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The Effects of a Retirement Age Reform on Couples' Labor Supply Decisions

By

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The Effects of a Retirement Age Reform on Couples' Labor Supply Decisions *

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Abstract

This paper estimates how a pension reform in Israel that raised both men's and women's ages of retirement benefits concurrently affected spousal labor supply decisions. We utilize detailed administrative data to estimate spouse retirement decisions and to understand their interdependencies. We find that one's own retirement age deferral increases their labor supply. However, spillover effects differ by gender. While for men, labor supply does not depend on their wife's retirement age deferral, for women, postponing their husband's retirement age increases their labor supply if their own retirement age has not been postponed.

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1. Introduction

Pension reform has been prioritized on the agenda of many governments in the past few decades. Due to population aging and declining fertility rates, many countries face the actuarial challenge of ensuring financially stable pension systems while simultaneously providing adequate income upon retirement for their citizens. In an effort to address these challenges, governments have launched significant pension reforms, including raising retirement ages, changing the way entitlements are calculated, and other measures intended to introduce sufficient savings in their pension systems. To understand the labor force consequences of such policy changes, a considerable number of studies have tried to detect and causally identify the effects of changes in pension system traits on labor supply and retirement decisions. Changes in pension systems that have been explored include, for example, changes in social security benefits, increases in the threshold as well as the subsequent gradual abolishment of earnings tests, changes in disability pension, increases in the age at which early retirement (ERA) and full retirement (FRA) first become available, and changes in the provision of occupational pension programs (see Hernæs 2013 for a literature review).

Most of these papers study the implications of pension reforms that are individually designed.[1] However, since in most households both spouses participate in the labor market, retirement decisions are likely to be interdependent within couples. Indeed, earlier papers have found that couples tend to coordinate their retirement decisions (see for example, Hurd 1990, Blau 1998, Zweimüller, et al. 1996, Gustman & Steinmeier 2000, and Coile, 2004). However, since timing coordination decisions can result from spouses' age differences, gender differences in life expectancy and other confounding effects, it is difficult to determine whether one’s retirement decision causally affects their spouse’s decision. Research considering spillover effects of spousal changes in pension system on retirement decision is quite limited, and there are even fewer papers that analyze the effects of changes in pension system attributes for both genders on joint retirement decisions.

To fill this gap, this paper studies the implications of a pension reform in Israel that raised both men's and women's ages for retirement benefits concurrently as of 2004. We exploit this

[1] Most research (Mastrobuoni 2009; Staubli and Zweimuller 2013; Hanel and Riphahn 2012; Atalay and Barrett 2015; Danzer 2013; Vestad 2013; in Israel, see Bank of Israel Reports 2010 and 2014 and Avioz and Kimhi 2018) focused on the effects of reforms on the individuals targeted by the program changes, neglecting potential spillover effects on spouses of those affected.

reform as a natural experiment in order to understand the interdependencies in spousal causal retirement decisions. In December 2003, the Israeli government approved a reform that defers the age of retirement benefits on a gradual basis. The reform was implemented beginning in March 2004 and through mid-2009 for both men and women. For men, the retirement age increased from 65 to 67, while for women it was from 60 to 62. Every year, the age of retirement was raised for men and women by four months. The main implications of this change were reflected in deferring the age of eligibility for employment-related pensions (second pillar), as well as the means-tested old age allowance (first pillar).

Our research design exploits the effect of raising both men's and women's age for retirement benefits, which enables to define treatment and control groups based on each spouse’s date of birth. Importantly, we allow the reform to affect each spouse directly, indirectly, and also allow for an interaction effect, which indicates that both spouses experienced the reform. We conduct this analysis for men and women separately. That is, we test how the husband's (wife's) probability of working is affected by raising his (her) own retirement age and how it is affected by raising their spouse’s retirement age which might, in turn, also depend on his (her) own retirement age deferral.

Our results indicate that one's own increase in retirement age has the largest positive effect on individual labor supply. For men, the estimated effect is slightly greater than for women and does not depend on any change in their wife's retirement age. For women, raising their husbands’ retirement age increases their labor supply but only if their own retirement age does not increase. If their retirement age is also postponed, it does not further raise their labor supply.

Theoretical models suggest that both income and leisure complementarity considerations might explain the above results.[2] The leisure complementarity channel predicts that raising a spouse's retirement age would decrease his/her leisure time which in turn would increase the willingness of the other spouse to continue working if spouses' leisure is complementary. In

[2] Most theoretical models characterize spouses' retirement decisions in a family decision making framework that incorporates both the fact that one spouse's valuation of leisure time depends on the other spouse's retirement, and that the budget constraint is determined at the household level (see for example, Hurd, 1990 and Blau, 1998). Alternative ways of modeling spousal interactions are bargaining models that can be either cooperative (see Vermeulen, 2002, and Michaud and Vermeulen, 2011) or noncooperative (see Gustman and Steinmeier 2000, 2004). In noncooperative models, spouses' retirement decisions are interdependent not only due to complementarity and wealth effects but also because of changes in the relative bargaining power between spouses.

contrast, the income channel predicts the opposite effect. It forecasts that raising a spouse's retirement age would increase this spouse's probability of working and increase household income, thereby reducing the need for the other spouse to continue working as well. Our result implies that, for women, the leisure complementarity channel might be stronger, thereby increasing the positive correlation between spouses' retirement decisions. This is consistent with the descriptive statistics discussed in the literature, on coordination of retirement decisions among spouses.

Our analysis also exhibits the importance of accounting for the interaction between both spouses' retirement age deferrals in regression specifications. We show that when the interaction between both spouses' retirement age deferrals is excluded from the regression analysis, the estimated effect of spouse retirement age deferral on women’s employment rate is lower by almost half compared to the baseline model. We infer that if the 2004 retirement age reform would have deferred only women’s retirement age, the magnitude of the effect on women’s employment would have been almost twice the size. Therefore, understanding the interdependencies of spouses' retirement age deferrals is necessary for designing better retirement age reform policies and correctly evaluating the consequences of postponing spouses' retirement ages.

Our identification strategy relies on comparing employment rates of couples with the same age structure before and after the reform was implemented. In order to validate our main identification strategy and show that our results are not driven by differences in years of birth between control and treatment groups, we conduct several robustness checks. First, we show that the characteristics of couples with the same age structure in the control and treatment groups are similar by running a series of balancing tests. Second, adding fixed effects that interact each spouse’s year of birth to the baseline specification changes our estimated effects only marginally. Third, part of our sample consists of individuals with unknown month of birth, for whom retirement benefits were given as if they were born in the middle of their year of birth. This feature enables us to examine whether the effect of retirement age deferral is different among individuals with and without an exact month of birth. Indeed, we find that the effects observed do not depend on an individual’s exact date of birth but rather on the actual deferral of the retirement age. Fourth, we demonstrate that restricting the sample to treatment and control group couples with closer years of birth changes our estimated coefficients only to a small extent. Finally, we are able to compare our main identification regression results to that of a double difference-in-differences empirical strategy. Using other empirical strategies enables us to validate our baseline results and show that they do not depend on the estimation strategy employed. In addition, it enables us to perform a placebo analysis and reject the possibility that our results could be replicated in a similar setting regardless of the reform's implementation.

Our research expands upon prior literature on couples and retirement and specifically assesses how couples react to increases in each other's retirement ages. Few recent papers have studied spillover effects of pension reforms among spouses and their causal effects.[3] However, as most reforms raised women's retirement age in order to equate it to that of men's, the majority of the papers studied neither the effect of a husband's retirement age on the wife's retirement behavior nor the effects of both spouses' retirement age deferrals.

Three papers are more closely related to our research since they study the effects of retirement age deferral of both spouses on each other's labor supply. Johnsen and Vaage (2017) study labor supply spillovers within households as a result of an early retirement reform in Norway. The reform, which was partially implemented and involved only some firms, enabled the use of a difference-in-differences model, comparing spousal employment before and after the workers reach the ERA for both treated and control workers. They found that workers eligible for early retirement reduced their employment rates compared to non-eligible workers. Moreover, they found that wives with treated husbands reduced their employment, but no effects were found on husbands' employment when their wives were treated. Additionally, Lalive & Parrota (2017) and Stancanelli (2017) both examine how labor force participation changes, using labor force survey data, as a function of both individual and spouses' FRA in similar settings in Switzerland and France, respectively. Both papers use a double RDD approach with the distance in months between the relevant policy

[3] Baker (2002) studies the introduction in 1975 of Spouse's Allowance to the Canadian Income Security system on the retirement behavior of couples. He used an empirical strategy that compared changes in retirement behavior of males and females who became eligible for the Spouse's Allowance to that of their counterparts of the same age, who due to the age of their spouse did not qualify for an allowance, and found significant spillover effects for men. Cribb, et al. (2013) examine an increase in women's early retirement age (ERA) in the UK and find, using a difference-in-differences approach, significant positive spillover effects on their husband's labor supply. Using a similar methodology, Selin (2012) exploited a Swedish pension reform to study the effect of a wife's retirement incentive on the husband's retirement behavior, but found no spillover effect. On the other hand, Atalay and Barrett (2015) who analyzed an increase in the eligibility age for pension benefits for women in Australia, found an increase in participation of men married to women in the affected cohorts. Atalay, Barrett and Sminski (2019) found additionally a large negative effect on the labor supply of veterans' wives due to an increase in Australian veterans' compensation and pension schemes.

threshold and the birth month as the running variable. This methodology enabled them to study the effect of reforms which increased the number of quarters required for maximum retirement benefits based on birth years.

Our paper goes beyond the previous literature by studying the effects of spouses' retirement age deferrals on their retirement decisions along with their interdependencies. We find that assessing the effects of deferring own and spouse's retirement ages without taking into account their interdependency or assessing only the effect of own retirement age deferral when both spouses' retirement ages are deferred, leads to an under-estimation of these effects. Moreover, we find that the spillover effect of one spouse’s change in retirement age on the other spouse’s labor supply, emphasized in the literature, might be canceled out if both spouses' retirement ages are deferred. Since we use a large panel of couples' administrative data instead of labor force survey data, it enables us to detect the effect of the pension reform specifically on the age cohorts affected by it. In addition, we are able to test the effect of the reform not only on employment rates, but also on several additional labor market outcomes, such as annual salary and the probability of keeping the same job throughout the year, and study heterogeneous effects of the reform over several dimensions, such as couples' ages, education levels and prior employment status.

The rest of the paper is organized as follows. Section 2 presents the institutional background, Section 3 describes the data, Section 4 discusses the identification strategy and Section 5 reports the results. Section 6 offers a summary and some conclusions.

2. Institutional Setting

Israel’s retirement income system is based on a universal social security allowance (first pillar) and on individual savings in pension funds (second pillar).[4]

The old-age social security allowance (the first pillar) is the basic amount paid by the state to retirees at retirement age. This pillar's aim is to guarantee a basic income for the elderly

[4] For a detailed description of Israel’s retirement income system, see Brender 2009.

population. The allowance is paid to every insured person[5] who has reached the absolute eligibility age.[6] At the age of conditional eligibility, it is paid on condition that the insured person passes an income test. The conditions for receiving the old-age allowance are not very exclusive and close to 90 percent of those eligible for the old-age allowance receive it already at the age of conditional eligibility. The conditions depend on total income (not including pension benefits) and are reduced if total income exceeds a certain threshold, so that most individuals are eligible for some old-age allowance, even if only partial. The minimum old-age allowance (the basic pension) ranged around 15.5 percent of the average wage, with additions for years worked, delay in receiving the allowance and pending additions, according to the individual's eligibility.[7]

The second pension pillar is pension savings that depend on employment and the wages of working-age employees, and its aim is to ensure that workers will be able to maintain their standard of living after retirement. Until 1995, Israelis' retirement savings were concentrated in occupational pension funds that offered defined-benefit schemes. Employees in the public sector and in large organizations were offered similar benefits in employer-funded programs with no direct employee contribution. Individuals could also enjoy tax benefits for depositing a portion of their uncovered salaries into private savings accounts—provided that the amounts were not withdrawn for at least 15 years from the date the account was opened.[8] At 2007 approximately 60 percent of the population was covered by the pension-savings pillar.

The government has carried out a series of reforms in both pillars over the past two decades, with the aim of covering its actuarial deficits and the projected growth of its budgetary expenditure. These reforms focused on the transition from defined benefit pension insurances to defined contribution schemes and on covering the actuarial deficits in the old pension funds. The transition from defined benefit plans to defined contribution plans took place in two main steps. The first step was taken in 1995, when the government ended the ability of

[5] The insured population includes all Israelis who have lived in Israel for a certain period of time prior to retirement age. New immigrants who arrive in Israel after the retirement age receive an old-age allowance that is not based on social security allowances.
[6] The age of absolute eligibility for men is 70. The age of absolute eligibility for women was 65 in 2004 and the state gradually raised it to 70 by 2020.
[7] The basic old-age allowance had reached about NIS 1,500 in 2013 (about USD 430), with fluctuations from this rate of about 3–4 percent from the economic stability reform of 1985, which automatically updated it in accordance with increases in the standard of living.
[8] Government support for pension saving took two forms: tax allowances at the times of deposit and withdrawal and on the returns.

new savers to join the old pension (defined benefit) funds and required that new, defined contribution, pension funds be established in their place. The second step was taken starting in 1999, when new public sector employees were no longer eligible to participate in the employer-funded pension scheme and were placed in the new pension funds. In order to deal with the actuarial deficit suffered by most of the old pension funds, the government formulated an arrangement that included a reduction in the benefits of members, an increase in monthly deposits by active members, and an injection of funds by the government.[9]

As part of this fiscal consolidation program, in December 2003, the Israeli government approved a reform to defer the age of retirement benefits. The change was implemented gradually between 2004 and 2009. Every year, the age of retirement was raised for men and women by four months: for men from 65 to 67 and for women from 60 to 62. The increase was reflected in the deferral of the age of eligibility for employment-related pensions (second pillar),[10] as well as the means-tested old age allowance (first pillar).[11] We exploit this reform to address how individuals' retirement age and their spouses' retirement age affects their labor force supply.

<u>3. Data</u>

Our empirical strategy relies on comparing adjacent cohorts while they are at the same age, with the younger cohort experiencing an increase in their retirement age at that age and the older cohort not experiencing this increase. Our cohort range of comparison is within two years of those first affected by the increased retirement age.

For example, the first increase in men's age of retirement from age 65 to age 65 and four months took place in March 2004. The first men affected by this increase were those born in March 1939, and as such, the first two-year cohort affected was born March 1939 through February 1941. We will compare their labor market outcomes while they are ages 65 to 65 and four months to the labor market outcomes of the adjacent two-year cohort that did not

[9] At the beginning of 2008, the government took an additional step in the pension area: it adopted a national compulsory pension accord covering all employees.
[10] Defined benefit pension funds allow retirement before the official pension age (i.e., age for early retirement) but at the cost of a reduction in the payment rate (the new funds are in any case based on previously defined contributions). This age for early retirement was 58 for women and 60 for men (the age of early retirement for women has been gradually raised in 2010 to 60, for women born after January 1952).
[11] In addition, the law also deferred the age at which employers can terminate, for any reason, the employment of the relevant workers and restricted the possibility of continuing to employ them in the public sector from 65 to 67.

experience this increase in retirement age, i.e. those born March 1937 through February 1939. In September 2004, the next increase in men's age of retirement is implemented and it is from age 65 and four month to age 65 and eight months. For this increase, we are comparing between men born September 1939 through August 1941 and men born September 1937 through August 1939 while they are between ages 65 and four months and 65 and eight months. Note that the cohort of men born March 1939 through August 1939 is considered treated at ages 65 to 65 and four months, but after age 65 and four months they are a control group, as at those ages they have already reached the statutory retirement age.

Our male (female) sample period begins when the oldest cohort that is in the control group turns 65 (60), March 2002, and ends when the youngest cohort that is in the treatment group turns 67 (62), May 2011. Our gender-specific dataset is at the individual-month level. For each month, we observe individuals from the relevant cohort, based on the age criteria defined above and accustomed to the incremental increases in the retirement age for each gender. Table 1 and Figure 1 describe this data construction process.

Our final dataset includes couples that have both spouses in the relevant age-range as defined above. While this implies a relatively narrow sample of couples with specific age differences between the spouses at given periods, it has the advantage of allowing us to compare the changes in labor force participation rates among highly comparable couples that are in the treatment and control groups. We also address the concern of our narrow sample of couples through a more standard difference-in-differences specification, which expands substantially the age-range for the spouses, as is discussed in Section 6.4 below.

To construct our dataset, we make use of administrative data from Israel's Central Bureau of Statistics (CBS). The dataset consists of the Jewish population of men born between March 1937 and May 1947 and women born between March 1942 and May 1949, who took part in the 1995 Israeli census (20% of population).[12] Each individual record contains information on the following variables: own and spouse's demographic variables (ethnicity based on parents' country of birth, years of education, type of higher education [secular versus religious], number of children, year of immigration to Israel, year and month of birth), labor force

[12] We concentrate on the Jewish population for two main reasons: first, because of the low labor force participation rates among female Arabs (the LFP was lower than 15 percent among Arab women aged 55–64 during our sample period). Second, because parts of the Arab population are characterized by a high percentage of polygamy (for example, about 30% of Arab Beduin families are estimated to be polygamist, Abuhazira 2010).

participation characteristics in 1995 (full/partial/unemployment, salaried employee or self-employed worker), individual and household income from 1995 (wage income, allowances and pension payments and income from other sources).

We match this sample of individuals with their spouses in 2001, 2007 and 2014 according to the population registries from 2001, 2007, and 2014. For each individual, the population registries provide their spouse as of that year, number of children, spouse's and children's place and date of birth (year and month), an indicator for immigrating from Israel (year and month of immigration) and an indicator for passing away (year and month of death).

We then match, for each individual in our sample, several additional variables from the Israel Tax Authority data for every year during 2002–11, also provided by the Central Bureau of Statistics. The tax data provides the labor force participation information of each individual, based on taxable income from work. This information includes annual salaries, monthly employment status, the two main employment positions[13] and number of employers.

We focus on individuals who were married according to at least one of the population registries (2001, 2007 or 2014). For each couple we have the demographic information of each spouse, individual and household income from the 1995 census for those who completed the full questionnaire, and each spouse's labor force participation information during the years 2001–14.

4. Estimation Strategy

[13] If an employee had more than two employment positions, the two with the highest annual salary were used.

We examine how changes in one's individual retirement age criteria, their spouse's eligibility change and changes for both the individual and their spouse affect one's employment rate. Our estimation strategy assumes that by comparing couples with a similar age structure, within a sufficiently narrowly defined set of birth cohorts (i.e., within two years before and after the birth cohort for which the law was implemented), the probability of each spouse in a specific month to be assigned to treatment versus control groups is as good as random, so that their characteristics are not correlated with the outcomes of interest. Thus, the differences between spouses' probability of working in a specific month among those assigned to control versus treatment groups result solely from the effect of the change in legislation.

We construct treatment and control groups for both genders in the following manner. We restrict the sample to spouses in the age interval for which the official age of retirement was deferred, i.e., women aged 60 and 61 and men aged 65 and 66, two years before and after the implementation of the reform (i.e., between the years 2002-2011). The implementation of the reform was gradual, i.e., the age of retirement was raised for men and women by four months, six times between 2004 and 2009. Therefore, we define six age groups for men and women, according to their different retirement ages. Each group is divided into a treatment group and a control group, according to individuals' date of birth. The treatment groups include individuals who were affected by the legislation—that is, all the individuals, at these ages, whose date of birth is later than the threshold date for which the new retirement age went into effect. The control groups include all individuals, at these ages, born earlier than that date. For example, the retirement age was raised at first in March 2004 by four months (from age 60 to 60 and four months) for women born after March 1944. Therefore, the first group of women consists of those aged 60 to age 60 and four months. The treatment group for this age group consists of those born up to two years after the threshold date for which the new retirement age went into effect (March 1944), while the control group for this group consists of those born in the two years before that date. Table 1 defines the exact dates of the gradual deferral of the retirement ages and the definition of each treatment and control group for each one of the six cohorts, for men and women, respectively. Figure 1 presents a graphical description of these groups.

We exploit the fact that given the couple's age structure the assignment of each spouse to control and treatment groups in a given month is as good as random, to analyze the effect on employment as a result of changes to individual retirement age, changes to spousal retirement age, and whether the effect is different if both the individual and their spouse have experienced changes in their retirement ages. We note that the gradual implementation of the law also enables us to isolate the effect of retirement age deferral from other changes that took place throughout this period by including year fixed effects in the regression.

Specifically, we consider the following benchmark estimation strategy, separately for men and women:

$$(1)\ y_{ijt} = \alpha + \alpha_1 T_{it} + \alpha_2 T_{jt} + \alpha_3 T_{it} * T_{jt} + \beta_1 A_{it} + \beta_2 A_{jt} + \gamma_t + \delta X_{ij} + \varepsilon_{ijt}$$

where y_{ijt} is an indicator equal to one if individual i (with spouse j) is observed working in month t; T_{it} is a dummy variable that equals one if individual i in period t belongs to the treatment group, with treatment defined as being in the age range during that period that is under the official retirement age (see Table 1). T_{jt} is a dummy variable that equals one if his/her spouse j in period t belongs to the treatment group.[14] The baseline specification includes year (γ_t) fixed effects and controls for the individual's and his/her spouse's ages (at a monthly level) (A_{it} *and* A_{jt} **respectively**). We further include individual and spouse characteristics (X_{ij}). ε_{ijt} is an error term clustered within the spouse's year of birth interaction.

This specification enables the individual's employment probability to be affected by his/her own retirement age deferral (α_1); his/her spouse's retirement age deferral (α_2); and allows the effect of his/her own retirement age deferral to be differentially affected based on his/her spouse's retirement age status (α_3). α_3 estimates by how much an individual's employment probability is affected by his or her own retirement age deferral when their spouse's retirement age was deferred, compared to a case when it was not. Moreover, we will compare the effects on employment of men and women from this model to two additional models: the first is the most simplified version, where the spouse effect is not incorporated and does not affect own employment rate, and the second is the case where both spouses' retirement age deferrals are incorporated, but their interaction is not.

[14] We note that this estimation strategy might underestimate the effect of the reform due to the control group consisting of individuals who may have been also affected to some extent by the reform. For example, the probability of working among a woman who is over 61 might increase as a result of deferring her retirement age from 60 to 61. However, this woman will be in the control group when she is over 61, as she is past the retirement age.

4.1 Summary Statistics and Balance Tests

The main analysis consists of two datasets, one for men and their wives and the other for women and their husbands, at a monthly level. The characteristics of individuals and their spouses in both datasets are presented in Table 2.[15] Since the data consist of couples, the characteristics of men (women) are similar to the characteristics of husbands (wives) in the sample (the small difference might reflect changes in the marital status of some of the couples).

In order to examine whether the assignment of individuals and their spouses in a specific month, controlling for their ages, to the control versus treatment groups is as good as random, we run a series of balancing tests. Each regression includes one of 16 individual and spouse characteristics as a dependent variable and the probability of being assigned to the treatment group in a specific month as the explanatory variable. In addition, each regression controls for year fixed effects and the ages of the spouses (at a monthly level). Individual and spouse characteristics are: both spouses' education levels, ethnicities (four ethnicity groups according to place of birth of each spouse), number of children, dummy for studying in a religious school, dummy for new immigrant (for both spouses), and household income and employment status in 1995. Table 3 presents the results of these balancing tests for the assignment of individuals in a specific month to the treatment group. Except for two cases, none of the 32 estimated coefficients in the table are significantly different from zero, indicating that the characteristics of individuals and their spouses in a specific month that were assigned to the control group are similar to the characteristics of individuals and their spouses that were assigned to the treatment group, when comparing couples with the same age structure.

5. Results

The main findings are presented in Table 4. The table presents the effects on an individual's probability of working in response to own retirement age deferral, to spouse's retirement age deferral and their interaction, separately for men and women. The table presents the

[15] Appendix Table A1 presents the number of observations (at a monthly level and individual level) of couples belonging to treatment versus control groups (without counting cases where the same individual belongs to both treatment and control groups).

coefficient estimates in Equation 1, according to four different specifications. The first specification includes only year fixed effects, the second specification includes the ages of the couples as well (at a monthly level); and the third specification, which is our baseline specification, includes additional couples' characteristics. The fourth specification includes an additional control: spouse’s year of birth interaction fixed effects.

The table presents the estimated effects of the model specified in Equation 1. Columns 1-3 present the estimates of own retirement age deferral (column 1), spouse's retirement age deferral (column 2) and its interaction effect (column 3) for men, based on the four specifications, and columns 4-6 present the respective estimates for women. In the first specification, which includes only year fixed effects, the estimated effects of own retirement age deferral on the probability of working are positive and significant for both women and men, the estimates of spouse retirement age deferral are also positive but statistically different from zero only for women, and the estimates of the interaction terms are negative but again statistically significant only for women. These results do not change dramatically when spouses' ages are added to the regression (second row), though the estimated coefficients decrease slightly in all regressions. Adding spouses' characteristics leaves the estimates almost unchanged. The fourth specification, with spouse’s year of birth interaction fixed effects, slightly decreases the estimates of men, while slightly increasing those of women.[16]

The estimated effects of men's own retirement age deferral on their probability of working are higher than that of women's in both the baseline specification (0.081, SE=0.013 versus 0.56, SE=0.011) and the spouse’s year of birth interaction fixed effects specification (0.067, SE=0.026 versus 0.06, SE=0.016). Moreover, while men's employment status does not depend on spouse's retirement age deferral, women's probability of working does depend on that of their husbands.[17] The estimated effects of husbands' retirement age deferral and the

[16] Appendix Table A2 presents results from two additional robustness checks: In the first row, the specification includes the characteristics of both spouses including their ages, year fixed effects and both spouses' years of birth fixed effects instead of the interaction between the spouse’s year of birth interaction fixed effects; and in the second row, the specification includes year fixed effects, the characteristics of both spouses and the couples' age group cohorts interaction fixed effects. The stability of the estimated coefficients provides evidence that our findings are robust to the specification used.

[17] The gender asymmetry in the effects of the retirement age deferral might be related to gender preference or to lower attachment of women to the labor market, as will be discussed in the next section. We note that this asymmetry might also be driven by the fact that for men the retirement age deferral also deferred the age at which employers can terminate their employment and restricted the possibility of continuing to employ them in the public sector. In order to test to what extent this aspect of the law affected men's employment rate, we use the fact that women of the same ages (aged 65-67) had also experienced the same policy change and replicate the same analysis for women in these cohorts. The estimated effects of own and spouses' retirement rate

interaction term are statistically significant only for women (0.027, SE=0.01 and -0.019, SE=0.011 according to the baseline specification and 0.046, SE=0.016 and -0.048, SE=0.022 according to the spouse's year of birth interaction fixed effects specification). Thus, husbands' retirement age deferral has a positive effect on women's employment rate according to both specifications. But deferring both spouses' retirement age does not significantly increase women's employment rate relative to the case where only her own retirement age is postponed (0.064 ,SE=0.014 compared to 0.056, SE=0.011, according to the baseline specification, where the p-value of the difference is 0.417, and 0.06 ,SE=0.016 compared to 0.058 ,SE=0.027, according to the spouse's year of birth interaction fixed effects specification, where the p-value of the difference is 0.93).[18]

To illustrate the magnitude of the estimated effects, we simulate the changes of retirement age deferral of both spouses on women's and men's employment rates. The employment rates in the pre-reform period (year 2003/2004) for men aged 65 to 67 and women aged 60 to 62 were 29 percent and 33 percent, respectively. Increasing own retirement age raises the employment rate of men by 7 or 8 percentage points depending on the specification, from 29 percent to 36 or 37, regardless of their spouses' retirement age deferral. For women, the size of the effect of own retirement age on employment rates depends on their spouse's retirement age deferral. Their employment rate increases from 33 percent to 39 percent due to only their own retirement age deferral; it increases from 33 percent to 36 percent due to only their spouses' retirement age deferral (or to 38 percent according to spouse's year of birth interaction fixed effects specification). However, there is no added benefit from both spouses experiencing an increase in retirement age relative to the case where only women are experiencing it (their employment rate increases also from 33 percent to 39 percent).

This result is consistent with the descriptive statistics evidence, discussed in the literature, on coordination of retirement decisions among spouses. Theoretical models stress the possible contradicting channels that create spillover effects of one spouse's retirement age deferral to

deferrals and their interaction on the probability of working among women aged 65-67 are all not statistically significant according to both baseline and spouse's year of birth interaction fixed effects specifications implying that this aspect of the law was not the main driver of our results (the respective estimates are 0.013 SE=0.008, -0.001 SE=0.016 and -0.002 SE=0.014 according to the baseline specification, and -0.008 SE=0.013, -0.003 SE=0.022 and -0.005 SE=0.019 according to the spouse's year of birth interaction fixed effects specification).

[18] Replicating the analysis for the probability of being retired instead of the probability of working yield similar opposite results (although, for women, the interaction term is no longer significant), as presented in Appendix Table A3, since the correlation between the probability of being retired and the probability of working is about -0.92 for both genders (retiring is defined as the last month in which the individual worked for at least two months in a given year).

the other spouse's probability of working: the leisure complementarity channel predicts that raising a spouse's retirement age would decrease his/her leisure time which in turn would increase the willingness of the other spouse to continue working if spouses' leisure is complementary. In contrast, the income channel predicts the opposite effect. It forecasts that raising a spouse's retirement age would increase this spouse's probability of working and increase household income, thereby reducing the need for the other spouse to continue working as well. Our result implies that, for women, the leisure complementarity channel might be stronger creating a positive correlation between spouses' retirement decisions. This results from the positive and significant spillover effects of husbands' retirement age deferral on their wives' probability of working, though this effect is present only when their own retirement age is not postponed. If their own retirement age is postponed, there is a small and positive spillover effect of husbands' retirement age deferral but it is not statistically significant (according to the baseline specification, and no spillover effect according to the spouse’s year of birth interaction fixed effects specification), implying that there exists an upper threshold above which women will not increase their employment rates further due to the retirement age reform.

To the best of our knowledge, we are the first to explore possible interactions between both spouses’ retirement age deferrals. Other papers examine either how individuals are affected by own retirement age deferral or by their spouse's own retirement age deferral, and a few examine both but do not include the interaction term. In order to stress the importance of including this interaction term as well as the implication of the overall effect of a spouse's retirement age deferral on the other spouse’s employment rate, we compare our results to two simplified versions of our model: a version where the interaction term is not incorporated in the model, i.e., assuming that the effect of own retirement age deferral on own employment is the same regardless of spouses' retirement age deferral; and another version where no spouses' effects are incorporated at all. These results are presented in Table 5. This table presents the same main specifications presented in Table 4 (baseline and spouse’s year of birth interaction fixed effects specifications). Columns 1 and 4 present estimates of a version where only retirement age deferral is included, for men and women respectively. Columns 2–3 and 5–6 present similar estimates of a version where own and spouse retirement age deferrals are both included, but without the interaction term, again for men and women, respectively.[19]

Comparing the estimates of the different versions of the model reveals that the estimated effects of own retirement age deferral do not change by much when including spouse's retirement age deferral, according to both baseline and spouse's year of birth interaction fixed effects specifications (the effect of own retirement age deferral are: 0.083, SE=0.015 and 0.074, SE=0.021 for men and 0.046, SE=0.011 and 0.033, SE=0.014 for women in Table 5, versus 0.081, SE=0.013 and 0.067, SE=0.026 for men and 0.056, SE=0.011 and 0.06, SE=0.016 for women in Table 4, respectively). This is also true when comparing own and spouse's estimates of men from this table (columns 2 and 3) to the respective estimates in Table 4 (columns 1 and 2).

For women, however, not including the interaction term lowers by almost half the coefficients of each of the spouses' retirement age deferrals on women's employment rate (the coefficients of own deferral is about 4 compared to 6, and the coefficients of husband's deferral is about 2 compared to 4). The fact that these coefficients of own and spouse retirement age deferral on women's employment rate are both lower when the interaction term is not taken into account results from the dependency of spouse's retirement age status: deferring own retirement age increases women's employment rate by about 6% (no matter if spouse retirement age is deferred or not (treated group), but when it is not deferred it increases by about 4% if their spouses retirement age is deferred (control group). Thus, the difference between women's employment rates in the control versus treated groups is smaller, when spouses' retirement age deferral effect is not taken into account, leading to an overall smaller impact in the simplified versions on the model. Similar results are obtained for the effect of their spouses retirement age deferral: deferring spouse retirement age increases women's employment rate by about 4% when their own retirement age is not deferred and by about 6% otherwise (treated group). When spouse retirement age is not deferred, women's employment rate still increases if own retirement rate is deferred by about 6% (control group). Therefore, smaller difference between control and treated groups are obtained in this case also. Thus, if the interaction term is not taken into account, one would underestimate the expected effect sizes of deferring the retirement age on women. Therefore,

[19] The version without spouse's effects includes as controls only individual's age and year of birth fixed effects. Standard errors are also clustered within an individual's year of birth.

if the 2004 retirement age reform would have deferred only women's retirement age, the effect size on women's employment would have been doubled. Understanding the interdependencies of spouses' retirement age deferrals is therefore necessary for designing better retirement age reform policies and correctly evaluate the consequences of postponing spouses' retirement ages.

The size of the effect of own retirement age deferral is somewhat similar to past estimates in the literature, though it is hard to compare since the features of each reform are different. For example, Atalay and Barrettt (2015) find an approximately 8 percentage points increase in the participation of Australian women due to the increase in their eligibility age for pension benefits from 60 to 65. Cribb, et al. (2013) find that women's employment rates at age 60 increased by 7.3 percentage points when the pension age in the UK was increased to 61. Other studies find stronger effects: Selin (2017) finds a relative decrease of nearly 20 percentage points in the retirement probability of Swedish wives aged 63 as a result of a delay in pension benefits reform, whereas Johnsen and Vaage (2017) find that Norwegian workers of both genders who are eligible for early retirement reduce their employment rates by about 35 percent compared to non-eligible workers. There is also variation in spillover effects, though most studies find higher positive spillover effects of retirement age deferral on wives' employment when both men's and women's retirement ages are postponed (see Lalive and Parrotta, 2017, and Johnsen and Vaage, 2017).

6. Alternative Specifications and Robustness Checks

6.1 Does the increase in employment rates result from year of birth differences between treatment and control groups?

The empirical strategy relies on the comparison of couples' employment rates, controlling for their ages, before and after the reform was implemented. Although analyzing the control versus the treatment groups' characteristics of these couples stresses their similarity (Table 1), it can still be claimed that the fact that the control group consists of individuals born in earlier years may affect the results, as their employment rates might be lower regardless of the implementation of the reform. We address this concern in several ways.

First, in Table 4, column 4, we show that the inclusion of spouse's year of birth interaction fixed effects to the regression changes the results only to a small extent and maintains the relevant coefficient estimates' statistiacl significance. This suggests the results do not depend substantially on birth cohort differences between treatment and control groups. Moreover, it also implies that our findings cannot result from within couples' birth cohort differences between those assigned to treatment and control groups. As noted, the estimated effects of own retirement age deferral remain similar and statistically significant for both men and women (0.081, SE=0.013 versus 0.067, SE=0.026 for men and 0.056, SE=0.011 versus 0.06, SE=0.016 for women, according to the baseline versus spouse's year of birth interaction fixed effects specifications, respectively), while the effect of spouses' retirement age deferral and the interaction term increase to some extent, though continue to be insignificantly different from zero for men and significantly different from zero for women (spouses' estimated effect are 0.027, SE=0.01 versus 0.046, SE=0.016 and the interaction terms are -0.019, SE=0.011 versus -0.048, SE=0.022, according to the baseline versus spouse's year of birth interaction fixed effects specifications respectively).

In order to gain further insights into the dependency of our results on the differences in birth years in control versus treatment groups, we exploit an additional feature of our dataset. For around one quarter of individuals in our sample the exact month of birth does not exist (though the year of birth is given), due to unknown month of birth or inadequate registry by the Population and Immigration Authority.[20] Pension benefits are given to those individuals based on a fixed date (i.e., April 1st). For the purpose of this analysis, in addition to assigning April 1st as their date of birth as in the main analysis, we split the two samples for men and women into two, one consisting of individuals with an adequate birth registry and the other with individuals without it, in order to check the similarity of the retirement age deferral estimated effects based on these two samples. Table 6, columns 1–2 and 3–4 provide evidence of this issue by presenting the estimated effects of own retirement age deferral based on the two stratified subsamples, for both men and women respectively.[21] The regressions include the effect of own retirement age deferral, controlling for individuals' age and other characteristics and year of birth fixed effect. The differences between the estimated effects of own retirement age deferral according to both subsamples are not statistically different from zero, for both men and women. Moreover, the estimated effect for men based on analyzing the subsample of exact birth date individuals is lower than the estimated effect

[20] The sample includes mostly older individuals who immigrated to Israel before or after the founding of the State of Israel (according to the summary statics presented in Table 2, only about 3 percent were born in Israel).

[21] The analysis was not performed on the main version of the model that includes spouses' retirement age deferral effect and the interaction term because there are only a few couples in which both spouses have inadequate birth registration.

in the second subsample (0.063, SE=0.015 and 0.074, SE=0.025 respectively), while the opposite is true for women (0.048, SE=0.002 and 0.035, SE=0.018 respectively). This provides further evidence that the effect of retirement age deferral does not depend on the exact date of birth of individuals.

Finally, we replicate the analysis for smaller time intervals before and after the implementation of the law. We redefined the control and treatment groups to the same age interval for which the official age of retirement was deferred as before—that is, women aged 60 to 62 and men aged 65 to 67, but now we concentrate on a smaller time interval: only one and a half years before and after the implementation of the reform, instead of two years. Appendix Table A4 presents the restricted sample estimated coefficients according to the two last specifications in Table 4, for men and women (i.e. the baseline and the spouse's year of birth interaction fixed effects specifications). Comparing these estimates to the respective estimates in Table 4 reveals that the estimated standard errors are higher due to the smaller interval before and after the treatment, but the estimated coefficients are very similar for both genders. Although the estimated coefficients for women are slightly lower, while being marginally higher for men, all estimated coefficients for women are still statistically significant according to the spouse's year of birth interaction fixed effects specification (for example, the estimates of own retirement age deferral effects based on this specification in Appendix Table A4 are 0.082 SE=0.033 for men and 0.043, SE=0.015 for women and the estimates of spouse's retirement age deferral effect and interaction term for women are 0.048 SE=0.023 and -0.033 SE=0.019 respectively). The similarity of the results provides further support for our identification strategy.

The fact that including spouse's year of birth interaction fixed effects as additional controls only marginally changes the estimated effects and that they stay statistically significant indicates that our results are not driven by differences between control and treated spouses' years of birth. Moreover, we demonstrate that the effect of the retirement age deferral is similar regardless of its dependency on individuals' exact month of birth cohorts. These two robustness checks, as well as deriving similar results based on the restricted sample of individuals with closer years of birth, provide overall evidence that even though our identification strategy relies on comparing individuals from control and treatment groups who experienced a retirement age deferral based on their years of birth, the effect of this dissimilarity on our results hardly exists. We provide further robustness checks in the last section using a double difference-in-differences estimation strategy.

6.2 Heterogeneous Effects

In order to gain further insights into which type of households were more affected by the reform and to learn more about the different mechanisms at place, we explore heterogeneous effects across several dimensions. First, we look at whether the reform affected individuals differently based on their working status ten years before the reform was implemented. We expect the reform to have a stronger effect on individuals who participated in the labor market in the past, as we examine the effect of the reform on their working status at the time the reform took place. Second, we test whether the reform affected individuals differently based on their salary—again, as reported ten years before the implementation of the reform. In this case, it is not clear ex ante which group should be more affected, since the reform decreased the pension paid to low income households mainly through the deferral of means-tested old age allowance, but also decreased pension payments to high income households mainly through the deferral of the age of eligibility for employment-related pension. Finally, we stratify the sample based on levels of education, which provides another approximation for earning capability instead of past monthly wages.

Table 7 presents the estimated effects of three stratifications of the sample: Panel A shows the results of the stratification based on whether both spouses were working in 1995 or not, according to the 1995 Israeli census; Panel B presents the stratification of the sample by high versus low individual income in 1995 (higher or lower than the median salary income); and Panel C displays the results of the reform by individuals' level of education (dummy for highly educated=1 if holding a B.A. degree or higher). The estimates are from both the baseline specification and the spouse's year of birth interaction fixed effects specification, separately for men and women.

The heterogeneous estimates by couples' working status suggest that, as expected, the effect of the reform on employment rates is largely driven by couples who participated in the labor force ten years before the reform was implemented. Most of the estimated effects of retirement age deferral that are statistically significant according to Table 4 are also significant for the working couple sub-sample and are even greater than those of the non-working sub-samples, especially according to the spouse's year of birth interaction fixed effects specification (according to this specification, the effects of own retirement age deferral, spouse's retirement age deferral, and the interaction term are 0.1, SE=0.02 versus 0.006, SE=0.02; 0.062, SE=0.031 versus 0.025, SE=0.032; and -0.077, SE=0.032 versus -

0.008, SE=0.048, respectively, for women; and for men, the respective estimates are 0.077, SE=0.031 versus 0.06, SE=0.042; 0.036, SE=0.016 versus -0.019, SE=0.03; and the interaction terms are not statistically significant.)

The results of the second stratification in Panel B are similar to those in Panel A, when stratifying the sample by income level, though the high-income level subsample is smaller than that of the working couple subsample. Thus, the effects are mostly driven not only by working couples, but by working couples from the top of the income distribution, especially for women since the estimated effect differences for men are marginal (for example, according to the spouse's year of birth interaction fixed effects specification, the estimates for own, spouse and interaction term for women are: 0.086, SE=0.037 versus 0.026, SE=0.039; 0.087, SE=0.034 versus -0.006, SE=0.016; and -0.09, SE=0.055 versus 0.015, SE=0.037). The fact that the reform affected mostly high-income individuals might imply, for example, that pension benefits for the low-income earners constitute a lower percentage of total earning compared to alternative transfer benefits when unemployed, or that the demand side is more binding for low-income individuals preventing them from staying in the labor force at older ages, or possibly that the low-income earners were the ones working beyond retirement age to begin with.

Panel C presents the estimates based on stratifying the sample by level of education. Since we use a narrow definition of high education—holding a B.A. degree or higher—the sample of individuals with high education constitutes only a small subsample of individuals. Since the level of education is an additional measure of individuals' earning capacity, we expect the results to be similar as in Panel B. Indeed, we find that for both genders, highly educated individuals are more influenced by the postponement of the retirement age.

Surprisingly, we find now evidence of a spousal effect when evaluating males with both spouses working in 1995 or highly educated (according to the spouse's year of birth interaction fixed effects specification, the estimates are 0.036, SE=0.016 and 0.178, SE=0.038 respectively). We interpret this as evidence that the gender asymmetry of the baseline results are not driven by differential gender preferences for joint couple leisure but rather by financial constraints that limit the husband's response to his wife's labor force participation status among low-income couples.

6.3 Heterogeneous Age Cohort Effects

We now test whether the retirement age deferral affected individuals differently according to their age cohorts. Thus, instead of imposing the same treatment effect on all age cohorts as in the analysis so far and referring to the impact of the average treatment of the reform, we now examine non-linear effects. Due to the gradual implementation of the reform, two contradicting types of effects are expected over time. We would expect a gradual assimilation to the reform, with the highest effect toward the end of the period, since the reform was not anticipated. Yet in contrast, as the reform was gradually implemented affecting older age cohorts at later stages, we would expect that the effect of retirement age deferral on the employment rate to be smaller as an individual becomes older.

Table 8 presents the effects of postponing the retirement age on the different age cohorts of the sample, gradually adding age cohorts one at a time (the different age cohorts are defined in Table 1). Panel A presents the effect of retirement age deferral only on the first two age cohorts (men/women aged 65/60 to 65/60 and 8 months); Panel B presents similarly the effect on the first three age cohorts (men/women aged 65/60 to 66/61); Panel C presents similarly the effect on the first four age cohorts (men/women aged 65/60 to 66/61 and 4 months); and Panel D presents similarly the effect on the first five age cohorts (men/women aged 65/60 to 66/61 and 8 months). The estimated effects are presented for the baseline specification and on a specification that includes, in addition, spouse's year of birth interaction fixed effects.

Restricting the sample to the first two age cohort groups reveals the highest effect. The effect fades out when adding older age cohorts to the sample. For example, the first row estimated effects according to the baseline specification, which includes a sub-sample of men aged 65 to 65 and 8 months and their wives of age 60 to 60 and 8 months (or women in the same ages and their husbands), reveals that the effect size doubles or more compared to the estimated effects from the baseline specification where all individuals are included in the sample, as presented in the third row of Table 4 (the estimated effects of own retirement age deferral are 0.119, SE=0.034 versus 0.081, SE=0.013 for men and 0.117, SE=0.036 versus 0.056, SE=0.011 for women; the respective estimated effects of spouse retirement age deferral are 0.062, SE=0.027 versus 0.001, SE=0.011 for men and 0.06, SE=0.034 versus 0.027, SE=0.01 for women; and the estimated effects of the interaction term are -0.133, SE=0.027 versus 0.002, SE=0.017 for men and -0.082, SE=0.036 versus -0.019, SE=0.011 for women). This means that even though individuals were able to assimilate to the new law gradually, later cohorts were less affected by the reform possibly due to the fact that these cohorts were also older, with less probability of working due to their retirement age being postponed.[22] Moreover, for younger cohorts, the estimated effects of own and spouses' retirement age deferrals on individual's employment and their interactions are more similar across genders than for older cohorts.

6.4 Additional Estimation Strategies

We now check the consistency of the results by comparing them to the results from two other specifications.

The first, is a regression discontinuity design measuring the effect of the reform separately for each gender. Using similar definition of treatment and control groups we additionally define a running variable which counts the number of months elapsed between the cutoff and the individual birth month. Assuming a linear model, we estimated the following model:[23]

$$(1)\ y_{it} = \alpha + \alpha_1 T_{it} + \alpha_2 M_i * T_{it} + \alpha_3 M_i * (1 - T_{it}) + \gamma_t + \delta X_i + \varepsilon_{it}$$

[22] As noted in footnote 14, this could also result from the fact that later control groups consist of individuals who may have been also affected to some extent by the reform.

[23] The underlying assumption is as before that the reform was not anticipated. Appendix Figure A1 reports the McCrary test results for the continuity of the running variable.

Where M_i is the running variable that measures the difference between the individual birth month and the date of retirement age deferral (see Table 1); T_i is as before (equation 1), the dummy variable that equals one if individual i in month t is below his/her statutory age of retirement; γ_t is year fixed effects; $X_i$is individual characteristics and ε_{it} is an error term.

The results of this alternative strategy are very similar to that of the baseline estimations which include the effect of only own retirement age deferral (Table 5). The effect of own retirement age deferral increase men's probability to work by 0.068 (SE=0.003) compared to 0.08 (SE=0.015) and 0.074 (SE=0.021) and women's probability to work by 0.033 (SE=0.042) compared to 0.046 (SE=0.011) and 0.033 (SE=0.014) according to baseline specification and the specification that includes, in addition, spouse's year of birth interaction fixed effects respectively.[24] Figures 2 presents the graphical analysis.

In addition, we employ a double difference-in-differences (DID) analysis that evaluates different age groups and genders before and after the reform and varies their treatment status based on their own birthdate and their spouse's birthdate. We use this empirical strategy for several reasons: first, it enables us to test the effect of the reform on additional labor market variables defined annually; second, the double difference-in-differences specification makes it possible to examine anticipation effects of the reform on younger cohorts; and finally, it enables us to carry out two additional robustness checks in order to validate our identifying assumptions.

In the baseline double difference-in-differences (DID) analysis, we estimate the effect of the reform by comparing the changes in labor force outcomes before and after the implementation of the reform of spouses affected by the reform, belonging to the treatment group (i.e., men aged 63–66 and women aged 58–61), to those not affected by the reform, belonging to the control group (i.e., men aged 67–70 and women aged 62–65).

Within this framework, we estimate the change in own and spouses' retirement age deferral effects and their interaction on several labor force characteristics: the mean number of working months in a year, annual salary of all individuals, annual salary of workers and the

[24] We implemented the procedure of Calonico et al (2014), to produce the optimal bandwidth intervals for men and women (36.8 and 13.4 respectively). We choose to present results using the same bandwidth of 24 months for both genders, although results are robust to using the optimal individual as well. The linear regression model is non-parametrically fitted using a uniform kernel. The standard errors are robust and are also clustered at the individual level.

probability of keeping the same job within a given year. Since these variables (except the probability of working in a certain month that we aggregate within a year) are given annually, our dataset is at the individual by-year level. Separate regressions are estimated for men and women.

The double difference-in-differences estimation strategy assumes that the control group is a valid counterfactual to the treatment group in order to estimate the effect of the reform, if both groups have similar pre-reform trends in our no as in equation (1), and are estimated separately for men (columns 1–4) and women (columns 5–8). All the differences between the two groups' pre-reform time trends for the years 2001–04 are small and insignificant for each of the four outcomes.

To compare husbands' labor force variables at age 63–70 (wives at age 58–65) between a period before the reform (i.e., 2003–04) and a period after the reform (i.e., 2009–10), we consider the following double difference-in-differences specification, separately for men and women:

(3)

$$y_{ijt} = \alpha_0 + \alpha_1 T_{it} + \alpha_2 T_{jt} + \alpha_3 T_{it} * T_{jt} + \alpha_4 R_t + \alpha_5 R_t * T_{it} + \alpha_6 R_t * T_{jt} + \alpha_7 T_{it} * T_{jt} * R_t + \rho X_{ij} + \varepsilon_{ijt}$$

The indicator variable T_{it}– is equal to one if individual i in year t belongs to the treatment group, i.e., men aged 63–66 and women aged 58–61; and the indicator variable T_{jt}– is equal to one if his/her spouse j in year t belongs to the treatment group. We interact each of these variables with the R_t indicator variable which equals one for the period after the reform (i.e., 2009–10). In order to estimate the effect of either being individually treated or having a treated spouse, we obtain two of our coefficients of interest: α_5 and α_6. We also interact the R_t indicator with the interaction between T_{it} and T_{jt} and obtain our third coefficient of interest: α_7, the added effect in case of joint retirement age deferral changes. The couples' characteristics are denoted by X_{ij} (the characteristics are the same as in Equation 1). The error term ε_{ijt} is clustered at the couple's birth years interaction.

The results of the estimation of Equation 2 are presented in Table 9. The estimated effects of own and spouse retirement age deferral interactions with post-reform dummy variable (α_5

and α_6, respectively) and their interaction (α_7) are presented in columns 1, 2 and 3 for men, and in columns 4, 5 and 6 for women, respectively. Panel A presents the estimated effects on the mean number of working months in a year; Panel B presents similar estimated effects on annual salary of all individuals (the salary of those who did not work was set to zero); Panel C presents similar estimated effects on annual salary of workers; and Panel D presents similar estimated effects on the probability of keeping the same job all year.

Overall, the estimated effects of retirement age deferral variables interacted with the post-reform dummy on all four outcomes in Table 9 have similar signs as the estimated effects reported in Table 4, but are less statistically significant. This might result from the fact that while the baseline identification strategy estimates the effects of the reform specifically on the cohorts while they are directly affected by it, the double difference-in-differences estimation strategy captures its effects on a wider range of age cohorts, including younger ones.[25] The estimated effects of own retirement age deferral's interaction with the post-reform dummy are all positive and statistically significant for men (the estimated effects of own retirement age deferrals on the mean yearly working months for men is: 0.086, SE=0.021; on annual salary of all individuals: 31,375, SE=5,948; on annual salary of workers: 14,823, SE=6,620; and on the probability of keeping the same job: 0.036, SE=0.01). For women, the estimated effects of own and spouse's retirement age deferrals interacted with the post-reform dummy and the interaction term of both spouses' retirement age deferrals interacted with the post-reform dummy follow a pattern similar to that in Table 4, but only a few of them are statistically significant.

We simulated the effect size of postponing the retirement age on men's mean working months per year, from 5.14 to 6.18 months per year. The annual salary of all individuals increased from NIS 64,300 to NIS 95,600; the annual salary of workers also increased from NIS 97,300 to NIS 112,100; and the probability of keeping the same job among those who continued working increased from 93 percent to 96.6 percent as a result of postponing their retirement age. For women, annual salary of all individuals increased to a smaller extent but it does not depend solely on own retirement age deferral. Women's annual salary increased from approximately NIS 36,000 to NIS 45,000, whether only their retirement age was postponed;

[25] We note that as in the baseline estimation strategy, the effect of the reform might be underestimated due to the control group consisting of individuals who may have been also affected to some extent by the reform.

only their spouses' retirement age was postponed; or both spouses' retirement ages were postponed.

The double difference-in-differences specification enables us to examine anticipation effects of the reform on younger cohorts. Appendix Table A11 replicates the analysis presented in Table 9, for younger cohorts treated groups: men aged 61–64 and women aged 56–59.[26] A female planning to retire at age 57 when the retirement age was 60 (or her husband's retirement age was 65) may postpone her retirement to age 60 following an increase in her own retirement age (or her husband's retirement age). Evaluating females solely at ages 58–61 will thus not capture the effects of the reform on younger cohorts. Indeed, Appendix Table A11 reports that the estimated effect of the reform on younger cohorts has similar patterns as for the older cohorts (Table 9) though most of the sizes of the effects are marginally lower (for example, for men, the sizes of the effect of own retirement age deferral on all four outcomes for younger cohorts are: 0.059, SE=0.016, 28,477, SE=5,311, 9,539, SE=6,690 and 0.0212, SE=0.01, respectively, and for older cohorts the sizes of the effects are: 0.0865, SE=0.021, 31,375, SE=5,948, 14,823, SE=6,620 and 0.0365, SE=0.01, respectively).

We note that we can utilize this additional estimation strategy to test whether the same results on spouses' employment rates are obtained using two different estimation strategies with different underlying assumptions. The baseline identification strategy assumes that by comparing couples with a similar age structure, within a sufficiently narrowly defined set of birth cohorts, the differences between spouses' probability of working in a specific month among those assigned to control versus treatment groups results solely from the effect of the change in legislation. In contrast, the double difference-in-differences estimation strategy assumes that comparing treatment to control groups' differences in spouses' mean working months per year before and after the reform results solely from the effect of the reform, if their pre-reform trends are similar.

Indeed, comparing the sizes of the effects on the monthly probability of working according to the baseline specification (Table 4) to the mean yearly working months according to the baseline double difference-in-differences approach (Table 9, Panel A) reveals that the

[26] Appendix Table A8 and Appendix Table A9 present the number of observations and the summary statistics of couples' characteristics in pre-reform and post-reform groups. Appendix Table A10 replicates also the pre-reform trend analysis for these younger cohorts. Similar to the conclusions derived for older cohorts, we find that the treatment and control groups have in almost all cases the same pre-reform time trends in all four outcomes.

coefficients are of similar magnitude.[27] Simulating the effect size of postponing the retirement age increases men's probability of working from 0.287 to 0.371 percent, meaning by 8.4 percentage points based on the double regression discontinuity approach (baseline specification). According to the double difference-in-differences approach, it increases men's mean working months per year by almost one month, meaning men's probability of working rise by 8.65 percentage points.[28] The same calculations for women also yield similar sizes of the effects, but the estimates of the double difference-in-differences approach are not statistically significant (the respective estimated effects of own retirement age deferral according to both tables are 5.6 versus 3 percentage points; the estimated effects of spouse retirement age deferral are 2.7 versus 1.23 percentage points; and the estimated effects of the interaction term are -1.9 versus -2.1 percentage points).

As an additional robustness check to further test the validity of the double difference-in-differences estimation strategy's assumption, we carry out a placebo analysis. We replicate the analysis presented in Table 9 for couples before the reform was implemented. Our treatment and control groups include, as before, four age cohorts each: our treatment group includes men aged 63–66 and women aged 58–61, and our control group includes men aged 67–70 and women aged 62–65. However, now the "pre-reform" and "post-reform" periods both cover years before the reform actually took place. This placebo analysis tests whether the results of our baseline double difference-in-difference estimation can be derived solely from control versus treatment groups' labor market outcomes differences over time due to their different age cohorts, regardless of the reform's implementation. The estimates of this placebo analysis, presented in Appendix Table A12, reveal that the two groups' labor market outcome differences between two periods before the reform was implemented are indeed not statistically different from each other.

To conclude, using the double difference-in-differences estimation strategy we showed that the reform affected not only spouses' employment rates but also their annual salaries and job stability. In addition, this estimation strategy enabled us to test the anticipation effects of the reform also on younger cohorts, revealing that they were indeed affected by the reform, though to a lesser extent. Moreover, we used this estimation strategy for two additional

[27] As noted, the main differences between these two estimation strategies' results might be driven by the fact that the double difference-in-differences estimates the total effect of the reform instead of the average effect and includes in the treatment group age cohorts which are not directly affected by the reform.
[28] We note that this calculation assumes that the monthly probabilities of working are independent.

robustness checks. We showed that the baseline identification strategy and double difference-in-differences identification strategies yield overall similar patterns and are of comparable effect sizes when analyzing the work status case, providing additional evidence that our results capture the causal effect of the reform and do not depend on the identification strategy utilized. We also perform a placebo test in order to reject the alternative interpretation that our results could be derived only by control versus treatment groups' age differences regardless of the reform's implementation

6. Conclusion

The challenge of balancing sustainability of social security systems and providing an adequate income in retirement is expected to grow and become more pronounced in many countries in the coming years. Since most couples nearing retirement age are dual-earners, it is important to understand the implication of pension reforms at a household level, rather than the individual level. In this paper, we analyze couples' joint retirement decisions as a result of their retirement age deferrals.

We exploit a reform in Israel that gradually increased both males' and females' retirement age by two years, based on specific months of birth. Our baseline identification strategy relies on comparing employment rates of couples with the same age structure, before and after the reform was implemented. Our results show that increasing own retirement age raises the employment rate of men by 7 to 8 percentage points. For women, the effect size of own retirement age on employment rates depends on their husband's retirement age deferral. Their employment rate increases by about 6 percentage points due to only their own retirement age deferral, and it increases by 3 to 5 percentage points due to only their husband's retirement age deferral. However, deferring both spouses' retirement ages will not further increase women's labor supply relative to the case where only their own retirement age is postponed.

We show that when the interaction between both spouses' retirement age deferrals is omitted from the regression specification, the model predicts that a husband's retirement age increase will incrementally increases the wife's labor force participation. However, if the regression specification includes the interaction, then the husband's retirement age effect is only present as long as the wife's legal retirement age is not raised. Our results thus demonstrate not only the importance of considering spousal effects of changes in the retirement age but also the need to specify the interaction between them correctly and accounting for differential affects based on whether one spouse or both spouses experience a change in their retirement age.

Lastly, we also analyze heterogeneous effects of the reform over several dimensions, such as individuals' ages, education levels and prior employment status. We find that the effect of the reform is stronger among younger, more highly paid and more educated individuals, thereby suggesting that the gender differences are not driven by differential preferences for joint couple leisure based on leisure but rather by financial constraints. Moreover, we show that the reform affects in similar ways not only spouses' probability of working, but also several additional labor market outcomes, such as annual salary and the probability of keeping the same job throughout the year.

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Table 1: Definition of Treatment and Control Groups of Men and Women

	New Statutory Retirement Age		Treatment Group		Control Group	
	Retirement Age	**Birthdates**	**Age in the Sample**	**Birthdates**	**Age in the Sample**	**Birthdates**
	(1)	(2)	(3)	(4)	(5)	(6)
A. Men						
First Age Group	65.04	[3/1939,8/1939)	65 - 65.04	[3/1939,3/1941)	65 - 65.04	[3/1937,3/1939)
Second Age Group	65.08	[9/1939,4/1940)	65.04 - 65.08	[9/1939,9/1941)	65.04 - 65.08	[9/1937,9/1939)
Third Age Group	66	[5/1940,12/1940)	65.08 - 66	[5/1940,5/1942)	65.08 - 66	[5/1938,5/1940)
Fourth Age Group	66.04	[1/1941,8/1941)	66 - 66.04	[1/1941,1/1943)	66 - 66.04	[1/1939,1/1941)
Fifth Age Group	66.08	[9/1941,4/1942)	66.04 - 66.08	[9/1941,9/1943)	66.04 - 66.08	[9/1939,9/1941)
Sixth Group	67	[5/1942, .)	66.08 - 67	[5/1942,5/1944)	66.08 - 67	[5/1940,5/1942)
B. Women						
First Age Group	60.04	[3/1944,8/1944)	60 - 60.04	[3/1944,3/1946)	60 - 60.04	[3/1942,3/1944)
Second Age Group	60.08	[9/1944,4/1945)	60.04 - 60.08	[9/1944,9/1946)	60.04 - 60.08	[9/1942,9/1944)
Third Age Group	61	[5/1945,12/1945)	60.08 - 61	[5/1945,5/1947)	60.08 - 61	[5/1943,5/1945)
Fourth Age Group	61.04	[1/1946,8/1946)	61 - 61.04	[1/1946,1/1948)	61 - 61.04	[1/1944,1/1946)
Fifth Age Group	61.08	[9/1946,9/1947)	61.04 - 61.08	[9/1946,9/1948)	61.04 - 61.08	[9/1944,9/1946)
Sixth Age Group	62	[5/1947, .)	61.08 - 62	[5/1947,5/1949)	61.08 - 62	[5/1945,5/1947)

Notes: The table presents the definition of treatment and control groups, of men (Panel A) and women (Panel B). The first two columns present the new statutory retirement ages (column 1) for the different birth cohorts (column 2). Since the implementation of the reform was gradual, six age groups of men and women were defined according to their different retirement ages deferrals (columns 3 and 5). The treatment groups include all individuals at these ages, whose date of birth is up to two years later than the date for which the new retirement age went into effect (column 4). The control groups include all the individuals at these ages born up to two years prior to that date (column 6).

Table 2: Summary Statistics of Individuals and their Spouses, by Gender

	Men	Women
	(1)	(2)
Number of Children	3.264	3.179
	(1.825)	(1.811)
High Education	0.222	0.182
	(0.416)	(0.386)
Asian Ethnicity	0.267	0.242
	(0.442)	(0.428)
African Ethnicity	0.211	0.210
	(0.408)	(0.407)
European/American Ethnicity	0.491	0.523
	(0.500)	(0.499)
Israeli Ethnicity	0.031	0.025
	(0.173)	(0.016)
New Immigrant	0.069	0.068
	(0.253)	(0.252)
Religiosity (Religious Studies=1)	0.016	0.001
	(0.127)	(0.025)
High Education Spouse	0.179	0.226
	(0.384)	(0.418)
Spouse of Asian Ethnicity	0.242	0.272
	(0.428)	(0.445)
Spouse of African Ethnicity	0.211	0.207
	(0.408)	(0.405)
Spouse of European/American Ethnicity	0.521	0.491
	(0.499)	(0.500)
Spouse of Israeli Ethnicity	0.026	0.031
	(0.159)	(0.173)
New Immigrant Spouse	0.079	0.079
	(0.269)	(0.270)
Household Income in 1995	11384	11398
	(15344)	(15009)
Work Status in 1995 (Employed==1)	0.874	0.716
	(0.332)	(0.451)
Number of Observations	3477	3285

Notes: The table presents the characteristics of individuals and their spouses, for the two datasets of men and women. Higher education is a dummy that equals 1 if holding a B.A. degree or higher. New immigrant is a dummy that equals 1 if the individual immigrated to Israel after 1990. Household income consists of wage income, allowances and pension payments and income from other sources in 1995. Working status equals 1 if the individual was employed in 1995. Standard deviations are reported in parentheses.

Table 3: Balancing Tests for the Assignments of Individuals and their Spouses in a Specific Month to the Treatment Group, by Gender

	Men (1)	Women (2)
Number of Children	-0.061	-0.152
	(0.054)	(0.053)
High Education	0.021	0.007
	(0.014)	(0.013)
Asian Ethnicity	-0.016	-0.020
	(0.026)	(0.016)
African Ethnicity	0.011	-0.019
	(0.009)	(0.013)
European/American Ethnicity	0.011	0.042
	(0.030)	(0.023)
Israeli Ethnicity	-0.006	-0.002
	(0.006)	(0.004)
New Immigrant	0.005	0.008
	(0.008)	(0.009)
Religiosity (Religious Studies=1)	-0.001	0.001
	(0.002)	(0.001)
High Education Spouse	0.023	-0.007
	(0.016)	(0.014)
Spouse of Asian Ethnicity	-0.003	-0.009
	(0.019)	(0.018)
Spouse of African Ethnicity	-0.002	-0.007
	(0.011)	(0.011)
Spouse of European/American Ethnicity	0.011	0.022
	(0.025)	(0.021)
Spouse of Israeli Ethnicity	-0.005	-0.007
	(0.004)	(0.004)
New Immigrant Spouse	0.004	0.008
	(0.008)	(0.010)
Household Income in 1995	1248	-606
	(859)	(546)
Work Status in 1995 (Employed==1)	0.008	-0.005
	(0.010)	(0.014)
Number of Observations	24,963	24,756

Notes: The table presents balancing tests for the assignment of individuals and their spouses in a specific month to the treatment group, separately for men (column 1) and women (column 2). The dependent variable in each regression is the characteristic of the individual or his/her spouse and the explanatory variable is a dummy for being assigned to the treatment group in a specific month. Additionally, all regressions include the ages of both spouses (at a monthly level) and year fixed effects, and are run separately for men (column 1) and women (column 2). Standard errors are corrected for spouse's year of birth interaction clustering and are presented in parentheses.

Table 4: Estimated Effects of Own and Spouse's Retirement Age Deferrals and their Interaction on Own Probability of Working

	Men			Women		
	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term
	(1)	(2)	(3)	(4)	(5)	(6)
Year FE	0.097	0.007	-0.016	0.059	0.044	-0.033
	(0.006)	(0.014)	(0.020)	(0.014)	(0.011)	(0.013)
Year FE and Spouses' Ages	0.094	0.016	-0.015	0.062	0.044	-0.032
	(0.007)	(0.017)	(0.020)	(0.014)	(0.009)	(0.012)
Year FE and Spouses' Ages and Characteristics	0.081	0.001	0.002	0.056	0.027	-0.019
	(0.013)	(0.011)	(0.017)	(0.011)	(0.010)	(0.011)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.067	0.013	0.004	0.060	0.046	-0.048
	(0.026)	(0.011)	(0.031)	(0.016)	(0.016)	(0.022)
Number of Observations		23,862			23,720	

Notes: The table presents the effects on individual's probability of working as a result of own retirement age deferral (columns 1 and 4), of spouse's retirement age deferral (columns 2 and 5) and its interaction (columns 3 and 6), separately for men and women according to the baseline estimation strategy. The table presents the coefficient estimated according to four different specifications. The first specification includes only year fixed effects, the second specification includes also the ages of the couples (at a monthly level); and the third specification, which is our baseline specification, includes additional couples' characteristics. The fourth specification include an additional control: the fixed effects that interact each spouse's year of birth. Standard errors are corrected for spouse's year of birth interaction clustering and are presented in parentheses.

Table 5: Estimated Effects of Own and Spouse's Retirement Age Deferrals on Own Probability of Working, from Simplified Versions of the Model

	Men			Women		
	Including only Own Deferral Effect	Including both Own and Spouse Deferral Effects		Including only Own Deferral Effect	Including both Own and Spouse Deferral Effects	
	Own Deferral	Own Deferral	Spouse's Deferral	Own Deferral	Own Deferral	Spouse's Deferral
	(1)	(2)	(3)	(4)	(5)	(6)
Year FE and Spouses' Ages and Characteristics	0.083 (0.015)	0.082 (0.011)	0.001 (0.014)	0.046 (0.011)	0.046 (0.010)	0.017 (0.008)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.074 (0.021)	0.069 (0.019)	0.015 (0.021)	0.033 (0.014)	0.036 (0.016)	0.021 (0.018)
Number of Observations						

Notes: The table presents the effects on individual's probability of working from two simplified versions of the model: 1) a version where the interaction term is not incorporated in the model (columns 2-3 for men and columns 5-6 for women); 2) and another version where no spouses' effects are incorporated at all (column 1 for men and column 4 for women). In both versions, the main specifications (baseline and spouse's year of birth interaction fixed effects specifications) are the same as in Table 4. In the first version standard errors are corrected for spouse's year of birth interaction clustering. In the second version without spouse's effects, standard errors are clustered within individual's year of birth and are presented in parentheses.

Table 6: Estimated Effects of Own Retirement Age Deferral on Own Probability of Working, by Sub-Samples of Missing and Non-Missing Month of Birth

	Men		Women	
	Individuals with Inadequate Month of Birth	Individuals with Adequate Month of Birth	Individuals with Inadequate Month of Birth	Individuals with Adequate Month of Birth
	(1)	(2)	(3)	(4)
Year FE, Individuals' Age and Characteristics and Year of Birth FE	0.063	0.074	0.048	0.035
	(0.015)	(0.025)	(0.002)	(0.018)
Number of Observations	6,691	18,272	6333	18,418

Notes: The table presents the effect of own retirement age deferral for men and women respectively, based on the simplified version of the model without including spouses' retirement age deferral effects. Columns 1-2 and 3-4 present the effects of own retirement age deferral for men and women respectively according to two sub-samples: one sub-sample of individuals with inadequate date of birth and the other with adequate date of birth. The regressions include the effect of own retirement age deferral, controlling for individuals' age and other characteristics and year of birth fixed effect. Standard Errors are clustered within individuals' year of birth and are presented in parentheses.

Table 7: Estimated Effects of Own and Spouse's Retirement Age Deferrals and their Interaction on Own Probability of Working, by Couples' Characteristics

	Men						Women					
	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Couples' Working Status	**Working Couples**			**At Least One Spouse is not Working**			**Working Couples**			**At Least One Spouse is not Working**		
Year FE, Spouses' Ages and Characteristics	0.083	0.009	0.000	0.082	-0.014	-0.005	0.070	0.022	-0.013	0.033	0.051	-0.033
	(0.018)	(0.015)	(0.027)	(0.040)	(0.027)	(0.048)	(0.023)	(0.028)	(0.027)	(0.021)	(0.037)	(0.042)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.077	0.036	-0.012	0.060	-0.019	0.011	0.099	0.062	-0.077	0.006	0.025	-0.008
	(0.031)	(0.016)	(0.043)	(0.042)	(0.030)	(0.051)	(0.070)	(0.022)	(0.013)	(0.033)	(0.051)	(0.033)
Number of Observations		15291			8344			15251			8245	
B. Individual Salary	**High Individual Salary**			**Low Individual Salary**			**High Individual Salary**			**Low Individual Salary**		
Year FE, Spouses' Ages and Characteristics	0.090	-0.002	-0.016	0.071	0.006	0.019	0.071	0.036	-0.033	0.038	0.016	0.007
	(0.027)	(0.024)	(0.037)	(0.021)	(0.025)	(0.026)	(0.031)	(0.022)	(0.033)	(0.025)	(0.017)	(0.029)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.077	0.011	-0.013	0.057	0.020	0.014	0.086	0.087	-0.090	0.026	-0.006	0.015
	(0.051)	(0.026)	(0.051)	(0.046)	(0.028)	(0.030)	(0.037)	(0.034)	(0.055)	(0.039)	(0.016)	(0.037)
Number of Observations		12507			11355			12031			11689	

Table 7: Estimated Effects of Own and Spouse's Retirement Age Deferrals and their Interaction on Own Probability of Working, by Couples' Characteristics- Continued

	Men						Women					
	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
C. Individual's Education Level	**High Education**			**Low Education**			**High Education**			**Low Education**		
Year FE, Spouses' Ages and Characteristics	0.180	0.144	-0.126	0.067	-0.017	0.018	0.156	0.114	-0.159	0.047	0.018	-0.003
	(0.061)	(0.021)	(0.079)	(0.013)	(0.014)	(0.016)	(0.045)	(0.051)	(0.061)	(0.010)	(0.012)	(0.013)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.088	0.178	-0.060	0.073	-0.005	0.001	0.223	0.127	-0.222	0.048	0.040	-0.031
	(0.071)	(0.038)	(0.095)	(0.024)	(0.017)	(0.029)	(0.042)	(0.024)	(0.089)	(0.013)	(0.018)	(0.022)
Number of Observations		2770			21048			2760			20889	

Notes: The table presents the effect on individual's probability of working of own retirement age deferral, of spouse's retirement age deferral and its interaction, separately for men and women, based on three stratification of the sample: Panel A shows the results of the stratification based on whether both spouses were working in 1995 or not, according to 1995 Israeli census; Panel B presents the stratification of the sample by high versus low individual income in 1995 (higher or lower than the median salary income); and Panel C displays the results of the reform by individuals' level of education (dummy for highly educated=1 if holding a B.A. degree or higher). The estimates are from both the baseline specification and the spouse’s year of birth interaction fixed effects specification. Standard errors are corrected for spouse’s year of birth interaction clustering and are presented in parentheses.

Table 8: Estimated Effects of Own and Spouse's Retirement Age Deferrals and their Interaction on Own Probability of Working, by Age Group Cohorts

	Men			Women		
	Own Deferral	Spouse's Deferral	Interaction Term	Own Deferral	Spouse's Deferral	Interaction Term
	(1)	(2)	(3)	(4)	(5)	(6)
A. First Two Age Cohorts						
Year FE, Spouses' Ages and Characteristics	0.119	0.062	-0.133	0.117	0.060	-0.082
	(0.034)	(0.027)	(0.027)	(0.036)	(0.034)	(0.036)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.086	0.035	-0.120	0.112	0.064	-0.099
	(0.021)	(0.031)	(0.037)	(0.048)	(0.043)	(0.048)
Number of Observations		8943			8898	
B. First Three Age Cohorts						
Year FE, Spouses' Ages and Other Characteristics	0.094	0.010	-0.047	0.091	0.021	-0.042
	(0.025)	(0.015)	(0.020)	(0.021)	(0.020)	(0.019)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.088	0.007	-0.049	0.088	0.033	-0.066
	(0.036)	(0.014)	(0.026)	(0.030)	(0.020)	(0.027)
Number of Observations		14202			14149	
C. First Four Age Cohorts						
Year FE, Spouses' Ages and Other Characteristics	0.096	0.000	-0.013	0.060	0.023	-0.013
	(0.017)	(0.012)	(0.018)	(0.016)	(0.014)	(0.015)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.088	0.011	-0.019	0.067	0.038	-0.048
	(0.032)	(0.013)	(0.030)	(0.024)	(0.016)	(0.027)
Number of Observations		18819			18741	
D. First Five Age Cohorts						
Year FE, Spouses' Ages and Other Characteristics	0.088	0.006	-0.010	0.060	0.032	-0.019
	(0.013)	(0.012)	(0.016)	(0.012)	(0.011)	(0.011)
Year FE, Spouses' Ages and Characteristics and Year of Birth Interaction FE	0.074	0.017	-0.003	0.066	0.052	-0.056
	(0.028)	(0.011)	(0.031)	(0.017)	(0.018)	(0.022)
Number of Observations		22079			21968	

Notes: The table presents the effects of retirement age deferral on the different age cohorts of the sample, adding gradually each age cohort at a time (the different age cohorts are defined in Table 1). Panel A, presents the effect of retirement age deferral only on the first two age cohorts (men/women aged 65/60 to 65/60 and 8 months); Panel B, presents similarly the effects for the first three age cohorts (men/women aged 65/60 to 66/61); Panel C, presents similarly the effects for the first four age cohorts (men/women aged 65/60 to 66/61 and 4 months); and Panel D, presents similarly the effects for the first five age cohorts (men/women aged 65/60 to 66/61 and 8 months). The estimated effects are presented for the baseline specification and on a specification that includes additionally spouse’s year of birth interaction fixed effects. Standard errors are corrected for spouse’s year of birth interaction clustering and are presented in parentheses.

Table 9: Double Difference-in-Differences Estimation

	Men			Women		
	Own Deferral	**Spouse's Deferral**	**Interaction Term**	**Own Deferral**	**Spouse's Deferral**	**Interaction Term**
	(1)	(2)	(3)	(4)	(5)	(6)
A. Mean Working Month per Year						
Couples' Characteristics and Year Fixed Effects	0.086	0.006	-0.022	0.031	0.012	-0.021
	(0.0210)	(0.018)	(0.031)	(0.021)	(0.016)	(0.028)
Number of Observations		50,216			49,761	
B. Annual Salary of all Individuals						
Couples' Characteristics and Year Fixed Effects	31,375	1,410	-12,508	8,933	9,145	-8,865
	(5,948)	(5,256)	(8,547)	(3,879)	(2,196)	(4,613)
Number of Observations		50,506			49,633	
C. Annual Salary of Working Individuals						
Couples' Characteristics and Year Fixed Effects	14,823	6,262	-16,077	5,949	11,847	-13,130
	(6,620)	(7,020)	(9,340)	(5,408)	(3,525)	(5,932)
Number of Observations		29,620			22,190	
D. Probability of Keeping the Same Job in a Given Year						
Couples' Characteristics and Year Fixed Effects	0.036	-0.028	0.0142	0	0.007	-0.08
	(0.01)	(0.014)	(0.017)	(0.011)	(0.01)	(0.01)
Number of Observations		20,366			19,656	

Notes: The table presents the difference-in-difference estimated effects of own retirement age deferral (columns 1 and 4), of spouse's retirement age deferral (columns 2 and 5) and their interaction (columns 3 and 6) on several labor force characteristics: the mean number of working months within a year (Panel A), annual salary of all individuals (Panel B), annual salary of workers (Panel C) and the probability of keeping the same job within a given year (Panel D). Each regression includes the couple's characteristics, separately for men and women. Standard errors are corrected for spouse's year of birth interaction clustering and are presented in parentheses.

Figure 1: Treatment and Control Groups, by Gender

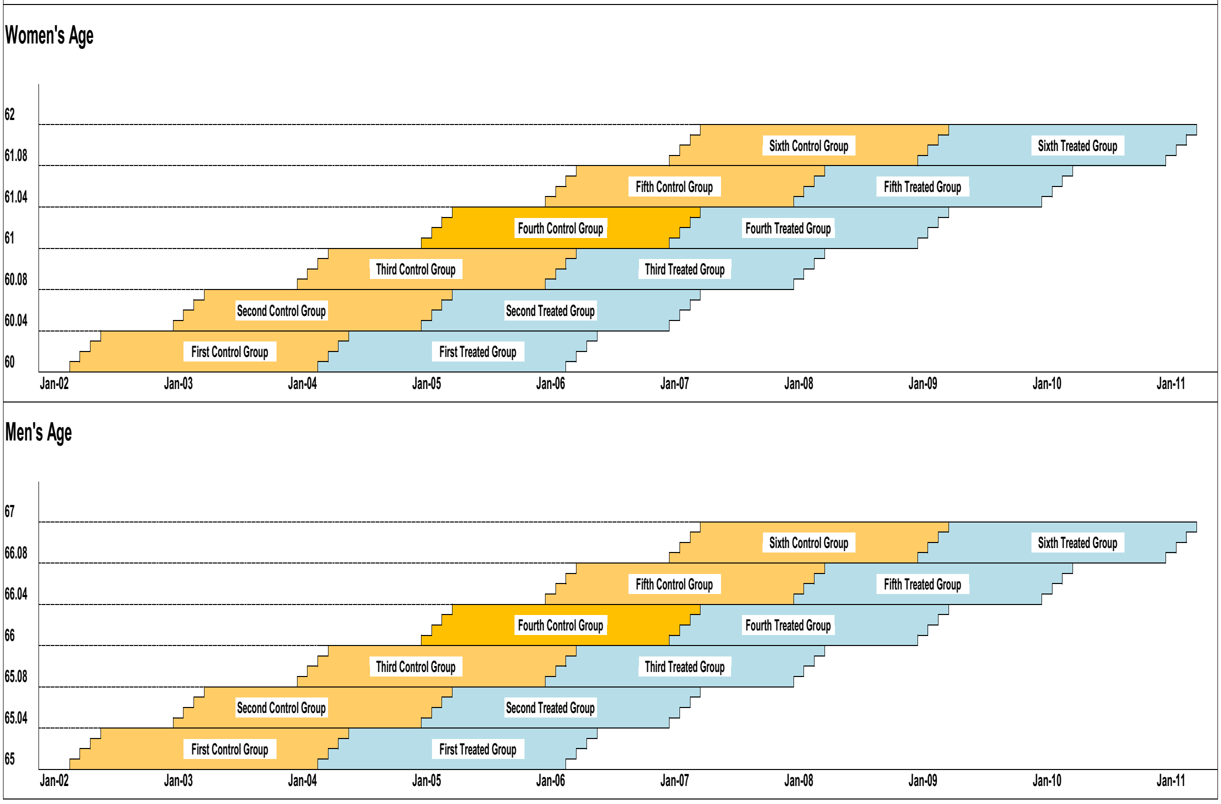
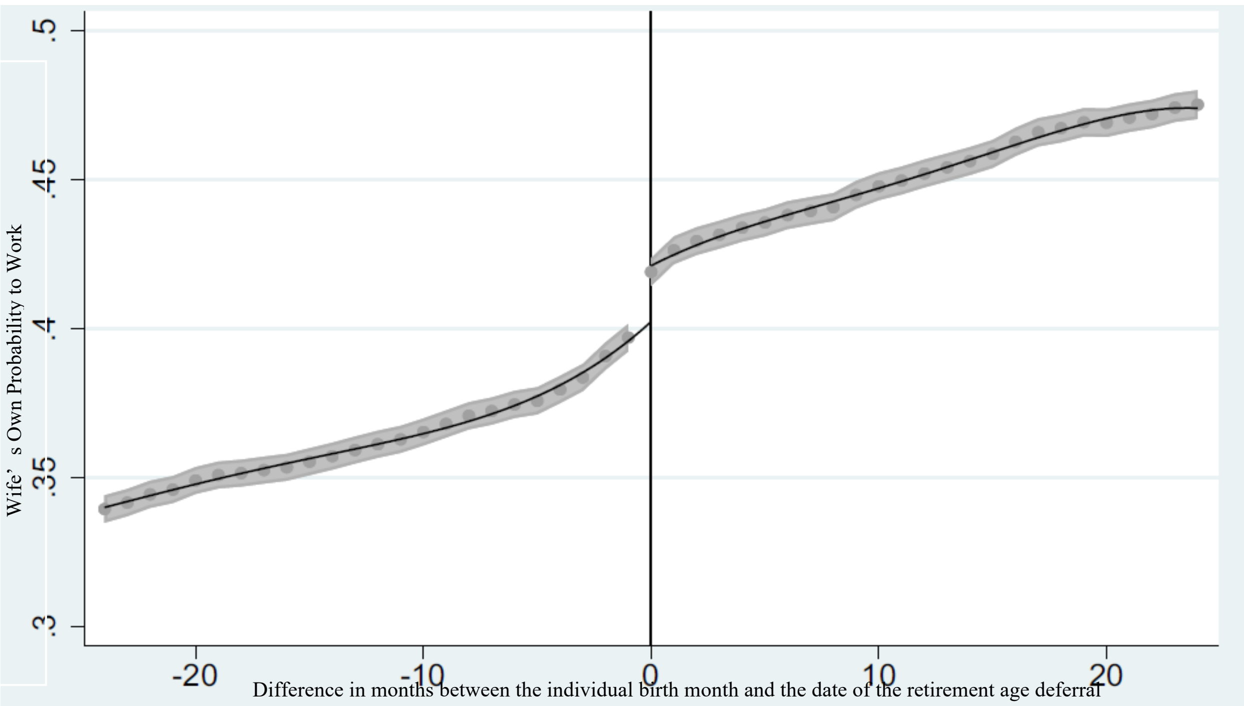
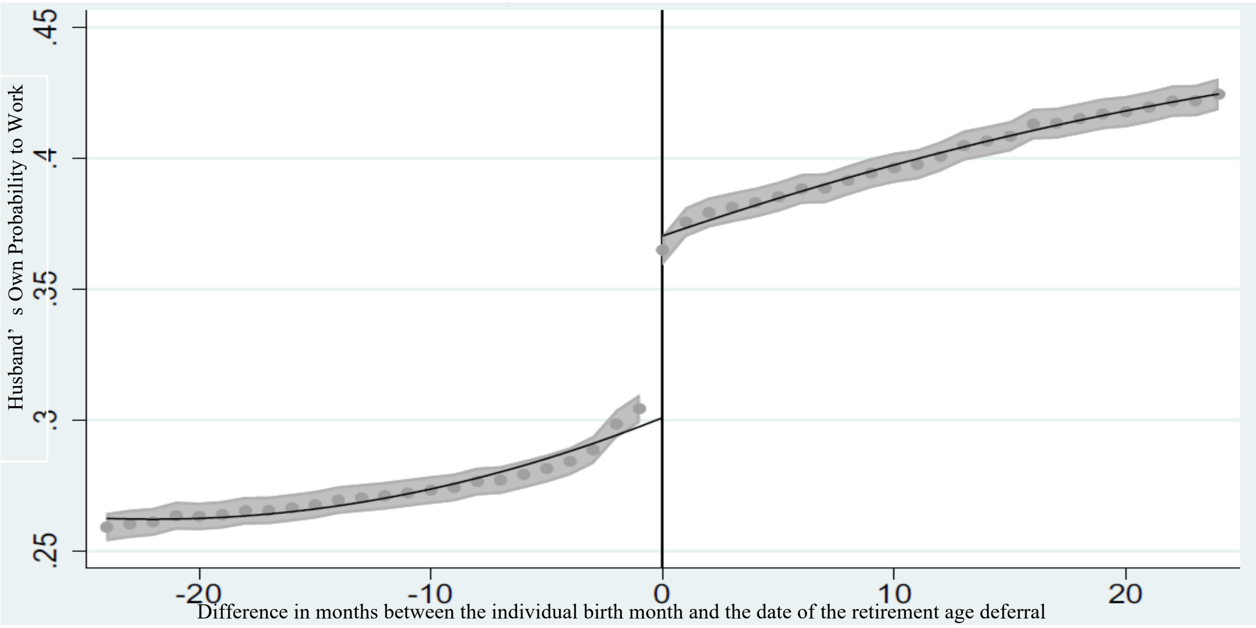


Figure 2:



Notes: The graph shows the wife's probability to work by month of birth before and after the implementation of the reform. The solid line is non-parametric fitted using uniform kernel with an optimal bandwidth of 24 months, with a 5% confident bounds around the kernel estimates.



Notes: The graph shows the husband’s probability to work by month of birth before and after the implementation of the reform. The solid line is non-parametric fitted using uniform kernel with an optimal bandwidth of 24 months, with a 5% confident bounds around the kernel estimates.