed claiming are low by international standards and not actuarially fair. As a consequence, there still exists a positive implicit tax on working, even after accounting for the financial penalty. The pension benefit also depends on the type of pension. This factor is equal to one for the old-age pension, and is less than one for disability pensions. Almost all subsidy recipients claim an old-age pension.

The worker's personal pension base is the sum of the EPs accumulated over time, plus additional EPs credited by the subsidy program. For example, an average wage earner with 15 contribution years accumulates 15 EPs. At the time of the claim t, this personal pension base is scaled up by the pension value PV_t, which is determined aggregately by factors such as the average wage of all insured, the contribution rate and demographic changes. This pension value PV_t is adjusted on July 1 of each year. For example, one EP was equivalent to 31.03 euros per month in 2018. Overall, workers with short contribution years or low relative wage incomes are more likely to face old-age poverty. On average, one less year of full value contribution decreases the gross replacement rate by around 1.17%. This is one of the reasons that women are the majority of the subsidy recipients as they have short employment periods and a lower wage over their life cycle.

Pensioners can work while claiming their pensions, however, they face a stringent earnings test between the early retirement age (ERA) and the normal retirement age (NRA). If pensioners work at jobs paid more than 450 euros per month, they need to file for partial retirement. This makes working at a regular job while claiming a full pension impossible. After the NRA, pension recipients no longer face earnings tests.[42]

2.B.2 Pension reforms and pension pathways

Since the 1990s, there has been a number of pension reforms, which introduced the early retirement actuarial adjustment (Berkel and Börsch-Supan, 2004), increased the statutory retirement ages (Engels, Geyer, and Haan, 2017), encouraged a tax-advantaged private savings plan (Börsch-Supan et al., 2015) and included a sustainability factor in the pension benefits formula (Börsch-Supan, Wilke, et al., 2004).

Workers can claim the standard old-age pension (SGB VI §235) at age 65 throughout our sample period. The eligibility condition is at least 5 years of contributions. For cohorts 1947 to 1964, this age will gradually increase by one month for each birth-year from age 65 to 67. These changes began in 2012 and will be complete in 2030 (See SGB VI §235(2)).

Several alternate pathways make retiring before the regular retirement age 65 possible in Germany. There are four main early retirement pathways: old-age pensions for women, old-age pensions due to unemployment (and part-time work), old-age pensions for the long-term insured and old-age pensions for severely disabled persons. Each pathway has its own eligibility conditions. Each pathway has also its own full retirement age (FRA) and early retirement age (ERA). For example, age 60 is the early retirement age for the women's pension pathway. Age 63 is the early retirement age for the long-term insured pathway.

The pension reforms in the past few decades typically reduce public pension generosity by raising the retirement age and penalizing early claiming. The increase in statutory retirement age and the financial penalty for early claiming were phased in gradually in monthly increments. An individual can claim, at the earliest, at the ERA, however each year before FRA renders a 3.6% benefit deduction. (See Engels, Geyer, and Haan (2017) for more details).

For women born between 1932 and 1942, The changes in ERA, FRA and the corresponding deductions when claim at the ERA remain rather stable.

[42] The benefits that are "taxed" away due to the earnings test are not lost but postponed at an actuarially fair rate.

Women could claim pension the earliest at age 60, either via the *women's pension*. Women with at least 15 years of contributions, of which at least 10 must have occurred after age 40, were eligible for the women's pathway. The penalty for retiring at 60 via women's pension was phased in gradually in monthly increments. The FRA for women's pension was increased from 60 to 65 by monthly steps since the cohort of 1941. For the cohort born in 1941, the penalty is 7.2%. For the cohort born in 1942, the penalty is 10.8%. It stabilised at 18% for cohorts born after 1945.

Another way to claim pension as early as age 60 is via *pension for severely disabled*. Workers who have lost their earnings capacity can claim the old-age pension for disabled workers. This pathway is also referred to as invalidity pathway. The eligibility condition is having lost of at least 50% of one's earnings capacity and at least 35 years of waiting period, which include, for example, periods of raising a child who is less than 10 years old. The ERA for this pathway was 60 for our sample. It is scheduled to gradually increase to age 62 between the 1952 and 1963 cohorts. The FRA for retiring at 60 via disabled pension was increased gradually by 1 month for each month of birth from 60 to 63 for cohorts 1941 to 1943. It is scheduled to gradually increase from age 63 to 65 for the 1952 to 1963 cohorts.

Another possible way to claim pension at age 60 is via *old-age pension due to unemployment*. The eligibility requirements for the UI pathway were: 1) at least 15 years of contributions, at least 8 of which must have occurred in the past 10 years, and 2) being unemployed for at least 1 year after the age of 58 and a half, or in old-age part-time work. For cohorts younger than 1946, ERA remained at 60. However, the FRA has increased gradually from 60 to 65 between the 1937 and 1941 cohorts. Therefore, for cohorts born after 1937, the penalty to claim at 60 ranges from 3.6% to 18%.

At age 63, workers with at least 35 years of contributions to claim the *old-age pension for long-term insured* as early as age 63 (SGB VI §236). The FRA without penalty for early claims was 63 until the 1936 cohort. It was increased gradually, in monthly steps, from age 63 to 65 for cohorts 1937 to 1938 and remained at 65 until the 1948 cohort. The ERA, meanwhile, remained stable at age 63. Hence, workers eligible for this pathway could always claim as early as age 63, however they faced an actuarial adjustment in the form of a 0.3% permanent pension reduction per each month they retired in advance of the FRA. For cohorts born after 1938, the penalty to claim at 63 remained at 7.2%.

2.B.3 An example of pension subsidy calculation

The *de jure* eligibility condition of the subsidy program requires only the average monthly EP of full-value contribution years at retirement (aep^t) to be less than 0.0625 (t is the year of retirement). Yet, because the average monthly EP of full-value contribution periods before 1992 (aep_i^{92}) cannot exceed 0.0625 after the subsidy, this implies that the *de facto* eligibility condition requires both aep^t and aep_i^{92} to be less than 0.0625. Following is one example showing how pension benefits and subsidy are calculated, provided the German Pension Office website:

Example: Calculation of the monthly average The total EPs for the contribution periods are 46.6909. Of this total amount, 31.6900 earning points are attributed to the 517 months of full-value contribution period. Of the 31.6900 earning points, 26.5000 earning points are attributed to 400 months of full-value contribution before 31.12.1991.

Solution

- Dividing 31.6909 earning points by 517 months gives us 0.0613 earning points. The monthly average of all full-value contribution periods does not reach (is below) the value of 0.0625.
- Dividing 26.5000 earning points by 400 months gives us 0.0663 earning points. The monthly average of all full-value contribution periods until 31.12.1991 reaches/is above the value of 0.0625.
- Therefore, additional (extra/add-on) earning points do not have to be calculated.

2.B.4 Details on pension-related periods

The total creditable/pension period (Wartezeit/Anrechenbare Zeiten) is approximately composed of the contribution period ((SGB VI § 55 Beitragszeiten) and the consideration period (SGB VI § 57 Berücksichtigungszeiten). The contribution periods consist of full value contribution periods (Vollwertigen Beiträgen) and reduced contribution periods (Beitragsgeminderte). Full value contribution periods are periods when compulsory contributions are paid according to the social security regulation. Reduced contribution periods include periods of unemployment, sickness and vocational training. During those periods, EPs are accumulated even though the worker has made no contributions. The consideration periods include child-raising periods. The time of raising a child to age 10 counts in the consideration period. The package is 10 years for one child, 15 years for two children and 20 years for more than two children.

Appendix 2.C Data Appendix

Our main dataset covers the universe of pensioners who left the the German public pension system between 1994 and 2018, provided by the German State Pension Fund (FDZ-RV). For the main analysis, we further restrict our sample to individuals born between 1932 and 1942 and who left the pension system due to death. For these cohorts, we observe all deaths that occurred between the ages of 62 and 76, as we only observe deaths that occurred between 1994 and 2018. For some of the older cohorts, we can observe deaths between 76 and 86; for some of the younger cohorts, we can observe deaths between 52 and 62. One potential concern for identification is that deaths before age 62 and deaths after age 76 can be affected by the eligibility conditions of the pension subsidy. In other words, we might have an eligible population who are healthier or less healthier to start with if that is the case.

To rule out this concern, we perform the following analysis. First, we check the impact of eligibility for the pension subsidy on probability dying between age 50 and 60 by using cohorts born between 1945 and 1955. For these cohorts, we observe all counts of death between 50 and 60. Note that because the subsidy is only available after claiming a pension, it is unlikely that subsidy eligibility affects death before claiming a pension. The only possibilities for selection are 1) anticipation effect, and 2) that the mortality trend between these ages changes by years of contributions and earnings levels exactly at the two cutoffs for pension subsidy. For people who died before claiming a pension, we impute contribution years at retirement by assuming a retirement age of 63. Therefore are only around 2% of the sample for whom we have made this correction. Column 1 of Table 2.A.4 shows that eligibility has no significant impact on probability dying between ages of 50 and 60 and the coefficient is close to zero and insignificant for the full sample and for men and women.

Second, we check the impact of eligibility for the pension subsidy on death after age 75 by using older cohorts. Specifically, we examine the impact on the probability of dying between the ages of 75 and 80 by using cohorts born between 1932 and 1937 and the probability of dying between 80 and 85 by using cohorts born between 1922 and 1931. Columns 2 and 3 of Table 2.A.4 show that eligibility has no significant impact on probability of dying between these older ages.

Appendix 2.D Details on Robustness

Several exercises further establish the robustness of the estimates. Table 2.A.16 shows the DID estimates by varying sample selection. First, column (2) shows that the estimates are robust to the exclusion of individuals who retired after exactly 420 months (35 years). Second, we narrow the bandwidth of *aep* to 0.6-0.9 in column (3). While the first stage effects on subsidy size and pension income are smaller,[43] the estimated changes in the probability of being a recipient and in mortality outcomes are similar to the baseline estimates. Third, we enlarge the bandwidth of contribution years to 30-40 in column (5). Results remain similar to baseline estimates. Fourth, our estimates are also robust to the inclusion of individuals born 1943 to 1948 and when restricting the analysis to cohorts born between 1932 and 1937, i.e. the cohorts born before the Second World War. Columns (6)-(8) show the estimates with the additional cohorts, with the additional cohorts while excluding individuals who retire with 420 months worth of contribution periods, with the additional cohorts and narrower *aep* restriction, and with the additional cohorts and larger *CY* bandwidth, respectively. Columns (7)-(11) show the estimates with cohorts 1932 to 1937. The estimated impacts are similar to the baseline results. For each specification, we also estimate the impact on the age at which individuals begin to claim pension benefits (panel (c)), and the point estimates are always close to zero. The only exception is Column (5), when we include retirees with less and more contribution years than our baseline bandwidth.

Appendix 2.E Calculation of the Monetary Gain in Life Expectancy

We perform a simple cost-benefit analysis by computing the associated increase in the value of a statistical life when receiving an additional 100€ pension benefits per month. Following are the steps of this calculation.

First, by combining our estimated improvements in the probability of dying before 70 and the life tables for the average German (Destatis, 2023), we calculate an implied average improvement of life expectancy at 60 of about 10.4 months for men.

Our IV estimates for men imply a 6.7 percentage point increase in the probability of surviving to age 70, conditional on living past age 60. Thus, the cumulative product of survival probabilities between ages 60 and 70 increases by 6.7 percentage points. We then calculate life expectancy at age 60 using this formula: life expectancy at age τ is calculated as $\sum_{j=\tau}^{\tau^m} \prod_{q=1}^{\tau^m-\tau} s(q)$, where τ_m is the maximum

[43] This is a consequence of the subsidy schedule, which decreases with aep^{92} after the 0.5 cutoff (see Figure 2.A.1). Because aep^{92} and aep are highly correlated, individuals with $aep \in [0.6, 0.75]$ are more likely to have $aep^{92} > 0.5$, on average, than individuals with $aep \in [0.45, 1.05]$.

attainable age (Collett, 2015). We assume $\tau_m = 100$. In the last step, we compute the gain in life expectancy as the difference between our estimated life expectancy at age 60 and the life expectancy implied by the life tables for the average German in 2000 (Destatis, 2023), which is considered as the life expectancy without the subsidy-induced increase in survival probability.

Second, we calculate the gain in the value of a statistical life. We use the value of a statistical life year at age 60 implied by Aldy and Viscusi (2007), which is 262,910€ . Thus, for each 100€ subsidy, the mortality improvements for men are worth 183,785€.

Lastly, we calculate the fiscal cost of providing the subsidy. Given an average pre-subsidy pension income of 686€/month for men in our treatment group, a 100€ increase in monthly pension benefits will cost about 31,224€ per male subsidy recipient. According to the life table for Germany in 2000 (Destatis, 2023), the life expectancy of men at the age of 60 is 21.75 years or 261 months. Thus, the net cost of the additional life expectancy due to a subsidy of 100€/month for the average male recipient is 100€*(261+10.4)+686€*10.4, which is about 31,224€. As we do not find any significant changes in the age when the pension is claimed, we do not take into account the loss of tax revenue due to early retirement.

Therefore, the net monetary benefit of the life expectancy gains in our sample is about 152,561€ on average per male subsidy recipient. pension subsidy program was cost-effective in increasing the life expectancy of male recipients.

Chapter 3

Intergenerational Returns to Migration: Evidence from Italian Migrants Worldwide

Joint with Guido Neidhöfer

3.1 Introduction

The prevailing rationale behind migration decisions is to enhance life opportunities, both for migrants themselves and for their offspring (Sjaastad, 1962). This notion aligns with theories and empirical evidence on intergenerational transmission and parental investments in children (see, e.g. Becker and Tomes, 1979; Borjas, 1993; Cunha and Heckman, 2007). Thus, parental migration decisions can be viewed as investments in their children's human capital and future prospects.

This paper tests for the first time whether migrants accurately anticipate these improvements when deciding to migrate and settle in a destination country. Specifically, we estimate the *intergenerational returns to migration* – i.e. the causal impact of parental migration and choice of destination country on their children's future outcomes – and analyze whether the parents' initial migration decision was influenced by the expectations of these long-term returns, alongside their own short-term income opportunities.

In the literature, migrants' performance have often been compared to that of natives in their destination country (e.g. Borjas, 1985; Bleakley and Chin, 2004; Card, 2009; Dustmann and Glitz, 2011; Abramitzky, Boustan, and Eriksson, 2014; Bönke and Neidhöfer, 2018; Abramitzky et al., 2021). However, a further appropriate comparison group to evaluate the impact of migration decisions on quality of life are non-migrants in their country of origin (*stayers*). Several contributions compared the outcomes of first generation migrants (*movers*) with the outcomes of stayers, hereby directly or indirectly providing estimates for the returns to the migration decision (e.g. Borjas, 1987; Abramitzky, Boustan, and Eriksson, 2012; Bryan, Chowdhury, and Mobarak, 2014; Parey et al., 2017; Borjas, Kauppinen, and Poutvaara, 2019; Lagakos, 2020; Corneo and Neidhöfer, 2021; Sarvimäki, Uusitalo, and Jäntti, 2022). Very few studies analyse the causal effect of migration on the long-term outcomes of migrants' children in the country or area of destination. Mostly, these studies focus on migration within countries (Chetty, Hendren, and Katz, 2016; Alesina et al., 2021; Nakamura, Sigurdsson, and Steinsson, 2022), while for international migration the existing evidence has been mostly of descriptive nature (Dustmann, Frattini, and Lanzara, 2012; Guveli et al., 2016; Zuccotti, Ganzeboom, and Guveli, 2017). Using unique administrative data and adopting a novel identification strategy, this paper provides new evidence on the causal intergenerational returns to migration across various destinations by comparing the performance of second-generation Italian migrants worldwide with that of their peers in Italy. Most importantly, our findings present the first empirical evidence that migrants consider the future prospects of their children when selecting a destination country.

Our main data source is the Registry of Italian Citizens Living Abroad (AIRE - *Anagrafe Italiani Residenti all'Estero*) collected by the Italian Ministry of Foreign Affairs and the Italian embassies around the world. Our dataset includes administrative information on around five million Italian migrants worldwide and their household members, even if the latter do not possess an Italian citizenship, in 2015. Using information on residents of Italy from the Survey on Household, Income and Wealth collected by the Bank of Italy, we compare the level of education, employment status, and predicted income of Italian second generation migrants to that of their peers in Italy.[1]

Since self-selection of (first generation) migrants is a known identification threat in the study of migrants' performance (e.g. Borjas, 1987), selection in the parents' generation might affect their children' performance via parental investments and intergenerational transmission of human capital (e.g. Borjas, 1993). Our dataset is unique in that it provides information on several background characteristics, including parents' education, age, Italian place of origin and exact destination in the host country, which we use to abstract from self-selection on observable characteristics.

[1] A peer is a resident of Italy born in the same year as the second generation migrant, who lives in the Italian region from where his or her parents migrated from. Information on education and occupation are reported in AIRE data, while income is not. Hence, we use the Luxembourg Income Study, a worldwide cross-country representative survey, to estimate second generation migrants' earnings and income in each destination country, and compare them to that of their Italian peers.

However, as migrants might be also selected on unobservable characteristics, such as innate skills and motivation, we adopt a multinomial logit selection bias correction model to take also this dimension of selection into account (for a review of the methodology, see Bourguignon, Fournier, and Gurgand, 2007). This identification strategy allows us to abstract not only from selection into the migration decision, but also into the choice of destination country. In the estimation, we rely on the exogenous variation of two proxies, following the literature on push and pull factors influencing migration choices (e.g. Borjas, 1987; McKenzie and Rapoport, 2010; Beine, Bertoli, and Fernandez-Huertas Moraga, 2016). On the one hand, we measure push factors from each Italian region through the migration cohort size, that is the number of emigrants from the parents' Italian region of origin and birth cohort. On the other hand, we measure pull factors into each destination country as the interaction of the Gini coefficient of disposable income in each destination country and the parents education level. This follows the established literature results that high skilled migrants are often attracted to more unequal countries, and low skilled migrants to more equal ones (see, e.g. Parey et al., 2017; Borjas, Kauppinen, and Poutvaara, 2019; Corneo and Neidhöfer, 2021).

Our findings show that the intergenerational returns to migration of Italian migrants are strongly heterogeneous by destination country, gender, and parental socioeconomic background. Returns in terms of educational attainment are not always positive, reflecting differences in education systems and incentives to undertake vocational training instead of college in different countries. However, on average the children of Italian migrants in most destination countries are outperforming their peers in Italy in terms of employment status and predicted income.

Finally, we test whether migrant parents have been maximizing intertemporal utility by examining whether parents place greater emphasis on their own income or on their children's future opportunities when deciding which country to migrate to. We estimate an alternative specific conditional logit model of the probability of choosing each destination country as a function of migrants' and their children' expected income. This allows us to quantify the relative importance the two dimensions play in the migration choice, and to give some insights of intergenerational maximization of the utility function. We find that children' expected income plays an important role in the choice of destination country for individuals that migrated after the birth of their first child. These migrants seem to be willing to give up some income in exchange of an increase in their children' expected income when deciding in which country to resettle. These findings confirm a common wisdom that most parents would acknowledge (see for instance the anecdotal evidence included in Abramitzky and Boustan, 2022): parents give a high weight to their children's future opportunities when taking (migration) decisions, sometimes even at the expense of their own immediate income gains.

Our paper contributes to the literature examining the impact of parental migration on children's outcomes. One part of this literature focuses on the effects of children left-behind by their parents in the country or region of origin (e.g. McKenzie and Rapoport, 2011; Antman, 2013; Meng and Yamauchi, 2017). Other works study the situation of children born or raised in the host country. Dustmann, Frattini, and Lanzara (2012) and Zuccotti, Ganzeboom, and Guveli (2017) use PISA data and the European Social Survey, respectively, to investigate the educational achievements of second-generation Turks relative to their peers in Turkey, finding that children of Turkish emigrants generally attain higher educational outcomes than those who remain in Turkey. However, the analysis by Zuccotti, Ganzeboom, and Guveli (2017) reveals a more nuanced picture regarding labor market results. Although these children surpass their parents' occupational levels, their employment outcomes are not as favorable when compared to similarly educated individuals in Turkey. Guveli et al. (2016) confirm these patterns by collecting information on 2000 Turkish families and their descendants, including those who migrated to Europe as well as those who stayed behind. To the best of our knowledge, our paper provides the first causal evidence of the impact of migration on second generation migrants' educational achievements and labor market performance based on administrative data.

Furthermore, our paper is related to studies that estimate the impact of growing up in specific places or neighborhoods on children's performance (e.g. Damm and Dustmann, 2014; Chetty, Hendren, and Katz, 2016; Chyn, 2018; Deutscher, 2020; Alesina et al., 2021; Derenoncourt, 2022; Nakamura, Sigurdsson, and Steinsson, 2022).[2] As in some of these studies, we use the variation in age at arrival at the new place to study the impact of migration. Chetty and Hendren (2018a, 2018b) analyse internal migrants in the USA and find that moving to a lower poverty or upward mobility area at a young age improves substantially long-term education performance and earnings. (Deutscher, 2020) confirms these findings for Australia. Chetty, Hendren, and Katz (2016) further elaborate on the effect of moving into different neighborhood at different ages, and find significant evidence on childhood exposure effects: children moving at later ages benefit less from exposure to better neighborhood than their younger peers. Alesina et al. (2021) focus on intergenerational mobility in educational attainment in Africa. They find strong regional differences in mobility and establish that both spatial sorting and regional exposure

[2] For a review of place effects, see (Chyn and Katz, 2021).

effects are behind such differences. Their results show that additional years spent in a high-mobility region at age 5-12 significantly increases the likelihood for children of low-educated parents to complete primary school. Our findings are in line with these results, and show a convergence towards the achievement of Italian peers if migration happened from the end of primary school onward (i.e. about age 11). Instead, children born in the host country and those that migrated at earlier ages have substantially higher outcomes. Interestingly, the length of stay of the family in the country of destination before the child was born does not contribute to higher outcomes.

The remainder of this work is organized as follows. Section 3.2 outlines our empirical strategy, describing the adopted bias correction procedure to estimate intergenerational returns to migration, and the alternative specific conditional logit used to test for intertemporal utility maximizing behaviour. Section 3.3 describes the data sources. Section 3.4 shows our main results, including heterogeneity analysis and robustness checks. Finally, Section 3.5 draws some concluding remarks.

3.2 Empirical strategy

3.2.1 Returns to migration

The first part of this paper estimates returns for 2G migrants to their parents' migration choice. As an outcome, we consider different education, labour market performance, and predicted income measures. Educational outcomes are measured in terms of number of schooling years and likelihood to obtain a tertiary education degree. Labour market performance is measured as likelihood of being employed, unemployed or inactive. As mentioned in the previous section, income is predicted from LIS data on the basis of education, employment status, gender, and age of 2G migrants. Therefore, they provide a summarizing statistics for the 2G migrants' returns to migration. We predict equivalised household income, yearly earnings, and hourly earnings. The latter to measures are predicted separately for males and females, and all income measures are estimated in 2015 PPP USD.

The simplest way to estimate 2G's returns to migration is to run the following regression:

$$y_{ik} = \alpha_0 + \boldsymbol{\alpha_1} \boldsymbol{c}_{ik} + \boldsymbol{\alpha_2} \boldsymbol{X}_i + \mu_{ik} \tag{3.1}$$

where y_{ik} indicates the outcome of interest for individual i in country k, $\boldsymbol{c}_{ik}$ is a set of indicators for residence country of individual i[3] and $\boldsymbol{X}_i$ includes gender and age

[3] Italy is taken as base.

controls. Returns to migration, with respect to Italian peers, are captured by the vector of $\boldsymbol{\beta}$ coefficients.

However, 1G migrants are not a random sample of the residents of Italy in the year of migration. Self-selection in parents generation likely determines self-selection in the population of their children as well, through human capital and skills transmission. If self-selection plays a role, OLS estimates of $\boldsymbol{\beta}$ would be biased (Heckman, 1979; Dubin and McFadden, 1984; Dahl, 2002). The first straightforward attempt to account for self-selection is to control for parental characteristics in Equation 3.1. Specifically, AIRE includes information on last Italian region of registration and, for our baseline linked sample, parents' education.[4] Thus, Equation 3.1 becomes:

$$y_{ik} = \beta_0 + \boldsymbol{\beta_1 c_{ik}} + \boldsymbol{\beta_2 X_i} + \boldsymbol{\beta_3 P_i} + \varepsilon_{ik} \tag{3.2}$$

where $\boldsymbol{P}_i$ includes 1G's education and Italian region of origin fixed effects. Even with these additional controls, equation 3.2 does not take into account self-selection of 1G migrants into unobservable characteristics. OLS estimates of $\boldsymbol{\beta}$ would still be biased, for instance, if, for a given level of education, migrants had peculiar skills or abilities in comparison to stayers. Because we cannot rule out this possibility a priori, we adopt a two-step self-selection bias correction procedure (see Bourguignon, Fournier, and Gurgand (2007) for a review).

In a first step, we estimate the probability of migrating into each destination country via multinomial logit. In comparison to a probit or logit model, this has the advantage of taking into account selection in both the migration choice and the choice of host country. Thus, for each destination country j, we estimate via multinomial logit:

$$P_{ij} = \theta_0 + \theta_1 Z_{ij} + \theta_2 X_i + \eta_{ij} \qquad \forall j \tag{3.3}$$

where P_{ij} indicates the probability that i's parents decided to migrate into country j, and Z_{ij} indicates a vector of excluded variables related to the migration choice. These mostly come from previous literature on migration and are meant to provide an indication of push and pull factors. As proxy for push factors, we use the size of the migration cohort, that is the number of migrants born in the same Italian region and belonging to the same birth cohort[5] as i's parents. As proxy for pull factors, we consider a measure of education-specific inequality: the interaction of the Gini index

[4] If both mother and father information are available, we compute parents' education as the highest level of education reached by at least one parent. The Italian region of origin is the last residence of the first parent migrating.

[5] We divide birth cohorts into 5-years groups.

in i's host country at the time of i's birth and i's parents' education level. For each 2G migrant we use predicted values of P_{ij} from 3.3 to estimate the probability of their parents to migrate in each potential destination country.

In a second step, we estimate Equation 3.2, including as additional control a function of the estimated migration probabilities ($\hat{P}_{ij}$):

$$y_{ik} = \gamma_0 + \boldsymbol{\gamma_1} c_{ik} + \boldsymbol{\gamma_2} X_i + f(\hat{P}_{ij,\forall j}) + \nu_{ik} \tag{3.4}$$

The choice of the exact specification of $f(\hat{P}_{ij})$ varies across the literature. In line with Dahl (2002) and simulation results Bourguignon, Fournier, and Gurgand (2007), the self-selection bias correction term is given by a quadratic polynomial expansion of i's parents' the probability of migrating in country k:

$$f(\hat{P}_{ij,\forall j}) = \lambda_1 \hat{P}_{ik} + \lambda_2 \hat{P}_{ik}^2 \tag{3.5}$$

The choice of the degree of polynomial to include in the regression must take into account the trade off between more precise specification of the self-selection bias correction term and number of parameters to be estimated in the regression (curse of dimensionality). Our selection bias correction terms aims at balancing these two factors. Nonetheless, robustness checks show our results are robust to different specifications of the bias correction term. Figure 3.A.5 depicts the estimates of 2G returns to parental migration in each destination country when different specifications of $f(\hat{P}_{ij})$ are considered. Most point estimates are similar to each other, and their differences are never significant. Results are also reported in Tables 3.A.7 and 3.A.8.

As a robustness check, we also estimate returns to migration using an instrumental variable approach. As instruments, we use the same Z_{ij} of regression 3.3, either together or separately.

3.2.2 Intertemporal utility maximization

As a final step of our analysis, we estimate the relevance of second generation's expected returns to migration in parents' utility function. The idea is quite intuitive. When evaluating whether to migrate, parents consider not only the expectation of better opportunity for themselves, but for their children as well. We propose a test for this statement, through the estimation of an alternative-specific conditional logit model à la McFadden et al. (1973). To some extent, this can be considered a test of parents' altruism with regard to their children. Measuring opportunities through expected income in the destination country, we estimate the following equation:

$$P_{ij} = \kappa_0 + \kappa_1 \hat{y}_{ij}^{1G} + \kappa_2 \hat{y}_{ij}^{2G} + \boldsymbol{\kappa}_3 X_i + \rho_{ij} \tag{3.6}$$

where P_{ij} is the probability that i's parent migrates in country j, $\hat{y}_{ij}^{2G}$ is the expected income in country j, $\hat{y}_{ij}^{1G}$ is their parents' expected income in j and X_i indicates i's demographic characteristics and Italian area of origin fixed effect[6]. Both $\hat{y}_{ij}^{2G}$ and $\hat{y}_{ij}^{1G}$ are estimated from LIS data by country of destination. Our hypothesis is that, if i's parents took i's expected future income into account when taking the migration choice, then κ_2 should be positive and significant. We expect also κ_1 to be non-negative.

3.3 Data

We exploit three data sources: the Registry of Italians living abroad (Anagrafe Italiani Residenti all'Estero), the Survey on Household Income and Wealth (SHIW) and the Luxembourg Income study (LIS).

Anagrafe Italiani Residenti all'Estero (AIRE). The basis of our analysis is the Registry of Italians living abroad. This is an administrative dataset containing information on the full population of Italian emigrants abroad, managed by the Italian Ministry of Foreign Affairs. We have access to about 88% of all Italian citizens registered in AIRE worldwide. This includes 4,067,604 Italians in thirteen foreign countries in the years 2014-2015, as well as information on their spouses and children. Italian citizens are required by law to register to AIRE: (i) upon moving abroad for at least 12 months; (ii) if born abroad; (iii) if they acquired Italian citizenship while abroad (for instance through marriage). Even though there is no penalty for not registering to AIRE, incentives to do so are rather strong for anybody planning to stably reside in a foreign country. First, registration to AIRE brings substantial fiscal advantages. It allows to avoid double taxation by the Italian fiscal authority, and upon return in Italy makes one eligible for a substantial discount on taxable income (a reduction between 50 and 70% for 3 to 10 years depending on education level, sector of employment and number of children). Second, registration is required in order to be able to renew documents (id, passport, driving licence) at the local embassy. Registration is also necessary to register marriage and transfer Italian citizenship to children. Finally, it allows to vote for Italian elections at the local embassy, instead of having to travel back to the original municipality of origin

[6] Thus, X_i are characteristics constant across choice.

or to vote per post - which must be requested for each election through a rather cumbersome administrative process. Thus, AIRE data should deliver an accurate picture of the stock of Italian emigrants living in a given country at a given point in time and planning to spend a substantial part of their life there. On the other hand, AIRE data are unlikely to include information on short-term migrants, such as students or seasonal workers. Other information contained in the data are country of residence and birth, date of birth and of arrival in the host country, last Italian region of residence (for those born in Italy), gender, education and occupation.

Even when focusing on long-term migration, AIRE data have some limitations. One concern is related to the absence of income information, which must be imputed through survey data (see Appendix 3.C for details on the imputation procedure). Second, AIRE does not include individuals who lived a substantial part of their life abroad but returned to Italy before 2014-2015, and only allow us to observe individuals' last destination abroad. Finally, the most important concern is related to the tracking of individuals once they exit their parents' household. Family identifiers change, in some cases, once the individual moves away from the embassy in which their origin family is registered. This happens whenever one fills out a new registration, instead of updating the previous one - which is often required upon moving into a new foreign country. We explore how these limitations could affect our results in Section 3.4.1.2.

***Survey on Household Income and Wealth* (SHIW).** To derive information on stayers and their descendants, we use the Survey on Household Income and Wealth (SHIW) provided by the Bank of Italy. The SHIW collects information on Italian households, including individual characteristics for each adult household member. It collects information on about 8,000 households (20,000 individuals) with a biannual frequency between 1977 and 2016. For the purpose of our study, we use information from survey wave 2014, in line with the information available in AIRE. Section 3.4.1.2 shows results are robust to using different waves of SHIW. We use these data to estimate counterfactual educational, occupational and income outcomes of stayers' descendants.

***Luxembourg Income Study* (LIS).** The Luxembourg Income Study Database is the largest available income database worldwide. It includes data on 50 countries from 1980 to 2019, and collects information at household and personal level on income, demography and employment. We use LIS data to estimate the household income and hourly earnings in the country of residence of emigrants and their counterfactual incomes in Italy. This part of the analysis is based on survey sample for each destination country and Italy around year 2014 and exclude Argentina, New Zealand

and Venezuela, as they are not in LIS. Using country-specific LIS data we estimate an augmented Mincer regression of equivalised disposable household income and hourly earnings on demographic controls (age and gender) and education level of the individual.[7] We estimate income variables excluding individuals with disability from the each country's population, including a gender dummy or by estimating two different equations for male and females. Further, we estimate income variables either on the basis of natives' or of migrants' population in each destination country[8], to obtain respectively an upper and lower bound of emigrants' income estimates. Unfortunately, because of data limitations we cannot estimate this Mincer-type of equation from 2G Italians in all destination country. We provide some evidence of how these estimates are, if anything, a lower bound of the actual income of 2G Italians around the world in Section 3.4.1.2. [9]. In the case of Italy, we only estimate income on the basis of the natives' population. Finally, apply estimated Mincer coefficients to AIRE and SHIW data to obtain estimates for household income and hourly earnings. More information on how exactly the estimation in LIS is carried on can be found in Appendix 3.C.

Main sample. Our main sample combines second generation (2G) migrants in AIRE with Italian residents in 2014 from SHIW. We define 2G migrants as individuals registered in AIRE in 2015 born abroad or registered in AIRE before age 18, and with at least one known first-generation (1G) parent (see Appendix 3.B for details on how we define generations). We show some robustness checks on these restrictions and heterogeneity by age of migration in Section 3.4. A 1G parent must be born in Italy and be registered to AIRE after age 18. Furthermore, we restrict the sample to individuals old enough to complete a full higher education cycle (up to tertiary education). As information in AIRE are likely to be updated upon documents renewal[10], we restrict the sample to individuals born before 1980 - that is, at least 35 in 2015. Because we are going to focus on labour market outcomes, we further exclude individuals older than 55 in 2015 (i.e. born before 1960). We also exclude individuals for whom information on occupation or parental education

[7] Equivalised household disposable income is the sum of all income types perceived by any member of the household, net of tax and transfers, divided by the number of equivalent adults living in the household (the square root of the members, following LIS recommendations). Hourly earnings refer to the total earnings perceived by the individual in a year divided by the reported amount of worked hours.

[8] Except for Italy, where we only exploit natives' population to estimate income.

[9] Because of the wide heterogeneity within immigrants in a destination country, estimates on the migrants' population lead to a rather dispersed income distribution among this group in each country. Hence, this can only provide a lower bound of the actual income perceived by Italians emigrants.

[10] Italian passport and ID are renewed every ten years.

is not available. The former restriction ensures, again, that at least the education information are up-to-date. We further drop individuals residing in New Zealand or the Netherlands in 2015, because of the limited sample size in these countries. Summary statistics for our main estimation sample are reported in Table 3.A.1. Compared to Italian residents in 2015, migrants' children are slightly younger and more often males. Furthermore, their parents more often come from centre-Italy regions. They more often obtain a higher than compulsory education degree, even though their parents' education is not different from the education of their Italy-resident peers' parents. In terms of predicted income, on average, 2G migrants earn more per hour and also perceive an higher disposable income than their Italian peers. This holds independently of the baseline population used in LIS for the income prediction. Table 3.A.2 reports sample size by generation and destination country. Note that because of LIS data limitations, the sample looses Argentina and Venezuela when income is an outcome.

3.4 Results

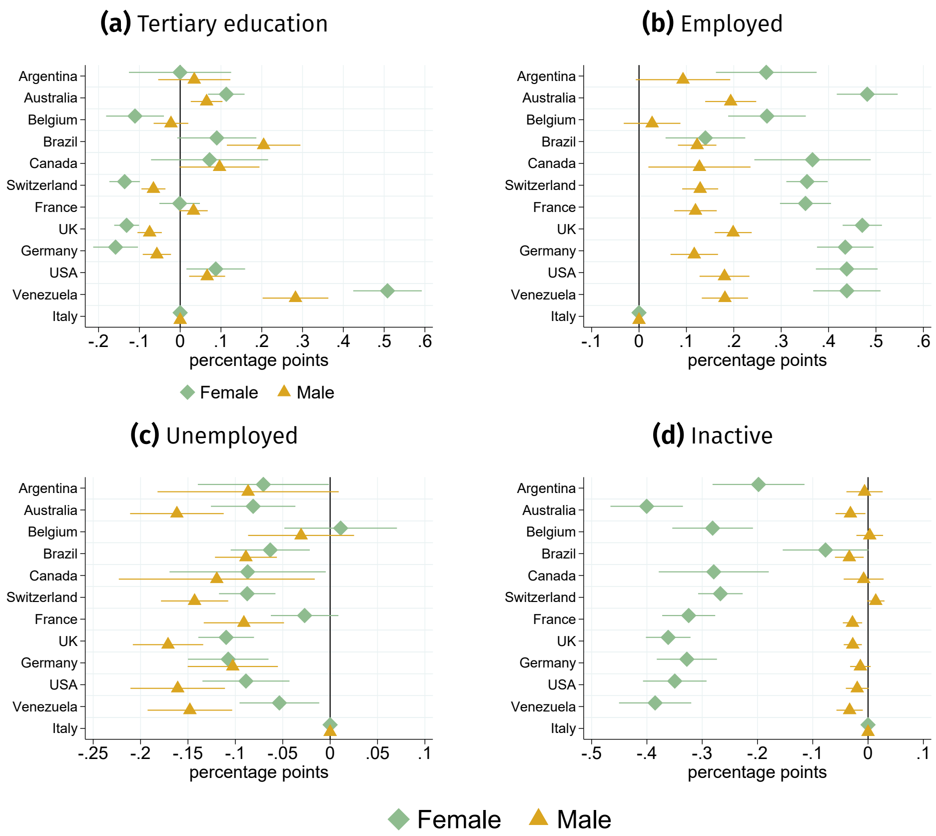
In this section, we first present estimation results under the multinomial logit framework. We show the effect of host country choice on education, occupation and monetary outcomes of 2G Italian migrants worldwide. We provide evidence of heterogeneous results by gender and parental background, as well as a number of robustness checks. Finally, we test if parents took into account their children' outcomes in the choice of host country, in an intertemporal utility maximization framework. We show the relative importance of parental and children' income in the choice of destination country via simulation.

3.4.1 Intergenerational returns to migration

Educational and occupational outcomes. First, we examine the effect of parental migration on education and likelihood of employment of their children.

Figure 3.1 shows the host country effect on the likelihood of completing tertiary education by gender of the second generation. Coefficients for each destination country are to be interpreted as compared to the Italian case. For instance, 2G Italian males and females in Argentina are not more likely to obtain tertiary education than their male and female Italian peers. Overall, we document heterogeneous returns in terms of education by both host country and gender. 2G Italians migrating into European countries are generally less likely to complete tertiary education than their Italian peers - with the only exception of 2G French men. On the other hand, it

Figure 3.1. Returns to migration: education and employment.



Notes: Figure 3.1 displays estimated returns to migration by country of destination in terms of likelihood of the second generation completing a tertiary education degree (Panel a), being employed (Panel b), unemployed (Panel c) or inactive (Panel d) by gender. Reported coefficients are those of country fixed-effects. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

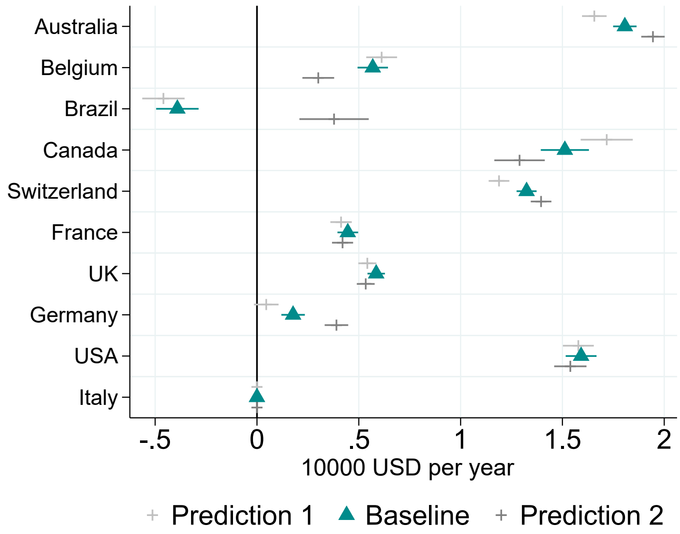
seems 2G Italians migrating to extra-European countries are at least as likely than their peers in Italy to complete tertiary education. Gender differences are important only in some countries, namely Switzerland, UK, Germany and Venezuela. Because these results are to be interpreted net of parental self-selection, the heterogeneous effects are to be ascribed mainly to institutional differences in the school system of the different countries. For instance, the German school system typically incentives vocational paths more than the Italian one. Thus, 2G Italians in Germany are more likely to undergo vocational training and hence to not complete tertiary education. As we are about to see, this will not prevent them to reach better living standards than their Italian peers - at least as far as our measures can indicate. Table 3.A.9 columns (1) and (2) report exact estimates by destination country.

Figure 3.1 shows the host country effect on the likelihood of being employed, unemployed, or inactive by gender. Even though 2G migrants are sometimes less likely to complete tertiary education, results on occupational outcomes indicate they're always more likely to be employed than their peers in Italy. This is true for both male and female 2G migrants, even though females show the strongest returns. It is interesting to notice how female are substantially less likely to be inactive than their peers in Italy, while males are more unlikely to be unemployed. Notably, Italy is the country with lowest female labour force participation rate among the ones under study. This likely drives the results for female 2G migrants. Table 3.A.9 columns (3)-(8) report exact estimates by destination country.

Income. To provide a summarizing measure of the performance of 2G Italian migrants around the world, and a better proxy for their actual living standards, we predict their income by destination country using their education level, employment status and demographic characteristics (see Appendix 3.C for details). We use predict to income measures: yearly equivalised household disposable income (henceforth household income), and hourly earnings. The latter measure is estimated separately for males and females. To get a measure of their position in the host country income distribution, we also predict their household income percentile. Estimated results for all monetary outcomes are reported in Table 3.A.10 for baseline, prediction 1 and 2 estimates.

Figure 3.2 shows host country effect on estimated household income. Different income predictions depend on the underlying LIS population used to estimate income: baseline estimates use the full LIS population, prediction 1 estimates use only natives in LIS, and prediction 2 estimates use only migrants in LIS. This should provide a range in which the actual income of 2G Italian migrants likely lies. Results are quite striking: with the only exception of Brazil, 2G Italian migrants perceive higher

Figure 3.2. Returns to migration: estimated household income.



Notes: Figure 3.2 displays returns to migration in terms of estimated equivalised household income. Reported coefficients are those of country fixed-effects. Prediction 1 considers only natives and Prediction 2 only migrants in LIS. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). Monetary values are expressed in 2015 PPP USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

household income than their peers in Italy. It is important to notice, however, that even though income is expressed in PPP and thus estimates consider different living costs in different countries, they do not take into account differences in welfare state generosity. Indeed, one must keep such differences in mind when trying to estimate different living standards across countries. For instance, we estimate 2G Italian migrants in USA perceiv about 15 thousands USD more than their peers in Italy. However, they likely will have to face higher education and health costs, making the net effect unclear. Nonetheless, we estimate a positive monetary gain for 2G Italian migrants even in countries where the welfare system is typically more generous than the Italian one, such as France or Germany, which gives a clear indication of positive net gains from parental migration.

Interestingly, higher gains in absolute terms do not correspond to gains in positions in income distribution. Figure 3.A.6 shows the effect of migration in terms of percentile of the household income distribution in the destination country relative to that in Italy. Indeed, our estimates suggest 2G migrants loose positions in most destination countries with respect to the positions of their Italian peers, with the only exceptions of Australia and Brazil. Prediction 1 and 2 estimates for this plot have also an interesting interpretation. In these cases, the position in the income distribution is computed with respect to the distribution of the underlying population, i.e. respectively natives and migrants in the host country. That is, prediction 1 results imply that 2G Italian migrants are often poorer than natives but substantially richer than other migrants groups in their host country.

Finally, we investigate the effects of parental migration on hourly earnings of their children. Columns (4)-(6) of Table 3.A.10 shows positive monetary gains are, at least partially, driven by higher wages for both male and females. Returns are positive for male 2Gs in all destination countries, and are mostly stronger than those for female 2Gs. In UK and Germany, female 2G migrants do not earn more per hour than their peers in Italy.

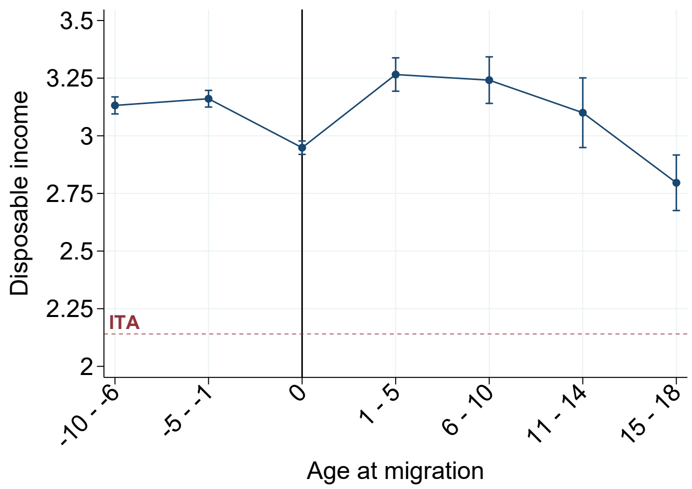
3.4.1.1 Heterogeneity

We explore two important dimensions of heterogeneity in our results - other than the already discussed gender differences. First, we look at heterogeneity by parental background. Second, we look at heterogeneity by age at migration, in the spirit of Chetty, Hendren, and Katz (2016) and Alesina et al. (2021). Finally, we look at heterogeneous effects by number of Italian parents.

Figure 3.A.7 shows returns in terms of our main outcome variables by parents' education level. We distinguish between 2G migrants whose parents at most completed compulsory school (low education) from those whose parents completed higher school levels (high education). Overall, we see 2G migrants from lower socioeconomic backgrounds gain more from their parents' migration choice: even though they do not always complete tertiary education, they are more likely to be employed and often perceive higher income and hourly earnings than their Italian peers and 2G migrants from higher SES background. Thus, better opportunities given by the parents' migration choice are especially exploited by those with more room for improving their initial conditions.

Figure 3.3 shows the effect of parental migration on 2G migrants' household income by age of migration. Negative values indicate families that registered to AIRE before the birth of their first child. Red line indicates household income for residents of Italy. In line with previous literature, we find that migrating at a younger age implies a better performance in monetary terms. For 2G migrants that arrived in their host country later on, returns to their parents' migration choice are somewhat smaller, with the decline starting if they migrated after completing the first school cycle.

Figure 3.3. Age effect.



Notes: Figure 3.3 displays estimated returns to migration by country of destination in terms of estimated yearly equivalised household income by 2G's age of migration. Reported coefficients are those of country fixed-effects. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). Monetary values are expressed in 2015 10,000 USD per year. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.13 depicts estimated returns in terms of household income by number of parents with Italian citizenship. In most countries, having one or two Italian parents does not affect income returns. In Australia, Canada, and UK having two Italian parents seem to imply a marginally higher income for the second generation. Unfortunately, we do not have information on which citizenship(s), other than the Italian one, an individual holds. Thus, we cannot be sure the parent without Italian nationality is a citizen of the host country.

3.4.1.2 Robustness checks

We perform a number of exercises to make sure our estimates are robust. First, we show robustness of our results to different specification of the parents' self-selection bias correction term. Second, we discuss potential selection of the 2G migrants' that can be linked to their parents with respect to those that cannot be linked - and hence are excluded in the main analysis. Third, we reproduce our results in an IV framework, to show the adopted method indeed take parents' self-selection bias into account. Finally, we show our estimated results are robust when considering different waves of SHIW for the residents of Italy, to including 1G migrants born between 1960 and 1980 to the Italian residnets group, and to using different waves of LIS to estimate monetary outcomes.

Accounting for self-selection. Figure 3.A.3 looks at how estimated returns depending on whether we control or not for self-selection of the first generation. Specifically, we compare estimates $\hat{\boldsymbol{\alpha}}_{\mathbf{1}}$, $\hat{\boldsymbol{\beta}}_{\mathbf{1}}$, and $\hat{\boldsymbol{\gamma}}_{\mathbf{1}}$ from, respectively, Equations 3.1, 3.2, and 3.4. For all outcomes, controlling for parental observable characteristics takes already into account most of 1G's selection. To some extent, this is not surprising, as 1G's selection only impacts 2G's outcomes through human capital transmission. Thus, if cultural background (as proxied by Italian region of origin) and education level are capturing most of dimensions through which human capital is transmitted from one generation to the other, these two variables would be enough to take self-selection of the first generation into account.

Self-selection bias correction term Section 3.2 describes the self-selection bias correction procedure we adopt to control for self-selection on unobservables of 1G migrants. We mentioned there one could consider different specifications of $f(\hat{P}_{ij,\forall j})$, the bias correction term. Figure 3.A.5 shows estimated returns are not sensitive to the specification of $f(\hat{P}_{ij,\forall j})$. Specifically, we consider the following polynomial functions:

$$
\begin{array}{ll}
Prob & \sum_j \hat{P}_{ij} \\
Prob2 & \sum_j \hat{P}_{ij} + \sum_j \hat{P}_{ij}^2 \\
Prob3 & \sum_j \hat{P}_{ij} + \sum_j \hat{P}_{ij}^2 + \sum_j \hat{P}_{ij}^3 \\
Prob4 & \sum_j \hat{P}_{ij} + \sum_j \hat{P}_{ij}^2 + \sum_j \hat{P}_{ij}^3 + \sum_j \hat{P}_{ij}^4 \\
Prob4+Int & \sum_j \hat{P}_{ij} + \sum_j \hat{P}_{ij}^2 + \sum_j \hat{P}_{ij}^3 + \sum_j \hat{P}_{ij}^4 + \sum_j \sum_{w \neq j} \hat{P}_{ij}\hat{P}_{iw} \\
Dahl & \hat{P}_{ik} \\
Dahl2 & \hat{P}_{ik} + \hat{P}_{ik}^2 \\
Dahl3 & \hat{P}_{ik} + \hat{P}_{ik}^2 + \hat{P}_{ik}^3 \\
Dahl4 & \hat{P}_{ik} + \hat{P}_{ik}^2 + \hat{P}_{ik}^3 + \hat{P}_{ik}^4
\end{array}
\tag{3.7}
$$

Naturally, a higher order polynomial or the inclusion of interaction terms leads to widening of the confidence intervals, but point estimates remain mostly close to each other. The only two exceptions to this pattern are, to some extnet Brazil and Venezuela. In both cases, the inclusion of additional elements to the bias correction term makes education and occupation estimates more negative. Such differences are, however, mostly contained for monetary outcomes.

2G self-selection. In section 3.3, we mention we can link some of the descendants of Italian migrants with their parents. This is possible because, in some cases, embassies keep the same family identifiers for individuals even after they move out of their parents' household. We can link about 14% of migrants' descendants with their parents. Even though there is no official rule on who gets to keep the same family identifier and who will get a new one, we observe about 98% of linked descendants live in the same consulate area as their parents. To make sure that the linked sample, of which our baseline sample is a subset, is not positive selected[11] with respect to the full population of 2G migrants, we compare their characteristics with that of any individual registered to AIRE born between 1960 and 1980 outside of Italy. These are likely migrants' descendants that entered the sample with a different family identifier of their parents. Table 3.A.4 reports mean values of demographic and socioeconomic characteristics of the linked and not linked sample and test for their differences. We observe linked individuals are a few years younger, slightly more often males. Geographical differences with respect to their parents' Italian region of origin are not strong, with linked families being four percentage points more likely to come from central Italy. Notably, linked individuals are less often holding a tertiary education degree and three percentage points more likely to be employed, one percentage point more likely to be unemployed and about 5 percentage points less likely to be inactive. We further test for differences between the linked and not linked sample by estimating the following regression:

$$y_{ij} \quad = \delta_0 + \delta_1 \mathbb{I}\{linked\} + \boldsymbol{\delta_2 D_{ij}} + phi_{ij} \tag{3.8}$$

where $\mathbb{I}\{linked\}$ is an indicator for i being in the linked sample and $\boldsymbol{D_{ij}}$ including age, gender, and italian region and host country fixed effects. Table 3.A.5 reports estimated results for δ_1 when the likelihood of obtaining tertiary education and employment, with and without controlling for $\boldsymbol{D_{ij}}$. We estimate those in the linked sample are less likely to have completed tertiary education by about 8 percentage points, and about 2 percentage points more likely to be employed. We explore these differences by country in Figure 3.A.2. We see linked individuals are less likely to complete tertiary education in any destination country (Panel (a)). However, they

[11] We are mostly interested in testing for positive selection because, if the linked descendants are negatively selected, our results on employment and income would constitute a lower bound of the actual gains from migration.

are more likely to be employed in Argentina, Belgium, Brazil, Canada, UK, Germany and USA, and less likely to be employed in the remaining destinations. Differences, however, are not higher than 6 percentage points.

To fully rule out any positive bias on our estimated returns to migration, we estimate them again for our baseline sample and a larger sample including not linked individuals. In this case, we can only estimate Equation 3.2, using the information on the Italian region of origin (also available for not linked individuals) to control for self-selection on observables. Results for our main outcomes are depicted in Figure 3.A.10. For all outcomes, we see that results for the not linked sample are more positive than for our baseline sample, confirming we are likely estimating a lower bounds of intergenerational returns to migration.

IV. To check robustness of our results, we adopt as alternative identification strategy a more traditional instrumental variable approach. In this case, we use the excluded variables described in Section 3.2 as instruments for the probability of migrating into each destination country or staying in Italy. Relevance of the instrument is ensured by first stage results, reported in Table 3.A.13. As for exogeneity, we argue, on the one hand, that the size of the parents' migration cohort can only affect the parents probability of migrating, while it does not directly affect outcomes for the second generation. It is important to notice this is not a measure of network in the destination country, as it is origin-area specific only. Second, parents' education specific Gini index in the destination country will hardly impact 2G's performance directly, once we control for Gini index and parents' education separately in the regression. On the other hand, country inequality is an important skill-specific pull factor, as postulated by the welfare magnet hypothesis and empirically tested using this dataset by Corneo and Neidhöfer (2021). Second stage results compared with our baseline estimates are depicted in Figure 3.A.16 and reported in Table 3.A.14. For all outcomes, we do not see virtually any significant change in our results.

Income estimation. Figure 3.A.12 show estimated returns in terms of household income when different waves of LIS are used to predict income. Our baseline estimates use LIS 2014 waves to predict income. When using 2010 or 2016 waves, results remain close to our baseline, with changes being mostly not statistically significant or within 2000 USD/year. The picture is similar for the natives and migrants LIS population.

Figure 3.A.8 predict income in LIS for those country in which it is possible to identify a population closer to 2G Italian migrants and uses that population to predict income. Specifically, we estimate income in France using only individuals whose parents have a migration background, but do not necessarily come from Italy. In Ger-

many, we add a control to the Mincer equation in LIS for individuals whose parents are both Italian, which gives an average adjustment for children of Italians in Germany. It is, unfortunately, not possible to identify those with only one Italian parent. In the USA, we can instead identify individuals whose mother is Italian. In comparison to our baseline estimates in Figure 3.2, we see virtually no change for 2G in France, a substantially increase for 2G in Germany and in the USA. Thus, evidence suggests our baseline estimates are a lower bound of the actual monetary gains.

SHIW. Figure 3.A.11 shows estimated returns to migration to using different waves of SHIW as comparison group, i.e. as proxy for the performance of Italian residents. If returns are driven by a peculiar economic situation in Italy in a specific year, our estimated pattern should only show up when using some specific SHIW waves. For all outcomes, especially monetary ones, estimated returns are similar when using different waves of SHIW.

2G born in host country. Figure 3.A.14 compares baseline estimated returns to estimates using only the sample of 2G migrants born in the country they currently live in. Indeed, one might worry that results are driven by 2G migrants that grew up in a different host country and only moved to their current residence place later on. Unfortunately, we cannot rule out the case of a 2G migrant born, for instance, in the USA, who moved away for some years after their birth and then moved back to the USA before 2015. Nonetheless, excluding those born in a different country than their current residence helps us bringing our sample closer to the ideal sample of only 2G migrants who actually grew up in their current host country. Results virtually do not change, with point estimates being almost the same in each country and always within the confidence intervals.

Including 1G. Another interesting aspect to consider is the definition of the most appropriate comparison group to evaluate the performance of 2G Italian migrants. So far we only consider the split in the Italian population created by their parents migration choice. However, the idea comparison group would also include Italians born between 1960 and 1980 (as our baseline) who decided themselves to migrate before 2015. Because our administrative dataset includes information on any Italian aborad in 2015, we can include 1G Italian migrants born between 1960 and 1980 registered to AIRE in 2015 to our “Italy” base group. Unfortunately, we do not have information on parents’ education for this group, and hence can only control for selection on observables via Italian region of origin fixed effects. Figure 3.A.15 displays the results of this exercise, and compares them to our baseline estimates without controlling for self-selection on unobservables. Again, results virtually do

Table 3.1. Utility maximization by time of migration.

		Child born	
	All families	aft migration	bf migration
Predicted income:			
First child	0.208***	-0.053	0.654***
	(0.057)	(0.075)	(0.102)
Parents	0.516***	0.872***	-0.008
	(0.095)	(0.127)	(0.143)
Obs.	56,331	41,895	14,436
Cases	6,259	4,655	1,604

*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows the effect of estimated yearly equivalised household income of migrants and their first born children on their probability of migrating on the chosen country. Sample includes families where parents (1G) took the migration decisions and first-born children (2G) are born abroad or migrated before age 18. First-born children must also be born between 1960 and 1980 living abroad in 2015. Estimation is carried on through an alternative-specific conditional logit model where the outcome is the utility of migrating into the different potential destinations. Case-specific variables include both parents' birth years, and migration age, first-born's birth year and gender, Italian area (north, centre, south and islands) of origin fixed effects. Alternative-specific variables include first born's and parents' predicted income in each host country. Parents' income is predicted at age 45 on the basis of 1995 LIS data for each host country. Column (1) considers all families, Column (2) only those that migrated before the birth of their first child, Column (3) only those that migrated after the birth of their first child. Monetary values are in USD in 2015 PPP. Yearly earnings are further expressed in 10,000 USD/year. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

not changem with the exception of some estimates for Argentina and Brazil. However, even these results point, if anything, towards an underestimation of intergenerational returns to migration.

3.4.2 Intertemporal Utility Maximization

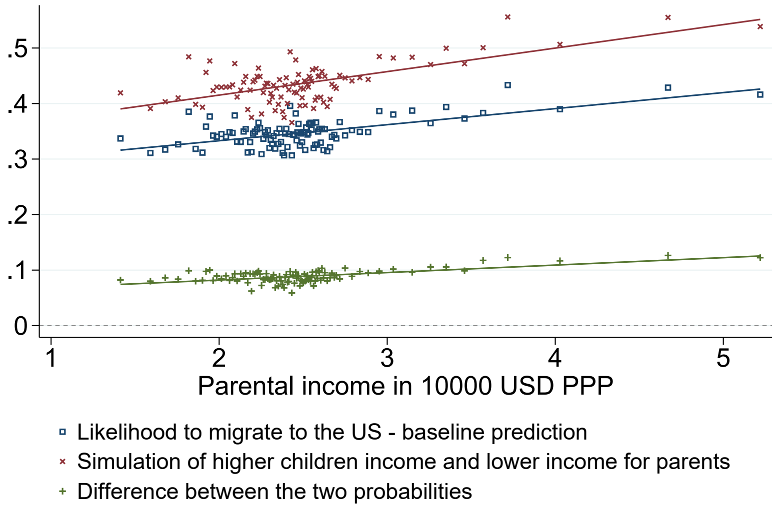
The last part of our analysis is to test for the relative importance of parents' and children opportunities in the parents' choice of migration country, in an intertemporal utility maximization framework. We focus on migrants' families only and consider, for each of them, an estimate of the parents' and their first born child's household income. Income is, in both cases, estimated via LIS.

Table 3.1 reports estimates for $\hat{\kappa}_1$ and $\hat{\kappa}_2$ from Equation eq:umax. First column considers the full sample of all families in AIRE. Second and third column distinguish between families that, respectively, had their first child after or before migration. This is an interesting heterogeneity dimension, as couples or single individuals that migrated without plans of having children in the forseeable future should not take their children' opportunities into account, or at least only limitedly so. We find quite clear-cut results: when pooling all families, both the parents and first borns' predicted household income are correlated with the probability of migrating into different countries. However, when looking at date of birth with respect to year of arrival, predicted child's income is correlated with the host country choice only if the child had already been born at time of migration (third column). Otherwise, parents' predicted income is the only measure that matters. Table 3.A.15 reports results using different measure of children' income, such us focusing on the second born child or a combination of all children' predicted income. Our main results are robust to the different specifications.

Even though sample size is limited, we try to explore additional dimensions of heterogeneity. Table 3.A.16 reports estimated coefficients when combining the distinction of birth of first child and parents' education level. Interestingly, results for families with children born after migration are driven by low educated parents, while higher educated parents seem to already put some wait on their children' income. As lower education level often implies lower income as well, it seems intuitive that poorer families might put a bigger weight on parents' income opportunities, which will also be fundamental to sustain the family financially in the long run. However, it is interesting to see that higher education families give importance to their future children income. Columns (4) and (5) focus instead on households in which the first child was born already before migration. Here, we see that for lower education (likely poorer) families both parents' and children' income are important factors in the choice of host country. Similarly as before, this is not surprising, if parents need also to ensure a minimum level of living standards. On the other hand, we see in Column (5) that more educated (likely richer) parents seem to be willing to even forgo some of their income in lieu of additional income for their children.

To better understand the magnitude of these results, we move forward and simulate different scenarios that might change the incentives of migrating towards specific countries. We simulate a simple college expansion policy in the USA, that pays college for 2G migrants via a lump-sum tax of 20% of their parents' household income. To keep things as simple as possible, we assume all families migrating to the USA will now loose 20% of household income of the first generation, while the second generation will gain an additional 20%. Such additional gain are set in line with the literature on effects of college education on earnings, which suggests that a college degree increases one's earnings indeed by 20%. Results of the simulation are shown in Figure 3.4. Here, the blue line depicts the baseline prediction of the likelihood of 1G to migrate to the USA depending on their predicted income there. The red line depicts the simulated change in the estimated probability after the college expansion, and the green line the difference between the two probabilities. Such policy increases the likelihood that anyone decides to migrate to the USA by

Figure 3.4. Simulation of a college expansion.



Notes: Figure 3.4 displays the effect of a simple college expansion policy in the USA, that transfers 20% of household income from 1G migrants to their children. Blue line indicates the baseline prediction of the probability of migrating to the USA by parental household income. Red line indicates the predicted probability after the college expansion takes place. Green line indicates the difference between the two. Monetary values are expressed in 2015 PPP 10,000 USD. *Source*: own elaborations of AIRE and SHIW data.

about 10 percentage points. However, the increase is higher for parents with higher income potential in the USA, moving from about 9 percentage points for lower income 1Gs to about 11 percentage points for higher income 1Gs.

3.5 Conclusions

In this paper, we estimate intergenerational returns to migration in terms of education, employment and predicted income. We look at the performance of descendants of Italian migrants worldwide in 2015 by exploiting a unique administrative dataset on Italians abroad. Our novel setting allows to compare migrants' descendants to their peers in Italy, and to test for whether their parents where maximizing intertemporal utility when taking the migration choice.

We find that Italians' descendants around the world do not necessarily study longer, but are more often employed and likely to perceive higher income and hourly earnings than their peers in Italy. Our unique dataset allows us to control for parental self-selection thanks to key information on parents' education level and Italian region of origin - which is a key proxy for their cultural background. We also provide evidence that self-selection on unobservables in parental generation does not play a major role in our setting, as it only affects their children' performance via human capital transmission. Thus, our controls for selection on observables already take into account most of the selection in the first generation.

We explore important heterogeneity dimensions, such as gender and parents' SES. We find both female and male 2G migrants outperform their peers in Italy. However, monetary returns are higher for male 2G migrants, who are also more likely to be employed than female 2Gs. Interestingly, however, female 2G migrants seem to participate more often to the labour market than their counterparts in Italy. Furthermore, returns are especially positive for 2G migrants from lower SES.

Our estimated returns extrapolate the host country effect on 2G migrants performance. Therefore, education returns are likely influenced by the structure of the school systems, employment status by different labour markets' characteristics, and household income by welfare state and taxation system. Additionally, gender differences are likely influenced by culture and gender gaps in each host country. Future research should focus on exploring which dimensions are most relevant, and the role played migrants' integration in the different destination countries.

Finally, we provide empirical evidence that 1G migrants do consider whether their chosen destination country provides better opportunities, not only for themselves, but for their children as well. Specifically, families migrating after the birth of their first child put a higher weight on their children' income when choosing the destination country. Furthermore, our results suggest that higher SES parents are willing to forgo some income in lieu of additional monetary gains for their children.

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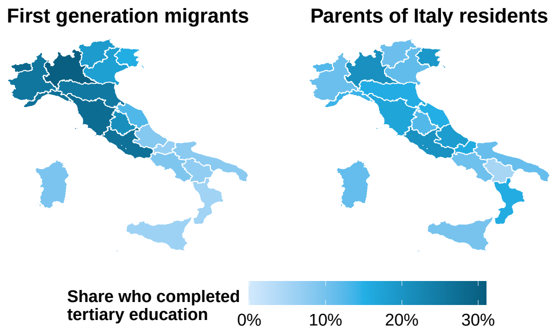
Appendices to Chapter 3

Appendix 3.A Additional Figures and Tables

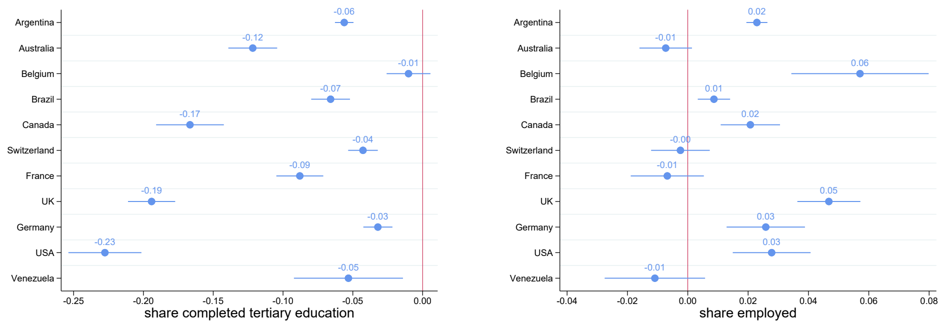
Figure 3.A.1. Self-selection of 1G migrants.



Notes: Figure 3.A.1 displays share of individuals with tertiary education degree by Italian region of origin. Left panel considers only individuals in AIRE born in Italy and living abroad in 2015. Right panel considers parents of Italian residents in 2015 from SHIW. *Source: own elaborations of AIRE and SHIW data.*

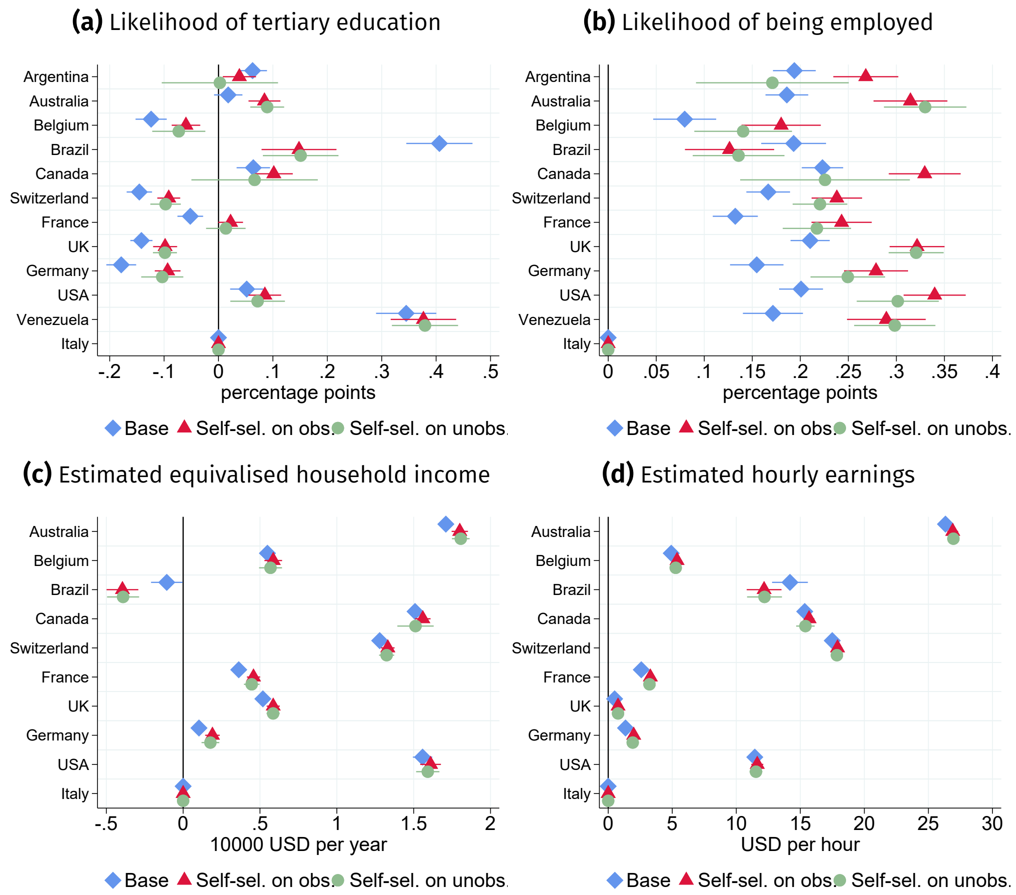
Figure 3.A.2. Selection in the second generation.

(a) Likelihood of tertiary education **(b)** Likelihood of being employed



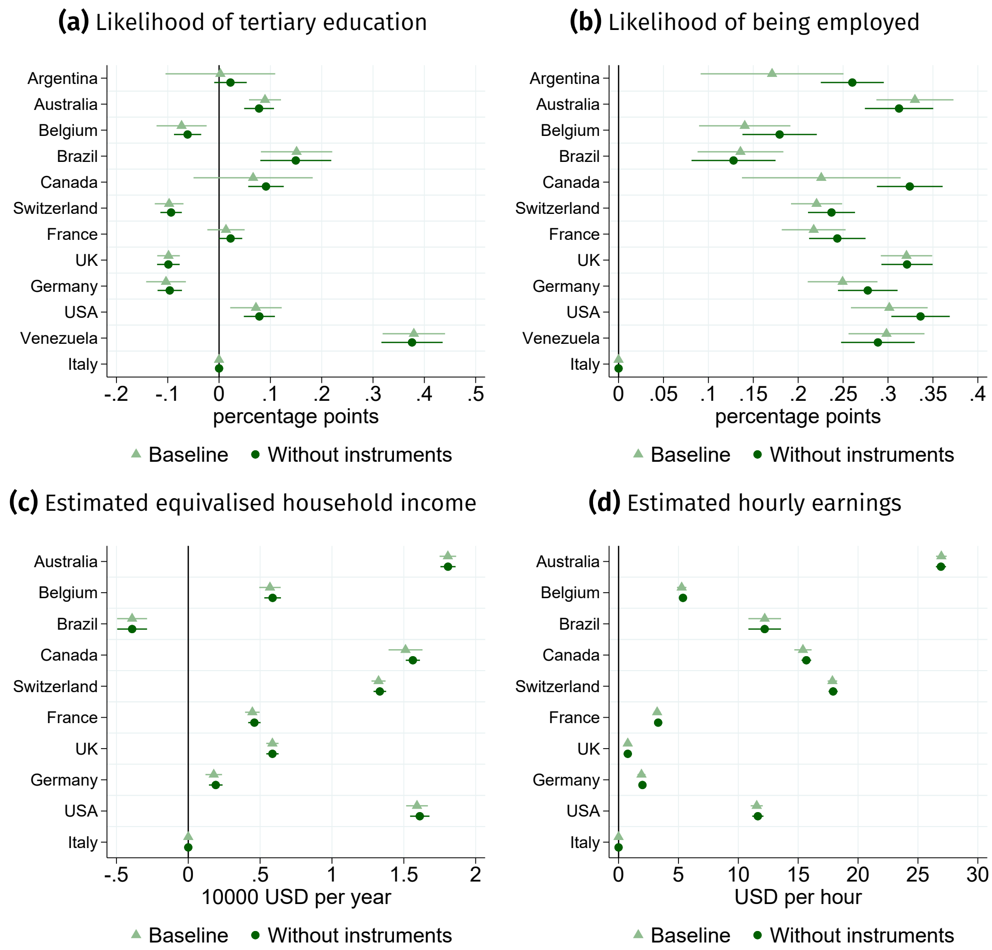
Notes: Figure 3.A.2 displays estimated OLS regression coefficient of the probability of being included in the linked sample on migrants' descendants outcomes. Panel (a) considers as outcome likelihood of completing tertiary education, and Panel (b) considers the likelihood of being employed. Sample includes individuals born abroad between 1960 and 1980 and living abroad in 2015. Linked individuals are those can be linked to their parents. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.3. Accounting for self-selection.



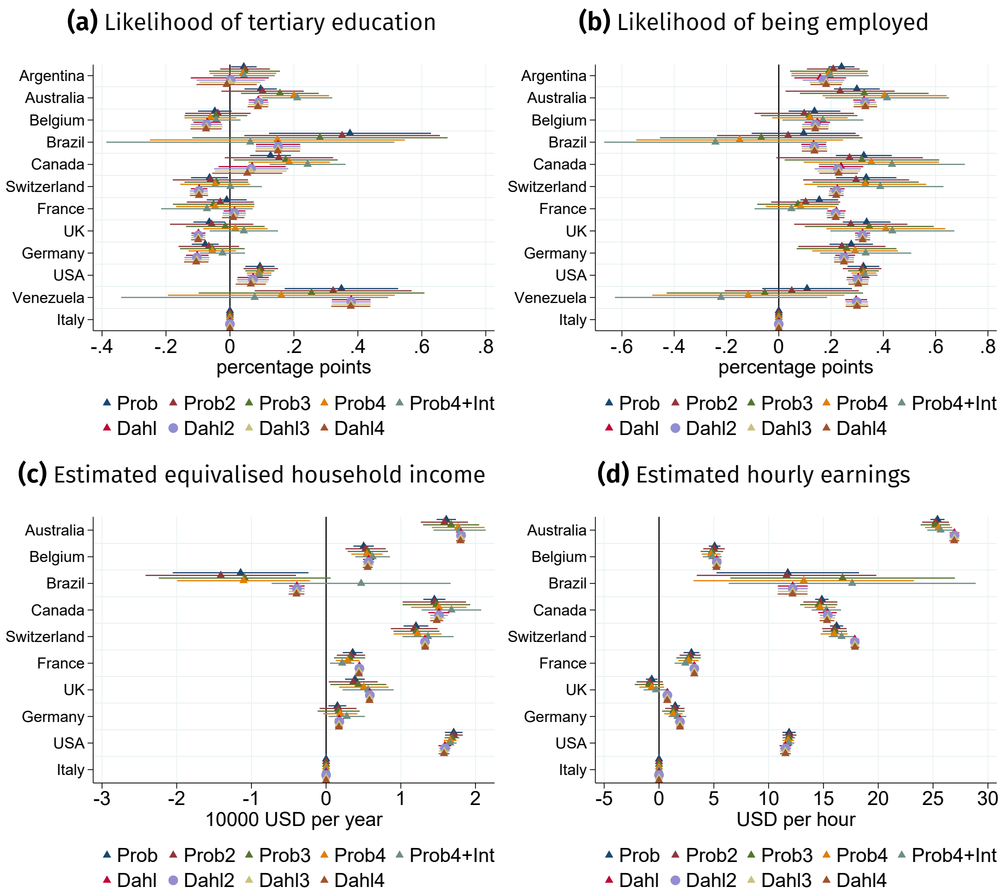
Notes: Figure 3.A.3 displays estimated returns to migration by country of destination. Reported coefficients are those of country fixed-effects. Panel (a) considers as outcome the likelihood of the second generation completing tertiary education. Panel (b) considers as outcome the likelihood of the second generation being unemployed. Panel (c) considers as outcome the estimated equivalised household income. Panel (d) considers as outcome the estimated hourly earnings. Blue diamonds estimates control for age and gender of the second generation. Red triangles estimates additionally include controls for first generation's Italian region of origin and education category (self-selection on observables). Green circles estimates additionally include controls for first generation's probability of migrating in the chosen destination country (self-selection on unobservables). Monetary values are expressed in 2015 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.4. Accounting for self-selection with and without excluded variables.



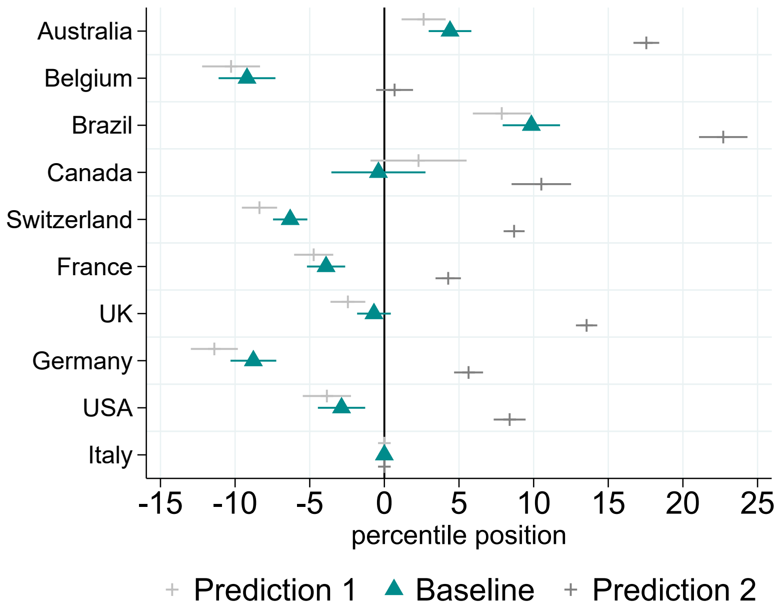
Notes: Figure 3.A.4 displays estimated returns to migration by country of destination. Reported coefficients are those of country fixed-effects. Light green triangles account for self-selection on unobservables including excluded variables in the first stage, while dark green circles do not include any excluded variable in the first stage. Monetary values are expressed in 2015 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.5. Self-selection bias correction term.



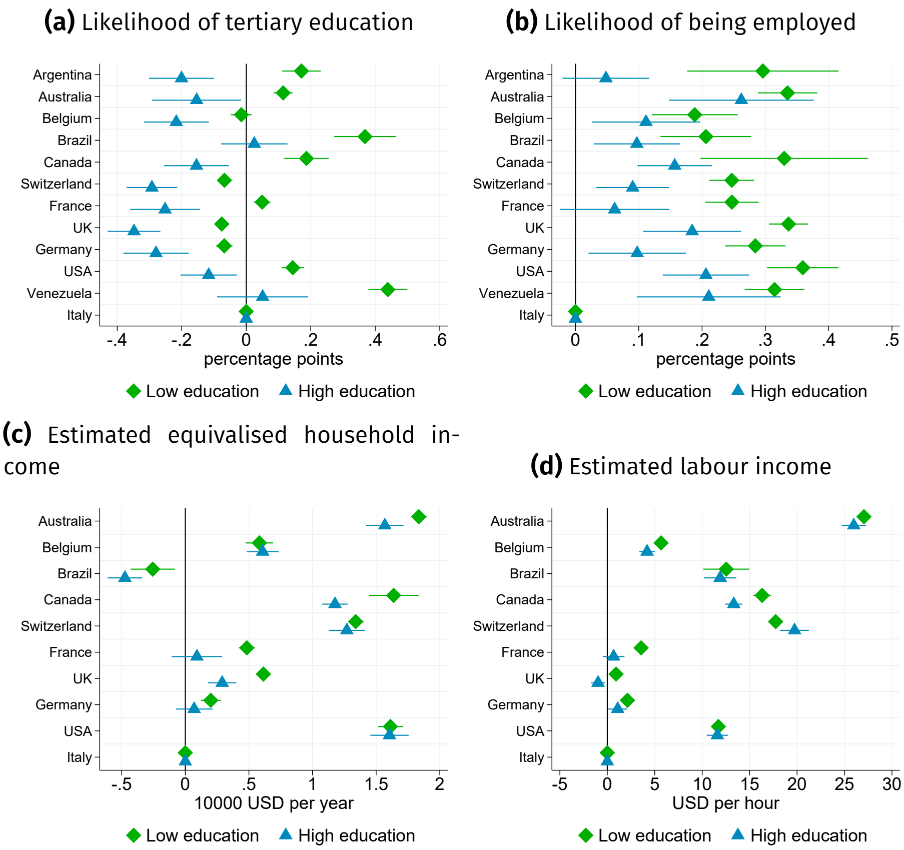
Notes: Figure 3.A.5 displays estimated returns to migration by country of destination with different self-selection bias correction terms. Reported coefficients are those of country fixed-effects. Panel (a) considers as outcome the likelihood of the second generation completing tertiary education. Panel (b) considers the estimated equivalised household income. All regressions include 2G migrants' gender, age and age squared control, parents' education and Italian region fixed effects and a self-selection bias correction term. Parents' education is defined as highest level of education reached by one of the parents. Bias correction terms are constructed from estimated probabilities of migrating in each potential destination country from the first stage. "Prob" include the linear sum of the probabilities of migrating in each potential destination. "Prob2", "Prob3" and "Prob4" add progressively, the square, the cubes, and the fourth power of each estimated probability. "Prob4+int" adds further the linear interaction of each probability. "Dahl" specifications consider a bias-correction term á la Dahl. "Dahl" includes the estimated probabilities of migrating in the chosen country. "Dahl2","Dahl3", and "Dahl4" add, progressively, their square, cube and fourth power. Monetary values are expressed in 2015 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.6. Returns to migration: position in the income distribution.



Notes: Figure 3.A.6 displays returns to migration in terms of estimated percentile in the host country's income distribution of equivalised household income. Reported coefficients are those of country fixed-effects. When estimating income, baseline estimates consider the full host country LIS population. Prediction 1 considers only natives and Prediction 2 only migrants in LIS. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.7. Heterogeneity by parents' education level.



(a) Likelihood of tertiary education

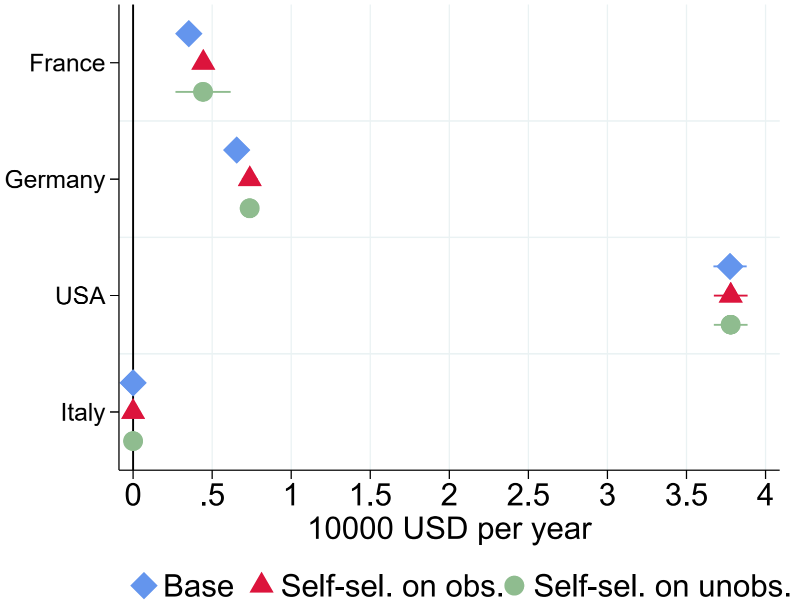
(b) Likelihood of being employed

(c) Estimated equivalised household income

(d) Estimated labour income

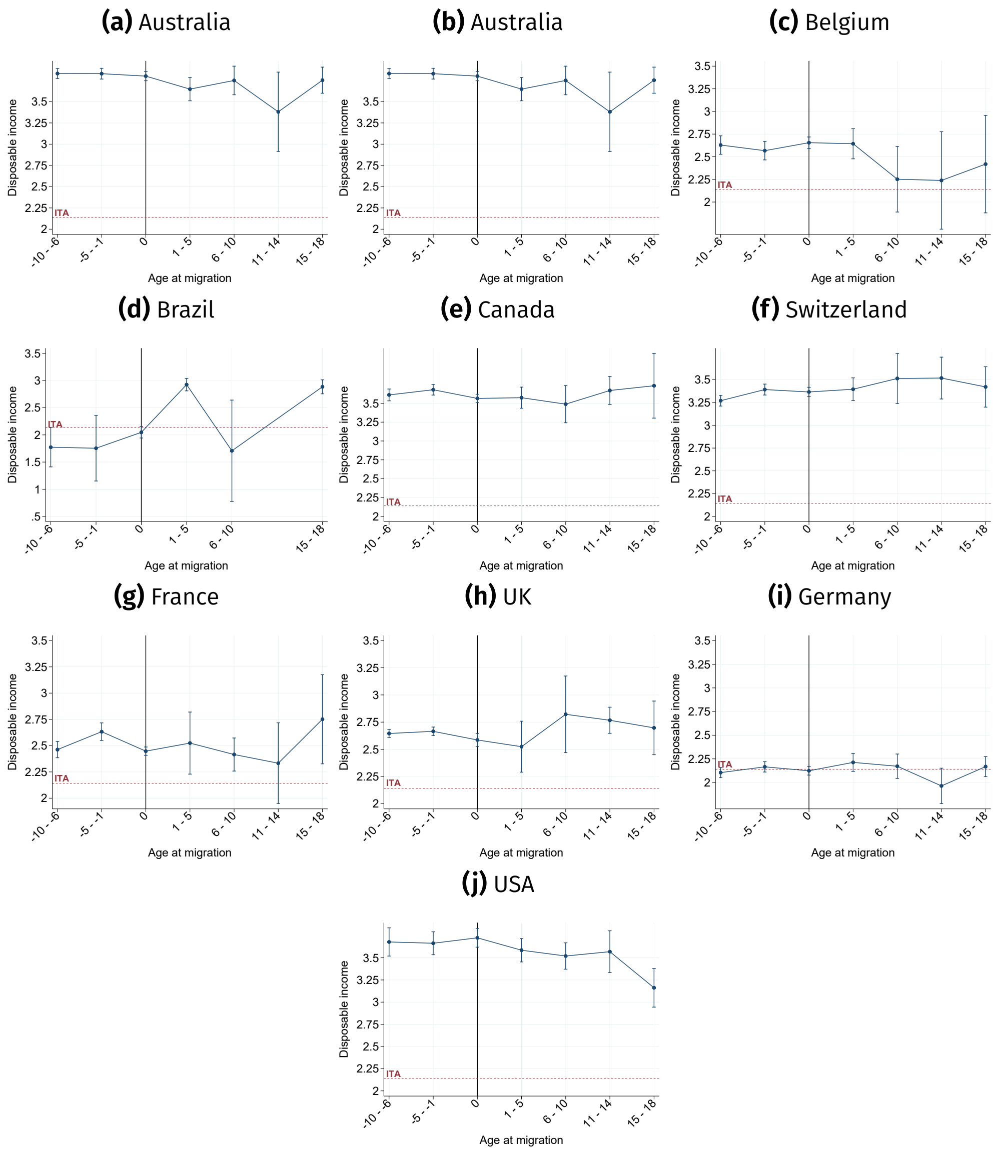
Notes: Figure 3.A.7 displays estimated returns to migration by country of destination distinguishing by parents' level of education. Panel (a) considers as outcome the likelihood of completing tertiary education. Panel (b) considers likelihood of being employed. Panelc (c) and (d) consider, respectively, estimated yearly equivalised household income and estimated hourly earnings. Reported coefficients are those of country fixed-effects. High education implies parents completed more than compulsory education (ISCED≥3). Low education implies parents completed at most compulsory education (ISCED<3). Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). Monetary values are expressed in 2015 PPP 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.8. Robustness checks: income estimation via 2G Italians in LIS.



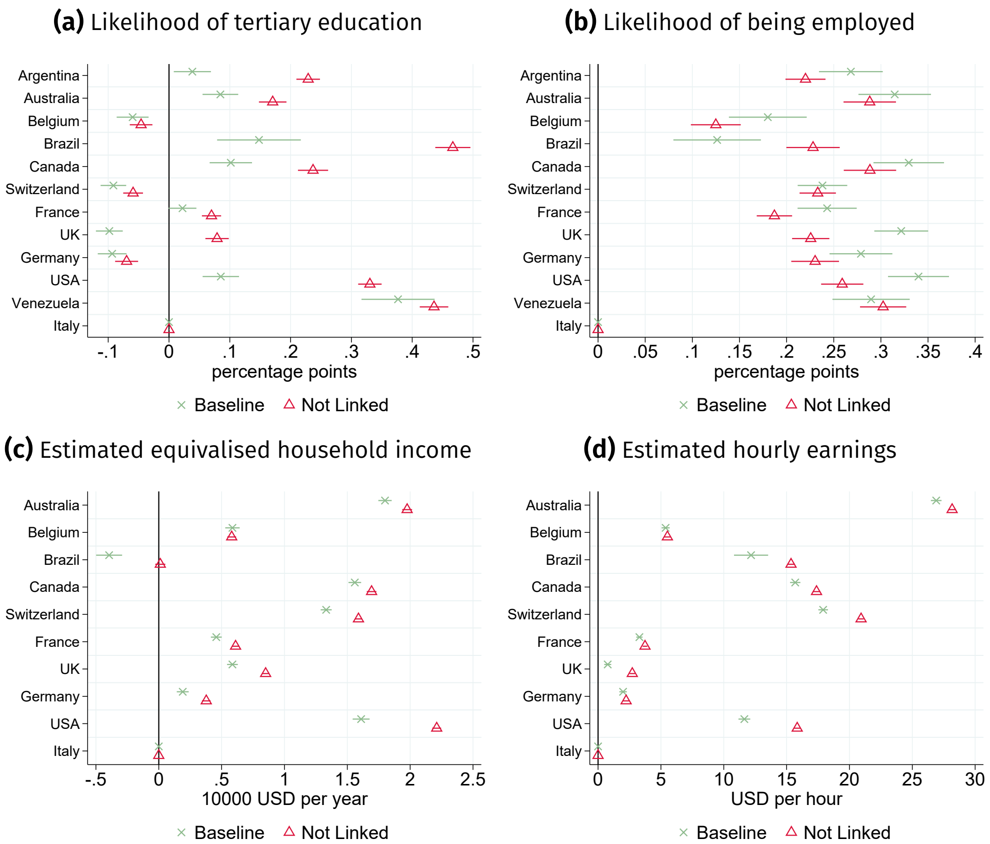
Notes: Figure 3.A.8 displays estimated returns to migration by country of destination in terms of estimated yearly equivalised household income. Reported coefficients are those of country fixed-effects. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). For France, income is estimated on the basis of individuals whose parents have a migration background. For Germany, income estimation includes a control for individuals whose parents are both Italian. For USA, income is estimated on the basis of individuals with Italian mother. Monetary values are expressed in 2015 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.9. Age effect by host country.



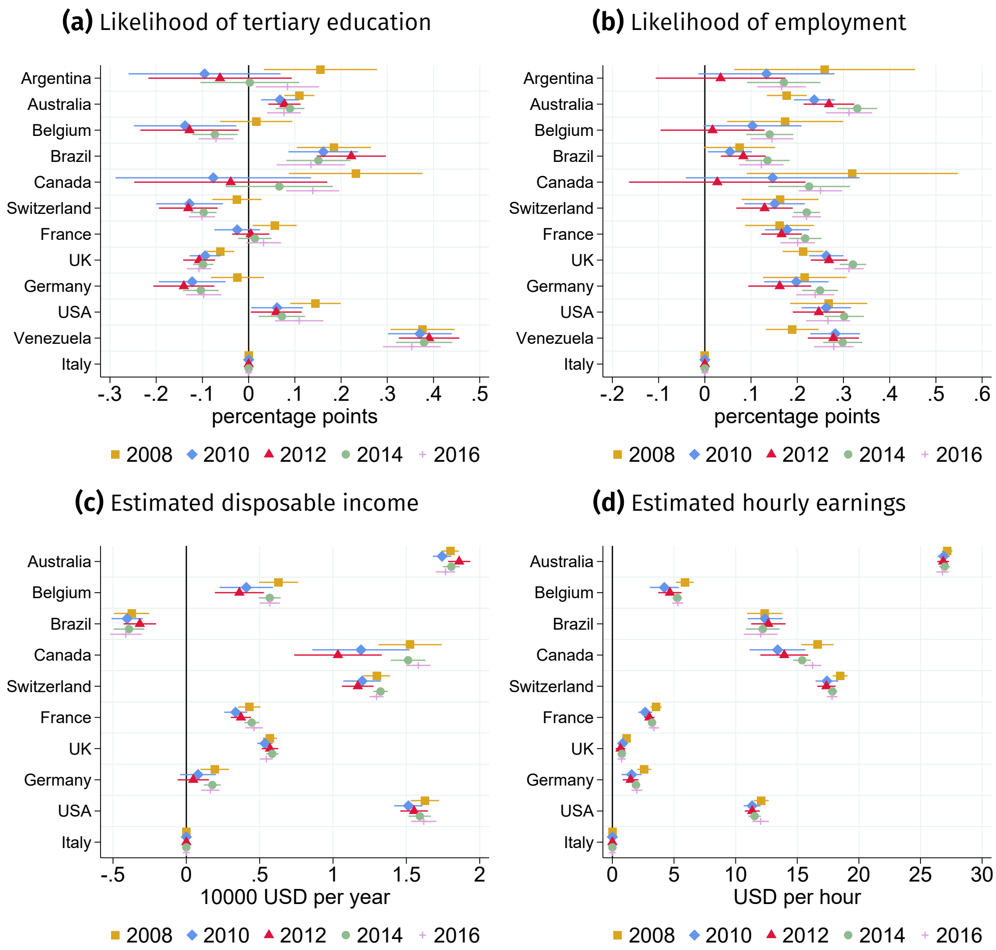
Notes: Figure 3.A.9 displays estimated returns to migration by country of destination in terms of estimated yearly equivalised household income by 2G's age of migration. Reported coefficients are those of country fixed-effects. Controls include age and gender of the second generation, first generation's Italian region of origin and education category (self-selection on observables) and first generation's probability of migrating in the chosen destination country (self-selection on unobservables). Monetary values are expressed in 2015 10,000 USD per year. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.10. Robustness checks: selection in 2G sample.



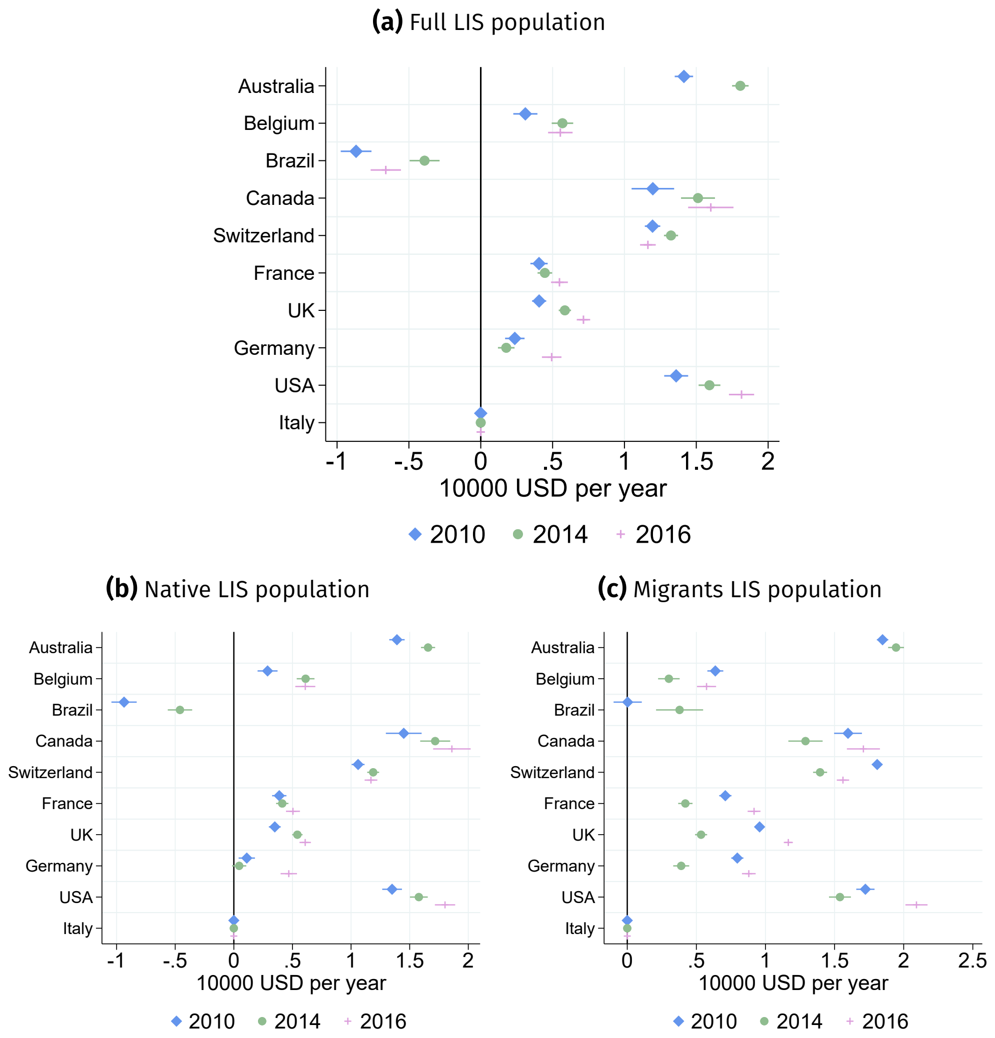
Notes: Figure 3.A.10 displays estimated returns to migration by country of destination in the baseline ("L") and the not linked ("NL") samples. The not linked sample includes all individuals born abroad and residing abroad registered to AIRE in 2015. These are at least second generation migrants for whom we do not have parents information. Panel (a) considers as outcome the likelihood of completing tertiary education. Panel (b) considers likelihood of being employed. Panel (c) and (d) consider, respectively, estimated yearly equivalised household income and estimated hourly earnings. Reported coefficients are those of country fixed-effects. Controls include age and gender of the second generation and first generation's Italian region of origin. Monetary values are expressed in 2015 PPP 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.11. Returns to migration with different SHIW years.



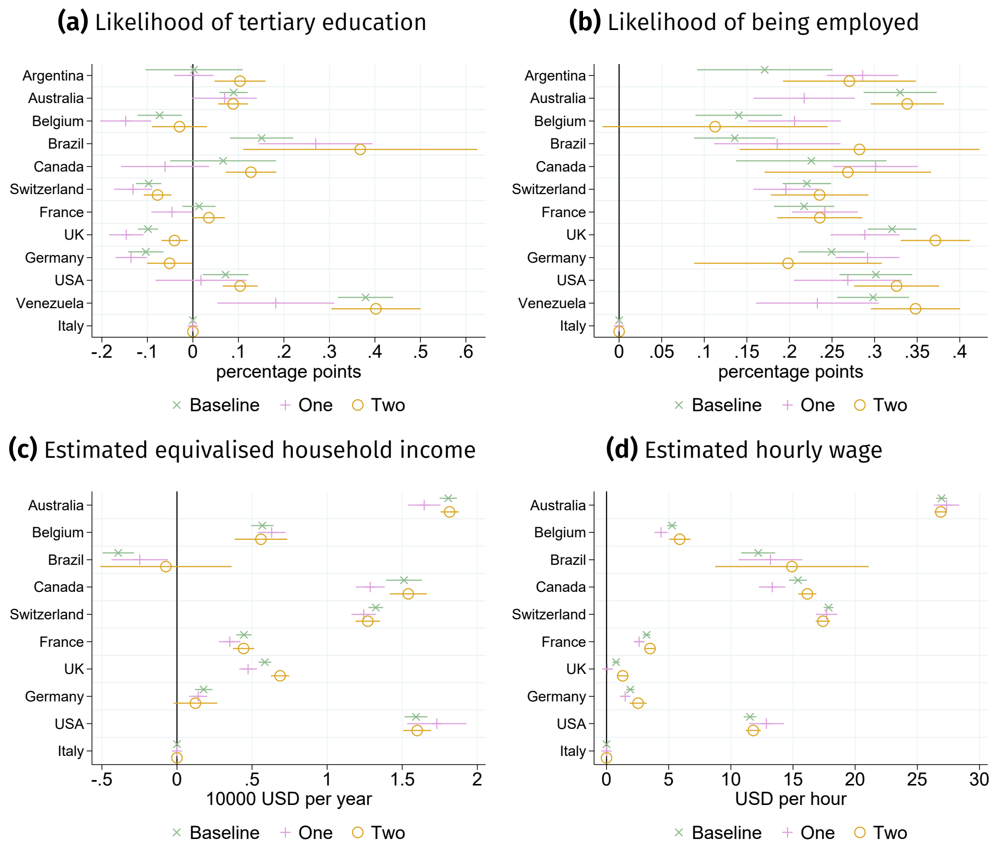
Notes: Figure 3.A.11 displays estimated returns to migration by country of destination. Reported coefficients are those of country fixed-effects. outcome is equivalised household disposable income in 2015 PPP 10,000 USD. Different estimates compare 2G migrants to residents of Italy in different years: yellow squares in 2008, blue diamonds in 2010, red triangles in 2012, green circles in 2014, and pink plus symbols in 2016. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.12. Returns to migration with different LIS years.



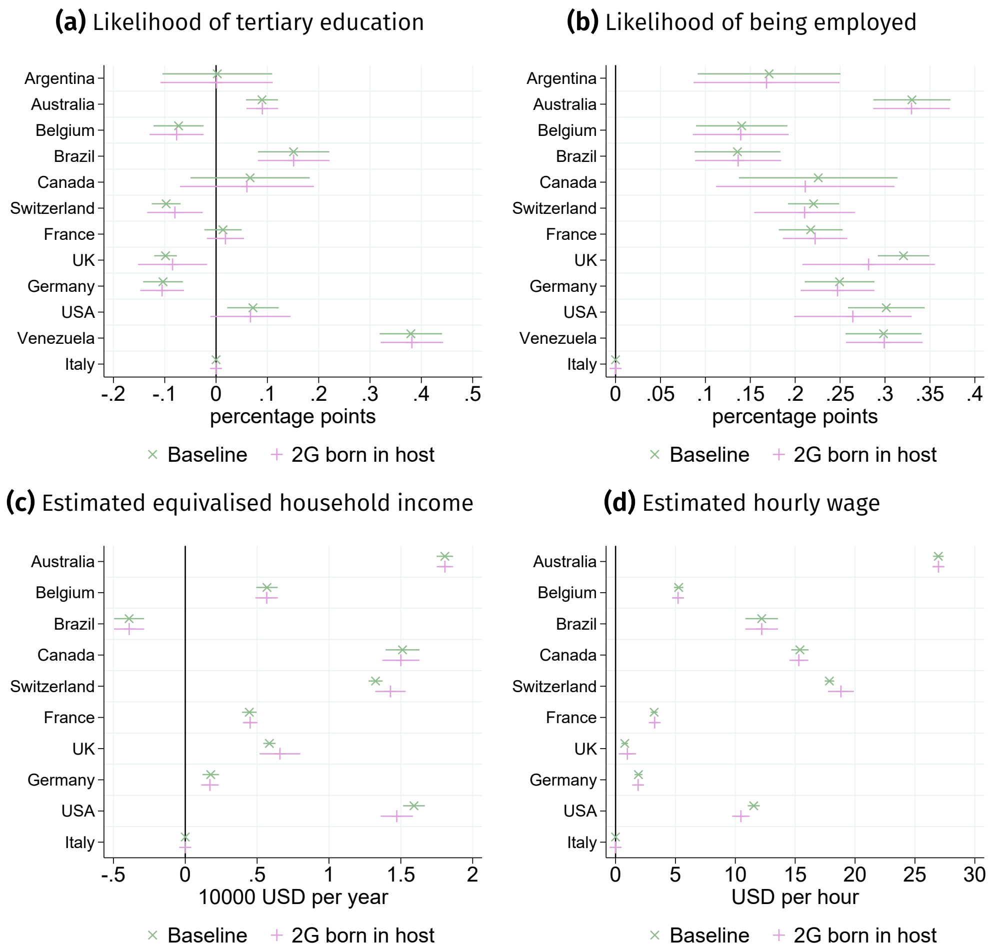
Notes: Figure 3.A.12 displays estimated returns to migration by country of destination. Reported coefficients are those of country fixed-effects. outcome is equivalised household disposable income in 2015 PPP 10,000 USD. Different estimates use different years of LIS to predict income (outcome variable): blue diamonds 2010, green circles in 2014, pink pluses in 2016. Panel (a) uses the full population per country in LIS. Panel (b) and (c) use, respectively, only the natives and migrants population in LIS. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.13. Returns to migration by number of Italian parents.



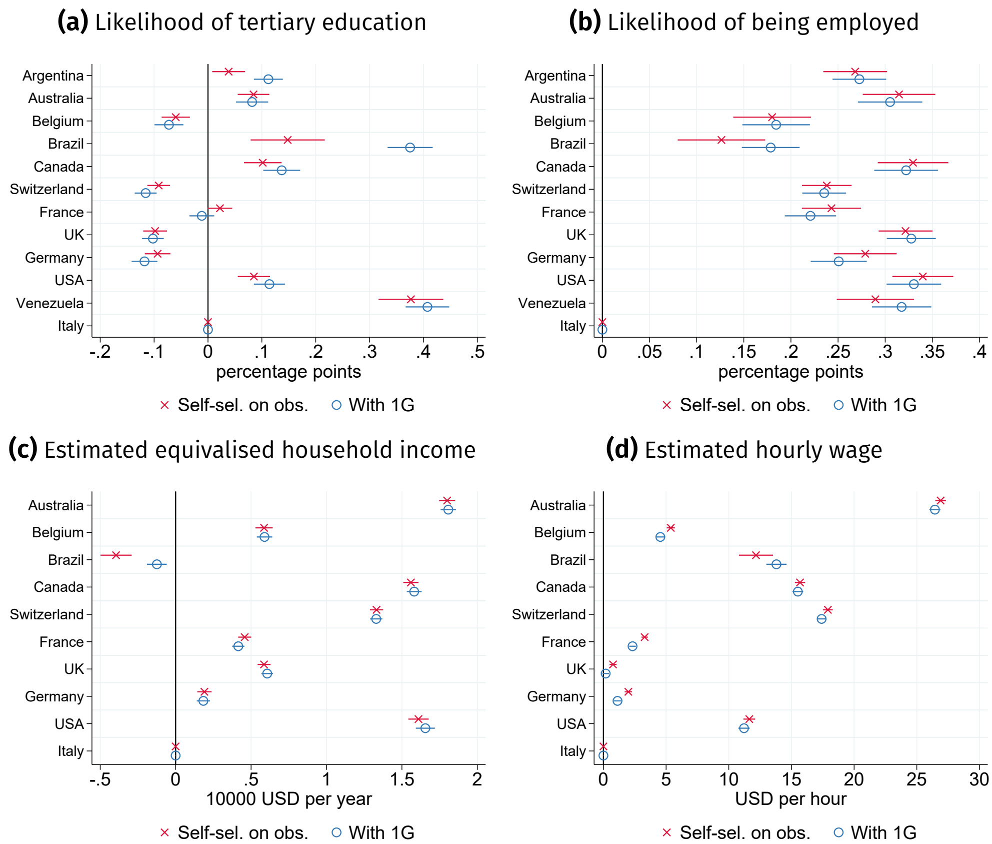
Notes: Figure 3.A.13 displays estimated returns to migration by country of destination and number of Italian parents. "One" sample considers 2G migrants with only one parent with Italian nationality. "Two" sample considers 2G migrants with two parents with Italian nationality. Baseline sample includes both these sample, and 2G migrants for which only one parent is known. Reported coefficients are those of country fixed-effects. outcomes are likelihood of teritary education (Panel (a)), likelihood of employment (Panel (b)), estimated equivalised household disposable income in 10,000 USD per year (Panel (c)), and estimated hourly earnings in USD per hour (Panel (d)). Monetary values are in 2015 PPP USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.14. Returns to migration for 2G migrants born in host country.



Notes: Figure 3.A.14 displays estimated returns to migration by country of destination for 2G migrants born or migrated before age 18 into their current host country. Reported coefficients are those of country fixed-effects. outcomes are likelihood of teritary education (Panel (a)), likelihood of employment (Panel (b)), estimated equivalised household disposable income in 10,000 USD per year (Panel (c)), and estimated hourly earnings in USD per hour (Panel (d)). Monetary values are in 2015 PPP USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.15. Robustness checks: first generation migrants.



(a) Likelihood of tertiary education

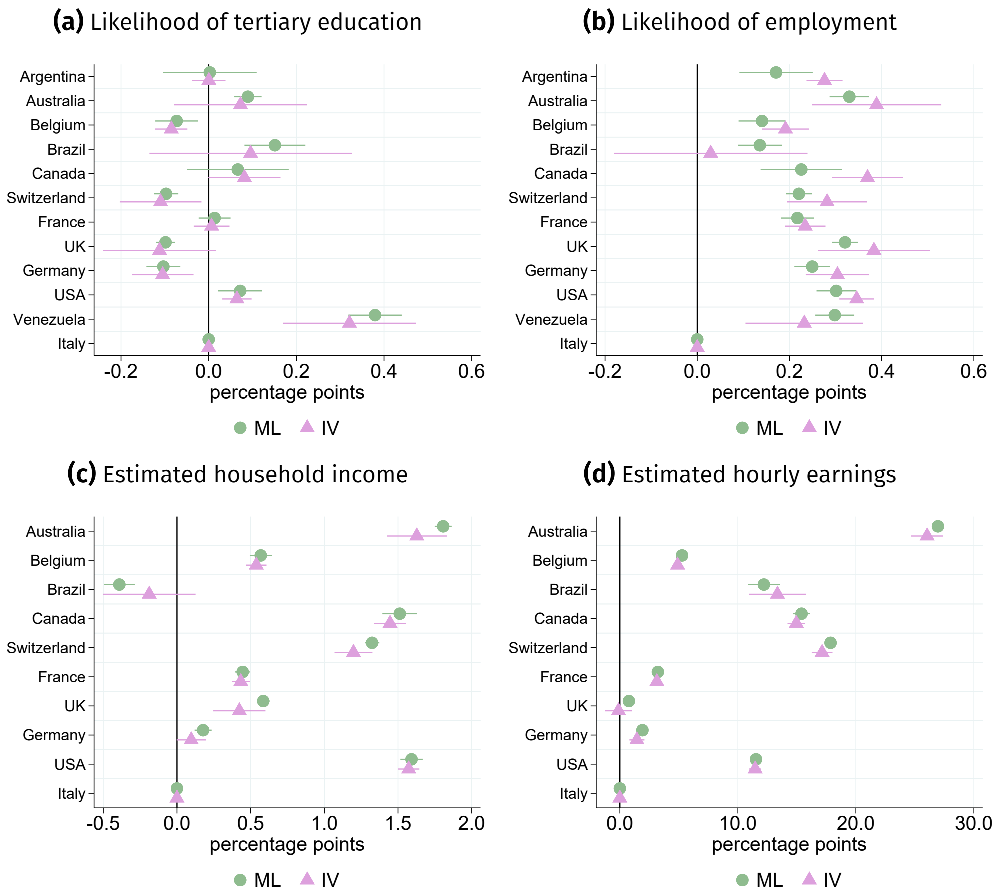
(b) Likelihood of being employed

(c) Estimated equivalised household income

(d) Estimated hourly wage

Notes: Figure 3.A.15 displays estimated returns to migration by country of destination in the baseline, and the "with 1G" sample. The "with 1G" sample includes 1G migrants born between 1960 and 1980 as part of the resident population in Italy. Panel (a) considers as outcome the likelihood of completing tertiary education, panel (b) considers likelihood of being employed, panel (c) considers estimated yearly equivalised household income, and (d) considers estimated hourly wage. Income for the 1G migrants is estimated as if they were living in Italy. Reported coefficients are those of country fixed-effects. Controls include age, gender, and Italian region of origin. Monetary values are expressed in 2015 PPP 10,000 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Figure 3.A.16. Compare methods: multinomial logit and IV.



Notes: Figure 3.A.16 displays estimated returns to migration by country of destination. Reported coefficients are those of country fixed-effects. Green circles indicate multinomial logit estimation while pink triangles indicate IV estimation. outcomes are likelihood of tertiary education (Panel (a)), likelihood of employment (Panel (b)), estimated household income (Panel (c)), and estimated hourly earnings (Panel (d)). Monetary values are expressed in 2015 USD. We report normal 95% confidence interval for each estimate. *Source*: own elaborations of AIRE and SHIW data.

Table 3.A.1. Descriptive statistics, baseline sample.

	2G Migrants			Italy residents		
	Mean	Std.Dev.	Obs.	Mean	Std.Dev.	Obs.
Age	42.24	5.39	18,768	44.70	5.80	4,195
% males	0.56	0.50	18,768	0.48	0.50	4,195
% north Italy	0.37	0.48	18,768	0.42	0.49	4,195
% centre Italy	0.28	0.45	18,768	0.11	0.31	4,195
% south Italy	0.32	0.46	18,768	0.47	0.50	4,195
Education						
% no degree	0.02	0.12	18,768	0.00	0.05	4,195
% less than compulsory	0.04	0.20	18,768	0.04	0.18	4,195
% compulsory	0.35	0.48	18,768	0.43	0.50	4,195
% more than compulsory	0.42	0.49	18,768	0.36	0.48	4,195
% tertiary	0.17	0.38	18,768	0.18	0.38	4,195
Parents' education						
% no degree	0.06	0.24	18,768	0.06	0.25	4,195
% less than compulsory	0.48	0.50	18,768	0.48	0.50	4,195
% compulsory	0.28	0.45	18,768	0.27	0.44	4,195
% more than compulsory	0.14	0.35	18,768	0.14	0.35	4,195
% tertiary	0.04	0.20	18,768	0.05	0.22	4,195
Employment						
% employed	0.93	0.26	17,514	0.73	0.44	4,195
% unemployed	0.04	0.20	17,514	0.11	0.31	4,195
% inactive	0.03	0.17	17,514	0.16	0.36	4,195
Predicted income						
Equiv. HH disposable income	30,548.89	9,148.67	13,644	21,405.25	5,847.16	4,195
Earnings per hour	25.99	11.37	12,689	14.37	3.41	2,995
Natives-based predicted income						
Equiv. HH disposable income	30,446.73	9,357.70	13,644	21,851.13	5,982.73	4,195
Earnings per hour	26.72	11.61	12,689	14.75	3.57	2,995
Migrants-based predicted income						
Equiv. HH disposable income	31,346.17	9,174.23	13,644	21,851.13	5,982.73	4,195
Earnings per hour	24.96	11.72	12,689	14.75	3.57	2,995

Notes: This table shows descriptive statistics for the baseline estimation sample. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015, migrated before age 18 and with at least one known Italian parent (2G Migrants) and Italian residents in 2015 and born between 1960 and 1980 (Italy residents). *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.2. Distribution of migrants across destination countries.

		Migrants' descendants			
	1G	**2G (not linked)**	**2G (linked)**	**≥3G (linked)**	**Baseline**
Argentina	131,449	737,201	19,303	300,712	3,640
Australia	94,396	67,413	33,089	25,868	2,902
Belgium	94,627	139,912	34,971	62,719	990
Brazil	42,062	421,095	8,964	123,828	344
Canada	90,639	49,229	26,988	21,039	1,589
Switzerland	228,550	256,751	82,868	125,064	2,318
France	191,783	155,862	74,189	69,262	1,880
United Kingdom	136,052	84,826	49,136	37,168	1,899
Germany	301,928	252,442	143,708	112,910	1,628
Netherlands	20,624	13,332	9,148	5,633	23
New Zealand	1,770	1,805	749	711	17
USA	152,159	97,008	49,908	33,856	1,161
Venezuela	60,696	89,253	15,406	52,199	417
Total	1,546,735	2,366,129	548,427	970,969	17,116

Notes: This table shows the distribution of migrants across country by generation and intergenerational linkages. Baseline sample includes only 2G migrants for whom parents are known, born between 1960 and 1980. Furthermore, they must have non-missing education and parents' education information. In the estimation, migrants in the Netherlands and New Zealand are excluded because of the limited sample size. *Source*: AIRE data.

Table 3.A.3. Descriptive statistics, baseline sample and 2G migrants born abroad.

	2G Migrants			2G Migrants aged 1-18			Italy residents		
	Mean	Std.Dev.	Obs.	Mean	Std.Dev.	Obs.	Mean	Std.Dev.	Obs.
Age	42.24	5.39	18,768	44.63	5.69	2,069	44.70	5.80	4,195
% males	0.56	0.50	18,768	0.63	0.48	2,069	0.48	0.50	4,195
% north Italy	0.37	0.48	18,768	0.51	0.50	2,069	0.42	0.49	4,195
% centre Italy	0.28	0.45	18,768	0.19	0.40	2,069	0.11	0.31	4,195
% south Italy	0.32	0.46	18,768	0.28	0.45	2,069	0.47	0.50	4,195
Education									
% no degree	0.02	0.12	18,768	0.03	0.16	2,069	0.00	0.05	4,195
% less than compulsory	0.04	0.20	18,768	0.06	0.23	2,069	0.04	0.18	4,195
% compulsory	0.35	0.48	18,768	0.40	0.49	2,069	0.43	0.50	4,195
% more than compulsory	0.42	0.49	18,768	0.40	0.49	2,069	0.36	0.48	4,195
% tertiary	0.17	0.38	18,768	0.12	0.32	2,069	0.18	0.38	4,195
Parents' education									
% no degree	0.06	0.24	18,768	0.12	0.32	2,069	0.06	0.25	4,195
% less than compulsory	0.48	0.50	18,768	0.55	0.50	2,069	0.48	0.50	4,195
% compulsory	0.28	0.45	18,768	0.24	0.43	2,069	0.27	0.44	4,195
% more than compulsory	0.14	0.35	18,768	0.07	0.25	2,069	0.14	0.35	4,195
% tertiary	0.04	0.20	18,768	0.02	0.13	2,069	0.05	0.22	4,195
Employment									
% employed	0.93	0.26	17,514	0.90	0.30	1,937	0.73	0.44	4,195
% unemployed	0.04	0.20	17,514	0.07	0.25	1,937	0.11	0.31	4,195
% inactive	0.03	0.17	17,514	0.03	0.18	1,937	0.16	0.36	4,195
Predicted income									
Equiv. HH disposable income	30,548.89	9,148.67	13,644	31,899.77	10,339.80	1,906	21,405.25	5,847.16	4,195
Earnings per hour	25.99	11.37	12,689	25.69	9.87	1,710	14.37	3.41	2,995
Natives-based predicted income									
Equiv. HH disposable income	30,446.73	9,357.70	13,644	32,239.90	10,481.23	1,906	21,851.13	5,982.73	4,195
Earnings per hour	26.72	11.61	12,689	26.70	9.79	1,710	14.75	3.57	2,995
Migrants-based predicted income									
Equiv. HH disposable income	31,346.17	9,174.23	13,644	32,011.87	9,890.85	1,906	21,851.13	5,982.73	4,195
Earnings per hour	24.96	11.72	12,689	24.37	10.31	1,710	14.75	3.57	2,995

Notes: This table shows descriptive statistics for the baseline estimation sample. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 (2G Migrants) and Italian residents in 2015 and born between 1960 and 1980 (Italy residents). *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.4. Descriptive statistics of migrants' descendants linked and not linked samples.

	Linked	**Not linked**	**Diff.**
Age	41.00	44.17	-3.17***
			(0.025)
% males	0.56	0.52	0.03***
			(0.002)
% north Italy	0.39	0.42	-0.03***
			(0.002)
% centre Italy	0.21	0.17	0.04***
			(0.002)
% south Italy	0.38	0.41	-0.03***
			(0.002)
% university degree	0.31	0.38	-0.08***
			(0.002)
% employed	0.95	0.92	0.03***
			(0.001)
% unemployed	0.03	0.01	0.01***
			(0.001)
% inactive	0.02	0.07	-0.05***
			(0.001)
Observations	53,476	369,013	

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows mean values of observable characteristics of the migrants' descendants linked and not linked samples. Both samples include individuals born abroad between 1960 and 1980 and living abroad in 2015. Linked sample only includes individuals that can be linked to their parents (first column). Not linked sample includes only those that cannot be linked to their parents (second column). Third column indicates the difference in meanse between the link and not linked sample. *Source*: Authors' calculations from the AIRE data.

Table 3.A.5. Selection in migrants' descendants sample.

	Tertiary education		Employment	
	(1)	(2)	(3)	(4)
$\mathbb{I}\{linked\}$	-0.078***	-0.081***	0.032***	0.020***
	(0.002)	(0.002)	(0.001)	(0.001)
Age	No	Yes	No	Yes
Male	No	Yes	No	Yes
Ita region FE	No	Yes	No	Yes
Host country FE	No	Yes	No	Yes
Observations	422,489	422,489	422,489	422,489
Adj. R-squared	0.003	0.183	0.002	0.077

*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows estimated OLS regression coefficient of the probability of being included in the linked sample on migrants' descendants outcomes. Columns (1)-(2) consider likelihood of completing tertiary education, Columns (3)-(4) consider likelihood of being employed. Sample includes individuals born abroad between 1960 and 1980 and living abroad in 2015. Linked individuals are those can be linked to their parents. *Source*: Authors' calculations from the AIRE data.

Table 3.A.6. Multinomial logit, first stage.

	ARG	AUS	BEL	BRA	CAN	CH	FRA	GBR	GER	USA	VEN
Migrants' in origin area	-0.037***	-0.027***	-0.034***	-0.060***	-0.025***	-0.024***	-0.044***	-0.035***	-0.028***	-0.035***	-0.039***
	(0.004)	(0.004)	(0.004)	(0.009)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Gini at birth	-0.838***	-2.444***	-1.323***	2.989***	-1.766***	-1.973***	1.066***	-2.231***	-1.842***	-0.056	1.371***
	(0.130)	(0.118)	(0.108)	(0.236)	(0.066)	(0.075)	(0.071)	(0.102)	(0.089)	(0.130)	(0.068)
Parents' education											
< compulsory	-1.213	14.511***	-10.780**	-31.326*	3.214	7.162**	8.133**	14.481***	2.521	3.407	6.344**
	(4.876)	(4.501)	(4.402)	(17.452)	(2.733)	(3.099)	(3.274)	(4.073)	(3.525)	(5.005)	(2.990)
Compulsory	-15.203***	9.128*	-25.796***	-33.413	-0.476	0.765	24.394***	10.443**	-4.054	-8.275	4.537
	(5.355)	(4.845)	(5.244)	(21.393)	(2.970)	(3.238)	(4.865)	(4.441)	(3.589)	(5.082)	(4.761)
> compulsory	-15.488***	19.656***	-13.839*	-87.251**	8.637*	10.241*	29.205***	19.442***	7.538	-3.062	-4.312
	(5.384)	(7.415)	(7.665)	(41.506)	(4.893)	(5.466)	(6.193)	(7.029)	(5.988)	(5.231)	(8.314)
Tertiary	-3.651	83.467***	-20.565*	-134.427***	55.147***	59.033***	40.983***	84.846***	38.574**	-3.416	-11.305
	(6.120)	(25.092)	(11.642)	(36.427)	(15.582)	(17.556)	(11.829)	(26.496)	(17.817)	(5.860)	(10.523)
Parents' education× Gini											
Gini × < compulsory	0.062	-0.445***	0.279**	0.524	-0.078	-0.191**	-0.201**	-0.441***	-0.075	-0.065	-0.140*
	(0.126)	(0.138)	(0.117)	(0.322)	(0.073)	(0.086)	(0.079)	(0.121)	(0.101)	(0.127)	(0.072)
Gini × Compulsory	0.401***	-0.318**	0.692***	0.594	0.001	-0.005	-0.616***	-0.327**	0.115	0.238*	-0.093
	(0.139)	(0.149)	(0.139)	(0.395)	(0.082)	(0.091)	(0.121)	(0.134)	(0.103)	(0.129)	(0.114)
Gini × > compulsory	0.456***	-0.624***	0.366*	1.614**	-0.244*	-0.297*	-0.749***	-0.603***	-0.244	0.119	0.116
	(0.138)	(0.227)	(0.209)	(0.769)	(0.138)	(0.157)	(0.155)	(0.212)	(0.175)	(0.133)	(0.197)
Gini × Tertiary	0.134	-2.520***	0.528*	2.585***	-1.577***	-1.710***	-1.060***	-2.560***	-1.113**	0.127	0.303
	(0.159)	(0.775)	(0.309)	(0.685)	(0.450)	(0.513)	(0.310)	(0.828)	(0.518)	(0.150)	(0.251)
Age	0.063***	0.388***	0.082**	0.163**	0.328***	0.325***	-0.142***	0.429***	0.220***	-0.132***	-0.094***
	(0.017)	(0.016)	(0.034)	(0.067)	(0.012)	(0.012)	(0.012)	(0.016)	(0.015)	(0.016)	(0.029)
Male	0.157	0.144	0.068	-0.281	0.152	0.503***	0.451***	0.272*	0.419***	0.258***	0.026
	(0.102)	(0.145)	(0.130)	(0.417)	(0.139)	(0.137)	(0.084)	(0.144)	(0.150)	(0.097)	(0.212)
Constant	23.202***	63.260***	40.602***	-160.392***	44.778***	50.041***	-40.551***	53.999***	51.364***	0.557	-58.678***
	(4.820)	(3.857)	(3.460)	(14.584)	(2.528)	(2.771)	(3.087)	(3.492)	(3.189)	(4.867)	(3.338)

* p<0.10, ** p<0.05, *** p<.01

Notes: This table shows multinomial logit results on the effects of observable migrants characteristics on their probability of migrating in each potential destination. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Controls include number of migrants from the same Italian region of origin and born in the same year as 2G migrants' parents, Gini index in the country of residence and year of birth of the 2G migrant, parents' level of education, an interaction between parents' education level and Gini index, age and gender of the 2G migrant, and Italian region of origin fixed effects. Parents' education is defined as highest level of education reached by one of the parents. Standard errors clustered at family level.

Source: Authors' calculations from the AIRE and SHIW data.

Table 3.A.7. Accounting for self-selection: likelihood of tertiary education.

	Likelihood of tertiary education										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Argentina	0.063***	0.038**	0.043**	0.047	0.046	0.041	0.044	-0.000	0.003	0.000	-0.010
	(0.013)	(0.016)	(0.021)	(0.040)	(0.057)	(0.053)	(0.049)	(0.062)	(0.055)	(0.048)	(0.048)
Australia	0.018	0.085***	0.096***	0.102	0.156**	0.200***	0.211***	0.089***	0.090***	0.089***	0.087***
	(0.013)	(0.015)	(0.026)	(0.066)	(0.062)	(0.056)	(0.056)	(0.015)	(0.016)	(0.016)	(0.016)
Belgium	-0.124***	-0.060***	-0.047*	-0.038	-0.044	-0.061	-0.043	-0.071***	-0.073***	-0.073***	-0.075***
	(0.015)	(0.013)	(0.027)	(0.052)	(0.050)	(0.041)	(0.039)	(0.022)	(0.025)	(0.025)	(0.025)
Brazil	0.406***	0.148***	0.375***	0.351**	0.281	0.148	0.064	0.151***	0.151***	0.151***	0.150***
	(0.031)	(0.035)	(0.129)	(0.156)	(0.204)	(0.203)	(0.230)	(0.035)	(0.035)	(0.036)	(0.036)
Canada	0.064***	0.102***	0.127***	0.153*	0.174**	0.185***	0.243***	0.069	0.066	0.065	0.054
	(0.016)	(0.018)	(0.033)	(0.086)	(0.083)	(0.065)	(0.060)	(0.054)	(0.059)	(0.057)	(0.056)
Switzerland	-0.145***	-0.091***	-0.064**	-0.061	-0.043	-0.046	0.001	-0.096***	-0.097***	-0.097***	-0.097***
	(0.012)	(0.011)	(0.030)	(0.060)	(0.051)	(0.056)	(0.050)	(0.013)	(0.014)	(0.015)	(0.015)
France	-0.052***	0.022*	-0.010	-0.030	-0.051	-0.046	-0.072	0.014	0.014	0.013	0.010
	(0.012)	(0.012)	(0.032)	(0.053)	(0.065)	(0.062)	(0.073)	(0.017)	(0.019)	(0.018)	(0.018)
United Kingdom	-0.142***	-0.098***	-0.065**	-0.057	-0.014	0.017	0.043	-0.098***	-0.098***	-0.099***	-0.099***
	(0.010)	(0.011)	(0.025)	(0.066)	(0.063)	(0.051)	(0.055)	(0.011)	(0.011)	(0.011)	(0.011)
Germany	-0.179***	-0.093***	-0.077***	-0.066	-0.055	-0.055	-0.023	-0.102***	-0.103***	-0.103***	-0.105***
	(0.014)	(0.012)	(0.021)	(0.048)	(0.052)	(0.038)	(0.036)	(0.018)	(0.020)	(0.020)	(0.020)
United States	0.052***	0.085***	0.094***	0.097***	0.093***	0.091***	0.092***	0.072***	0.072***	0.071***	0.065***
	(0.016)	(0.015)	(0.023)	(0.028)	(0.023)	(0.020)	(0.019)	(0.025)	(0.026)	(0.024)	(0.024)
Venezuela	0.345***	0.377***	0.349***	0.323***	0.255	0.161	0.077	0.379***	0.380***	0.379***	0.378***
	(0.028)	(0.031)	(0.091)	(0.125)	(0.180)	(0.181)	(0.213)	(0.031)	(0.031)	(0.031)	(0.031)
Constant	1.460***	0.582	0.635	0.614	0.627	0.688	0.582	0.529	0.517	0.522	0.547
	(0.539)	(0.451)	(0.493)	(0.512)	(0.568)	(0.562)	(0.505)	(0.459)	(0.465)	(0.469)	(0.472)
Parents educ.			✓	✓	✓	✓	✓	✓		✓	✓
Ita region FE			✓	✓	✓	✓	✓	✓		✓	✓
Est. prob			✓	✓	✓	✓	✓				
Est. prob^2				✓	✓	✓	✓				
Est. prob^3					✓	✓	✓				
Est. prob^4						✓	✓				
Est. prob. interactions							✓				
Dahl prob.								✓		✓	✓
Dahl prob.^{2}										✓	✓
Dahl prob.^{3}										✓	✓
Dahl prob.^{4}											✓
Obs.	23,056	23,056	23,056	23,056	23,056	23,056	23,056	23,056	23,056	23,056	23,056
Adj. R-sqr	0.023	0.236	0.235	0.235	0.235	0.235	0.234	0.236	0.236	0.236	0.236

*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows the host-country effect on likelihood of completing tertiary education for various self-selection controls. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Controls always include gender, age and squared age of 2G migrant. Column (2) further adds parents' education level and Italian region fixed effects (self-selection on observables). Columns (3)-(11) consider different specifications of the self-selection bias correction term from the estimated probabilities of migrating in the different destinations from the first stage. Column (3) defines the bias-correction term as the linear sum of the probabilities of migrating in each potential destination. Column (4), (5) and (6) add, progressively, the square, the cubes, and the fourth power of each estimated probability. Column (7) adds further the linear interaction of each probability. Columns (8)-(11) consider a bias-correction term á la Dahl. Column (8) includes the estimated probabilities of migrating in the chosen country. Columns (9)-(11), progressively, add their square, cube and fourth power. Parents' education is defined as highest level of education reached by one of the parents. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.8. Accounting for self-selection: estimated household income.

	Estimated household income										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Australia	1.710***	1.800***	1.609***	1.584***	1.674***	1.768***	1.788***	1.806***	1.807***	1.804***	1.799***
	(0.021)	(0.027)	(0.066)	(0.161)	(0.192)	(0.180)	(0.178)	(0.029)	(0.029)	(0.030)	(0.031)
Belgium	0.548***	0.586***	0.503***	0.529***	0.559***	0.546***	0.622***	0.569***	0.569***	0.564***	0.557***
	(0.027)	(0.029)	(0.069)	(0.138)	(0.137)	(0.107)	(0.118)	(0.038)	(0.038)	(0.036)	(0.034)
Brazil	-0.107**	-0.395***	-1.145**	-1.410***	-1.090*	-1.105**	0.469	-0.391***	-0.391***	-0.393***	-0.396***
	(0.052)	(0.053)	(0.463)	(0.514)	(0.587)	(0.454)	(0.611)	(0.053)	(0.053)	(0.053)	(0.054)
Canada	1.509***	1.560***	1.450***	1.449***	1.477***	1.509***	1.678***	1.510***	1.512***	1.492***	1.481***
	(0.020)	(0.026)	(0.075)	(0.217)	(0.231)	(0.189)	(0.204)	(0.072)	(0.060)	(0.053)	(0.045)
Switzerland	1.279***	1.332***	1.203***	1.180***	1.210***	1.225***	1.364***	1.324***	1.324***	1.324***	1.326***
	(0.022)	(0.022)	(0.084)	(0.160)	(0.157)	(0.164)	(0.175)	(0.025)	(0.025)	(0.025)	(0.025)
France	0.362***	0.458***	0.356***	0.333***	0.315***	0.289***	0.215***	0.446***	0.446***	0.440***	0.439***
	(0.020)	(0.022)	(0.068)	(0.097)	(0.107)	(0.081)	(0.083)	(0.027)	(0.026)	(0.025)	(0.024)
United Kingdom	0.519***	0.586***	0.386***	0.364**	0.432**	0.505***	0.563***	0.585***	0.586***	0.584***	0.581***
	(0.017)	(0.022)	(0.067)	(0.167)	(0.191)	(0.169)	(0.175)	(0.022)	(0.022)	(0.022)	(0.022)
Germany	0.103***	0.191***	0.152**	0.160	0.168	0.190	0.276**	0.177***	0.177***	0.173***	0.171***
	(0.023)	(0.024)	(0.061)	(0.126)	(0.144)	(0.117)	(0.124)	(0.030)	(0.029)	(0.028)	(0.027)
United States	1.558***	1.610***	1.708***	1.711***	1.685***	1.662***	1.662***	1.590***	1.592***	1.584***	1.579***
	(0.032)	(0.034)	(0.060)	(0.061)	(0.050)	(0.043)	(0.043)	(0.044)	(0.039)	(0.038)	(0.037)
Constant	3.381***	2.122***	3.164***	3.719***	3.978***	4.053***	2.228***	2.039***	2.035***	2.070***	2.135***
	(0.757)	(0.667)	(0.765)	(0.852)	(0.954)	(0.837)	(0.718)	(0.681)	(0.686)	(0.690)	(0.695)
Parents educ.			✓	✓	✓	✓	✓	✓		✓	✓
Ita region FE			✓	✓	✓	✓	✓	✓		✓	✓
Est. prob			✓	✓	✓	✓	✓				
Est. prob^2				✓	✓	✓	✓				
Est. prob^3					✓	✓	✓				
Est. prob^4						✓	✓				
Est. prob. interactions							✓				
Dahl prob.								✓		✓	✓
Dahl prob.^{2}										✓	✓
Dahl prob.^{3}										✓	✓
Dahl prob.^{4}											✓
Obs.	19,172	19,172	19,172	19,172	19,172	19,172	19,172	19,172	19,172	19,172	19,172
Adj. R-sqr	0.118	0.282	0.282	0.283	0.283	0.283	0.282	0.282	0.282	0.282	0.282

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the host-country effect on estimated yearly equivalised household disposable income for various self-selection controls. Income is expressed in 10,000 USD in 2015 PPP. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Controls always include gender, age and squared age of 2G migrant. Column (2) further adds parents' education level and Italian region fixed effects (self-selection on observables). Columns (3)-(11) consider different specifications of the self-selection bias correction term from the estimated probabilities of migrating in the different destinations from the first stage. Column (3) defines the bias-correction term as the linear sum of the probabilities of migrating in each potential destination. Column (4), (5) and (6) add, progressively, the square, the cubes, and the fourth power of each estimated probability. Column (7) adds further the linear interaction of each probability. Columns (8)-(11) consider a bias-correction term á la Dahl. Column (8) includes the estimated probabilities of migrating in the chosen country. Columns (9)-(11), progressively, add their square, cube and fourth power. Parents' education is defined as highest level of education reached by one of the parents. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.9. Returns to migration: education and occupation.

	Tertiary degree		**Employment**		**Unemployment**		**Inactiveness**	
	(1) *Female*	(2) *Male*	(3) *Female*	(4) *Male*	(5) *Female*	(6) *Male*	(7) *Female*	(8) *Male*
Argentina	-0.000	0.035	0.269***	0.093*	-0.070**	-0.087*	-0.198***	-0.006
	(0.064)	(0.045)	(0.054)	(0.051)	(0.035)	(0.049)	(0.042)	(0.017)
Australia	0.113***	0.065***	0.482***	0.193***	-0.081***	-0.162***	-0.400***	-0.032**
	(0.023)	(0.020)	(0.033)	(0.027)	(0.023)	(0.025)	(0.033)	(0.014)
Belgium	-0.111***	-0.023	0.270***	0.028	0.011	-0.031	-0.281***	0.003
	(0.036)	(0.022)	(0.042)	(0.031)	(0.030)	(0.029)	(0.037)	(0.012)
Brazil	0.090*	0.205***	0.140***	0.123***	-0.063***	-0.089***	-0.077*	-0.034**
	(0.049)	(0.046)	(0.043)	(0.021)	(0.021)	(0.017)	(0.039)	(0.013)
Canada	0.072	0.097*	0.366***	0.128**	-0.087**	-0.120**	-0.279***	-0.008
	(0.073)	(0.050)	(0.063)	(0.055)	(0.042)	(0.053)	(0.051)	(0.018)
Switzerland	-0.136***	-0.066***	0.355***	0.129***	-0.087***	-0.143***	-0.267***	0.014*
	(0.019)	(0.015)	(0.022)	(0.019)	(0.015)	(0.018)	(0.021)	(0.008)
France	-0.001	0.033*	0.351***	0.119***	-0.027	-0.091***	-0.324***	-0.028***
	(0.025)	(0.018)	(0.027)	(0.023)	(0.018)	(0.022)	(0.024)	(0.009)
United Kingdom	-0.131***	-0.075***	0.471***	0.199***	-0.110***	-0.171***	-0.361***	-0.028***
	(0.016)	(0.015)	(0.021)	(0.020)	(0.015)	(0.019)	(0.021)	(0.008)
Germany	-0.158***	-0.057***	0.435***	0.117***	-0.108***	-0.103***	-0.328***	-0.014
	(0.028)	(0.018)	(0.030)	(0.026)	(0.022)	(0.024)	(0.028)	(0.009)
United States	0.087**	0.066***	0.438***	0.180***	-0.089***	-0.161***	-0.350***	-0.020*
	(0.037)	(0.022)	(0.033)	(0.027)	(0.023)	(0.025)	(0.029)	(0.011)
Venezuela	0.508***	0.283***	0.439***	0.181***	-0.054**	-0.148***	-0.385***	-0.033***
	(0.043)	(0.041)	(0.036)	(0.025)	(0.021)	(0.023)	(0.033)	(0.012)
Constant	0.770	0.356	-1.352*	-0.300	0.761	1.155**	1.591**	0.146
	(0.677)	(0.591)	(0.812)	(0.636)	(0.569)	(0.583)	(0.775)	(0.284)
Obs.	10,379	12,677	10,379	12,677	10,379	12,677	10,379	12,677
Adj. R-sqr	0.260	0.217	0.187	0.114	0.051	0.114	0.162	0.024

*** p<0.01, ** p<0.05, * p<0.1.

Notes: This table shows the host-country effect on education and occupation outcomes of 2G migrants by gender, net of self-selection. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects, self-selection on unobservables control. Parents' education is defined as highest level of education reached by one of the parents. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.10. Returns to migration: estimated household income.

	Est. household income			*Est. hourly earnings*		
	(1)	(2)	(3)	(4)	(5)	(6)
Australia	1.807***	1.657***	1.945***	26.955***	26.813***	25.779***
	(0.029)	(0.031)	(0.029)	(0.227)	(0.232)	(0.247)
Belgium	0.569***	0.612***	0.301***	5.270***	5.218***	3.769***
	(0.038)	(0.039)	(0.040)	(0.200)	(0.209)	(0.220)
Brazil	-0.391***	-0.459***	0.379***	12.204***	11.673***	16.800***
	(0.053)	(0.053)	(0.087)	(0.695)	(0.693)	(1.053)
Canada	1.512***	1.718***	1.289***	15.398***	18.508***	11.477***
	(0.060)	(0.065)	(0.063)	(0.368)	(0.409)	(0.375)
Switzerland	1.324***	1.189***	1.395***	17.858***	17.933***	16.572***
	(0.025)	(0.026)	(0.026)	(0.202)	(0.223)	(0.203)
France	0.446***	0.413***	0.420***	3.223***	2.862***	2.957***
	(0.026)	(0.027)	(0.026)	(0.146)	(0.153)	(0.150)
United Kingdom	0.586***	0.542***	0.534***	0.767***	0.751***	-1.431***
	(0.022)	(0.023)	(0.022)	(0.142)	(0.149)	(0.149)
Germany	0.177***	0.045	0.390***	1.913***	2.740***	0.866***
	(0.029)	(0.031)	(0.030)	(0.168)	(0.179)	(0.175)
United States	1.592***	1.578***	1.539***	11.538***	11.714***	9.789***
	(0.039)	(0.039)	(0.040)	(0.257)	(0.255)	(0.281)
Constant	2.035***	1.963***	1.977***	9.104*	7.610	7.590
	(0.686)	(0.711)	(0.711)	(4.792)	(5.066)	(5.066)
Obs.	19,172	19,172	19,172	16,884	16,884	16,884
Adj. R-sqr	0.282	0.263	0.264	0.343	0.333	0.325

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the host-country effect on estimated household income of 2G migrants, net of self-selection. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. outcome variable is estimated yearly equivalised household income from LIS data. Column (1) considers as outcome estimated income from the full LIS population. Column (2) considers only natives and column (3) only migrants in LIS. Controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects, self-selection on unobservables control. Parents' education is defined as highest level of education reached by one of the parents. Monetary values are in 10,000 USD in 2015 PPP. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.11. Different years of SHIW, estimated income.

	SHIW Year				
	2008	**2010**	**2012**	**2014**	**2016**
Australia	1.800***	1.743***	1.860***	1.807***	1.767***
	(0.029)	(0.032)	(0.039)	(0.029)	(0.034)
Belgium	0.629***	0.409***	0.362***	0.569***	0.569***
	(0.068)	(0.093)	(0.085)	(0.038)	(0.035)
Brazil	-0.373***	-0.404***	-0.317***	-0.391***	-0.413***
	(0.061)	(0.054)	(0.057)	(0.053)	(0.055)
Canada	1.525***	1.190***	1.034***	1.512***	1.581***
	(0.110)	(0.169)	(0.152)	(0.060)	(0.042)
Switzerland	1.298***	1.199***	1.169***	1.324***	1.297***
	(0.046)	(0.065)	(0.055)	(0.025)	(0.025)
France	0.429***	0.336***	0.372***	0.446***	0.462***
	(0.039)	(0.040)	(0.035)	(0.026)	(0.031)
United Kingdom	0.570***	0.536***	0.570***	0.586***	0.546***
	(0.025)	(0.027)	(0.029)	(0.022)	(0.022)
Germany	0.194***	0.080	0.047	0.177***	0.164***
	(0.050)	(0.062)	(0.054)	(0.029)	(0.031)
United States	1.626***	1.514***	1.553***	1.592***	1.619***
	(0.050)	(0.049)	(0.048)	(0.039)	(0.044)
Constant	1.562***	2.599***	1.548*	2.035***	3.521***
	(0.548)	(0.640)	(0.873)	(0.686)	(0.772)
Obs.	17,638	17,573	17,531	19,172	18,628
Adj. R-sqr	0.243	0.272	0.245	0.282	0.286

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the host-country effect on estimated equivalised household disposable income of 2G migrants, net of self-selection. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents born between 1960 and 1980 and living in Italy in 2008, 2010, 2012, 2014 or 2016. Controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects, self-selection on unobservables control. Parents' education is defined as highest level of education reached by one of the parents. Monetary values are in 10,000 USD/year in 2015 PPP. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.12. Changes in 2G sample: 2G born in current host country and number of Italian parents.

			ITA parents	
	Baseline	**Living in host**	**Only one**	**Two**
	(1)	(2)	(3)	(4)
Australia	1.807***	1.807***	1.646***	1.815***
	(0.029)	(0.029)	(0.055)	(0.031)
Belgium	0.569***	0.567***	0.631***	0.559***
	(0.038)	(0.040)	(0.047)	(0.089)
Brazil	-0.391***	-0.390***	-0.248**	-0.074
	(0.053)	(0.053)	(0.096)	(0.223)
Canada	1.512***	1.500***	1.287***	1.541***
	(0.060)	(0.066)	(0.049)	(0.063)
Switzerland	1.324***	1.428***	1.244***	1.271***
	(0.025)	(0.054)	(0.042)	(0.042)
France	0.446***	0.451***	0.352***	0.444***
	(0.026)	(0.026)	(0.037)	(0.036)
United Kingdom	0.586***	0.659***	0.474***	0.687***
	(0.022)	(0.072)	(0.030)	(0.031)
Germany	0.177***	0.172***	0.142***	0.123*
	(0.029)	(0.031)	(0.031)	(0.074)
United States	1.592***	1.472***	1.730***	1.600***
	(0.039)	(0.058)	(0.101)	(0.048)
Constant	2.035***	2.040***	2.279***	2.104***
	(0.686)	(0.687)	(0.693)	(0.670)
Obs.	19,172	14,166	7,159	11,391
Adj. R-sqr	0.282	0.280	0.275	0.278

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the host-country effect on estimated equivalised household disposable income of 2G migrants, net of self-selection. Sample includes 2G migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2014 born between 1960 and 1980. Baseline sample defines 2G migrants as born abroad or migrated before age 18, and that have at least one known Italian parent. Column (2) includes migrants born abroad and living in their birth country or migrated before age 18, and that have at least one known Italian parent. Column (3) considers only 2G migrants with surely only one Italian parent from the baseline. Column (4) considers only 2G migrants with two known Italian parents from baseline. Controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects, self-selection on unobservables control. Parents' education is defined as highest level of education reached by one of the parents. Monetary values are in 10,000 USD/year in 2015 PPP. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.13. IV estimation of returns to migration, first stage.

	ARG (1)	AUS (2)	BEL (3)	BRA (4)	CAN (5)	CH (6)	FRA (7)	GBR (8)	GER (9)	ITA (10)	USA (11)	VEN (12)
Panel A: Both instruments (F=84.538)												
Migrants' network	0.004***	0.004***	0.003***	0.003***	0.004***	0.003***	0.004***	0.003***	0.003***	-	0.004***	0.004***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Gini × Yrs. school parents	0.002***	0.003***	0.002***	0.001***	0.002***	0.003***	0.002***	0.003***	0.003***	-	0.002***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel B: Only Network (F=27.903)												
Migrants' network	0.008***	0.007***	0.006***	0.011***	0.008***	0.008***	0.008***	0.008***	0.006***	-	0.007***	0.008***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel C: Only Gini (F=298.662)												
Gini × Yrs. school parents	0.003***	0.005***	0.003***	0.001***	0.004***	0.004***	0.003***	0.005***	0.004***	-	0.003***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Ita region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parents' educ	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows first stage estimates of effect of host-country fixed effects on 2G outcomes. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Country fixed effects are endogenous controls, instrumented by migrants network and interaction between Gini at 2G birth year and 1G years of school Exogenous controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects and Gini index in 2G year of birth. Parents' education is defined as highest level of education reached by one of the parents. Monetary values are in USD in 2015 PPP. Yearly earnings are further expressed in 10,000 USD/year, hourly earnings in USD/hour. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.14. IV estimation of returns to migration.

	Tertiary education			*Employment*			*Est. HH income*			*Est. hourly earnings*		
	Both	**Network**	**Gini**	**Both**	**Network**	**Gini**	**Both**	**Network**	**Gini**	**Both**	**Network**	**Gini**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Argentina	0.000	0.048***	-0.031	0.276***	0.332***	0.239***						
	(0.019)	(0.019)	(0.021)	(0.020)	(0.024)	(0.019)						
Australia	0.073	0.092***	0.028	0.389***	0.366***	0.355***	1.628***	1.797***	1.600***	26.040***	26.823***	25.852***
	(0.077)	(0.017)	(0.085)	(0.072)	(0.024)	(0.075)	(0.104)	(0.032)	(0.107)	(0.687)	(0.252)	(0.721)
Belgium	-0.085***	-0.055***	-0.111***	0.191***	0.214***	0.166***	0.538***	0.551***	0.541***	4.895***	5.140***	4.781***
	(0.019)	(0.015)	(0.021)	(0.026)	(0.030)	(0.023)	(0.035)	(0.040)	(0.034)	(0.199)	(0.216)	(0.199)
Brazil	0.096	0.135***	0.097	0.029	0.187***	0.011	-0.188	-0.406***	-0.186	13.359***	11.642***	13.556***
	(0.118)	(0.044)	(0.120)	(0.107)	(0.028)	(0.108)	(0.160)	(0.072)	(0.160)	(1.232)	(0.976)	(1.223)
Canada	0.082*	0.109***	0.044	0.369***	0.381***	0.334***	1.446***	1.559***	1.405***	14.956***	15.495***	14.716***
	(0.042)	(0.020)	(0.047)	(0.039)	(0.024)	(0.041)	(0.056)	(0.032)	(0.058)	(0.382)	(0.246)	(0.410)
Switzerland	-0.110**	-0.088***	-0.128**	0.282***	0.303***	0.256***	1.198***	1.275***	1.200***	17.141***	17.063***	17.313***
	(0.048)	(0.012)	(0.050)	(0.044)	(0.018)	(0.045)	(0.066)	(0.028)	(0.066)	(0.449)	(0.224)	(0.464)
France	0.007	0.037**	-0.014	0.234***	0.297***	0.199***	0.433***	0.413***	0.432***	3.137***	3.201***	3.019***
	(0.021)	(0.015)	(0.024)	(0.022)	(0.023)	(0.023)	(0.031)	(0.030)	(0.033)	(0.200)	(0.176)	(0.215)
United Kingdom	-0.112*	-0.083***	-0.144**	0.383***	0.384***	0.353***	0.423***	0.606***	0.389***	-0.113	0.840***	-0.364
	(0.066)	(0.013)	(0.069)	(0.062)	(0.019)	(0.063)	(0.090)	(0.028)	(0.090)	(0.582)	(0.181)	(0.592)
Germany	-0.105***	-0.073***	-0.136***	0.304***	0.310***	0.283***	0.097*	0.174***	0.087*	1.447***	2.004***	1.255***
	(0.036)	(0.013)	(0.039)	(0.035)	(0.023)	(0.035)	(0.050)	(0.030)	(0.051)	(0.325)	(0.181)	(0.336)
United States	0.064***	0.091***	0.031	0.346***	0.378***	0.305***	1.573***	1.560***	1.589***	11.469***	11.409***	11.538***
	(0.017)	(0.017)	(0.019)	(0.019)	(0.022)	(0.017)	(0.037)	(0.040)	(0.038)	(0.258)	(0.273)	(0.277)
Venezuela	0.321***	0.351***	0.314***	0.232***	0.337***	0.192***						
	(0.077)	(0.035)	(0.085)	(0.065)	(0.026)	(0.069)						
Ita region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parents' educ	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,056	23,056	23,056	23,056	23,056	23,056	19,172	19,172	19,172	16,884	16,884	16,884
Adj. R-squared	0.235	0.235	0.235	0.196	0.196	0.196	0.281	0.280	0.281	0.343	0.342	0.342
1st stage F	84.538	27.903	298.662	84.538	27.903	298.662	406.848	33.464	358.793	380.680	30.437	329.910

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the IV estimation of host-country effect on 2G migrants outcomes. Sample includes second generation migrants born between 1960 and 1980 living abroad in 2015 and Italian residents in 2015 and born between 1960 and 1980. Columns (1)-(3) use likelihood of obtaining tertiary education as outcome. Columns (4)-(6) use likelihood of being employed as outcome. Columns (7)-(9) use estimated yearly equivalised household income as outcome. Columns (10)-(12) use estimated hourly earnings as outcome. Controls include gender, age and squared age of 2G migrant, parents' education level, Italian region fixed effects, Gini index in year of birth. Parents' education is defined as highest level of education reached by one of the parents. Country fixed effects are considered as endogenous regressors. Instruments are host-country-specific migrants' network and interaction between Gini index in host country at year of birth of 2G migrant and their parents' education level. Monetary values are in USD in 2015 PPP. Yearly earnings are further expressed in 10,000 USD/year, hourly earnings in USD/hour. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Table 3.A.15. Utility maximization, robustness checks.

		Child's income measure				
	Baseline (1)	**2nd child** (2)	**Mean** (3)	**Max** (4)	**Min** (5)	**Sum** (6)
Panel A: All families						
Pred. income:						
Child	0.211***	0.336**	0.250***	0.228***	0.248***	0.176***
	(0.057)	(0.147)	(0.058)	(0.056)	(0.058)	(0.031)
Parents	0.515***	0.690***	0.496***	0.507***	0.491***	0.561***
	(0.095)	(0.240)	(0.094)	(0.094)	(0.095)	(0.092)
Obs.	56,331	10,584	56,736	56,736	56,736	60,687
Cases	6,259	1,176	6,304	6,304	6,304	6,743
Panel B: First child born after migration						
Pred. income:						
Child	-0.047	-0.036	-0.029	-0.040	-0.021	0.089**
	(0.074)	(0.196)	(0.077)	(0.074)	(0.076)	(0.036)
Parents	0.871***	0.759***	0.850***	0.851***	0.849***	0.881***
	(0.127)	(0.283)	(0.126)	(0.126)	(0.127)	(0.122)
Obs.	41,895	8,217	42,192	42,192	42,192	45,189
Cases	4,655	913	4,688	4,688	4,688	5,021
Panel C: First child born before migration						
Pred. income:						
Child	0.654***	1.035***	0.725***	0.703***	0.691***	0.459***
	(0.102)	(0.276)	(0.104)	(0.100)	(0.102)	(0.068)
Parents	-0.008	0.923	-0.032	-0.014	-0.036	0.029
	(0.143)	(0.974)	(0.142)	(0.143)	(0.141)	(0.138)
Obs.	14,436	2,367	14,544	14,544	14,544	15,498
Cases	1,604	263	1,616	1,616	1,616	1,722

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the effect of estimated yearly equivalised household income of migrants and their first born children on their probability of migrating on the chosen country. Sample includes families where parents (1G) took the migration decisions and first-born children (2G) are born abroad or migrated before age 18. First-born children must also be born between 1960 and 1980 living abroad in 2015. Estimation is carried on through an alternative-specific conditional logit model where the outcome is the utility of migrating into the different potential destinations. Case-specific variables include both parents' birth years, and migration age, first-born's birth year and gender, Italian area (north, centre, south and islands) of origin fixed effects. Alternative-specific variables include first born's and parents' predicted income in each host country. Parents' income is predicted at age 45 on the basis of 1995 LIS data for each host country. For the child's predicted income, Column (1) uses first born child's income, Column (2) second born child's income, Column (3) mean children' income, Column (4) highest children' income, Column (5) lowest children' income, and Column (6) sum of all children' income. Monetary values are in USD in 2015 PPP. Yearly earnings are further expressed in 10,000 USD/year. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE data.

Table 3.A.16. Utility maximization by time of migration and parents' education.

		Parents' education	
	All	**Low**	**High**
Pred. income:			
First child	0.208***	0.210***	0.404***
	(0.057)	(0.064)	(0.132)
Parents	0.516***	1.374***	-0.166
	(0.095)	(0.150)	(0.130)
Obs.	56,331	49,347	6,984
Cases	6,259	5,483	776

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Notes: This table shows the effect of estimated yearly equivalised household income of migrants and their first born children on their probability of migrating on the chosen country. Sample includes families where parents (1G) took the migration decisions and first-born children (2G) are born abroad or migrated before age 18. First-born children must also be born between 1960 and 1980 living abroad in 2015. Estimation is carried on through an alternative-specific conditional logit model where the outcome is the utility of migrating into the different potential destinations. Case-specific variables include both parents' birth years, and migration age, first-born's birth year and gender, Italian area (north, centre, south and islands) of origin fixed effects. Alternative-specific variables include first born's and parents' predicted income in each host country. Parents' income is predicted at age 45 on the basis of 1995 LIS data for each host country. Column (1) considers all families, columns (2)-(3) only those that migrated before the birth of their first child, column (4)-(5) only those that migrated after the birth of their first child. Further, columns (2) and (4) only those where at least one of the parents completed at most compulsory education, and columns (3) and (5) only those where at least one of the parents completed more than compulsory education. Monetary values are in USD in 2015 PPP. Yearly earnings are further expressed in 10,000 USD/year. Standard errors clustered at family level. *Source*: Authors' calculations from the AIRE and SHIW data.

Appendix 3.B Definition of generations

AIRE does not contain a variable indicating family relationships, hence we deduct such information from birth dates and gender variables. We define family relationships with respect to the youngest member of the household. Any member less than 20 older than the youngest is named "child", any member between 20 and 50 years older than the youngest as "parent" and any member at least 50 years older than the youngest as "grandparent". Further, we adjust family relationships on the bases of the age gap of each individual to the oldest member. Any individual with a less than 15 years age gap to the oldest member of the household and of opposite gender is defined as partner of the oldest, and its family relationship is adjusted accordingly. If the oldest member of the household is a man, we further classify as his partner a female no more than 20 years younger and whose occupation is indicated as "housewife". Then, we distinguish between migrants' generations. We define as "first generation" individuals born in Italy, living abroad and who migrated after age 18, as "second generation" individuals born abroad or migrated before age 18, with at least one Italy-born parent, and as "third generation" anybody born abroad from two parents born abroad. Summary statistics by generation are in Table 3.A.2.

Appendix 3.C Income estimation in LIS

Through LIS data, we estimate migrants' descendants' and Italians living in Italy income with the following regression:

$$log(inc_{ij}) = \mu_0 + \mu_1 \boldsymbol{edu}_i + \mu_2 \boldsymbol{X}_i + \xi_{ij} \tag{3.C.1}$$

where inc_{ij} indicates either labour or disposable income for individual i residing into country j, $\boldsymbol{edu}_i$ indicates a set of dummies measuring education level of individual i, and $\boldsymbol{X}_i$ includes gender, age and age squared controls. Equivalised household disposable income is the sum of all income types perceived by any member of the household, net of tax and transfers, divided by the number of equivalent adults living in the household (the square root of the members, following LIS recommendations). Labour income indicates any income the individual perceived from any work carried on during the year (including self-employed). Labour income is estimated separately for men and women. Because migrants' descendants might not face the same opportunities as natives, we estimate their income either by using only natives or only migrant population in each destination country but Italy - where we only estimate income from natives' population. Natives- and migrants- based estimates should offer a plausible range of migrants' descendants estimated income. We compare estimated income for Italians living in Italy with information on income provided by the SHIW. Results show LIS estimates significantly overestimate disposable income by approximately 2024€ per year and overestimate labour income by approximately 1561€/year. Even though these differences are non-negligible, our analysis is unlikely to be harmed. This is particularly true if the differences are constant across destination countries.

Declaration

This dissertation is the result of my own work, and no other sources or means, except the ones listed, have been employed.

11. April 2025

Chiara Malavasi

Curriculum Vitae

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