Interactions in Public Policies: Spousal Responses and Program Spillovers of Welfare Reforms

BY Julian Vedeler Johnsen, Kjell Vaage & Alexander Willén

DISCUSSION PAPER

Institutt for samfunnsøkonomi
Department of Economics

SAM 20/2020

ISSN: 0804-6824
September 2020

Interactions in Public Policies:
Spousal Responses and Program Spillovers of Welfare Reforms

By Julian Vedeler Johnsen, Kjell Vaage and Alexander Willén*

September 2020

Abstract:

Anticipating the labor market effects of welfare reforms is difficult due to public policy interactions across programs and among household members. Specifically, changes to one program may affect individual take-up of other programs, and individual participation in specific programs may generate labor market responses from other household members. This paper exploits an early retirement reform in Norway to provide new insights into these interactions. We first show that the reform had a substantial impact on the labor supply of those individuals who were directly affected by the reform, reducing the probability of employment by more than 30 percent. We then demonstrate that the increased take-up of early retirement had an offsetting effect on the take-up of alternative social security programs. Next, we reveal that the reform had a negative indirect impact on the labor supply of spouses of individuals directly affected by the reform, with an effect size of 5.5 percent. Finally, we show that the indirect effect on spousal labor force participation is accompanied by a significant increase in spousal take-up of disability insurance. We conclude that neglecting how public policies interact across both programs and household members can result in a miscalculation of the total impact of welfare reforms.

KEYWORDS: Public Policy, Welfare Reform, Early Retirement

JEL CODES: H55, J14, J18, J26

* Johnsen: SNF - Center for Applied Research at NHH (e-mail: julian.johnsen@snf.no). Vaage: Department of Economics, University of Bergen (e-mail: Kjell.Vaage@uib.no) Willén: Department of Economics, Norwegian School of Economics (e-mail: alexander.willen@nhh.no). We gratefully acknowledge comments from three anonymous reviewers, Espen Bratberg, Gordon Dahl, Nezih Guner, Katrine Løken, Teis Lømo, Oivind Anti Nilsen, Katrine Holm Reiso, Justin Valasek, Barton Willage, and participants in several seminars, conferences, and workshops. This research is partially supported by the Research Council of Norway through Project No. 257598 and through its Centers of Excellence Scheme, FAIR Project No. 262675.

1. Introduction

Most OECD countries operate a complex web of social security 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 how individuals may respond to such reforms is difficult owing 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 labor 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 among household members. While a small and growing literature examines the cross-program spillovers of government welfare programs (e.g., Johnsen and Reiso, 2020; Brown et al., 2020; Elwell, 2019; Hernæs et al., 2016; Vestad, 2013), and a rapidly growing literature examines household complementarities in labor supply and leisure (e.g., Sánchez-Marcos and Bethencourt, 2018; Stancanelli, 2017; Selin, 2017; Lalive and Parrotta, 2016; Kaygusuz, 2015), this is the first paper to trace the full 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 underestimate the full impact of the reform.

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 for retirement in other firms. Using rich population-wide registry data, we investigate (1) the direct effect of the reform on individual labor market behavior, (2) the cross-program effect of the reform on individual participation in other social security programs, and (3) the indirect spillover effect of the reform on the labor market and social security 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 security 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 reforms may therefore result in an underestimation of the full impact of these reforms.

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

Having established the direct effect of the reform and shown the existence of important cross-program spillovers, we investigate whether the change in retirement behavior and social security participation among individuals directly affected by the reform had an indirect effect on the labor market outcomes of their spouses. Specifically, we examine whether spouses of individuals directly affected by the reform become more likely to leave the labor force themselves, and if so, whether this has an impact on their take-up of alternative social security programs. Such indirect spousal labor supply complementarities have been argued to represent a main reason for why individual-level estimates of labor supply often provide lower estimated elasticities than most macroeconomic calibrations (Chetty et al., 2011).

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 did 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 and female spouses when discussing the indirect effects of the reform on the spouses of directly affected individuals.

The source of variation we exploit comes from changes in outcomes among individuals employed at firms affected by the early retirement reform compared to changes in outcomes among individuals employed at firms not affected by the early retirement 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 labor supply, and the indirect effect on spousal participation in other social security 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 early retirement reform that differentially affect the labor market and social security 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 the results of a nonparametric event study that directly 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 simply identifying effects off of secular trends. Third, we perform a battery of sensitivity analyses on our sample selection and model specification. The results from these exercises are inconsistent with plausible sources of bias from other programs or trends, and support a causal interpretation of our estimates.

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

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

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 and current earnings 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 female spouses, 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 labor 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 labor force is much greater.[2]

To examine this possibility in more detail, we estimate a modified version of our main specification in which we interact the treatment variable with the within-household income gap. Interestingly, when we fix the income gap at zero, we find statistically 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 the idea that differences in the household cost of joint exit from the labor market could drive 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 codetermination in decisions relating to labor supply (Goux, Maurin, and Petrongolo, 2014; Hospido and Zamarro, 2014), health behaviors (Fletcher and Marksteiner, 2017; McGeary, 2015), and social insurance (Boyle and Lahey, 2016; Witman, 2015). However, none of these papers have examined how an individual's participation in a specific welfare program may affect the spouse's participation in other welfare programs. By focusing on such cross-program indirect spousal effects, we are able to contribute to this literature and improve our knowledge on the importance of codetermination in household decisions.

Second, there is a rich literature on interdependencies in couples' retirement decisions. The earlier research in this field exploits cross-sectional data to examine spousal correlation in

[2] To the extent that the direct effect of eligibility for early retirement 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 fulfill the individual eligibility criteria for early retirement (see Sections 2 and 4), such that the direct effect scaled by the fraction eligible is larger for women than men.

retirement decision (Coile, 2004; Zweimüller, et al. 1996; Hurd, 1990), 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 labor market participation and home production (e.g. Kruse, 2019; Bloemen et al., 2019; Stancanelli, 2017; Selin, 2017; Cribb et al., 2016; Lalive and Parrotta, 2016; Atalay and Barrett, 2015; Stancanelli and Van Soest, 2012; Kapur and Rogowski, 2007).[4] While the results from this literature are relatively mixed, most of these studies find evidence of interdependencies in couples' retirement decisions.[5] Our main contributions to this literature are twofold. First, we provide complementary evidence on the interdependencies in couples' retirement decisions by studying a reform that generated a more substantial shift in retirement eligibility (reducing the pension eligible age from 67 to 62 years), which permits us to better isolate the spousal spillover effects. Second, we provide the first evidence in the literature on which labor market exit routes spouses may take if they decide to exit in response to the reform. That is, do they simply leave the labor force or do they exit through other social welfare programs? The results from this analysis have interesting policy implications. Specifically, they show that neglecting the interactions of public policies across programs and among household members can result in a sizable underestimation of the total cost of welfare reforms.

Finally, our setup enables us to develop further the literature on cross-program spillovers of government welfare programs. Specifically, a number of studies have explored how changes to the eligibility requirements of specific welfare programs, such as Medicaid in the US, affect the eligibility and take-up of other safety net programs (Brown et al., 2020; Elwell, 2019; Baicker et al., 2014; Decker and Selck, 2012; Shore-Sheppard, 2008). With respect to program

[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) exploits cohort differences in the participation rates of older women and shows that a wife's labor force participation decision positively affects her husband's participation decision. Using data from Australia, Mavromarasa and Zhub (2015) document a similar relationship between a wife's labor 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 find that the elimination of these policies lead to increases in the labor 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; Cribb et al., 2016; Atalay and Barrett, 2015) 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, 2019). The exception is Selin (2017), who finds no evidence of spousal spillovers. However, Selin (2017) examines a retirement reform that primarily affected women, and is therefore restricted to looking at whether husbands respond to wives' retirement decisions.

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 labor market (Hernæs et al., 2016; Staubli and Zweimuller, 2013; Vestad, 2013; Duggan et al., 2007; Bratberg et al., 2004; Røed and Haugen, 2003). Our main contribution to this literature is to show that such program spillovers are not limited to those individuals affected by the reform, but also extend to other members of the household. To our best knowledge, this has not been documented before.

2 Institutional Background[6]

2.1 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 labor force participation among older workers is high. During our analysis period, around 70 percent of men, and 60 percent of women, aged 55 to 64 years were employed.[8]

The public state pension is administered by the Norwegian Labor and Welfare Administration (NAV). The pension comprises two main components. First, a fixed pension paid in full to individuals who have resided in Norway for at least 40 years, and gradually reduced for individuals with shorter resident histories.[9] The second component is an earnings-based pension paid to individuals with at least 3 years of employment history, in which earnings translate into pensions at a rate that decreases with earnings until it eventually reaches a cap. Combined, these two components suggest that the replacement rate in the public pension system declines with earnings.

The early retirement (ER) scheme was introduced on January 1, 1989. This was the result of a comprehensive collective bargaining agreement between the Norwegian Confederation of Trade Unions (the largest umbrella organization of labor unions in Norway) and the Confederation of Norwegian Enterprise (the largest employers' organization in the

[6] This section describes the Norwegian social security programs that were in place during our analysis period (1993 - 2007). Since then there have been several minor and major reforms, most notably a pension reform in 2011 and a reform of the disability benefit system in 2015. However, they do not affect our analysis and are therefore not discussed in this section.

[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.

country), co-sponsored by the government. With respect to the motivation behind the ER scheme, the objective was to provide "worn-out" workers with a dignified exit route from the labor 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 labor 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 enrollment 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 gradually 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 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, and as noted above, 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 elderly 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.

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 entire early

retirement period. It is also important to note that ER benefits are conditional on withdrawal from employment. These two factors suggest that the ER scheme provides a strong work disincentive for ER eligible workers.

Finally, the fixed amount of ER benefits is 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 in the ER scheme. The exact same rules apply to the DI scheme, in which married individuals also receive a 25% lower fixed amount compared to unmarried individuals. It is worth noting that we focus exclusively on married individuals in this paper. Thus, this does not pose a concern for our analysis.[10]

2.2 Alternative Pathways to Early Retirement

All permanent residents of Norway are automatically enrolled in the public social security system, commonly 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 percent of his/her earnings, levied as an automatic payroll deduction. It is important to note that – 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. With respect to the goal of the current paper – to examine interactions in public policies across programs and among household members driven by the ER reform – three social security programs are of particular interest: DI, UI, and SL. We focus on these programs because they are the largest (non-pension) social security programs in Norway, and represent clear alternative exit routes – both temporary and permanent – from the labor market.[11]

[10] The ER program has been subject to additional changes over the years. The most recent of these additional changes took place in 2011, in which work incentives among the elderly was increased. More specifically, the 2011 reform removed a confiscatory earnings test among private sector workers with access to the early retirement scheme, making it more financially attractive to continue working. This reform targeted a slightly different population than that examined in our analysis, and had a positive work incentive effect as opposed to the negative work incentive effect studies in our setting. To the best of our knowledge, there are currently no published papers that have examined the joint retirement effects of this reform, but there are two ongoing projects that examine this question: Bratsberg and Stancanelli (2018) as well as Kruse (2019). Both these papers identify spousal spillover effects associated with increased work incentives among the elderly.

[11] 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

SL benefits provide compensation for income loss caused by a temporary illness or injury. The replacement rate is 100% from day one subject to a maximum amount ($62,000 in 2019). To be entitled to SL benefits, an individual must have been in employment for the past four weeks. Long-term sick leave (beyond three days) requires a certificate from a doctor, or – if the injury is related to the muscular-skeletal system – a chiropractor or manual therapist. SL benefits are paid by the employer for the first 16 days, and then by the government for a maximum of 52 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 appropriate 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 percent for low-income groups, but is decreasing in income. The after-tax replacement rate for fully disabled, previously average earners, is around 65 percent (Blöndal and Pearson, 1995).

UI is available to individuals who have had their work hours reduced by at least 50 percent, are registered as jobseekers and submit an employment status form every 14 days, and had an income over a certain minimum amount ($16,500 in 2019) before becoming unemployed. The replacement rate is 62 percent 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).

---

significantly less 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. While this is informative for shedding light on the composition of individuals across the different programs, the differences in lifetime earnings and educational attainment 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 percent) intended to provide support rehabilitation to facilitate reintegration into the labor 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.

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

DI, SL, and UI all represent potential exit routes from the labor market among older workers who do not yet qualify for retirement. However, some of these social security programs are more likely exit routes than others. First, sick leave is only offered on a temporary basis and for a maximum of 52 weeks, making this a relatively unlikely choice for someone looking to permanently leave the labor 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 security system. There is also an inconvenient time cost as jobseekers must be registered as jobseekers and submit employment status forms every 14 days. Finally, disability insurance is associated with generous benefit levels and provides a permanent exit from the labor 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). This suggests that there may be important effects of the retirement reform on the individual – and spousal – take-up of DI, whereas it is less likely to affect UI and SL take-up.

2.3 Conceptual framework

To analyze the direct labor market effect of the ER reform on eligible individuals and the indirect labor market effects of the ER reform on the spouses of ER eligible individuals, we present a simple quantitative model of an individual's retirement decision and how this may affect the spouse's labor market behavior. We begin by considering the direct impact of the reform on affected individuals, and then extend the conceptual framework to consider the indirect effect on the spouse. Note that we make a number of simplifying assumptions and that the model is intended as a purely conceptual 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).[14]

Our starting point is an individual choosing between three alternative labor market states: work (W), retire (R), or exiting the labor force through some other welfare program such

[14] An advantage of approaching this question from a reduced-form perspective 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).

as DI, UI, or SL (O). We assume that these states are mutually exclusive and collectively exhaustive ($W,\ R,\ O \in \{0; 1\}$ and $W + R + O = 1$), 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*). To keep the model tractable, we assume that the individual receives no leisure if working, but full leisure if not working. We express this simple optimization problem for individual *i* as follows:

$$\max U(c_i, l_i) = \alpha_i c_i + \beta_i l_i \tag{1}$$

$$\text{s.t.} \quad c_i = \begin{cases} w_i \ if \ W_i = 1 \\ o_i \ if \ O_i = 1 \\ r_i \ if \ R_i = 1 \end{cases} \tag{2}$$

$$l_i = \begin{cases} 1 \ if \ W_i = 0 \\ 0 \ if \ W_i = 1 \end{cases} \tag{3}$$

where *c* is consumption, *l* is leisure, *w* is the monetary reward associated with working, *r* is the monetary reward associated with retirement, and *o* is the monetary reward associated with labor force exit through some other welfare program. The preference parameters for consumption and leisure are denoted as α and β, respectively.[15]

In this framework, individual *i* chooses between three labor market states to maximize utility from consumption and leisure subject to a budget constraint and a leisure constraint. We allow for heterogeneity in the relative values of *w*, *r*, and *o*. Substituting (2) and (3) into (1), we get that the utility from working (U^W) is $\alpha_i w_i$, the utility from retirement (U^R) is $\alpha_i r_i + \beta_i$, and the utility from exiting on other programs (U^O) is $\alpha_i o_i + \beta_i$. The individual will choose to retire if $U^R > U^W, U^O$, implying that the following two conditions must hold:[16]

$$\frac{\beta_i}{\alpha_i} > w_i - r_i \text{ and } r_i > o_i \tag{4}$$

That is, the individual will choose to retire on retirement benefits if (a) the individual's preference for leisure over consumption is greater than the income difference between the wage and the retirement benefits and (b) the retirement benefits are greater than the benefits that can be received from other welfare programs. While (a) is required to ensure that state *R* is preferred

[15] We assume perfect substitutability between leisure and consumption to make the conceptual framework more tractable; the results from this section extend to settings in which this assumption is relaxed.

[16] $U^R > U^W$ implies that $(\alpha_i r_i + \beta_i > \alpha_i w_i)$, which equals $(\frac{\beta_i}{\alpha_i} > w_i - r_i)$. $U^R > U^O$ implies that $(\alpha_i r_i + \beta_i > \alpha_i o_i + \beta_i)$, which equals $(r_i > o_i)$.

to state *W*, (b) is necessary to ensure that state *R* is preferred to state *O*.[17] Fixing the monetary payoffs associated with the different states, the propensity to retire is thus increasing in the preference for leisure (β_i) and decreasing in the preference for consumption (α_i).

Based on this simple framework, we can predict the likely effect of the reform on individuals who would have chosen *W* absent the reform, as well as on individuals who would have chosen *O* absent the reform:

Type	**Choice before the reform**	**Choice after the reform**
$\frac{\beta_i}{\alpha_i} > w_i - o_i$	O	O if $o_i > r_i$ R if $o_i < r_i$
$\frac{\beta_i}{\alpha_i} < w_i - o_i$	W	W if $o_i > r_i$ and $\frac{\beta_i}{\alpha_i} < w_i - r_i$ R if $o_i < r_i$ and $\frac{\beta_i}{\alpha_i} > w_i - r_i$

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 $w_i - o_i > \frac{\beta_i}{\alpha_i} > w_i - r_i$ such that individuals prefer *W* to *O*, but *R* to *W* ($U^R > U^W > U^O$). Second, the reform will induce program substitution among individuals on alternative welfare programs if $\frac{\beta_i}{\alpha_i} > w_i - o_i$ and $o_i < r_i$ such that individuals prefer *O* to *W*, but *R* to *O* ($U^R > U^O > U^W$).[18]

The above discussion demonstrates that the ER reform may not only encourage working individuals to retire, but it may also induce program substitution from other welfare programs to retirement. This has potentially important policy implications as the per person program cost for ER is higher than the per person program cost for these other programs. In Section 4, we will show that the program substitution effects represent a relatively sizable cost. This highlights the importance of considering program substitution effects when designing welfare reforms, and that we may underestimate the cost of the reform if assuming that the substitution elasticities are zero.

Having examined the direct impact of the reform on affected individuals, we extend the conceptual framework to analyze the indirect effect of the ER reform on the spouses of these

[17] If $r_i < o_i$, individual *i* would exit the labor market through some other welfare program rather than retirement.
[18] Without heterogeneity in *r* and *o*, the reform would either have no effect on retirement (if $o > r$) or result in complete program substitution *(*if $r > o$).

directly affected individuals.[19] To this end, we consider an expanded version of the maximization problem presented above in which *i* denotes the ER eligible individual and *j* denotes *i*'s spouse:

$$\max U(c_i, c_j, l_i, l_j) = \alpha_j(c_i + c_j) + \beta_j(l_i + l_j) + \gamma_j l_i l_j \tag{5}$$

$$\text{s.t.} \quad c_{i/j} = \begin{cases} w_{i/j} \; if \; W_{i/j} = 1 \\ o_{i/j} \; if \; O_{i/j} = 1 \\ r_i \quad if \quad R_i = 1 \end{cases} \tag{6}$$

$$l_{i/j} = \begin{cases} 1 \; if \; W_{i/j} \quad = 0 \\ 0 \; if \; W_{i/j} \quad = 1 \end{cases} \tag{7}$$

where γ allows for (but does not require) a preference for joint leisure within the household. All other variables are defined as above.

The spouse chooses between working and exiting the labor market through some other welfare program. This is because retirement is by design not a state available to the spouses in our setting.[20] The reform can therefore only have an indirect effect on the spouses of individuals directly affected by the reform. The spouse will choose to exit through some other welfare program if $U_j^O > U_j^W$, but note that spouse *j*'s utility from state W_j and state O_j will depend on which state individual *i* chooses. Substituting (6) and (7) into (5), we get the following conditions for $U_j^O > U_j^W$ depending on which state individual *i* chooses:

$$\frac{\beta_j+\gamma_j}{\alpha_j} > w_j - o_j \text{ if } O_i = 1 \text{ or } R_i = 1, \qquad \frac{\beta_j}{\alpha_j} > w_j - o_j \text{ if } W_i = 1 \tag{8}$$

That is, if individual *i* is working ($W_i = 1$), spouse *j* will choose to exit the labor market if the preference for individual leisure over consumption is greater than the income difference between the wage and the income received from the other welfare program ($\frac{\beta_j}{\alpha_j} > w_j - o_j$). If individual *i* is on other benefits or retired ($O_i = 1$ or $R_i = 1$), spouse *j* will choose to exit the labor market if the preference for individual and joint leisure over consumption is greater than the income difference between the wage and the income received from the other welfare program ($\frac{\beta_j+\gamma_j}{\alpha_j} > w_j - o_j$).

[19] Note that we have chosen to present this as a sequential decision process. This is purely for facilitating the interpretation of the results; it has no impact on the stylized facts presented in this section.
[20] Recall that we exclude spouses who qualify for early retirement.

The above framework implies that if the reform moves individual *i* from *O* to *R*, it will have no impact on the choice of the spouse, as it does not affect the conditions for $U_j^O > U_j^W$.[21] In other words, the reform will have no indirect spousal effect through program substitution of individual *i*. However, if the reform moves individual *i* from *W* to *R*, it will increase the likelihood that the spouse exits on other benefits if $[(U_j^O - U_j^W)|R_i = 1] > [(U_j^O - U_j^W)|W_i = 1]$. This would only be true if $\gamma_j > 0$.[22] In other words, if the reform causes individual *i* to retire, this will increase the probability that spouse *j* exists the labor force only if the spouse derives utility from joint leisure. If the spouse gets disutility from joint leisure, or if spousal leisure are substitutes, the reform will reduce the rate of labor exits among spouses.

With respect to the current analysis, it is important to note that our empirical strategy consists of estimating a reduced-form model in which we compare the labor supply and social security program participation of spouses to ER eligible individuals with spouses to non-ER eligible individuals in a difference-in-differences framework. This means that we cannot directly identify the preference parameters α, β, and γ. However, 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 have important policy implications, potentially leading to reduced tax revenue and increased public spending on welfare programs. In Section 4, we will show that these spillover effects yield economically meaningful increases in the cost of the ER program, highlighting their importance when designing welfare reforms.

Taken together, this conceptual framework is useful for considering the importance of the potential 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, nor provide an account of the optimal choice of some policy instruments in this framework, we can identify the extent of the cross-program and indirect spousal spillover effects. Furthermore, we can provide information on the relative importance of the cross-program and indirect spillover effects. In the Section 7, we use the results from our empirical exercise to estimate the cost of the reform in a world where the social planner recognized these spillovers and interactions,

[21] $[(U_j^O - U_j^W)|R_i = 1] = [(U_j^O - U_j^W)|O_i = 1]$.

[22] To see this, note that $((U_j^O - U_j^W)|R_i = 1) = (\alpha_j(r_i + o_j) + 2\beta_j + \gamma_j)$- $(\alpha_j(r_i + w_j) + \beta_j) = \alpha_j(o_j + w_j) + \beta_j + \gamma_j$. Further note that $((U_j^O - U_j^W)|W_i = 1) = (\alpha_j(w_i + o_j) + \beta_j)$- $(\alpha_j(w_i + w_j)) = \alpha_j(o_j + w_j) + \beta_j$. Therefore, $((U_j^O - U_j^W)|R_i = 1) > ((U_j^O - U_j^W)|W_i = 1))$ requires that $\alpha_j(o_j + w_j) + \beta_j + \gamma_j > \alpha_j(o_j + w_j) + \beta_j$, or that $\gamma_j > 0$.

compared to a world in which the social planner considers the associated elasticities to be zero. Of course, the calculations we provide are restricted to Norway and its ER reform. While the general principle of such substitution and spillover effects apply to other countries and settings, the extent of these effects will depend on the relative benefits associated with the available welfare programs that individuals and spouses can utilize.

Finally, we note that any indirect effect of the reform on the spouse's decision to exit the labor market need not only be due to within-household complementarities in leisure, but could also be due to indirect effects on the spouse's health. While the above model abstracts from this possibility, we discuss it at length in Section 4.

3 Data and Method

3.1 Data

We rely on detailed population-wide administrative data from 1988 to 2007, drawn from several registries of the Statistics Norway database. First, we use a matched employer-employee registry to obtain information on earnings, work hours and place of work (firm identifier). Second, we use a demographic population registry to collect information on birth year and marital status. Third, we use a range of social security registries to obtain complete information on the take-up of public welfare and social security programs, most importantly early retirement, disability insurance, unemployment insurance, and sick leave.

The key strength of our data is that we can link individuals across these different data sets through unique individual identifiers, allowing us to merge the social security data with the employer-employee data and the demographic data. Using a unique family identifier, we are further able to link all married individuals to their spouses, providing us with a comprehensive dataset on earnings, employment, and social security, for all individuals and their spouses.

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 classification approach likely results in some measurement error, as treated firms will erroneously be classified as control firms if they do not have any workers taking up early retirement during our analysis period. The risk of misclassifying a treated firm as a control firm is greater the smaller the firm is, and in our main analysis we therefore omit

firms with 10 or fewer employees.[23] Any potential misclassification of treated firms as control firms will result in an attenuation bias (bias toward zero). To the extent that such misclassification exists, our results are best interpreted as a lower bound of the true effect.

As described in Section 2.1, the early retirement 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). To facilitate the interpretation of our results, 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 labor force will be unaffected by the reform, we restrict our sample to individuals who worked at the age of 57. Appendix Table A2 shows the cohorts included in our main 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 potentially endogenous, we rely on pre-reform (1988) affiliation to identify the treatment status of each worker.[24] We therefore drop any individuals (and spouses) with missing information on firm affiliation in 1988 as well as individuals (and spouses) working in firms that cannot be tracked during the 1990s and 2000s when our outcome variables are measured.

Second, a main focus of this paper is to examine if the early retirement reform had an indirect effect on the labor market behavior and social security take-up of the spouse. To isolate this effect, we restrict our sample to couples in which the spouse worked in a control firm in 1988. This means that there cannot be a direct effect of the reform on the spouses as they are ER ineligible.

Our third and final sample restriction is to drop individuals (and their spouses) whose spouses are older than they are. We impose this restriction to ensure that all spouses in our sample have the option to respond to the individual's early retirement decision in all years before that individual reaches the standard retirement age of 67. While this restriction does not have a significant effect on men since wives tend to be a couple of years younger than their

[23] Our results are robust to changing this threshold; results using alternative cutoffs are presented in Appendix B.
[24] This potential endogeneity is due to two reasons. First, individuals in non-ER firms face an incentive to switch to ER firms in order to reap the benefits of the early retirement reform. Second, if a spouse of a treated individual works in a non-ER firm, s/he also face an incentive to switch to ER firms due to preferences for joint retirement. We examine this in detail in Section 4.

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. With respect to our analysis, approximately 25 percent of all women that meet our sample restrictions, excluding the age gap requirement, have husbands that are not older than them. Interestingly, the women married to younger men are relatively similar to the women married to older men, both in terms of educational attainment and earnings. Specifically, the difference in educational attainment between these two groups of women is less than 0.5 years (4.5 percent), and the difference in annual earnings between these two groups of women is less than 15,000 Norwegian krone (or € 1,500). The husbands of older women are also relatively comparable to the husbands of younger women in terms of educational attainment, although they have earnings that are approximately 15 percent larger (on average). This is perhaps unsurprising, as many of the husbands to younger women have already left the labor force due to old age. Summary statistics on women (and their male spouses) stratified by whether the husband is older or younger than the woman is shown in Appendix Table A3.

Descriptive statistics of our analysis sample are provided in 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 preliminary descriptive evidence on the direct effect of the reform on early retirement take-up and employment status. The figure plots raw trend in early retirement (Panels A and B) and employment (Panels C and D) 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 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 (62 years), both with respect to early retirement and employment. While this result follows by construction for early retirement take-up (since individuals below the age of 62 are ineligible for early retirement), this is not the case for employment, and provides suggestive evidence in favor of the parallel trend assumption required for our main analysis. Second, once the individuals reach 62, there is a substantial jump in the probability of early retirement take-up, and a substantial 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.

Taken together, Figure 2 demonstrates that the reform likely had an economically meaningful direct effect on the early retirement and employment behavior of individuals. Figure 2 also highlights that our results should be interpreted as intent-to-treat effects, and that they likely represent a lower bound of the full program effect. While we cannot use Figure 2 to make causal inference, it is instructive for understanding the underlying idea behind our identification strategy and for demonstrating that the identification assumption of parallel trends is likely to hold.

3.2 Empirical Strategy

To identify the direct effect of the reform on early retirement, labor supply, and the take-up of alternative social security programs, we compare the outcomes of individuals in firms that participate in the ER scheme to those of individuals in firms that do not participate in the ER scheme through a difference-in-differences approach. As post-reform firm affiliation is endogenous, we use pre-reform (1988) affiliation as a proxy for the treatment status of each worker. We estimate models of the following form:

$$Y_{it} = \alpha + \beta_1(FirmEligible_i * AgeEligible_{it}) + \delta_i + \tau_t + \theta_{it} + \varepsilon_{it}, \quad (9)$$

where Y_{it} is one of the labor market or social security outcomes discussed above for individual *i* at time *t*: take-up of early retirement, employment, and the use of alternative welfare programs (DI, UI, and SL). $AgeEligible_{it}$ is a dichotomous variable taking the value of 1 if the worker is above the early retirement age of 62 and 0 otherwise. $FirmEligible_i$ 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 early retirement reform on Y. Since treatment is based on pre-reform firm affiliation, β_1 should be interpreted as an intent-to-treat effect.

Equation (9) 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 systematic differences across individuals that could potentially confound our results, the year fixed effects absorb any time-specific events that affect all individuals similarly, and the age fixed effects account for

systematic differences in Y across age cohorts.[25] We estimate equation (9) separately for men and women.

To examine the indirect effect of the reform on spouses to ER-eligible individuals– both in terms of labor supply and take-up of alternative social security programs – we estimate equation (9) using spousal labor market and social security outcomes as dependent variables.[26] The individual fixed effects in equation (9) can be viewed as a household fixed effect when estimating the equation using spousal outcomes as the dependent variables.

Conditional on the controls and the fixed effects in equation (9), 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 underlying assumption, we employ nonparametric 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 following model:

$$Y_{it} = \alpha + \sum_{a=58}^{66}(\pi_a[FirmEligible_i * Age_{ita}]) + \delta_i + \tau_t + \theta_{it} + \varepsilon_{it}, \quad (10)$$

for each $a \in [58(1)66]$, where a is the age of the individual. The coefficients of interest, π_a, allow us to nonparametrically trace out relative pre-treatment trends (for π_{58} to π_{61}) 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). 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 equation (10) is that it permits identification of time-varying treatment effects (π_{62} through π_{66}), which is interesting as it is possible that the direct effect of the reform, as well as the cross-program and

[25] 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. Specifically, we use pre-reform (1988) firm affiliation as a proxy for the treatment status of each worker. As such, firm affiliation is a time-invariant characteristic of the individual, and any systematic differences across individuals derived from the fact that they are affiliated with different firms are already controlled for by the individual fixed effects.

[26] Note that we cannot include spousal age fixed effects due to perfect collinearity. However, our results are robust to replacing individual age fixed effects with spousal age fixed effects. Our results are also robust to including both a linear and a quadratic control for spousal age (Section 5).

cross-household effects of the reform, differ in magnitude as individuals approach the standard retirement age of 67.[27]

While equations (9) and (10) are valuable for identifying the full matrix of effects associated with the reform across programs and household members, it is 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 (LATE), which is the average effect of spousal employment on own employment for the compliers in our sample (Imbens and Angrist, 1994).[28] The instrumental variable approach is useful for better understanding interdependencies in couples' retirement decisions and the likely spousal employment response to the ER reform given that the ER reform bites. To keep the exposition clear, we first discuss the ITT effects (Sections 4 and 5) and then provide the results from the instrumental variable approach (Section 6).

4 Results

4.1 Direct Effect on Early Retirement, Employment, and Social Security Take-Up

Table 2 presents baseline estimates of the direct effect of the ER reform on early retirement take-up, employment, and participation in alternative social security programs. Each cell in each column comes from a separate estimation of equation (9), controlling for calendar year, age, and individual fixed effects. We discuss the effects for men (Panel A) and women (Panel B) in turn.

i. 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.

[27] In addition to estimating nonparametric 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 in the ER scheme 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.

[28] In our setting, the compliers are the couples in which the individual chooses to retire earlier because s/he was eligible for ER, but would not have retired earlier had s/he not been eligible for ER.

Specifically, 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 with men not affiliated with an ER firm in 1988. This demonstrates that the ER reform had a substantial effect on retirement, and that it may affect not only the labor force participation of eligible individuals, but also their participation in other social security programs and how their spouses interact with the social security system.

The direct effect of the ER reform on the early retirement behavior of men shown in column (1) should likely lead to a drop in employment among these individuals, as early retirement benefits are conditional on withdrawal from employment. To examine this in detail, column (2) of Panel A in Table 2 reports the coefficient obtained from estimating equation (9) using employment as the dependent variable. The results show that the introduction of the reform led to a substantial drop in employment, reducing the labor force participation of eligible men by approximately 22 percentage points. This represents a 35 percent reduction in employment relative to the mean, shown directly below the point estimate in the table. That the ER reform reduced employment of older workers has been shown before by Vestad (2013). Our results are comparable to his.

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 security programs to early retirement.[29] To explore the existence of such substitution effects and program spillovers, columns (3) through (5) show estimates of equation (9) in which participation in the welfare programs discussed in Section 2 are used as dependent variable: DI, UI, and SL. Looking across the columns, there is a statistically significant and negative effect across all three alternative social security programs. These results are consistent with the conceptual framework outlined in Section 2.3, showing that the ER reform may push eligible individuals into retirement through two distinct channels, not only inducing working individuals to retire but also inducing program substitution among individuals on alternative welfare programs.

In terms of magnitudes, the negative effect on DI is substantially larger than the others, accounting for approximately 60 percent of the entire cross-program spillover effect. In total, cross-program substitution accounts for almost a third of the entire reform effect on ER take-

[29] 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.

up in our setting. This suggests that analyses ignoring such program substitution effects are likely to underestimate the full impact of the reform.

ii. 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, albeit slightly smaller than that among men. However, even though the point estimate of 0.23 is smaller than the point estimate for men (0.27), it remains both economically meaningful and statistically significant at the 1 percent level. This result shows that women affiliated with an ER firm in 1988 are 23 percentage points more likely to enter early retirement during the ages of 62 through 66 compared with those not affiliated with an ER firm in 1988.

Column (2) of Panel B in Table 2 demonstrates that, just as with men, the reform is associated with a substantial reduction in the likelihood of being employed. Specifically, eligible women are 22.1 percentage points more likely to leave the labor 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 percent reduction in employment relative to the mean, shown directly below the point estimate in the table. The large reduction in employment is expected given the fact that take-up of early retirement benefits in the Norwegian setting is conditional on withdrawal from employment.

The employment result in column (2) can explain almost the entire early retirement effect on women, suggesting that substitution from other social security 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 security programs discussed in Section 2: DI, UI, and SL.

iii. 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 2, there are several individual eligibility requirements for early retirement related to earnings history. In Panel A of Figure 3, we show that women have substantially lower cumulative lifetime earnings than men. In this figure, we also plot the 10^{th} percentile of the cumulative earnings among

individuals who meet the earnings requirements for ER. As can be seen in the figure, most men have cumulative earnings higher than this threshold while a large share of women do not. This provides suggestive evidence 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 (Panel B of Figure 3). 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 equation (9) for men in which we weight each observations 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 are presented in Appendix Table A5. The results show that the reform would have had no cross-program substitution effect on men would their earnings history and current income have been similar to that of the women.[30]

4.2 Indirect Effect on Spousal Employment and Program Take-up

In Table 3, we present estimates of the indirect effect of the reform on early retirement take-up, employment, and participation in alternative social security programs, for the spouses of ER-eligible men and women. Each cell in each column comes from a separate estimation of equation (9), controlling for calendar year, age, and individual (household) fixed effects.

As noted in Section 3, our treatment group consists of individuals affiliated with a non-ER firm in the pre-reform period and married to individuals affiliated with an ER firm in the pre-reform period. Our control group consists of individuals affiliated with a non-ER firm in the pre-reform period and married to individuals affiliated with a non-ER firm in the pre-reform period. We restrict our sample to spouses who worked in non-ER firms in the pre-reform period because any potential effect among spouses in ER firms will be contaminated by the direct effect of early retirement eligibility. By focusing on couples in which the spouse worked in a

[30] 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.

control firm, we eliminate contamination of the direct effect. 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.

i. Indirect Effect on Female Spouses

Column (1) of Panel A in Table 3 shows that the reform had a statistically significant and economically 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 labor force compared to female spouses of non ER-eligible men. Interpreted in light of the conceptual framework outlined in Section 2.3, this implies that couples do have a preference for join leisure ($\gamma > 0$). More broadly, it suggests that there are substantial interdependencies in spousal retirement decisions, 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 identified in Panel A of Table 3 likely represents a relatively large reduction in household income, as these spouses do not qualify for early retirement. One way to mitigate the cost of this employment response is for spouses to switch into alternative social security programs (e.g. disability insurance), providing an opportunity for spouses to leave the labor force without experiencing a substantial drop in income. However, while the take-up of alternative social security programs can serve to lower the spouse's cost of exiting the labor force, it may be difficult to switch into these programs.

To examine this question in detail, Columns (2) through (5) of Panel A in Table 3 report the coefficients obtained from estimating equation (9) using spousal participation in alternative social security programs (DI, UI, SL, and ER) as dependent variables. It is important to note that the take-up of early retirement is only possible for individuals affiliated with ER-eligible firms. Thus, a significant effect on this variable would imply that the spouse has switched from a control firm to a treatment firm in order to become eligible for, and take up, early retirement (recall that the sample is restricted to couples in which the spouses worked in control firms in the pre-reform period).

Columns (2) through (5) of Panel A in Table 3 show a statistically significant and economically meaningful indirect effect on the social security participation of the female spouses of ER-eligible men. Specifically, female spouses of ER-eligible men are 1.7 percentage points (40 percent relative to the mean) more likely to take up early retirement, and are 2.9 percentage points (17 percent 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. Conditional on financing the exit from the labor market through take-up of alternative social security programs, the use of DI and ER rather than UI and SL is not surprising. Both alternatives provide permanent exits from the labor market without imposing time-restrictions or follow-up requirements similar to the other programs.[31]

It is worth noting that any indirect effect of the reform on the spouse's decision to exit the labor market need not only stem from within-household complementarities in leisure, but could also be due to indirect effects on the spouse's health. For example, Bertoni and Brunello (2017) exploits variation in the maximum age of guaranteed employment induced by a 2006 Japanese reform and find that husbands' retirement has a negative impact on the mental wellbeing of the wife. Zang (2020), on the other hand, exploits a large increase in the probability of retirement at the legal retirement age for urban male wage earners in China and finds that husband's retirement has a positive effect on the physical as well as the mental well-being of the wife. Thus, some of our indirect effects on spouses could operate through changes in health rather than through complementarities in leisure, though the direction of this 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, this data is 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 the firm each individual was in at the age of 57. Otherwise, the sample is constructed in the same way 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 (GP). We acknowledge that any mental health effects on the spouse would have to be substantial in order to translate into changes in these outcomes, and that there may be less severe effects that we cannot identify,

[31] With respect to DI benefits, it should be noted that leniency in the DI screening process in Norway has been subject to substantial debate over the past several decades, and a number of empirical studies have presented strong suggestive evidence in favor of this concern. For example, existing research (e.g. Bratsberg et al., 2013) has shown that individuals laid off from their jobs are significantly overrepresented among disability insurance beneficiaries, and more recent work has shown significant local screening leniency across the country (Schreiner, 2019). Some of the reasons underlying this leniency is believed to stem from the substantial autonomy granted to DI case workers, who can grant DI benefits for a variety of reasons in which validation is difficult. More than 60 percent of individuals on DI have been granted benefits due to «hard-to-verify» conditions (Kostøl and Mogstad, 2014). The autonomy of case workers to grant DI benefits also extends to GPs, who acts as a first point of contact with individuals interested in applying for DI benefits. Abstracting from the potential health confounder discussed in Section 2.3, we find it unlikely that we would observe significant indirect spousal effects had the DI screening process been much stricter.

such as an effect on stress or sleep deprivation as identified in Bertoni and Brunello (2017). However, we also note that GPs act as a gatekeepers to the health system in Norway, and are responsible for examining, diagnosing, and treating patients (including referring the patient to specialists and prescribing medicine – such as anti-depressants). While any potential mental health consequences are unlikely to translate into significant mortality and emergency room visits effects, we believe that moderate mental health effects could be picked up by an increase in GP visits. Looking across the columns in 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.

ii. 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 alternative social security programs. Looking across the columns in Panel B, the point estimates are of a magnitude similar to those for female spouses. However, the point 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, as discussed above. 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 as indicating null effects among male spouses to ER-eligible women.

iii. Explaining the Gender Differences in the Indirect Spousal Effects

If we interpret the above results as providing evidence for labor market and social security responses among female but not male spouses, we believe this can be explained by differences in the household costs of joint exit from the labor market. Specifically, as demonstrated in Figure 3, the women in our sample earn much less than the men. This means that the household cost associated with male spouses leaving the labor force is much greater than the household cost associated with female spouses leaving the labor force (see Appendix A, Figure A1). Thus, to the extent that the lack of statistically significant indirect effects among male spouses to ER-

eligible women are not driven by differences in sample size, we speculate that it is driven by the fact that the household cost associated with male spouses leaving the labor force is much greater than the household cost associated with female spouses leaving the labor force.

Suggestive evidence on the relationship between the spouse's share of total household income and the spousal labor market and social security response is provided in Appendix Table A7. This table shows results obtained from estimating a modified version of equation (9) in which we have interacted the treatment variable of interest with a variable measuring the wife-husband income gap. Interestingly, when we fix the income gap at zero, we do 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 (implying that the spousal response among male spouses is larger the larger the share of total household income earned by the wife is). These results are consistent with the idea that differences in the household cost of joint exit from the labor market depending on whether the spouse is a female or a male could drive the identified gender difference in spousal response.[32]

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 between ER-eligible women and their male spouses (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 as discussed above, 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 study this in detail, we estimate a modified version of equation (9) in which we interact the treatment variable with the age gap between husband and wife. The results from this exercise are presented in Appendix Table A8, and show 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

[32] To the extent that the direct effect of ER eligibility on early retirement also represents a household decision, which may be the case, this argument would also imply that the direct effect should be larger for ER-eligible women than for ER-eligible men (since 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 fulfill the individual eligibility criteria for early retirement (see Sections 2 and 4) , such that the direct effect scaled by the fraction eligible is larger for women than men.

than male spouses, and we still find no statistically significant indirect effect on DI uptake for male spouses.

Another potential reason for asymmetry in the response of female/male spouses is that the within-couple complementarities in leisure may also differ across men and women. Specifically, if females value joint leisure more than males, this could also explain the gender differences in the indirect spousal effects we identify. However, the empirical evidence on asymmetric complementarities in leisure is relatively mixed, with some finding suggestive evidence that females value joint leisure more than males (Kruse, 2019) and others that males value joint leisure more than females (Coile, 2004). While Kruse (2019) 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.[33]

Finally, as noted in Section 4.1, we cannot completely 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 this channel rather than through complementarities in leisure.

4.3 Nonparametric Event Studies

As discussed in Section 3, the identifying assumption underlying our estimation strategy is that there are no other 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 on the plausibility of this assumption by estimating nonparametric event studies based on equation (10). 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).[34]

Results from estimating the direct effect of the reform using equation (10) are shown in Figure 4.[35] Each dot in each subfigure corresponds to the π_a estimate indicated on the x-axis,

[33] In reference to recent work on gender identity and relative incomes within the household, it is also worth noting that a number of studies have found evidence of the behavioral prescription that "a man should earn more than his wife" (e.g. Bertrand, Pan, and Kamenica, 2015). This provides another channel through which the asymmetry in the response for female/male spouses may operate.

[34] 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 Appendix Figure A2.

[35] 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 Appendix Figure A3.

and the bars extending from each dot represent the 95 percent 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 placed at 0 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 are relative to this.

Looking across all subfigures of Figure 4, three things are worth noting. First, there is no indication of pre-treatment trends in any of our outcomes, suggesting that there is no selection on fixed trends over time. Second, there is a substantial 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 through the use of equation (10) 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 nonparametric event studies for spouses to ER-eligible individuals closely mirror the results obtained from plotting raw trends in outcomes separately for spouses of ER-eligible and non ER-eligible individuals. Results from this exercise are shown in Figure 6 with respect to the take up of ER, employment, and DI. This is an encouraging result, providing even stronger evidence against potential selection on fixed trends over time and in favor of our identifying assumption.

5 Robustness and Sensitivity Analysis

One of the main concerns associated with our empirical approach is that we assign treatment based on an individual's pre-reform firm affiliation. Specifically, 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. This will attenuate the direct effects we estimate. However, as we identify relatively large direct effects, this 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 potential spousal effect would be a combination

of the indirect spousal response to the treated individual's ER eligibility 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.

To examine if our results are driven by firm switching, we remove all individuals and spouses who switched firm affiliation in any time period and estimate equation (9).[36] The result 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.

After having examined the robustness of our results with respect to job switchers, we next investigate the sensitivity of our results to a battery of sample and specification checks. As discussed in Section 3, we impose a number of restrictions on our sample prior to performing our main analysis. In Appendix Tables B1 and B2, we investigate the sensitivity of our results to relaxing each of these restrictions. This allows us to better understand the generalizability of our findings to the broader Norwegian population. The results from these auxiliary analyses are discussed in detail in Appendix B, and demonstrate that our findings are robust to each of the sample restrictions we impose in our main analysis.

Furthermore, our findings are robust to a number of different model specification checks. These specification checks are also discussed in Appendix B and shown in Appendix Tables B3 and B4. Specifically, both the direct effects and the indirect spousal responses 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.

One 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 labor force early than individuals (and their spouses) who were not affiliated with ER firms in 1988. The raw data plots (Figures 2 and 6) as well as the nonparametric event studies (Figures 4 and 5) provide evidence against this concern. We also provide evidence that no such ER versus non-

[36] 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 which provide ER benefits, or we could have rare cases of incomplete information on work history.

ER firm affiliation difference in labor force exit rates exists among younger cohorts who should not be affected by the reform (see Appendix C).

6 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 (Imbens and Angrist, 1994). In our setting, 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. In this alternative 2SLS setup, the empirical model is defined by the following two equations:

$$SpousalEmployment_{it} = \beta_0 + \beta_1 \widehat{Employment}_{it} + \delta_i + \tau_t + \theta_{it} + e_{it}, \quad (11)$$

$$Employment_{it} = \emptyset_0 + \emptyset_1 (FirmEligible_i * AgeEligible_{it}) + \delta_i + \tau_t + \theta_{it} + u_{it}, \quad (12)$$

where the first-stage equation (12) estimates the effect of being eligible for ER on own employment. Predicted employment, $\widehat{Employment}_{it}$, is then inserted into equation (11). $\beta_1$is the coefficient of interest in equation (11) and yields the LATE of own employment on spousal labor market outcomes. This IV approach relies on two key assumptions. First, the instrument needs to have an effect on the endogenous variable. This assumption is valid in our case, as we have shown that eligibility for ER clearly reduces own employment.[37] 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 equation (12). As is well-known, the exclusion restriction cannot be tested.[38]

The results from the IV method are presented in Table 6. Panel A shows the estimated effects of men's employment on their female spouses' employment and take-up of disability

[37] The first stage F-stat is 676 for men's employment and 213 for women's employment.

[38] Instead of using ER eligibility as an instrumental variable for employment, one can of course also use ER eligibility as an instrumental variable for retirement. We focus on employment rather than retirement 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 labor market, but also due to individuals that have already left the labor market switching into retirement from other social security programs (DI, UI, and sick leave). Thus, if we were to use ER eligibility as an instrumental variable for retirement rather than employment, we would not be able to isolate the effect that is driven by the individual exiting the labor 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 of employment would yield a relatively similar result.

benefits. We estimate that men's employment in a given year increases their female spouses' employment by, on average, 17.4 percentage points (23 percent). We further estimate that men's employment decreases their female spouses' take-up of disability insurance by, on average, 13.9 percentage points (84 percent). There are no statistically significant effects of women's employment on their male spouses' employment or take-up of disability insurance, as shown in Panel B.[39]

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 percent higher than the micro response, which corresponds to a social multiplier of 1.17. In other words, if the policy induces 100 married men to retire early, an additional 17 female spouses will drop out of employment due to indirect spousal effects induced by the policy. As discussed in Section 1, such spousal labor supply complementarities are often argued to be an important reason for why individual-level estimates of labor supply often provide lower estimated elasticities than most macroeconomic calibrations.

7 Discussion and Conclusion

Anticipating the labor market effects of welfare reforms is difficult due to public policy interactions across programs and among household members. Specifically, changes to one program may affect individual take-up of other programs, and individual participation in specific programs may generate labor market responses from other household members. Little work has been able to comprehensively examine the extent and magnitude of such interactions. This is unfortunate, as a failure to understand the cross-program and cross-household member effects of welfare reforms may result in a substantial underestimation of the full impact of the reforms.

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 labor market behavior, (2) the cross-program effect of the reform on individual participation in other social security programs, and (3) the indirect effect of the reform on spousal labor market and social security participation. We first show that the reform had a substantial impact on the labor supply of individuals who became eligible for early retirement,

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

reducing the probability of employment by more than 30 percent. We then show that the increased take-up of early retirement had an offsetting effect on the take-up of alternative social security programs (unemployment insurance, disability insurance, and sick leave). Next, we demonstrate that the reform had a negative impact on the labor supply of spouses to early retirement eligible individuals, with an effect size of approximately 5 percent. We speculate that this is driven by interdependencies in couples' retirement decisions. Finally, we show that the reduced labor force participation among spouses is accompanied by a significant increase in the take-up of disability insurance.

We document interesting effect heterogeneity across genders. First, the direct program spillovers – in particular the reduced take-up of disability insurance - are much stronger for men than women. We show suggestive evidence that this is likely due to the fact that fewer women than men (particularly among those who select into social welfare programs) qualify for early retirement, such that their ability to substitute across programs is smaller. It is also consistent with the fact that the relative benefit of switching from DI to 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. Second, the spousal responses are only present among women, especially with respect to take-up of disability insurance. We believe that this likely is due to the fact that 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 labor force is much greater than the household cost associated with wives of ER-eligible males men leaving the labor force.

In terms of policy implications, our analysis demonstrates that the labor market effects of welfare reforms may extend beyond the direct program that is being targeted, and may affect more 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 security 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.[40] Among men, the program substitution effects increase the per person cost of the ER

[40] 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).

program by 6 percent (the corresponding number for women is 2 percent).[41] The indirect spousal effect on female spouses increases the per person cost of the ER program by an additional 12 percent (the corresponding number on male spouses is 4 percent). This implies that the cost of the reform among men, taking the associated elasticities into account, is 18 percent greater than if there were no program substitution effects and spousal spillovers. The corresponding number for women is 6 percent. The magnitude of this difference is both statistically significant and economically meaningful, and demonstrates that a social planner who assumes zero substitution and spillover elasticities will significantly underestimate the cost of the reform.

References

Atalay, K. and G. Barret (2015). "The impact of age pension eligibility age on retirement and program dependence: Evidence from an Australian experiment" *Review of Economics and Statistics* 97(1): pp. 71-87.

Baicker, K., A. Finkelstein, J. Song, and S. Taubman (2014). "The Impact of Medicaid on Labor Market Activity and Program Participation: Evidence from the Oregon Health Insurance Experiment" *American Economic Review: Papers & Proceedings* 104(5): pp. 322–328.

Baker, M. (2002). "The Retirement Behavior of Married Couples: Evidence from the Spouse's Allowance" *Journal of Human Resources* 37(1): pp. 1-34.

Banks, J., R. Blundell and M. Casanova (2010). "The dynamics of retirement behavior in couples: Reduced-form evidence from England and the US" *Mimeo.*

Bertoni, M. and G. Brunello (2017). "Pappa Ante Portas: The Effect of the Husband's Retirement on the Wife's Mental Health in Japan" *Social Science and Medicine* 175: pp. 135-142.

Bertrand, M., E. Kamenica and J. Pan (2015). "Gender identity and relative income within households" *Quarterly Journal of Economics* 130 (2): pp. 571-614.

Blau, D. M. (1998). "Labor Force Dynamics of Older Married Couples", *Journal of Labor Economics* 16(3): pp. 595-629.

Bloemen, H., S. Hochguertel and J. Zweerink (2019). "The effect of incentive-induced retirement on spousal retirement rates: Evidence from a natural experiment" *Economic Inquiry* 57: pp. 910-930.

Blöndal, S., and M. Pearson (1995). "Unemployment and other Non-employment Benefits" *Oxford Review of Economic Policy* 11(1): pp. 136–169.

Bratberg, E., T. H. Holmås, and Ø. Thøgersen (2004). "Assessing the effects of an early retirement program" *Journal of Population Economics* 17: pp. 387–408.

Bratsberg, B., E. Fevang, and K. Røed (2013). "Job loss and disability insurance" *Labour Economics* 24: pp. 137-150.

[41] 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.

Bratsberg, B. and E. Stancanelli (2018), "Labor supply in couples: Learning from a Norwegian early retirement reform." Paper presented at the EALE meetings, Lyon, Sept 15, 2018.

Boyle, M. and J. Lahey (2016). "Spousal labor market effects from government health insurance: Evidence from a veterans affairs expansion" *Journal of Health Economics* 45: pp. 63-76.

Brown, D., A. E. Kowalski, and I. Lurie (2020). "Long-Term Impacts of Childhood Medicaid Expansions on Outcomes in Adulthood" *Review of Economic Studies* 87: pp. 792-821

Casanova, M. (2010). "Happy together: A structural model of couples' joint retirement choices" *Mimeo.*

Chetty, R., A. Guren, D. Manoli, and A. Weber (2011). "Are Micro and Macro Labor Supply Elasticities Consistent? A Review of Evidence on the Intensive and Extensive Margins" *American Economic Review* 101(3): pp. 471–75.

Coile, C. (2004). "Retirement incentives and couples' retirement decisions" *The B.E. Press Journal of Economic Analysis & Policy* 4(1).

Cribb, J., C. Emmerson and G. Tetlow (2016). "Signals matter? Large retirement responses to limited financial incentives" *Labour Economics* 42: pp. 203-212.

Decker, S. and F. Selck (2012). "The effect of the original introduction of Medicaid on welfare participation and female labor supply" *Review of Economics of the Household*, 10(4): pp. 541-556.

Duggan, M., P. Singleton and J. Song (2007). "Aching to Retire? The Rise in the Full Retirement Age and its Impact on the Social Security Disability Rolls" *Journal of Public Economics* 91(7-8): pp. 1327–1350.

Elwell, J. (2019). "The Effects of Expansions of Children's Medicaid Eligibility on Program Participation and Labor Supply" *Mimeo.*

Fletcher, J. and R. Marksteiner (2017). "Causal spousal health spillover effects and implications for program evaluation" *American Economic journal: Economic Policy* 9: pp. 144-66.

Glaeser, E. L., J. A. Scheinkman, and B. I. Sacerdote (2003). "The Social Multiplier" *Journal of the European Economic Association* 1(2-3): pp. 345-353.

Goux, D., E. Maurin and B. Petrongolo (2014). "Worktime regulations and spousal labor supply" *American Economic Review* 104(1): pp. 252-76.

Gustman, A. and T. Steinmeier (2004). "Social Security, Pensions And Retirement Behavior Within The Family" *Journal of Applied Econometrics* (19)6: pp. 723-737.

Hospido, L. and G. Zamarro (2014). "Retirement patterns of couples in Europe" *IZA Journal of European Labor Studies* 12 (3).

Hurd, M. D. (1990). "The joint retirement decision of husbands and wives" in *Issues in the Economics of Aging*, pp. 231–258 (Chicago, University of Chicago Press)

Hernæs, E., S. Markussen, J. Piggott, K. Røed (2016). "Pension reform and labor supply" *Journal of Public Economics* 142: pp. 39-55.

Imbens, G. and J. Angrist (1994). "Identification and estimation of local average treatment Effects" *Econometrica* 62: pp. 467-475.

Johnsen, J. V. and K. Reiso (2020). "Economic Impacts of Workfare Reforms for Single Mothers" *Scandinavian Journal of Economics* 122: pp. 494-523.

Kaygusuz, R. (2015). "Social security and two-earner households" *Journal of Economic Dynamics and Control* 59: pp. 163-178.

Kapur, K. and J. Rogowski (2007). "The role of health insurance in joint retirement among married couples" *Industrial and Labor Relations Review* 60(3).

Kostøl, A. and M. Mogstad (2014). «How financial incentives induce disability insurance recipients to return to work", *American Economic Review* 104: pp. 624-655.

Kruse H, (2019). “Joint retirement in couples evidence of asymmetric complementarity in leisure” *Mimeo.*

Lalive, R., and P. Parrotta (2016). “How does pension eligibility affect labor supply in couples?” *Labour Economics* 46: pp. 177-188.

Maestas, N. (2001). “Labor, Love, & Leisure: Complementarity and the Tieming of Retirement by Working Couples” *Mimeo.*

OECD (2013). *Pension at a Glance 2013*: *OECD and G20 Indicators* (OECD Publishing, Paris).

Mavromarasa, K. and R. Zhub (2015). “Labour force participation of older men in Australia: the role of spousal participation” *Oxford Economic Papers,* 67(2), 310–333.

McGeary, K. (2015). “Spousal Effects in Smoking Cessation: Matching, Learning, or Bargaining?” *Eastern Economic Journal* 41 (1): pp. 40–50.

Røed, K., and F. Haugen (2003). “Early retirement and economic incentives: Evidence from a quasi-natural experiment” *Labour* 17: pp. 203–228.

Sánchez-Marcos, V. and C. Bethencourt (2018). “The Effect Of Public Pensions On Women's Labor Market Participation Over A Full Life Cycle” *Quantitative Economics* 9(2): pp. 707-733.

Schirle, T. (2008). “Why Have the Labor Force Participation Rates of Older Men Increased since the Mid-1990s?” *Journal of Labor Economics* 26(4): pp. 549-594.

Schreiner, R. (2019). “Unemployed or disabled? Disability screening and labor market outcomes of youth” *Mimeo.*

Selin, H. (2017). “What Happens to the Husband's Retirement Decision when the Wife's Retirement Incentives Change?" *International Tax and Public Finance* 24(3): pp. 432-458.

Shore-Sheppard, L. (2008). “Stemming the Tide? The Effect of Expanding Medicaid on Health Insurance Coverage” *The B.E. Journal of Economic Analysis and Policy* 8(2): pp. 1-35.

Stancanelli, E. and A. Van Soest (2012). “Retirement and Home Production: A Regression Discontinuity Approach” *American Economic Review: Papers & Proceedings* 102(3): pp. 600– 605.

Stancanelli, E. (2017). “Couples’ retirement under individual pension design: A regression discontinuity study for France” *Labor Economics* 49(C): pp. 14-26.

Staubli, S. and J. Zweimüller (2013). “Does raising the early retirement age increase employment of older workers?” *Journal of Public Economics* 108: pp. 17-32.

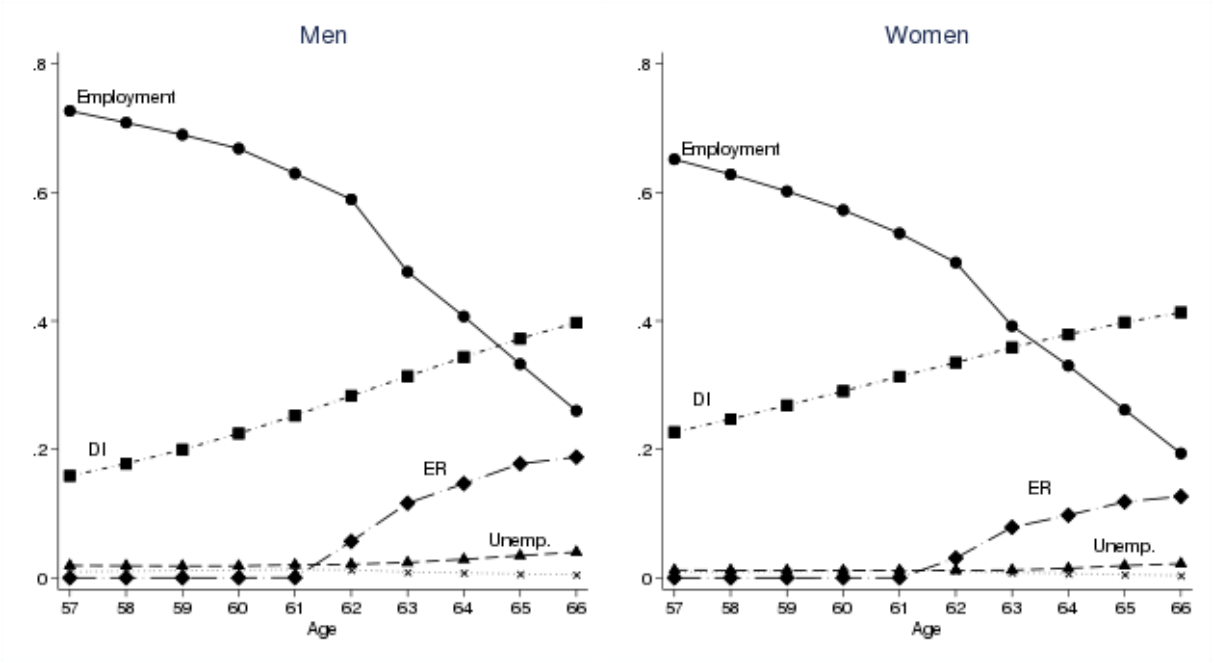
Vestad, O. L. (2013). “Labour supply effects of early retirement provision” *Labour Economics* 25: pp. 98–109.

Witman, A. (2015). “Public health insurance and disparate eligibility of spouses: The medicare eligibility gap” *Journal of Health Economics* 40(0): pp. 10-25.

Zang, E. (2020). “Spillover effects of a huband’s retirement on a woman’s health: Evidence from urban China” *Social Science and Medicine* 245.

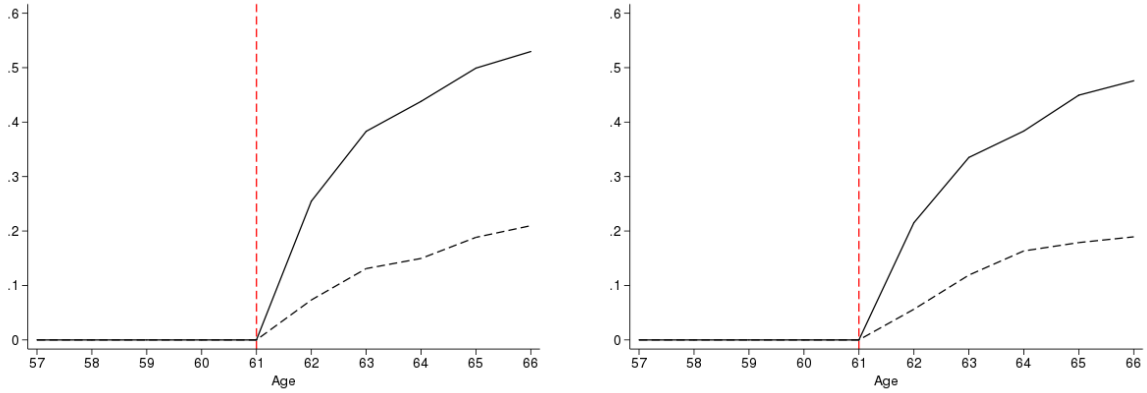
Zweimüller, J., R. Winter-Ebmer, and J. Falkinger (1996). “Retirement of spouses and social security reform” *European Economic Review* 40: pp. 449–472.

Figure 1: Employment and take-up of social welfare benefits among the elderly Norwegian population



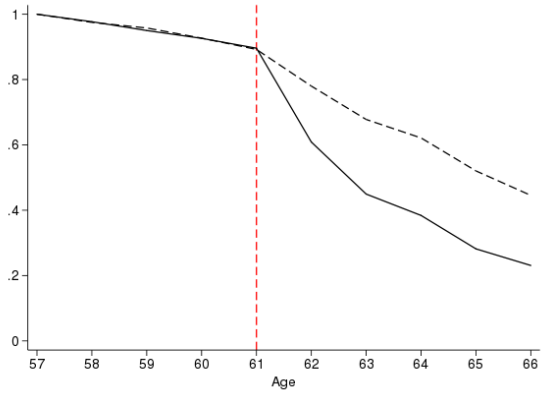
Notes: The figure presents the employment and take-up of various social welfare policies for the full Norwegian population of men (left) and women (right) between the ages of 57 to 66. The dots represent the average share of individuals of a given age in employment from1993 to 2007 (our observation period). 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 is from tax registers and the data on take-up of social welfare policies is from national social welfare registers.

Figure 2: Direct effect of the reform on early retirement take-up and employment, raw trends

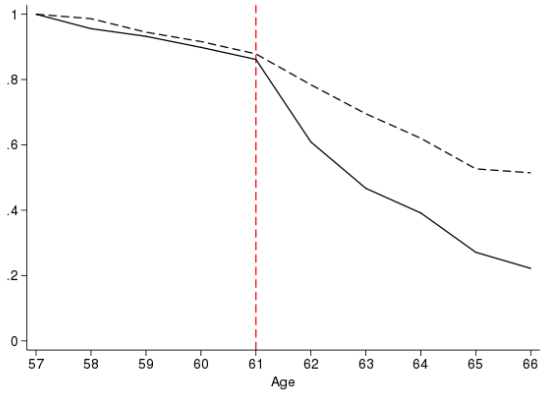


A. Early retirement, men

B. Early retirement, women



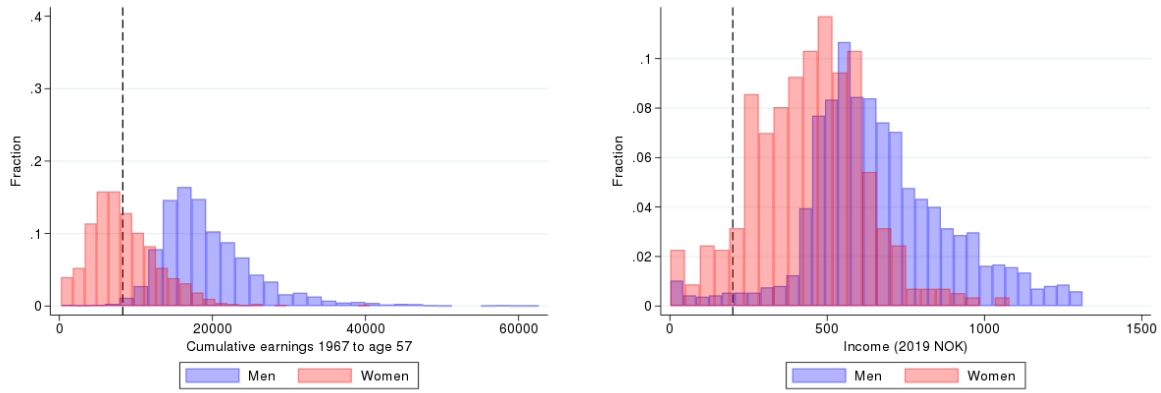
C. Employment, men



D. Employment, women

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 early retirement (ER) benefits through pre-reform affiliation with a firm which 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 security registers.

Figure 3: Cumulative and current income at age 57 for men and women

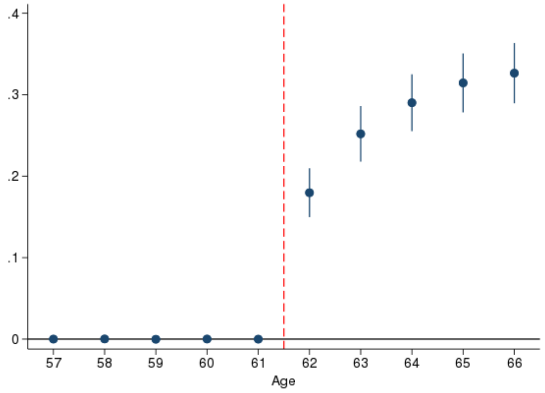


A. Cumulative income

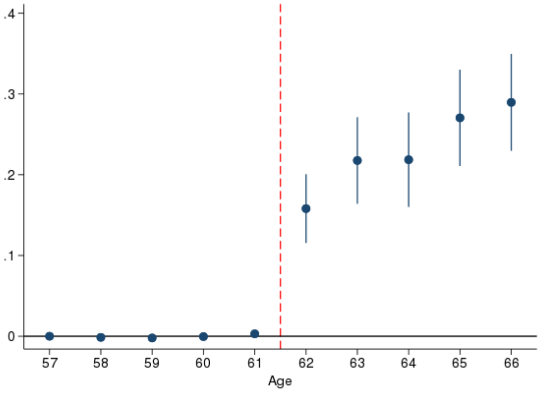
B. Current income

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 10-best years of labor 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.

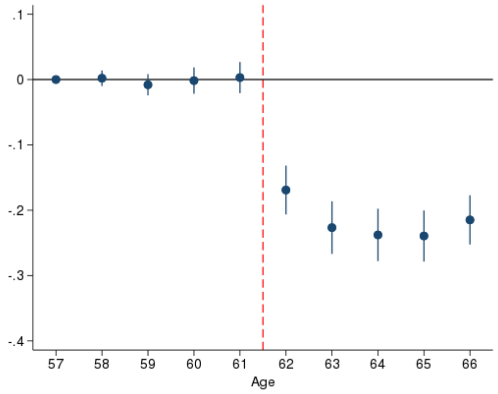
Figure 4: Direct effect of the reform on own outcomes, event study results



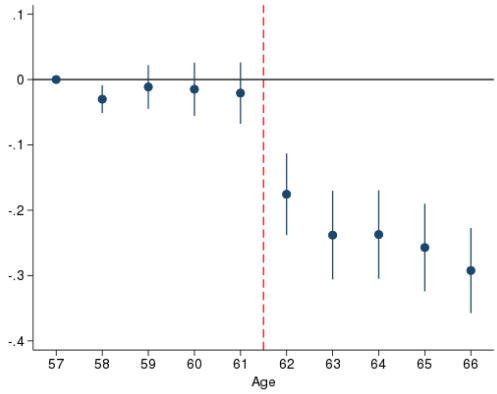
A. Early retirement, men



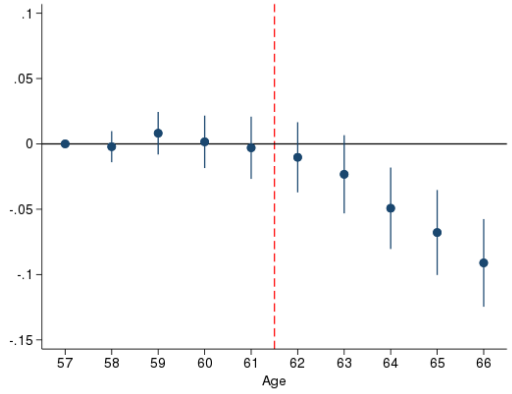
B. Early retirement, women



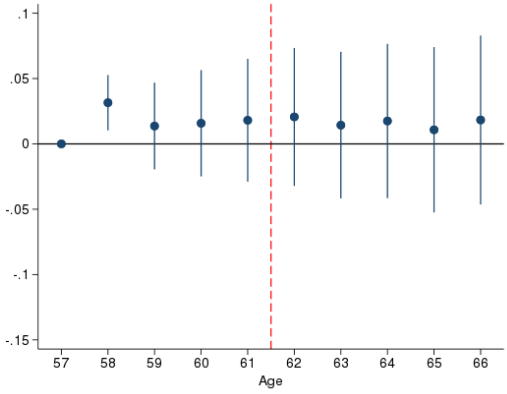
C. Employment, men



D. Employment, women



E. Disability insurance take-up, men

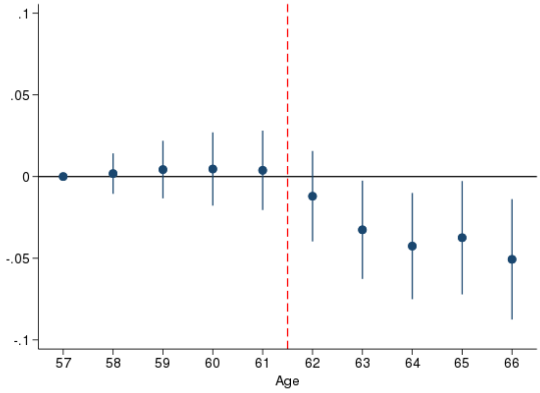


F. Disability insurance take-up, women

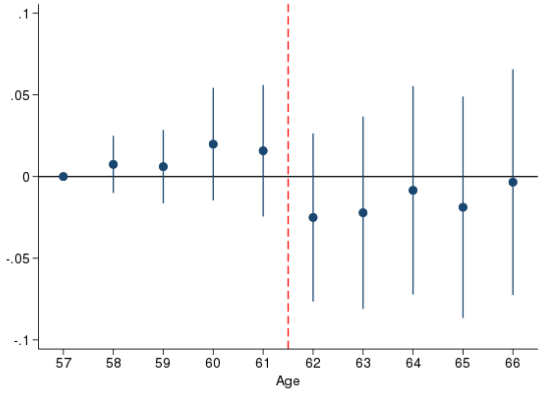
Notes: Each dot in each figure corresponds to the treatment effect at the given age indicated on the x-axis, and the bar extending from each dot represents 95% confidence intervals. The dotted vertical line denotes the age cut-off at which individuals affiliated with ER firms become eligible for early 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 early retirement (ER) benefits through pre-reform affiliation with a firm which 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 security registers.

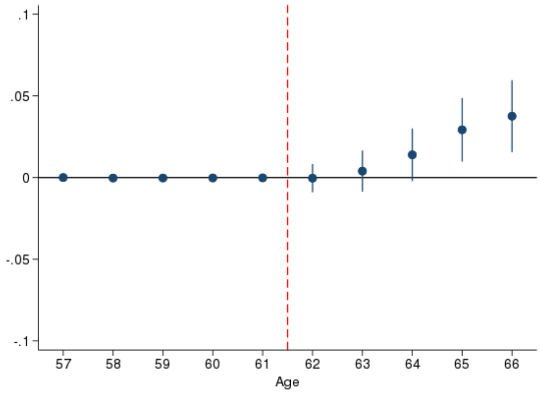
Figure 5: Indirect effects of the reform on spousal outcomes, event study results



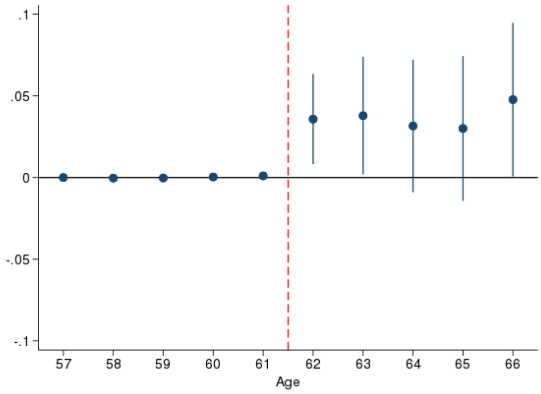
A. Employment, female spouses



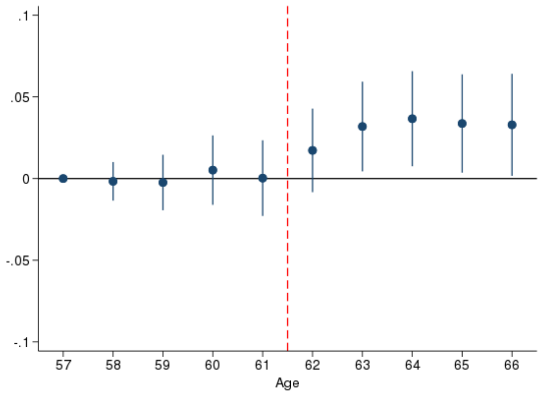
B. Employment, male spouses



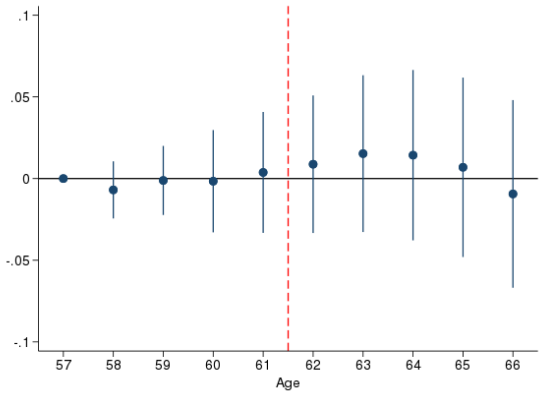
C. Early retirement, female spouses



D. Early retirement, male spouses



E. DI take-up, female spouses

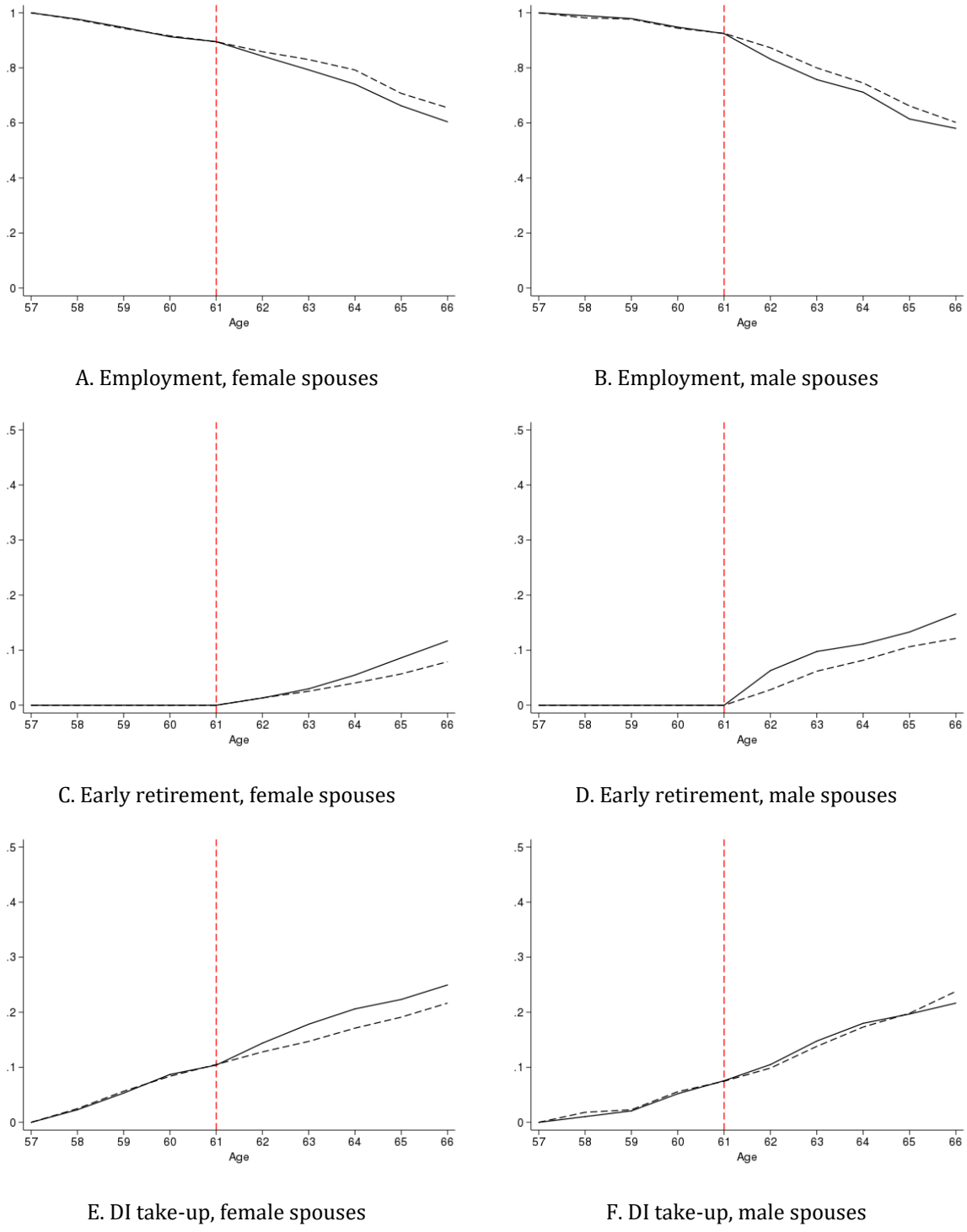


F. DI take-up, male spouses

Notes: Each dot in each figure corresponds to the treatment effect at the given age indicated on the x-axis, and the bar extending from each dot represents the 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 early retirement (ER) benefits through pre-reform affiliation with a firm which 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 security registers.

Figure 6: Indirect effect of the reform on spousal outcomes, raw trends



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

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 early retirement (ER) benefits through pre-reform affiliation with a firm which 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 security registers.

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 early retirement (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.

Table 2. The direct effect of the reform on employment and program take-up

			Program Substitution		
	ER	Employment	DI	Unemployment	Sick Leave
	(1)	(2)	(3)	(4)	(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			19596		
N couples			1989		
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			5936		
N couples			603		

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (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 which 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 security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 3. The indirect effect of the reform on spousal employment and program take-up

		Program take-up			
	Employment	ER	DI	Unemployment	Sick leave
	(1)	(2)	(3)	(4)	(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			19596		
N couples			1989		
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			5936		
N couples			603		

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (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 which 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 whose spouses are 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 of age-eligible individuals in the control group. Data are from Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 4. The direct effect of the reform among workers that did not switch eligibility status

			Program substitution		
	ER	Employment	DI	Unemployment	Sick leave
	(1)	(2)	(3)	(4)	(5)
Panel A. Men					
ITT effect	0.4613***	-0.3348***	-0.1034***	-0.0365***	-0.0352**
SE	(0.022)	(0.027)	(0.023)	(0.012)	(0.014)
Mean	0.007	0.678	0.288	0.066	0.067
N			7730		
N couples			788		
Panel B. Women					
ITT effect	0.4008***	-0.3669***	-0.0067	-0.0032	-0.0603**
SE	(0.034)	(0.040)	(0.037)	(0.024)	(0.025)
Mean	0.006	0.739	0.228	0.053	0.070
N			2696		
N couples			275		

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (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 which 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 comes are Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 5. The indirect effect of the reform on spouses to workers that did not switch eligibility status

		Program take-up			
	Employment	ER	DI	Unemployment	Sick leave
	(1)	(2)	(3)	(4)	(5)
Panel A. Wives					
ITT effect	-0.0564***	0.0018	0.0666***	-0.0206	-0.0374**
SE	(0.019)	(0.003)	(0.020)	(0.014)	(0.016)
Mean	0.804	0.002	0.165	0.058	0.053
N			7730		
N couples			788		
Panel B. Husbands					
ITT effect	0.0076	-0.0096	0.0048	-0.0053	-0.0474*
SE	(0.039)	(0.014)	(0.038)	(0.026)	(0.027)
Mean	0.747	0.021	0.210	0.058	0.088
N			2696		
N couples			275		

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (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 which 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 security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

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***
	(0.033)	(0.027)
Mean	0.748	0.166
N	19596	
N couples	1989	
Panel B. Male spouses		
Wife employed	0.113	-0.037
	(0.127)	(0.102)
Mean	0.703	0.161
N	5936	
N couples	603	

Notes: The table reports 2SLS estimates of equation (12) of the effect of employment on spousal employment and take-up of disability insurance, instrumenting employment with eligibility for early retirement (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 which 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 security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Online Appendix for:
Interactions in Public Policies:
Spousal Responses and Program Spillovers of Welfare Reforms

Julian Vedeler Johnsen
SNF - Centre for Applied Research at NHH

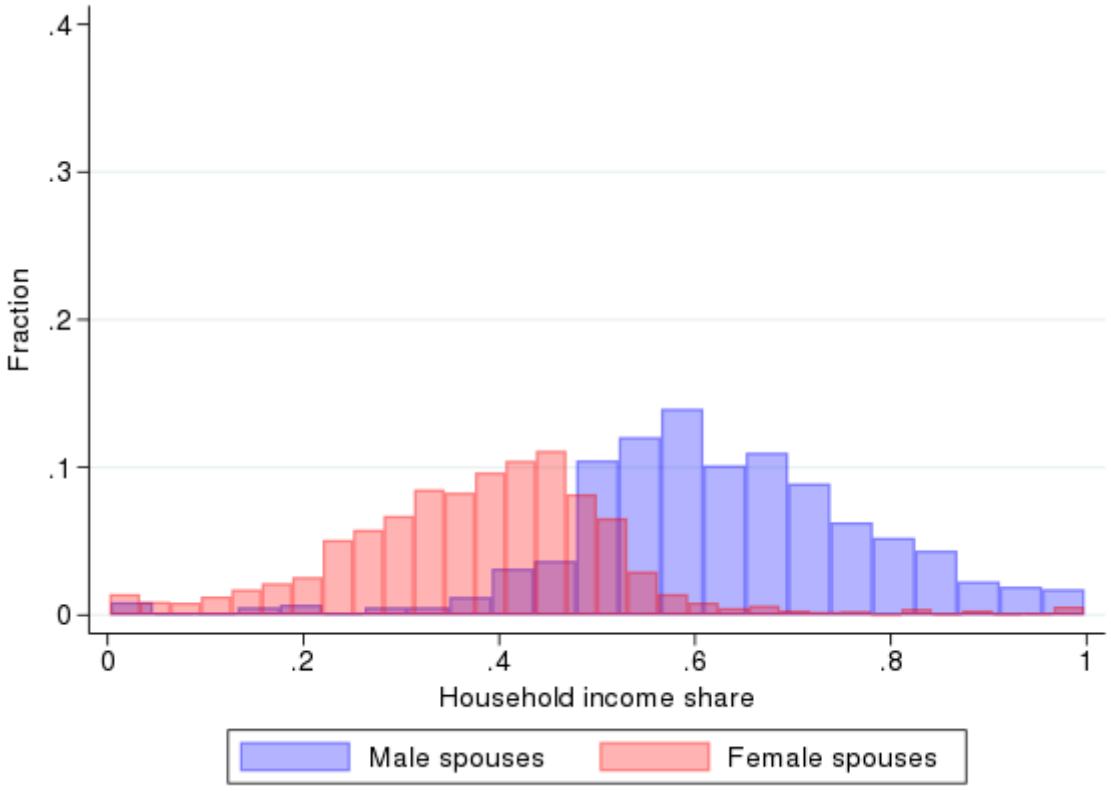
Kjell Vaage
University of Bergen

Alexander Willén
Norwegian School of Economics

September 2020

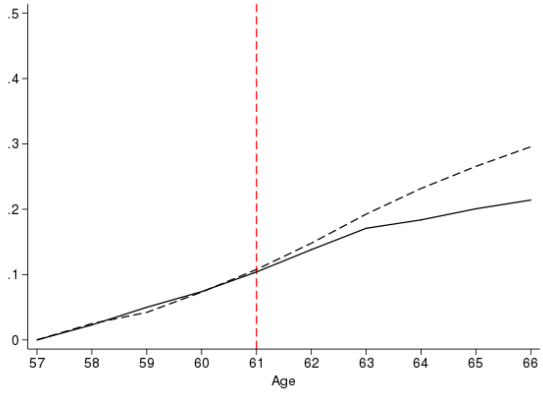
Section A – Additional Tables and Figures

Figure A1. Share of total household income for male and female spouses

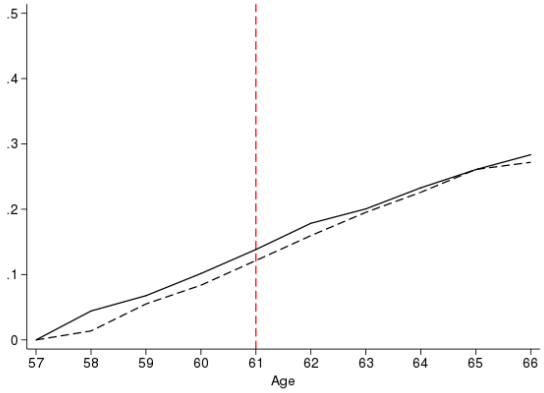


Notes: The figure reports the distribution of the share of household earnings earned by male spouses (blue) and female spouses (red) in our sample. Our sample consists of married individuals born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 57, and with spouses not treated by the reform based on their 1988 firm affiliation. Data are from Norwegian employer-employee data and tax registers.

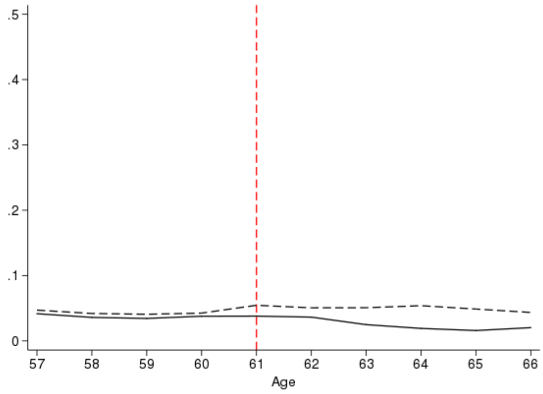
Figure A2: Direct effect of the reform on various social security programs, raw trends



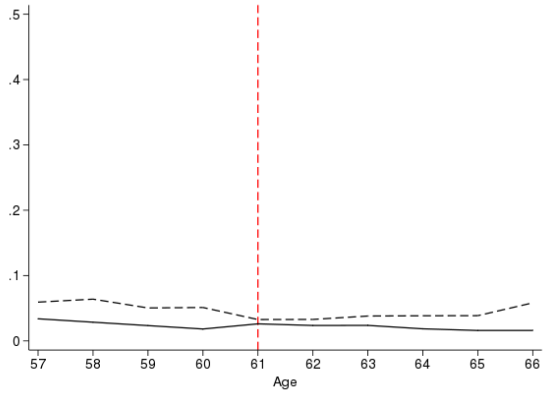
A. Disability insurance take-up, men



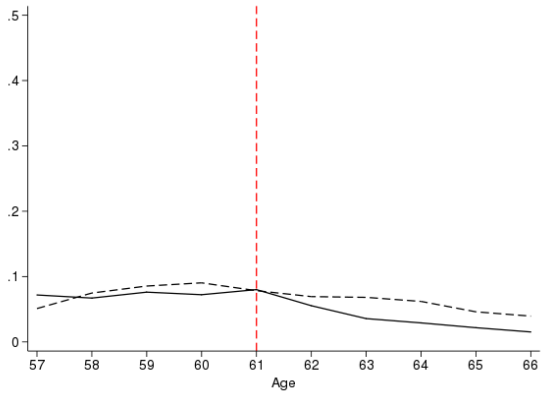
B. Disability insurance take-up, women



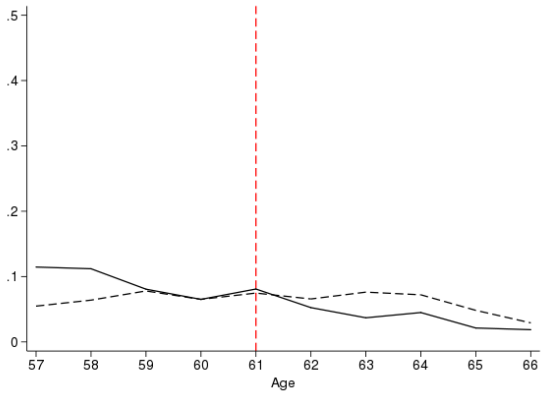
C. Unemployment insurance take-up, men



D. Unemployment insurance take-up, women



E. Paid sick leave take-up, men

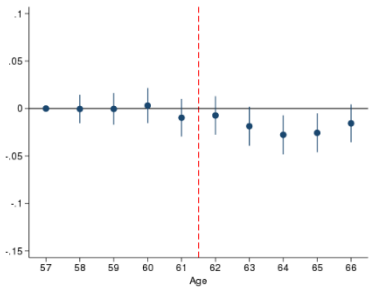


F. Paid sick leave take-up, women

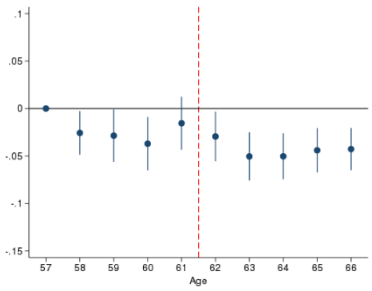
Notes: The figure presents raw trends in the take-up of disability insurance, unemployment insurance, and paid sick leave for men (left) and women (right) in our treatment group (solid line) and control group (dashed line) between the ages of 57 and 66 in the post-reform period 1993-2007 We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm which 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 security registers.

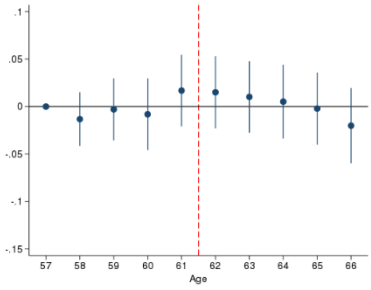
Figure A3. Direct effects of the reform, supplemental event study plots



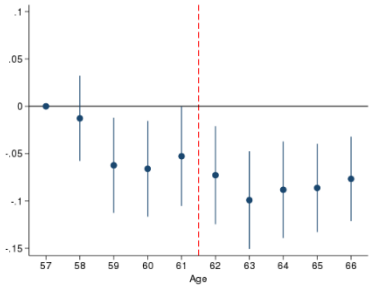
A. Unemployment, men



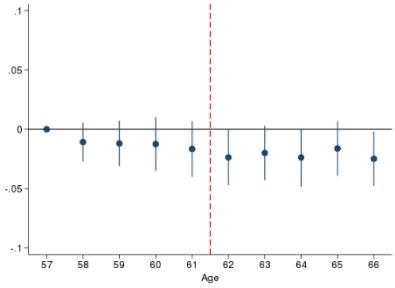
B. paid sick leave, men



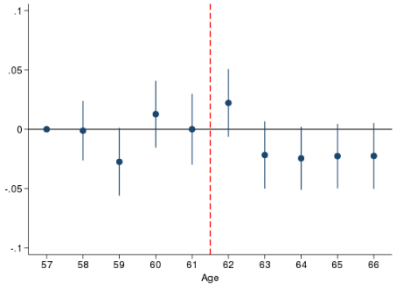
C. Unemployment, women



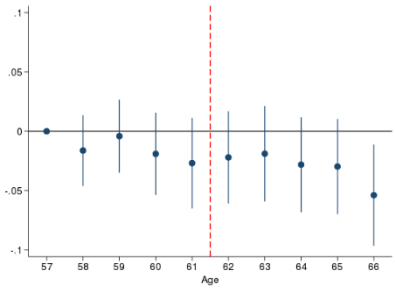
D. paid sick leave, women



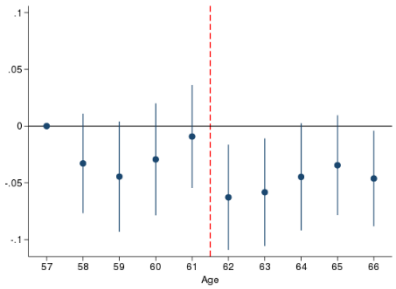
E. Unemployment, female spouses



F. paid sick leave, female spouses



G. Unemployment, male spouses



H. paid sick leave, male spouses

Notes: Each dot in each figure corresponds to the treatment effect at the given age indicated on the x-axis, and the bar extending from each dot represents the 95% confidence intervals. The dotted vertical line denotes the age cut-off at which individuals affiliated with ER firms become eligible for ER, 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. 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 security registers.

Table A1. Characteristics of men and women on various welfare programs compared with the employed and overall population

	Overall	Employed	ER	DI	UI	Sick leave
Panel A. Men						
% of male pop. 62-66	100 %	16 %	19 %	34 %	3 %	4 %
Lifetime earnings	5 786	8 545	5 985	3 379	5 530	8 053
	(4 592)	(6 515)	(2 147)	(2 147)	(2 466)	(3 722)
Years of education	10.67	11.50	11.18	9.51	10.12	11.11
	(3.49)	(3.68)	(3.30)	(3.02)	(3.06)	(3.53)
Panel B. Women						
% of female pop. 62-66	100 %	13 %	15 %	43 %	2 %	5 %
Lifetime earnings	2 750	4 820	3 760	1 814	3 104	5 074
	(2 228)	(2 508)	(1 612)	(1 483)	(1 484)	(2 037)
Years of education	9.79	10.77	10.75	9.24	9.52	10.83
	(3.01)	(3.33)	(3.05)	(2.68)	(2.37)	(3.33)

Notes: This table presents average characteristics (SD in parentheses) over the period 1998-2007 of men and women aged 62 to 66 (N men: 933,754, N women: 951,464), comparing the overall population to the employed population and the population on various welfare programs. Lifetime earnings are in 1,000 NOK and are the sum of all earnings over the period 1967-2010.

Table A2. Age of selected birth cohorts across our years of observation

Birth cohort	Year of observation														
	'93	'94	'95	'96	'97	'98	'99	'00	'01	'02	'03	'04	'05	'06	'07
1936	57	58	59	60	61	62	63	64	65	66					
1937		57	58	59	60	61	62	63	64	65	66				
1938			57	58	59	60	61	62	63	64	65	66			
1939				57	58	59	60	61	62	63	64	65	66		
1940					57	58	59	60	61	62	63	64	65	66	
1941						57	58	59	60	61	62	63	64	65	66

Notes: This table presents the birth cohorts included in our main sample, the years we have selected to observe them, and the age of each given birth cohort in each given year of observation. All the selected birth cohorts are eligible for standard old age retirement at the age of 67, and for early retirement (ER) at the age of 62 if working in an ER firm. We start following individuals from the age of 57 to select a balanced sample with 5 years each of pre- and post-ER-eligibility. Our sample consists of married individuals reaching the early retirement age of 62 in the period 1998-2003. 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.

Table A3. Descriptive statistics for selected sample of women and their male spouses, according to whether the male spouse is older / not older.

	Full sample		Husband not older		Husband older	
	Mean	SD	Mean	SD	Mean	SD
Women						
Birth year	1938.68	1.69	1939.05	1.61	1938.56	1.70
Education (years)	10.65	2.66	11.00	2.81	10.54	2.60
Earnings (1,000 NOK)	178	91	188	92	174	91
Age gap (husband – wife)	-2.43	3.65	1.65	2.66	-3.76	2.86
Male spouses						
Birth year	1936.25	4.22	1940.00	3.12	1934.80	3.45
Education (years)	11.47	2.97	12.00	2.90	11.30	2.97
Earnings (1,000 NOK)	311	258	353	251	297	259
Number of couples	2462		603		1859	

Notes: The table presents summary statistics for our selected sample of women (minus age gap requirement), according to whether the husband is older than the wife or not. The sample consists of married women born between 1936 and 1941 reaching the early retirement age of 62 in the period 1998 to 2003. Furthermore, we restrict our sample to women employed when aged 57, and with male spouses not treated by the reform based on their pre-reform firm affiliation. All outcomes are measured when the women are 57 years of age. Data are from Norwegian employer-employee registers, national education registers, and tax registers.

Table A4. Descriptive statistics for full and selected samples

	Full Sample		Selected Sample	
	Mean	SD	Mean	SD
Panel A. Men and their female spouses				
Men				
Birth year	1938.60	1.69	1939.02	1.60
Education (years)	11.35	3.27	11.36	3.02
Earnings (1,000 NOK)	291	232	310	210
Age gap (husband – wife)	3.44	4.28	4.56	3.41
Female spouses				
Birth year	1942.48	4.46	1943.64	3.39
Education (years)	10.52	2.64	10.49	2.37
Earnings (1,000 NOK)	165	89	184	92
Number of couples	38411		1990	
Panel B. Women and their male spouses				
Women				
Birth year	1938.62	1.68	1939.05	1.61
Education (years)	10.27	2.67	11.00	2.81
Earnings (1,000 NOK)	162	83	188	92
Age gap (husband – wife)	-3.44	3.92	1.65	2.66
Male spouses				
Birth year	1935.23	4.45	1940.00	3.12
Education (years)	10.87	3.18	12.00	2.90
Earnings (1,000 NOK)	243	189	353	251
Number of couples	31970		603	

Notes: The table presents summary statistics for the full sample of individuals born between 1936 and 1941 with information on firms in 1988, and for our selected sample. Our selected 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 in 1988 and aged 57, and with spouses not treated by the reform based on their 1988 pre-reform firm affiliation. All outcomes are measured when the men or women are 57 years of age. Data are from Norwegian employer-employee registers, national education registers, and tax registers.

Table A5. The direct effect of the reform among men, reweighted by the share of women with similar earnings history and current income

			Program Substitution		
	ER	Employment	DI	Unemployment	Sick Leave
	(1)	(2)	(3)	(4)	(5)
ITT effect	0.2799***	-0.2787***	0.0132	0.0263	-0.0361
SE	(0.041)	(0.045)	(0.036)	(0.023)	(0.029)
N			19596		

Notes: This table reports reduced-form estimates of equation (9) of men's eligibility for early retirement (ER) benefits on own employment and take-up of various public policies, reweighting each observation by the share of women in our sample with similar earnings history and current income. For the reweighting procedure, we construct 25 cells comprising each unique combination of quintiles of earnings history and quintiles of current income observed in our sample, and weight each observation by the share of women in his respective cell. We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm which 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. Data are from Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A6. Indirect effect of the reform on the health of female spouses

	Mortality	Doctor visits	Emergency room visits
ITT effect	0.018	0.074	0.014
SE	(0.009)	(0.154)	(0.021)
Mean	0.046	3.223	0.141
N		2468	

Notes: This table reports estimates from a cross-sectional regression of men's eligibility for early retirement (ER) on their female spouses' health outcomes, controlling for age fixed effects of both spouses. Mortality is defined as death by 2007. Doctor and emergency room visits measured in 2007. Eligibility for ER is based on firm affiliation when aged 57. The sample is restricted to married men employed aged 57 and with female spouses working in a non-ER firm when their husband was aged 57. Data are from Norwegian employer-employee data, the national death register, hospital registers, and national registers used to reimburse doctors. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A7. The indirect effect of the reform on spousal outcomes interacted with the within-household income gap

		Program take-up			
	Employment	ER	DI	Unempl.	Sick leave
	(1)	(2)	(3)	(4)	(5)
Panel A. Female spouses					
ITT effect	-0.034**	0.017*	0.028**	-0.014	-0.000
	(0.016)	(0.009)	(0.013)	(0.009)	(0.010)
ITT effect × inc. gap	-0.000	-0.000	-0.000	0.0000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N			19060		
Panel B. Male spouses					
ITT effect	-0.054*	0.042*	0.038	-0.030	-0.027
	(0.031)	(0.024)	(0.026)	(0.020)	(0.017)
ITT effect × inc. gap	-0.000***	0.000*	0.000***	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N			5730		

Notes: This table reports reduced-form estimates from an alternative specification of equation (9) in which we interact the main treatment indicator ($FirmEligible_i * AgeEligible_{it}$) with a variable measuring the income gap between wife and husband. We define treatment as being eligible for early retirement (ER) benefits through pre-reform affiliation with a firm which 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. Data are from Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A8. The indirect effect of the reform on spousal outcomes interacted with the spousal age gap

		Program take-up			
	Employment	ER	DI	Unempl.	Sick leave
	(1)	(2)	(3)	(4)	(5)
Panel A. Female spouses					
ITT effect	-0.176***	0.105***	0.082***	-0.021*	-0.022*
	(0.020)	(0.016)	(0.018)	(0.012)	(0.013)
ITT effect × age gap	0.030***	-0.019***	-0.012***	0.002	0.003
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)
N			19596		
Panel B. Male spouses					
ITT effect	-0.010***	0.068***	0.031	-0.031*	-0.043**
	(0.032)	(0.026)	(0.026)	(0.018)	(0.017)
ITT effect × age gap	0.039***	-0.022***	-0.018***	0.008*	0.006*
	(0.006)	(0.004)	(0.005)	(0.004)	(0.003)
N			5936		

Notes: This table reports reduced-form estimates from an alternative specification of equation (9) in which we interact the main treatment indicator ($FirmEligible_i * AgeEligible_{it}$) with a variable measuring the income gap between the man (woman) and the female (male) spouse. We define treatment as being eligible for early retirement (ER) benefits through pre-reform affiliation with a firm which 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. Data are from Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Section B – Sample and Specification Checks

i. Sample selection checks

As discussed in Section 3 of the paper, we impose a number of restrictions on our sample prior to performing our main analysis. Here, we investigate the sensitivity of our results to relaxing each of these restrictions, with the results presented in Tables B1 (direct effects) and B2 (spousal responses). Each row in each table tests a separate sample restriction. To facilitate comparison with our main results, we include the results from our preferred specification in row (1).

First, as discussed in Section 3, we identify treated firms based on backward induction. This classification approach likely results in some measurement error; we will erroneously classify some treated firms as control firms when they do not have any workers taking-up early retirement during our analysis period. The risk of misclassifying a treated firm as a control firm is greater the smaller the firm, and in our main analysis we, therefore, omit small firms with 10 or fewer employees. Rows (2) and (3) of Tables B1 and B2 indicate that the main findings are robust to altering the minimum number of employees as either 5 or 15.

Second, our treatment group consists of workers in both the public and the private sectors, while the control group consists of workers only in the private sector. As there could be systematic differences between workers in public and private firms that are correlated with being of ER eligible age, we re-estimate equation (9) using only individuals in the private sector. Row (4) of Tables B1 and B2 show that our main findings are robust to imposing this restriction. Interestingly, using the sample of only private sector workers, we also find that the reform reduces the labor force participation of male spouses to ER-eligible women by 10.7 percentage points. However, it should be noted that here the sample size is only 319 couples.

Third, our main results are based on individuals subject to the lowest ER eligibility age (born between 1936 and 1941). We impose this cohort restriction to facilitate interpretation of our results. We explore the robustness of this cohort selection by including cohorts born between 1933 and 1935, and eligible for ER from the age of 64. To perform this auxiliary analysis, we estimate a modified version of equation (9) in which we replace the $AgeEligible$ term with a categorical time-to-eligibility variable. Specifically, instead of using the individual's age to construct the pre- and post-periods, we use a variable which takes a value of 0 the year the individual reaches ER eligibility (64 or 62, depending on year of birth), -1 the year before reaching ER eligibility, 1 the year after reaching ER eligibility, and so on. Thus, the pre-period is when the individual is younger than the ER eligibility age and the post-period

is when the individual is older than the ER eligibility age. As shown in Row (5) of Tables B1 and B2, our results are robust to the inclusion of additional birth cohorts. It is also worth noting that, when more cohorts are included, the negative employment effect on male spouses to ER-eligible women also becomes significant at conventional levels.

Fourth, as noted in Section 3 of the paper, we also restrict the sample to couples in which the spouse is younger than the ER-eligible individual. We impose this restriction as spouses older than the ER-eligible individuals could already have reached standard retirement age by the time the ER-eligible individual reaches ER age. This restriction drastically reduces the sample available to examine the effect of the reform on the male spouses of ER-eligible women, as most husbands are older than their wives. In Row (6) of Tables B1 and B2 we relax this restriction, allowing inclusion of spouses up to two years older than the ER-eligible individual in the sample. Relaxing this sample restriction more than doubles the sample size available for studying the effect of the reform on male spouses of ER-eligible women. Again, with the larger sample size, the effect of the reform on the labor supply of male spouses of ER-eligible women is now statistically significant at conventional levels.

Finally, a focus of this paper is to examine if the reform had an effect on the labor market behavior and social security take-up of the spouse. To isolate this effect, we restrict our sample to couples in which the spouse worked in a non-ER firm in 1988. The reason for this is that any potential effect among spouses in treatment firms will be a combination of direct effects due to early retirement eligibility and indirect effects due to interdependencies in couples' retirement decisions. By focusing on couples in which the spouse worked in a control firm, we eliminate the direct effect contamination. Row (7) of Tables B1 and B2 shows that when we relax this restriction, no significant cross-program effect is found on spousal take-up of disability insurance. This means that the female spouses of ER-eligible men only respond to the reform by switching into DI if they do not qualify for ER.

ii. Model specification checks

In addition to being robust to different sample selections, our main findings are also robust to several model specification checks. Most importantly, our main findings are robust to controlling for spousal age in different ways and to replacing individual fixed effects with firm fixed effects and other time-invariant controls. First, as explained in Section 3, our main specification, which includes individual and age fixed effects, cannot include spousal age fixed effects as these would be perfectly collinear with the individual age fixed effects. However, our

findings are robust to including both linear and quadratic controls for spousal age, something shown in Rows (2) and (3) of Tables B3 and B4.

Second, there are some opponents of the use of individual fixed effects in settings where the dependent variable is an absorbing state, which is the case for our main outcome variables. Therefore, we show that our results are robust to dropping individual fixed effects from the model. Removing individual fixed effects allows for the inclusion of spousal age fixed effects, and in row (4) of Tables B3 and B4, we show that our results are robust to this model specification as well. In row (5) of Tables B3 and B4, we show that our results are also robust to including firm fixed effects and linear earnings controls.

Table B1. Robustness of direct effect to different sample selection checks

	ER	Empl.	DI	Unempl.	Sick leave
Panel A. Men					
(1) Baseline	0.273***	-0.217***	-0.049***	-0.018**	-0.022**
(N=19,596)	(0.019)	(0.019)	(0.014)	(0.008)	(0.009)
(2) At least 5 employees	0.296***	-0.236***	-0.050***	-0.016***	-0.021***
(N=28,854)	(0.015)	(0.016)	(0.012)	(0.006)	(0.007)
(3) At least 15 employees	0.238***	-0.190***	-0.041**	-0.021**	-0.022**
(N=14,672)	(0.023)	(0.023)	(0.016)	(0.008)	(0.010)
(4) Private sector only	0.304***	-0.253***	-0.043***	-0.025***	-0.028***
(N=16,004)	(0.022)	(0.021)	(0.015)	(0.009)	(0.009)
(5) Include cohorts born 1933-35	0.287***	-0.243***	-0.039***	-0.019***	-0.018**
(N=24,240)	(0.017)	(0.017)	(0.012)	(0.007)	(0.007)
(6) Including older spouses	0.271***	-0.217***	-0.046***	-0.017**	-0.021**
(N=21,589)	(0.019)	(0.019)	(0.013)	(0.007)	(0.008)
(7) Including treated spouses	0.244***	-0.194***	-0.039***	0.002	-0.011**
(N=88,387)	(0.012)	(0.013)	(0.008)	(0.004)	(0.005)
Panel B. Women					
(1) Baseline	0.231***	-0.225***	0.001	0.003	-0.046***
(N=5,936)	(0.030)	(0.032)	(0.026)	(0.014)	(0.016)
(2) At least 5 employees	0.252***	-0.239***	-0.003	0.005	-0.0271**
(N=8,039)	(0.025)	(0.027)	(0.022)	(0.012)	(0.014)
(3) At least 15 employees	0.191***	-0.220***	0.033	0.017	-0.061***
(N=4,449)	(0.037)	(0.039)	(0.030)	(0.016)	(0.020)
(4) Private sector only	0.276***	-0.242***	-0.026	0.013	-0.061***
(N=3,201)	(0.047)	(0.044)	(0.035)	(0.026)	(0.021)
(5) Include cohorts born 1933-35	0.234***	-0.223***	-0.006	0.013	-0.044***
(N=7,707)	(0.026)	(0.027)	(0.020)	(0.012)	(0.014)
(6) Including older spouses	0.229***	-0.205***	-0.021	-0.004	-0.036***
(N=12,815)	(0.020)	(0.023)	(0.018)	(0.010)	(0.012)
(7) Including treated spouses	0.220***	-0.196***	-0.012	0.007	-0.027***
(N=23,313)	(0.020)	(0.019)	(0.016)	(0.010)	(0.010)

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (ER) benefits on own employment and take-up of various public policies, for various alternative sample selection criteria specified from (1) to (7). We define treatment as being eligible for early retirement (ER) benefits through pre-reform affiliation with a firm which 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, except in specification (5). 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, except in specifications (6) and (7). 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 security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table B2. Robustness of spousal response to different sample selection checks

	Empl.	ER	DI	Unempl.	Sick leave
Panel A. Female spouses					
(1) Baseline	-0.038***	0.017**	0.030**	-0.011	-0.011
(N=19,596)	(0.014)	(0.008)	(0.012)	(0.008)	(0.009)
(2) At least 5 employees	-0.0435***	0.0193***	0.0303***	-0.0055	-0.0159**
(N=28,854)	(0.011)	(0.006)	(0.010)	(0.007)	(0.008)
(3) At least 15 employees	-0.0428***	0.0143	0.0328**	-0.0161*	-0.0078
(N=14,672)	(0.016)	(0.011)	(0.014)	(0.009)	(0.011)
(4) Private sector only	-0.0330**	0.0109	0.0285**	-0.0107	-0.0137
(N=16,004)	(0.015)	(0.009)	(0.013)	(0.009)	(0.010)
(5) Include cohorts born 1933-35	-0.0379***	0.0236***	0.0185*	-0.0098	-0.0047
(N=23,866)	(0.012)	(0.008)	(0.011)	(0.008)	(0.008)
(6) Including older spouses	-0.0317**	0.0141	0.0220*	-0.0103	-0.0105
(N=21,589)	(0.013)	(0.009)	(0.012)	(0.008)	(0.009)
(7) Including treated spouses	-0.0155*	0.0126**	0.0057	-0.0031	-0.0028
(N=88,387)	(0.008)	(0.006)	(0.007)	(0.003)	(0.005)
Panel B. Male spouses					
(1) Baseline	-0.025	0.036*	0.008	-0.017	-0.026
(N=5,936)	(0.029)	(0.022)	(0.023)	(0.016)	(0.016)
(2) At least 5 employees	-0.0342	0.0318*	0.0002	-0.0092	-0.0158
(N=8,039)	(0.024)	(0.018)	(0.020)	(0.013)	(0.014)
(3) At least 15 employees	-0.0082	0.0152	-0.0139	-0.0009	-0.0141
(N=4,449)	(0.036)	(0.029)	(0.028)	(0.018)	(0.017)
(4) Private sector only	-0.1070**	0.0668*	0.0389	-0.0167	-0.0439*
(N=3,201)	(0.045)	(0.035)	(0.037)	(0.027)	(0.023)
(5) Include cohorts born 1933-35	-0.0446*	0.0384**	0.0094	-0.0177	-0.0311**
(N=7,707)	(0.023)	(0.018)	(0.019)	(0.013)	(0.014)
(6) Including older spouses	-0.0362*	0.0445***	-0.0110	-0.0175	-0.0018
(N=12,815)	(0.020)	(0.017)	(0.016)	(0.011)	(0.011)
(7) Including treated spouses	-0.0238	0.0286	0.0014	-0.0024	-0.0214**
(N=23,313)	(0.020)	(0.019)	(0.013)	(0.007)	(0.009)

Notes: This table reports reduced-form estimates of equation (9) of eligibility for early retirement (ER) benefits on spousal employment and take-up of various public policies, for various alternative sample selection criteria specified from (1) to (7). We define treatment as being eligible for early retirement (ER) benefits through pre-reform affiliation with a firm which 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, except in specification (5). 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 (except in specifications (6) and (7)). 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 security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table B3. Direct effect, robustness to different specification checks

	ER	Empl.	DI	Unempl.	Sick leave
Panel A. Men					
(1) Baseline	0.273***	-0.217***	-0.049***	-0.018**	-0.022**
	(0.019)	(0.019)	(0.014)	(0.008)	(0.009)
(2) Linear control for spouse age	0.270***	-0.216***	-.048***	-.018**	-.023***
	(0.019)	(0.020)	(0.014)	(0.008)	(0.009)
(3) Quadratic control for spouse age	.270***	-.216***	-.048***	-.018**	-.023***
	(.019)	(0.020)	(0.014)	(0.008)	(0.009)
(4) Without individual FE	.270***	-.216***	-.047***	-.026***	-.026***
	(0.019)	(0.021)	(0.018)	(0.007)	(0.006)
(5) Firm FEs + earnings controls	0.270***	-0.215***	-0.049***	-0.018**	-0.24**
	(0.020)	(0.021)	(0.015)	(0.008)	(0.009)
N			19596		
Panel B. Women					
(1) Baseline	0.231***	-0.225***	0.001	0.003	-0.046***
	(0.030)	(0.032)	(0.026)	(0.014)	(0.016)
(2) Linear control for spouse age	0.230***	-0.221***	-0.005	0.004	-0.048***
	(0.031)	(0.032)	(0.026)	(0.015)	(0.017)
(3) Quadratic control for spouse age	0.229***	-0.221***	-0.004	0.004	-0.048***
	(0.031)	(0.032)	(0.026)	(0.015)	(0.017)
(4) Without individual FE	0.230***	-0.234***	0.008	-0.021*	-0.023**
	(0.031)	(0.036)	(0.035)	(0.012)	(0.010)
(5) Firm FEs + earnings controls	0.228***	-0.220***	-0.004	0.003	-0.046***
	(0.032)	(0.034)	(0.028)	(0.016)	(0.017)
N			5936		

Notes: Direct effect of eligibility for early retirement (ER) benefits on employment and take-up of various public policies. See Section 3 of the paper for the full baseline specification (1). In specification (2), we include a linear control for spousal age. In specification (3), we include a quadratic control for spousal age. In specification (4), we drop the individual fixed effects (FE) from the baseline specification and include spousal age FE instead. Specification (5) adds firm FE and linear controls for individual and spousal earnings at age 57 to specification (4). We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm which 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. Data are from Norwegian employer-employee data, tax registers, and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table B4. Spousal response, robustness to different specification checks

	Empl.	ER	DI	Unempl.	Sick leave
Panel A. Female spouses					
(1) Baseline	-0.038***	0.017**	0.030**	-0.011	-0.011
	(0.014)	(0.008)	(0.012)	(0.008)	(0.009)
(2) Linear control for spouse age	-0.041***	0.017**	0.029**	-0.012	-0.011
	(0.014)	(0.008)	(0.012)	(0.008)	(0.010)
(3) Quadratic control for spouse age	-0.038***	0.016*	0.028**	-0.012	-0.010
	(0.013)	(0.008)	(0.012)	(0.008)	(0.010)
(4) Without individual FE	-0.042**	0.018**	0.030*	-0.004	0.005
	(0.017)	(0.008)	(0.017)	(0.008)	(0.006)
(5) Firm FEs + earnings controls	-0.041***	0.018**	0.029**	-0.013	-0.010
	(0.020)	(0.021)	(0.015)	(0.008)	(0.009)
N			19596		
Panel B. Male spouses					
(1) Baseline	-0.025	0.036*	0.008	-0.017	-0.026
	(0.029)	(0.022)	(0.023)	(0.016)	(0.016)
(2) Linear control for spouse age	-0.039	0.034	0.004	-0.019	-0.034**
	(0.029)	(0.022)	(0.024)	(0.016)	(0.016)
(3) Quadratic control for spouse age	-0.032	0.030	0.001	-0.019	-0.033**
	(0.028)	(0.022)	(0.023)	(0.016)	(0.016)
(4) Without individual FE	-0.032	0.032	-0.002	0.001	-0.018
	(0.032)	(0.022)	(0.029)	(0.015)	(0.011)
(5) Firm FEs + earnings controls	-0.035	0.032	0.001	-0.019	-0.032*
	(0.030)	(0.023)	(0.025)	(0.017)	(0.017)
N			5936		

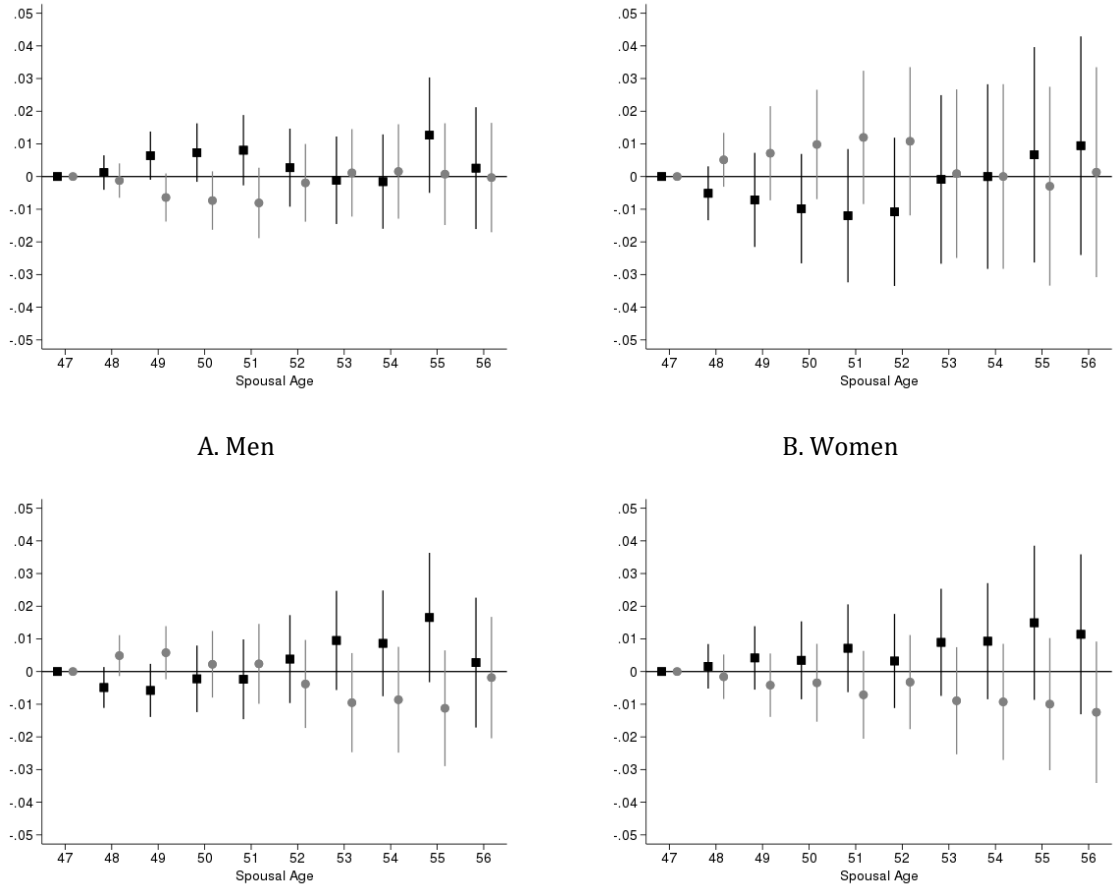
Notes: Spousal response to eligibility for early retirement (ER) benefits on employment and take-up of various public policies. See Section 3 of the paper for the full baseline specification (1). In specification (2), we include a linear control for spousal age. In specification (3), we include a quadratic control for spousal age. In specification (4), we drop the individual fixed effects (FE) from the baseline specification and include spousal age FE instead. Specification (5) adds firm FE and linear controls for worker and spousal earnings at age 57 to specification (4). We define treatment as being eligible for ER benefits through pre-reform affiliation with a firm which 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. Data are from Norwegian employer-employee data, tax registers and social security registers. Standard errors clustered at the firm level are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Section C – Placebo Test

One potential concern associated with our results is that individuals (and their spouses) affiliated with ER firms in 1988 are systematically more likely to leave the labor force early than individuals (and their spouses) who were not affiliated with ER firms in 1988. While the raw data plots (Figures 2 and 6) as well as the nonparametric event studies (Figures 4 and 5) provide evidence against this concern, in this section we probe the data further and examine this concern in more detail using a placebo test.

Our placebo test consists of estimating a modified version of equation (10) on a set of younger cohorts born between 1946 and 1951 unaffected by the reform. Our period of observation is from 1993 to 2007, and all our sample selection criteria remain the same. The only difference from equation (10) as described in Section 3, is that we re-specify the age dummies to $a \in [48(1)56]$, where a represents the age of the individual. If the π_{48} to π_{56} estimates are economically small and not statistically significantly different from zero, that implies that there are no secular trends across treatment and control groups that are driving our results. On the other hand, if ER-eligible individuals (and their spouses) are more likely to exit the labor market early, then the π_{48} to π_{56} estimates should be negative and statistically significant in our placebo set-up. Figure C1 illustrates the results of this exercise. Looking across subfigures A through D, all estimates of π_a are economically small and none is statistically significant. The results from this placebo test provide additional support for our identifying assumption.

Figure C1: Placebo test on employment and disability insurance take-up



A. Men

B. Women

C. Female spouses

D. Male spouses

Notes: Each dot in each figure corresponds to the "placebo effect" at the given age indicated on the x-axis, and the bar extending from each dot represents the 95% confidence intervals. The dashed horizontal line is at zero to facilitate interpretation of the results. Note that we define the year the worker was aged 47 as the base year, such that all estimates will be relative to this baseline. We define treatment as pre-reform affiliation with a firm which later joined the ER scheme. Individuals in the control group can 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 1946 and 1951 reaching the age of 52 in the period 1998 to 2003. Furthermore, we restrict our sample to individuals employed aged 47, 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 security registers.

Issued in the series Discussion Papers 2019

2019

01/19 January, **Aline Bütikofer**, Christopher Cronin, Meghan Skira, "Employment Effects of Healthcare Policy: Evidence from the 2007 FDA Black Box Warning on Antidepressants"

02/19 February, **Ingar Haaland** and Cristopher Roth "Beliefs About Racial Discrimination and Support for Pro-Black Policies "

03/19 February, **Astrid Kunze** and Xingfei Liu, "Universal Childcare for the Youngest and the Maternal Labour Supply"

04/19 February, **Ingvild Almas, Alexander W. Cappelen, Bertil Tungodden**. "Cutthroat capitalism versus cuddly socialism: Are Americans more meritocratic and efficiency-seeking than Scandinavians?"

05/19 February**, Chang Koo Chi**, Kyoung Jin Choi. "Performance Measurement in Agency Models"

06/19 March, **Alexander W. Cappelen**, **Ranveig Falch** and **Bertil Tungodden.** "The Boy Crisis: Experimental Evidence on the Acceptance of Males Falling Behind"

07/19 March, Frode Skjeret, **Frode Steen** and **Timothy G.A Wyndham**. "Paywalls and the demand for online news"

08/19 April, **Ola. H. Grytten** and Viktoriia Koilo. "The Financial Instability Hypothesis and the Financial Crisis in Eastern European Emerging Economies"

09/19 April, **Alexander W. Cappelen**, Johanna Mollerstrom, Bjørn-Atle Reme and **Bertil Tungodden.** "A Meritocratic Origin of Egalitarian Behavior"

10/19 April, **Fanny Landaud**. "From Employment to Engagement? Stable Jobs, Temporary Jobs, and Cohabiting Relationships"

11/19 May, **Ola Honningdal Grytten** and Viktoriia Koilo. "Evidence of the Environmental Kuznets Curve in Emerging Eastern European Economies"

12/19 June, Rune Midjord, Tomás Rodríguez Barraquer, and **Justin Valasek.** "Robust Information Aggregation Through Voting"

13/19 June, Eva M. Berger, **Henning Hermes**, Guenther Koenig, Felix Schmidt, and Daniel Schunk. "Self-regulation Training and Job Search Behavior: A Natural Field Experiment Within an Active Labor Market Program"

14/19 June, **Henning Hermes**, Martin Huschens, Franz Rothlauf and Daniel Schunk. "Motivating Low-Achievers—Relative Performance Feedback in Primary Schools"

15/19 August, Viktoriia Koilo and Ola **Honningdal Grytten.** "The Blue Maritime C luster Crisis Financial Instability and Supply Chain Management Effects"

16/19 September, Jonas Andersson, **Fred Schroyen** and Gaute Torsvik. "The impact of international tax information exchange agreements on the use of tax amnesty: evidence from Norway"

17/19 September, Ingrid Kristine Folgerø, **Torfinn Harding** and Benjamin S. Westby. "Going Fast or Going Green? Evidence from Environmental Speed Limits in Norway"

18/19 September. Julie Riise, Barton Willage and **Alexander Willén.** "Can Female Doctors Cure the Gender STEMM Gap? Evidence from Randomly Assigned General Practitioners"

19/19 September**, Aline Bütikofer**, **Katrine V. Løken** and **Alexander Willén.** "Building Bridges and Widening Gaps: Efficiency Gains and Equity Concerns of Labor Market Expansions"

20/19 September, Richard Friberg, **Frode Steen** and **Simen A. Ulsaker**. "Hump-shaped cross-price effects and the extensive margin in cross-border shopping"

21/19 July, Mai Nguyen-Ones, and **Frode Steen. "**Market Power in Retail Gasoline Markets"

22/19 October, Tunç Durmaz and **Fred Schroyen.** "Evaluating Carbon Capture and Storage in a Climate Model with Endogenous Technical Change"

23/19 November, **Henning Hermes** and Daniel Schunk. "If You Could Read My Mind – An Experimental Beauty-Contest Game With Children"

24/19 December, **Vincent Somville.** "Having a Daughter Reduces Male Violence Against a Partner"

2020

01/20 January, **Laura Khoury**, Clément Brébion and Simon Briole. "Entitled to Leave: the impact of Unenployment Insurance Eligibility on Employment Duration and Job Quality"

02/20 January, Thomas Buser, **Alexander Cappelen**, Uri Gneezy, Moshe Hoffman and **Bertil Tungodden**. "Competitiveness, gender and handedness: a large-sample intercultural study"

03/20 February, **Patrick Bennett**, Chiara Ravetti and Po Yin Wong. "Losing in a Boom: Long-term Consequences of a Local Economic Shock for Female Labour Market Outcomes"

04/20 April, **Øivind A. Nilsen**. "The Labor Market in Norway: 2000-2018"

05/20 April, **Simen A. Ulsaker**. "Exclusionary contracts and incentives to innovate"

06/20 May, **Alexander W. Cappelen**, **Ranveig Falch**, **Erik Ø. Sørensen** and **Bertil Tungodden**. "Solidarity and Fairness in Times of Crisis"

07/20 May, Gozde Corekcioglu, Marco Francesconi and **Astrid Kunze**. "Do Generous Parental Leave Policies Help Top Female Earners?"

08/20 June, **Ola Honningdal Grytten**. "Weber revisited: A literature review on the possible Link between Protestantism, Entrepreneurship and Economic Growth"

09/20 June, Eva M. Berger , Ernst Fehr, **Henning Hermes** , Daniel Schunk and Kirsten Winkel. "The Impact of Working Memory Training on Children's Cognitive and Noncognitive Skills"

10/20 June, **Ola Honningdal Grytten**. "Two centuries of economic growth: Norwegian GDP 1816-2020"

11/20 July, **Ola Honningdal Grytten**, Magnus Lindmark and Kjell Bjørn Minde. "Energy Intensity and the Environmental Kuznets Curve"

12/20 August, **Ola Honningdal Grytten**. "Puritan Motivation for Serial Entrepreneurship: The Haugean Example"

13/20 August, Julian Johnsen, Hyejin Ku and **Kjell G. Salvanes**. "Competition and Career Advancement: The Hidden Costs of Paid Leave"

14/20 August, Patrick Bennett, Richard Blundell and **Kjell G. Salvanes**. "A Second Chance? Labor Market Returns to Adult Education Using School Reforms"

15/20 August, Paul Brandily, Clément Brébion, Simon Briole and **Laura Khoury**. "A Poorly Understood Disease? The Unequal Distribution of Excess Mortality Due to COVID-19 Across French Municipalities"

16/20 September, **Ingvild Almås**, **Vincent Somville** and Lore Vandewalle. "The Effect of Gender-Targeted Transfers: Experimental Evidence From India"

17/20 September, **Ola Honningdal Grytten.** "The Wealth of a Nation: Norways Road to Prosperity"

18/20 September, Asbjørn G. Andersen, Simon Franklin, Tigabu Getahun, Andreas Kotsadam, **Vincent Somville** and Espen Villanger. "Does Wealth Reduce Support for Redistribution? Evidence from an Ethiopian Housing Lottery"

19/20 September, **Ingvild Almås**, Lars Ivar Berge, **Kjetil Bjorvatn**, **Vincent Somville** and **Bertil Tungodden**. "Adverse selection into competition: Evidence from a large-scale field experiment in Tanzania"

20/20 September, Julian Vedeler Johnsen, Kjell Vaage and **Alexander Willén**. "Interactions in Public Policies: Spousal Responses and Program Spillovers of Welfare Reforms"

NORGES HANDELSHØYSKOLE
Norwegian School of Economics

Helleveien 30
NO-5045 Bergen
Norway

T +47 55 95 90 00
E nhh.postmottak@nhh.no
W www.nhh.no