INTERACTIONS IN PUBLIC POLICIES: SPOUSAL RESPONSES AND PROGRAM SPILLOVERS OF WELFARE REFORMS*

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In this paper we exploit the introduction of an early retirement reform in Norway to provide new evidence on interactions in public policies across programs and household members. The analysis generates four results. First, the reform decreased the employment of the directly affected individuals. Second, the introduction of the early retirement option caused program substitution away from alternative welfare programs. Third, it reduced employment among spouses of directly affected individuals. Finally, the reduction in spousal employment was driven by take-up of disability insurance. These results demonstrate that interactions in public policies can have a substantial impact on the effect of welfare reforms.

Most countries operate a complex web of social insurance programs aimed at promoting the health and wellbeing of their citizens. These programs, ranging from disability and unemployment insurance to old age pension, are subject to continual reform in response to changing societal needs. However, anticipating individuals' response to such reforms is difficult due to potential cross-program spillovers: changes to one program may affect individual eligibility and take-up of other programs. In addition, individual participation in specific programs may generate indirect responses from other household members due to factors such as complementarities in labour supply and leisure. Little work has been able to comprehensively examine the extent and magnitude of such interactions.

The goal of this paper is to move beyond the existing literature in understanding interactions in public policies across programs and household members. While a small and growing literature examines the cross-program spillovers of government welfare programs (e.g., Vestad, 2013; Hernæs *et al.*, 2016; Elwell, 2019; Brown *et al.*, 2020; Johnsen and Reiso, 2020), and a rapidly growing literature examines household complementarities in labour supply and leisure (e.g., Kaygusuz, 2015; Lalive and Parrotta, 2016; Stancanelli, 2017; Selin, 2017; Sánchez-Marcos and Bethencourt, 2018), this is the first paper to trace the effect of a welfare reform across both programs and spouses. The main contribution of this paper is to develop, expand and merge these two literatures, and to demonstrate that analyses focusing exclusively on the direct effect of welfare reforms may misestimate the full impact of the reform.

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The setting of our study is Norway, and the reform we exploit is an early retirement reform implemented between 1989 and 1998. This reform lowered the age requirement for retirement from 67 to 62 years for workers in some firms, while maintaining the age requirement of 67 years in other firms. Using population-wide registry data, we investigate (1) the direct effect of the reform on individual labour market behaviour, (2) the cross-program effect of the reform on individual participation in other social insurance programs, and (3) the indirect spillover effect of the reform on the labour market and social insurance participation of the individuals' spouses.

Current demographic changes (population ageing and declining fertility) coupled with large-scale pension reforms across the globe make it especially important to understand how pension policies interact with existing social insurance programs. Specifically, over the last decade, all OECD countries have reformed parts of their pension systems (OECD, 2013). A limited understanding of the cross-program and cross-household member effects of these policy changes may result in a misestimation of their full impact.

We begin by examining how the reform affected the early retirement decision of individuals in affected firms, and whether any potential increase in early retirement take-up had an offsetting effect on the take-up of other social insurance programs: sick leave, disability insurance and unemployment insurance. We focus on these programs because they represent alternative exit routes—both temporary and permanent—from the labour market that may become less attractive to individuals who qualify for early retirement.

Having identified the direct effect of the reform and shown the existence of important cross-program spillovers, we investigate whether the change in retirement behaviour and social insurance participation among individuals directly affected by the reform had an indirect effect on the labour market outcomes of their spouses. Specifically, we examine whether spouses of individuals directly affected by the reform become more likely to leave the labour force themselves, and if so, whether this has an impact on their take-up of alternative social insurance programs. Such labour supply complementarities have been proposed as a potential reason for why micro- and macroeconomic labour supply elasticity estimates often differ (e.g., Chetty *et al.*, 2011; Chetty, 2012; Goux *et al.*, 2014).

To perform our analysis, we compare the outcomes of individuals (and their spouses) in the firms that participate in the early retirement scheme to those that do not using a difference-in-differences approach. For ease of exposition, we talk about men and women when discussing the direct effects of the reform, and we talk about male spouses (husbands) and female spouses (wives) when discussing the indirect effects of the reform on the spouses of directly affected individuals.

The variation we exploit comes from changes in outcomes among individuals employed at firms affected by the reform compared to individuals employed at firms not affected by the reform. To account for potential selection into working for a firm covered by the reform, we assign treatment based on the pre-reform firm affiliation of individuals. By using this approach to examine the direct effect of the reform on own program take-up, cross-program spillovers, the indirect effect on spousal labour supply and the indirect effect on spousal participation in other social insurance programs, we provide novel insights into the importance of accounting for interactions in public policies when designing welfare reforms.

The main identifying assumption underlying our analysis is that there are no secular trends, policies or shocks concurrent with the reform that differentially affect the outcomes of workers and their spouses in treatment and control firms. We provide extensive evidence that our estimates are unlikely to be driven by such factors. First, we present event study results that test for the

existence of pre-treatment trends across outcomes. Second, we perform a placebo test on younger workers to ensure that we are not identifying effects off of secular trends. Third, we perform a battery of sensitivity analyses on our sample selection and model specification.

We present four key findings. First, we show that the reform had a substantial impact on the labour supply of individuals who became eligible for early retirement, reducing the probability of employment by 30%. Second, we find that the increased take-up of early retirement reduced enrolment in alternative social insurance programs, most notably in disability insurance. Third, we reveal that the reform had an indirect negative effect on the employment of the spouses of individuals directly affected by the reform, with an effect size of approximately 5.5%. A back-of-the-envelope calculation suggests that the social multiplier in this setting is around 1.17, demonstrating that focusing only on individuals directly affected by the reform would substantially underestimate its full impact.[1] Finally, we show that the indirect labour force participation effect among spouses is accompanied by a significant increase in spousal take-up of disability insurance.

We document interesting effect heterogeneity across genders. In particular, while the reform had a large positive impact on the early retirement take-up among both men and women, the effects are larger for men and the cross-program spillover with respect to disability insurance is only present among men. This is consistent with the fact that fewer women qualify for early retirement due to eligibility requirements related to earnings history, such that their ability to substitute across programs is smaller. It is also consistent with the fact that the relative benefit of substituting from disability insurance to early retirement is lower for women. The reason for this is that the compensation rate for disability insurance is greater than the compensation rate for early retirement for low-income individuals, and most of the women in our sample have lower incomes than do the men.

To examine if gender differences in earnings history and current income can explain the differences across genders, we re-estimate our preferred specification for men, weighting each observation by the share of women with similar earnings histories and current earnings. This allows us to examine what the effect of the reform on men would have been had their earnings histories and current earnings been similar to that of the women. The results from this exercise support our hypothesis: the reform would have had no cross-program substitution effect on men had their earnings histories been similar to that of the women in our analysis.

A second cross-gender heterogeneity effect we identify is that the indirect effect of the reform on spouses is present only among females, especially with regard to the take-up of disability insurance. That is, the female spouses of men directly affected by the reform are more likely to leave the labour market and take up disability insurance, but the male spouses of women directly affected by the reform do not appear to respond. We speculate that this is because most female spouses in our sample are secondary household earners, such that the household cost associated with male spouses leaving the labour force is much greater.[2]

To explore this possibility, we estimate a modified version of our main specification in which we interact the treatment variable with the within-household income gap. When we fix the income

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[1] The social multiplier is the ratio of the aggregate effect to the direct effect on the individual (Glaeser *et al.*, 2003).

[2] To the extent that the direct effect of early retirement eligibility on employment also represents a household decision, which may be the case, this argument would also imply that the direct effect should be larger for women eligible for early retirement than for men eligible for early retirement (as their share of total household income is on average smaller in our sample). This is consistent with our findings: While the magnitude of the direct effect on employment is similar in absolute terms for men and women, a larger share of women do not fulfil the individual eligibility criteria for early retirement (see Sections 1 and 3), such that the direct effect scaled by the fraction eligible is larger for women than men.

gap at zero, we find significant effects on employment and early retirement for male spouses. The results also reveal a positive (but not statistically significant) coefficient on disability insurance take-up for male spouses. These results are consistent with differences in the household cost of joint exit driving the gender difference in spousal response.

This paper contributes to the existing literature in several ways. First, several models of household decision-making allude to the importance of co-determination in decisions related to labour supply (Goux *et al.*, 2014; Hospido and Zamarro, 2014), health behaviours (McGeary, 2015; Fletcher and Marksteiner, 2017) and social insurance (Witman, 2015; Boyle and Lahey, 2016). However, no paper has examined how an individual's participation in a specific welfare program affects the spouse's participation in other programs. By focusing on such indirect cross-program spousal effects, we contribute to the literature and improve our knowledge on the importance of co-determination in household decisions.

Second, there is a rich literature on interdependencies in couples' retirement. The earlier research in this field exploits cross-sectional data to examine spousal correlation in retirement decision (Hurd, 1990; Zweimüller *et al.*, 1996; Blau, 1998; Coile, 2004), but the lack of exogenous variation in retirement eligibility makes it difficult to interpret these results as causal.[3] A more recent strand of research has made use of exogenous variation in pension eligibility driven by retirement laws and policy reforms to investigate the impact of retirement on spousal employment and home production (e.g., Baker, 2002; Kapur and Rogowski, 2007; Stancanelli and Van Soest, 2012; Atalay and Barrett, 2015; Cribb *et al.*, 2016; Lalive and Parrotta, 2016; Selin, 2017; Stancanelli, 2017; Bloemen *et al.*, 2019; Kruse, 2021).[4] While the results from this literature are mixed, most studies find evidence of interdependencies in couples' retirement decisions.[5] Our contributions to this literature are two-fold. First, we provide complementary evidence on the interdependencies in couples' retirement by studying a reform that generated a more substantial shift in retirement eligibility. This enables us to better isolate the spousal spillover effects. Second, we provide the first evidence on which exit routes spouses take if they decide to exit in response to the reform. That is, do they simply leave the labour force or do they exit through other social insurance programs? These results have important policy implications. Specifically, they show that neglecting the interactions of public policies across programs and household members can result in a sizable underestimation of the total cost of welfare reforms.

Finally, our setup enables us to develop the literature on cross-program spillovers of government welfare programs. A number of studies have explored how changes to the eligibility requirements of specific welfare programs, such as Medicaid in the United States, affect the eligibility and take-up of other safety net programs (Shore-Sheppard, 2008; Decker and Selck, 2012; Baicker

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[3] In addition to examining the joint retirement decision of couples, a number of studies have examined the existence of couples' joint employment decisions. For example, Schirle (2008) exploited cohort differences in the participation rates of older women and showed that a wife's labour force participation decision positively affects her husband's participation decision. Using data from Australia, Mavromarasa and Zhub (2015) documented a similar relationship between a wife's labour force participation decision and her husband's participation decision.

[4] Another related set of papers has examined the effects of altering the spousal and survivor benefits of the US social security system (e.g., Kaygusuz, 2015; Sánchez-Marcos and Bethencourt, 2018), and found that the elimination of these policies leads to increases in the labour force participation of married females.

[5] The mixed results are primarily related to asymmetry in the way spouses react to each other's incentives, with some studies finding that husbands respond to their wives' retirement decisions but not that wives respond to their husbands' retirement decisions (e.g., Stancanelli and Van Soest, 2012; Atalay and Barrett, 2015; Cribb *et al.*, 2016), while other studies find that wives respond to their husbands' retirement decisions but not that husbands respond to their wives' retirement decisions (e.g., Lalive and Parrotta, 2016; Kruse, 2021). The exception is Selin (2017), who found no evidence of spousal spillovers. However, Selin (2017) examined a retirement reform that primarily affected women, and is therefore restricted to looking at whether husbands respond to wives' retirement decisions.

et al. 2014; Elwell, 2019; Brown *et al.* 2020). With respect to program spillovers of pension reforms, several studies have found that reforms that reduce the access to, or generosity of, retirement programs lead to increased take-up of alternative exit routes from the labour market (Røed and Haugen, 2003; Bratberg *et al.*, 2004; Duggan *et al.* 2007; Staubli and Zweimuller, 2013; Vestad, 2013; Hernæs *et al.*, 2016). Our contribution to this literature is to show that such spillovers are not limited to those individuals affected by the reform, but also extend to other members of the household.

1. Background[6]

1.1. *The Norwegian Pension System and the Early Retirement Reform*

Prior to the introduction of the early retirement reform, the Norwegian old age pension system consisted only of a pay-as-you-go public state pension available to all permanent residents aged 67 years or older.[7] This retirement age is higher than the current retirement age in most other European countries, and the labour force participation among older workers in Norway is high. During our analysis period (1993 through 2007), around 70% of men, and 60% of women, aged 55 to 64 years were employed.[8]

The public pension is managed by the Norwegian Labour and Welfare Administration. The pension comprises two main components. First, a fixed pension paid in full to individuals who have resided in Norway for at least forty years, and gradually reduced for individuals with shorter resident histories.[9] Second, an earnings-based pension paid to individuals with at least three years of employment history, in which earnings translate to pensions at a rate that decreases with earnings until it eventually reaches a cap. As a consequence, the pension replacement rate declines with earnings.

The early retirement (ER) scheme was introduced on 1 January 1989. This was the result of a comprehensive collective bargaining agreement between the Norwegian Confederation of Trade Unions (the largest umbrella organisation of labour unions in Norway) and the Confederation of Norwegian Enterprise (the largest employers' organisation in the country), co-sponsored by the government. The objective of the reform was to provide 'worn-out' workers with a dignified exit route from the labour market. Specifically, prior to the introduction of this scheme there was no official exit route available to individuals under the age of 67 years. If these individuals were interested in exiting the labour market, they either had to exit without receiving benefits or exit through the use of alternative welfare programs such as disability insurance (DI), unemployment insurance (UI) or paid sick leave (SL). While it was not the explicit goal of the reform to reduce enrolment in these alternative programs, the policymakers did anticipate a potential movement from these programs to the ER scheme. The scheme was initially introduced with an early retirement age of 66 years, but the age limit has since been reduced to 62 through reforms implemented in 1990, 1994, 1997 and 1998.

The ER provision applies to all workers employed in firms with a collective bargaining agreement that includes the ER scheme as part of the agreement. While public sector coverage has been 100% since the introduction of the scheme, private sector coverage is lower, but has

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[6] In this section we describe the Norwegian social insurance programs during our analysis period (1993–2007).

[7] Private pensions play a marginal role in Norway.

[8] In comparison, the average OECD employment rate among individuals aged 55–64 was 44% in 2000.

[9] The fixed pension is indexed annually by expected national income growth. All pensioners are guaranteed a certain minimum amount.

Table 1. *The Importance of ER and Non-ER Firms in the Norwegian Economy.*

	Public ER firms	Private ER firms	Non-ER firms
Number of firms	14,512	11,860	129,330
Share of total employees	32.3%	28.1%	39.6%
Share of total GDP	30.2%	31.5%	38.2%
Workers covered by industry			
Primary	8.5%	16.8%	74.7%
Manufacturing	1.2%	75.2%	23.6%
Construction	13.7%	35.1%	51.2%
Services and trade	2.7%	27.3%	70.0%
Transport and communication	26.4%	31.0%	42.6%
Finance and business	9.3%	32.6%	58.1%
Administration	90.3%	0.3%	9.3%
Education	89.7%	0.6%	9.6%
Health	73.5%	2.9%	23.6%

Notes: The table presents summary statistics for public and private firms participating in the ER scheme and private firms not participating in the ER scheme. Public firms are identified using NACE industry codes, while the identification of private firms' affiliation with the ER scheme is through backward induction: firms are classified as participating in the scheme if at least one employee left the firm on ER benefits. All data are from the 1998 Norwegian employer-employee registers. We define total GDP as total income received by the full population of Norwegian employees, as observed in the employer-employee registers. Industries are defined using NACE industry codes in the employer-employee register.

increased over time. Table 1 shows the importance of ER and non-ER firms in the Norwegian economy in 1998: out of everyone employed, 40% worked in non-ER firms, 32% worked in public ER firms and 28% worked in private ER firms. Coverage rates are higher for workers in the administrative, education, health and manufacturing sectors, and lower for workers in services and trade, and finance and business. All workers in ER firms—irrespective of their union membership status—are eligible for early retirement, provided they meet certain conditions. In particular, individual eligibility is based on lifetime earnings history. In addition, eligibility requires three or more years of work experience at the firm. This makes post-reform sorting into ER firms difficult as job mobility is relatively low among older workers.

The retirement benefits under the ER scheme are equivalent to what the individuals would have received as public state pension from age 67 had they continued in employment until that age, plus a net-of-tax annual bonus of approximately $2,000. Employers cover the full cost of ER pensions for retirees aged 62 and 63, and 60% of pensions for retirees aged 64 to 66. Public funding covers the remaining 40%. Benefit consists of a fixed amount plus an income-based amount. The net replacement rate is approximately 65% for the median earner, very similar to the replacement rate for the old age state pension.[10]

It is important to note that the use of the ER scheme has no impact on the size of the individuals' public pensions that they transition to at age 67. The reason is that the public pension benefits are calculated as if the ER retiree was a full wage earner during the early retirement period. It should also be noted that ER benefits are conditional on withdrawal from employment. Thus, the ER scheme provides a strong work disincentive for ER-eligible workers.

ER benefits are 25% lower for married individuals compared to unmarried individuals. Conditional on marital status, there are no spousal links in the benefit eligibility or replacement rates

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[10] The ER program has been subject to additional changes over the years. The most recent took place in 2011 and removed a confiscatory earnings test among early retirement pensioners from the private sector, making it more financially attractive to continue working, as opposed to the negative work incentive effect studied in our setting. Bratsberg and Stancanelli (2018) and Kruse (2021) examined the joint retirement effects of the 2011 reform.

in the ER scheme. The exact same rules apply to the DI scheme. As we focus exclusively on married individuals in this paper, this does not pose a concern for our analysis.

1.2. *Alternative Pathways to Early Retirement*

All permanent residents of Norway are automatically enrolled in the public social insurance system, known as the National Insurance Scheme. This system is financed through a national insurance contribution imposed on both employers and employees. The employee's contribution is equivalent to 7.8% of his/her earnings, levied as an automatic payroll deduction. Apart from married individuals receiving a 25% lower fixed amount in the ER and DI schemes, there is no spousal link in benefit eligibility or replacement rates in the National Insurance Scheme. Therefore, any potential indirect spousal responses we identify cannot be driven by interdependencies in the National Insurance Scheme.

Participation in the National Insurance Scheme is mandatory. The system encompasses several welfare programs ranging from old age pension and health-related social insurance to transitional benefits for survivors and funeral grants. We focus on three programs: DI, UI and SL. These programs are the largest (non-pension) social insurance programs in Norway and represent clear exit routes—both temporary and permanent—from the labour market.[11]

SL benefits provide compensation for income loss caused by a temporary illness or injury. Thus, individuals will receive SL even if they are employed. The replacement rate is 100% from day one. Initially, it was subject to a maximum amount ($62,000 in 2019), but large groups of employees, including the entire public sector, have negotiated 100% compensation without any cap. To be entitled to SL benefits, an individual must have been in employment for the past four weeks. SL beyond three days requires a certificate from a doctor. SL benefits are paid by the employer for the first sixteen days, and then by the government for a maximum of fifty-two weeks.[12]

DI is the largest (non-pension) welfare program in Norway and is provided to those who experience an injury or disability that causes a permanent reduction in earnings capacity. For the vast majority, the route to DI benefits goes through one year of sick leave. To receive DI benefits, a doctor must certify that the individual has attempted all treatments that could help improve their work ability. During our analysis period, DI benefits are equivalent to what the individuals would have received as public state pension from age 67 had they continued in employment until that age. Similar to the old age pension, the DI replacement rate depends on an individual's pre-DI earnings. The after-tax replacement rate can be above 100% for low-income groups but is decreasing in income. The after-tax replacement rate for fully disabled, previously average earners, is around 65% (Blöndal and Pearson, 1995).

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[11] Online Appendix Table A1 provides summary statistics of the lifetime earnings and education attainment of individuals aged 62 through 66 stratified by welfare program participation. Individuals on ER, DI, UI and SL have lower lifetime earnings and educational qualifications than individuals who are employed. Across individuals in the various welfare programs, individuals on ER and SL have slightly higher lifetime earnings than those on UI and DI, and individuals on ER and UI have slightly higher educational attainment than those on DI and SL. However, these differences are relatively modest.

[12] After the sick leave period expires, individuals can apply for rehabilitation benefits, a time-limited extension to sick leave (but with benefits reduced from 100% to 66%) intended to provide support rehabilitation to facilitate reintegration into the labour market. Even though disability insurance is conditional on individuals having attempted (and failed) rehabilitation, rehabilitation success is considered highly unlikely among the elderly, and the application for disability insurance is usually prepared before or immediately after their sick leave expires.

UI is available to individuals who have had their work hours reduced by at least 50%, are registered as jobseekers and submit an employment status form every fourteen days and had an income over a certain minimum amount ($16,500 in 2019) before becoming unemployed. Thus, individuals can receive UI benefits even if they are employed. The replacement rate is 62% of the annual income the person received before becoming unemployed. The standard entitlement period is 186 weeks for most of our analysis period.[13] Once an individual turns 64 years old, the time restriction on UI benefits is removed, such that the individual can keep receiving UI benefits until reaching the retirement age of 67 (at which point the UI benefits are replaced by public pension benefits).

DI, SL and UI all represent potential exit routes from the labour market among older workers who do not yet qualify for retirement. However, some of these programs are more likely exit routes than others. First, sick leave is only offered on a temporary basis and for a maximum of fifty-two weeks, making this an unlikely choice for someone looking to permanently leave the labour force. Second, for the average earner, the unemployment insurance scheme is less generous than other aspects of the welfare system and is traditionally not used to the same extent as other programs in the social insurance system. There is also an inconvenient time cost as applicants must be registered as jobseekers and submit employment status forms every fourteen days. Finally, disability insurance is associated with generous benefit levels and provides a permanent exit from the labour market without imposing time restrictions or follow-up requirements similar to the other programs. In addition, previous research has found a strong relationship between job loss and the take-up of disability insurance (Bratsberg *et al.*, 2013). Indeed, while the share of individuals on sick leave and unemployment benefits is low and relatively constant across all age groups in Norway, the number of people on disability insurance is substantially higher for individuals close to retirement, suggesting that this may represent an important alternative path to early retirement (Figure 1).

1.3. *Conceptual Framework*

In this section, we present a simple model of an individual's retirement decision and how this affects the spouse's labour market behaviour. This model is intended as a framework for informing our empirical analyses and developing hypotheses; detailed structural models on the topic are available elsewhere (e.g., Maestas, 2001; Gustman and Steinmeier, 2004; Casanova, 2010).

Our starting point is an individual choosing between three alternative labour market states: work (W), retire (R) or exiting the labour force through other welfare programs such as DI, UI or SL (O). We assume that these states are mutually exclusive and collectively exhaustive, and that each state is associated with a different monetary reward (W yields wage w, R yields retirement benefits r and O yields other welfare benefits o). We assume that the individual receives no leisure if working, but full leisure if not working.

Utility of individual i is given by $\alpha_i c_i + \beta_i l_i$, where c_i is consumption and l_i is leisure. Consumption is equal to income and leisure is either 0 or 1. We normalise α_i to 1 without loss of generalisability. Individual i's maximisation problem is $\max_{W,R,O}\{(w_i), (r_i + \beta_i), (o_i + \beta_i)\}$.

Individual i will choose to retire if the utility from retiring ($r_i + \beta_i$) is greater than the utility from working (w_i) and from exiting on other welfare programs ($o_i + \beta_i$). That is, individual i will choose R if (a) the relative preference for leisure over consumption is greater than the income

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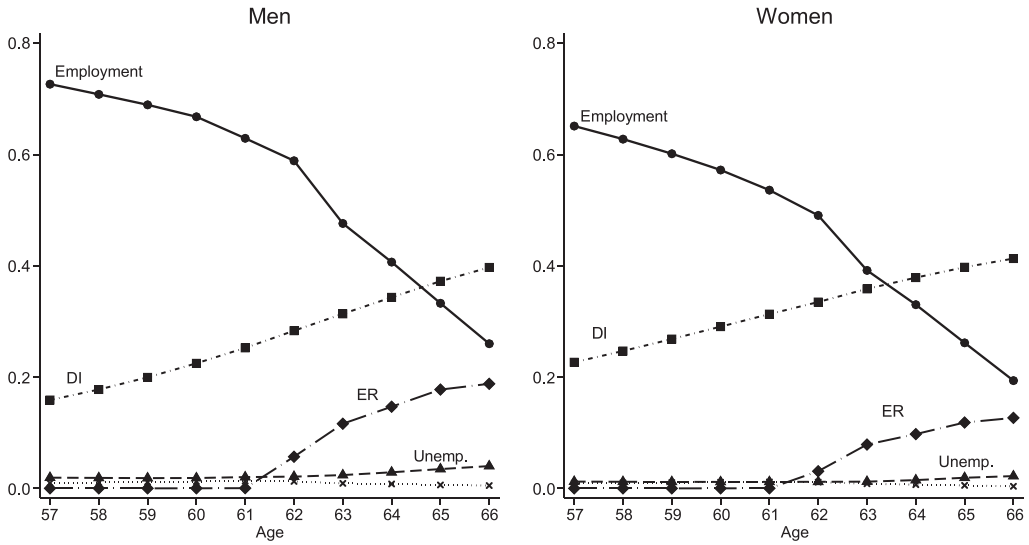[13] In 2004, the entitlement period was reduced from 186 to 104 weeks.

Fig. 1. *Employment and Take-Up of Social Welfare Benefits among the Elderly Norwegian Population.*
Notes: The figure presents the employment and take-up of various social welfare policies for the full
Norwegian population of men (left) and women (right) between the ages of 57 to 66. The dots represent the
average share of individuals of a given age in employment from 1993 to 2007 (our observation period). The
squares, diamonds, triangles and crosses represent the corresponding shares on disability insurance, early
retirement benefits, unemployment benefits and paid sick leave benefits. The data on employment are from
tax registers and the data on take-up of social welfare policies are from national social welfare registers.

difference between the wage and the retirement benefits ($\beta_i > w_i - r_i$) and (b) the retirement
benefits are greater than the benefits from other welfare programs ($r_i > o_i$). While (a) is required
to ensure that R is preferred to W, (b) is necessary to ensure that R is preferred to O. Conditional
on the monetary payoffs associated with W, R and O, the propensity to retire is increasing in the
preference for leisure and decreasing in the preference for consumption.

This framework allows us to predict the effect of the reform on individuals who would have
chosen W absent the reform, and on those who would have chosen O absent the reform:

i's type	Choice before the reform	Choice after the reform
$\beta_i > w_i - o_i$	O	O if $o_i > r_i$ R if $o_i < r_i$
$\beta_i < w_i - o_i$	W	W if $o_i > r_i$ and $\beta_i < w_i - r_i$ R if $o_i < r_i$ and $\beta_i > w_i - r_i$

The above table reveals that the ER reform may push eligible individuals into retirement
through two distinct channels. First, the reform will induce working individuals to retire if
$w_i - o_i > \beta_i > w_i - r_i$, such that individuals prefer W to O, but R to W. Second, the reform will
induce program substitution among individuals on alternative welfare programs if $\beta_i > w_i - o_i$
and $o_i < r_i$, such that individuals prefer O to W, but R to O.[14]

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[14] Without heterogeneity across individuals in r_i and o_i, the reform would either have no effect on retirement (if $r < o$)
or result in complete program substitution (if $r > o$).

Having considered the direct impact of the reform on affected individuals, we analyse the indirect effect of the ER reform on the spouses of these directly affected individuals.[15] We let j denote i's spouse, and we focus on individual j's decision given individual i's decision. Note that R is by design not available to the spouses, as we exclude spouses with access to early retirement.

Individual j's utility is given by $\alpha_j c + \beta_j l + \gamma_j l_i l_j$, where c and l are total household consumption and leisure. Hence, consumption and leisure are public goods. The term γ_j captures complementarities in leisure. Again, we normalise α_j to 1. Individual j's maximisation problem and criterion for choosing O over W depend on individual i's state and are as follows:

	Individual i's state		
	O	R	W
j's maximisation problem	$\max\limits_{W,O}\left\{\begin{array}{c} o_i + w_j + \beta_j, \\ o_i + o_j + 2\beta_j + \gamma_j \end{array}\right\}$	$\max\limits_{W,O}\left\{\begin{array}{c} r_i + w_j + \beta_j, \\ r_i + o_j + 2\beta_j + \gamma_j \end{array}\right\}$	$\max\limits_{W,O}\left\{\begin{array}{c} w_i + w_j, \\ w_i + o_j + \beta_j \end{array}\right\}$
j's criterion for choosing O over W	$\beta_j + \gamma_j > (w_j - o_j)$	$\beta_j + \gamma_j > (w_j - o_j)$	$\beta_j > (w_j - o_j)$

Because individual j's necessary condition for choosing O over W is the same irrespective of whether individual i is retired or on other welfare programs, direct program substitution (the reform moving individual i from O to R) will not impact individual j's choice. However, moving individual i from W to R will cause individual j to switch from W to O if $\beta_j + \gamma_j > (w_j - o_j) > \beta_j$. This only holds if $\gamma_j > 0$.

It is important to note that our empirical strategy consists of estimating a reduced-form model in which we compare the labour supply and social insurance program participation of spouses to ER-eligible individuals with spouses to non-ER-eligible individuals in a difference-in-differences framework. An advantage of approaching this question from a reduced-form perspective rather than through a structural model is that it does not require us to invoke any distributional assumptions, something that has proven to be one of the main drawbacks with the structural approach used to address this question (see Banks *et al.*, 2010). However, the reduced-form approach also means that we cannot directly identify the preference parameters α, β and γ. Nevertheless, a positive spousal effect is consistent with $\gamma > 0$.

Similar to the discussion of the program substitution effects, the potential indirect effect on spousal participation in other welfare programs has important policy implications, affecting both expected tax revenue and public spending on welfare programs. In Section 3, we show that these spillover effects yield economically meaningful increases in the cost of the ER program, highlighting their importance when designing welfare reforms.

The conceptual framework is useful for considering the importance of cross-program and indirect spousal spillover effects associated with welfare reforms. This highlights the value of the empirical exercise in the next section. While we do not have sufficient information to produce a full welfare analysis of the ER reform, we can identify the extent of the cross-program and indirect spousal spillover effects and provide information on their relative importance. In Section 6, we use the results from our empirical exercise to estimate the cost of the reform in a world where the social planner recognised these spillovers and interactions, compared to a world in which the social planner considers the associated elasticities to be zero. While the general principle of

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[15] Note that we have chosen to present this as a sequential decision process to mimic our empirical design and to facilitate the exposition of the conceptual framework. Note that our main theoretical point about the spousal impact of the reform—that it will depend on the preference for joint leisure—will hold also if the decision process is not sequential.

such substitution and spillover effects apply to other countries and settings, the calculations we provide will of course be specific to Norway and its ER reform.

2. Data and Method

2.1. *Data*

We rely on detailed administrative data from 1988 to 2007. First, we use a matched employer-employee registry to obtain information on earnings, work hours and place of work. Second, we use a population registry to collect information on birth year and marital status. Third, we use a range of social insurance registries to obtain information on the take-up of social insurance programs, most importantly early retirement, DI, UI and SL.

Crucial for this analysis is our ability to identify the firms affected by the early retirement reform, which we do through backward induction. That is, we identify all workers who start receiving early retirement pension in each year, identify which firms they worked at prior to taking up early retirement and classify those firms as treated. We classify all other firms as control firms. This approach results in some measurement error, as some treated firms will erroneously be classified as control firms if they do not have any workers taking up early retirement during our time period. The risk of misclassifying a treated firm is greater the smaller the firm size, and we therefore omit firms with ten or fewer employees.[16] Any misclassification of treated firms will result in an attenuation bias (bias toward zero).

The ER reform was first implemented in 1989 with an eligibility age of 66, and the early retirement age was then gradually lowered to 62 through reform amendments in 1990, 1994, 1997 and 1998 (the other main aspects of the early retirement scheme were unchanged). We focus on cohorts subject to the lowest ER eligibility age (62 years). The main sample consists of individuals born between 1936 and 1941 and who reached the early retirement eligibility age of 62 between 1998 and 2003. We follow these individuals (and their spouses) from age 57 to 66, such that we observe each individual for ten years. As individuals who have already left the labour force will be unaffected by the reform, we restrict our sample to individuals who worked at the age of 57. Online Appendix Table A2 shows the cohorts included in our sample and the period of observation for each cohort.

We impose three sample restrictions. First, to assign individuals to treatment and control groups, we need to know each individual's firm affiliation. As post-reform firm affiliation is likely endogenous, we rely on pre-reform (1988) affiliation to identify treatment status. We drop any individuals (and spouses) with missing information on firm affiliation in 1988.

Second, to isolate the *indirect* effect of the early retirement reform on spouses, we remove couples in which the spouse worked in a treated firm in 1988. This removes any *direct* effect of the reform on the spouses.

Third, we drop individuals whose spouses are older than they are. This ensures that spouses can respond to the affected individual's early retirement in all years before the affected individual reaches the standard retirement age of 67. While this restriction does not have a significant effect on men since wives tend to be younger than their husbands, the number of women married to men younger than them is considerably smaller than the number of women married to men older than them. In our setting, 25% of all women that meet our first two sample restrictions have husbands that are not older than them. Interestingly, the women married to younger men are similar to

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[16] Our results are robust to changing this threshold. See Online Appendix B.

the women married to older men, both in terms of education and earnings (Online Appendix Table A3). The husbands of older women are also relatively comparable to the husbands of younger women in terms of education, though they have earnings that are approximately 15% larger. This is unsurprising, as many of the husbands to younger women have already left the labour force due to old age.

Descriptive statistics of our sample are provided in Online Appendix Table A4. To better understand how the analytical sample compares to the full population, Table A4 also provides descriptive statistics of the full sample of individuals in Norway born between 1936 and 1941. While there are some noticeable differences related to earnings and the age gap between husband and wife, the composition of our analysis sample is comparable to the full population.

Figure 2 provides descriptive evidence on the direct effect of the reform on early retirement take-up and employment status. The figure plots raw trends in early retirement and employment separately for individuals in our treatment group and control group between the ages of 57 and 66 in the post-reform years. Three things are worth noting. First, individuals in the treatment group are trending similarly to individuals in the control group prior to reaching the eligibility age for early retirement. While this result follows by construction for early retirement take-up, this is not the case for employment, and provides evidence in favour of the parallel trend assumption required for our analysis. Second, once the individuals reach 62, there is a jump in the probability of early retirement take-up, and a drop in employment, among individuals in the treatment group. Finally, the take-up of early retirement also increases among individuals in the control group. The reason is that treatment is based on the pre-reform firm affiliation of the worker, and some of the workers in the control group may have switched to the treatment group between the time that treatment was assigned and the time they turned 62. This highlights that our results likely represent a lower bound of the full program effect and should be interpreted as intent-to-treat effects.

2.2. *Empirical Strategy*

To identify the direct effect of the reform on early retirement, labour supply and the take-up of alternative social insurance programs, we compare individuals in firms that participate in the ER scheme to individuals in firms that do not participate in the ER scheme through a difference-in-differences approach. We estimate models of the form

$$Y_{it} = \alpha + \beta_1(\text{FirmEligible}_i \times \text{AgeEligible}_{it}) + \delta_i + \tau_t + \theta_{it} + \varepsilon_{it}, \tag{1}$$

where Y_{it} is an outcome for individual i at time t: take-up of early retirement, employment and the use of alternative welfare programs (DI, UI and SL). The variable AgeEligible_{it} is a dichotomous variable taking the value of 1 if the worker is above the early retirement age of 62 and 0 otherwise. The variable FirmEligible_i is a dichotomous variable taking the value of 1 if the worker is affiliated with a firm that participates in the ER scheme and 0 otherwise. The coefficient of interest is β_1 and measures the effect of the ER reform on Y. Since treatment is based on pre-reform firm affiliation, β_1 should be interpreted as an intent-to-treat effect.

Equation (1) also includes a set of calendar year (τ_t), age (θ_{it}) and individual (δ_i) fixed effects. The individual fixed effects control for any time-invariant differences across individuals that could potentially confound our results, the year fixed effects absorb any time-specific events that
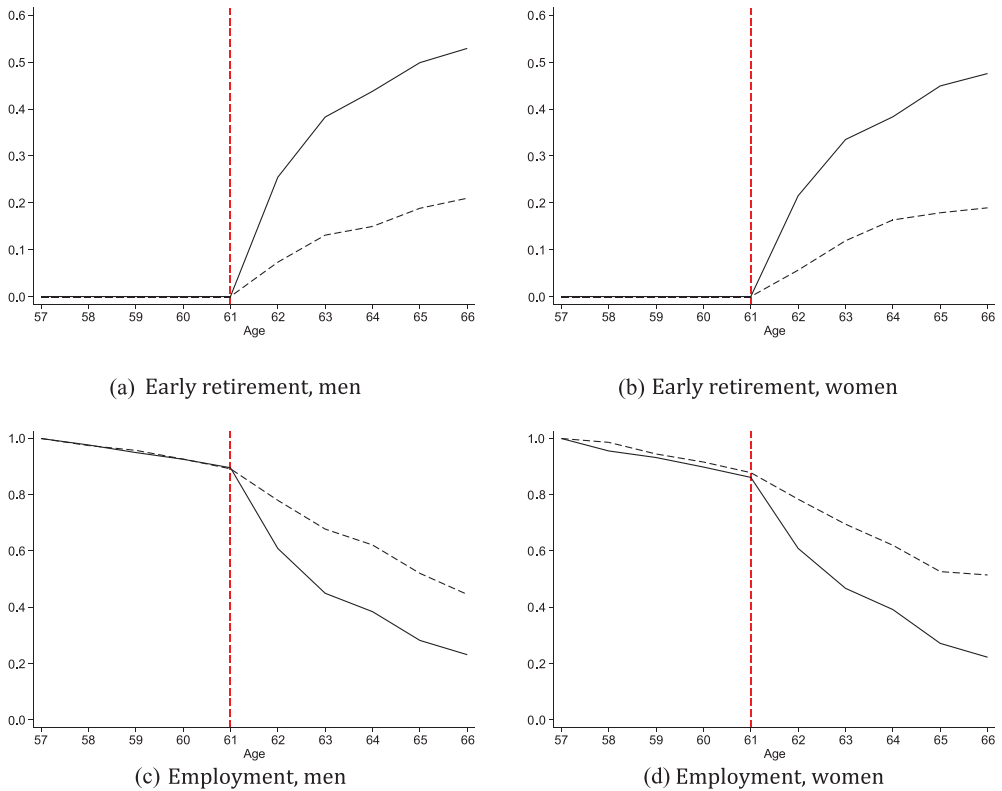
(a) Early retirement, men

(b) Early retirement, women

(c) Employment, men

(d) Employment, women

Fig. 2. *Direct Effect of the Reform on Early Retirement Take-Up and Employment, Raw Trends.*
Notes: The figure presents raw trends in early retirement and employment separately for individuals in our treatment group (solid line) and control group (dashed line) between the ages of 57 and 66 in the post-reform period 1993–2007. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941, reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. The figures are constructed using linked Norwegian employer-employee data, tax registers and social insurance registers.

affect all individuals similarly and the age fixed effects account for systematic differences in *Y* across age cohorts.[17] We estimate (1) separately for men and women.

To examine the indirect effect of the reform on spouses to ER-eligible individuals—both in terms of labour supply and take-up of alternative social insurance programs—we estimate (1) using spousal labour market and social insurance outcomes as dependent variables.[18] The

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[17] We do not include firm fixed effects in our empirical specification as they are fully accounted for by the individual fixed effects in the model.

[18] While we cannot include spousal age fixed effects due to perfect collinearity, our results are robust to including a linear or quadratic control for spousal age and to replacing individual age fixed effects with spousal age fixed effects (Section 4).

individual fixed effects in (1) can be viewed as a household fixed effect when estimating the equation using spousal outcomes as the dependent variables.

Our identifying variation comes from differences in early retirement eligibility across individuals based on whether they work at a firm in 1988 that participates in the ER scheme or not. Our identifying assumption is similar to that of all difference-in-differences models, namely that there are no secular trends, shocks or policies that occur concurrently with the early retirement reform and that differentially affect individuals affiliated with an ER firm in 1988 and individuals not affiliated with an ER firm in 1988.

To obtain support for our identifying assumption, we employ event studies that directly test for the existence of relative pre-treatment trends. Defining the year the individual was 57 years old as the base year, we estimate the model

$$Y_{it} = \alpha + \sum_{a=58}^{66} (\pi_a[\text{FirmEligible}_i \times \text{Age}_{ita}]) + \delta_i + \tau_t + \theta_{it} + \varepsilon_{it}, \tag{2}$$

for each $a \in [58(1)66]$, where a is the age of the individual. The coefficients of interest, π_a, allow us to trace out relative pre-treatment trends (for π_{58} to π_{61}) and directly test for selection on fixed trends over time. If the π_{58} to π_{61} estimates are economically small and not statistically significantly different from zero, that implies that there likely is no such selection that bias our results. Another benefit with (2) is that it permits identification of time-varying treatment effects (π_{62} through π_{66}).[19]

While (1) and (2) are valuable for identifying the full matrix of effects associated with the reform across programs and household members, they are somewhat restrictive for understanding interdependencies in couples' retirement decisions. The reason is that the difference-in-differences approach yields the intent-to-treat (ITT) effect of giving workers increased opportunities to retire early on the employment of the spouse. The ITT effect is smaller than the average treatment effect because not all ER-eligible workers choose to retire early. An alternative way to estimate how the individual's response to the early retirement reform may affect spousal employment is to use ER eligibility as an instrument for employment in an instrumental variable approach. This approach yields the local average treatment effect. To keep the exposition clear, we first discuss the ITT effects (Sections 3 and 4) and then provide the results from the instrumental variable approach (Section 5).

3. Results

3.1. *Direct Effect on Early Retirement, Employment and Social Insurance Take-Up*

Table 2 presents estimates of the direct effect of the ER reform on early retirement take-up, employment and participation in alternative social insurance programs. We discuss the effects for men (panel A) and women (panel B) in turn.

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[19] In addition to estimating non-parametric event studies, we perform a placebo test on age groups not affected by the reform, we account for individuals that switch between firms that participate and firms that do not participate in the ER scheme, and we ensure that our results are robust to relaxing our sample restrictions and changing our model specifications. The results from these robustness and sensitivity checks are consistent with our identifying assumptions, and support a causal interpretation of the results. See the Online Appendix.

Table 2. *The Direct Effect of the Reform on Employment and Program Take-Up.*

	ER (1)	Employment (2)	Program substitution		
			DI (3)	Unemployment (4)	Sick leave (5)
Panel A: men					
ITT effect	0.270***	−0.216***	−0.048***	−0.018**	−0.023***
SE	(0.019)	(0.020)	(0.014)	(0.008)	(0.009)
Mean	0.150	0.611	0.226	0.049	0.057
N			19,596		
N couples			1,989		
Panel B: women					
ITT effect	0.230***	−0.221***	−0.005	0.004	−0.048***
SE	(0.031)	(0.032)	(0.026)	(0.015)	(0.017)
Mean	0.137	0.632	0.221	0.041	0.057
N			5,936		
N couples			603		

Notes: This table reports reduced-form estimates of (1) of eligibility for ER benefits on own employment and take-up of various public policies. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. We control for individual/couple, calendar year and age fixed effects. The mean is the mean of the dependent variable among age-eligible individuals in the control group. Note that individuals can receive UI and SL even if they are employed, such that columns (1) through (5) do not mechanically sum to zero. Data are from Norwegian employer-employee data, tax registers and social insurance registers. SEs clustered at the firm level are in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

3.1.1. *Direct effect on men*

Column (1) of panel A in Table 2 shows that the reform had a statistically significant and economically meaningful direct effect on the take-up of early retirement among men. Men affiliated with an ER firm in 1988 are 27 percentage points more likely to enter early retirement during the ages of 62 through 66 compared to men not affiliated with an ER firm.

The direct effect of the ER reform on the early retirement behaviour of men should lead to a drop in employment among these individuals, as early retirement benefits are conditional on withdrawal from employment. This is confirmed in column (2) of panel A, which shows that the ER reform reduced the employment of eligible men by 22 percentage points. This is a 35% reduction relative to the mean, shown directly below the point estimate. That the ER reform reduced employment of older workers has been shown before by Vestad (2013).

The employment effect (22 percentage points) is not large enough to fully explain the increase in early retirement take-up (27 percentage points), suggesting that eligible men not only switch from employment to early retirement, but also substitute from other social insurance programs to early retirement.[20] To explore such substitution effects, columns (3) through (5) show effects on participation in other major welfare programs: DI, UI and SL.[21] There is a statistically significant

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[20] Note that, since individuals who are already on disability or unemployment insurance are not allowed to switch to early retirement, the program substitution we identify comes from individuals who take up early retirement instead of taking up disability or unemployment insurance.

[21] Note that individuals can be employed while receiving UI and SL, such that the increase in early retirement minus the change in DI, UI and SL will not mechanically equal the size of the employment reduction.

and negative effect across all three alternative programs. This is consistent with the conceptual framework outlined in Subsection 1.3 showing that the ER reform may not only induce working individuals to retire but also induce individuals on alternative welfare programs to substitute into early retirement. In terms of magnitudes, the negative effect on DI accounts for 60% of the entire cross-program effect. In total, cross-program substitution accounts for a third of the entire reform effect on ER take-up. Ignoring such program substitution effects will therefore cause an underestimation of the impact of the reform.

3.1.2. *Direct effect on women*

Panel B of Table 2 provides results for women. Column (1) of panel B shows that the reform has a substantial direct effect on the take-up of early retirement among women. Although the point estimate of 0.23 is smaller than that for men (0.27), it remains both economically meaningful and statistically significant. Column (2) of panel B demonstrates that eligible women are 22.1 percentage points more likely to leave the labour force during the ages of 62 through 66 compared with those not eligible for early retirement. Similar to the effect among men, this represents a 35% reduction in employment relative to the mean, shown below the point estimate in the table.

The employment result in column (2) can explain almost the entire early retirement effect on women, suggesting that substitution from other social insurance programs play a smaller role when it comes to women. The results provided in columns (3) through (5) support this interpretation, showing relatively small and often not statistically significant effects across the alternative social insurance programs discussed in Section 1: DI, UI and SL.

3.1.3. *Explaining the gender differences in the direct effects*

What might explain our findings of strong negative cross-program effects among men but ostensibly no effects among women? We argue that the lack of negative cross-program effects among women is consistent with the fact that fewer women qualify for early retirement, such that their ability to substitute across programs is smaller. As described in Section 1, there are several individual eligibility requirements for early retirement related to earnings history. In Figure 3(a), we show that women have lower cumulative lifetime earnings than men. In this figure, we also plot the 10th percentile of the cumulative earnings among individuals who meet the earnings requirements for ER. Most men have cumulative earnings higher than this threshold while a large share of women do not. This suggests that fewer women are likely to qualify for ER, and that the differences in earnings histories between men and women could explain the lack of a cross-program response among women. The lack of negative cross-program effect among women is also consistent with the fact that the relative benefit of substituting DI for ER is lower for women. The reason for this is that the compensation rate for DI is greater than the compensation rate for ER for low-income individuals, and the overwhelming majority of women in our sample have lower income than the men (Figure 3(b)). In terms of our conceptual framework, this implies that, on average, $r_i > o_i$ is more likely to hold for men than for women, a necessary condition for program substitution.

To examine if gender differences in earnings history and current income can explain the differences in effects across genders that we identify in our analysis, we estimate a modified version of (1) for men in which we weight each observation by the share of women with similar earnings histories and current earnings. The results show that the reform would have had no
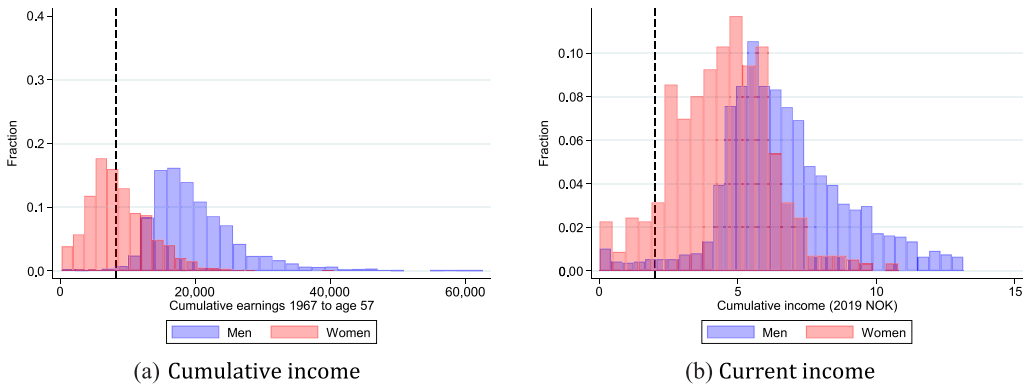
(a) Cumulative income

(b) Current income

Fig. 3. *Cumulative and Current Income at Age 57 for Men and Women.*
Notes: The figure in panel (a) shows the distribution of cumulative earnings between 1967 and the year of reaching the age of 57, separately for men (blue) and women (red). Cumulative earnings are defined as the sum of yearly earnings adjusted to their net present value and presented in 1,000 (2019) Norwegian kroner (NOK). The dashed line represents the 10th percentile of cumulative earnings of individuals aged 62 receiving ER benefits in the period 1998 to 2003. The figure in panel (b) shows the distribution of income at age 57, separately for men (blue) and women (red). Current income is also presented in 1,000 (2019) NOK. The dashed line indicates both the minimum guaranteed amount of benefits under the public disability insurance scheme and the minimum income required (during the ten-best years of labour market earnings) to qualify for the early retirement scheme. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. The figures are constructed using linked Norwegian employer-employee data and tax registers.

cross-program substitution effect on men had their earnings history and current income been similar to that of the women (Online Appendix Table A5).[22]

3.2. *Indirect Effect on Spousal Employment and Program Take-Up*

Table 3 shows the indirect effect of the reform on early retirement take-up, employment and participation in alternative social insurance programs for the spouses (i.e., husbands and wives) of ER-eligible individuals. We discuss the indirect effects on female spouses to ER-eligible men (panel A) and male spouses to ER-eligible women (panel B) in turn.

3.2.1. *Indirect effect on female spouses*
Column (1) of panel A in Table 3 shows that the reform had a significant and meaningful indirect effect on the employment decisions of female spouses of ER-eligible men. Specifically, female spouses of ER-eligible men are 4.1 percentage points more likely to leave the labour force compared to female spouses of non-ER-eligible men. With respect to the conceptual framework outlined in Section 1, this implies that couples have a preference for joint leisure ($\gamma > 0$). More broadly, it suggests that there are substantial interdependencies in spousal retirement decisions

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[22] We also acknowledge that the sample size is substantially different for our male and female samples, and that part of the lack of a cross-program effect among women could be due to less power.

Table 3. *The Indirect Effect of the Reform on Spousal Employment and Program Take-Up.*

| | Employment (1) | Program take-up | | | |
		ER (2)	DI (3)	Unemployment (4)	Sick leave (5)
Panel A: female spouses					
ITT effect	−0.041***	0.017**	0.029**	−0.012	−0.011
SE	(0.014)	(0.008)	(0.012)	(0.008)	(0.010)
Mean	0.748	0.042	0.166	0.049	0.054
N			19,596		
N couples			1,989		
Panel B: male spouses					
ITT effect	−0.039	0.034	0.004	−0.019	−0.034**
SE	(0.029)	(0.022)	(0.024)	(0.016)	(0.016)
Mean	0.703	0.076	0.161	0.049	0.063
N			5,936		
N couples			603		

Notes: This table reports reduced-form estimates of (1) of eligibility for ER benefits on spousal employment and take-up of various public policies. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. We control for individual/couple, calendar year and age fixed effects. The mean is the mean of the dependent variable among spouses in the control group. Data are from Norwegian employer-employee data, tax registers and social insurance registers. SEs clustered at the firm level are in parentheses. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

and demonstrates how welfare reforms intended for a specific group of individuals may have significant spillover effects across household members.

The negative indirect effect on spousal employment likely represents a relatively large reduction in household income, as these spouses do not qualify for early retirement. One way to mitigate this reduction in household income is to switch into alternative social insurance programs. However, such switching may be difficult. To study this question, columns (2) through (5) of panel A report effects on spousal participation in alternative social insurance programs (DI, UI, SL and ER). As take-up of early retirement is only possible through affiliations with ER-eligible firms, a significant effect on this outcome would imply that the spouse has switched from a control firm to a treatment firm to become eligible for early retirement (recall that the sample is restricted to couples in which the spouses worked in control firms in the pre-reform period).

We find a significant and meaningful indirect effect on the social insurance participation of the female spouses of ER-eligible men. Specifically, female spouses of ER-eligible men are 1.7 percentage points (40% relative to the mean) more likely to take up early retirement and 2.9 percentage points (17% relative to the mean) more likely to claim disability insurance, compared with the spouses of non-ER-eligible men. There are no effects on the spouse's probability to take up UI or SL. The use of DI and ER rather than UI and SL is expected. Both provide permanent exits from the labour market without imposing time restrictions or follow-up requirements similar to the other programs.[23]

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[23] Several empirical studies have presented strong suggestive evidence of leniency in the DI screening process in Norway. Bratsberg *et al.* (2013) showed that individuals laid off from their jobs are significantly overrepresented among disability insurance beneficiaries, and Schreiner (2019) showed significant local screening leniency across the country.

While the indirect effect of the reform on the spouse's decision to exit the labour market is consistent with household complementarities in leisure, it could also reflect indirect effects on the spouse's health. For example, Bertoni and Brunello (2017) exploited variation in the maximum age of guaranteed employment induced by a 2006 Japanese reform and found that the husband's retirement negatively impacts the mental wellbeing of the wife. Zang (2020), on the other hand, exploited a large increase in the probability of retirement at the legal retirement age in China and found that the husband's retirement has a positive effect on the physical and mental wellbeing of the wife. Thus, the direction of a potential health effect is ambiguous.

To examine whether the spousal DI effects we identify are driven (or muted) by health effects, we use data from the death and medical health registers. Unfortunately, these data are only available from 2007 onwards. Furthermore, we cannot link it to the data we use for our main analysis. When investigating health outcomes, we therefore rely on identifying treatment based on individual firm affiliation at age 57. Otherwise, the sample is constructed as the main sample. We then estimate a cross-sectional regression of eligibility for early retirement on spousal health outcomes in 2007. The outcomes we examine are mortality, emergency room visits and visits to general practitioners (GPs). We acknowledge that these outcomes may not be able to identify less severe mental stress or sleep deprivation effects as those examined in Bertoni and Brunello (2017). However, we believe that moderate mental health effects should be picked up by an increase in GP visits as GPs are gatekeepers to the Norwegian health care system and are responsible for referring the patient to specialists and prescribing medicines such as anti-depressants. Looking across the columns in Online Appendix Table A6, there is no suggestive evidence of indirect negative health effects among female spouses of ER-eligible men. However, our auxiliary health analysis is based on a subset of analysis years, and there are limitations associated with the data we use that make it difficult to identify more nuanced mental health effects. Thus, this analysis does not allow us to completely discard the hypothesis that the wife's health worsened due to the husband's retirement.

3.2.2. *Indirect effect on male spouses*

Panel B of Table 3 shows the potential indirect effects for male spouses to ER-eligible women, both in terms of employment and the take-up of social insurance programs. Looking across the columns, the estimates are similar in size to those for female spouses. However, the estimates are much noisier, and the effects on employment and early retirement are not statistically significant at conventional levels. Interestingly, the point estimate on take-up of disability insurance is zero. We believe that this could be because most men qualify for early retirement benefits while many women do not. However, we stress that the sample underlying the estimations in panel B is significantly smaller than the sample underlying the estimations in panel A (600 versus 2,000 observations). Thus, we encourage caution when interpreting these results.

3.2.3. *Explaining the gender differences in the indirect spousal effects*

If we interpret the above results as providing evidence for labour market and social insurance responses among female but not male spouses, we believe this can be explained by differences in the household costs of joint exit from the labour market. As demonstrated in Figure 3, the women

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Some of this leniency is believed to stem from the substantial autonomy granted to GPs and DI case workers, who can grant DI benefits for a variety of reasons in which validation is difficult. More than 60% of individuals on DI have been granted benefits due to 'hard-to-verify' conditions (Kostøl and Mogstad, 2014). Abstracting from the potential health confounder discussed in Subsection 1.3, we find it unlikely that we would observe significant indirect spousal effects had the DI screening process been much stricter.

in our sample earn much less than the men. This means that the household cost associated with male spouses leaving the labour force is much greater than the household cost associated with female spouses leaving the labour force (see Online Appendix A, Figure A1).

Suggestive evidence on the relationship between the spouse's share of total household income and the spousal labour market and social insurance response is provided in Online Appendix Table A7. This table shows results obtained from estimating a modified version of (1) that includes an interaction of the treatment variable with a variable measuring the wife-husband income gap. When fixing the income gap at zero, we find statistically significant effects on employment and early retirement for male spouses. The results also show a positive (but not statistically significant) coefficient on DI take-up for male spouses. As expected, the effect on male spouses is increasing in the wife-husband income gap. These results are consistent with the idea that differences in the household cost of joint exit from the labour market could drive the identified gender difference in spousal response.[24]

In addition to differences in the earnings of male and female spouses, there is a substantial difference in the age gap between ER-eligible men and their female spouses, and ER-eligible women and their male spouses (Online Appendix Table A4). Specifically, the female spouses of ER-eligible men are much younger than the male spouses of ER-eligible women. In addition to differences in the household cost of joint exit, it is possible that the larger age gap between ER-eligible men and their female spouses could drive some of the heterogeneous effects that we observe. To examine this, we estimate a modified version of (1) in which we interact the treatment with the age gap between husband and wife. Online Appendix Table A8 shows that the differences in age gap cannot explain the gender differences in spousal response. On the contrary, we find that, for both male and female spouses, the indirect spousal response is larger the smaller the age gap between husband and wife. However, even if we fix the age gap at zero, the indirect employment effect is significantly larger for female spouses than male spouses, and we still find no indirect effect on DI uptake for male spouses.

Another reason for asymmetry in the response of female/male spouses is that the within-couple complementarities in leisure could differ across men and women. Specifically, if females value joint leisure more than males, this could explain the gender differences in the indirect spousal effects. However, the empirical evidence on asymmetric complementarities in leisure is mixed, with some finding that females value joint leisure more than males (Kruse, 2021) and others that males value joint leisure more than females (Coile, 2004). While Kruse (2021) is perhaps more relevant to the current study given that the paper uses a recent pension reform in Norway to examine joint retirement, the relatively scarce and mixed literature on the topic makes it difficult to ascertain to what extent such asymmetry may drive our results.[25]

Finally, as noted in Section 3, we cannot fully discard the hypothesis that the wife's health worsened due to the husband's retirement, and that some of the indirect spousal effect among women is operating through health rather than through complementarities in leisure.

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[24] To the extent that the direct effect of ER eligibility on early retirement potentially also represents a household decision, this argument also implies that the direct effect should be larger for ER-eligible women than for ER-eligible men (since women's share of household income is smaller). This is consistent with our findings: While the magnitude of the direct effect on employment is similar in absolute terms for men and women, a larger share of women do not fulfil the individual eligibility criteria for early retirement (see Sections 1 and 3), such that the direct effect scaled by the fraction eligible is larger for women than men.

[25] The prescription that 'a man should earn more than his wife' (e.g., Bertrand *et al.*, 2015) provides another channel through which the asymmetry in the response for female/male spouses may operate.

3.3. *Non-Parametric Event Studies*

The assumption underlying our estimation strategy is that there are no secular trends, policies or shocks concurrent with the ER reform that differentially affect individuals affiliated with an ER firm in 1988 and individuals not affiliated with an ER firm in 1988. While the parallel trend assumption cannot be tested directly, we can provide suggestive evidence by estimating event studies based on (2). This allows us to trace out relative pre-treatment trends and directly test for selection on fixed trends over time (that the outcomes of treated and control individuals are not moving in different directions prior to the reform).[26]

Results from estimating the direct effect of the reform are shown in Figure 4.[27] Each dot corresponds to the π_a estimate indicated on the x axis, and the bars extending from each dot represent the 95% confidence intervals. Note that we define the year the worker was aged 57 as the base year, such that all estimates are relative to this. Looking across all subfigures, three things are worth noting. First, there is no indication of pre-treatment trends in any outcome, suggesting that there is no selection on fixed trends over time. Second, there is a discontinuous jump in early retirement and employment at the relevant age cut-off for ER-eligible individuals followed by a gradual increase in effect size as the individuals approach the standard retirement age of 67. Third, with respect to the take-up of disability insurance, the effect is developing more gradually over time.

Results from estimating the indirect effect of the reform on spousal outcomes are shown in Figure 5. These results tell a story much similar to those for ER-eligible individuals in that there is no indication of any pre-treatment trends in any of our outcomes, suggesting that there is no selection on fixed trends over time. With respect to the evolution of the effect size over time, the spousal effects develop more gradually than the direct effects shown in Figure 4.

The non-parametric event studies for spouses to ER-eligible individuals closely mirror the raw trends in outcomes (Figure 6). This is encouraging, providing even stronger evidence against potential selection on fixed trends over time and in favour of our identifying assumption.

4. Robustness and Sensitivity Analysis

One concern associated with our empirical approach is that we assign treatment based on an individual's pre-reform firm affiliation, which implies that some individuals classified as untreated will switch to an ER firm before reaching early retirement age, and some individuals classified as treated will switch to a non-ER firm before reaching early retirement age. As we identify relatively large direct effects, the potential attenuation bias from such switching is a minor concern. Switching among spouses is a potentially larger issue. If a spouse switches from a non-ER firm to an ER firm, any spousal effect would be a combination of the indirect spousal response and the spouse's own direct response to becoming ER eligible. We mitigate such direct effect contamination in our main analysis by restricting the sample to couples in which the spouse worked in a control firm prior to the reform, but if spouses switch to ER firms after treatment assignment, there could still be a direct effect contamination.

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[26] Raw trends for these outcomes are provided in Figure 2 with respect to employment and ER take-up, and with respect to the other social security programs in Online Appendix Figure A2.

[27] We do not discuss the event study results for unemployment insurance and paid sick leave in this section as we do not find systematically statistically significant and economically meaningful effects with respect to these variables. However, for completeness, event studies for these outcomes are provided in Online Appendix Figure A3.
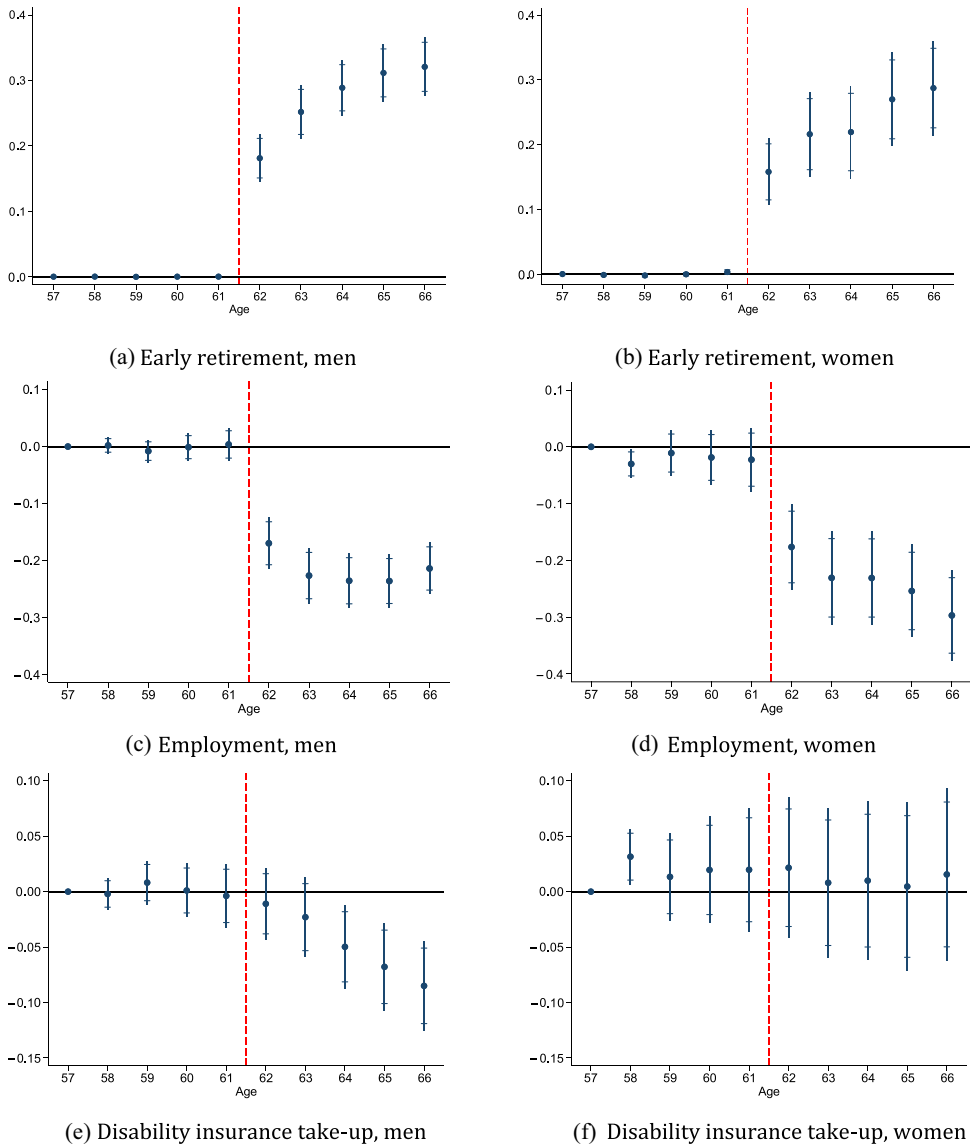
(a) Early retirement, men

(b) Early retirement, women

(c) Employment, men

(d) Employment, women

(e) Disability insurance take-up, men

(f) Disability insurance take-up, women

Fig. 4. *Direct Effect of the Reform on Own Outcomes, Event Study Results.*
Notes: Each dot corresponds to the treatment effect at the given age indicated on the *x* axis, and the bar
extending from each dot represents 90% and 95% confidence intervals. The dotted vertical line denotes the
age cut-off at which individuals affiliated with ER firms become eligible for early retirement, and the
dashed horizontal line is at zero to facilitate interpretation of the results. Note that we define the year the
worker was aged 57 as the base year, such that all estimates will be relative to this baseline. We define
treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the
ER scheme. Individuals in the control group could only retire with public pension benefits from the age of
67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample
consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the
period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with
spouses not treated by the reform based on their pre-reform firm affiliation. The figures are constructed
using linked Norwegian employer-employee data, tax registers and social insurance registers.

(a) Employment, female spouses

(b) Employment, male spouses

(c) Early retirement, female spouses

(d) Early retirement, male spouses

(e) DI take-up, female spouses

(f) DI take-up, male spouses

Fig. 5. *Indirect Effects of the Reform on Spousal Outcomes, Event Study Results.*
Notes: Each dot corresponds to the treatment effect at the given age indicated on the *x* axis, and the bar extending from each dot represents 90% and 95% confidence intervals. The dotted vertical line denotes the age cut-off at which individuals affiliated with ER firms become eligible for early retirement, and the dashed horizontal line is at zero to facilitate interpretation of the results. Note that we define the year the worker was aged 57 as the base year, such that all estimates will be relative to this baseline. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. The figures are constructed using linked Norwegian employer-employee data, tax registers and social insurance registers.
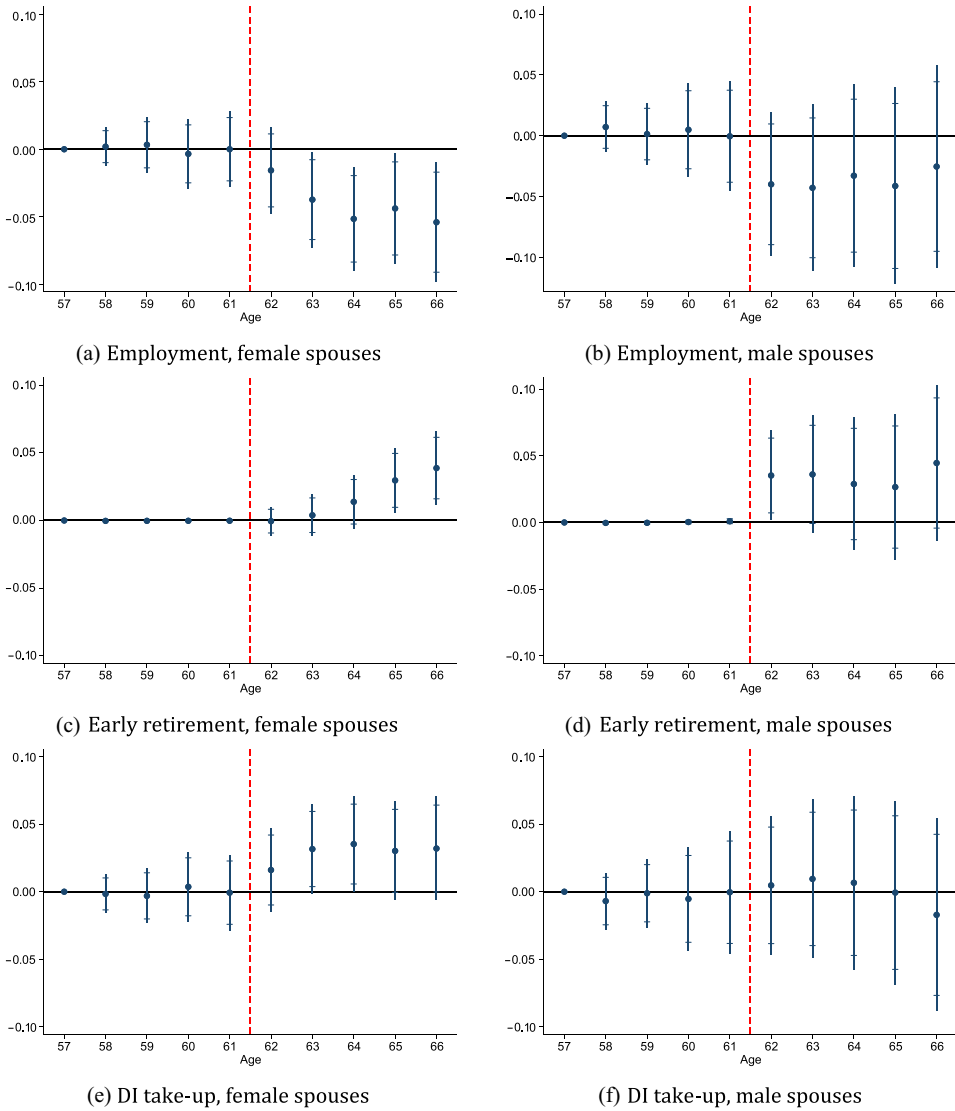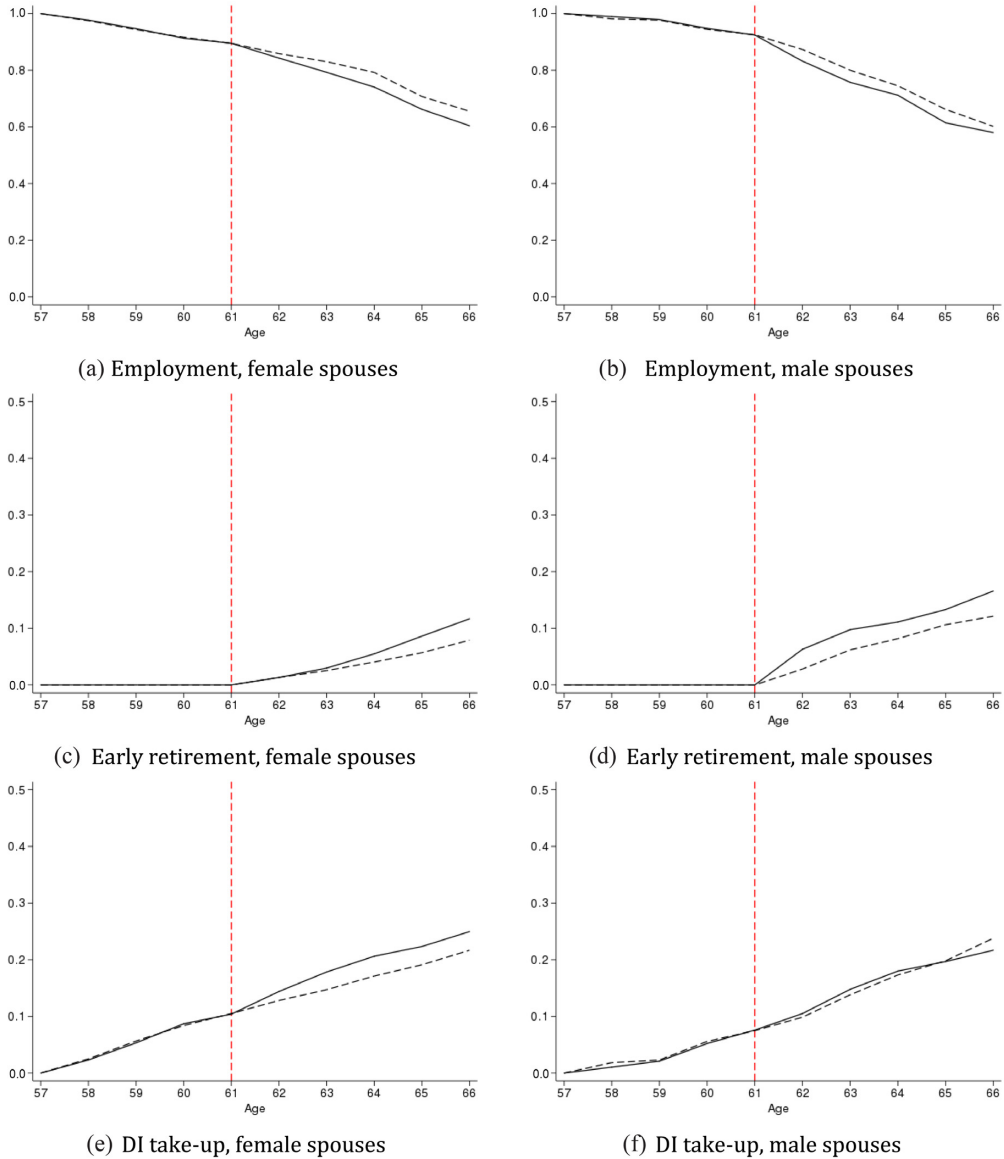
Fig. 6. *Indirect Effect of the Reform on Spousal Outcomes, Raw Trends.*
Notes: The figure plots raw trends in the employment rate, early retirement and take-up of disability
benefits for spouses in our treatment group (solid line) and control group (dashed line). We define
treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the
ER scheme. Individuals in the control group could only retire with public pension benefits from the age of
67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample
consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the
period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with
spouses not treated by the reform based on their pre-reform firm affiliation. The figures are constructed
using linked Norwegian employer-employee data, tax registers and social insurance registers.

Table 4. *The Direct Effect of the Reform among Workers that Did Not Switch Eligibility Status.*

	ER (1)	Employment (2)	Program substitution		
			DI (3)	Unemployment (4)	Sick leave (5)
Panel A: men					
ITT effect	0.461***	−0.335***	−0.103***	−0.037***	−0.035**
SE	(0.022)	(0.027)	(0.023)	(0.012)	(0.014)
Mean	0.007	0.678	0.288	0.066	0.067
N			7,730		
N couples			788		
Panel B: women					
ITT effect	0.401***	−0.367***	−0.007	−0.003	−0.060**
SE	(0.034)	(0.040)	(0.037)	(0.024)	(0.025)
Mean	0.006	0.739	0.228	0.053	0.070
N			2,696		
N couples			275		

Notes: This table reports reduced-form estimates of (1) of eligibility for ER benefits on own employment and take-up of various public policies. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. We control for individual/couple, calendar year and age fixed effects. The mean is the mean of the dependent variable among age-eligible individuals in the control group. Data are from Norwegian employer-employee data, tax registers and social insurance registers. SEs clustered at the firm level are in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

   To examine if our results are driven by firm switching, we remove all individuals and spouses who switched firm affiliation in any time period.[28] The results from this exercise are shown in Table 4 (for the direct effect) and Table 5 (for the indirect spousal response). These results suggest that all of our main findings are robust to this sample restriction. Thus, the potential direct effect contamination driven by the fact that some spouses switch to ER firms after treatment assignment does not to constitute an issue.

   We also investigate the sensitivity of our results to a battery of sample and specification checks. As discussed in Section 2, we impose a number of restrictions on our sample prior to performing our analysis. In Online Appendix Tables B1 and B2, we investigate the sensitivity of our results to relaxing each of these restrictions. The results from these auxiliary analyses are discussed in Online Appendix B and show that our findings are robust to each of the sample restrictions we impose. Furthermore, our findings are robust to a number of different model specification checks, also discussed in Online Appendix B. Specifically, both the direct effects (Online Appendix Table B3) and the indirect spousal responses (Online Appendix Table B4) are robust to the inclusion of linear and quadratic controls for spousal age, the exclusion of individual fixed effects, the inclusion of firm fixed effects and the inclusion of (pre-ER age) earnings controls.

   A final potential concern associated with our results is that individuals (and their spouses) who were affiliated with ER firms in 1988 are systematically more likely to leave the labour force early than individuals (and their spouses) who were not affiliated with ER firms in 1988. The raw

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[28] There is still a very small share of workers in the control group that retire with ER benefits. Even though we have excluded all control workers who switched to ER firms, some workers might have secondary jobs that provide ER benefits, or we could have rare cases of incomplete information on work history.

Table 5. *The Indirect Effect of the Reform on Spouses to Workers That Did Not Switch Eligibility Status.*

| | Employment (1) | Program take-up | | | |
		ER (2)	DI (3)	Unemployment (4)	Sick leave (5)
Panel A: wives					
ITT effect	−0.056***	0.002	0.067***	−0.021	−0.037**
SE	(0.019)	(0.003)	(0.020)	(0.014)	(0.016)
Mean	0.804	0.002	0.165	0.058	0.053
N			7,730		
N couples			788		
Panel B: husbands					
ITT effect	0.008	−0.010	0.005	−0.005	−0.047*
SE	(0.039)	(0.014)	(0.038)	(0.026)	(0.027)
Mean	0.747	0.021	0.210	0.058	0.088
N			2,696		
N couples			275		

Notes: This table reports reduced-form estimates of (1) of eligibility for ER benefits on spousal employment and take-up of various public policies. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm that later joined the ER scheme. Individuals in the control group could only retire with public pension benefits from the age of 67 unless they switched from a non-ER firm to an ER firm after the introduction of the reform. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their pre-reform firm affiliation. We control for individual/couple, calendar year and age fixed effects. The mean is the mean of the dependent variable among spouses in the control group. Data are from Norwegian employer-employee data, tax registers and social insurance registers. SEs clustered at the firm level are in parentheses. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

data plots (Figures 2 and 6) as well as the non-parametric event studies (Figures 4 and 5) provide evidence against this concern. We also provide evidence that no such ER versus non-ER firm affiliation difference in labour force exit rates exists among younger cohorts who are unaffected by the reform (see Online Appendix C).

5. Instrumental Variable Analysis

In this section, we use eligibility for ER as an instrumental variable (IV) for employment and estimate the effect on the spouses' employment and DI take-up. This approach yields the local average treatment effect (LATE), which is the average effect of spousal employment on own employment for the compliers in our sample. The compliers are men and women who choose to retire earlier because they were eligible for ER but would not have retired earlier had they not been eligible for ER. The empirical two-stage least-squares (2SLS) model is defined by the two equations

$$\text{SpousalEmployment}_{it} = \beta_0 + \beta_1 \widehat{\text{Employment}}_{it} + \delta_i + \tau_t + \theta_{it} + e_{it} \tag{3}$$

$$\text{Employment}_{it} = \emptyset_0 + \emptyset_1(\text{FirmEligible}_i \times \text{AgeEligible}_{it}) + \delta_i + \tau_t + \theta_{it} + u_{it}, \tag{4}$$

where the first-stage equation, (4), estimates the effect of being eligible for ER on own employment. Predicted employment, $\widehat{\text{Employment}}_{it}$, is then inserted into (3). The coefficient of interest in (3) is β_1 and yields the LATE of own employment on spousal labour market outcomes. This

Table 6. *IV Estimates of the Indirect Effect of the Reform on Spousal Employment and DI.*

	Employment	DI
Panel A: female spouses		
Husband employed	0.174***	−0.139**
	(0.063)	(0.055)
Mean	0.748	0.166
N	19,596	
N couples	1,989	
Panel B: male spouses		
Wife employed	0.113	−0.037
	(0.127)	(0.102)
Mean	0.703	0.161
N	5,936	
N couples	603	

Notes: The table reports 2SLS estimates of (3) of the effect of employment on spousal employment and take-up of disability insurance, instrumenting employment with eligibility for ER. We control for individual/couple, calendar year and age fixed effects. Eligibility for early retirement is based on pre-reform affiliation with a firm that later joined the ER scheme. The sample is restricted to married individuals employed aged 57, and with spouses not eligible for ER based on their pre-reform firm affiliation. The mean is the mean of the dependent variable among age-eligible individuals in the control group. Data are from Norwegian employer-employee data, tax registers and social insurance registers. SEs clustered at the firm level are in parentheses. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

IV approach relies on two key assumptions. First, the instrument must impact on the endogenous variable. This assumption is valid in our case, as eligibility for ER clearly reduces own employment.[29] Second, the exclusion restriction requires that the instrument is conditionally independent of the potential outcomes and only affects the outcomes through the first-stage channel specified in (4).[30]

The results from the IV method are shown in Table 6. Panel A shows that men's employment in a given year increases their female spouses' employment by, on average, 17.4 percentage points (23%). We further estimate that men's employment decreases their female spouses' take-up of DI by, on average, 13.9 percentage points (84%). We find no effects of women's employment on their male spouses' employment or DI (panel B).[31]

We can directly use these IV estimates to quantify the social multiplier. In our setting, the aggregate effect is the total effect on the household, including the indirect effect from the treated individual to the spouse. Our IV estimates imply that, when married men retire, the aggregate macro effect is 17% higher than the micro response, which corresponds to a social multiplier of 1.17. In other words, if the policy induces one hundred married men to retire early, an additional

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[29] The first stage *F*-statistic is 676 for men's employment and 213 for women's employment.

[30] Instead of using ER eligibility as an IV for employment, one can use ER eligibility as an IV for retirement. We focus on employment as we are interested in understanding interdependencies in leisure. Focusing on retirement would mute some of this effect due to the cross-program spillovers that we identified in the paper. Specifically, the retirement effect is not exclusively due to individuals exiting the labour market, but also due to individuals that have already left the labour market switching into retirement from other social insurance programs. Thus, if we were to use ER eligibility as an IV for retirement rather than employment, we would not be able to isolate the effect that is driven by the individual exiting the labour force. Having said that, with a first-stage employment effect of 0.22 and a first-stage retirement effect of 0.27 (Table 2), using retirement instead would yield a relatively similar result.

[31] This follows mechanically, since we do not find any statistically significant intent-to-treat effects of women's eligibility for early retirement on their male spouses' outcomes.

seventeen female spouses will drop out of employment due to indirect spousal effects induced by the policy.

6. Discussion and Conclusion

Anticipating the labour market effects of welfare reforms is difficult due to public policy interactions across programs and household members. Specifically, changes to one program may affect individual take-up of other programs, and individual participation in specific programs may generate labour market responses from other household members. Little work has been able to comprehensively examine the extent and magnitude of such interactions.

In this paper, we exploit the introduction of an early retirement reform in Norway in combination with rich administrative data to study (1) the direct effect of the reform on individual labour market behaviour, (2) the cross-program effect of the reform on individual participation in other social insurance programs and (3) the indirect effect of the reform on spousal labour market and social insurance participation. We first show that the reform had a substantial impact on the labour supply of individuals who became eligible for early retirement, reducing the probability of employment by more than 30%. We then show that the increased take-up of early retirement had an offsetting effect on the take-up of alternative social insurance programs. Next, we demonstrate that the reform had a negative impact on the labour supply of spouses to early retirement eligible individuals, with an effect size of approximately 5%. We speculate that this is driven by interdependencies in couples' retirement decisions. Finally, we show that the reduced labour force participation among spouses is accompanied by a significant increase in the take-up of disability insurance.

We reveal interesting effect heterogeneity across genders. First, the direct program spillovers are stronger for men than women. We show that this is likely due to the fact that fewer women than men qualify for early retirement, such that their ability to substitute across programs is smaller. It is also consistent with the relative benefit of switching from DI to ER being lower for women. The reason for this is that the compensation rate for DI is greater than the compensation rate for ER for low-income individuals, and the overwhelming majority of women in our sample have lower income than the men. Second, the spousal responses are only present among women, especially with respect to take-up of DI. We believe that this is likely because the majority of women in our sample are secondary household earners, such that the household cost associated with husbands of ER-eligible women leaving the labour force is much greater than the household cost associated with wives of ER-eligible males men leaving the labour force.

In terms of policy implications, our analysis shows that the labour market effects of welfare reforms may extend beyond the direct program that is being targeted and may affect other individuals than those directly implicated by the reform. The interactions in public policies across programs and among household members we document can have important consequences for welfare reforms, and this paper highlights the importance of taking the full matrix of effects into account when making adjustments to welfare programs.

Using official statistics of the per person costs associated with the different social insurance programs in Norway coupled with the results from our analysis, we can ask how the cost of the reform would differ in a world where the program substitution and spousal spillovers are taken into account, compared to a world in which we consider the associated elasticities to be zero.[32]

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[32] The per person costs are calculated for men and women aged 62 to 66 in the period 1998 to 2007 (our observed post-reform period).

Among men (women), the program substitution effects increase the per person cost of the ER program by 6% (2%).[33] The indirect spousal effect on female (male) spouses increases the per person cost of the ER program by an additional 12% (4%). This implies that the cost of the reform among men (women), taking the associated elasticities into account, is 18% (6%) greater than if there were no program substitution and spousal spillovers. The magnitude of this difference is both statistically significant and economically meaningful and shows that a social planner who assumes zero substitution and spillover elasticities will significantly underestimate the cost of the reform.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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[33] We calculate the increased costs coming from program substitution as the sum of the decrease in program participations times the difference in the yearly per person costs for ER and that particular program and take the ratio of that sum over all programs to the increase in ER participation times the cost of the ER program. Because the marginal person switching from other programs to ER is likely to qualify for higher than average benefits in these other programs, this is likely an overestimate of the total costs coming from program substitution.

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