JYU DISSERTATIONS 311

Ville Seppälä

Essays on Economic Links between Family Members

JYVÄSKYLÄ UNIVERSITY
SCHOOL OF BUSINESS
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ABSTRACT

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This thesis examines various labor market connections between family members. It consists of three empirical essays. The essays employ register panel data compiled by Statistics Finland, consisting of various demographics and labor market information on Finnish citizens. Several econometric methods are applied on the data to study the topic.

The first essay studies the effect of job displacement on the earnings of the adult children and parents of the displaced individuals. A difference-in-differences estimation methodology is used to estimate a causal effect. The results indicate that in some families the parents of the displaced have increased earnings in some of the years following the displacement. The effect suggests that parents increase their labor supply to help their displaced children financially. The effect is robust especially among fathers and parents who are older or have higher income than the median. Displacement does not seem to increase the earnings of the adult children of the displaced.

The second essay studies whether the spouses whose relationship begins while they work at the same workplace are more likely to stay at their workplaces than spouses whose relationship begins outside the same workplace. Coworking with spouse is found to increase the workplace retention probability. The result suggests that some aspects of coworking may be beneficial to couples. However, the effect diminishes over time.

The third essay studies how one spouse's retirement affects the other spouse's retirement probability. A regression discontinuity method is used for causal inference. The results show that in low-earnings households, wife's retirement at her pension eligibility age increases the husband's retirement probability if the husband is older than the wife. This implies that the husband delays his retirement timing until wife reaches her pensions eligibility, after which the couple retires jointly. In contrast, in high-earnings households the husband's retirement advances his younger wife's retirement timing to occur early.

Keywords: household decision-making, family economics, labor economics, register data

TIIVISTELMÄ (ABSTRACT IN FINNISH)

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Esseitä perheenjäsenten taloudellisista yhteyksistä
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Tämä väitöskirja tutkii erilaisia perheenjäsenten välisiä työmarkkinayhteyksiä. Se koostuu kolmesta empiirisestä esseestä. Esseissä käytetään Tilastokeskuksen paneelimuotoisia rekisteriaineistoja, jotka sisältävät tietoa suomalaisten työmarkkinatilanteesta. Aiheen tutkimiseksi aineistoihin hyödynnetään useita ekonometrisia menetelmiä

Ensimmäinen esseessä tutkitaan irtisanotuksi joutumisen vaikutusta irtisanotun henkilön aikuisten lasten ja vanhempien tuloihin. Esseessä käytetään difference-in-differences- estimointimenetelmää kausaalivaikutuksen arvioimiseksi. Tulosten mukaan irtisanotun perheenjäsenen vanhempien tulot kasvavat joissain kotitalouksissa muutamina vuosina irtisanomisen jälkeen. Tämä vaikutus viestii siitä, että vanhemmat lisäävät työnsä tarjontaa auttaakseen irtisanottua lastaan taloudellisesti. Vaikutus on selkeä etenkin irtisanottujen henkilöiden isien osalta, sekä niiden irtisanottujen henkilöiden vanhempien osalta, joiden ikä ja tulot ovat keskimääräistä korkeampia. Irtisanominen ei vaikuta lisäävän irtisanotun henkilön aikuisen lapsen tuloja.

Toisessa esseessä tutkitaan, että pysyvätkö samalla työpaikalla toisensa tapaavat puolisot todennäköisemmin töissä työpaikoillaan kuin puolisot, joiden parisuhde alkaa saman työpaikan ulkopuolella. Havaitaan, että työpaikalla tapaaminen lisää työpaikalla pysymisen todennäköisyyttä, mikä saattaa tarkoittaa, että puolisot kokevat sen hyödylliseksi. Vaikutus kuitenkin heikentyy ajan myötä.

Kolmannessa esseessä tutkitaan, miten yhden puolison eläköityminen vaikuttaa toisen puolison eläköitymistodennäköisyyteen. Esseessä käytetään regression epäjatkuvuus -menetelmää kausaalivaikutuksen arvioimiseksi. Tulosten mukaan vaimon eläköityminen hänen saavuttaessaan eläkeiän lisää hänen miehensä eläköitymistodennäköisyyttä perheissä, joissa tulotaso on keskimääräistä matalampi ja mies on vaimoa vanhempi. Tämä viestii siitä, että mies viivästyttää eläköitymistään niin kauan, että hänen vaimonsa saavuttaa eläkeiän, jonka jälkeen pariskunta eläköityy yhdessä. Sen sijaan korkean tulotason perheissä joissa vaimo on miestä nuorempi, miehen eläköityminen aikaistaa vaimon eläköitymistä.

Avainsanat: kotitalouksien päätöksenteko, perheen taloustiede, työn taloustiede, rekisteriaineistot

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Jyväskylä 20.10.2020
Ville Seppälä

CONTENTS

1 INTRODUCTION

1.1 Introduction

In the standard economics theory, an individual makes choices which maximize their personal welfare. In labor economics, these choices are often related to supply of labor and consumption of goods. To maximize welfare, an individual chooses an optimal balance between consumption (which increases with hours spent on labor) and leisure (which decreases with hours spent on labor). Because of the diminishing marginal returns to consumption and because of the differences in abilities and endowments, the government aims to maximize the social welfare by redistributing some of the consumption possibilities through taxation. Within this framework, the individuals do not influence each other directly, but only indirectly as agents who make self-regarding choices.

However, individuals within the society do actually affect other individuals' labor market outcomes and financial situation also more directly. These economic linkages are especially common between family members. They are partly mandated by laws and regulations. For example, parents have a legal obligation to safeguard the health and wellbeing of their children up to a certain age. Furthermore, an individual's social security benefits may depend on the income of their spouse or on the number of their children.

Family members still affect each other well beyond the legal framework. Household economics and family economics (I use these terms interchangeably) study the economic linkages between household and family members. These linkages cover a wide spectrum. According to Grossbard (2015), "Household economics analyzes all decisions made by households, including those regarding consumption, savings, labor supply, leisure, home production, health, education, fertility, marriage, divorce, environmental protection, emotional wellbeing, and participation in institutions."

In this thesis, I study interesting economic interactions between family members. Family economics studies mostly concern the interactions between spouses, as is the case also in two of my three essays. However, I also study the interactions between adult children and their parents. These interactions remain understudied in the existing family economics literature. In the first essay, I show that an individual's job displacement and the subsequent earnings loss increases their parents' earnings in some family types. This suggests that family members increase their labor supply to provide transfers to the displaced. In the second essay, I show that spouses who meet while working at the same workplace are more likely to continue working at their workplaces than spouses who meet while working at different workplaces. This suggests that spouses draw utility from coworking. In the third essay, I show that an individual's retirement increases their spouse's retirement probability. This implies that spouses synchronize their retirement timings to gain benefit from joint leisure time during retirement.

In summary, this thesis provides new insights on intergenerational effects of job displacements and on the joint time use preferences between family members.In this first section of this introduction chapter, I present the theoretical background for the economic linkages studied in the remainder of the thesis. First, I discuss of the main motivations between intra-family interactions – altruism and reciprocity. Second, I outline family surpluses, which can be thought of as externalities of household's production and which further motivate the intra-family interactions. Third, I overview the unitary and non-unitary family decision-making models, which are used to explain how families reach different labor market outcomes. Finally, I present an overview on my empirical thesis essays that cover the remainder of the thesis.

1.2 Background literature

1.2.1 Altruism and reciprocity as motives for family interactions

Standard economic theory assumes that individuals are self-regarding entities. However, in case of altruistic preferences, individual gains utility from the welfare of other persons as well (Barro 1974; Becker 1974). Altruism can vary from full altruism, in which the individual cares about the welfare of others as much as they care about their own welfare, to lesser degrees of altruism. Individual's altruism or caring towards other persons may vary from person to person.

Altruistic relationships may often be stronger and more common between members of the same household, or the family line in a broader sense (Laitner 1997). The affection and companionship between the family members is stronger than between other individuals. For example, Hamilton (1964) formalizes a rule for "relatedness" between individuals that determines how much one individual is willing to sacrifice their own wellbeing to improve the wellbeing of a specific

other individual. Hamilton refers especially to biological relatedness, but relatedness may also stem from family members' extended exposure to each other, which they often experience.

Most studies on intergenerational altruism between family members only consider parents' altruism towards their children (Laitner 1997). This is reasonable from the point of evolutionary psychology, since the survival of one's family line depends only on the wellbeing and fitness of the descendants and not the ascendants. According to Becker's (1974) Rotten Kid Theorem, children of altruistic parents may take advantage of their parent's altruism by decreasing their labor supply and living on the transfers their parents provide. Parents may have to set prerequisites on their transfers or bequests to prevent this. Models of two-sided intergenerational altruism, in which children also care about their parents wellbeing, also exist (Laitner 1997).

In addition to altruism, reciprocity motives contribute to the strong economics linkage between family members. In case of reciprocity, individuals give transfers and services to each other if they expect to receive other transfers and services in return. For family members, the reciprocal transfers may include cash, tuition fees, care payments, loans and bequests, whereas the reciprocal services may include the provision of housing or care services, for example (Bernheim et al. 1986; Cox 1987; McGarry and Schoeni 1995; Park 2014) .

There are several reasons for why the reciprocity may be stronger between family members than between other individuals in a society. First, family members from different generations are usually more demographically compatible in terms of what they can provide to each other. Parents can provide tuition fees or collaterals for housing loans that the younger generation might find difficult to get from the private market (Laitner 1997). Adult children can provide care services to their ageing parents in return, possibly much later in the future.

Second, family members know each other better than outsiders. Better information on trustworthiness enables the family members to make more informed decisions. It also prevents the "adverse selection" effect related to commercial insurances or loans, which leads to high market premiums (Laitner 1997).

Third, as explained before, the sense of affection is stronger between the family members. Both explicit and implicit reciprocity contracts are therefore obliged more firmly, since not obliging could lead to a loss of this affection (Laitner 1997). The sense of obligation is further reinforced by the "demonstration effect" (Cox and Stark 1996). It states that individuals treat their family members in a manner in which they wish to be treated by themselves by other family members. For example, adult children (middle generation) may care for their elderly parents (older generation) to set a positive example for their own children (younger generation), so that they (younger generation) will care for them (middle generation) in the future. Sense of affection may also make the care and other interactive services such as joint housing more valuable than the market options (Laitner 1997). Altruism and reciprocity motives are often

presented as alternatives to each other, but specific actions can be triggered by a combination of both motives.

1.2.2 Marital surplus and joint time use

The possibility for reciprocity and the comparative advantages between the family members means that it is often useful for family members to specialize on certain tasks. The standard example of this is the spouses' specialization to either market or household production. Market production refers to paid work outside the house, and household production refers to childbearing and other unpaid domestic work. If household members have comparative productivity advantages over the two sectors in comparison to each other, they can each specialize on their respectable sectors of advantage (Becker, 1981). Within-household financial transfers are used to compensate for the different monetary returns from market and household production.

Traditionally, women have more often specialized to household production due to biological and sociological factors. However, there are many forces that have diminished the gendered specialization. For example, technological progress and decreasing child count have reduced the need for labor intensive domestic production (Stevenson and Wolfers 2007). Norms regarding women's role in workplace have shifted to a more liberal direction (Greenwood et al. 2017). The subsequent increases in the employment and wage level of women have reduced the marital surplus from specialization. (Mansour and McKinnish 2014).

Even if specialization is becoming less prevalent, it is only a one form of marital surplus. Marital surplus means that spouses benefit from marriage relative to the situation where they would live by themselves (Bergstrom 1997). To include a greater amount of family members, the concept of marital surplus can be further extended to family surplus. Another commonly studied marital surplus is so called "household public goods". The term is derived from the concept of public goods. Private goods, such as food, are un-shareable, whereas public goods, such as roads, benefit everyone. Similarly, household public goods benefit all household members. Children are often used as an example of their parents' household public good. Other examples include shared living space, household lighting and heating and joint commuting (Bergstrom 1997).

Lundberg (Lundberg 2012) finds that in the older cohorts the men and women with distinctly gendered personality traits were more likely to marry each other, whereas in the younger cohorts the men and women with similar personality traits were more likely to marry each other. Greenwood et al. (2017) also note that this kind of positive assortative mating has become more common. Lundberg (2012) argues that this change over cohorts reflects how the marital surplus is increasingly due to household public goods rather than specialization.

Women's greater labor force participation may have increased the importance of another form of marital surplus - spouse's joint time use. Many joint retirement studies show that spouses retire at the same time or nearly at the same time with each other (Hospido and Zamarro 2014; Smith and Moen 1998; Warren 2015). This joint retirement behavior indicates that spouses prefer to

spend their leisure time together (Coile 2004). Another indication of joint leisure time preference is that spouses harmonize their work schedules to overlap, so that their leisure time also overlaps (Hamermesh 2002; Michaud and Vermeulen 2011).

1.2.3 Models on household decision-making

I have outlined motives for economic interactions between family members. Altruism, reciprocity, and family surpluses motivate family members to take each others into consideration when making labor market decisions. However, so far I have not addressed how the details of these interactions come to be. Different family members may get very different welfare allocations from the interactions, depending on their preferences and the distribution of goods. How do the family members decide on the distribution of goods and labor within the family? In the literature, these decisions are often modelled by either the unitary or the non-unitary household decision-making models (Grossbard 2015).

According to unitary decision-making models (coined by Browning et al. 1994), household makes decisions as a single agent and the household members' within-family income distribution does not affect the distribution of their consumption of goods or their labor supply (Vermeulen 2002). These models are also referred to as altruistic, benevolent, singe-agent or common preference models. Some unitary decision-making models incorporate the preferences of individual household members. Samuelson (1956) assumes that individuals have their own utility functions that are aggregated to a household utility function by consensus between the household members. Becker (1974) assumes that a benevolent household head makes the decisions after taking into account the individual preferences of the household members.

Unitary models are seen as the traditional approach to model household behavior. Recently, they have become criticized, mainly due to required strong assumptions about how individual preferences are incorporated in the decision-making process (Vermeulen 2002). Furthermore, results from many empirical studies imply that the within-household income distribution does affect its members' consumption decisions after all (Fortin and Lacroix 1997; Lundberg et al. 1997).Non-unitary models assume that household members' individual preferences and the within-household income distribution affect the household's labor supply and consumption decisions. They have gained more popularity during the last decades (Donni and Chiappori 2011).

Non-unitary models are commonly further categorized to non-cooperative and cooperative models. Non-cooperative models (or strategic models) assume that the household members maximize their individual utilities with regards to their individual budget constraints and take the actions of other household members as given (Donni and Chiappori 2011). Unlike non-cooperative models, cooperative models produce Pareto efficient outcomes, as they are based on the bargaining process between the household members (Donni and Chiappori 2011). The distribution of bargaining power between the family members determines the Pareto weight of each member's welfare function in the family decision-

making process (Browning and Chiappori 1998; Chiappori et al. 2002). The bargaining power of the household members depends e.g. on their income allocations and prices of market and household goods. Donni and Chiappori (2011) argue that the interaction between non-cooperative and cooperative models has **become increasingly important** in recent research.

1.3 Overview on empirical essays

1.3.1 Overview on data and methods

Throughout this thesis, we use the Finnish longitudinal employer-employee data (FLEED), compiled by Statistics Finland. The data draw from numerous register sources and include labor market and demographic information on total Finnish population over multiple years. The data are in a yearly panel form. The observation period for each year is the last calendar week. At that point, we observe the current workplace, spouse, education, etc. information for each individual. Earnings and other income data are measured as cumulative sums from the past year.

Full population data, together with parental and spousal link variables, enables us to study families at a precise level and over multiple time periods. Furthermore, the data provides exogenous retirement and job displacement events. This enables us to use quasi-experimental estimation techniques, such as regression discontinuity and difference-in-differences methods, to estimate causal effects of these events. These methods mitigate the omitted variable and reverse causality biases that may often be prevalent in studies conducted with more simplistic estimation settings and econometric methodologies.

1.3.2 Involuntary job displacement and earnings of family members

Job displacement causes significant earnings losses to the displaced (Couch and Placzek 2010; Hijzen et al. 2010). However, job displacement may also affect the labor market outcomes of the family members of the displaced. In this essay, I examine how job displacement affects the earnings of the adult children or the parents of the displaced.

So far, related studies have focused almost solely on the effect of displacement on the labor market outcomes of the displaced individual's spouse. Many of these studies find evidence that displacement increases spouse's labor supply (Hardoy and Schøne 2013; Kohara 2010). In the literature, this effect is referred to as the Added Worker Effect (AWE).

Outside of the AWE studies, few studies have estimated the effect of household head's unemployment on the labor market outcomes of their teenage child living in the same household. They find a increases in school dropout rate (Duryea et al. 2007; Skoufias and Parker 2006) and labor force participation among the children (Baldini et al. 2017; Duryea et al. 2007). This study fills and

important caveat by studying the earnings response of adult children and parents living outside the same physical household.

A family member may increase their labor supply in order to finance transfers to the displaced, whose income is lowered due to displacement. This is often presented as the main motive in previous literature. The increase in transfers and earnings may stem from altruistic or reciprocal motivations. Altruistic family member cares about the welfare of the displaced, which induces them to sacrifice part of their leisure to uphold the consumption of the displaced. With reciprocity, the sacrifice of leisure is motivated by expectation of repayment.

To estimate the effect on both children and parents, I use two separate samples. In the upstream sample, I estimate the effect of adult child's (18 years or older) displacement on their parent's (70 years or younger) earnings. In the downstream sample, I conversely estimate the effect of parent's displacement on their adult child's earnings. Non-displaced and their family members are used as a control group in both samples.

Following Couch and Placzek (2010), Huttunen and Kellokumpu (2016) and others, I use plant closures as exogenous job displacement shocks to estimate the causal displacement effect. I do this to prevent omitted variable and reverse causality biases. These biases could manifest if I simply classified employment contract terminations as displacements. With plant closures, displaced individual's, or her family member's, own attributes or intentions do not affect the occurrence of the displacement.

I use the FLEED panel data on displaced (and non-displaced) and their family members from three years before the displacement year to ten years following the displacement year, for 14 years in total. I apply a difference-in-differences (DID) method. The method is used to estimate the difference in the pre- and post displacement earnings change between the treatment and the control groups. This way, the earnings change of non-displaced is used as a reference to account for general trends that affect earnings. Assuming that the earnings of the displaced would progress in same manner in the absence of displacement as the earnings of the non-displaced (parallel trends assumption), the difference in earnings change between the groups indicates the effect of displacement on earnings. The estimation method also negates any time-invariant omitted variable bias. However, I use time-variant observable characteristics as control variables to control for differences in time-variant characteristics.

I find that adult child's displacement increases their parent's earnings. For the total sample, the effect is statistically significant at 5% level in third through fifth years after the displacement year. On average, parent's earnings increase by 348 euros, or 5.21 per cent in the total ten year period.

It is likely that the income loss due to child's displacement increases the net transfers from the parent to the child, and the parent increases her labor supply and earnings to finance the transfers increase. Studying parents' transfers for their children, Brandt and Deindl (2013) show that the transfers are greater the lower the child's income. Therefore, income loss because of displacement may

increase the probability or magnitude of transfers. Brand and Deindl (2013) find that unemployment is associated with a 50% increase in transfer probability and Edwards (2015) finds that it is linked to a 22%increase in transfer size.

I find that the parent's earnings increase is more evident if the parents are older than average. Older parents are more likely to be able to choose to either retire or continue working, so they have a better opportunity to increase their earnings by choosing not to retire. Parent's earnings increase is greater also if the child is older than average. This is partly explained due to older children's parents also being older. However, older children also have greater earnings losses due to displacement, which may lead to greater need for parental transfers and, subsequently, a greater earnings increase.

Parent's earnings increase is also greater among high income parents, also in relative terms. High income parents generally have higher wages, which means that their monetary return for additional work hours is greater. Furthermore, high income jobs may include greater flexibility to increase work hours. High income professionals may also have more opportunities to move to better paying jobs. Fathers' higher income may also partially explain why the earnings increase is greater among fathers than mothers.

Parent's displacement does not increase adult child's earnings. Instead, the effects of parent's displacement on adult child's earnings are mostly negative, although statistically insignificant. There are few possible explanations for the difference in results between downstream and upstream samples. Albertini et al. (2007) show that adult children give less transfers to their parents than vice versa. Children may be less altruistic towards their parents. This makes sense from an evolutionary standpoint, as the survival of family line is dependent on the fitness of the offspring.

Public employment transfers and other social security provisions may alleviate the impact of displacement on net income. This decreases the need for private family transfers and the family members' earnings increase. Great variation in public transfers between the countries should motivate other studies on the subject. From a policy perspective, the relatively rare increases in family members' earnings could indicate that the Finnish social security system is quite adequate in providing financial security to the displaced.

1.3.3 Workplace retention of coworking couples – evidence from population data

In the second essay, I study the workplace retention of coworking couples. Coworking couples are couples whose both spouses work in the same workplace with each other. I show that in Finland the share of coworking couples is approximately seven per cent of all dual-earner couples.

Previous studies show that coworking increses spousal support and reinforces the benefits it provides (Ferguson et al. 2016; Halbesleben et al. 2012; Janning 2006). Coworking spouses also benefit from performing work tasks together, being familiar with same collegues and having a better understanding of each other's work environment (Janning 2006). On the other hand, coworking

has been shown to increase the probability that work-related problems negatively affect the relationship domain (Halbesleben 2012).

Despite the expanding literature on spousal coworking, the literature on the effects of coworking on labour market outcomes is scarce. Coworking can be considered as joint time use, comparable to joint leisure time. Joint leisure time studies indicate that spouses have a preference for joint time use. Spouses harmonize their work schedules to spend more time together outside of work (Hamermesh 2002; Michaud and Vermeulen 2011). Spouses also often retire jointly to spend their retirement time together (Hospido and Zamarro 2014; Warren 2015). However, spouses may value joint leisure time more than joint work time. Sullivan (1996) reports that both spouses gain additional enjoyment from joint leisure time relative to individual leisure time, but only women gain additional enjoyment from joint domestic work. Similarly, Hallberg (2003) finds that the overlap in spouses time uses for sleep, personal care or domestic work is less common than for joint leisure time.

In the empirical section of the study, I examine how likely it is for spouses who meet through work to continue to work at the same workplace. Workplace retention should indicate if the spouses have a preference for joint work time. More specifically, I estimate the period t+1 to t+10 workplace retention of spouses who work at the same workplace at period t+0 and whose relationship is first observed (they marry each other or start cohabiting) at period t+1. I thus assume that these spouses meet each other at the workplace before their relationship begins. I use soon-to-be couples as the empirical sample because using existing coworking couples as the sample could cause self-selection bias: Couples who prefer coworking more than average are more likely to stay coworking as their relationship begins or start coworking during their relationship. Workplace retention is a binary outcome variable that measures if both spouses continue to work at the same workplace in which they were before the beginning of the relationship.

I find that coworking increases workplace retention by ten percentage points in the first year of the relationship. The effect diminishes to five percentage points throughout the ten-year observation period. Some positive aspect of coworking might diminish over time or some negative aspects might become more profound. Spouses in older couples (couples in which male is 27 years or older) are less likely to stay working at the same workplace when the relationship begins. However, spouses of those older couples who do initially stay are more likely to stay for the whole ten year observation period than the younger couples. Older couples have more life experience and might therefore be better at weighting their expected benefits and disbenefits from coworking. Experience may also help at handling unusual work-family interactions and spillover effects.

The interaction between coworking spouses is more common in smaller workplaces (Hedström et al. 2008; Pink et al. 2014). However, I find that effect of coworking on workplace retention is very similar between small (less than 28 workers) and large (28 workers or more) workplaces. This suggests that spending time together in the workplace may not be a crucial benefit of coworking. In that

sense, the results are in line with Hallberg (2003) and Sullivan (1996), whose findings related to domestic time use indicate that spouses do not appreciate joint time spent on domestic work as joint time spent on leisure. The benefits of coworking might be more related to better understanding of spouse's work life, common work-related friends and ease of logistics in term of commuting, for example.

1.3.4 Age, earnings and joint retirement – population-based evidence

In the third essay, I study the joint retirement effect in Finland. The share of pensioners of total population increases and causes stress on public finance. This has motivated research on retirement decisions. A growing subsection of retirement literature considers how an individual's retirement affects their spouse's retirement probability (Hospido and Zamarro 2014; Queiroz and Souza 2017; Warren 2015). In addition to increasing lifespans, the growth in these joint retirement studies is motivated by the increase in dual-earner couples and women's greater labor force participation (Van Gils and Kraaykamp 2008)

Joint retirement is usually attributed to the joint leisure preference between the spouses (Coile 2004). A retired individual gains more utility from spending their retirement period together with their spouse rather than alone. This can be thought as a form of marital surplus.

The results of previous joint retirement studies show considerable variation. For example, gender seems to affect the joint retirement effect in different ways. In some studies, only husband's retirement increases wife's retirement probability (Blau and Riphahn 1999; Hospido and Zamarro 2014; Smith and Moen 1998), while in others only wife's retirement increases husband's retirement probability (Coile 2004). There are also studies that find the joint retirement effect both ways (Queiroz and Souza 2017; Warren 2015) and studies that find no effects at all (Selin 2017; Stancanelli and Van Soest 2012). In this study, I explore the joint retirement effect in a combination of three dimensions - gender, age and income.

I estimate how an individual's retirement affects the retirement probability of their spouse. I use the earnings-related pension eligibility age as the exogenous variation to mitigate omitted variable bias. In Finland, the earnings-related pension eligibility age is 63 years. Individuals above 63 years are eligible to earnings-related pension, whereas individuals below 63 years are ineligible. Reaching the pension eligibility age thus increases the individual's retirement probability exogenously. If the spouse's retirement probability also increases upon individual's pension eligibility, the increase can be attributed to be caused by individual's retirement, since individual's pension eligibility does not affect their spouse's retirement probability directly.

To estimate the joint retirement effect, I use the pension eligibility variation to apply a regression discontinuity (RD henceforth) method (Imbens and Lemieux 2008). Other studies have used the same or similar methods (Hospido and Zamarro 2014; Stancanelli and Van Soest 2012). RD is a quasi-experimental method that can be used when the assignment to treatment group (in this case,

retired individuals) and control group (in this case, non-retired individuals) is at least partly determined by a "cut-off" point (in this case, 63 years) in a continuous variable (in this case, individual's age). In essence, I estimate the expected retirement rates of individuals and their spouses at the cutoff age using data of individuals both below and above the cutoff age separately. The difference in estimates between the groups indicates the joint retirement effect.

I find evidence of joint retirement effect in certain types of households: If the husband is pension eligible, wife's retirement increase his retirement probability in low-income households. This indicates that the husband continues to work after his own pension eligibility age and delays his retirement until his wife once the wife reaches pension eligibility. Then the family members retire jointly. Low household earnings may motivate the husband to continue working rather than retire. In high earnings household, it is more affordable for the husband to retire rather than wait for wife's retirement and retire jointly.

I also find that husband's retirement increases wife's retirement in high earnings households, especially if the wife is not yet pension eligible herself. In high earnings household it is more affordable for wife to advance her retirement timing. If the earnings-related pension eligibility age is the socially desired retirement age, then an increase in the progressivity of pension taxation (low pensions taxed even less, high pensions taxed even more) could help to shift spouses' retirement timings towards that, because it could reduce both the delaying joint retirement effect in low-earnings households and the advancing joint retirement effect in high-earnings households.

I also examine how the breadwinner position within the family contributes to the joint retirement effect. Results suggest that there are gender differences: Breadwinner husband's retirement delays wife's retirement, while breadwinner wife's retirement advances husband's retirement. The first result is more in line with the non-unitary household decision-making framework. It suggests that the husband uses his bargaining power, owing to his higher earnings, to negotiate the wife to delay her retirement. The second result suggests that the wife behaves altruistically towards the husband by enabling him to retire early.

1.4 Summary

This thesis consists of three empirically oriented essays on economic interaction between family members. I contribute to the family economics literature in multiple ways. In the first essay, we estimate the effect of individual's displacement on their adult child's or their parent's earnings, whereas previous literature focuses on the effect individual's displacement on spouse's earnings. In the second essay, I estimate the effect of spouses' coworking on workplace retention. So far, there are no previous studies on the topic. In the third essay, I expand the joint retirement literature by estimating the joint retirement effect with various age and earnings combinations.

Given the novelty of the studies, it would be beneficial to replicate them with data from other countries. This is important also in order to study how the institutional settings affect the results. Finland is a Nordic welfare country with high level of social security, which might contribute greatly to many of the results. To shortly compare the relevant features of Finnish labor market to EU averages: Finns work an average of 36.2 (EU28 36.3) * hours a week and the employment rate is 77.2(EU28 73.9)† percent. Finns retire to old-age pension at an average of 61.5 years in 2019 ‡ .

This thesis is focused on empirical methods and results. Register data provides good quasi-experimental settings to estimate causal effects. However, it is not always optimal for examining family members' motives behind the effects. Future studies, especially qualitative studies, could help in the interpretation of some of the results in this thesis. Furthermore, the topics could be expanded to more theoretically involved directions.

TABLE 1 Summary of results

Chapter	Topic	Data and methods	Main results
Chapter 2	- The effect of job displacement on family member's (parent, adult child) earnings	- Fleed data from 1987 to 2003 - Difference-in-differences method - Treatment group: employees (and their family members) whose plant is closed. - Control group: employees (and their family members) whose plants is not closed.	- Displacement increases earnings of the parent of the displaced - Effect is most robust in families with high-income or older-than-average parents - Displacement does not increase earnings of the adult child of the displaced
Chapter 3	- Workplace retention of couples who meet while working at the same workplace	- FLEED data from 1987 to 2012 - OLS and probit models - Treatment group consists of couples whose spouses work at same workplace in the year before the relationship - Control group of couples whose spouses at different workplaces	- Coworking is associated with a ten percentage point increase in workplace retention probability - The effect decreases over time, although less for older couples -Workplace size does not have a noticeable impact on the effect

* http://appsso.eurostat.ec.europa.eu/nui/submitViewTableAction.do
† https://ec.europa.eu/eurostat/databrowser/view/t2020_10/default/table?lang=en
‡ https://www.etk.fi/en/topical-issues/effective-retirement-age-risen-clearly/

Chapter 4	- The effect of own retirement on spouse's retirement probability	- FLEED data from 2008 to 2015 - Regression discontinuity method, with pension eligibiltiy as the exogenous retirement variation - Treatment group consists of individuals (and their spouses) who are barely eligible for work pension - Control group consists of individuals (and their spouses) who are barely not eligible for work pension	- Found evidence of joint retirement or harmonization of spouses' retirement timings - In low earnings households, husband delays his retirement timing to coincide with wife's retirement - In high-earnings households, wife advances her retirement timing to coincide with husband's retirement

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2 INVOLUNTARY JOB DISPLACEMENT AND EARNINGS OF FAMILY MEMBERS*

Abstract

Involuntary job displacement not only decreases the earnings of the displaced individual but also increases the labour supply of his/her spouse. Similar to the spouse, other family members may respond to the displacement by increasing their labour supply to compensate for the earnings loss of the displaced. This study examines the effect of job displacement on the earnings of the parent and adult children of the displaced. We estimate the effect using longitudinal employer-employee data on the displaced and their family members. We use plant closures as exogenous displacement shocks to mitigate potential omitted variable and reverse causality biases. We find that displacement increases the earnings of the parent of the displaced. The effect is most robust among high-income parents and in families in which the displaced or the parent is older than average. Fathers of the displaced also increase their earnings more than mothers. However, we find no evidence that displacement would increase the earnings of the adult children of the displaced.

2.1 Introduction

Job displacement causes significant and persistent income losses for the displaced (e.g. Couch and Placzek 2010; Hijzen et al. 2010; Verho 2020), but its effects on the displaced individual's family are less well known. Family members

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can, for example, provide financial transfers to the displaced to compensate for the income losses. These transfers may be inspired by caring motives (altruism) or by expectation of repayment (reciprocity). The transfers may decrease the disposable income of the family members, which in turn may increase their labour supply and earnings. Job displacements may also alter family members' expectations and social norms regarding work, if the displaced act as their role models (Clark 2003; Senik 2008). Changes in perception of work may affect family members' labour market participation and earnings.

The added worker effect (AWE) studies examine the effect of individual's displacement or unemployment on spouse's labour market outcomes (Baldini et al. 2017; Fernandes and Felicio 2005; Kohara 2010), but there is a lack of research on how displacement affects the labour market outcomes of the other family members of the displaced. This study extends the AWE literature by examining the effect of displacement on displaced individual's parents' or adult children's earnings (extended added worker effect, EAWE). Our motive is to increase the understanding of the economic dependencies between the family members. To further this, we estimate EAWE across different income classes and family types. EAWE results may also bear policy implications. Assuming that motives for transfers between the family members are similar across different groups or countries, the differences in EAWE across them can indicate differences in the adequacy of public transfers. Several studies (Brandt and Deindl 2013; Edwards 2015; Pesando 2019; Schoeni 2002) show that public transfers crowd out transfers from family members. If public transfers are large, then family transfers following displacements might be less necessary and EAWEs therefore smaller.

We estimate EAWE by using Finnish population data (Finnish longitudinal employer-employee data; FLEED) from 1987 to 2003. The displaced and their family members are connected to one another with a link variable. In studying displacement effect, it is important to use a research setting that mitigates potential endogeneity biases. For example, some employees may be more prone to be displaced than others due to their worse abilities and efforts, which may then also be connected to their and their parents' worse labor outcomes. We use plant closures as exogenous displacement shocks to estimate causal displacement effects, with the non-displaced employees and their family members as the control group. This way, employees' different characteristics won't cause endogeneity bias to the estimates. Furthermore, we apply difference-in-differences (DID) estimators to remove the bias from any remaining time-invariant unobserved heterogeneity between the groups. To acquire a broader perspective on the matter, we estimate both average and yearly earnings effects for the ten-year period following the displacement year.

This study is structured as follows. Section 2 provides a review of related literature. Section 3 presents the data and estimation methods. Section 4 presents the results and Section 5 concludes the paper. We show that adult child's job displacement increases the parent's earnings, especially if the age of the displaced or the parent is higher than the median or if the parent's income is

higher than the median or if the parent is the father. Conversely, we find that a parent's job displacement does not increase the adult child's earnings.

2.2 Background literature

2.2.1 Financial dependences between family members

In this section, we present findings from related studies to provide insight into how job displacement may affect the earnings of the family members of the displaced. In summary, job displacement could reduce family members' earnings through the transmission of social norms and expectations but it may also increase family members' earnings through the increase in net transfers from the family members to the displaced.

People adopt social norms, perceptions and information from various reference groups, such as colleagues, classmates, neighbours and family. If the family is an important reference group, then job displacement may alter family members' perceptions on work and thus change their labour marker behaviour. For example, Clark (2003) finds that while the unemployed report lower well-being than the employed, this well-being loss is smaller if their spouses are also unemployed. Clark's results can be linked to the relative-income hypothesis, which states that well-being depends on the ratio of own and reference group's income (Easterlin 1995). A decrease in the reference group income generally increases own relative income and well-being (Brown et al. 2015 for review). Consequently, the earnings loss of the displaced individual may also reduce their family member's earnings, since the family member needs less earnings than before to reach the same level of well-being. Furthermore, a family member may interpret the displaced individual's decreased earnings as a signal of bad employment prospects (Senik 2008), which might reduce the family member's efforts to search for better paying jobs and thus decrease their expected future earnings.

Job displacement may also affect the intergenerational monetary transfers between the displaced and their family members. Intergenerational transfers have been studied extensively. Albertini et al. (2007) show that transfers from parents to adult children are greater and more frequent than transfers from adult children to parents. Hämäläinen & Tanskanen (2010) find similar results for Finland. Edwards (2015) shows that in the USA, 11% of adult children report receiving transfers from parents annually, with a median transfer amount of $1,058. Brandt and Deindl (2013) find that the median parent-to-child transfers ranged between $860 and $2,475 across 13 European countries. They also show that these transfers increase by parent's income and decrease by adult child's income. Hämäläinen & Tanskanen (2010) report that those parents who evaluate their children to have a weaker economic situation are more likely to give them transfers. Similarly, Park (2014) finds that child-to-parent transfers increase by child's income and decrease by parent's income.

Since job displacement causes large and persistent income losses (Hijzen and Wright 2013; Couch and Placzek 2010), it could increase transfers from the family members to the displaced and decrease transfers from the displaced to the family members. Related to this, unemployment has been linked to a 50% increase in probability to receive transfers (Brandt and Deindl 2013) and a 22% increase in transfer size (Edwards 2015). If the displacement increases the net transfers from the family member to the displaced, it decreases the family member's own disposable income and thus motivates an increase in the family member's labour supply and earnings.

The family member's labour supply upon displacement can be expected to increase especially if the member is altruistic towards the displaced. If a family member A is altruistic towards another family member B, then A gains utility from B's welfare (Barro 1974; Becker 1974). A therefore increases transfers to B if B's marginal utility from consumption is sufficiently larger than A's marginal utility from consumption. Displacement decreases consumption, which, assuming a concave utility function, increases the marginal utility of consumption. This increases transfers from utility-maximizing altruistic family members to the displaced. The transfers can also be motivated by altruism towards another family member than the displaced. For example, if a grandparent is altruistic towards the grandchild, they can provide transfers to the adult child to assist in the grandchild's expenses. To further the point, Chu (1991) suggests that parent's transfers to their descendants may be partly targeted to decrease the probability of the family bloodline going extinct.

The expected effect of displacement on labour supply is more ambiguous if the transfers are reciprocal. In that case, family members provide transfers (tuition fees, care payments, loans, bequests) and services (housing, caring, upbringing) to each other if they expect other transfers and services in return (Bernheim et al. 1986; Cox 1987; McGarry and Schoeni 1995; Park 2014). If the transfer donor expects that the transfer will be fully reciprocated in the future and if they have no credit constraints, they can provide the transfer without increasing their own labour supply. However, displacement causes long-term income losses, which reduces the displaced individual's ability to reciprocate, which may decrease the transfers to the displaced. On the contrary, displacement may change the reciprocity contract to accommodate more net services from the displaced in exchange for more net transfers from the family member, thus increasing the family member's labour supply. Furthermore, family members may provide mutual insurance against displacement to one another as one form of reciprocity. If a displacement occurs, the displaced claims transfers as an insurance payment, and the other family member increases their labour supply to afford the payments.

Public transfers may reduce the need for intergenerational transfers. Upstream transfers are more frequent in European countries with low social security (Pesando 2019; Brandt and Deindl 2013; Hiilamo and Niemelä 2010). This indicates that they supplement the public transfers. In case of downstream transfers, the more generous the country's social security system, the more

frequent the transfers are, but the smaller their monetary value is (Albertini et al. 2007; Brandt and Deindl 2013). Brandt and Deindl (2013) suggest that generous social security enables parents to support their children with small transfers, whereas greater transfers are given in low social security countries, in which the need for transfers is greater. Country specific studies support the crowding-out hypothesis; Edwards (2015) shows that the increase in downward transfers due to unemployment is halved if the unemployed is eligible for unemployment benefits. Schoeni (2002), who uses the variation in unemployment benefit systems between American states as an instrument, finds that a one dollar increase in the unemployment insurance income decreases private transfers from family members by 40 cents.

2.2.2 Previous empirical evidence of the effects of job displacements on family members

Previous literature on the effects of job displacement on family member labour supply focuses on spouses (AWE). Kohara (2010), Hardoy and Schøne (2013) and Huttunen and Kellokumpu (2016) use plant closures or mass layoffs as exogenous job displacement shocks to estimate AWE. Kohara (2010) finds that in Japan, a husband's job displacement increases the work hours of the previously working wives and the labour force participation of the previously non-working wives. In Norway, a wife's earnings increase after the husband's job displacement compensates for 7% to 8% of the earnings loss, but only when the sample is restricted to spouses who do not work in the same industry with each other before the displacement (Hardoy and Schøne 2013). Spouses working in the same industry may be subject to the same industry-specific parallel shocks. Huttunen and Kellokumpu (2016) find that job displacement decreases spouse's employment among Finnish couples, especially if the displaced person is a woman.

To our knowledge, there are no previous studies on the effect of job displacement on own parents' or adult children's earnings, but some studies have examined the effects of parent's unemployment on the labour market outcomes of their teenage children living in the same household. The household head's unemployment increases a child's school dropout rate (Duryea et al. 2007; Skoufias and Parker 2006) and labour force participation (Baldini et al. 2017; Duryea et al. 2007). These findings indicate that some children drop out of school to start working in order to substitute for the parent's earnings losses. On the contrary, parent's unemployment during childhood increases a child's adulthood unemployment probability (Ekhaugen 2009; O'Neill and Sweetman 1998) and decreases the child's adulthood time earnings (Oreopoulos et al. 2008). Ekhagen (2009) suggests that, in addition to shifting the child's social norms, the parent's unemployment can inform the child of the benefits of increased leisure time.

2.3 Data and methods

2.3.1 Sample selection

We use the Finnish longitudinal employer-employee data (FLEED) from years 1987 to 2003. The data combines demographic and labour market information from several administrative sources for the total Finnish population. Parental link codes are used to connect the displaced to family members. To study EAWE, we estimate the effect of the individual's job displacement on their family member's annual earnings, which consist of income from work and entrepreneurial activity. This outcome variable is used to present a concise estimate of labour supply.

We use two separate samples. In the 'upstream' sample, we estimate the effect of the adult child's displacement on parent's earnings. In the 'downstream' sample, we estimate the effect of the parent's displacement on the adult child's earnings. Both samples are restricted to adult children and parents who are between 18 and 70 years old throughout the observation period. If the displaced individual has multiple parents (upstream sample) or multiple adult children (downstream sample), then only one of them is selected to the sample. The selection is done randomly to attain generalizable estimates. The observation period spans 14 years, from three years before the displacement year (D-3) to ten years after the displacement year (D+10). The sample is a balanced panel, that is, only individuals and their family members who are observed in each year (D-3 to D+10) are included. Using balanced panels reduces sample size but decreases potential sample selection bias.

We attempt to identify the causal displacement effect, and therefore it is important to mitigate the omitted variable bias. Simply comparing the displaced individuals' and their family members' post-displacement earnings to their pre-displacement earnings ignores the effects of other changes that occur concurrently with the displacement. Therefore, we compare the treatment group of displaced individuals and their family members to the control group of non-displaced individuals and their family members.

The definition of displacement is important in assigning treatment and control groups. Simply defining displacement as the termination of an employment contract could expose estimates to reverse causality or omitted variable bias. For example, an increase in a family member's earnings may increase transfers to the individual, which may induce the individual to quit work voluntarily. Furthermore, even if a worker was dismissed involuntarily, it could be because of inadequate professional qualities. Similar to many job displacement and AWE studies (e.g. Couch and Placzek 2010; Kohara 2010; Huttunen and Kellokumpu 2016), we mitigate these biases by using plant closures as exogenous job displacement shocks.[§] We assume that in case of a

[§] A plant is any type of establishment or workplace owned by a firm with a unique plant identification code. A firm can have multiple plants.

plant closure, an individual's personal motivations or abilities do not influence his/her probability of being displaced. To identify plant closures, we observe each private sector plant and its workers in the last week of each sample year. All variables in the study are measured at this annual observation period. An individual experiences a job displacement if the plant they work at in year D-1 is permanently closed during the following year D. An individual whose year D-1 plant is not closed during year D is assigned to the control group (non-displaced). The control group individuals can still leave their workplaces for other reasons than plant closure and still be included in the sample. Krolikowski (2017) finds that excluding those who leave for other reasons from the sample drastically overstate the estimates of earnings losses. Only individuals who work in their year D-1 plants also in years D-2 and D-3 are included in the sample in order to not misidentify individuals who stop working in temporary workplaces as displaced. Furthermore, the pre-displacement years provide comparison points for the earnings progression.

The greater the displaced individual's earnings loss, the greater the probability that any EAWE can be identified from the data. Following Huttunen and Kellokumpu (2016), we study the effects of job displacements occurring during the early 1990s Finnish recession. The recession was triggered by the financial sector crisis and the collapse of the Soviet Union, and lead to diminished earnings prospects. Korkeamäki and Kyyrä (2014) show that the earnings losses of those displaced during this 1990s Finnish recession are considerably greater than earnings losses of those displaced during the subsequent recovery period. We use years 1990 to 1993 as the base years (D-1) to construct four panel waves, each with observations from three years before (D-3) to ten years after (D+10) the displacement year D. The waves are combined into a single panel sample. Focusing on recession years means that the earnings losses of the displaced and their family members represent the upper bound of the displacement effect. If family members show no response to displacement during this time, then it may be unlikely for them to show response in other times either.

To ascertain that a single individual's work effort does not have a noticeable impact on plant closure probability itself, the sample only includes workers from plants that have a total of ten or more workers during the pre-displacement year D-1. The upper limit of workers at D-1 is set to 1,000 to reduce the possibility that a single plant closure has a disproportionately large impact on the local labour market. Organisational changes can alter the plant ID in the data, which could cause us to misinterpret a plant as closed. To mitigate this, a closed plant (no worker has the plant ID at year D) is not deemed closed if 50% or more of its year D-1 workers are working in one different plant at year D. In these cases, it is assumed that the workers' new plant is the same plant as in the previous year.

If the displaced and the family member live close to each other or work in the same industry before the displacement, then the family member's prospects for work could be affected by the same labour market shocks that caused the individual's displacement. For example, if workplaces in the same industry or area operate in the same supply chain as the closed plant, or if the earnings losses

and possible outmigration due to the plant closure cause a general loss of demand in the area. To reduce the bias from these possible parallel shocks, the sample is restricted to individuals whose family members do not live in the same municipality or work in the same company or industry (two-digit level categorization) before the displacement as the displaced. We explore the effects of this restriction in the results section.

The final upstream sample consists of 284,000 individual-parent pairs. In 1.89% of those pairs the individual is displaced during year D. The downstream sample consists of 218,000 individual-child pairs, with 1.63% of the individuals displaced during year D.

2.3.2 Descriptive evidence

Figures 1 to 4 provide graphical evidence on how displacement is linked to the labour market outcomes of the displaced (Figures 1 and 2) and their family members (Figures 3 and 4). Figures 1 and 3 depict the upstream sample, in which the displaced is an adult child and the family member is a parent. Figures 2 and 4 depict the downstream sample, in which the displaced is a parent and the family member is an adult child. In addition to earnings, the figures show the average income and employment progression for the treatment (displaced) and control (non-displaced) groups before and after the displacement year.

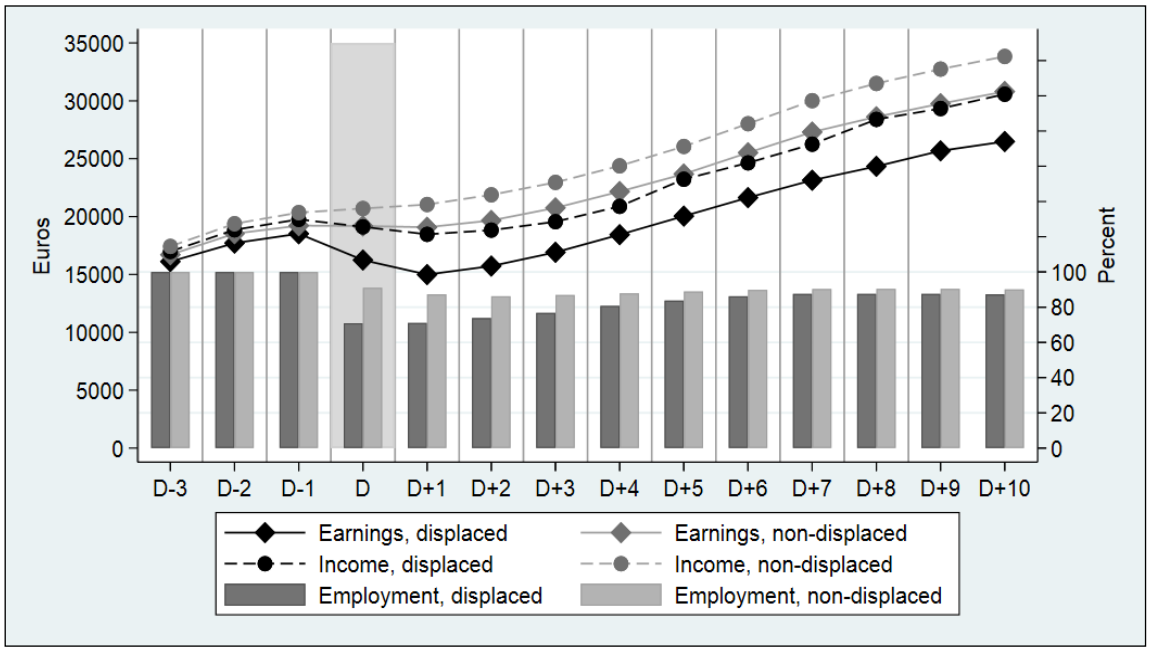
Figure 1 shows that, in the upstream sample, the earnings of the displaced decrease from €17,300 before their displacement (D-1) to €15,000 during the displacement year D and to €13,400 in D+1. The earnings loss is greater in the year following the displacement year, since the displaced still work some portion of the displacement year at the pre-displacement workplace. Meanwhile, the earnings of the non-displaced remain at around €18,000. After D+1, the earnings of both groups increase, following the business cycle and age-related trends. However, the earnings of the displaced remain at a lower level than the earnings of the non-displaced.

Figure 2 indicates that job displacement is related to an even greater earnings decrease in the downstream sample, in which the displaced are older. Similarly, Couch and Placzek (2010) and Hijzen and Wright (2010) find that older displaced workers have greater income losses than younger displaced workers. Older workers have probably accumulated more experience (Couch and Placzek 2010) and firm-specific human capital, which increases their earnings while employed, but also causes greater earnings losses upon displacement. Furthermore, older workers might have worse re-employment possibilities due to age discrimination. Supporting this, Figures 1 and 2 show that, when compared to the non-displaced, the employment progression of the displaced in the downstream sample is substantially worse than the upstream sample. As a form of age discrimination, employers may perceive older workers as less adaptable to new technologies and other new work characteristics (McGregor and Gray 2002). Employers may also be reluctant to spend time to train new workers who will have shorter remaining careers. In the upstream sample (Figure 1), the employment probability of the displaced decreases greatly at first,

but returns nearly to the level of the non-displaced by the tenth year since the displacement.

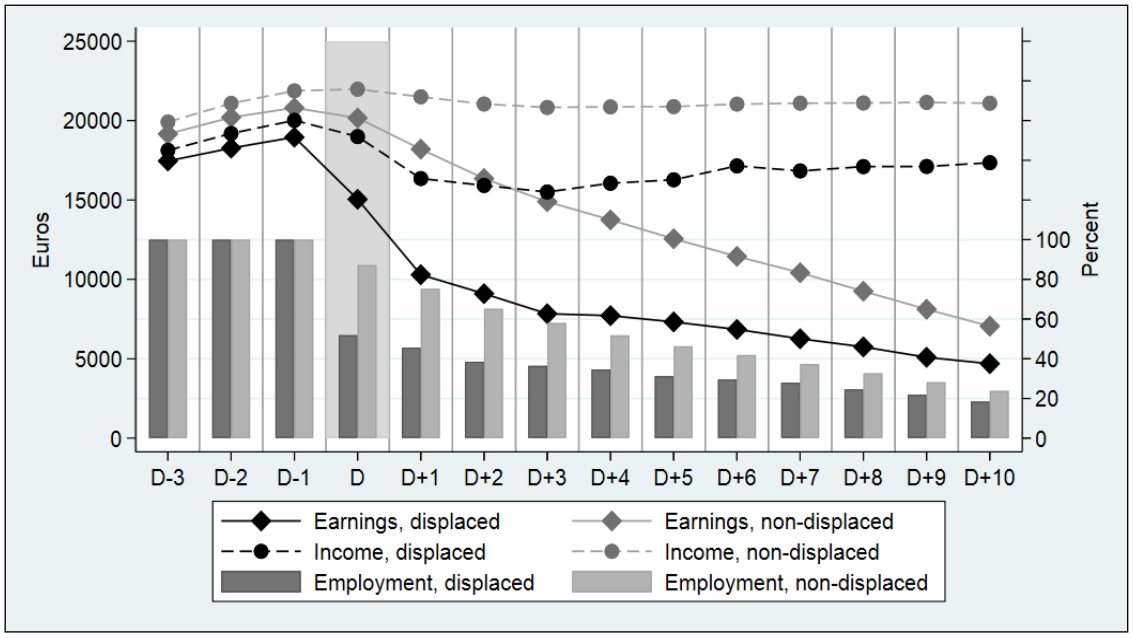
The income variable in Figures 1 to 4 represents the sum of all taxable income (e.g. earnings, unemployment benefits, pensions and various other forms of social security) and basic social assistance. The relationship between displacement and income depicts the financial losses to the displaced when public transfers are taken into account. Figures 1 and 2 show that the magnitude of income loss related to the displacement is similar to the earnings loss in both absolute and relative terms. This suggests that public transfers substitute the earnings loss only to some extent. Therefore, there is a motive for an increase in family members' transfers and earnings.

FIGURE 1 Labour market outcomes of the displaced and non-displaced; upstream sample



Note: Upstream sample studies the effect of adult child's displacement on parent's earnings. Displacement occurs during year D.

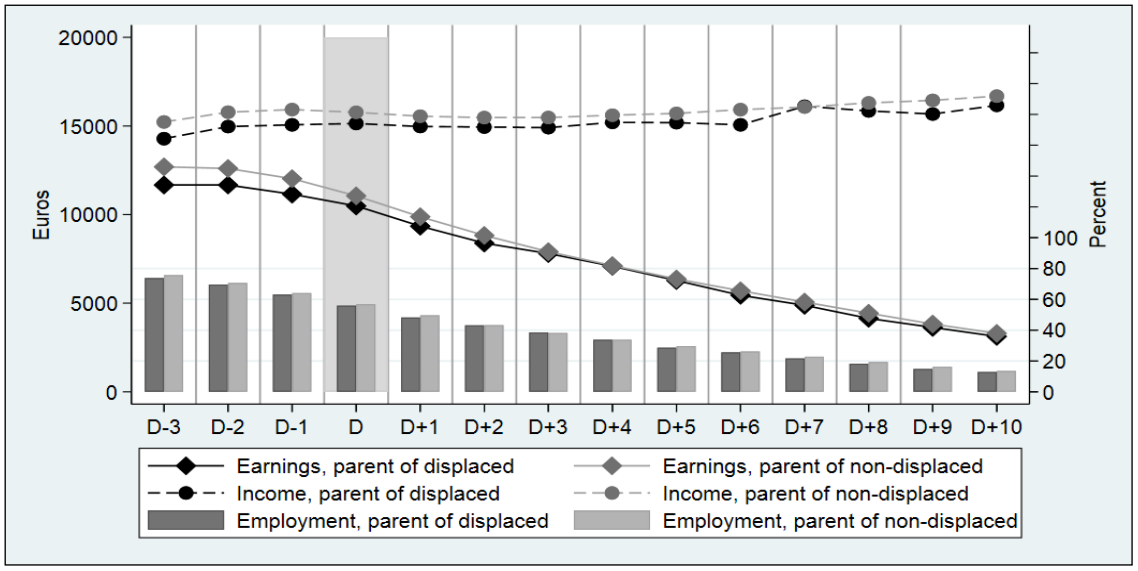
FIGURE 2 Labour market outcomes of the displaced and non-displaced; downstream sample



Note: Downstream sample studies the effect of parent's displacement on adult child's earnings. Displacement occurs during year D.

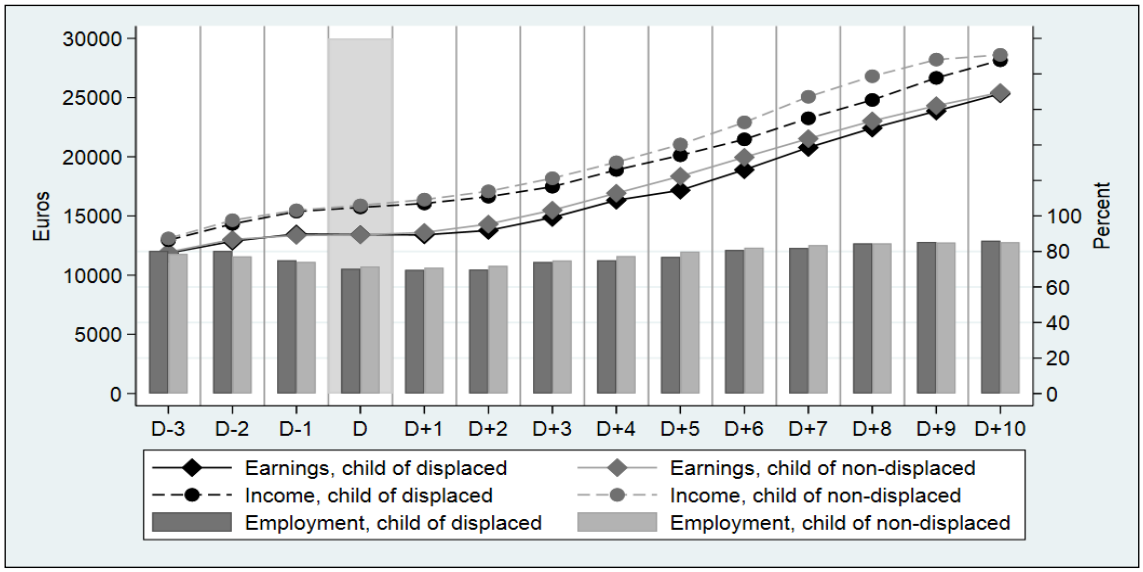
Figures 3 (upstream sample) and 4 (downstream sample) report the average annual earnings, income and employment rate of the family members of the displaced and non-displaced individuals. Figure 3 shows that in the upstream sample, earnings of the parents of the displaced decrease over time in both treatment and control groups, probably due to parents' ageing. However, the earnings difference narrows over time, suggesting that the parents of the displaced may respond to the displacement by increasing their labour supply, relative to the parents of the non-displaced. On the contrary, the adult children of the displaced in the downstream sample (Figure 4) have a smaller earnings increase than the children of the non-displaced, suggesting that they respond to the displacement by working less. In both Figures, the income and employment probability have similar trends as the earnings progression.

FIGURE 3 Labour market outcomes of the family members of the displaced and non-displaced; upstream sample



Note: Upstream sample studies the effect of adult child's displacement on parent's earnings. Displacement occurs during year D.

FIGURE 4 Labour market outcomes of the family members of the displaced and non-displaced; downstream sample



Note: Downstream sample studies the effect of parent's displacement on adult child's earnings. Displacement occurs during year D.

2.3.3 Estimation methods

The data indicated that displacement decreases earnings of the displaced; see Figures 1 to 4. On the contrary, they also showed that the non-displaced and their

family members have higher pre-displacement earnings than the displaced. This suggests that the earnings correlate negatively with the probability of displacement. Therefore, cross-sectional data on earnings between the displaced and non-displaced individuals and their families does not describe the effect of displacement on earnings accurately. However, Figures 1 to 4 show that the difference in earnings between the displaced and non-displaced remains relatively constant across the pre-displacement years D-3 to D-1. This suggests that, in the absence of displacement, the difference in earnings between these groups would remain constant for years D to D+10 as well. This is the *parallel trends* assumption. Assuming parallel trends, the displacement effect can be estimated by subtracting the pre-displacement earnings difference between the groups from their post-displacement earnings difference. This is known as the *difference-in-differences* method, or DID for short.

We use the DID method in two different ways to estimate the EAWE. We estimate the yearly displacement effects for all outcome years (D to D+10) with a fixed effects estimator and an average displacement effect across all outcome years with a standard DID estimator. Both the yearly and the average approach have their advantages. The average approach condenses the effect to a single estimate, which is easier to interpret. On the contrary, observing multiple outcome years uncovers more information about the time-varying dynamics of the effect. For example, family member's labour supply reaction may be delayed due to job search frictions or inadequate information on the displaced individual's future earnings prospects. Furthermore, (Stephens 2002) shows that spouse's labour supply response occurs to some extent already before the displacement period. If the displaced learns of the displacement beforehand, or assumes that the displacement probability has increased, family members can react to it in advance.

We follow Jacobson et al. (1993) in estimating the yearly effects. The model is outlined in equation 1.

$$y_{it} = \sum_{k=-2}^{10} \alpha_{D+k} {d^{D+k}}_{it} + \sum_{k=-2}^{10} T_{wt(D+k)} + \beta X_{it} + \epsilon_{it} \quad (1)$$

The dependent variable y_{it} depicts the individual's or their family member's earnings at year t. $T_{wt(D+k)}$ are coefficients for dummy variables that take value 1 if year t occurs k years after the year D and the panel wave is w. These variables control for the effect of unique year-wave combinations on earnings. Variables of interest, ${d^k}_{it}$, are a set of dummy variables that take value 1 if the individual was displaced k years before current period t. The model is estimated with a fixed-effects within estimation, that is, the observation-specific mean (over time) is subtracted from each variable value. This negates the effect of time-invariant differences between the groups. Therefore, assuming that the general time trends outside the displacement effect are similar across the displaced and non-displaced groups (parallel trends assumption), coefficient α_{D+k} indicates the effect of displacement on earnings in the k:th year since pre-displacement year D.

The earnings effect is measured as the difference to year $k = -3$ earnings, so the coefficients of the reference year D-3 are excluded. X_{it} are a set of household-specific, time-variant control variables detailed below.

To estimate average effects, we average the variable values from periods D to D+10 to a single post-displacement value, and use the variable values from year D-3 as the pre-displacement value. In all estimations, we use standard errors that are clustered at the individual level to account for individual specific correlation of error terms across different time periods and sample waves.

The estimation method negates any omitted variable bias from time-*invariant* differences between the groups. Moreover, we use some time-*variant* observable characteristics as control variables to mitigate potential omitted variable bias from time-*variant* differences between the groups that could violate the parallel trends assumption. Appendix Tables A1 and A2 report the mean values of the outcome and control variables for the upstream and downstream samples, respectively. We control both the individual's and the family member's age with yearly dummy age variables. The control variables also include average earnings and unemployment rates for 1) the region[**] in which each of them lives at year D-3 and 2) for the line of education (in two-digit classification) they have at year D-3. I use the region and education variables from year D-3 to avoid selection bias, for example, due to individuals migrating or beginning a new line of education after the displacement. As also indicated by Figures 1 and 2, Tables A1 and A2 show that the earnings of the displaced are lower before the displacement than earnings of the non-displaced, suggesting that low-earnings industries are more vulnerable to economic shocks. Pre-displacement earnings of the parents of the displaced are somewhat lower before the displacement year than earnings of the parents of the non-displaced (Table A1), whereas there is no statistically significant difference between the adult children of the displaced and non-displaced (Table A2).

2.4 Results

2.4.1 Main results

We estimate the effect of an individual's displacement on their own and their family member's earnings (EAWE). Table 1 presents the average effect results in the upstream and downstream samples. To ease the interpretation, the Table also reports the relative earnings effects, which are calculated as the ratio of EAWE and the average control group earnings. Figures 5 and 6 present the yearly effects for the upstream and downstream samples, respectively.

[**] Regions are defined by Statistics Finland. A region consists of one or more adjacent municipalities that have substantial between-municipality commuting. Regions cannot overlap.

Table 1 shows that displacement considerably decreases annual earnings of the displaced. The older displaced workers (downstream sample) face greater losses than the younger (upstream sample), with the annual average effects of 27.5% and 10.8%, respectively. The difference supports the descriptive evidence (see Figures 1 and 2), and is in line with previous literature (Couch and Placzek 2010; Hijzen and Wright 2013). The earnings loss is constant over time for the younger displaced (Figure 5), whereas it decreases over time for the older displaced (Figure 6). The decrease is partially explained by retirements, which reduce earnings also in the control group.

Displacement increases the earnings of the displaced individual's parent by €348, which is 5.2% of the parent's total earnings. The average effect is not statistically significant. However, Figure 5 indicates that the yearly effects are statistically significant during the third, fourth and fifth post-displacement years in the upstream sample; displacement prompts a delayed increase in the parent's earnings that eventually diminishes.

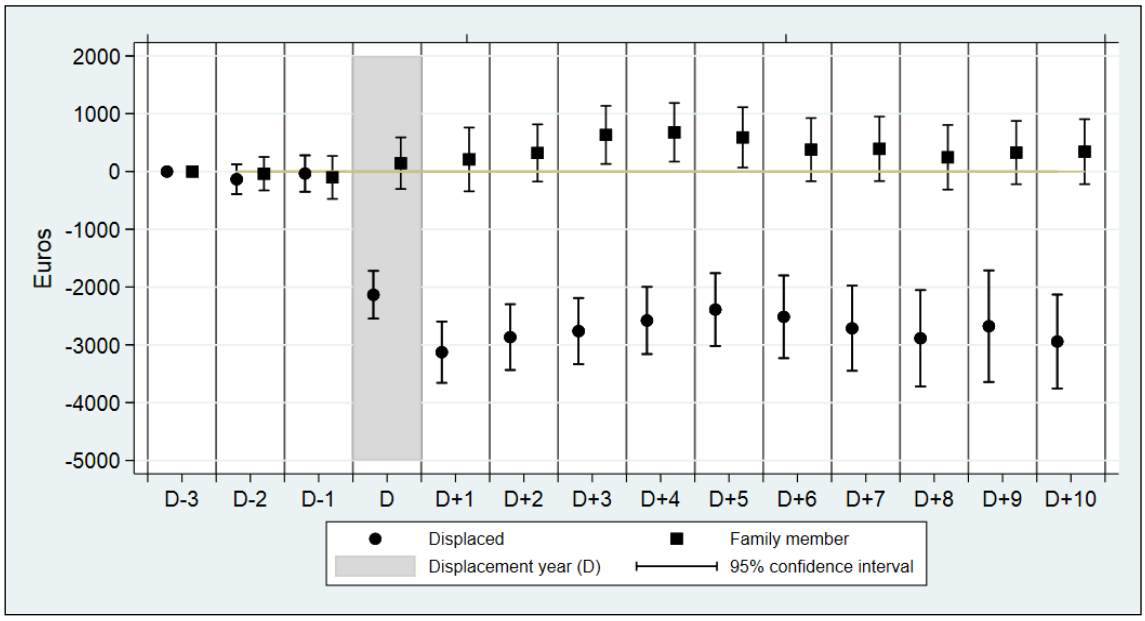
Panel B in Table 1 and Figure 6 indicate that displacement does not increase the earnings of the adult child of the displaced. The average effect is negative and statistically insignificant at €-327 or -2.0% per year. Since adult children provide less transfers to their parents than vice versa (Albertini 2007), the effect of displacement on transfers and earnings is probably lesser as well. Moreover, the displacement may decrease the family member's earnings through transmission of social norms and expectations regarding employment and career progression (Ekhaugen 2009; Senik 2008). These effects may be greater for children, who may be more adaptable to new influences. They may cancel out some of the transfer-related increase in earnings.

TABLE 1 Average effect of displacement on earnings; upstream and downstream samples

	Displaced		Family member		
Sample	Euros	Relative	Euros	Relative	N
A: Upstream sample	-2 615***	-10.79%	348	+5.21%	89 600
	(263)		(218)		
B: Downstream sample	-3 374***	-27.46%	-327	-1.99%	59 449
	(318)		(393)		

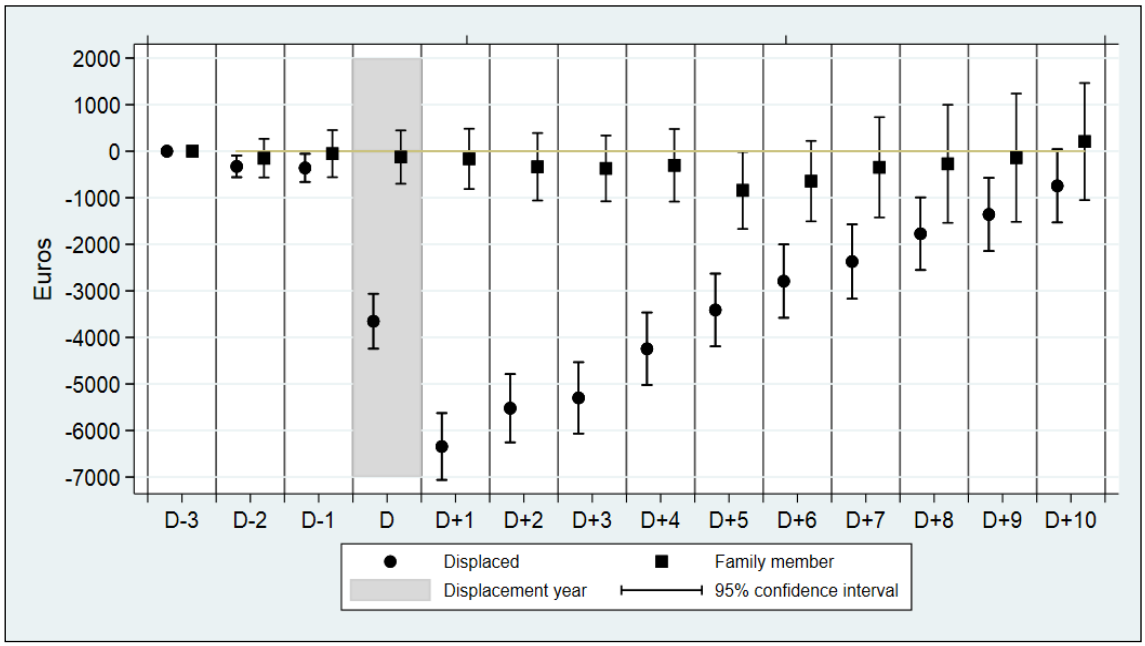
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings. Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings. Average annual effect for post-displacement years D to D+10 is estimated in comparison to the reference year D-3. The relative earnings effect is calculated as a ratio to average control group earnings. Individual-clustered standard errors in parentheses. P-values: * 10%, ** 5%, *** 1%.

FIGURE 5 Yearly effect of displacement on earnings; upstream sample



Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings. Displacement occurs during year D.

FIGURE 6 Yearly effect of displacement on earnings; the downstream sample



Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings. Displacement occurs during year D.

2.4.2 Results by family composition

In this section, we study how the differences in age, gender and existence of other family members affect EAWE. These factors could influence the displaced individual's need to receive transfers and the family member's capability to provide them and increase their earnings. They could also be telling of gender norms. The average estimates are presented in Tables 2 and 3 for the upstream and downstream samples, respectively. The yearly estimates are presented in Figures A1 to A11.

Panels A and B in Table 2 show that while the displacement of an individual who is younger than average (27 years or younger at the pre-displacement year D-3) does not affect the parent's earnings, an older individual's displacement increases them by 17%. There are at least two possible explanations for the age-related results difference. First, older workers have more advanced career progression and higher paying jobs, which makes their earnings losses upon job displacement greater in both absolute (€3,500 v. €1,800) and relative terms (13.0% v. 8.3%). Greater earnings losses imply a greater need for net transfers from family members. Second, older children's parents are usually also older than younger children's parents. Older parents are in a better position to control their labour supply since they often have the opportunity to choose to either retire or to continue working. The explicit comparison by parents' age supports this hypothesis. Panels C and D in Table 2 show that displacement causes a significant increase in the older parent's (older than 54 years) earnings but it has no effect on the younger parent's earnings. Figures A1 and A2 support the findings in Table 2. Furthermore, they show that EAWE is greatest during the first few years after the displacement year.

Results in panels E and F of Table 2 indicate that EAWE is statistically significant (at the 10% level) only if the displaced does not have siblings. The difference is also evident in Figure A3. Siblings may provide transfers to the displaced, which reduces the need for the increase in parental transfers and earnings. Furthermore, if the displaced who has siblings used to provide transfers to their parent before the displacement, the siblings could replace some of the transfer flow after the displacement. Moreover, parents may be more inclined to support an only child because the survival of their family bloodline is dependent on the child (Chu 1991). Regarding the survival of family bloodline, panels G and H of table 2 show that having children is not connected to greater earnings increase from the parent. In contrast, figure A4 indicates that the earnings increase may actually be greater for parents whose displaced children do not have children of their own. Having children may indicate that the displaced is more likely to be in a stable relationship and can thus benefit more from his spouse's financial support, compared to the childless displaced.

Panels I to L present the results for different gender specifications among the displaced and their family members. Displaced sons have far greater earnings losses, but their parents are not more likely to increase their earnings than parents of the displaced daughters. However, the earnings increase is far more likely for the fathers of the displaced than for mothers of the displaced. Fathers increase

their earnings by 11.3 % following their children's displacement. This could indicate of traditional gender norms, that assign father as the provider for the family. The norm may persists even when the child is adult. On the other hand, fathers have higher earnings than mothers, on average. In the next chapter we show that greater income is related to greater EAWE and explore what may cause it.

Table 3 and Figures A7 to A11 present EAWE estimates for different family compositions in the downstream sample. Panels A to D in Table 3 and Figures A7 and A8 indicate that neither the age of the displaced or the age of their adult child affects EAWE. If the child has siblings, the child's earnings decrease by 4.8% per year (panel F in Table 3). The effect is statistically significant at the 10% level. Similar to the upstream sample, the need for transfers from the child is probably lower if the child has siblings, who can also provide transfers to the displaced. Furthermore, parents may be less inclined to help a child so as to not show favouritism. For these reason, the displacement effects that decrease the adult child's earnings (related to perceptions on work, for example) are more likely to dominate the transfer-induced earnings-increase. Parent's or adult child's gender does not seem to affect the EAWE in the downstream sample.

TABLE 2 Average effect of displacement on earnings by family type; upstream sample

	Displaced		Family member		
Family type	Euros	Relative	Euros	Relative	N
A: Displaced ≤ 27 years	-1 790***	-8.32%	35	+0.38%	44 548
	(333)		(322)		
B: Displaced > 27 years	-3 494***	-12.98%	710**	+16.79%	45 052
	(402)		(293)		
C: Family member ≤ 54 years	-2 202***	-9.85%	100	+0.98%	43 036
	(334)		(319)		
D: Family member > 54 years	-3 039***	-11.71%	662**	+19.09%	46 564
	(397)		(296)		
E: Displaced does not have siblings	-3 121***	-12.29%	673*	+8.63%	37 208
	(411)		(351)		
F: Displaced has siblings	-2 261***	-9.66%	114	+1.94%	52 392
	(340)		(277)		
G: Displaced has child	-2832***	-11.31%	202	+4.42%	31008
	(444)		(331)		
H: Displaced does not have child	-2482***	-10.43%	424	+5.43%	58592
	(324)		(285)		
I: Displaced is son	-3544***	-12.06%	275	+4.30%	46517
	(409)		(287)		
J: Displaced is daughter	-1829***	-9.79%	426	+6.08%	43083
	(299)		(334)		
K: Family member is father	-2817***	-11.58%	969**	+11.29%	30586
	(446)		(494)		
L: Family member is mother	-2511***	-10.38%	91	+1.59%	59014
	(322)		(209)		

Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings. Average annual effect for post-displacement years D to D+10 is estimated in comparison to the reference year D-3. The relative earnings effect is calculated by dividing the euro effect by the average control group earnings. Individual-clustered standard errors in parentheses. P-values: * 10%, ** 5%, *** 1%.

TABLE 3 Average effect of displacement on earnings by family composition; downstream sample

	Displaced		Family member		
Family type	Euros	Relative	Euros	Relative	N
A: Displaced ≤ 50 years	-4 650***	-28.51%	-398	-2.76%	29 354
	(455)		(412)		
B: Displaced > 50 years	-2 335***	-27.95%	-234	-1.27%	30 095
	(429)		(641)		
C: Family member ≤ 25 years	-3 960***	-25.44%	-596	-4.33%	27 430
	(486)		(444)		
D: Family member > 26 years	-2 940***	-31.02%	-117	-0.62%	32 019
	(415)		(617)		
E: Family member does not have siblings	-3 522***	-25.6%	420	+2.68%	21 130
	(458)		(674)		
F: Family member has siblings	-3 298***	-28.74%	-810*	-4.8%	38 319
	(425)		(481)		
G: Displaced is mother	-2927***	-30.01%	-640	-3.80%	32718
	(252)		(458)		
H: Displaced is father	-4349***	-28.27%	136	+0.85%	26730
	(675)		(696)		
I: Family member is daughter	-3380***	-27.37%	-56	-0.43%	34747
	(450)		(371)		
J: Family member is son	-3405***	-27.91%	-854	-4.03%	24701
	(437)		(761)		

Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings. Average annual effect for post-displacement D to D+10 years is estimated in comparison to the reference year D-3. The relative earnings effect is calculated by dividing the euro effect by the average control group earnings. Individual-clustered standard errors in parentheses. P-values: * 10%, ** 5%, *** 1%.

2.4.3 Results by income group

In this section, we study how EAWE depends on the displaced individual's and their family member's income levels at the pre-displacement year (D-3). Income dependency is interesting, since income can affect both the need to receive and the ability to provide transfers. The observations are categorized into low income (below median) and high income (above median) groups. The average results are reported in Tables 4 and 5 for upstream and downstream samples, respectively. The yearly results are presented in Figures A12 to A15.

Brandt and Deindl (2013) show that the lower the transfer receiver's income, the greater the transfers from the transfer donor to the receiver. In essence, the poorer need more financial support. The displacement of a low-income individual reduces her income to an even lower level. It could therefore invoke a greater earnings response from the family member. On the contrary, panels A

and B in Tables 4 and 5 show that low-income displaced experience lowers earnings losses, which mediates the need for transfers from family members. Various effects may balance each other out, as results in panels A and B in Table 4 and Figure A12 indicate that EAWE is not strongly dependent on the income of the displaced in the upstream sample.

Brandt and Deindl (2013) also show that the greater the transfer donor's income, the greater the transfers from the donor to the receiver. High-income donors have more wealth to transfer. However, even if high-income family members are in a better position to increase their transfers to the displaced, they are also in a better position to reduce their own consumption, which reduces the need to increase their earnings to fund the transfers. On the contrary, they may also have more options at the job market to move to better paying positions or companies to increase their earnings. They may also more often work in professions or choose paths where they have greater freedom to choose how much they work, such as entrepreneurship. Moreover, high-income parents have higher wages and therefore a greater monetary return to an increase in their work hours. This might also more often manifest as delayed retirement. Results indicate that an increase in parents' income increases EAWE. Results in Table 5 (panels C and D) and Figure A13 show that displacement increases the earnings of a high-income parent by €947 euros or 9.38% per year. Displacement does not affect the earnings of a low-income parent.

Similar to other downstream sample results, panels A to D in Table 5 and Figures A14 and A15 indicate that the parent's displacement does not increase the adult child's earnings, regardless of either the parent's or the child's income level.

TABLE 4 Average effect of displacement on earnings by income group; upstream sample

	Displaced		Family member		
Income group	Euros	Relative	Euros	Relative	N
A: Low-income displaced	-1 843***	-10.88%	187	+2.58%	36 847
	(323)		(278)		
B: High-income displaced	-3 160***	-10.78%	475	+7.55%	52 753
	(390)		(320)		
C: Low-income family member	-2 653***	-11.69%	-46	-1.34%	45 837
	(319)		(213)		
D: High-income family member	-2 589***	-10.02%	947***	+9.38%	43 763
	(428)		(350)		

Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings. Average annual effect for post-displacement D to D+10 years is estimated in comparison to the reference year D-3. The relative earnings effect is calculated by dividing the euro effect by the average ten-year control group earnings. Individual-clustered standard errors in parentheses. P-values: * 10%, ** 5%, *** 1%.

TABLE 5 Average effect of displacement on earnings by income group; downstream sample

	Displaced		Family member		
Income group	Euros	Relative	Euros	Relative	N
A: Low-income displaced	-2 908***	-34.82%	-377	-2.39%	29 989
	(232)		(432)		
B: High-income displaced	-4 548***	-27.95%	-165	-0.96%	29 460
	(660)		(727)		
C: Low-income family member	-3 563***	-24.76%	344	+3.03%	18 657
	(563)		(565)		
D: High-income family member	-3 293***	-29.08%	-523	-2.79%	40 792
	(386)		(492)		

Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings. Average annual effect for post-displacement D to D+10 years is estimated in comparison to the reference year D-3. The relative earnings effect is calculated by dividing the euro effect by the average ten-year control group earnings. Individual-clustered standard errors in parentheses. P-values: * 10%, ** 5%, *** 1%.

Throughout this study, the sample is limited to the displaced who do not live in the same municipality or work in the same industry as their family members do before the displacement year D. In comparison, Figures A16 (upstream sample) and A17 (downstream sample) show the yearly estimation results also for families where the displaced live in the same municipality with their family members before the displacement and/or work in the same industry. Figure A16 shows that an adult child's displacement decreases parent's earnings for this type of an alternative upstream sample. Furthermore, Figure A17 shows that the negative effect of parent's displacement on adult child's earnings is more robust in the alternative downstream sample. The fact that the effects on family member's earnings are more negative suggests that the family members from the same region or industry are often exposed to the same labour market shocks that cause the individual's displacement. These *parallel shocks* violate the DID model assumptions, which highlights the importance of the sample limitations we use for the other results of this study.

In figurer A18 and A19 we explore further whether the family members working in the same municipality or industry contribute more to the discrepancy. For these figure, the samples include displaced who either A) live in the same municipality but work in different industry than their family members or B) live in the different municipality but work in same industry as their family members. Results indicate that the EAWE estimates are more negative among samples in which the displaced and their family members live in the same municipality, suggesting that local parallel labor markets shocks have greater negative earnings impacts than parallel industry shocks.

2.5 Discussion

This study, similar to Couch and Placzek (2010) and Hijzen and Wright (2013), shows that job displacement significantly decreases the earnings of the displaced. The effect is persistent, especially for younger displaced workers. Our focus is on how the displacement affects the earnings of the family members of the displaced (Extended Added Worker Effect, EAWE). We estimate EAWE by applying DID methods to Finnish population level data. We use plant closure events to capture the causal displacement effect.

We find that displacement increases the earnings of the parents of the displaced. The finding indicates parent's increased supply of labour, which may be required to provide transfers to the displaced whose financial situation is weakened. The earnings increase is prevalent especially among high-income parents. They may have more flexibility in their labour market choices, in both increasing their work hours and moving to better paying jobs. Furthermore, their monetary return for additional work hours is greater than for low-income parents. Greater income may also explain why fathers' earnings increase more than mothers' earnings when adult child is displaced. Traditional gender norm of provider-father may also contribute to the difference.

We also find strong EAWE for families where the parent is older than average. Older parents may also have greater flexibility in their labour marker choices, as they often have the option to either retire or continue working. Displaced child's age has a similar influence; the displacement of an older child increases the parent's earnings. Displacement causes greater earnings losses for an older child than a younger child, which increases the need for parental transfers and earnings. EAWE is statistically insignificant when the displaced has siblings. Intuitively, siblings can also provide transfers to either the displaced or the parent, which reduces the need for parent's transfers and earnings increase.

We do not find clear evidence that a parent's displacement would increase adult child's earnings. In contrast, downstream EAWE estimates are mostly negative, although statistically insignificant. Negative EAWE estimates could indicate that children adopt social norms and expectations related to their displaced parent's weaker labour market situation, which reduces their labour market ambitions. Parent's unemployment during the child's childhood has been shown to have negative implications for the child's labour market outcomes in adulthood (Ekhaugen 2009; O'Neill and Sweetman 1998; Oreopoulos et al. 2008). Since parents raise the children, the children may be more prone to imitate the parents than vice versa, even after the children have reached adulthood. These imitation effects may dominate any transfer-induced effects on earnings. Furthermore, Albertini et al. (2007) show that adult children make fewer and smaller transfers to their parents than vice versa. Lower EAWE and transfers could also imply that children are less altruistic towards their parents. Laitner (1993) argues that most of the literature on intergenerational altruism concerns one-sided altruism, in which parents draw utility also from their children's

welfare, whereas children only value their own welfare. Even if children do care about their parents, parents might still have greater altruism towards their children. From an evolutionary perspective, it appears sensible that parents are more concerned about the welfare and success of their children than the other way around, since the survival of the biological family line is dependent on the well-being and fitness of one's descendants, not one's ascendants. Parents may also need less transfers than adult children, who often have mortgage or non-adult children of their own to take care of.

Studies indicate that public transfers crowd out private transfers (Schoeni 2002; Brandt and Deindl 2013; Edwards 2015; Pesando 2018; Hiilamo and Niemelä 2010). The crowding out of private transfers probably decreases the need for family member's earnings increase. This may be especially relevant in Finland, which is a Nordic welfare state with a high level of social security transfers. It could be beneficial to extend the EAWE examinations in this paper to other countries with different welfare regimes. Differences in EAWE estimates across countries could be an indicator of the adequacy of a country's social security system.

We study the effects of displacement that occurred during a recession period. Future research on topic could be expanded to cover displacements that occur during economic growth. Workers face lesser earnings losses when displaced during growth period (Korkeamäki and Kyyrä 2014), so the family members' labor market response could also be smaller and harder to detect. Even in this study, we find only modest effects in certain groups. On the other hand, better labor market conditions may give family members more options to increase their labor supply.

Our supplementary findings emphasize the need for purposeful sample selection in this and similar studies. We find that the EAWE estimates are negative both in the upstream and downstream samples if they are estimated for family members who live in the same municipality or work in the same industry as the displaced. This is probably due to parallel labour market shocks. In this study, we aim to estimate the family member earnings response that is independent of these parallel shocks. Therefore, we exclude these observations from our estimations. Regardless, labour market shocks that concern multiple family members and the impact of geographical and professional proximity provide interesting venues for future research.

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APPENDIX

TABLE A1 Descriptive statistics; upstream sample

	Non-displaced	Displaced	P-value
Earnings			
Child	**16 706**	**16 110**	**0.003**
Parent	**12 696**	**11 674**	**0.000**
Control variables			
Education field, average earnings, child	14 921	14 441	0.000
Regional average earnings, child	16 763	16 352	0.000
Education field, average earnings, parent	12 577	12 288	0.001
Regional average earnings, parent	15 353	15 031	0.000
Education field, unemployment rate, child	0.066	0.067	0.188
Regional unemployment rate, child	0.058	0.057	0.235
Education field, unemployment rate, parent	0.076	0.074	0.016
Regional unemployment rate, parent	0.074	0.072	0.047
Age, child	27.728	27.686	0.675
Age, parent	51.911	51.764	0.166
Observations	88 134	1 533	

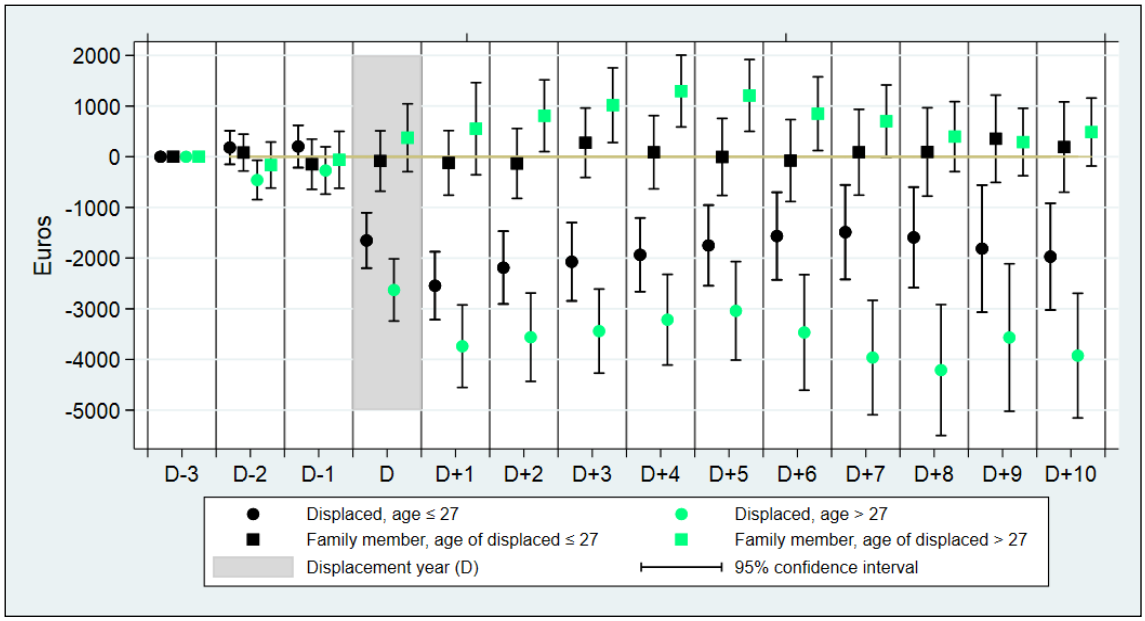
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings. Descriptive values measured three years (D-3) before the displacement year (D).

TABLE A2 Descriptive statistics; downstream sample

	Non-displaced	Displaced	P-value
Earnings			
Parent	**19 162**	**17 459**	**0.000**
Child	**11 947**	**11 866**	**0.767**
Control variables			
Education field, average earnings, parent	12 788	12 296	0,000
Regional average earnings, parent	15 830	15 424	0,000
Education field, average earnings, child	14 236	13 789	0.001
Regional average earnings, child	16 587	16 155	0.000
Education field, unemployment rate, parent	0.074	0.071	0.023
Regional unemployment rate, parent	0.063	0.059	0.007
Education field, unemployment rate, child	0.058	0.056	0.048
Regional unemployment rate, child	0.054	0.051	0.022
Age, parent	50.113	50.163	0.729
Age, child	26.094	26.139	0.731
Observations	58 525	945	

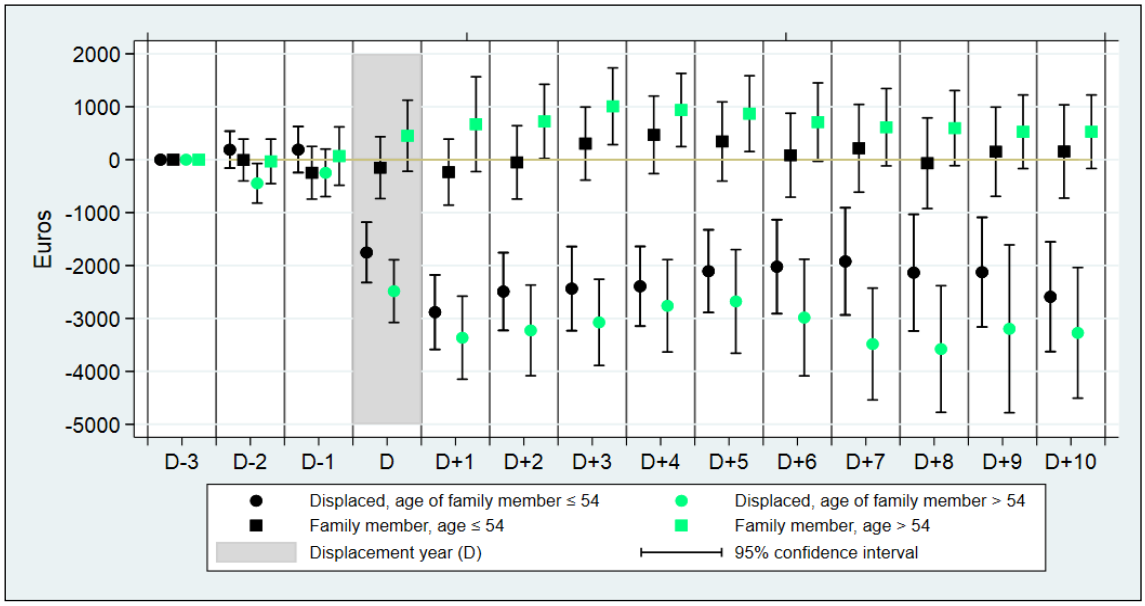
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings. Descriptive values measured three years (D-3) before the displacement year (D).

FIGURE A1 Upstream sample. Displaced who are younger or older than average. Yearly effect of displacement on earnings



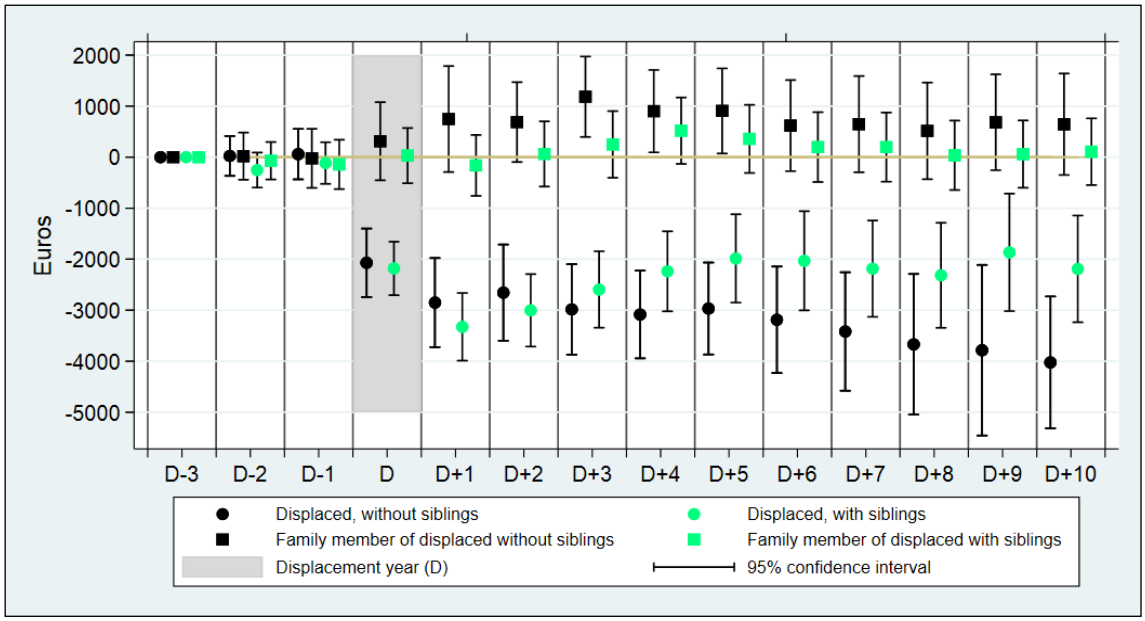
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A2 Upstream sample. Family members who are younger or older than average. Yearly effect of displacement on earnings



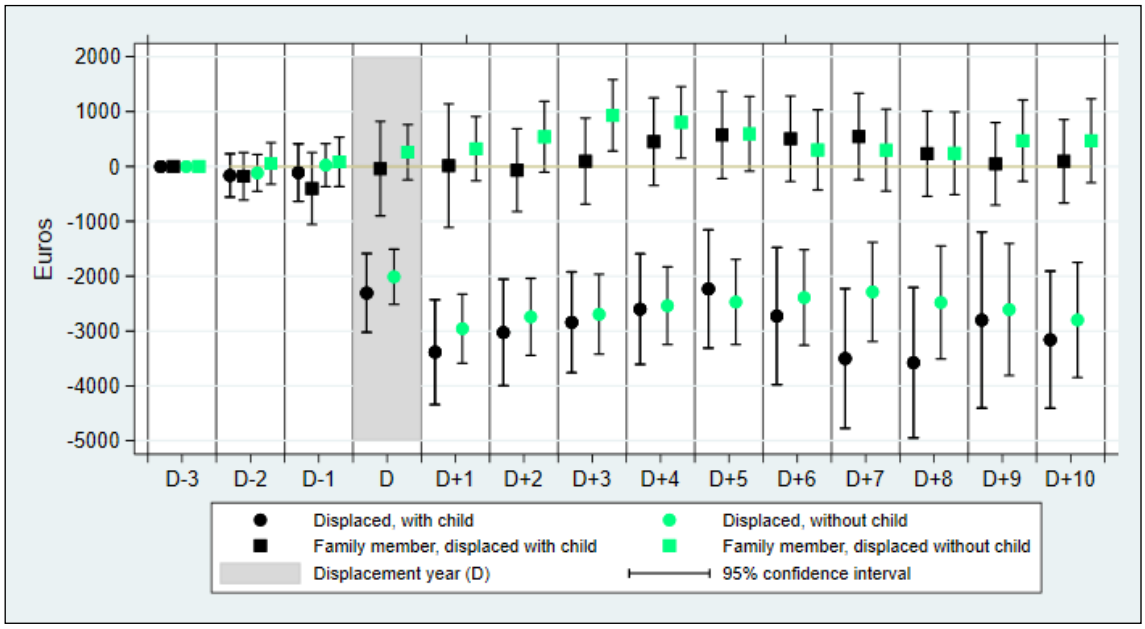
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A3 Upstream sample. Displaced who do or do not have siblings. Yearly effect of displacement on earnings



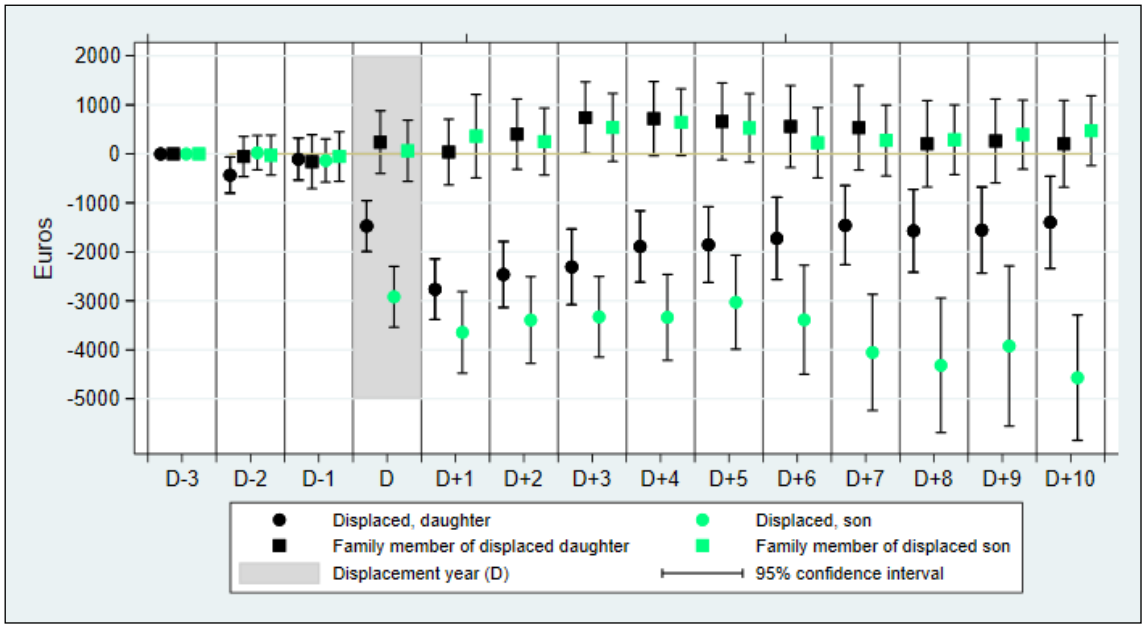
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A4 Upstream sample. Displaced who do or do not have children. Yearly effect of displacement on earnings



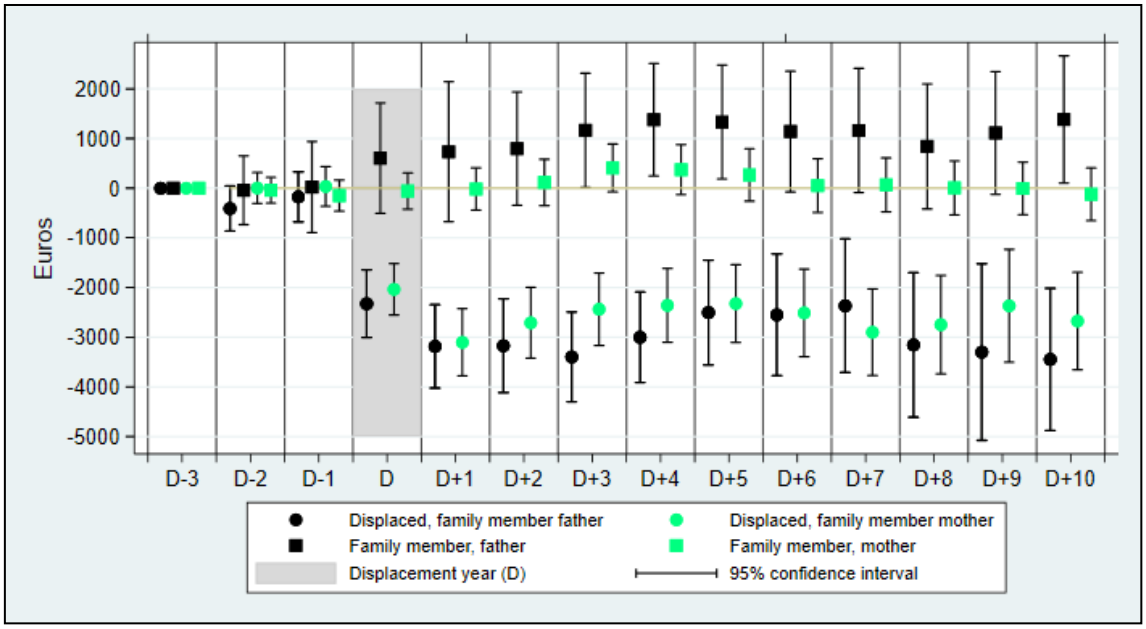
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A5 Upstream sample. Displaced who are either sons or daughters. Yearly effect of displacement on earnings



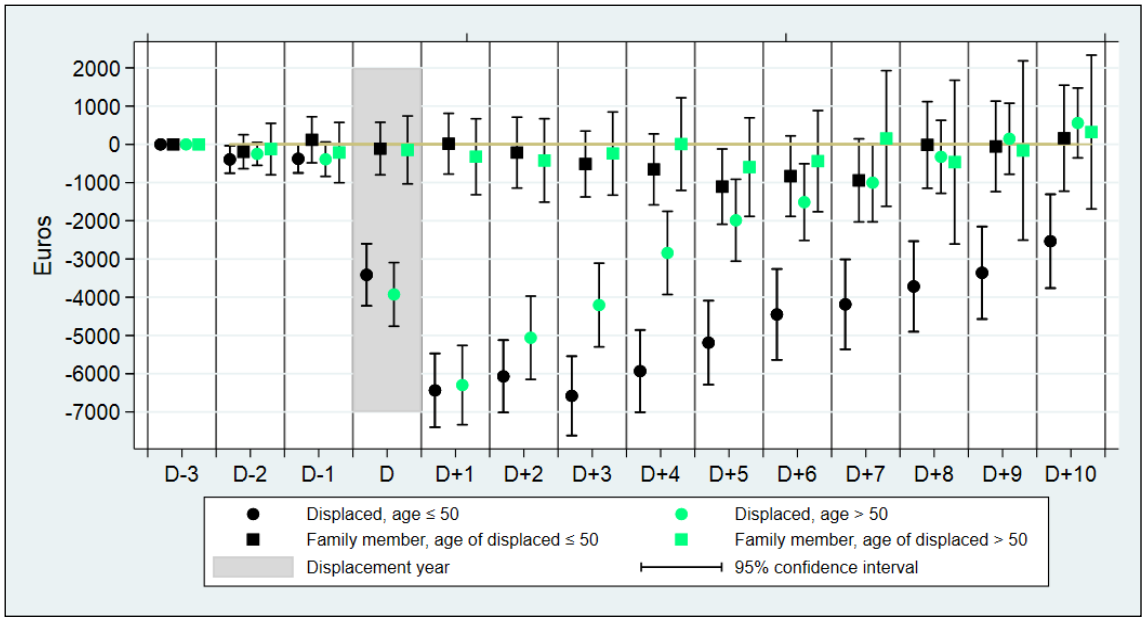
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A6 Upstream sample. Family members who are either mothers of fathers. Yearly effect of displacement on earnings



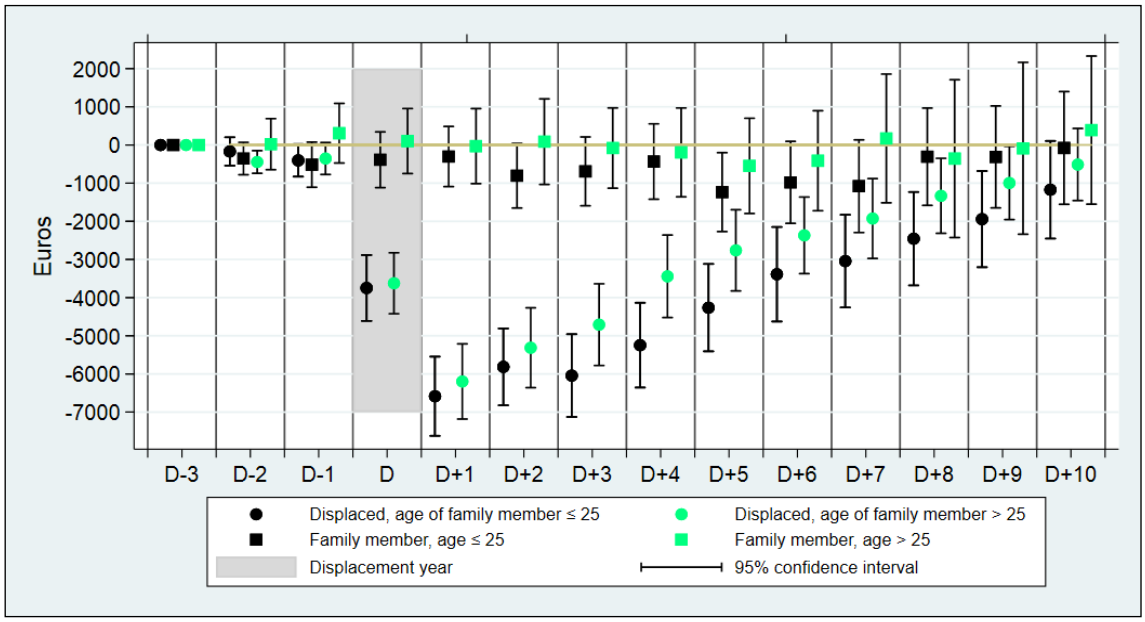
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A7 Downstream sample. Displaced who are younger or older than average. Yearly effect of displacement on earnings



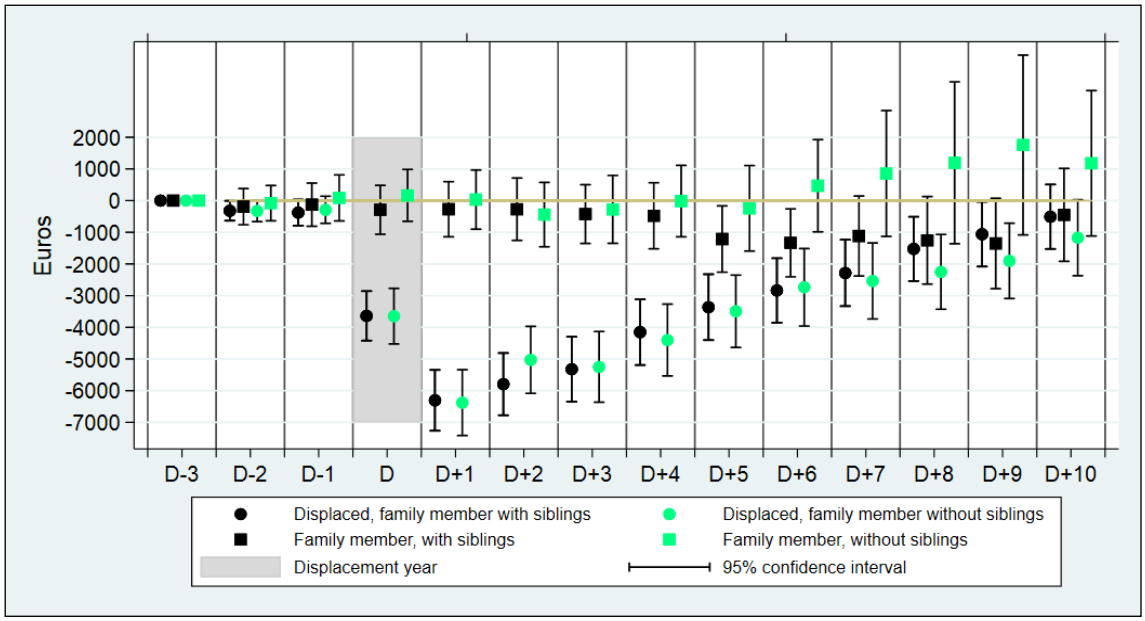
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A8 Downstream sample. Family members who are younger or older than average. Yearly effect of displacement on earnings



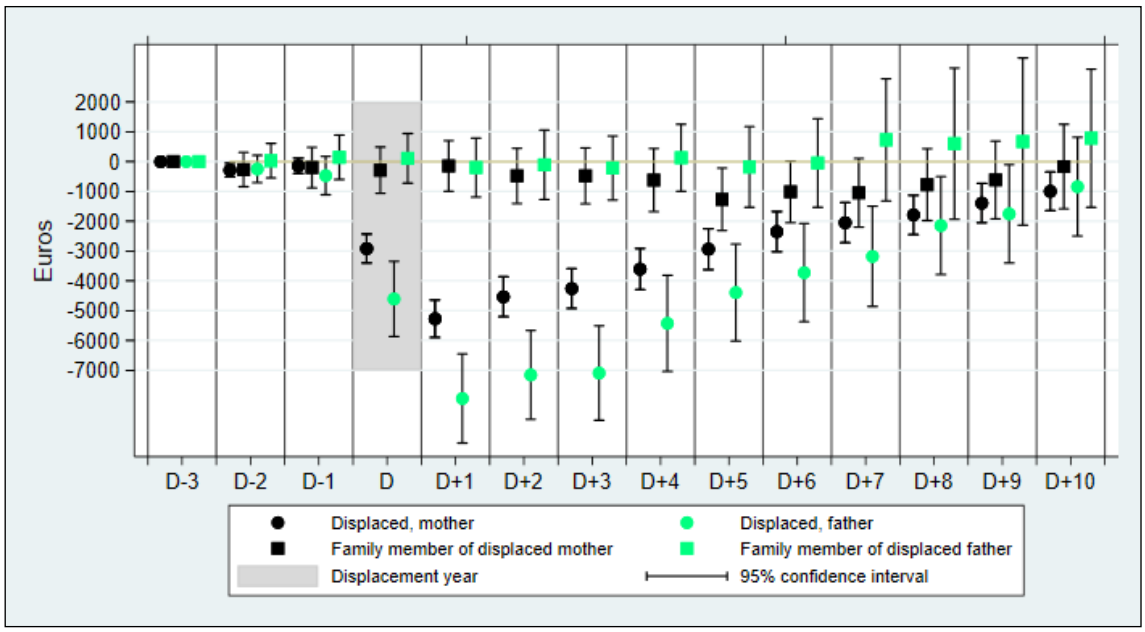
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A9 Downstream sample. Family members who do or do not have siblings. Yearly effect of displacement on earnings



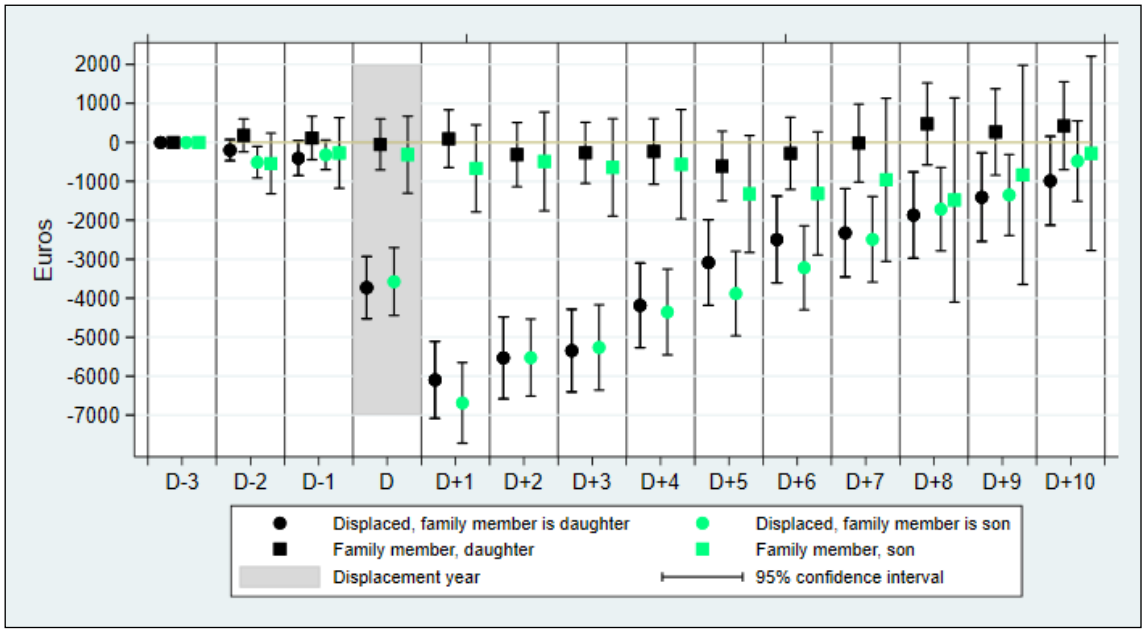
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A10 Downstream sample. Displaced who are either mothers of fathers. Yearly effect of displacement on earnings



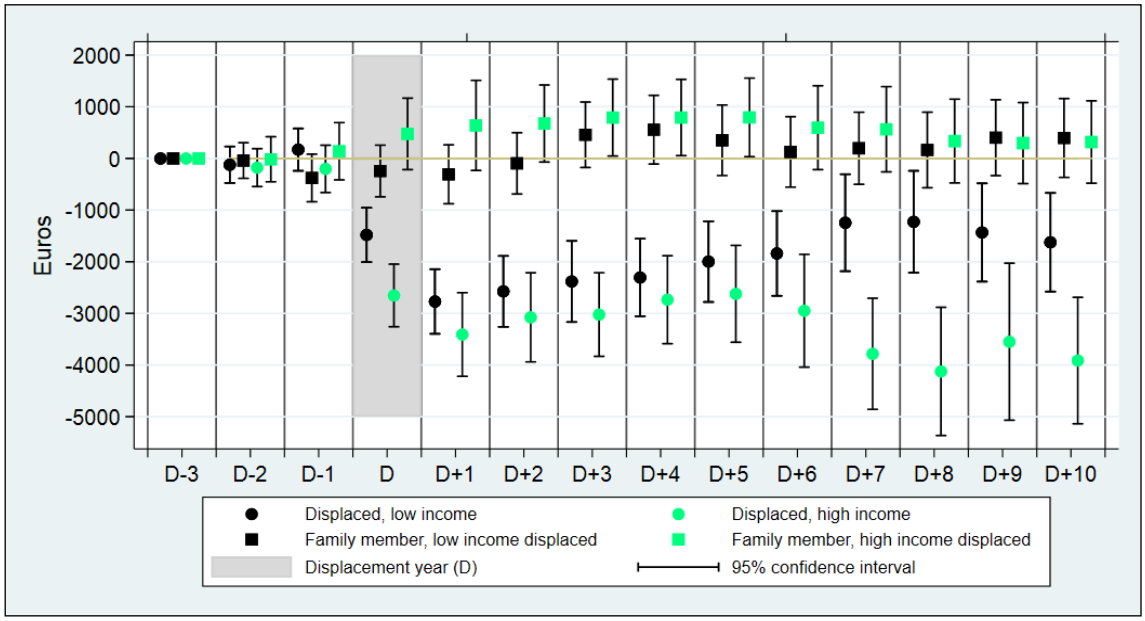
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A11 Downstream sample. Family members who are either sons or daughters. Yearly effect of displacement on earnings



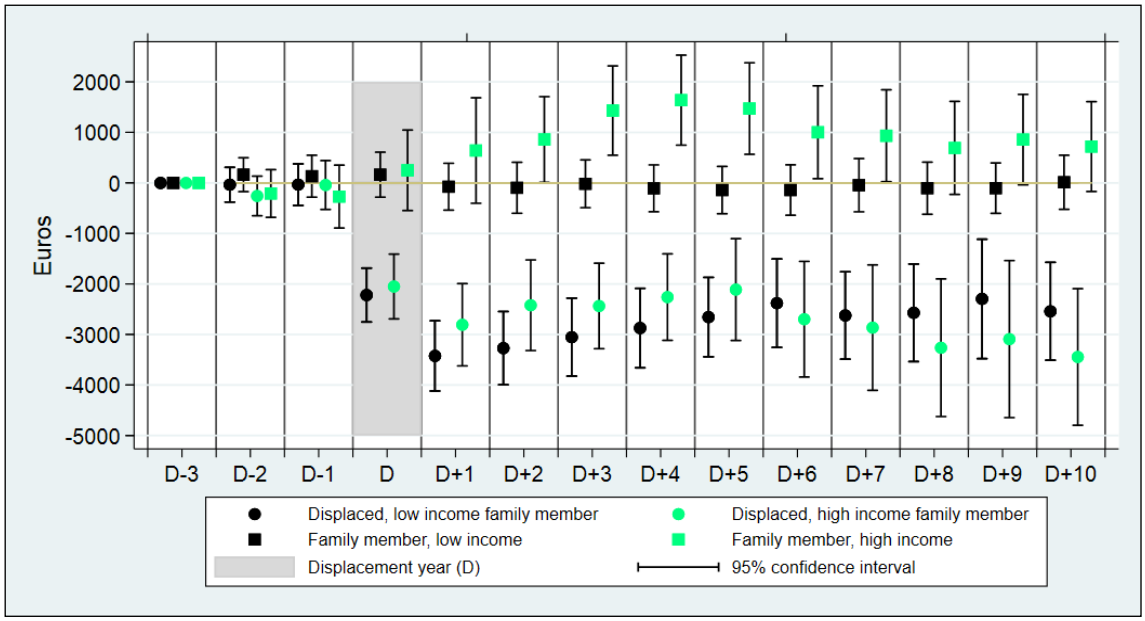
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A12 Upstream sample. Displaced who have low or high income. Yearly effect of displacement on earnings



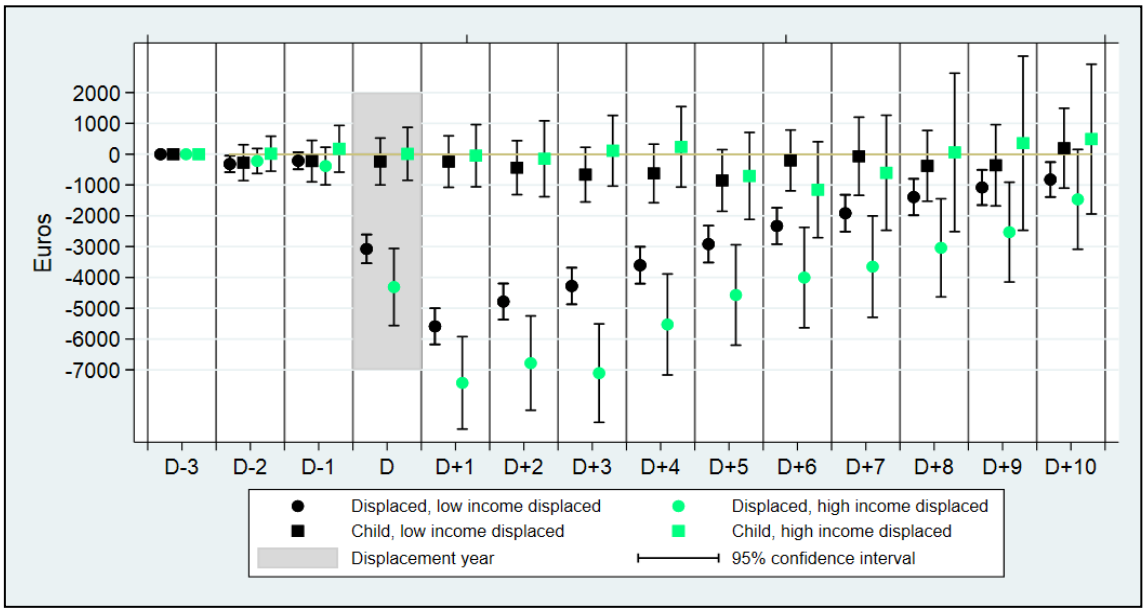
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A13 Upstream sample. Family members who have low or high income. Yearly effect of displacement on earnings



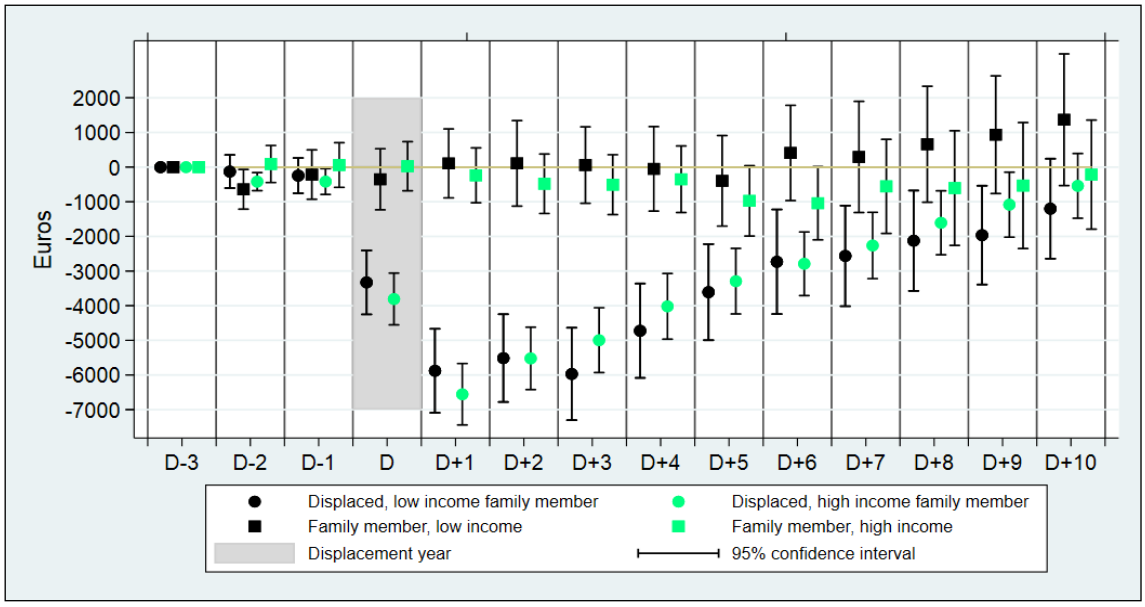
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A14 Downstream sample. Displaced who have low or high income. Yearly effect of displacement on earnings



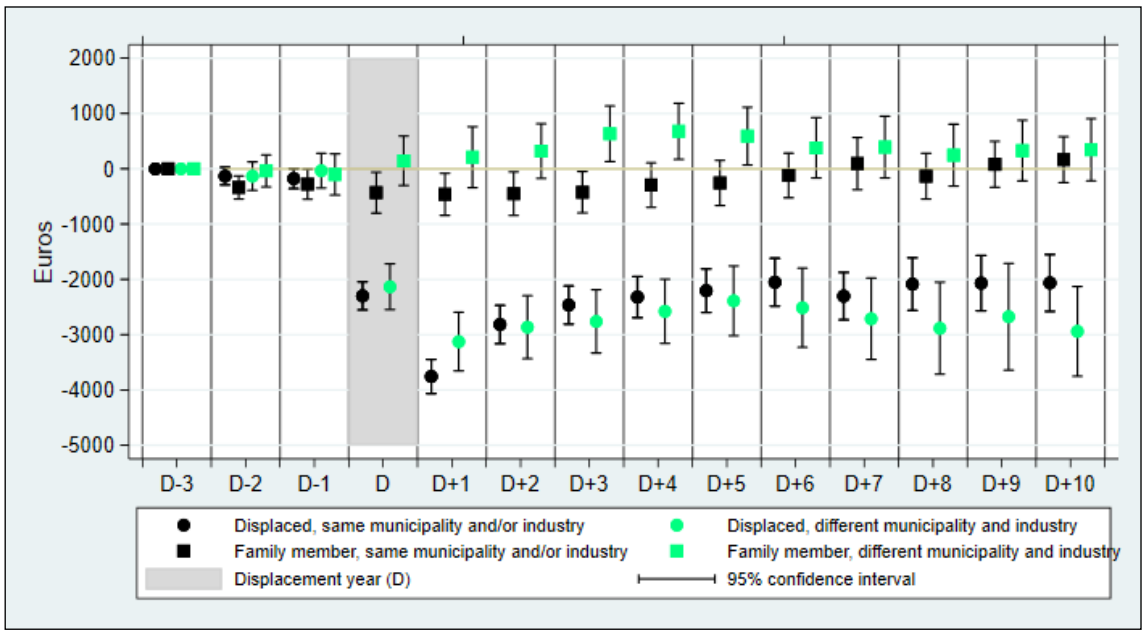
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A15 Downstream sample. Family members who have low or high income. Yearly effect of displacement on earnings



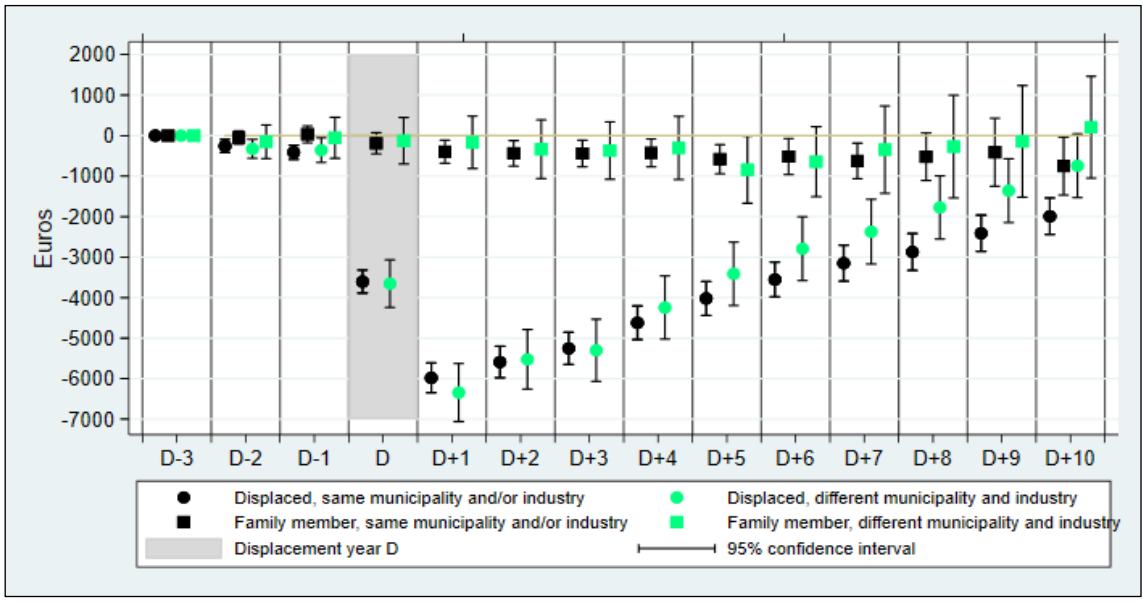
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A16 Upstream sample. Displaced who do or do not live in the same municipality or work in same industry with the family member before year D. Yearly effect of displacement on earnings



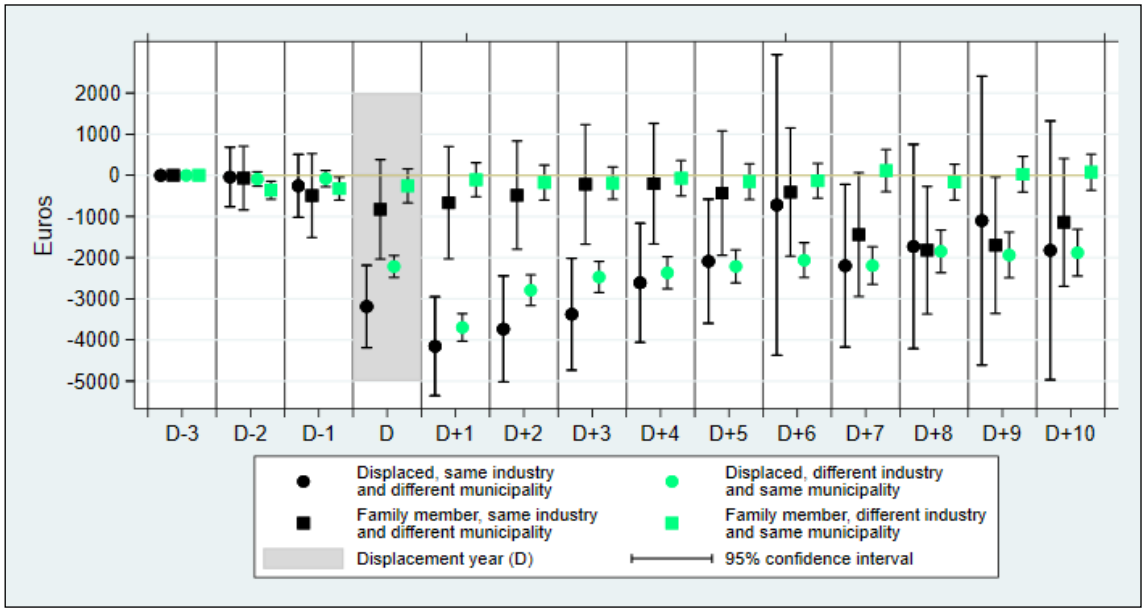
Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A17 Downstream sample. Displaced who do or do not live in same municipality or work in same industry with the family member before year D. Yearly effect of displacement on earnings



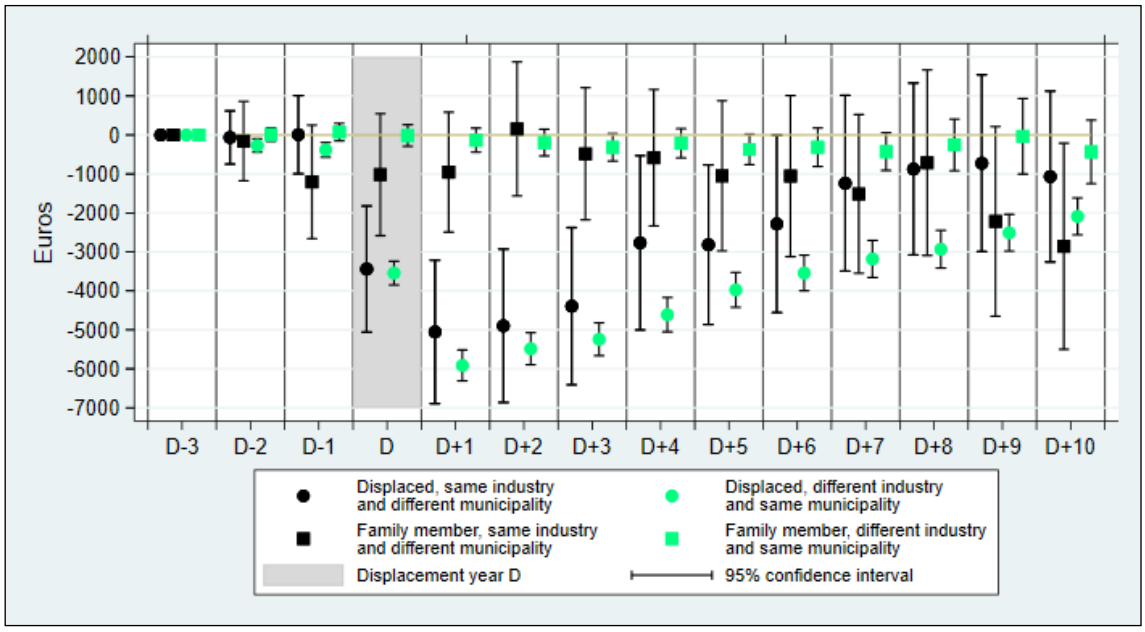
Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

FIGURE A18 Upstream sample. Displaced who work in same industry or live in same municipality with the family member before year D. Yearly effect of displacement on earnings



Note: Upstream sample studies the effect of adult child's displacement on parent's (family member) earnings.

FIGURE A19 Downstream sample. Displaced who work in same industry or live in same municipality with the family member before year D. Yearly effect of displacement on earnings



Note: Downstream sample studies the effect of parent's displacement on adult child's (family member) earnings.

3 WORKPLACE RETENTION OF COWORKING COUPLES: EVIDENCE FROM POPULATION DATA††

Abstract

Although coworking couples represent 7% of all dual-earner couples, they remain an understudied group. In this study, we examine how coworking affects a couple's workplace retention probability. We estimate the effect by using Finnish population data from 1987 to 2012. We control for both the household and the workplace characteristics to reduce the omitted variable bias. Furthermore, we mitigate self-selection bias by examining spouses whose relationship began while working at the same workplace. Results show that coworking is associated with a ten-percentage point increase in the workplace retention probability. This suggests that the benefits of coworking outweigh its disbenefits. The results also indicate that the effect decreases over time, although less for older couples. Workplace size does not have an impact on the magnitude of the effect.

3.1 Introduction

Work and family are two major life domains that are usually separate from each other in both time and spatial dimensions. Work is performed during working

†† I would like to thank the participants at the EEA-ESEM 2017 conference, the Allecon 2016 seminar, the Annual Meeting of the Finnish Economic Association 2016, and the JSBE XXXIII Summer Seminar, as well as Jaakko Pehkonen, Petri Böckerman, Terhi Maczulskij, and Jukka Pirttilä for their helpful comments. I gratefully acknowledge the financial support from the Finnish Cultural Foundation Central Regional Fund (grant 30141756) and the OP Group Research Foundation (grants 201500124 and 201600217).

hours at the workplace, and family life is spent during leisure time outside the workplace. However, family life influences work, and work influences family life. On the one hand, the extensive time requirements or mental and physical strain from one domain may negatively affect the other domain (Greenhaus and Beutell 1985). On the other hand, spousal support in its various forms can be an important source of worker well-being (Greenberger and O'Neil 1993). Intuitively, the effects between the domains are stronger for spouses who work in the same workplace with each other. Previous literature indicates that coworking enhances spousal support and its positive impacts (Ferguson et al. 2016; Halbesleben et al. 2012; Janning 2006). However, it also increases the negative spillover effects between the work and family domains (Halbesleben et al. 2012). Nevertheless, there is a lack of literature on the labour market outcomes of coworking couples.

This study estimates the effect of coworking on workplace retention. Section 2 presents an overview of earlier literature on coworking and spouses' joint time use. Section 3 provides descriptive statistics on coworking.. Throughout the study, we use the Finnish longitudinal employer-employee data (FLEED) from 1987 to 2012. We show that coworking spouses are a substantial portion of all dual-earner couples. Furthermore, we show how the origins of coworking relationships have changed over time. We also compare the prevalence of coworking across age groups and across different-sized workplaces.

Given the prevalence of coworking and its possible importance for well-being, we estimate how it affects spouses' workplace retention. This should indicate if the benefits of coworking outweigh its disbenefits. Section 4 presents the methodology and Section 5 presents the results. Unlike in Section 3, we do not study the workplace retention of spouses in existing relationships where they work in the same workplace since they are likely to be a self-selected group who benefit from coworking the most. Instead, we study the workplace retention of future spouses who work in the same workplace before their relationship began. We estimate if these work-meeting spouses are more likely to remain at their workplaces than spouses who meet while working at different workplaces. The results show that coworking increases workplace retention probability; however, the effect diminishes over time, especially among younger couples.

3.2 Previous literature

Several studies explore the connections between coworking spouses' work and family life. Ferguson et al. (2016) show that work-related spousal support has greater positive effects on family satisfaction, job satisfaction, and spousal relationship tension among coworking and other work-linked[7] couples. Janning

[7] Coworking spouses share the same workplace with each other, whereas work-linked couples share the same occupation or workplace (e.g. Halbesleben et al. 2012).

(2006) suggests that the increased spousal support is due to the better understanding of the spouse's work environment, including its time and demand stressors and the spouse's position at the workplace. Moreover, coworking spouses can perform work tasks together and they are familiar with the same colleagues and work-related friends. Halbesleben et al. (2012) also find evidence of increased spousal support for work-linked couples. Furthermore, they show that work-linked couples have fewer work-family conflicts that are caused by time constraints, but their work-related problems have more negative spillover effects on their family life. Some coworking spouses try to avoid work-related conversations during leisure time (Janning 2006), possibly to reduce the spillovers. Moen and Sweet (2002) find that both the negative and the positive spillover effects are greater for women, suggesting that their work and family domains are more integrated. On the contrary, Sweet and Moen (2004) find no considerable negative spillover effects among academic coworking couples. Moreover, the men in these couples are more work-committed while women experience higher family and marital satisfaction. Rosenfeld and Thomas (2012) report that work-based couples report having similar breakup rates and relationship quality as couples who meet through other avenues.

Despite vast literature on workers' experiences from working jointly with a spouse, the literature on how the joint work time affects the spouses' labour market outcomes is sparse, albeit results from Moen and Sweet's (2002) study suggest that there is no connection between coworking and the self-stated intention to leave the workplace. However, parallel to joint work time, there are numerous studies on joint leisure time. They indicate that spouses have a preference for joint time use, which manifests in their labour market outcomes. For example, spouses are shown to harmonize their work schedules (Hamermesh 2002; Michaud and Vermeulen 2011)(Hamermesh 2002; Michaud and Vermeulen 2011)(Hamermesh 2002; Michaud and Vermeulen 2011)(Hamermesh 2002; Michaud and Vermeulen 2011)(Hamermesh 2002; Michaud and Vermeulen 2011) to allow themselves to spend more leisure time together (Hallberg 2003; Hamermesh 2002; Mansour and McKinnish 2014). Spouses' retirement timings are also often closer to one another than they would be based solely on their age and other characteristics, which is indicative of joint retirement (e.g. Hospido and Zamarro 2014; Stancanelli 2017). Preference for joint time use is a likely motive for joint retirement; Stancanelli and Soest (2015) show that a couple's joint leisure time increases after the wife retires, and Gustman and Steinmeier (2004) find that the wife's retirement timing correlates with the husband's retirement timing only if she values spending time with him.

However, the preference for joint leisure time does not necessarily imply that there is a preference for joint work time among spouses. Hallberg (2003) finds that the overlap in spouses' non-work time is more common for leisure than for domestic work, personal care, or sleep. Similarly, Sullivan (1996) shows that both spouses gain additional enjoyment from joint leisure activities compared to individual leisure, whereas only women gain additional enjoyment from the

jointly performed domestic work. These findings indicate that the joint time spent at work may not be as valued by the spouses as the joint leisure time.

3.3 Prevalence of coworking couples in Finland

In this section, we provide descriptive statistics on coworking that We show how common coworking is, how coworking spouses end up as coworking spouses, and how spouses' age and workplace size are linked to the prevalence of coworking.

There are few studies that have investigated the prevalence of coworking spouses in a workplace. Using Swedish register data, Holm et al. (2018) show that married or cohabiting spouses represent 3.5 of all workers in workplaces in 2012. The year 2000 United States Census data shows that 10% of private sector non-self-employed dual-earner couples worked in the same workplace (Hyatt 2019). Zinovyeva and Tverdostup (2018) show that in Finland, around 6% of spouses in working age couples share a workplace with each other in a pooled sample from 1988 to 2014. When accounting for differences in how the data are constructed, these studies show relatively similar coworking prevalences. Our study is no exception.

Throughout this paper, we use the FLEED (Finnish Longitudinal Employer-Employee Data) data from 1987 to 2012. The data includes demographic and labour market information on Finnish population. Two adults are identified as spouses if they are married and/or if they are opposite sex non-siblings who live in the same household and have an age difference of less than 16 years. Spouses working at the same workplace are a coworking couple. Workplace is a plant or other establishment owned by a firm, and one firm can have multiple workplaces[8].

We show that the number of coworking couples in Finland has fluctuated from 15,000 to 20,000 over the 25-year observation period. Figure 1 shows that the share of coworking couples of all dual-earner couples (couples in which both spouses work) has declined from 13% in 1993 to 7% in 2012. There are two likely explanations for the decrease in coworking in Finland. First, the number of private sector workplaces has increased from 99,000 to 141,000 between 1993 and 2012, while the number of workers has not changed remarkably. The more scattered the workers are to different workplaces, the lower the probability that a worker's workplace is the same as his/her spouse's workplace. Second, online dating may have partially replaced workplace as an origin for relationships[9]. On

[8] Only private sector workplaces are included because of the year-to-year differences in the public sector workplace identification codes and because the separation of distinct workplaces might be less reliable in the public sector. Farmers and entrepreneurs are also excluded from the study.

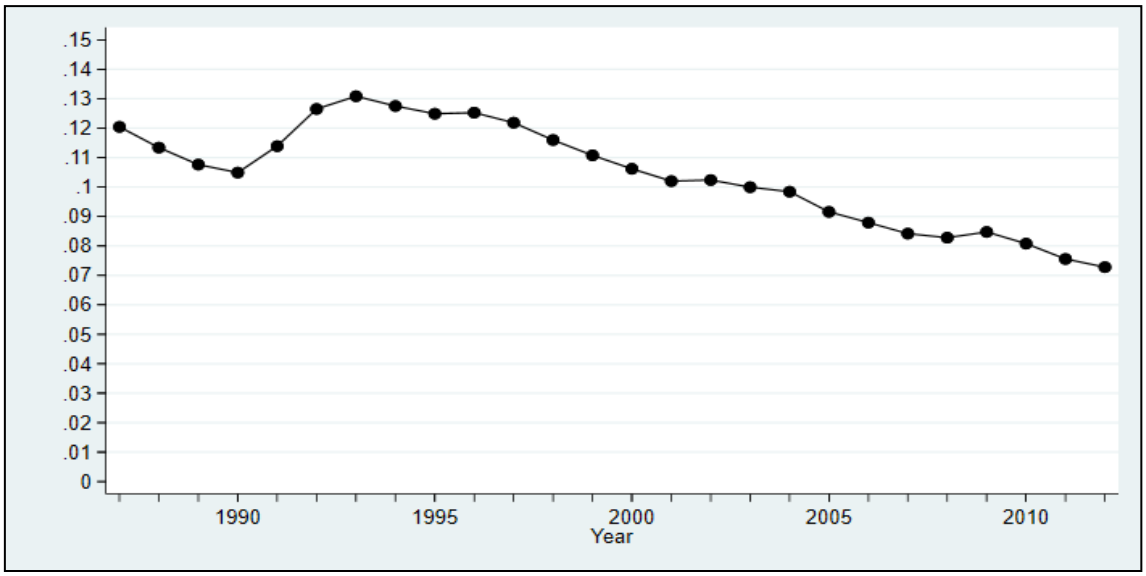
[9] To our knowledge, there are no studies on the subject with Finnish data. For the United States, the results of the How Couples Meet and Stay Together survey shows that the share of online dating as a partnership origin rose quickly from non-existent to over 20% at the turn of the millennium, while the share of partnerships originating from work

the contrary, Figure 2 shows that meeting a future spouse through work has become a more common inception point of new coworking couples. In recent years, nearly one third of both spouses in new coworking couples worked in the same workplace before their relationship began. Spouses in other coworking couples were already spouses (married or cohabiting) before starting to work at the same workplace. Regarding this, there is a gender difference: women are more likely to start working at their spouse's workplaces than vice versa. One reason for the difference is that, since women are more likely to have their work careers interrupted by childbearing (Albrecht et al. 1999), they are also more likely to be the second spouse entering a workplace.

Figure 3 shows that the share of coworkers among all dual-earner couples increases with age. Zinovyeva and Tverdostup (2018) find a similar result. This may indicate that those older spouses who prefer coworking have had more opportunities and time to relocate to the same workplace with one another than younger spouses who prefer coworking. This finding may also imply that the preference for coworking is higher among older spouses.

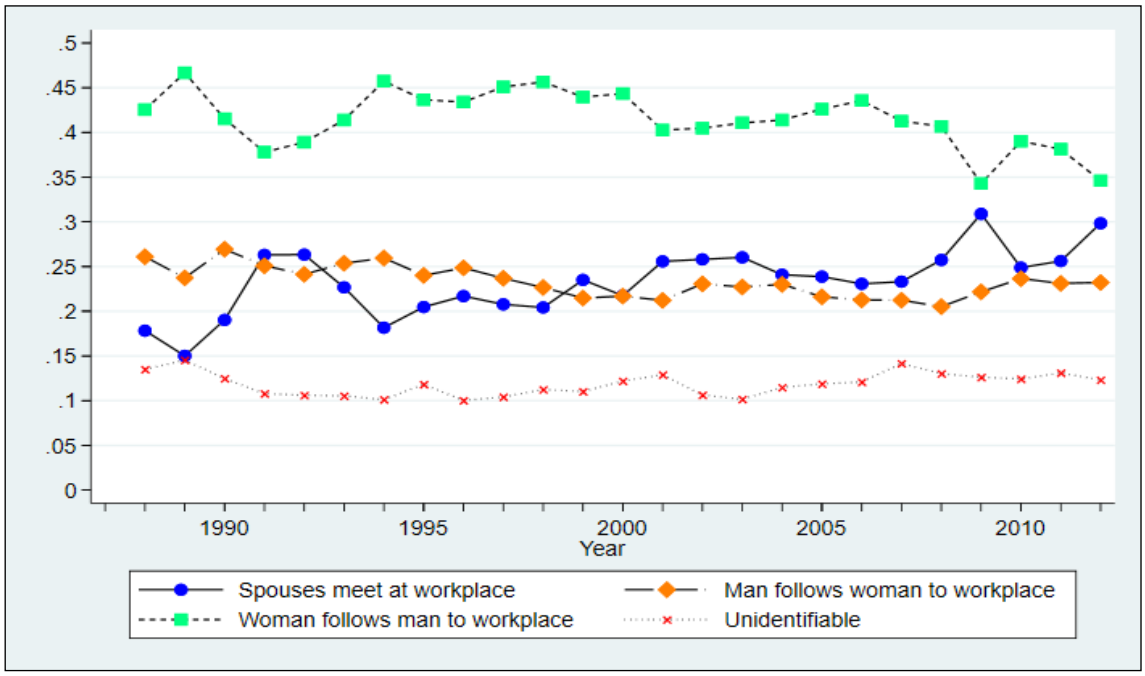
Figure 4 shows that coworking is most common in both very small and very large workplaces. The high share of coworkers in small workplaces may indicate that small workplaces are often family businesses that also employ the spouses of some of the family members. The high share of coworkers in large workplaces suggests that the probability of finding a spouse at the workplace is higher in large workplaces, since their pool of potential spousal candidates is larger.

FIGURE 1 Share of coworking couples of all dual-earner couples (1987-2012)



(with a coworker or with coworker's acquaintance) declined from 20% to 10% (Rosenfeld and Thomas 2012).

FIGURE 2 Shares of inflow types of new coworking couples (1988-2012)



Note: Unidentifiable = it is not possible to determine whether a couple worked at the same workplace before the relationship or if one spouse followed the other to the workplace.

FIGURE 3 Share of coworking spouses of all dual-earner spouses by age group (2010)

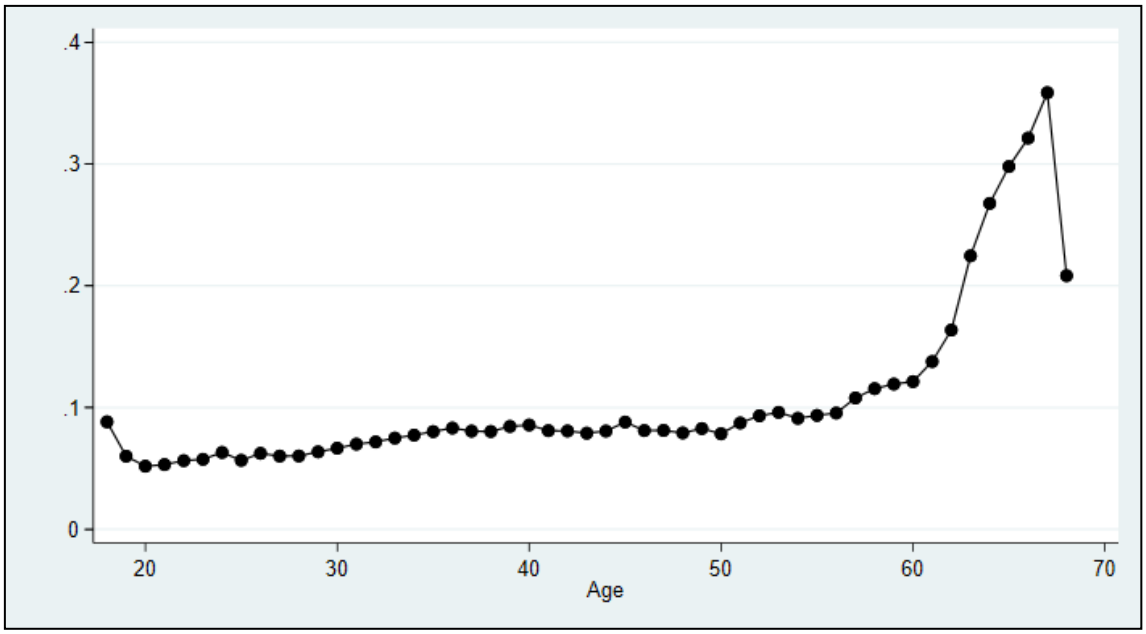
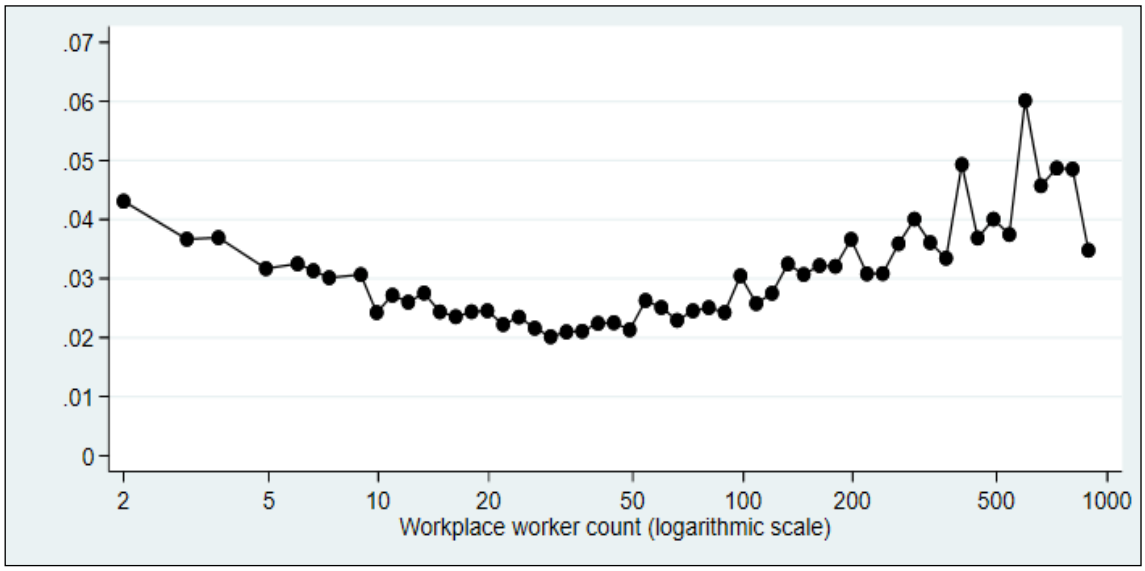


FIGURE 4 Share of coworking spouses of all workers by workplace size (2010)



3.4 Estimation

3.4.1 Sample selection

The purpose of this study is to estimate how coworking affects couple's workplace retention probability. We use the sample outlined in Table 1. The sample consists of couples whose relationship began between years *t+0* and *t+1*[10]. The relationship is deemed to begin during the year that the spouses marry each other or start cohabiting. We study new relationships to mitigate self-selection bias. Studying existing relationships could cause self-selection bias, since couples who benefit more from coworking are more likely to choose to cowork than couples who benefit less from it. This would imply that coworking couples in existing relationships are not a representative sample of the population. The treatment group couples consist of spouses who work at the same workplace at year *t+0,* before the relationship begins. The spouses in the control group work at different workplaces from one another at year t+0. We compare coworking couples to other new dual-earner couples to control for the effects that the new relationship has on workplace retention probability; for example, the specialization to earner and homemaker roles (Becker 1985). The outcome variable is assigned value 1 if both spouses continue to work in their pre-relationship year (*t+0*) workplaces at the outcome year (*t+1* and onwards).

The sample is restricted to spouses who live in the same municipality with each other, both before and after the beginning of the relationship (*t+0* and *t+1*).

10 All variable values are measured during the last week of their respectable years (t+0, t+1…).

Thus, there are no spouses who would have to leave their workplaces because of migration to the other spouse's municipality. To capture voluntary transitions, only spouses whose workplaces are not closed down by the outcome year are included in the sample. Furthermore, we only include spouses whose tenure in their workplace is observed. The sample is not restricted to couples who stay together or are observed in the data for the total ten year observation period. For each outcome year, we estimate the effect for male-female pairs who began their relationship between t+0 and t+1 and may or may not be together at observation period t+x. Including pairs who do not stay as couples may bias the estimates towards zero, but it also prevents the bias from possible differences in divorce behaviour between the treatment and control groups. The final sample at period t+1 consists of 65,126 couples of which 8,957 are coworking.

TABLE 1 Sample selection design

	Spouses in the control group		**Spouses in the treatment group**	
	Relationship status	**Workplace status**	**Relationship status**	**Workplace status**
Pre-relationship year *t+0*	Not in relationship with each other	Both work at *different* workplaces	Not in relationship with each other	Both work at the *same* workplace
Outcome year *t+1*	In relationship with each other	Outcome variable: 1 if both continue to work at their workplaces, 0 if otherwise	In relationship with each other	Outcome variable: 1 if both continue to work at the same workplace, 0 if otherwise

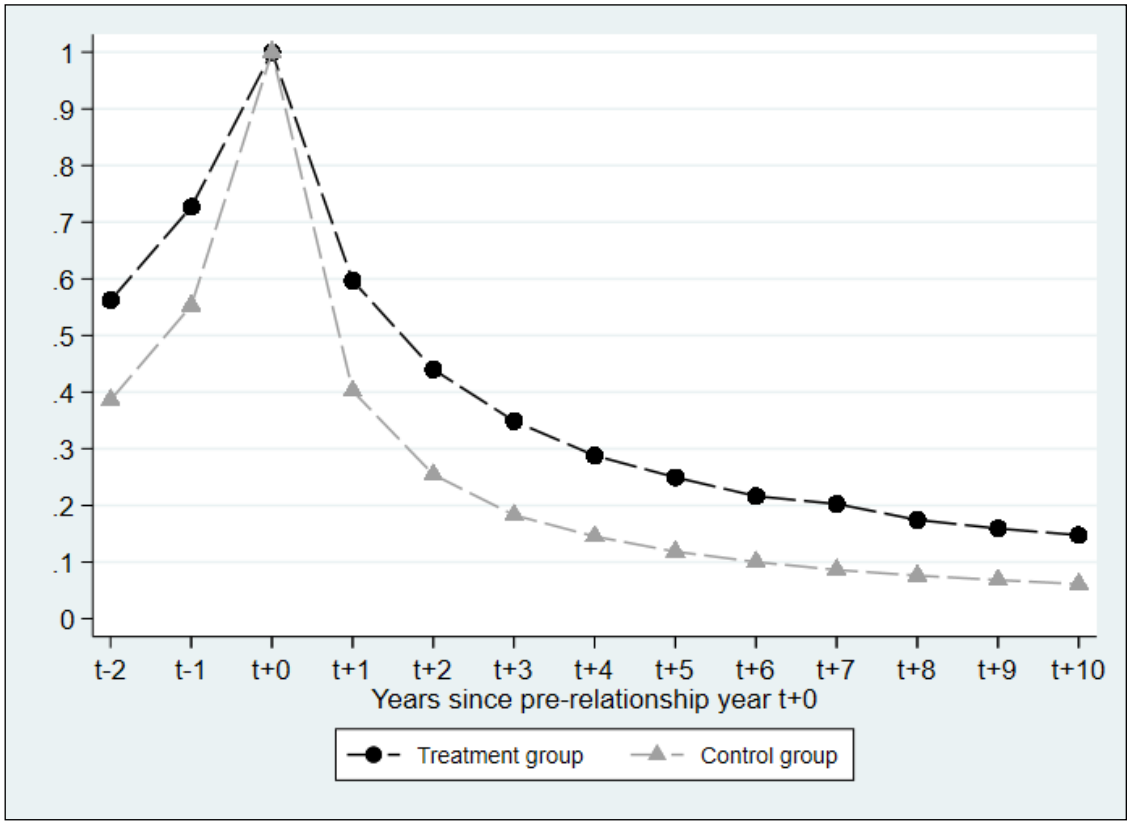
3.4.2 Observable characteristics

Figure 5 illustrates workplace retention probabilities in treatment and control groups for the first ten years since the beginning of the relationship (*t+1* to *t+10*). During the first year of the relationship, 60% of the coworking couples have both spouses remaining at their workplace, compared to only 40% of non-coworking couples. Naturally, the retention rate declines over time in both groups. However, it remains higher for the coworking group for the entire observation period.

Pre-relationship year data points (t-1 to t-2) in figure 5 show that treatment group spouses are more likely to be at their workplaces during these years than control group spouses. This might indicate that treatment group spouses are more likely to stay at their workplaces in general, which may explain the difference in latter years as well, instead of the coworking effects. To mitigate possible sample selection bias, we use a binary control variable that is assigned value 1 if the couple is in their t+0 workplace also at period t-1. We also use workplace specific control variables and robustness check to further mitigate the possible sample selection bias.

Appendix Figure A1 provides a more detailed presentation of spouses' transitions from the workplace. In addition to couple's workplace retention in Figure 5, A1 shows how likely it is for either or both of the spouses to leave the workplace. It also shows that if only one spouse remains at the workplace, that spouse is more often the male. Women are more likely to have their careers interrupted by childbearing (Albrecht et al. 1999), which might increase their departures from the workplace. The share of couples whose both spouses leave the workplace is quite equal across the groups.

FIGURE 5 Treatment and control group couples' workplace retention rates in the first ten years since the beginning of the relationship



Appendix Table A1 reports the means of observable variables in the control and treatment groups that are used in the estimations. The observational unit in this study is the couple, which consists of two persons with unique characteristics, such as income and age. In order to use many of the observable characteristics as

control variables, a division into person A and person B within the couple is necessary. For example, in Hallberg's (2003) study, the division into person A and person B is based on gender. In this study, we use separate control variables for the minimum and the maximum of each characteristic within the couple, where available. For example, minimum age is the younger spouse's age, and maximum income is the breadwinner spouse's income. Certain extreme values may have a greater impact on the outcome variable than values based on gender. We perform estimations with the male/female control variables as a robustness check. Variable means for the male/female control variable specification are presented in Table A2.

The comparison of personal characteristics in Table A1 indicates that treatment group spouses are more educated and older than control group spouses. High education and age are probably linked to several other observable variable values—tenure, education, salary, and the number of children—that have higher values among treatment group spouses. The pre-relationship distance variable indicates how far apart the spouses lived before the beginning of the relationship. It controls for the effects that the distance of moving from one address to the other has on workplace retention.

The data also provides workplace-level control variables. Comparable to couple's workplace retention, each spouse's workplace average retention rate indicates the share of colleagues who remain at the workplace in the outcome period. It controls for the turnover rate of colleagues at the workplace, as well as the effects of exogenous shocks such as mass layoffs[11]. The age difference to workplace average controls for the similarity between the spouse and his/her colleagues, which might affect the spouse's well-being at the workplace. Variables are measured at the pre-relationship period (t), excluding the workplace average retention rate and local unemployment rate variables, which are measured at the outcome period.

3.4.3 Main results

Table 2 reports the estimated effects of coworking on workplace retention, using various estimation specifications. Columns 1, 3 and 5 report the estimates from a linear regression model and columns 2, 4 and 6 report the average marginal effects from the probit model. It is likely that some unobserved components of outcomes, such as some distance factors and the availability of alternative workplaces, are correlated within municipalities. This motivates us to report standard errors that account for clustering by municipality (Abadie et al. 2017).

Both the ordinary least squares (OLS) and probit estimates indicate that coworking increases workplace retention by ten percentage points (columns 1 and 2), when using the minimum and maximum control variables presented in Table A1. When using the male and female control variables, the estimates are

[11] The spouses in the estimation sample couples are excluded when calculating the workplace's average retention rate in order to avoid simultaneity issues.

four percentage points higher (columns 3 and 4). Without control variables, the estimates are ten percentage points higher (columns 5 and 6), corresponding to the descriptive evidence presented before. Henceforth, we report the results from estimations with the minimum and maximum control variables, unless otherwise mentioned.

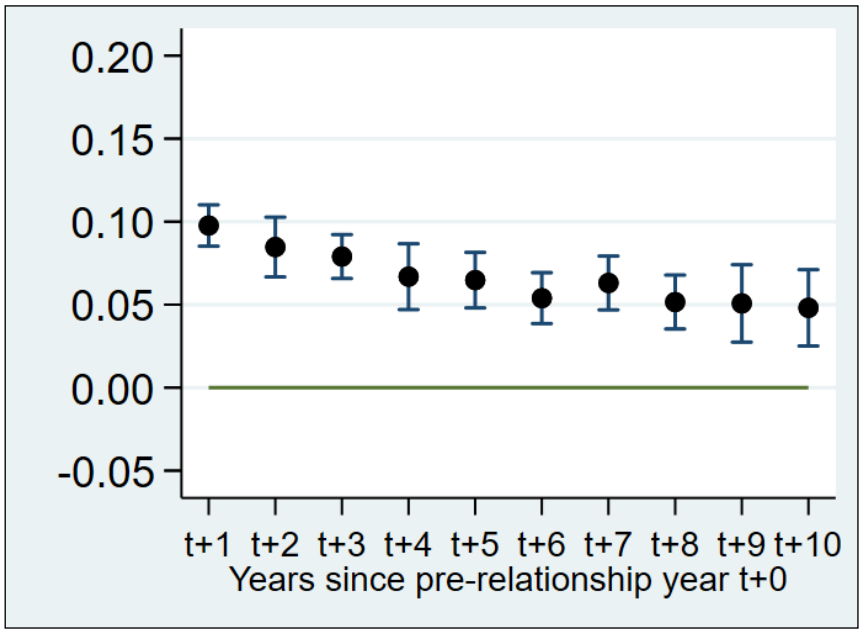
Our preliminary analysis (Figure 5) suggests that the effect of coworking on workplace retention decrease after the first year in a relationship. Therefore, we estimate the effect for ten consecutive years from the beginning of a relationship (t+1) to its tenth year (t+10). Figure 6 shows that the effect decreases from ten percentage points to five percentage points over the years. This implies that the negative aspects of coworking materialize with a delay and/or that its positive aspects gradually deteriorate.

TABLE 2 Effect of coworking on workplace retention probability in period t+1

	1	2	3	4	5	6
Estimation method	OLS	Probit	OLS	Probit	OLS	Probit
Control variable setting	Min/ Max	Min/ Max	Male/ Female	Male/ Female	No Controls	No Controls
Work-meeting effect	0.098***	0.098***	0.138***	0.133***	0.205***	0.202***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.009)
Observations	63,987	63,987	63,987	63,987	65,127	65,127
R2 / pseudo-R2	0.196	0.157	0.196	0.159	0.020	0.015

Significance levels: ***1%,**5%,*10%. Standard errors clustered by municipality

FIGURE 6 Effect of coworking on couple's workplace retention in periods t+1 to t+10



Brackets indicate the 95% confidence interval.

The control variable coefficients (Table A3) show that the increase in younger or less experienced worker's age or tenure increases couple's workplace retention probability more than the increase in older or more experienced worker's age or tenure. Low age and tenure can imply that the employee is less likely to have settled for a permanent position. The higher the age and tenure, the more stable the couple's current work situation, especially among the younger and less experienced spouses. The large positive coefficient for the minimum average workplace retention indicates that a high turnover rate or mass layoffs in a spouse's workplace predict low workplace retention for him/her, and therefore for the couple. In the case of yearly work income, the maximum value has a greater effect on workplace retention. Breadwinner's salary is probably more important for the couple than the non-breadwinner's salary.

3.4.4 Heterogeneity of results

The effect of coworking on workplace retention might be heterogeneous due to, for example, differences in work environments or demographic variables. To test this, we divided the sample into two equally sized subgroups by both the workplace worker count (Table 3 and Figure 7) and the couple's age (Table 4 and Figure 8)[12]. In Table 3 and Figure 7, the sample is divided into couples in which both spouses work in either small (less than 28 workers) or large (28 workers or

[12] Due to the similarity of results between the OLS and probit methods, only OLS estimates are reported henceforth. Furthermore, we omit results that are estimated without control variables since the results in Table 2 indicate that they are subject to omitted variables bias.

more) workplaces in the pre-relationship period *t+0*. In smaller workplaces, the interactions with any particular colleague, including the spouse, are more frequent. Moen and Sweet (2002) imply that the effects of coworking are greater if coworking spouses work close to each other. Studies also show that workers in small workplaces influence each other more in various ways, for example in terms of suicidal tendencies (Hedström et al. 2008) and commonness of childbearing (Pink et al. 2014). However, all the estimates in Table 3 indicate that the effects are very similar between small and large workplaces. Figure 7 shows that the effect of coworking on workplace retention rate decreases more during the first few years in large workplaces, but eventually the effect is approximately 5% in both the small and large workplaces.

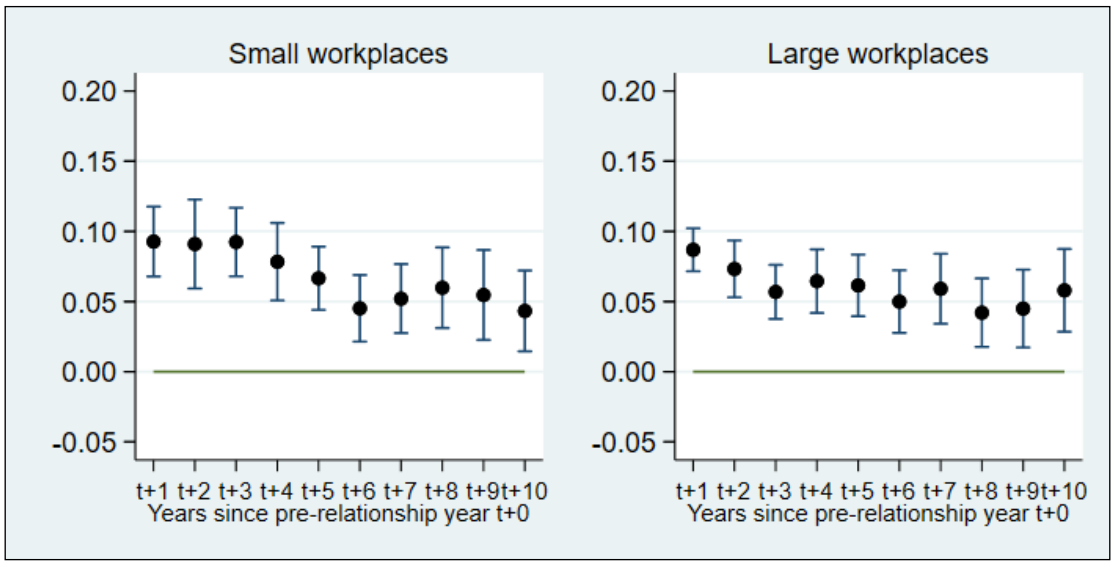
In Table 4 and Figure 8, the sample is divided into couples where males are either younger or older than 27 years at the pre-relationship period t. Figure 3 shows that the share of coworking spouses of all dual-earner spouses is higher among older spouses. However, the older the spouses, the more time those spouses who most prefer coworking have had to self-select to be coworkers. Therefore, the descriptive statistics may not provide a representative image of the preferences at the population level. Furthermore, Hallberg (2003) finds that younger couples synchronize their free time more than older couples, suggesting they might prefer the joint time use more. Supporting this, Table 4 shows that the positive effect of coworking for workplace retention is two to five percentage points higher for younger coworking couples than for older couples. Figure 8 demonstrates that age has a noticeable impact on the workplace retention trend. The coworking effect decreases from twelve percentage points to less than five percentage points for younger couples, whereas it remains between five and ten percentage points for older couples. Due to their life experiences, older couples' decisions to either stay or leave the common workplace may be more farsighted and permanent. Experience may also better prepare them for the work-family conflicts related to coworking.

TABLE 3 Effect of coworking on workplace retention in small and large workplaces in period t +1

	1	2	3	4
Workplace size	< 28 workers	≥ 28 workers	< 28 workers	≥ 28 workers
Estimation method	OLS	OLS	OLS	OLS
Control variable setting	Min/ Max	Min/ Max	Male/ Female	Male/ Female
Work-meeting effect	0.093***	0.087***	0.128***	0.137***
	(0.013)	(0.008)	(0.010)	(0.006)
Observations	18,807	18,727	18,807	18,727
R2 / pseudo-R2	0.153	0.270	0.154	0.267

Significance levels: ***1%,**5%,*10%. Standard errors clustered by municipality

FIGURE 7 Effect of coworking on workplace retention in small (< 28 workers) and large (≥ 28 workers) workplaces, periods t+1 to t+10



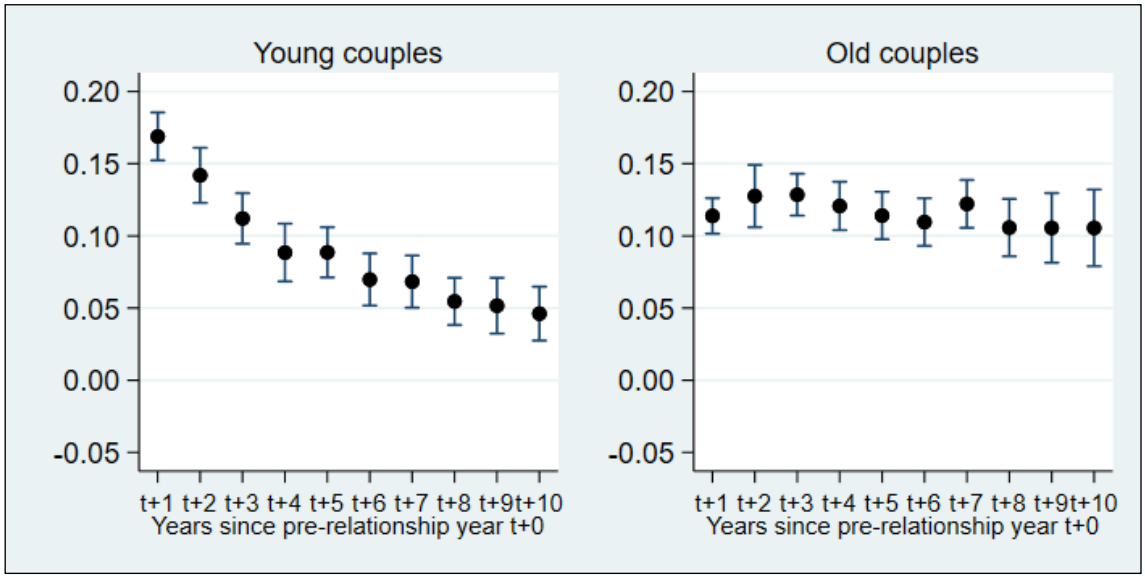
Brackets indicate the 95% confidence interval.

TABLE 4 Effect of coworking on workplace retention in young and old couples in period t+1

	1	2	3	4
Male age	< 27 years	≥ 27 years	< 27 years	≥ 27 years
Estimation method	OLS	OLS	OLS	OLS
Control variable setting	Min/ Max	Min/ Max	Male/ Female	Male/ Female
Work-meeting effect	0.117***	0.084***	0.169***	0.114***
	(0.010)	(0.007)	(0.008)	(0.006)
Observations	31,280	31,707	31,280	31,707
R2 / pseudo-R2	0.163	0.187	0.157	0.188

Significance levels: ***1%,**5%,*10%. Standard errors clustered by municipality

FIGURE 8 The effect of coworking on workplace retention in young (male < 27 years) and old (male ≥ 27 years) couples, periods t+1 to t+10



Brackets indicate the 95% confidence interval.

3.4.5 Robustness of results

In this section, we study how the alternative estimation settings affect the results. Appendix Figures A3 to A7 compare the results in Figures 6 to 8 (expressed as triangle markers in Figures A3 to A7) to the following: i) the results estimated with male/female control variables (square markers in Figures A3 to A7), and ii) two sets of results estimated with alternative samples (circle and cross markers in Figures A3 to A7).

Similar to the short-term effects presented in Tables 2 to 4, the long-term effects estimated with male/female control variables (square markers) are higher than the original min/max estimates (triangle markers). However, their trend progression is similar across all specifications, which gives credibility to the results in Figures 6 to 8.

Alternative samples are used to evaluate the robustness of results to the sample selection procedure. The original results in this study (Figures 6 to 8) are based on a sample of spouses who work in their workplaces at year t+0, before their relationship is first observed in the data at year t+1. The assumption for identification is that the spouses meet each other at the workplace before the relationship is observed. However, it is possible that some spouses meet outside the workplace before year t+0 and then move to the same workplace before the relationship is observed. If so, they would be misinterpreted as a coworking couple[13]. To study the relevance of the issue and its implications on self-selection bias, the coworking effect in the 'long tenure' sample (circle markers) is estimated for couples where both spouses work in their workplaces already at period t-1. This reduces the possibility that spouses first meet outside the same workplace. Furthermore, it reduces the possible self-selection bias that may arise if some treatment group spouses who meet at work stop coworking well over a year before the relationship is observed at t+1 and would thus be excluded from the original sample. The estimates for the long tenure sample in Figures A3 to A7 are very similar to the original sample, indicating that this type of bias is not relevant in the original sample.

In the 'short tenure' sample (cross markers), the coworking effect is estimated for couples in which one or both of the spouses do not work in their period t+0 workplace at period t-1. This sample is used to study if there is bias due to coworking spouses possibly knowing each other for a longer time before year t than control group spouses. The bias is mitigated if coworking spouses do not work together for an extensive time before the relationship. Again, the estimates are very similar to the original sample, indicating that any such bias in the original sample is small.

(Sassler 2004) Overall, the results from the alternative samples and from the male/female control variable estimations support our main finding: coworking increases workplace retention, but with a downward sloping trend over time.

3.5 Discussion

Our study explores the association between spousal coworking and workplace retention probability. Prior studies imply that coworking spouses have a better understanding of each other's workplace culture and colleagues (Janning 2006),

[13] This group is likely to be small, because dating couples who eventually start cohabiting will generally do it rather quickly. Sassler (2004) found that 13 out of 25 interviewed cohabiting couples had moved in together in the first half year of dating and only 7 after a year or more of dating.

increased spousal support in work matters (Ferguson et al. 2016), and less time-related work-family conflicts (Halbesleben et al. 2012). Other benefits might also exist; for example, logistics costs are lower if the couple needs only one car for commuting. Furthermore, coworking spouses can form an ad hoc labour union with each other; it can be uneconomical for the employer to lay off an unproductive spouse if there is a high probability that a productive spouse also leaves the workplace as a form of retaliation. On the other hand, results by Santhosh and Kutty (2012) indicate that coworking decreases worker productivity, especially for women. Any such effect could decrease workplace retention, if observed by employer.

We use Finnish register data to estimate the coworking effect. To mitigate self-selectin bias, we study the workplace retention probability of couples where spouses meet while working in the same workplace. Furthermore, we control for omitted variable bias with demographic and workplace specific control variables.

The results show that coworking spouses are significantly more likely to both continue working in their workplace than spouses who work in different workplaces before the relationship begins. Although the interaction between coworking spouses is more common in smaller workplaces, based on evidence by Hedström et al. (2008) and Pink et al. (2014), we find no clear differences in the coworking effects between couples working at small and large workplaces. This implies that interacting and working closely together with a spouse is not a critical benefit of coworking. The implication is supported by results from Hallberg (2003) and Sullivan (1996), which indicate that spouses enjoy joint leisure over individual leisure, but do not enjoy joint domestic work over individual domestic work as much. The benefits from coworking might be attributed more to better logistics (car-pooling), common work-related friends and a greater understanding of spouse's workplace characteristics, for example.

The positive effect of coworking on workplace retention decreases over time. The decrease suggests that some benefits of coworking decrease over time and/or that some disbenefits of coworking increase over time. The discussed benefits are seemingly independent of the length of the coworking relationship. Conversely, problems related to work spilling over into the family domain or vice versa (Halbesleben et al. 2012; Moen and Sweet 2002) might accumulate over time. Moreover, couples who are overly optimistic about the advantages and disadvantages of coworking may start by coworking but eventually move to work in different workplaces. We find that the effect persists most for older couples. Older couples have more experience in both work and relationships and may therefore be better at predicting their utility from coworking and at managing the unusual intersection of their work and family domains.

The results of this study can benefit both employees and employers by informing them about the various aspects of spousal coworking. The decreasing coworking effect that we find encourages future research to focus on changes in coworking spouses' experiences and behaviour over time. Another interesting aspect for future research is whether coworking spouses spend more leisure or domestic work time together than other dual-earner spouses. In this study, we

do not take different joint work preferences between spouses into account, but it would also be interesting to study how they manifest.

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APPENDIX

FIGURE A1 Spouse's workplace transitions

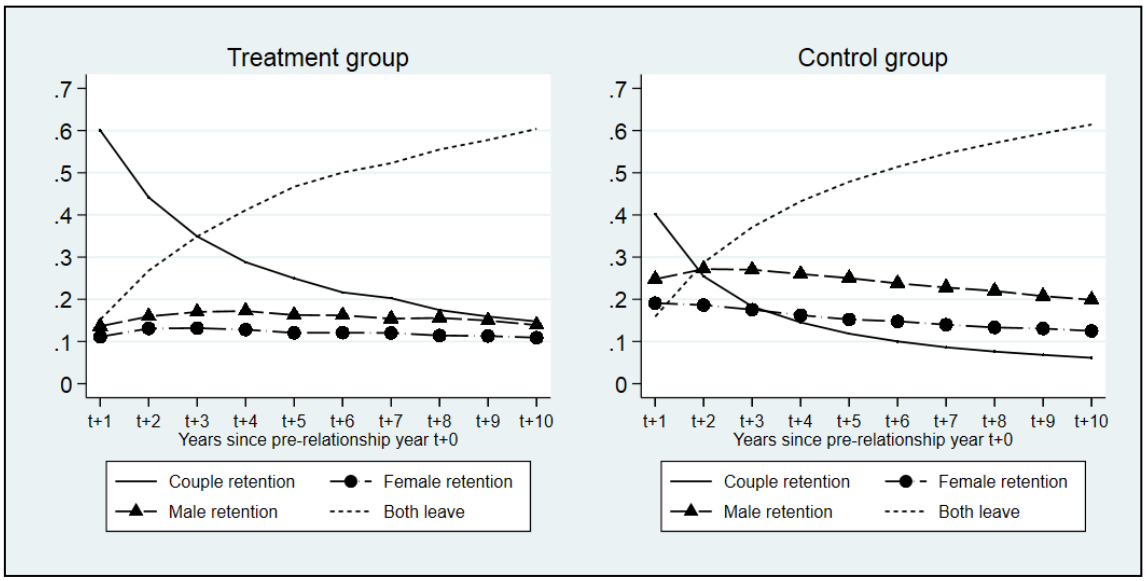


TABLE A1 Descriptive statistics for control and treatment groups and p-values for the difference

	Control	Treatment	p-value
Workplace retention for couple, t+1	0.41	0.62	0.00
Male primary education	0.21	0.21	0.74
Male secondary education	0.60	0.54	0.00
Male tertiary education	0.19	0.25	0.00
Female primary education	0.19	0.20	0.02
Female secondary education	0.57	0.51	0.00
Female tertiary education	0.24	0.29	0.00
Local unemployment rate	0.11	0.12	0.00
Pre-relationship distance, km	5.15	5.16	0.94
Pre-relationship distance 0km	0.29	0.31	0.02
Spouses at t+0 workplace in t-1	0.30	0.47	0.00
	Minimum / Maximum		
Population density, per km2	*1 437*	*1 336*	*0.00*
	2 672	2 471	0.00
Age	*25.8*	*26.5*	*0.00*
	29.5	31.0	0.00
Number of children	*0.15*	*0.17*	*0.00*
	0.51	0.66	0.00
Yearly work income	*13 716*	*17 602*	*0.00*
	25 416	27 981	0.00
Tenure at workplace	*0.57*	*1.08*	*0.00*
	2.41	2.75	0.00
Age difference to workplace average	*6.16*	*6.45*	*0.00*
	12.2	10.0	0.00
Workplace average retention rate, t+0 to t+1	*0.56*	*0.71*	*0.00*
	0.83	0.72	0.00
Number of workers at workplace	*31.5*	*248.8*	*0.00*
	215.5	248.8	0.00
Workplace average yearly income	*17 755*	*24 180*	*0.00*
	28 833	24 180	0.00
Observations	56 169	8 957	

TABLE A2 Descriptive statistics: male and female variables in treatment and control groups, with p-value for statistical difference

	Control	Treatment	p-value
Workplace retention for couple, t+1	0.41	0.62	0.00
Male primary education	0.21	0.21	0.74
Male secondary education	0.60	0.54	0.00
Male tertiary education	0.19	0.25	0.00
Female primary education	0.19	0.20	0.02
Female secondary education	0.57	0.51	0.00
Female tertiary education	0.24	0.29	0.00
Local unemployment rate	0.11	0.12	0.00
Pre-relationship distance, km	5.15	5.16	0.94
Pre-relationship distance 0km	0.29	0.31	0.02
Spouses at t+0 workplace in t-1	0.30	0.47	0.00
	Male / Female		
Population density, per km2	2 044	1 903	0.00
	2 085	1 912	0.00
Age	*28.6*	*29.7*	*0.00*
	26.8	27.8	0.00
Number of children	*0.32*	*0.42*	*0.00*
	0.33	0.42	0.00
Yearly work income	*23 083*	*25 831*	*0.00*
	16 033	19 732	0.00
Tenure at workplace	*1.70*	*2.21*	*0.00*
	1.28	1.62	0.00
Age difference to workplace average	*9.15*	*7.77*	*0.00*
	9.23	8.71	0.00
Workplace average retention rate, t+0 to t+1	*0.71*	*0.71*	*0.97*
	0.67	0.72	0.00
Number of workers at workplace	*136.3*	*248.8*	*0.00*
	110.6	248.8	0.00
Workplace average yearly income	*25 843*	*24 180*	*0.00*
	20 744	24 180	0.00
Observations	56 170	8 957	

TABLE A3 Linear regression coefficients of full sample for outcome year t+1

Treatment variable		
Coworking	0.100***	(0.006)
Personal characteristics		
Male secondary education	0.007	(0.006)
Male tertiary education	-0.005	(0.006)
Female secondary education	0.009**	(0.005)
Female tertiary education	-0.010*	(0.005)
Local unemployment rate	0.141**	(0.059)
Pre-relationship distance, km	-0.000	(0.001)
Pre-relationship distance 0km	0.008	(0.005)
Spouses at t+0 workplace in t-1	0.060***	(0.008)
	Minimum / Maximum	
Population density, per km2	-0.000***	(0.000)
	-0.000**	(0.000)
Age	0.067***	(0.015)
	-0.019	(0.013)
Number of children	-0.012**	(0.006)
	-0.006**	(0.003)
Yearly work income. Logarithm	0.000***	(0.000)
	0.053***	(0.004)
Tenure at workplace	0.006***	(0.002)
	0.012***	(0.001)
Workplace characteristics		
Age difference to workplace average	-0.002***	(0.000)
	-0.002***	(0.000)
Workplace average retention rate, t+0 to t+1	0.488***	(0.008)
	0.194***	(0.011)
Number of workers at workplace, logarithm	-0.004**	(0.002)
	-0.001	(0.001)
Average yearly income at workplace	-0.015***	(0.005)
	-0.050***	(0.010)
Observations	62,987	
R2	0.196	

FIGURE A2 Robustness estimations for all couples: the effect of coworking on couple's workplace retention

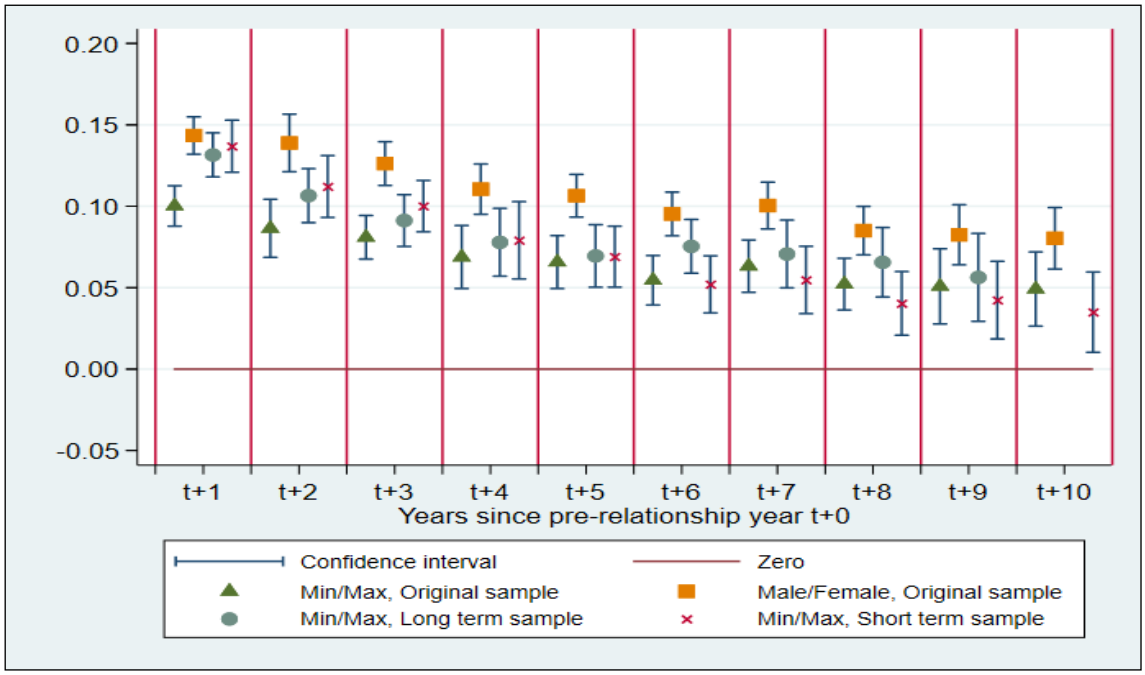


FIGURE A3 Robustness estimations for small workplace couples: the effect of coworking on couple's workplace retention

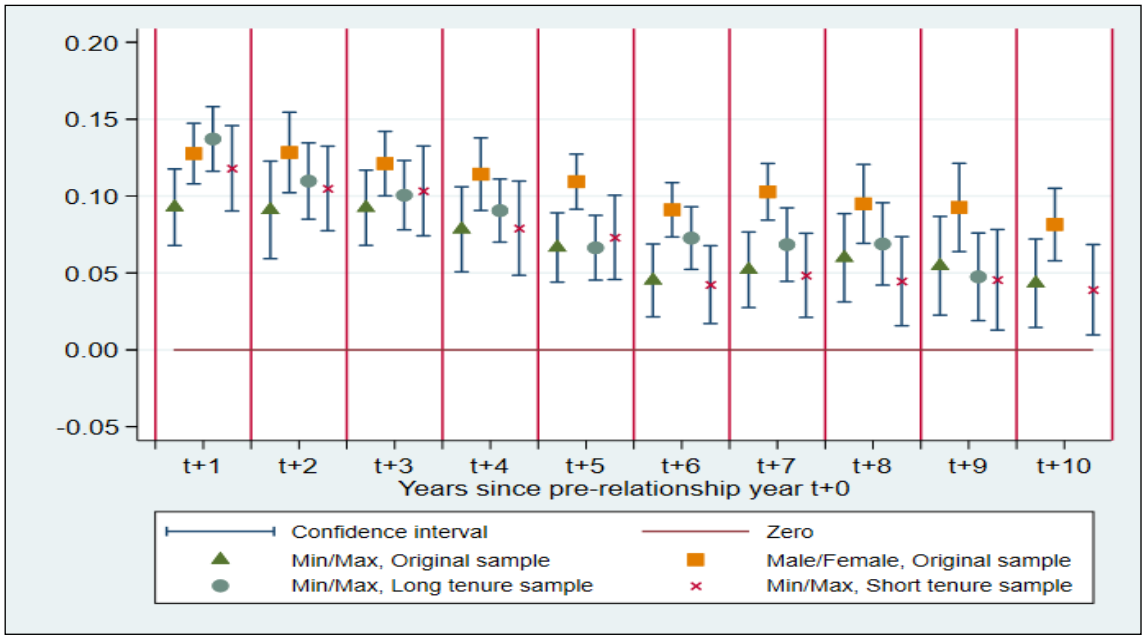


FIGURE A4 Robustness estimations in large workplace couples: the effect of coworking on couple's workplace retention

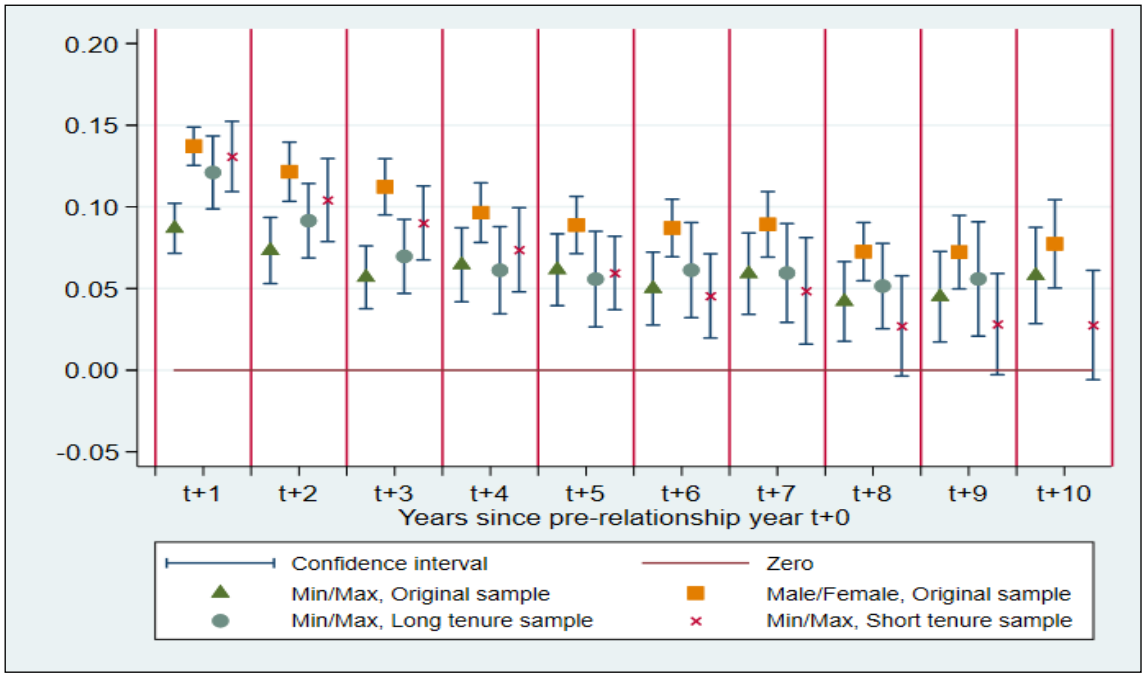


FIGURE A5 Robustness estimations in younger couples: the effect of coworking on couple's workplace retention

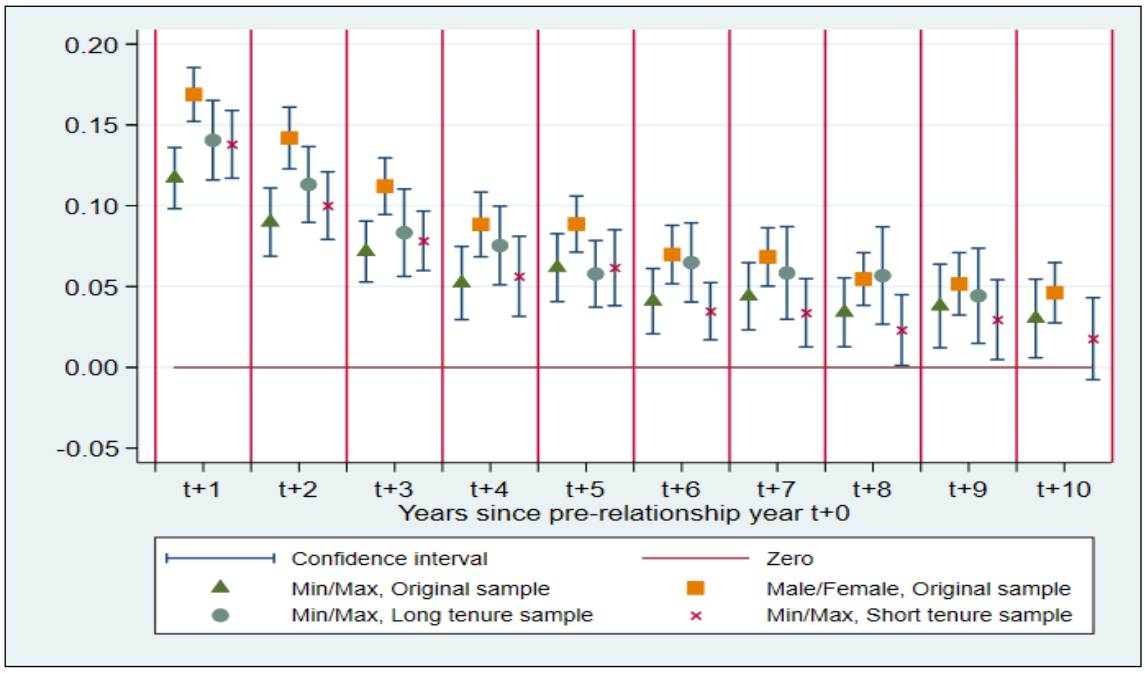
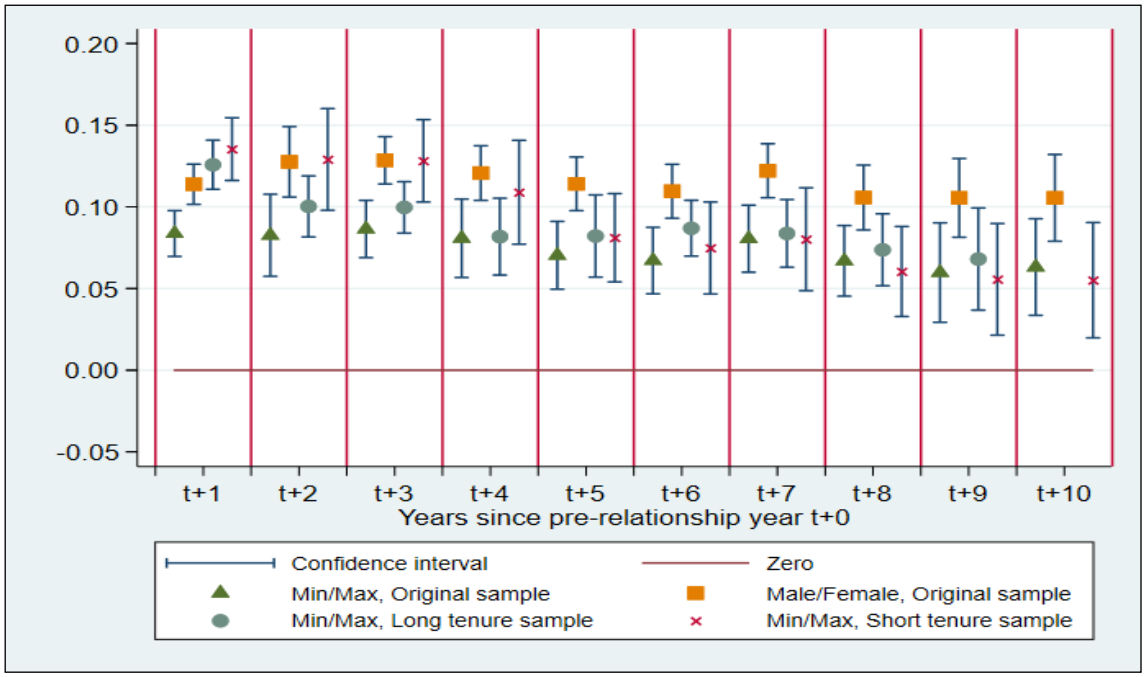


FIGURE A6 Robustness estimations in older couples: the effect of coworking on couple's workplace retention



4 AGE, EARNINGS AND JOINT RETIREMENT: POPULATION-BASED EVIDENCE*

Abstract

This study examines how retirement affects the retiree's spouse's retirement probability. To estimate the causal effect, we use large Finnish population data and a regression discontinuity method, with pension eligibility age as the source of exogenous retirement variation. The study contributes to the existing literature by analysing whether this effect is dependent on the household members' age and earnings. The findings support the existing literature, which shows that the husband and the wife have a tendency to retire simultaneously. However, our results indicate asymmetric behaviour, owing to the earnings differences. In the low-earnings household the husband delays his retirement timing until his wife reaches pension eligibility age, whereas in the high-earnings household the wife advances her retirement timing to occur jointly with her husband's retirement.

4.1 Introduction

The continuous growth in life expectancy increases the share of pensioners, which puts stress on public finances. This has inspired research on pension policies and retirement decisions. Most studies have focused on individual retirement decisions (Atalay and Barrett 2015; French 2005; Kyyrä 2015). However, there is also a growing body of literature on joint retirement, which

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refers to the synchronization of spouses' retirement timing (Coile 2004; Hospido and Zamarro 2014; Stancanelli and Van Soest 2016). The interest in this topic is motivated by the increase in women's labour force participation and the prevalence of dual-earner couples (Van Gils and Kraaykamp 2008). Some joint retirement studies have shown that the husband's retirement increases the wife's retirement probability (Blau and Riphahn 1999; Hospido and Zamarro 2014; Smith and Moen 1998), while results by Coile (2004) suggest that a wife's retirement increases the husband's retirement probability. Furthermore, some studies have found evidence for both possibilities (Queiroz and Souza 2017; Warren 2015) and others for neither (Selin 2017; Stancanelli and Van Soest 2012). Spouses' preference for joint leisure over their separate leisure is often presented as an important motivation for joint retirement behaviour (Coile 2004), and the empirical evidence supports this theory (Gustman and Steinmeier 2004; Stancanelli and Van Soest 2016).

Joint leisure preferences may be the main cause for joint retirement, but household characteristics may influence its occurrence. We contribute to the literature by studying differences in joint retirement across various households, categorized by their age and earnings. Household members' age and earnings generally correlate with gender—women are usually younger and have lower earnings—so the results may provide new insight into the findings of previous studies, which show mixed results on how gender affects joint retirement. Our results also provide further understanding on household decision-making processes and how earnings affect labour supply.

We use the regression discontinuity (RD) method to estimate the causal effect of one spouse's retirement on the other spouse's retirement probability. Within the RD framework, we use the work pension eligibility age (63 years) as the exogenous cut-off point in the estimation. The identification relies on the assumption that the first spouse's pension eligibility affects the other spouse's retirement probability only through the first spouse's own retirement due to their pension eligibility. We estimate the model using Finnish population level data. For brevity, we refer to the effect of the first spouse's retirement on the other spouse's retirement probability as the joint retirement effect. Also, for brevity and clarity, the first spouse is henceforth referred to as the retiree[14] and the other spouse as the spouse.

If the husband is pension eligible when the wife retires, we find that the wife's retirement increases the husband's retirement probability in low-earnings households (delaying joint retirement). If the wife is not pension eligible when the husband retires, we find that the husband's retirement increases the wife's retirement probability in high-earnings households (advancing joint retirement). These findings suggest that the income effect dominates the substitution effect, since the relationship between the household's earnings potential and the spouse's labour supply is negative. We also find that the breadwinner status has a gender-asymmetric effect on joint retirement. Breadwinner husband's

[14] To clarify, the retiree is the household member whose retirement's effect is studied, and not always a retired person per se.

retirement delays the wife's retirement timing, while breadwinner wife's retirement advances the husband's retirement timing. The asymmetry indicates that there is no definite support for either the unitary or the collective household decision-making model, but that their prevalence depends on the wife's and husband's breadwinner roles within the household.

4.2 Conceptual framework

To study the differences in the joint retirement effect by household type, we first divide households into two groups based on the age of the retiree's spouse. In the first group, the spouse is younger than 63 years. In the second group the spouse is older than 63 years. With this, we separate the estimation of the joint retirement effect into estimations of advancing (spouses under 63 years) and delaying (spouses over 63 years) joint retirement effects. The difference in the prevalence of these effects should indicate whether it is more common that spouses advance their retirements to occur jointly with retirees' retirements or that spouses delay their retirements to occur jointly with the retirees' retirements. If the pension eligibility age is the socially desired minimum retirement age, then the prevalence of advancing joint retirement effects implies that there is room for pension reforms that encourage married individuals to retire later.

We separate households into high- and low-earnings households based on the retiree's and the spouse's combined earnings prior to the sample year. The analysis is motived by Blau and Riphahn (1999) and Kapur and Rogowski (2007) who find that wealth increases joint retirement probability. We examine the earnings groups in conjunction with the age groups. Our results may provide information on whether the income or the substitution effect dominates the joint retirement decision (De Preter et al. 2015; Queiroz and Souza 2017). The higher the household earnings, the more affordable it is for the spouse to retire jointly with the retiree due to household's greater accrued wealth and pensions (income effect). The income effect therefore increases (decreases) the probability of the advancing joint retirement effect among high (low) earner households, and vice versa for the delaying joint retirement effect. On the contrary, high earnings also increase the opportunity costs of retirement due to the better ability to accrue more wealth and pensions (substitution effect). The substitution effect therefore decreases (increases) the probability of the advancing joint retirement effect among high (low) earner households, for whom the monetary benefits of remaining at work are greater (lesser).

We also categorize households based on which household member is the breadwinner. This categorization, especially in combination with the age composition categorization, may indicate whether the decision-making process between the household members is better described by the unitary or the non-unitary household decision-making model; see Vermeulen (2002) or Donni and Chiappori (2011) for an overview of the models. According to the unitary models, the household members don't have individual preferences, and the household

maximizes its members' combined welfare. In that sense, household members behave in a more or less altruistic manner. In these models, the breadwinner works more, owing to enhanced contribution to the joint wealth with an equal amount of lost leisure time (assuming that higher earnings are an indicator of higher wages). The unitary model therefore predicts that breadwinner spouses are more likely to delay their own retirement and non-breadwinner spouses are more likely to advance their retirement in response to the retiree's retirement. According to the non-unitary decision-making models, each household member has their own preferences and they maximize their own welfares. The distribution of bargaining power between the members determines the Pareto weight of each member's welfare in the household decision-making process (Browning and Chiappori 1998; Chiappori et al. 2002). The family member with the higher bargaining power may use divorce as a threat point to exert bargaining power over the other member on household decisions (McElroy and Horney 1981). Earnings, or income in general, are a key determinant of bargaining power within a household (Giovanis and Ozdamar 2018; Michaud and Vermeulen 2011). Michaud and Vermeulen (2011) show that an increase in the husband's relative earnings increased his Pareto weight, which increased his share of leisure and consumption. Regarding joint retirement, the breadwinner could use their greater bargaining power to advance their own retirement timing or delay the other member's retirement timing. Unitary and non-unitary models predict opposite results for the earnings group comparison. Therefore, differences in the advancing and delaying joint retirement effects according to the breadwinner status may indicate which model describes the household behaviour better.

4.3 Methods and data

4.3.1 Methods

The joint retirement effect is typically estimated by using quasi-experimental methods (e.g. Hospido and Zamarro 2014; Selin 2017; Warren 2015). These methods are used to mitigate possible reverse causality and omitted variable biases. Reverse causality bias arises if the retiree's retirement does not cause the spouse to retire, but rather the spouse's retirement causes the retiree to retire. Omitted variable bias arises if, for example, a local labour market shock causes both members of the household to simultaneously lose their jobs and decide to retire.

Similar to Hospido and Zamarro (2014), Queiroz and Souza (2017) and Stancanelli and Van Soest (2012), we mitigate these biases by applying a fuzzy RD model (Imbens and Lemieux 2008) to estimate the joint retirement effect. We use a feature of the Finnish pension system, namely the earnings-related pension eligibility age (63 years), as the cut-off point for the RD model. The estimation method is expressed in equations 1 to 5. Figures 1 and 2 in the results section illustrate the method.

Joint retirement effect RD estimator: $\frac{\mu_{SPO+} - \mu_{SPO-}}{\mu_{RET+} - \mu_{RET-}}$ (1)

For couples where the retiree's age is above 63 years:

$\mu_{RET+} : Y_i = \hat{\mu}_{RET+} + \theta_1(Age_{RET+} - 63) + \theta_2(Age_{RET+} - 63)^2 + \cdots + \theta_p(Age_{RET+} - 63)^p$ *(2)*

$\mu_{SPO+} : Y_i = \hat{\mu}_{SPO+} + \gamma_1(Age_{RET+} - 63) + \gamma_2(Age_{RET+} - 63)^2 + \cdots + \gamma_p(Age_{RET+} - 63)^p$ *(3)*

Similarly, for couples where the retiree's age is below 63 years:

$\mu_{RET-} : Y_i = \hat{\mu}_{RET-} + \theta_1(Age_{RET-} - 63) + \theta_2(Age_{RET-} - 63)^2 + \cdots + \theta_p(Age_{RET-} - 63)^p$ *(4)*

$\mu_{SPO-} : Y_i = \hat{\mu}_{SPO-} + \gamma_1(Age_{RET-} - 63) + \gamma_2(Age_{RET-} - 63)^2 + \cdots + \gamma_p(Age_{RET-} - 63)^p$ *(5)*

The denominator in equation 1 is the difference in retirees' predicted retirement rates (E(Y)) at the pension eligibility cut-off age (63) between pension eligible retirees ($\mu_{RET+} = E[Y_i(1)|Age_{RET} = 63]$) and non-pension eligible retirees ($\mu_{RET-} = E[Y_i(0)|Age_{RET} = 63]$). It depicts the effect of pension eligibility on the retiree's retirement probability, assuming that any abrupt increase in the retirement probability at age 63 is caused by pension eligibility. The numerator is the difference in the retiree's spouse's predicted retirement rate between couples where retirees are just above ($\mu_{SPO+} = E[Y_i(1)|Age_{RET} = 63]$) or below ($\mu_{SPO-} = E[Y_i(0)|Age_{RET} = 63]$) the cut-off age. It depicts the link between the retiree's pension eligibility and the spouse's retirement. In instrument variable terminology, it is the intention-to-treat (ITT) effect. The ITT effect relies on the assumption that when the retiree reaches age 63 and possibly retires, it affects the spouse's retirement probability only through the change in the retiree's retirement status.

The four predicted retirement rates in equation 1 are estimated with a polynomial order *p* function in equations 2 to 5. As the continuous age variable is controlled, the constant term $\hat{\mu}$ is the estimate for the predicted retirement rate at the cut-off age (63 years) for each group.

The RD method is 'fuzzy' (in RD terminology) because the treatment uptake for the retirees (retirement) is not complete; all pension eligible retirees do not retire when they reach pension eligibility. The smaller the share of retirees retiring at the cut-off age, the smaller the ITT effect, if the joint retirement effect is fixed. The joint retirement effect is obtained by dividing the ITT effect by the first stage treatment rate, which depicts the share of retirees who retire at the pension eligibility age. If the retiree's retirement was determined completely by pension eligibility, μ_{RET+} would be 1 and μ_{RET-} would be 0. The denominator would be 1, and the equation would simplify to just the numerator, a sharp (non-fuzzy) RD estimator. It should be noted that given the estimation setting, the joint retirement effects concern *compliers*, that is, those couples where the retiree retires once they

become pension eligible. The generalizability of the results depends on how close the joint retirement preferences of other couples are to those of the compliers.

4.3.2 Data

We use a sample from the Finnish longitudinal employer-employee data (FLEED), which is compiled from official registers and include labour market and demographic information on the Finnish population. The investigated period spans from 2008 to 2015. A spousal link variable is used to identify dual-earner households. For the household to be included in the sample, both the retiree and the spouse must be married and employed in the fourth year prior (henceforth, the prior year) to the year the joint retirement effect is estimated (henceforth, the sample year).[15] To assure that the eligibility age is relevant for the retiree, he/she must be employed in the private sector during the prior year and must be 59-67 years old during the sample year with a spouse younger than 67 years old [16] [17]. In the sample period of 2008-2015, there are a total 335,519 such households where the man is the retiree and 180,164 households where the woman is the retiree.

A household member is deemed retired if they are not employed during the sample year. Labour market status is measured in the last week of each year. Given the high age of retirees and spouses, exits from employment are deemed indicative of effective retirement. For example, among 63-year-old men (women) who were not working, only 2.81% (1.61%) were working in the following year. The data also include information on pension status, but this information is not used.

The retiree's birthday is used to calculate their age at the last day of the year, which is the forcing variable in the RD setting. However, a bureaucratic delay is taken into account regarding the cut-off age at 63; individuals cannot receive an earnings-related pension until the beginning of the next calendar month that follows the pension eligibility age. Therefore, the treatment spouse's cut-off age is moved to 63 years and one month (63.0833 years) to include in the treatment group only those who have been 63 for an entire month at the end of the year observation period. For brevity, we refer to the cut-off age as 63.

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[15] The fourth year before the sample year is used as the prior year because there are no discontinuities at age 59 in the Finnish social security legislation that could cause sample selection bias four years later (at sample year). If there were discontinuities at age 59, they could affect the employment probability around that age. Since those who are employed around that age are included in the sample four years later, the discontinuity could cause bias in the estimates.

[16] The sample is restricted to couples where the retiree works in the private sector because several public-sector workers (e.g. police officers, teachers and military staff) are entitled to specific earnings-related pension ages. Estimations that include public-sector workers imply considerably lower retirement probability increase at the cut-off age.

[17] Including individuals over 67 years old would threaten the continuity of the assignment variable's relation to the treatment variable. Because persons aged 67 and older were already pension eligible when still employed four years prior; they are less prone to retirement than average pension eligible persons.

We compare joint retirement effects between various types of households. The household types are presented in Tables 1 and 2 (husband and wife as the retiree, respectively) with their corresponding median earnings for both household members, combined (household) and separately (wife, husband). Earnings are defined as the total annual earnings from work or entrepreneurial activity from the prior year. Since earnings-related pensions are determined by past earnings, the prior year's earnings are an accurate indicator of retirement income in most cases. The household is a high- (low-) earnings household if its members' combined earnings were above (below) the median household earnings level of the original sample in the prior year. In both tables, the difference in median household earnings between high- (second row) and low- (third row) earnings households can be attributed more to the difference in households' husbands' earnings than wives' earnings. However, the wife's earnings in high-earnings households are also greater than the wife's earnings in low-earnings households. Household earnings are therefore a good proxy for either family member's earnings. The wife (husband) is a breadwinner if her (his) earnings were higher than her (his) husband's (wife's) earnings in the prior year. Tables 1 and 2 show that husband (fourth row) is the breadwinner more than twice as often as the wife (fifth row).

TABLE 1 Median household earnings by household earnings group. Husband as the retiree. Pooled sample, 2008-2015

		Median earnings, thousands of euros		
Household type	Observations	Combined	Wife	Husband
All	335 519	63.6	26.8	36.0
High-earnings	168 492	82.1	32.3	49.0
Low-earnings	167 027	50.0	22.6	27.8
Husband breadwinner	242 009	66.0	24.9	41.0
Wife breadwinner	93 182	56.3	33.0	23.5

TABLE 2 Median household earnings by household earnings group. Wife as the retiree. Pooled sample, 2008-2015

		Median earnings, thousands of euros		
Household type	Observations	Combined	Wife	Husband
All	180 164	61.7	26.5	34.8
High-earnings	90 330	81.4	32.4	48.5
Low-earnings	89 834	46.7	21.6	25.0
Husband breadwinner	123 009	65.9	24.3	41.0
Wife breadwinner	56 783	51.6	31.6	20.2

4.3.3 Estimation choices

There are two important methodological choices regarding the RD model: the polynomial degree of the forcing variable and the bandwidth. Gelman and

Imbens (2019) argue against using high order polynomials, based on, for example, often misleading confidence intervals they produce and the lack of methods to help choose the correct polynomial degree. The visual evidence of our data indicates that the relationship between age and retirement rate is linear. This implies that a first-degree polynomial of the forcing variable should be used. This is supported by the possibility that there are frictions that prevent either the retiree or the spouse from retiring exactly when the retiree reaches pension eligibility, even if that is the intention. For example, a household member might feel obliged to finish an ongoing work project before retirement or to train a new worker who will replace them. Furthermore, a plant closure that occurs shortly before a planned joint retirement might lead to an early retirement. Since the exact retirement timing is unknown, it is difficult to determine how common these frictions are or how much they affect the retirement timing of household members. With these frictions, the retirement rates just below (above) the cut-off age may be higher (lower) than without the frictions. This biases the estimate, with higher degrees of forcing variable leading to higher biases. Therefore, a first-degree polynomial of the forcing variable is used in this study. To also mitigate the possible friction bias, a uniform kernel is applied, meaning that the observations are weighted equally in estimation, regardless of their proximity to the cut-off.

Another important choice is the bandwidth, which determines how far away from the cut-off value, at maximum, the assignment variable value can be for the observation to be included in the estimation. In this case, the bandwidth determines how much older or younger than 63 years can the retiree be for the couple to be included in the estimation. The wider the bandwidth, the more observations included in the estimation, which increases the statistical power of the estimation. However, the further away from the cut-off value the observations are, the more likely it is that the relationship between the assignment and outcomes variables is not determined by the same functional form than for the observations closer to the cut-off value. This could increase the bias of the RD estimator. The RD estimations are conducted with Stata's *rdrobust* package by Calonico et al. (2014a), which provides a mean squared error (MSE) based bandwidth selection method. It selects a bandwidth that balances between having i) a large enough sample size to decrease the variance and ii) observations that are close enough to the cut-off to provide asymptotically valid estimates. MSE is the sum of the estimate's leading bias and variance, and the selected bandwidth is the one that minimizes the MSE. The leading bias is calculated by comparing the estimate to an alternative estimate calculated with a one-degree higher polynomial order than the forcing variable. The smaller the leading bias, the smaller the possible bias from selecting a particular polynomial order.

Calonico et al. (2014b) argued that the MSE optimal bandwidth selection provides a valid bandwidth to estimate the effect, but that the confidence interval that is calculated from that estimate is biased. The bias occurs because the leading bias is assumed to be zero. To mitigate this, their *rdrobust* package provides bias-corrected confidence intervals. The procedure uses a higher polynomial order RD

estimate to account for the leading bias from a chosen lower order estimate. Benchmark study results by Hyytinen et al. (2018) support the use of bias-corrected confidence intervals. For completeness, we report both the conventional and bias-corrected confidence intervals for the results.

4.4 Estimation results

4.4.1 Main results

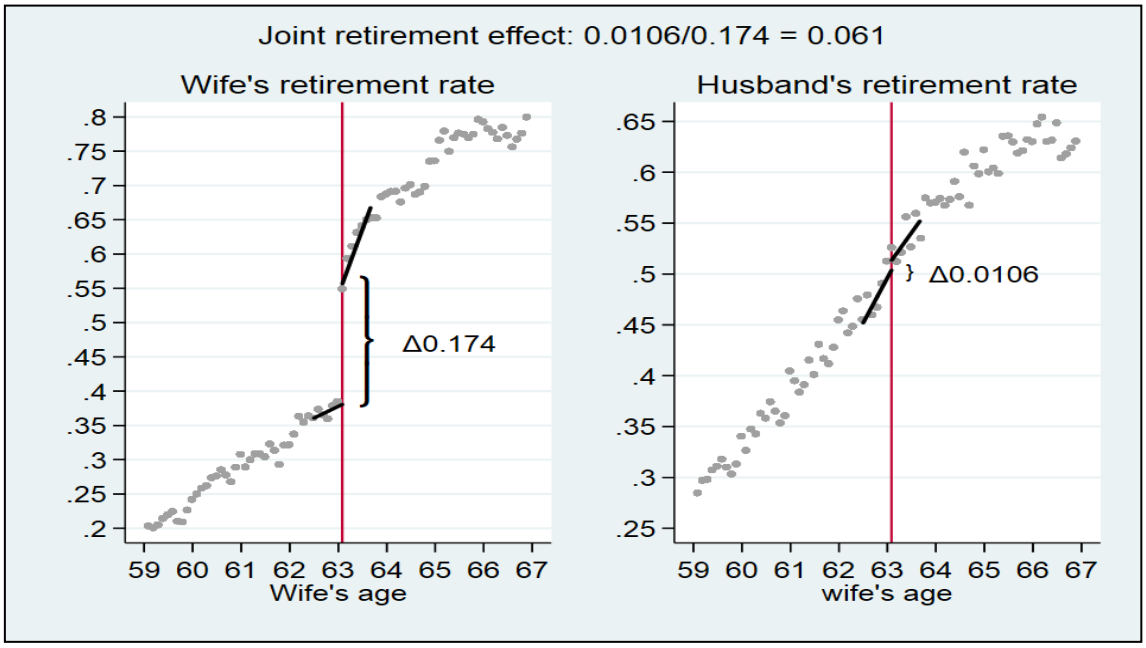
The main results are presented visually in Figures 1 and 2 and numerically in Tables 3 and 4, based on samples with the husband or the wife as the retiree, respectively. The black lines in the figures represent the fitted values from regressions in which the age variable is measured on a daily precision. The length of the line represents the bandwidth. The scatterplot points represent the average retirement rate values on a $1/12^{th}$ year (monthly) precision. The bracketed values indicate the estimated increase in the retirement rates at the cut-off age.

Figure 1 shows that the relationship between the husband's age and his own retirement rate (left graph) follows a steady linear trend, with the exception of an abrupt major increase at age 63, when the husband becomes pension eligible. The 18.4 percentage point increase is the effect of pension eligibility on the retirement rate. It corresponds to the denominator in equation 1. The 1.75 percentage point jump in the wife's retirement rate (right graph) depicts the ITT effect, which corresponds to the numerator in equation 1. Together, the first-stage effect and the ITT effect indicate that the husband's retirement increases the wife's retirement rate by 9.5 percentage points. Similarly, Figure 2 shows that the wife's retirement increases the husband's retirement probability by 6.1 percentage points.

FIGURE 1 Husband's and wife's retirement rates. Husband as the retiree


Joint retirement effect: 0.0175/0.184 = 0.095
Husband's retirement rate
.8
.75
.7
.65
.6
.55
.5
.45
.4
.35
.3
.25
.2
Δ0.184
59 60 61 62 63 64 65 66 67
Husband's age
Wife's retirement rate
.55
.5
.45
.4
.35
.3
.25
.2
.15
}Δ 0.0175
59 60 61 62 63 64 65 66 67
Husband's age

FIGURE 2 Wife's and husband's retirement rates. Wife as the retiree



Tables 3 and 4 report the numerical estimates and their confidence intervals. The first column shows the results for the total sample, corresponding to Figures 1 and 2. The second and third columns report evidence of advancing and delaying joint retirement effects, respectively. In other words, in column 2, the spouse is not pension eligible (spouse is younger than 63 years), and in column 3, the spouse is pension eligible (spouse is older than 63 years).

The results show that the husband's retirement increases the wife's retirement probability by 9.5 percentage points (Table 3, first column). The effect is strongest, at 14 percentage points, in households in which the wife is pension eligible (Table 3, third column). In these households, the household members retire jointly once the husband reaches pension eligibility age. However, the estimates are statistically significant at the 5% level only with conventional confidence intervals, but not with bias-corrected confidence intervals. The results in Table 4 suggest that the wife's retirement increases especially the pension-eligible husband's retirement probability (Table 4, column 3), although the effect is not quite statistically significant.

TABLE 3 Joint retirement effects. Husband as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
Joint retirement effect	0.095 */-	0.063 -/-	0.140 */-
Conventional 95% CI	[0.008, 0.182]	[-0.018, 0.143]	[0.009, 0.272]
Bias-corrected 95% CI	[-0.034, 0.228]	[-0.018, 0.225]	[-0.068, 0.331]
Bandwidth	0,550	0,693	0,930
Observations	51 584	52 564	16 766

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014a). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE 4 Joint retirement effects. Wife as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
Joint retirement effect	0.061 -/-	-0.055 -/-	0.081 -/-
Standard 95% CI	[-0.077, 0.199]	[-0.203, 0.094]	[-0.037, 0.2]
Bias-corrected 95% CI	[-0.172, 0.247]	[-0.169, 0.287]	[-0.09, 0.269]
Bandwidth	0,584	0,916	0,938
Observations	27 295	17 811	25 198

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014a). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

Appendix Figure A1 further illustrates our results tabulated in Tables 3 and 4. For readability, we exclude the retirees' retirement rates (first-stage effects) that were included in Figures 1 and 2.[18] The uppermost scatterplots illustrate the joint retirement effect when the spouse is pension eligible (delaying joint retirement effect), and the lowermost when the spouse is not pension eligible (advancing joint retirement effect). The middle scatterplots include all spouses. Similar to Table 3, the scatterplots show that the discontinuity appears more robust for pension eligible spouses. Figure A1 also shows that the retirement rates of pension eligible spouses are considerably higher than the retirement rates of non-pension eligible spouses. This is expected, given that the retiree's and the spouse's age variables are correlated.

[18] The first stage effects vary between 12 and 24 percentage points. The rdrobust package does not produce the confidence intervals for the first-stage effect, but the first-stage standard errors indicate that all the first-stage estimates are highly statistically significant.

4.4.2 Results by household type

Joint retirement effects may vary across households due to differences in financial incentives and decision-making dynamics between couples. Tables 5 and 6 report the joint retirement effects across the household types that were presented in Tables 1 and 2. The results indicate that the husband's retirement increases the retirement probability of the wife in high-earnings households (Table 5, panel B, columns 1 and 2). Similarly, the wife's retirement increases the retirement probability of the husband in low-earnings households (Table 6, panel A, columns 1 and 3). In particular, if the husband is older than 63 years, both the conventional and bias-corrected confidence intervals indicate strong statistical significance. The findings imply that the household's financial situation plays a significant role in the occurrence of joint retirement. In high-earnings households, the wife's retirement is advanced compared to her pension eligibility age, indicating that the household's accumulated wealth enables her to retire early. Voluntary pension policies are also more prevalent in high-earnings households, which may In low-earnings households, the husband's retirement is delayed compared to his pension eligibility age, perhaps reflecting low accumulated wealth and pensions that motivate the husband to continue to work.

Results also show that when the breadwinner husband retires, the wife's retirement probability increases (Table 5, panel C, columns 1 and 3). When the wife is pension eligible, the effect is statistically significant in terms of the conventional confidence interval but not in terms of the bias-corrected confidence interval. When the breadwinner wife retires, the joint retirement effect is evident provided that the husband is not yet pension eligible (Table 6, panel D, column 2). The findings indicate that the retiree's gender affects how the breadwinner position is used in household decision-making. One interpretation is that the husband uses his breadwinner position as a bargaining advantage to induce the wife to delay her retirement and continue to accumulate wealth. In contrast, the breadwinner wife may induce the non-breadwinner husband to retire early, since his work effort does not contribute as much to the household wealth.

TABLE 5 Joint retirement effects in various household earnings groups. Husband as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
A: Low-earnings household			
Joint retirement effect	0.055 -/-	0.019 -/-	0.066 -/-
Conventional 95% CI	[-0.055, 0.165]	[-0.095, 0.132]	[-0.125, 0.258]
Bias-corrected 95% CI	[-0.159, 0.172]	[-0.133, 0.207]	[-0.177, 0.409]
Bandwidth	0,648	0,633	0,919
Observations	30 588	24 577	7 651
B: High-earnings household			
Joint retirement effect	0.124 */*	0.098 */*	0.153 -/-
Conventional 95% CI	[0.013, 0.235]	[0.004, 0.193]	[-0.069, 0.374]
Bias-corrected 95% CI	[0.04, 0.376]	[0.011, 0.297]	[-0.064, 0.595]
Bandwidth	0,714	0,972	0,582
Observations	33 374	35 332	5 679
C: Husband breadwinner			
Joint retirement effect	0.118 */*	0.094 -/-	0.196 */-
Conventional 95% CI	[0.046, 0.19]	[-0.01, 0.199]	[0.028, 0.364]
Bias-corrected 95% CI	[0.014, 0.235]	[-0.013, 0.304]	[-0.149, 0.361]
Bandwidth	1,003	0,640	0,760
Observations	66 622	34 474	10 392
D: Wife breadwinner			
Joint retirement effect	0.05 -/-	0.06 -/-	0.001 -/-
Conventional 95% CI	[-0.053, 0.152]	[-0.093, 0.214]	[-0.319, 0.322]
Bias-corrected 95% CI	[-0.136, 0.191]	[-0.272, 0.183]	[-0.432, 0.536]
Bandwidth	1,040	0,584	0,797
Observations	26 679	12 824	3 514

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014a). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE 6 Joint retirement effects in various household earnings groups. Wife as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
A: Low-earnings household			
Joint retirement effect	0.156 */-	-0.052 -/-	0.207 */*
Conventional 95% CI	[0.055, 0.258]	[-0.243, 0.14]	[0.051, 0.364]
Bias-corrected 95% CI	[-0.062, 0.29]	[-0.232, 0.352]	[0.009, 0.48]
Bandwidth	1,165	0,869	0,798
Observations	27 027	8 782	10 583
B: High-earnings household			
Joint retirement effect	0.086 -/-	-0.076 -/-	-0.136 -/-
Conventional 95% CI	[-0.111, 0.282]	[-0.336, 0.185]	[-0.409, 0.138]
Bias-corrected 95% CI	[-0.548, 0.049]	[-0.385, 0.42]	[-0.834, -0.01]
Bandwidth	0,712	0,945	0,580
Observations	16 173	8 849	8 141
C: Husband breadwinner			
Joint retirement effect	0.018 -/-	-0.118 -/-	0.117 -/-
Conventional 95% CI	[-0.106, 0.143]	[-0.283, 0.046]	[-0.032, 0.266]
Bias-corrected 95% CI	[-0.14, 0.24]	[-0.374, 0.131]	[-0.227, 0.226]
Bandwidth	0,829	0,953	0,778
Observations	26 237	13 364	13 606
D: Wife breadwinner			
Joint retirement effect	0.188 -/-	0.405 -/*	0.151 -/-
Conventional 95% CI	[-0.066, 0.442]	[-0.004, 0.814]	[-0.116, 0.417]
Bias-corrected 95% CI	[-0.133, 0.63]	[0.099, 1.318]	[-0.477, 0.331]
Bandwidth	0,654	0,654	0,694
Observations	9 830	3 485	6 717

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014a). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

4.4.3 Robustness of the results

Fuzzy RD is an instrument variable (IV) version of the RD method and its results are based on several assumptions. First, the approach requires that the retiree's pension eligibility affects the spouse's retirement only through changes in the retiree's retirement status. We do not know of any other possible mechanisms. Second, a strong correlation between the instrument (retiree's pension eligibility) and the treatment variable (retiree's retirement rate) is required. This is evident in the high statistical significance of the first-stage estimates. Third, there must be no other way by which reaching age 63 for a retiree affects either the retiree's or spouse's retirement rate. There are no other known discontinuities at age 63 in

the Finnish social security system or other institutions that would affect the estimator, so the assumption is credible.

Fourth, the household members should be incapable to manipulate the retiree's age. We examine this by using the McCrary density test (McCrary 2008) that tests if the forcing variable's (retiree's age) density distribution is uneven around the cut-off age. Stata's *ddensity* package (Cattaneo et al. 2018) is used with its default options. The density test p-values of all households (first columns in Tables 3 and 4) are 0.353 for the sample of husbands and 0.146 for the sample of wives. The difference is not statistically significant and thus there is no evidence of sample manipulation or sample selection bias.

Fifth, the relations between the retiree's age and other observable variables on both sides of the cut-off age (63) must be smooth and continuous. This is tested by using the observable variables as outcome variables in RD estimations. Table A1 presents the RD estimates for several observable characteristics (education, local unemployment and number of children and grandchildren). The results indicate that there are certain statistically significant discontinuities in the variables. Most notably, there is an increase in the husband's pension eligibility probability at the cut-off age when the wife is the retiree (Table A1, first column). This could indicate that the retiree's pension eligibility correlates with the husband's pension eligibility, which could bias the results upwards. However, any such bias will not be present in the second and third columns of any of the results, since the samples are restricted to households with only non-pension-eligible spouses and pension-eligible spouses, respectively. In Tables A2 and A3, the spouse's pension eligibility and other variables are used as control variables to mitigate the potential bias from the husband's pension eligibility and other observable variables. The results in Tables 6 and A3 are similar, indicating that there are no such biases. The estimates in Table A2 are smaller and less often statistically significant compared to the estimates in Table 5. This suggests that the results in Table 5 should be interpreted with caution, even though the regression discontinuity estimates are, by construct, generally not subject to omitted variable bias.

Finally, we test the robustness of the results to different bandwidth choices. Tables A4 and A5 present the results estimated with bandwidths that correspond to 66% and 150% of the length of the chosen bandwidth for the results in Tables 5 and 6. The coefficients are similar, giving credibility to the original findings. In general, the estimates are not statistically significant with smaller (66%) bandwidths, probably due to the smaller sample sizes. Tables A6 and A7 report the results when the bandwidth is chosen separately on both sides of the cut-off. The results with the *two bandwidths* method are similar to the results with the regular *one bandwidth* method (Tables 5 and 6), even though there are great differences in the above and below cut-off bandwidth lengths.

4.5 Discussion

This study contributes to the literature by examining the joint retirement effect by earnings groups and by categorizing the effect into advancing and delaying joint retirement effects. We apply a fuzzy RD method to estimate such effects in Finland, with the earnings-related pension eligibility threshold at age 63 as the discontinuity point.

We find evidence of joint retirement effects in certain household groups. A wife's retirement increases the husband's retirement probability in the low-earnings households if the husband is already pension eligible. The result implies that because of low household earnings, it is optimal for the pension-eligible husband to delay his retirement and accumulate wealth and boost future pension by working until the wife reaches her pension eligibility age. As the wife reaches this age, the new optimal state for the household is both spouses' simultaneous retirement due to their joint leisure preference (Coile 2004). In high-earnings households, the wife's retirement does not increase a pension-eligible husband's retirement probability, but that does not imply that they have no preference for joint leisure. The husband's retirement at his own pension eligibility age might be more affordable, and the husband might quite likely retire at that point and wait for the joint retirement days rather than stay employed.

The second main finding is that the husband's retirement increases the wife's retirement probability in high-earnings households. Similarly, Blau and Riphahn (1999) and Kapur and Rogowski (2007) find that greater household wealth increases joint retirement probability. We find that the effect is most evident when the wife is not pension eligible herself. This advancing joint retirement effect suggests that high earnings enable the couple to accumulate greater wealth and future pensions, which makes it easier for the wife to leave employment at a younger age.

The findings suggest that the income effect dominates the substitution effect (De Preter et al. 2015; Queiroz and Souza 2017). Greater (lesser) wealth and pension accumulation from high (low) earnings incentivizes an advanced (delayed) joint retirement, even though the monetary return from continuing to work is high (low). From a policy perspective, if the earnings-related pension age is considered as the socially desired pension age, an increase in the progressivity of pension taxation (low pensions taxed less than now, high pensions taxed more than now) could reduce the advancing joint retirement effects among high-earnings households and the delaying joint retirement effects among low-earning households.

The results also indicate that there might be gender differences in how the breadwinner position affects joint retirement. The breadwinner wife's retirement advances the husband's retirement, while the breadwinner husband's retirement delays the wife's retirement. This suggests that the wives are altruistic towards their husbands, whereas the husbands use their bargaining power, owing to their breadwinner position, to persuade the wives to delay retirement. The latter result

is more in line with non-unitary household decision-making models and earlier results (Giovanis and Ozdamar 2018; Michaud and Vermeulen 2011), which indicate that an increase in family member's income increases the member's share of household resources, such as consumption goods and leisure. Findings by Radchenko (2016) support the gender-asymmetry in results. They show that an increase in a wife's wage increased both the wife's and the husband's household resources, whereas an increase in the husband's wage increased only his resources and decreased the wife's resources. It is possible that wives are more likely than husbands to behave according to unitary household decision-making models and exhibit altruism towards their spouse.

Noticeably, the delaying joint retirement effect in households with a breadwinner husband is not statistically significant in all robustness estimations. The effect may be partially tempered by the substitution effect of earnings, as the monetary return from hours spent on labour is greater for the breadwinner than for the non-breadwinner. Therefore, the breadwinner might exert bargaining power more on consumption goods than on labour market outcomes. This could also explain why husband's retirement is not delayed in households with a breadwinner wife. Supporting this, Giovanis and Ozdamar (2018) show that an increase in the wife's wage increases her share of the household resources while it also increases her labour supply.

Joint retirement effects are estimated with RD methodology for couples in which the retiree is approximately 63 years old. The external validity of results depends on how similar the joint retirement patterns are for couples in which the retiree is older or younger than 63 years. The farther away from this cutoff age, the weaker the external validity, but the degree of validity cannot be interpreted from the data. The sample is also restricted to couples in which the retiree works in the private sector. The external validity of results depends on whether retirement from public sector causes different joint retirement responses. In Finland, a significant portion of workforce, especially among women, works in the public sector, so the differences could be important. In both of these cases (age and sector), there are no clear hypothetical factors that would threaten the external validity.

The results in this study show how gender and financial situation contribute to differences in joint retirement effects. Due to the nature of the register data, the interpretation of the differences is based on economic theory and related empirical literature. Further studies could be helpful for better understanding the motives of family members' leading to these differences.

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APPENDIX

FIGURE A1 Spouse's (wife in left graph, husband in right graph) retirement rates by retiree's (husband in left graph, wife in right graph) age.

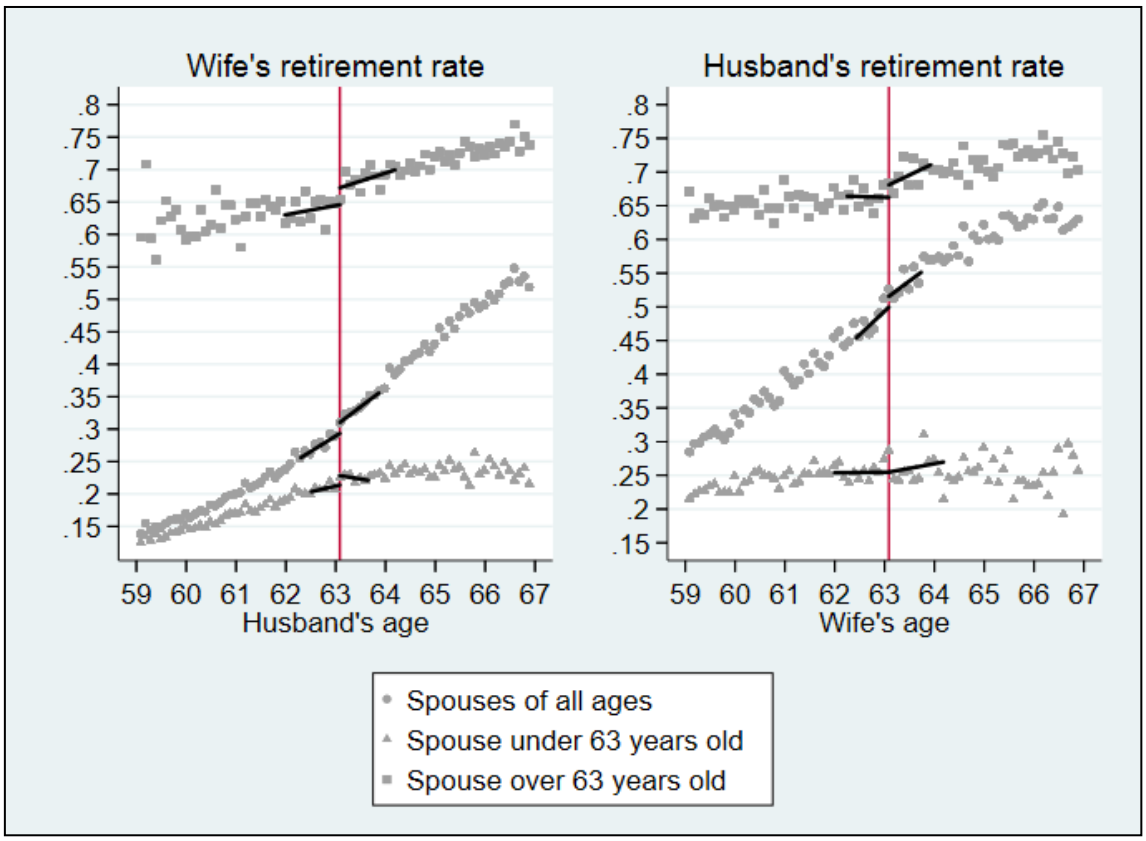

TABLE A1 Observable variable regression discontinuity estimates

	(1) Wife as the retiree	(2) Husband as the retiree
Spouse pension eligible	0.385 -/-	0.067 */*
	[-0.235, 1.006]	[0.002, 0.131]
	[-0.973, 1.096]	[0.009, 0.215]
Wife's primary education	-0.799 -/-	-0.068 -/-
	[-2.034, 0.435]	[-0.195, 0.059]
	[-3.306, 0.416]	[-0.17, 0.214]
Wife's secondary education	1.246 -/-	-0.013 -/-
	[-0.273, 2.766]	[-0.12, 0.095]
	[-2.183, 2.386]	[-0.411, -0.084]
Wife's tertiary education	-0.255 -/-	0.06 -/*
	[-0.946, 0.436]	[-0.038, 0.157]
	[-2.148, 0.489]	[0.044, 0.337]
Husband's primary education	-1.055 -/-	-0.013 -/-
	[-2.444, 0.334]	[-0.13, 0.105]
	[-4.196, -0.006]	[-0.12, 0.236]
Husband's secondary education	0.39 -/-	-0.032 -/-
	[-0.373, 1.152]	[-0.128, 0.064]
	[-0.943, 1.354]	[-0.183, 0.109]
Husband's tertiary education	0.674 -/*	0.038 -/-
	[-0.346, 1.694]	[-0.087, 0.164]
	[0.037, 3.114]	[-0.229, 0.152]
Local unemployment rate	-0.077 -/-	-0.005 -/-
	[-0.183, 0.03]	[-0.015, 0.006]
	[-0.226, 0.095]	[-0.012, 0.019]
Number of children	-0.967 -/-	-0.105 -/-
	[-2.8, 0.866]	[-0.267, 0.058]
	[-4.026, 3.141]	[-0.502, 0.142]
Number of grandchildren	-0.919 -/-	0.15 -/-
	[-3.115, 1.277]	[-0.111, 0.41]
	[-3.339, 4.058]	[-0.106, 0.684]
Observations	335 519	180 164

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A2 Joint retirement effects, estimations with control observable variables as controls: husband as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
A: All households			
Joint retirement effect	0.06 -/-	0.03 -/-	0.15 */-
Conventional 95% CI	[-0.02, 0.14]	[-0.01, 0.07]	[0.03, 0.26]
Bias-corrected 95% CI	[-0.03, 0.2]	[-0.04, 0.11]	[-0.06, 0.29]
Bandwidth	0,560	1,450	1,010
Observations	53 080	106 591	18 213
B: Low-earnings households			
Joint retirement effect	0.03 -/-	0 -/-	0.09 -/-
Conventional 95% CI	[-0.06, 0.12]	[-0.08, 0.08]	[-0.09, 0.26]
Bias-corrected 95% CI	[-0.12, 0.15]	[-0.14, 0.12]	[-0.15, 0.37]
Bandwidth	0,740	1,040	0,970
Observations	34 756	39 387	8 036
C: High-earnings households			
Joint retirement effect	0.06 -/-	0.15 -/-	0.1 -/-
Conventional 95% CI	[-0.04, 0.15]	[-0.01, 0.3]	[-0.11, 0.32]
Bias-corrected 95% CI	[0, 0.28]	[-0.08, 0.39]	[-0.07, 0.58]
Bandwidth	0,820	0,490	0,610
Observations	38 173	18 101	5 903
D: Husband breadwinner			
Joint retirement effect	0.07 */*	0.07 -/-	0.14 -/-
Conventional 95% CI	[0.02, 0.12]	[-0.01, 0.15]	[-0.02, 0.31]
Bias-corrected 95% CI	[0.02, 0.18]	[-0.04, 0.2]	[-0.07, 0.42]
Bandwidth	1,410	0,900	0,720
Observations	92 472	47 774	9 850
E: Wife breadwinner			
Joint retirement effect	0.01 -/-	-0.01 -/-	0.05 -/-
Conventional 95% CI	[-0.07, 0.09]	[-0.16, 0.14]	[-0.29, 0.38]
Bias-corrected 95% CI	[-0.16, 0.11]	[-0.29, 0.16]	[-0.58, 0.41]
Bandwidth	1,200	0,580	0,710
Observations	30 532	12 681	3 099

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A3 Joint retirement effects, estimations with control observable variables as controls: wife as the retiree

	(1) All Spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
A: All households			
Joint retirement effect	0.03 -/-	-0.07 -/-	0.11 */-
Conventional 95% CI	[-0.09, 0.15]	[-0.24, 0.1]	[0.01, 0.22]
Bias-corrected 95% CI	[-0.15, 0.22]	[-0.17, 0.35]	[-0.09, 0.24]
Bandwidth	0,610	0,790	1,000
Observations	28 353	15 309	26 598
B: Low-earnings households			
Joint retirement effect	0.16 */-	-0.1 -/-	0.22 */*
Conventional 95% CI	[0.03, 0.29]	[-0.3, 0.1]	[0.07, 0.38]
Bias-corrected 95% CI	[-0.05, 0.35]	[-0.18, 0.43]	[0.01, 0.47]
Bandwidth	0,690	0,820	0,790
Observations	16 492	8 375	10 459
C: High-earnings households			
Joint retirement effect	-0.11 -/-	-0.03 -/-	-0.1 -/-
Conventional 95% CI	[-0.34, 0.11]	[-0.41, 0.34]	[-0.39, 0.2]
Bias-corrected 95% CI	[-0.62, 0.06]	[-0.59, 0.57]	[-0.89, -0.01]
Bandwidth	0,570	0,650	0,540
Observations	13 068	6 052	7 513
D: Husband breadwinner			
Joint retirement effect	0.02 -/-	-0.16 -/-	0.1 -/-
Conventional 95% CI	[-0.1, 0.14]	[-0.33, 0.02]	[-0.06, 0.26]
Bias-corrected 95% CI	[-0.23, 0.13]	[-0.41, 0.13]	[-0.22, 0.27]
Bandwidth	0,730	0,860	0,660
Observations	22 924	12 239	11 520
E: Wife breadwinner			
Joint retirement effect	0.23 -/-	0.24 -/-	0.1 -/-
Conventional 95% CI	[-0.05, 0.52]	[-0.05, 0.54]	[-0.17, 0.38]
Bias-corrected 95% CI	[-0.25, 0.61]	[-0.02, 0.96]	[-0.51, 0.32]
Bandwidth	0,570	1,070	0,680
Observations	8 514	5 651	6 541

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A4 Joint retirement effects, estimation bandwidths corresponding to 66% and 150% of the chosen bandwidth in Table 6: husband as the retiree

	66% of the optimal bandwidth			150% of the optimal bandwidth		
	(1)	(2)	(3)	(4)	(5)	(6)
	All spouses	Spouse younger than 63 years	Spouse older than 63 years	All spouses	Spouse younger than 63 years	Spouse older than 63 years
A: All households						
Joint retirement effect	0.1 -/-	0.08 -/-	0.12 -/-	0.08 */-	0.06 -/-	0.15 */-
Conventional 95% CI	[-0.02, 0.21]	[-0.02, 0.19]	[-0.04, 0.28]	[0.02, 0.15]	[0, 0.12]	[0.05, 0.25]
Bias-corrected 95% CI	[-0.05, 0.28]	[-0.06, 0.25]	[-0.11, 0.37]	[-0.01, 0.2]	[-0.02, 0.17]	[-0.04, 0.29]
Bandwidth	0,360	0,460	0,610	0,830	1,040	1,400
Observations	33 846	34 649	11 150	77 225	76 913	24 908
B: Low-earnings households						
Joint retirement effect	0.01 -/-	0.02 -/-	0.09 -/-	0.05 -/-	0.02 -/-	0.06 -/-
Conventional 95% CI	[-0.11, 0.14]	[-0.11, 0.16]	[-0.15, 0.32]	[-0.03, 0.14]	[-0.07, 0.11]	[-0.08, 0.19]
Bias-corrected 95% CI	[-0.13, 0.23]	[-0.13, 0.27]	[-0.39, 0.35]	[-0.09, 0.16]	[-0.11, 0.16]	[-0.14, 0.3]
Bandwidth	0,430	0,420	0,610	0,970	0,950	1,380
Observations	20 061	16 138	5 090	44 985	36 148	11 420
C: High-earnings households						
Joint retirement effect	0.2 */-	0.14 */-	0.17 -/-	0.11 */*	0.05 -/*	0.23 */-
Conventional 95% CI	[0.05, 0.35]	[0.01, 0.27]	[-0.09, 0.44]	[0.03, 0.19]	[-0.02, 0.12]	[0.03, 0.42]
Bias-corrected 95% CI	[-0.04, 0.42]	[-0.01, 0.39]	[-0.23, 0.57]	[0.03, 0.31]	[0.02, 0.24]	[-0.18, 0.4]
Bandwidth	0,470	0,640	0,380	1,070	1,460	0,870
Observations	22 003	23 781	3 657	48 994	52 668	8 515
D: Husband breadwinner						
Joint retirement effect	0.13 */-	0.13 -/-	0.18 -/-	0.07 */*	0.08 -/-	0.15 */-
Conventional 95% CI	[0.04, 0.23]	[-0.01, 0.27]	[-0.02, 0.38]	[0.02, 0.13]	[0, 0.16]	[0.02, 0.28]
Bias-corrected 95% CI	[-0.02, 0.27]	[-0.07, 0.35]	[-0.2, 0.41]	[0.04, 0.21]	[-0.01, 0.23]	[0, 0.43]
Bandwidth	0,660	0,420	0,500	1,500	0,960	1,140
Observations	44 638	22 629	6 839	98 885	50 856	15 320
E: Wife breadwinner						
Joint retirement effect	0.06 -/-	0.01 -/-	0.11 -/-	0.02 -/-	0.01 -/-	0.1 -/-
Conventional 95% CI	[-0.09, 0.2]	[-0.16, 0.19]	[-0.22, 0.44]	[-0.06, 0.1]	[-0.11, 0.13]	[-0.13, 0.33]
Bias-corrected 95% CI	[-0.23, 0.21]	[-0.26, 0.25]	[-0.68, 0.32]	[-0.06, 0.18]	[-0.15, 0.21]	[-0.4, 0.34]
Bandwidth	0,690	0,390	0,530	1,560	0,880	1,200
Observations	18 076	8 470	2 321	39 678	19 025	5 168

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A5 Joint retirement effects, estimation bandwidths corresponding to 66% and 150% of the chosen bandwidth in Table 5: wife as the retiree

	66% of the optimal bandwidth	150% of the optimal bandwidth

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years	(4) All Spouses	(5) Spouse younger than 63 years	(6) Spouse older than 63 years
A: All households						
Joint retirement effect	0.00 -/-	0.03 -/-	0.08 -/-	0.08 -/-	0.04 -/-	0.11 */-
Conventional 95% CI	[-0.19, 0.19]	[-0.17, 0.22]	[-0.07, 0.23]	[-0.02, 0.18]	[-0.07, 0.14]	[0.03, 0.2]
Bias-corrected 95% CI	[-0.22, 0.36]	[-0.22, 0.38]	[-0.27, 0.19]	[-0.06, 0.25]	[-0.19, 0.16]	[-0.05, 0.23]
Bandwidth	0,390	0,600	0,620	0,880	1,370	1,410
Observations	17 807	11 726	16 962	40 733	27 234	36 514
B: Low-earnings household						
Joint retirement effect	0.11 -/-	0.01 -/-	0.24 */-	0.1 */*	0.05 -/-	0.19 */*
Conventional 95% CI	[-0.03, 0.25]	[-0.23, 0.25]	[0.03, 0.46]	[0.03, 0.18]	[-0.09, 0.18]	[0.07, 0.31]
Bias-corrected 95% CI	[-0.02, 0.38]	[-0.24, 0.49]	[-0.05, 0.6]	[0.05, 0.28]	[-0.27, 0.19]	[0.03, 0.44]
Bandwidth	0,770	0,570	0,530	1,750	1,300	1,200
Observations	18 395	5 718	7 046	40 891	13 175	15 281
C: High-earnings household						
Joint retirement effect	-0.13 -/-	0.00 -/-	-0.35 -/-	0.05 -/-	0.02 -/-	-0.04 -/-
Conventional 95% CI	[-0.41, 0.16]	[-0.38, 0.38]	[-0.76, 0.05]	[-0.09, 0.2]	[-0.16, 0.2]	[-0.24, 0.16]
Bias-corrected 95% CI	[-0.77, 0.09]	[-0.56, 0.61]	[-1.12, 0.07]	[-0.3, 0.2]	[-0.35, 0.24]	[-0.45, 0.16]
Bandwidth	0,470	0,620	0,380	1,070	1,420	0,870
Observations	10 725	5 846	5 438	23 972	13 803	12 099
D: Husband breadwinner						
Joint retirement effect	0.00 -/-	-0.1 -/-	0.07 -/-	0.08 -/-	0.01 -/-	0.1 -/-
Conventional 95% CI	[-0.16, 0.16]	[-0.32, 0.12]	[-0.12, 0.26]	[-0.01, 0.17]	[-0.1, 0.13]	[-0.01, 0.22]
Bias-corrected 95% CI	[-0.29, 0.2]	[-0.5, 0.18]	[-0.32, 0.25]	[-0.14, 0.18]	[-0.32, 0.05]	[-0.16, 0.24]
Bandwidth	0,550	0,630	0,510	1,240	1,430	1,170
Observations	17 318	8 830	9 077	38 478	20 617	19 723
E: Wife breadwinner						
Joint retirement effect	0.28 -/-	0.73 -/-	-0.03 -/-	0.21 */-	0.22 -/*	0.15 -/-
Conventional 95% CI	[-0.16, 0.72]	[0, 1.46]	[-0.58, 0.53]	[0.03, 0.39]	[-0.09, 0.52]	[-0.05, 0.35]
Bias-corrected 95% CI	[-0.65, 0.67]	[-0.77, 1.35]	[-1.16, 0.54]	[-0.05, 0.5]	[0.02, 0.95]	[-0.24, 0.39]
Bandwidth	0,430	0,430	0,460	0,980	0,980	1,040
Observations	6 431	2 257	4 452	14 544	5 197	9 829

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A6 Joint retirement effects, estimations with separate bandwidths selected on both sides of the cut-off value: husband as the retiree

	(1)	(2)	(3)

	All spouses	Spouse younger than 63 years	Spouse older than 63 years
A: All households			
Joint retirement effect	0.092 */-	0.086 -/-	0.155 */-
Conventional 95% CI	[0.004, 0.18]	[-0.007, 0.179]	[0.016, 0.294]
Bias-corrected 95% CI	[-0.042, 0.223]	[-0.079, 0.2]	[-0.115, 0.306]
Bandwidth(below/above)	0.813/0.484	0.971/0.443	0.841/0.856
Observations	61 985	56 029	15 399
B: Low-earnings households			
Joint retirement effect	0.02 -/-	0.003 -/-	0.072 -/-
Conventional 95% CI	[-0.088, 0.128]	[-0.108, 0.115]	[-0.098, 0.241]
Bias-corrected 95% CI	[-0.141, 0.196]	[-0.14, 0.199]	[-0.194, 0.327]
Bandwidth(below/above)	1.203/0.532	1.449/0.516	0.896/1.05
Observations	41 113	40 155	8 295
C: High-earnings households			
Joint retirement effect	0.16 */*	0.16 */-	0.167 -/-
Conventional 95% CI	[0.036, 0.283]	[0.024, 0.295]	[-0.039, 0.374]
Bias-corrected 95% CI	[0.013, 0.386]	[-0.086, 0.324]	[-0.11, 0.508]
Bandwidth	0.936/0.494	1.003/0.481	0.689/0.79
Observations	34 147	28 976	7 319
D: Husband breadwinner			
Joint retirement effect	0.136 */-	0.122 -/-	0.22 */-
Conventional 95% CI	[0.031, 0.241]	[-0.007, 0.251]	[0.075, 0.365]
Bias-corrected 95% CI	[-0.038, 0.28]	[-0.116, 0.272]	[-0.062, 0.389]
Bandwidth (below/above)	0.903/0.48	0.94/0.377	0.745/1.209
Observations	47 777	37 673	14 275
E: Wife breadwinner			
Joint retirement effect	0.01 -/-	0.022 -/-	-0.032 -/-
Conventional 95% CI	[-0.106, 0.126]	[-0.099, 0.142]	[-0.273, 0.209]
Bias-corrected 95% CI	[-0.097, 0.268]	[-0.164, 0.203]	[-0.438, 0.294]
Bandwidth (below/above)	1.189/0.79	1.284/0.584	0.519/1.265
Observations	25 804	20 934	4 482

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.

TABLE A7 Joint retirement effects, estimations with separate bandwidths selected on both sides of the cut-off value: wife as the retiree

	(1) All spouses	(2) Spouse younger than 63 years	(3) Spouse older than 63 years
A: All households			
Joint retirement effect	0.139 -/-	0.15 -/*	0.057 -/-
Conventional 95% CI	[-0.012, 0.291]	[-0.056, 0.356]	[-0.088, 0.203]
Bias-corrected 95% CI	[-0.044, 0.426]	[0.024, 0.641]	[-0.155, 0.298]
Bandwidth(below/above)	1.148/0.391	1.94/0.397	1.089/0.562
Observations	38 189	32 748	22 196
B: Low-earnings households			
Joint retirement effect	0.124 */*	-0.022 -/-	0.214 */*
Conventional 95% CI	[0.014, 0.234]	[-0.208, 0.164]	[0.052, 0.376]
Bias-corrected 95% CI	[0.009, 0.347]	[-0.135, 0.454]	[0.008, 0.495]
Bandwidth(below/above)	1.498/0.925	1.278/0.814	1.013/0.668
Observations	30 154	11 605	11 037
C: High-earnings households			
Joint retirement effect	0.03 -/-	0.147 -/-	-0.137 -/-
Conventional 95% CI	[-0.188, 0.248]	[-0.127, 0.421]	[-0.404, 0.129]
Bias-corrected 95% CI	[-0.428, 0.234]	[-0.524, 0.356]	[-0.634, 0.168]
Bandwidth (below/above)	0.975/0.542	1.066/0.722	0.939/0.553
Observations	18 025	9 076	10 412
D: Husband breadwinner			
Joint retirement effect	0.083 -/-	-0.053 -/-	0.081 -/-
Conventional 95% CI	[-0.038, 0.203]	[-0.203, 0.098]	[-0.073, 0.236]
Bias-corrected 95% CI	[-0.203, 0.172]	[-0.447, 0.048]	[-0.24, 0.246]
Bandwidth (below/above)	1.029/0.758	1.037/1.059	1.133/0.667
Observations	28 674	14 530	15 417
E: Wife breadwinner			
Joint retirement effect	0.387 */-	0.796 */-	0.156 -/-
Conventional 95% CI	[0.074, 0.701]	[0.198, 1.394]	[-0.134, 0.447]
Bias-corrected 95% CI	[-0.179, 0.771]	[-0.548, 1.262]	[-0.445, 0.446]
Bandwidth (below/above)	1.553/0.469	1.232/0.442	1.026/0.566
Observations	17 012	5 386	7 708

Note: -/- denotes that the estimate is not statistically different from zero at the 5% p-value level for either conventional or bias-corrected confidence intervals. */- denotes that the estimate is statistically significant only for the conventional confidence interval, -/* only for the bias-corrected confidence interval and */* for both. The bandwidth was chosen using the bandwidth selection method by Calonico et al. (2014). Number of observations refers to the number of observations included in the estimation with the chosen bandwidth. Uniform kernel and household clustered standard errors are applied.