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RELABELING, RETIREMENT AND REGRET

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ABSTRACT

Focal retirement ages are a central feature of Social Security programs around the world, and provide a potentially powerful tool for policy makers who are interested in reforming retirement systems to address the growing funding shortfalls. But these tools often come hand in hand with significant changes in the financial structure of Social Security that can have independent, and potentially deleterious, impacts on retirees. In this paper, we use a major reformulation of the retirement system in Finland, featuring a relabeling of retirement ages with modest and continuous changes in financial incentives allows us to separately estimate the impact of relabeling from financial incentives in driving retirement decisions. We find that relabeling is particularly powerful as a determinant of date of retirement. Both graphical evidence and estimated hazard models reveal an enormous change in retirement when individuals face a newly defined "normal retirement" age. We also present a new approach to assessing the welfare implications of induced earlier retirement: looking at the impact on return to work. We show that the marginal workers induced to retire by relabeling are much more likely to return to work over the next three years than is the typical worker. This suggests that there is a marginal increase in regret among those who respond to this change in retirement ages.

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Developed countries around the world face enormous long run deficits with respect to their public pension systems. As a result, pension reform is a constant source of public policy debate. A wide variety of reform strategies have been contemplated and/or implemented around the world. A common approach to addressing such fiscal deficits is to reform the underlying structure of pension plans – most notably by changing either the "early retirement age" at which individuals can first qualify for benefits or the "normal retirement age" around which benefit determination is centered.

The focus on changing these ages as a tool of pension reform is natural given results such as those in Figure 1, created using data from Gruber and Wise (1999a). Figure 1 shows the conditional retirement rate at early retirement and normal retirement across a sample of developed countries, as a multiple of the average of the retirement rate in the year before and after (a summary measure of the retirement "spike").[1] In every case but one, the spike is positive, and it is generally large. On average across these countries, "excess retirement" at the early retirement age is 156% higher than the retirement rates on either side, and "excess retirement" at the normal retirement age is 238% higher than the retirement rates on either side. A large literature confirms that this relationship is causal and not just correlational, using reforms in retirement ages (Gruber and Wise, 1999a; Börsch-Supan and Coile, 2019; Manoli and Weber, 2016b).

What is less well understood, however, is the reason for such spikes in retirement hazards. In a standard model of optimizing retirement behavior, what should matter is the financial incentives to retire at a particular age. However, existing evidence suggests that financial incentives alone cannot explain these spikes; the "excess" retirement at legislated ages suggests that there are other behavioral mechanisms at play in driving retirement. At the same time, contemporaneous changes in retirement

[1] In particular, for each of these nations, we find the age of early and normal retirement eligibility, then compare the conditional retirement rate at that age with the average of the conditional retirement rate in the year before and after. In some nations, there is no distinct early or normal retirement age, so we only present one bar in those cases.

ages and changes in financial incentives make it difficult to measure convincingly the impact of the retirement age norm itself. For example, the move to a new "normal" retirement age of 67 in the U.S. was accompanied by a change in both Social Security wealth and the marginal incentive to retire at different ages. As a result, while quasi-experimental studies can clearly establish that changing retirement ages has a transformational impact on retirement decisions, they cannot definitively speak to the role of financial versus behavioral incentives. What is needed to do so is a change in the labeling of retirement ages without an associated change in financial incentives.

This is a critical issue because nations would like to assess ways to reduce pension liabilities without burdening vulnerable retirees. Given the actuarially unfair structure of pensions in most nations, inducing longer working careers improves fiscal balances. But the typical approach to inducing longer working careers is through increasing financial penalties on those retiring earlier, which penalizes those who have a particularly high disutility of continued work at older ages. If it is possible to change retirement behavior through extreme "nudges" like retirement age relabeling, it offers the possibility of extending working lives without hurting those who need to retire earlier.

In this paper, we study a reform in Finland that allows us to separate financial incentives and norms associated with retirement age. Before 2005, retirees in Finland faced an "early" retirement regime which ran from age 60 to age 65, with "normal" retirement at age 65. In 2005, the system was reformed so that a new "flexible" retirement age was introduced at age 63 – opening up a new and unanticipated retirement possibility at ages 63 and 64. Yet while the reform also included changes in financial incentives, these changes were both modest and more continuous across cohorts than was this "relabeling" – allowing us to separate the two.

We analyze the impact of this reform using data on 100% of the workers in Finland, which allows us to include large samples of workers at and around the key retirement ages at the time of reform.

These data include a rich set of covariates which allows us as well to explore the heterogeneity in the response to relabeling.

The longitudinal nature of our data also allows us to extend the retirement literature in a new direction: modeling the impact of relabeling and financial incentives on return to work. If the response to reforms is driven by behavioral considerations rather than pure financial optimization, it raises the possibility that individuals may regret their decisions ex-post. A potential measure of regret is reversal of the retirement decision. By examining the marginal impact of retirement changes on the return to work among those retired, we can assess whether the individuals who retire in response to these changes "regret" their retirement more than the typical retiree. If so, this heightens concerns about optimization failures in these behavioral responses.

Our results from this analysis are quite striking. We find that there is an enormous and immediate response to the relabeling of retirement in 2005. We estimate that for the cohorts of individuals who were suddenly made eligible for retirement, there was around a 40 percentage point rise in retirement rates at age 63. These changes arise despite very modest changes in financial incentives, and respond to a sharp age discontinuity that is not present in financial incentive changes. At the same time, we find that there is significant evidence of excess regret among those who responded to the relabeling; the return to work rates are much higher among this population than among the average retiree.

Our paper proceeds as follows. Part I discusses the existing literature on retirement decisions and the lack of evidence on retirement labels versus financial incentives in determining retirement. Part II introduces the Finnish context and the reform that we study. Part III discusses the data that we use and our empirical strategy. Part IV presents our results, and Part V concludes.

Part I: Retirement Systems and Retirement Decisions

There is an enormous and rich literature on how the structure of retirement systems impacts retirement decisions. This literature was summarized and extended to an international framework in Gruber and Wise (1999b). A recent follow on volume by Börsch-Supan and Coile (2018) updates the results, providing both updated literature reviews and new evidence based on two decades of new data.

The conclusions of both volumes, and the retirement literature in general, are threefold. First, financial incentives matter both in within nation studies, and when comparing across nations. These financial incentives are generally summarized through a wealth measure and a dynamic incentive measure. The wealth measure captures the entitlement at a point in time of individuals to their expected net present value of pension wealth under the system. The incentive measure captures the marginal change in pension wealth from additional work effort, either in terms of the marginal change for the additional year of work (accrual rate) or the change relative to the optimal date of retirement (option value).

This is illustrated vividly in Figure 2, from Gruber and Wise (1999a). This figure shows the cross-country correlation between a measure of the implicit tax burden and the rate of labor force non-participation across a cross-section of countries. Countries with higher implicit taxes around retirement age have a much higher non-participation rate.[2] Börsch-Supan and Coile (2019) extend this analysis to within country-analysis for a comparable sample of nations, and find that this relationship holds within countries over time.

Second, key retirement ages are primary determinants of retirement decisions. The age at which pensions become available (the “early retirement age,” or ERA), as well as the age at which full benefits are achieved (often called the “normal retirement age,” or NRA) are magnets for retirement, as

[2] In particular, the X axis measures the logarithm of the sum of the implicit taxes on work from the age of early pension entitlement to age 69, while the Y axis measures the share of those age 55 to 65 who are not working.

shown in Figure 1 from the introduction. Third, it is impossible to explain these retirement age "spikes" with financial incentives. Including rich functions of financial incentives does not remove the fundamental explanatory power of retirement age spikes.

Both the first and third of these conclusions are subject to important statistical identification concerns. The financial incentives facing potential retirees are a function of past earnings and labor force attachment, which may in turn have direct impacts on retirement. For example, in the U.S., pension entitlements are a complicated and non-linear function of past earnings and length of working life. Typically, the literature has attempted to address this through rich controls for these outside factors, fundamentally identifying the effect of financial incentives through functional form (e.g. Coile and Gruber, 2004). More satisfactory is work that uses changes in entitlement ages to show that these changes fundamentally alter retirement patterns (e.g. Manoli and Weber, 2016b).

Recent literature has used quasi-experimental evidence to study the effect of incentives on retirement (Brown, 2013; Manoli and Weber, 2016a; Furgeson et al., 2006) and also the effect of a change in statutory retirement age and found that labels affect behavior in a manner which cannot be rationalized by standard preferences (Behaghel and Blau, 2012; Cribb et al., 2016; Manoli and Weber, 2016b; Seibold, 2019; Staubli and Zweimüller, 2013).

Most quasi-experimental approaches, however, do not satisfactorily address the third question, whether financial incentives can truly explain retirement spikes. This is because changes in retirement ages are accompanied by major changes in financial incentives. Consider the shift in the normal retirement age from age 65 to age 67 in the U.S. This shift was accompanied by significant changes in pension entitlements; for example, at any given retirement age, benefits fell, and the marginal returns to additional work changed. What is required to separate these is a change in labeling that is not accompanied by changes in financial incentives. That is what is provided by the Finnish reform, which affected three cohorts contemporaneously and unexpectedly.

Perhaps most closely related to our paper is a recent paper by Seibold (2019). Seibold uses rich variation across a large number of discontinuities in the German retirement system to study retirement effects using bunching methodology. As part of his analysis he compares purely financial discontinuities to those associated as well with changes in retirement ages, and highlights that the responses are larger when the financial changes are associated with retirement age changes. This is consistent with our view that labeling matters above and beyond financial incentives, but it still requires strong structural assumptions to separate out the pure labeling effect. Without a sharp and unexpected change in relabeling that is not itself associated with specific financial changes, it is impossible to cleanly identify the impact of the relabeling alone.

Various alternative hypotheses have been offered and some explicitly tested to explain the importance of labels. A statutory retirement age conveys information to individuals about the optimal retirement ages and could offer one possible mechanism to explain the observed bunching. Since the information is always attached to the statutory age itself, it is difficult to disentangle this effect empirically.

Social norms would also be a mechanism consistent with bunching, although the immediate response to changes in statutory ages suggests this is unlikely (Behaghel and Blau, 2012). Asch et al. (2005) study the retirement behavior of federal civil service workers employed by the US Department of Defense, who face a different retirement scheme from the general population. They find no evidence of bunching at the statutory ages of the Social Security system, suggesting that society-wide norms are not the main drivers of the relabeling effect. A reference-dependent utility function with a kink the reference point set by the statutory ages (see Behaghel and Blau, 2012 and Seibold, 2019) is consistent with bunching at said statutory ages but remains to be confirmed.

One fundamental limitation of the previous literature extends to our work as well: it is possible that some of the increased retirement that we attribute to worker decisions may be reflecting employer behavior as well. For example, when retirement ages change, employers may find ways to change incentives for work at different ages to accompany this. As we note below, in our sample, there is very little enrollment in supplemental private pensions. But we can't rule out other employer tools such as age-specific wage changes. Seibold (2019) shows that bunching in Germany takes place also in small firms, which are exempt from employment protection.

Part II: The Finnish Pension Reform

The Finnish statutory pension system is a combination of earnings-related pension and residence-based national pension. The earnings-related pension system is mandatory for the workers and self-employed and cover virtually all earnings. It is a defined benefit system where the pension level is determined by the length of work history and by the amount of past earnings. The average gross replacement rate is 56.5% (OECD 2019).

The national pension (and a complementary guarantee pension starting 2011) is a demogrant to the entire population that is clawed back in proportion to the earnings-related pension. The grant is around 500 euros per month, and it is reduced by 50 cents for each additional earnings-related euro below around 1000 euros per month. As a result, only individuals with very short careers or low earnings history are granted the full national pension; before the reform in 2004, around 45 percent of pensioners had only earnings-related pension and only 6% of pensioners received no earnings-related pension[3]. The national pension and the guarantee pension are administered by the Social Insurance Institution of Finland while the earnings-related pensions are administered by several earnings-related pension providers.

[3] Finnish Centre for Pensions, Statistical Database, retrieved from https://tilastot.etk.fi/pxweb/en/ETK/ETK__110kaikki_elakkeensaajat__10elakkeensaajien_lkm/elsa_k04_rak.px/?rxid=32650b38-9599-4d41-8122-0ccd565648a0 on May 6, 2020.

There are also voluntary pension plans in Finland but these are in a minor role. In 2004, 12% of households had private pension savings and these constituted 6 percent of the gross wealth of these households. In our target population of those above 60, only 4% of households had private pension savings (Ahonen and Moilanen, 2007).

Under both the earnings-related and national pensions, before the reform, the early retirement age at which benefits could first be claimed was 60, with a full benefits (normal) retirement age of 65. There were additional early retirement pathways as well. Partial or full disability pension was claimable if working capability has decreased by 40 % (for partial) or 60 % (for full) from the previous levels; for those with at least a 10-calendar-year career, the pension level was defined as a sum of accrued pension rights until the moment of disability and the projected pension benefits. An individual early retirement pension scheme was also granted based on health conditions, except that the criteria were somewhat more lenient than for the disability pension and the eligible workers needed to be over 58 years of age. And an unemployment pension was granted for those workers who had been long-term unemployed and over the age of 60.

The pension system was reformed substantially in 2005. One of the reasons for a need to reform the system was that the proportion of working-age population was forecast to start decreasing in the near future. Also, a major economic downturn in the 1990's and the resulting rise in public debt had increased awareness for the need for fiscal balancing. The goal of this reform was "to increase the employment rate in the long term and to increase the effective retirement age by 2 to 3 years in order to alleviate the pressure to raise the pension contributions".[4] The tradition of pension reforms in Finland is that the employer and employee unions negotiate an agreement for the reform and the government converts the agreement into a government proposal which is presented to the parliament. The

[4] The government program was retrieved from https://valtioneuvosto.fi/hallitusohjelmat/-/asset_publisher/67-paaministeri-paavo-lipposen-ii-hallituksen-ohjelma on Oct 3, 2019.

government proposal of the 2005 reform was presented to the parliament in November 2002 and the bill was passed in June 2003 (Government proposal, 242/2002). The reform itself caused very little debate even with a parliamentary election in the spring 2003. In fact, none of the major parties even mentioned the pension reform in their election programs.[5]

Information letters regarding the reform were sent in January 2004 by most earnings-related pension providers. However, we do not know, what exact proportion of the relevant population was sent such letters.

A number of restrictions on early retirement were also put in place. The early retirement age was raised from age 60 to age 62, and both the individual early retirement pension scheme and the unemployment pension scheme were abolished. At the same time, the reform introduced a new "flexible" retirement age that would allow the individual to "retire flexibly between the ages 63 to 68". Arguably, the reform did not increase actual flexibility, since retiring before and after the full retirement age of 65 was fully possible before the reform in the private sector. In the public sector, the reform increased flexibility by allowing work until age 68 instead of the old upper limit of 65. Retiring at ages 63 to 68 is called retiring at the full retirement age (FRA), whereas retiring at 62 is called retiring at the early retirement age (ERA) and is associated with an early retirement penalty (described below).

This reform also included a series of changes in financial incentives for retirement. Before 2005, the pension was calculated based on the earnings from the last 10 years of each employment contract prior to retirement. Accrual rates were 1.5% below the age of 59 and 2.5% between ages 60–65. There was also a pension cap at 60% of the highest annual salary during the period where pension was calculated. Early old-age retirement (possible from the age of 60 onwards) reduced pension permanently by 0.4 percent of accrued pension for each month before the age 65. If retiring was postponed after the age of 65, each month increased the pension by 0.6 percent.

[5] The election programs were retrieved from https://www.fsd.uta.fi/pohtiva/ on Oct 2, 2019.

From 2005 onwards, the whole working history is considered when calculating the pension. For individuals who work past the new flexible retirement age of 63, there is a pension accrual of 4.5% per year. This high accrual rate was popularly dubbed the ’super accrual’. The rationale for the move was to “encourage the prolonging of careers also by making old-age retirement flexible and by incentivizing the prolonging of careers after the age 63 with a markedly higher accrual” (Government proposal 242/2002). The accrual rate was set to 1.9% for ages 53–62 and to 1.5% for work done before the age of 53. The early old-age minimum age was increased to 62 and the penalty for claiming pension early was 0.6% per month. The increase in pension for delaying retirement after the age 68 was 0.4% per month.

For each month of work, the incremental change in pension is based on the accrual rate (which is a function of earnings) in addition to one less month of early retirement penalty (which is a function of accrued penalty). The penalty and the accrual rate together define the *effective accrual rate*, which we calculate as a proportion of accrued pensions, making the results comparable to estimated wealth effects.

The important point for our analysis is that there was not a meaningful change in the effective accrual rate at age 63 because the new “super accrual” was replacing an existing penalty for early retirement. The change in effective accrual rates was on average -0.7% (SD: 1.96%) of accrued pension wealth, compared to a mean of 8.9%. Thus, despite the intended effect of the reform, the financial incentive for continued work actually declined slightly.

The reform only made one meaningful change to the national pension, abolishing the implicit tax on earnings-related pension between ages 63 and 65. There are some differences in the pension rules between public and private sector workers; we only study the private sector, since the rule for the public sector are more complex and the data less coherent.

The overall change in pension wealth and accrual rates due to the reform are shown in Figure 3. The figure shows age at the time of the reform on the X-axis, and percentage changes on the Y-axis. We demarcate three areas through vertical dashed lines: those who were too young to be relabeled; those relabeled at some point over the next twelve months (to allow a focus on annual retirement); and those who were already too old to be relabeled.

The upper line shows the change in pension wealth due to the reform. There is a large jump in pension wealth at age 62 that stems from a reduction in early retirement penalty. For example, at age 63, the reform reduced the penalty from 9.6% (24 times 0.4%) to zero (0 times 0.6%) for a total of 9.6% overnight increase in pension wealth. Note that the change in pension wealth is strictly a function of age, first rising to a peak for those at their 63rd birthday at the time of reform, and then declining steadily through age 64.

The lower dashed line, and the associated shaded confidence interval, shows the percentage change in the effective pension accrual rate. The rate is more rapid until age 62, since the early retirement penalty went from 0.4% to 0.6% per month between ages 62 to 63. At the annual level, the difference gives 2.4% more accrual as a proportion of accrued pension. This more than compensates the lower accrual rate (1.9% vs 2.5% of earnings). Then at age 62 it starts to decline because early retirement penalty was abolished in the reform after age 63, more than negating the increase in accrual from 2.5% to 4.5%. It the flattens out after age 63, then begins to increase after age 64, since before the reform, accrual rate goes to zero at age 65 and early retirement penalty of 0.4% per month changes to an increase of 0.6% per month of postponed retirement. Together the changes after age 65 decrease the pre-reform effective accrual rate such that by age 65, the difference between the old and the new rate is on average zero. There is some modest within age variation, but it is small relative to the time patterns. Most importantly for our purposes, both the change in pension wealth and accrual rates are continuous at age 64.

Part III: Data and Empirical Strategy

Data

We use administrative data from Finnish Centre for Pensions, as well as from Statistics Finland for years 2000–2015. The main data from Finnish Centre for Pensions include individuals' earnings for those years, and pension claiming including the exact day of the start (and end) of specific pension spell for everybody who was insured in Finland. Beside earnings, the data include the official calculation for accrued pension at the end of 2004, based on which we calculate accrued pensions for earlier periods, using their earnings information. Thus, the measure we have for the reform year is the administrative number that actually defines the pension. For earlier years, the measure is our calculation, tracking back from end of 2004. Our calculation is based on the same administrative data used for official calculations.

The supplementary data from Statistics Finland include a wide set of labor market characteristics and individual characteristics for all Finnish individuals between the ages 40 and 75. This includes sickness absences of more than nine days, non-pension net wealth (collected for wealth tax purposes), spouse, highest educational achievement and sex.

Our main sample is individuals who still are not working in the public sector and are still in the labor market, i.e., are not claiming any of the early exit pension programs. A provision in the reform allowed individuals whose pension was lower under the new regime to continue under the old system – given the general rise in pension wealth under this reform this impacted only 0.5% of the sample, and we drop them for the analysis.[6]

As reviewed here, the 2005 reform had a variety of elements that impacted workers throughout the age distribution, from abolishing early retirement at age 60 to introducing new work incentives at

[6] The pension was calculated with the old and the new formula and the more favorable for the retiree would be applied. This rule was relevant mostly a couple of years after the reform for individuals with high earnings relative to accrued pensions, since accrual rates relative to earnings increased and early retirement penalties and increases due to postponed retirement, which are relative to accrued pensions, were abolished for age brackets 63 to 68.

age 68. The goal in our analysis is not to provide an overall analysis of this reform, but to instead focus on the effect of relabeling. To do so, we impose a number of sample restrictions.

Our main sample includes those who are employed and had an accrued pension income high enough that their national pension had phased out, so that they were claiming only earnings-related pension (~11,000 euros or higher, depending on marital status and municipality). We focus on this subset, since they faced the full impact of the relabeling. We also study a control group of those with very low accrued pension, who rely primarily on the national pensions. The national pension was not relabeled and thus those who have low accrued pension act as a control group.

We also limit our sample is to individuals aged 62 to 65 at the start of the year, for two years pre-reform (2003 and 2004) and one year post-reform (2005).[7] This sample excludes anyone who is impacted by the other changes in the law, such as changes in early retirement provisions. Thus, this restriction allows us to focus exclusively on the impact of the relabeling and other financial changes occurring in this limited age range. As noted earlier, the reform was announced well before implementation; below we test for anticipation effects that might bias our analysis.

Our definition of retirement is based on claiming old-age pension; we define retirement date as the day before the start of the pension spell. The timing of the retirement is registered at the monthly level such that virtually all claims are timed on the first day of the following month.

Empirical Strategy

Our goal is to understand how relabeling of retirement in Finland impacted retirement, distinct from changes in financial incentives. Our primary empirical strategy is a difference-in-difference comparison of the cohorts that were immediately impacted by the change in retirement to those born immediately before who were not impacted. For those in 2005, there was suddenly a new retirement

[7] We add another year post-reform when we examine return to work, as described below.

option available between ages 63 and 65; since we define retirement over an annual period, this impacts those between ages 62 and 64 (birth cohorts 1941 and 1942). For individuals at those ages in 2004, however, there was no such retirement option available. By comparing how retirement changes in the former group relative to the latter, we can assess the impact of relabeling.

This empirical strategy is illustrated in Figure 3. The x-axis on this figure is age at the date of reform. Those over 62 and under 64 were suddenly given the new retirement option, so we use the vertical dashed lines to demark the "relabeling" region. The solid line shows the change in pension wealth. There is a discrete jump in pension wealth at age 62 due to the reform-induced reductions in early retirement penalties (see Part II for details). The accrual rate falls at that age, albeit more smoothly, and the magnitude of the change is quite small, since the increase in nominal accrual rates was more than offset by the reductions in early retirement penalties, as explained in more detail in Part II. At age 62, therefore, we cannot separate the change in financial incentives from the relabeling.

This is not the case at age 64. Individuals in the treatment cohort born on Jan 1, 1941 have a new retirement option under relabeling; for those born one day earlier, there is no relabeling, since they would have reached eligibility in the counterfactual case of no reform. Yet, there is a continuous change in both pension wealth and accrual at that age. Therefore, the change in retirement behavior around that date provides a test of the relabeling effect.

We will begin by illustrating this graphically to gauge the magnitude of the response. We then turn to a retirement hazard model that embeds the relabeling effect along with financial incentives. The advantage of such a model is that it allows us to provide well-identified estimates of the impact of financial incentives that come from reform-induced changes in these incentives. We can then use these to compare the size of the relabeling effect to the size of financial incentive effects.

To be more precise, we estimate Cox proportional hazard models of the form:

$$\lambda(t|X_{ip}) = \lambda_0(t)\exp(\beta_0 X_{i0}, \dots, \beta_p X_{ip}). \tag{1}$$

This expression gives the retirement hazard function at time t for person *i* with a covariate vector X_i. The dependent variable is retirement over the next year. Retirement is defined as starting to claim old-age pension. Our main specification is a regression with cohorts which were aged 62 to 65 at the start of the year for years 2003–2005 in the pure earnings-related pension sample. We use two control years (2003 and 2004) to better pin down the covariates.

As noted earlier, we focus on two financial measures, pension wealth and accrual. In the model, we include the traditionally computed measures, which are identified from cross-sectional comparisons of individuals with different earnings histories. We also control for monthly age, year, pension wealth, non-pension wealth decile, spouse, pre-reform marginal accrual rate, sex, a dummy for sickness absence longer than nine days in the three preceding years and tertiary education.

We are also able, however, to include in the model measures of financial incentives which are exogenously varied by the reform and we consider as continuous treatments. The change in pension wealth is a direct function of age due to the changes in retirement penalties.[8] The change in accrual rates is a more complicated function of age, accrued pension and earnings, since the accrual rates were affected by the changes in early retirement penalties and accrual rates and these varied by age. See Part II for more details. The change in incentives was largely exogenous. The only thing that could endogenously be adjusted in the short window of the reform, would be earnings levels, but we use pre-reform earnings to remove this potential source of endogeneity**.** The income effect is identified by comparing individuals at each age in 2005 vs. 2004 and controlling for age. The substitution effect is identified by the changes in accrual rates due to the reform across individuals in different cohorts and different ratios of earnings to accrued pensions.

[8] Pension wealth is calculated as the net present value of all future pension income assuming immediate retirement, discounted by survival probabilities using Finnish mortality tables in addition to a 2% discount rate.

The regression model also includes the relabeling effect, which is just a dummy variable for reaching full retirement age in 12 months due to the reform. This variable is not just identified by the discontinuity at age 64, but by the general difference-in-difference comparison across cohorts and the timing of reform, controlling for the associated changes in financial differences.

The sample size of the main sample is 25,088. The descriptive statistics are presented in Table 1. One-quarter of our sample is female, since this is an older cohort as of the early 2000s so female labor market attachment was low and women were more likely to work in the public sector which is excluded in the analysis. Three-quarters of our sample is married, and 32% have tertiary educational attainment. We measure health by the presence of a sickness absence from work of nine days of longer during the three preceding years (2000–2002 for 2003 data, etc.); only absences longer than nine days get registered in the data. One third of our sample has a sickness absence. The increase in pension wealth averages 6% of baseline, while the change in accrual is much smaller at only -0.7 on average. In our sample, 80% are subject to relabeling. The control birth cohort 1940 is smaller than the treatment cohorts 1941 and 1942 due to Second World War, pushing the proportion subject to relabeling above the two thirds, which would occur with equally sized birth cohorts.

Part IV: Results

Graphical Illustration

The graphical illustration of our key result is presented in Figures 4 and 5. Figure 4 replicates Figure 3 but adds the empirical retirement rates in 2004 and 2005. Before age 62, retirement behavior is quite similar for those in both cohorts. Then, at age 62, the retirement rate jumps for the 2005 cohort but not the 2004 cohort. After age 64, the two series converge again.

Figure 5 summarizes this by plotting instead the difference in retirement rates across cohorts at each age. The impact of relabeling is now obvious. There is little pre-existing difference in retirement rates before age 62. There is then an enormous jump in the retirement rate difference, from close to zero to 40%. The differential retirement rate remains escalated in the entire relabeling range, with no clear pattern. It then immediately jumps down once the relabeling regime is over (once the retirement age labels are the same in both years). After age 64, the retirement rate remains somewhat lower, as those who remain in the labor force in the relabeled cohort are less likely to retire at each age.

The implied effect of relabeling can be computed most cleanly by comparing the red and green dots, right before and after the discontinuity at age 64. Doing so, we estimate that relabeling led to a 40pp change in the retirement hazard.

This effect is directly visible in the overall retirement hazard in Finland. Figure 6 shows the retirement hazard in 2004, 2005, and 2015 (in steady state). Several immediate changes are noticeable between 2004 and 2005. First, there is a reduction in the hazard rate at the pre-reform early retirement age of 60. Given that the early retirement age has been moved to age 62, this is not surprising. What is more notable is that there is no corresponding spike at the new early retirement age of 62. It appears that the ability to have "normal" retirement one year later makes this early retirement option less attractive. Second, there is a large new spike at age 63, the relabeled normal retirement age. Third, there is a corresponding reduction in the hazard rate at age 65, the previous normal retirement age.

Ten years later, in 2015, 65 is practically irrelevant as a retirement age. Only 9% of all old-age retirement takes place at that age as opposed to 73% in 2004. A small spike has by then emerged at age 68, which was one of the original aims of the reform.

We can also confirm the importance of rebelling, relative to financial incentives, by examining within-year changes at each age. Figure 7 shows the timing of retirement within the year in before (blue line) and after the reform (red line) across the three relevant age groups. Each line shows the monthly survival rate in the labor force for those who are in the labor force at the start of the calendar year in which they are 62, 63, or 64. More specifically, those who turn age 63 in the first month of 2004 (pre-reform) are represented by the first step of the blue line in the first panel; those who turn 63 in the first month of 2005 (post-reform) are represented by the first step in the red line in the first panel.

The first panel shows the results for those who are age 62. For these individuals, the law change relabels them as they turn 63, which happens equally throughout the year. Before the reform, there was some retirement through the year under early retirement provisions. After the reform, there is a much larger share retiring in the month that they turn 63, so that there is a growing gap between the lines over time.

The second panel shows the results for those who are age 63. For these individuals, relabeled retirement is available for them at the start of the year, regardless of their birthday. And, in fact, we see a large increase in retirement (reduction in survival) in January. This is followed by a slow pattern throughout the year that largely mimics the pattern that we saw before reform. This is consistent once again with the effect being through relabeling and not financial incentives, which operate more smoothly throughout the year.

The third panel shows those who are age 64 when relabeling occurs. For this group, there is a high retirement rate as each cohort turns 65 in the baseline, due to reaching normal retirement age. After reform, as in the second panel, everyone was relabeled at the start of the year, showing an increase in retirement in January. There is also a change in financial incentives. Together these changes result in a slightly larger retirement rate that disappears by the end of the year.

Regression Results

Table 2 shows the estimates from equation (1). The first panel shows the coefficients on the change in financial incentives from reform, as well as on the relabeling dummy. We find highly significant coefficients on both the financial variables and the relabeling dummy. We estimate that each reform-induced 1% rise in pension wealth multiplies the retirement hazard by 1.11 and each 1% rise in accrual rates by 0.94.

We also find a huge coefficient on the relabeling indicator, showing that cohorts subject to relabeling were 678% more likely to retire. This is equivalent to a change in pension wealth of around 20% or in accrual rates of 32%.

Our other covariates show that prior sickness absences are associated with a 12% higher retirement hazard and having a spouse 7% higher. Those with tertiary education have a retirement hazard that is similar to the rest of the sample and females have a 13% higher hazard.

The model also includes the cross-sectionally identified coefficients on pension wealth and accrual. These effects are much more modest than what the exogenous changes imply. A change of 1% in pension wealth is associated with a multiplier on the retirement hazard of 1.037 (compared to 1.11 for the exogenous change in wealth); for the effective accrual rate, the multiplier is 0.984 (compared to 0.94 for the exogenous change in accrual).

This may reflect the fact that there is a stronger short run reaction to changes in incentives, so that the lower cross-sectional estimates are proper long-run response estimates. But it is also possible that these estimates are simply poorly estimated since they are largely based on cross-sectional factors which are also correlated with retirement.

How robust is our main result to the omission of some control variables? In table 1, columns 2 and 3, we show our main results with less controls. Column 2 leaves out individual controls other than age, year and cross-sectional financial variables. The results on our variables of interest stay essentially the same as our main results.

The cross-sectional financial variables, however, are more strongly affected by the omission of control variables. Both estimates are lower. In particular, the estimate for pension wealth is reduced by about a third. In column 3, we only leave age and year dummies in addition to our variables of interest. Again, the result is mostly in line with our main result. Now, the accrual rate grows in absolute value.

Previously, Gruber and Wise (2004) have estimated income and accrual effects for several countries. Of these, Sweden is the most comparable to Finland. Palme and Svensson (2004) have estimated the income and accrual effects for Sweden, using cross-sectional variation.[9] They find that a 1 million kronor increase in accrual rates decreases retirement by a percentage point compared to a baseline of 5.5%, or around 18%. A change equivalent to 1% of median pension wealth (~15,000 kronor) would only decrease retirement by around 0.3%. Using the same logic, a 1% increase in pension wealth relative to the median would increase retirement by 0.5% to 1%, depending on the specification.

Their estimate for the substitution effect of 0.3% decrease in retirement rates for a 1% change in accrual, is one order of magnitude below our quasi-experimental estimate of 6%. It is also lower than our cross-sectional estimate of 1.5%. For pension wealth, their estimates (0.5% to 1%) are around one tenth of the magnitude we estimate quasi-experimentally (11%), and below but closer to the 3.5% we estimate in the cross-section. Thus, our results suggest that cross-sectionally estimated retirement incentive coefficients are both less robust to controls and much smaller than those exogenously identified by pension reforms.

Specification Check: National Pensioners

As noted above, our sample consists of individuals whose full retirement income comes from the earnings-related pension. But for other workers, where most of their retirement income comes from the national pension, the relabeling effect should be much weaker, since the eligibility age for the national pension was not reformed, but rather stayed at 65. This sample therefore serves as a control group for our identification of the relabeling effect.

[9] Their Table 10.18 second column is the most comparable to our main specification. It is estimated for males only.

In the last column of Table 2, we show the results for this control group sample. The control group earns on average one quarter of their retirement income from earnings-related pensions. The estimate for the relabeling effect is low and insignificant, although positive. The estimate is consistent with this sample being only slightly affected by relabeling. However, incentive effects are still relevant for this sample, since they are computed on an individual basis using the actual magnitude of earnings related pension for that worker.[10] The estimated incentive effects are close to what we observe with the main sample, giving support to our primary estimates.

At the same time, cross-sectional wealth and accrual estimates give the reverse sign compared to our main sample – further confirming the identification problems plaguing these measures. In this lower income sample, the crucial omitted variable is other means tested social insurance benefits such as housing and income support. The main sample, however, doesn't suffer from such measurement errors, since the cutoff point for national pension is above the income levels at which individuals qualify for these benefits.

Regret: Impacts on Return to Work

As discussed earlier, there is the possibility that individuals reacting to the relabeling in Finland may be departing from the standard life cycle model. As such, it is possible that there are welfare losses from this policy change that go beyond the standard model. For example, Diamond and Koszegi (2003) develop a model of retirement for individuals with quasi-hyperbolic discounting. In such a model, individuals excessively retire relative to their own long run preferences. As a result, they have demand for commitment devices that limit their retirement probabilities. Relabeling can be viewed as loosening

[10] That is, the incentive effects are computed to be small for these workers, so the coefficients can be interpreted as parallel to what we observe for the main sample.

this commitment, leading to excessive retirement and ex-post welfare loss. Other models of self-control limitations could yield similar “regret” among early retirees.

Measuring such regret quantitatively is difficult in most settings, but in our setting we have an excellent revealed preference measure of regret: return to work. In the standard life cycle model, individuals optimize their retirement date given their available information about wages, the value of leisure, and retirement incentives. Some of those who retire will subsequently return to work. This doesn’t necessarily reflect any failure of optimization over retirement, but rather could indicate the arrival of new information such as realized preferences for leisure or the realization of income supports that were uncertain at the time of retirement.

As a result, the *level* of return to work doesn’t allow us to separate regret from preference variation. But so long as the policy change doesn’t itself change preferences for retirement, any *change* in return to work due to this policy change could indicate regret. Of course, the policy change may change preferences for retirement to the extent that it changes social norms. But such changes to social norms would make retirement more acceptable at earlier ages, not less, so it should lead to less return to work, not more. If we see an increase in return to work due to relabeling, it is more consistent with regret over retirement for the incremental workers who are induced to retire by relabeling.

To understand our empirical strategy, consider a randomized trial of retirement incentives. The treatment group *t* includes n_t individuals, while the control group *c* includes n_c individuals. Before the trial, out of each group r_t^0 and r_c^0 workers retire, respectively. In addition, before the intervention, x_t^0 and x_c^0 workers return to work within three years of retiring. By randomization pre-treatment retirement rates are equal across the groups $\left(\frac{r_t^0}{n_t} = \frac{r_c^0}{n_c}\right)$, and pre-treatment return to work rates among those who retire are equal among the groups $\left(\frac{x_t^0}{r_t^0} = \frac{x_c^0}{r_c^0}\right)$.

The experiment randomly provides large new retirement incentives to the treatment group. After the experiment, there are r_t^1 and r_c^1 retired workers, respectively, and of those workers x_t^1 and x_c^1 return to work. If randomization is appropriate, we can measure the impact of the intervention simply by measuring the ex-post difference in retirement probabilities across groups $\left(\frac{r_t^1}{n_t} - \frac{r_c^1}{n_c}\right)$. And as long as the experiment changes nothing else about participant preferences, and randomization holds, we can measure marginal regret as $\frac{x_t^1}{r_t^1} - \frac{x_c^1}{r_c^1}$. That is, any differences in return to work as a result of this experiment are driven by the *marginal regret* among those retiring due to the experiment.

In our implementation, we follow this same strategy using our quasi-experimental variation. The treatment is being relabeled. As before, the treatment group is those who were between 62 and 64 in 2005, and controls are those who were older. In our case, we don't have randomization, so instead of assuming randomization we use a difference-in-difference strategy, comparing to those same age groups pre-reform. So we can measure marginal regret as:

$$\left(\frac{x_t^1}{r_t^1} - \frac{x_c^1}{r_c^1}\right) - \left(\frac{x_t^0}{r_t^0} - \frac{x_c^0}{r_c^0}\right).$$

Where period 1 is 2005 and period 0 is the pre-reform period.

To implement this test, we focus on the sample of individuals who retire. We then use as a dependent variable return to work over the next three years. Return to work is defined as earning at least 25% of their highest earnings over the past three years.

The correspondence between retirement incentives and age is the same as before: for those who are age 63 in 2005, we include their financial incentives to retire in 2005. We then keep all independent variables and financial/relabeling measures at those same values for 63-year-olds, but now we change the dependent variable to be returning to work over the next three calendar years. Therefore, this regression is assessing whether financial incentives or relabeling have a different effect on those who retired under different regimes.

Return to work is rare, with only 17% of those who retire return at baseline. In order to ensure a large enough sample to statistically identify the impact of relabeling on return to work, we therefore expand our observation window to a period of two years. We now compare the 24-month pre-reform period of 2003–2004 to the post-reform period of 2005–2006. When we extend the window, 68% of the sample retire compared to the 35% in the 12-month setup, giving us more statistical power. Figure A1 shows that relabeling produces a sharp increase in retirement over this two-year window as well. But the relabeling effect is limited to those age 62–63, since anyone above age 63 would have reached full old-age retirement eligibility in the counterfactual case of no reform.

Table 3 shows the results for return to work. The impact of the financial incentive variables is small and insignificant, suggesting that those induced to retire by financial incentives are not differentially likely to return to work. In contrast, relabeling increases return to the labor market at a hazard ratio of 1.48, meaning that they were 50% likelier to return to the labor market relative to the baseline. Such a pattern is consistent with individuals regretting their decision to respond to the rebelling of retirement dates.

As our randomized trial example makes clear, however, there is a major empirical concern with this approach: while the overall sample is balanced across treatment and control, the additional individuals who retire due to the financial incentive may not be identical. For example, suppose that the individuals who retire more in response to relabeling are particularly healthy, and suppose that healthy individuals are in general more likely to return to work. In that case, if we find higher rates of return to work among those retiring due to relabeling it could simply reflect this heterogeneity and not true differences in regret.

To address this concern, we directly examine heterogeneity in the larger pool of retirees that results from relabeling. In particular, we re-estimate the regression shown in the first column of Table 3, but replace the dependent variable with various characteristics of the pool of retirees. If there is differential selection into retirement as a result of relabeling, and it is demonstrated along observable dimensions, then it will be reflected in these regressions.

Table 4 shows these regressions. First example, the first column uses as a dependent variable our health measure, whether the individual has a sickness absence of 9 or more days in the preceding three years. We find that there is no significant effect of relabeling on the odds that retired workers have sickness absences. This indicates that there is no selection along this dimension in response to relabeling.

The next six columns repeat this exercise for the other measures of observables that are available in our data: having only primary education; having tertiary education; being in the first or third wealth tercile; gender; and marital status. We find that there are significant coefficients on three of these six indicators. Those who are most highly educated are less likely to be selected into retirement by relabeling. This is consistent with the notion that individuals who are more educated are able to assess the fact that relabeling is just nominal and not meaningful in terms of retirement income. Perhaps due the same logic, we find those with higher wealth are also less likely to be selected into retirement by relabeling. Finally, we find that women are less likely to be selected into retirement by relabeling. This may reflect the fact that women's retirement decisions are driven primarily by spousal concerns and not by own retirement preferences.

Most importantly for us, these results indicate that the response to relabeling should lead to *less* return to work, not more. Appendix Table A1 shows results from a regression for a dummy for returning to work before the reform in 2003. The dependent variable is whether individuals returned to work over the next three years. We find that sicker individuals are less likely to return to work, more educated people are more likely to return to work, and those in both the first and third wealth terciles are more likely to return to work. As we show in Table 4, there is no association of relabeling with selection along the dimension of illness, but it is more educated and wealthier individuals who are less likely to respond to relabeling. But these groups are inherently *more* likely to return to work, not less. So, this cannot explain our return to work finding.

We illustrate this point in the second column of Table 3. This is identical to the first column described earlier, except that actual return to work is replaced with predicted return to work, using the prediction model estimated in Table A1; the dependent variable splits predicted work at the median of the distribution. We find that in fact relabeling is associated with a decrease in predicted return to work, which is unsurprising given the results in Table 4, while in fact there is a positive impact on actual return to work. This strongly suggests that selection isn't driving our finding.

Anticipation

One possible issue with our identification strategy is that the individuals might have anticipated the reform and thus changed their behavior already before the changes in incentives and labels. This would cause a twofold issue for us: the behavior in the baseline years would be mismeasured, and there could be selection bias due to the attrition caused by anticipation of the reform.

To assess the role of anticipation, in Figure 8, we replicate Figure 5. However, in Figure 8 we show the change in retirement rates between 2003 and 2004, before the reform, compared to the incentive and relabeling changes that each age group will face in 2005. The figure shows that there is hardly any systematic change in retirement behavior in anticipation of the reform.

Part V: Conclusion

Focal retirement ages are a central feature of Social Security programs around the world. These focal ages can play an important role in setting retirement expectations and norms. As such, they provide a powerful tool for policy makers who are interested in reforming retirement systems to address the growing funding shortfalls facing these systems around the world. But these tools often come hand in hand with significant changes in the financial structure of Social Security that can have independent, and potentially deleterious, impacts on retirees. A natural question is whether simply relabeling key retirement ages, holding financial incentives constant, can drive retirement behavior.

In this paper, we use a major reformulation of the retirement system in Finland to investigate the independent effects of retirement age labeling on behavior. A relabeling of retirement ages with modest and continuous changes in financial incentives allows us to separately estimate the impact of relabeling from financial incentives in driving retirement decisions.

We find that relabeling is particularly powerful as a determinant of date of retirement. Both graphical evidence and estimated hazard models reveal an enormous change in retirement when individuals face a newly defined "normal retirement" age. Our findings suggest that such relabeling is as powerful as enormous changes in pension wealth or dynamic pension retirement incentives.

We also present a new approach to assessing the welfare implications of induced earlier retirement: looking at the impact on return to work. We show that the marginal workers induced to retire by relabeling are much more likely to return to work over the next three years than is the typical worker. This suggests that there is a marginal increase in regret among those who respond to this change in retirement ages – suggesting a potential source of welfare loss from inducing excess retirement.

While more work needs to be done to assess the importance of worker regret, there is clear revealed preference evidence of *policy maker* regret in Finland, as this policy was repealed in steps. The stated reason for the reform, as with the 2005 reform, was to increase effective retirement ages (Government proposal, 2015). Starting from cohort 1955, each new cohort will have their full retirement age raised by 3 months until the new retirement age of 65 is reached by cohort 1962. Cohorts 1965 and after will have their retirement age based of life expectancy. Also, the higher accrual rate of 4.5% will be repealed and the monthly increase of 0.4% per month for delaying retirement beyond the full retirement age will be brought back.

Finally, a limitation of the analysis in this paper is that we cannot separate changes in worker retirement choices due to this policy change from potential employer responses to the law change that might independently impact retirement. A priority for future work in this area is to assess employer responses to this dramatic change in order to incorporate into a fuller analysis of the overall effects of relabeling.

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Figures and Tables

Table 1: Descriptive statistics

Variable	Mean	SD
Female	0.27	0.44
Spouse	0.77	0.42
Tertiary education	0.32	0.47
Net wealth (excl. pension wealth)	62,378	420,132
Maximum annual earnings in past 3 years	34,438	77,807
Sickness absence in past 3 years	0.32	0.46
For 2005 (the reform year):		
Immediate increase in pension wealth, %	6.63	2.58
Increase in marginal accrual rate, % of pension wealth	-0.71	1.96
Reach full retirement age in 12 months	0.79	0.41

Notes. Descriptive statistics for the sample in the main specification (col (1) of Table 2).

Table 2: Cox proportional hazard model regressions.

Dependent variable: Old-age retirement	Treatment: Earnings-related pension only	Robustness 1: only financial, age and year controls	Robustness 2: only age and year controls	Control: Mostly national pension
	(1)	(2)	(3)	(4)
Immediate increase in pension wealth, %	0.104*** (0.0195)	0.107*** (0.0194)	0.104*** (0.0154)	0.129*** (0.0236)
Increase in marginal accrual rate, % of pension wealth	-0.0648*** (0.0109)	-0.0603*** (0.0107)	-0.0813*** (0.0074)	-0.1023*** (0.0080)
Reach full retirement age in 12 months	2.052*** (0.122)	2.047*** (0.122)	2.057*** (0.098)	0.1828 (0.112)
Sickness in past 3 years	0.121*** (0.023)			0.195*** (0.0414)
Has spouse	0.070*** (0.027)			0.0594 (0.0423)
Tertiary education	-0.002 (0.031)			-0.052 (0.0452)
Female	0.126*** (0.027)			-0.239*** (0.0427)
Log pension wealth	0.572*** (0.098)	0.369*** (0.093)		-1.143*** (0.107)
Log accrual rate (prop. to pension wealth)	-0.409*** (0.092)	-0.338*** (0.062)		0.592*** (0.049)
Year and monthly age controls	Yes	Yes	Yes	Yes
N	25,088	25,088	25,088	8,201

*Notes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Years covered: 2003–2004 (control) 2005 (treatment). The estimated model is the Cox proportional hazard model regression (Equation 1). The dependent variable is the time (0 to 1) of old-age retirement within one-year period. The threshold refers to the limit when individuals have only earnings-related pension. Control variables are monthly age, non-pension wealth decile, having been on sick leave in past three years, having a spouse, tertiary education, female, pension wealth at the beginning of the year in logs, marginal accrual rate assuming no reform in logs and year dummies.*

Table 3: Cox proportional hazard model regressions for returning to work, within the sample of retirees.

	Main specification	Specification Check
Dependent variable	Returning in 3 years	Predicted Returning: Binarized at median
	(1)	(2)
Immediate increase in pension wealth, %	-0.0222 (0.0202)	-0.0064 (0.0109)
Increase in marginal accrual rate, % of pension wealth	0.0005 (0.0213)	-0.0061 (0.0119)
Reach full retirement age in 12 months	0.3902** (0.183)	-0.0876 (0.105)
N	11,092	11,092

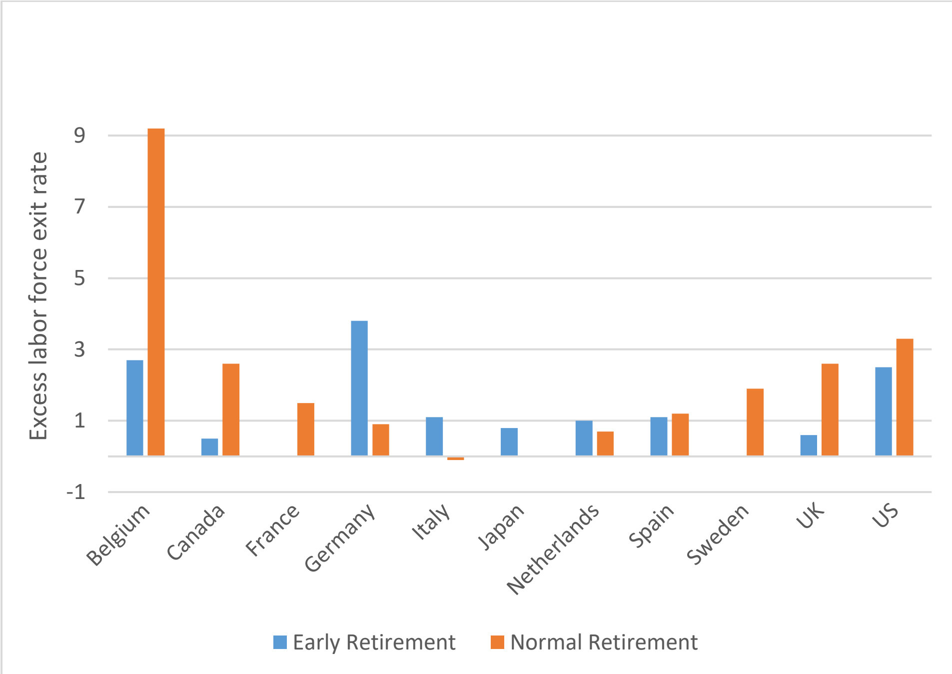
*Notes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Years covered: 2003–2004 (control) 2005–2006 (treatment). The estimated model is the Cox proportional hazard model regression (Equation 1). The sample is those who retired in control or treatment years. The dependent variable in column (1) is an indicator of whether the individual returned to the labor market in the following three years earning at least 25% of their maximum earnings of past three years. The dependent variable in columns (2) and (3) is the predicted return to labor market using the prediction model estimated in Table A1. Control variables are monthly age, non-pension wealth decile, having been on sick leave in the three prior years, having a spouse, tertiary education, female, pension wealth at the beginning of the year in logs, marginal accrual rate assuming no reform in logs and year dummies.*

Table 4: Cox proportional hazard model regressions with covariates as dependent variable, within the sample of retirees.

Dependent variable	Sickness absences	Primary education	Tertiary education	First wealth tercile	Third wealth tercile	Female	Having a spouse	Spouse working
	Covariates as dependent variable							
	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Immediate increase in pension wealth, %	0.0018	-0.0009	-0.0132	-0.0095	-0.0075	-0.0211	-0.0098	-0.0005
	(0.0146)	(0.0118)	(0.0159)	(0.0140)	(0.0147)	(0.0159)	(0.0094)	(0.0094)
Increase in marginal accrual rate, % of pension wealth	0.0014	-0.0070	-0.0059	0.0015	-0.0158	-0.0027	0.0028	0.0166
	(0.0077)	(0.0094)	(0.0144)	(0.0123)	(0.0146)	(0.0094)	(0.0071)	(0.0123)
Reach full retirement age in 12 months	0.0693	-0.1462	-0.2159**	0.1337	-0.2591***	-0.3110***	0.0181	0.0442
	(0.128)	(0.111)	(0.105)	(0.124)	(0.111)	(0.113)	(0.076)	(0.153)
N	11,092	11,092	11,092	11,092	11,092	11,092	11,092	11,092

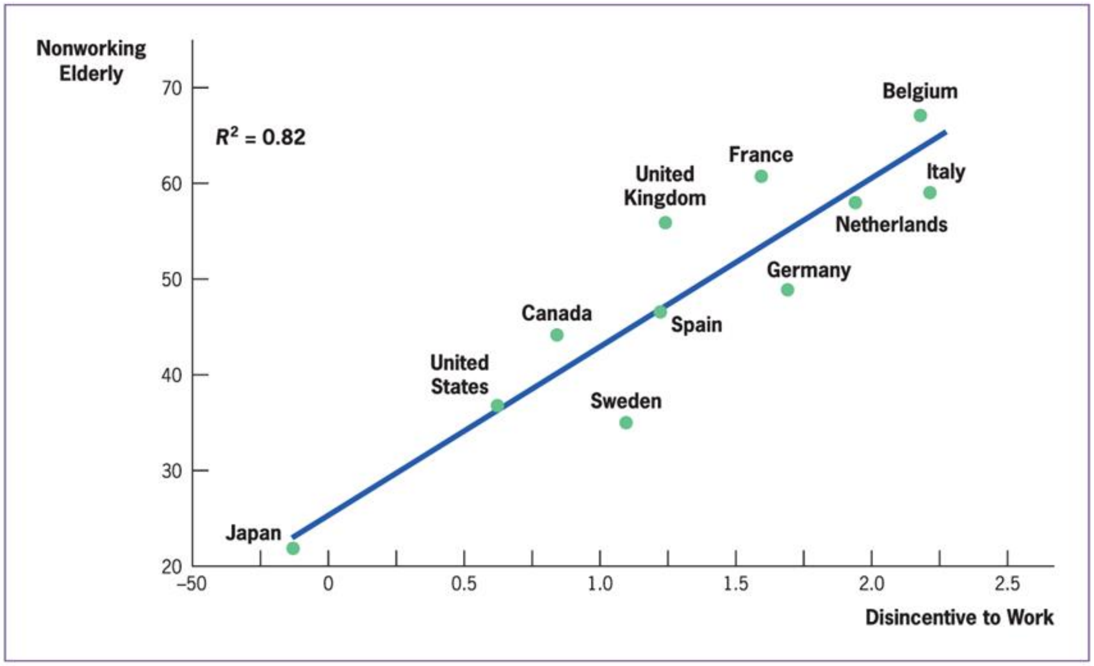
*Notes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Years covered: 2003–2004 (control) 2005–2006 (treatment). The estimated model is the Cox proportional hazard model regression (Equation 1). The sample is those who retired in control or treatment years. The dependent variable is the named covariate. Control variables are monthly age, non-pension wealth decile, having been on sick leave in the three prior years, having a spouse, tertiary education, female, pension wealth at the beginning of the year in logs, marginal accrual rate assuming no reform in logs and year dummies. In each regression, the other covariates of the same category are not controlled, e.g., for primary education, tertiary education is not controlled.*

Figure 1: Excess labor force exit at retirement ages.



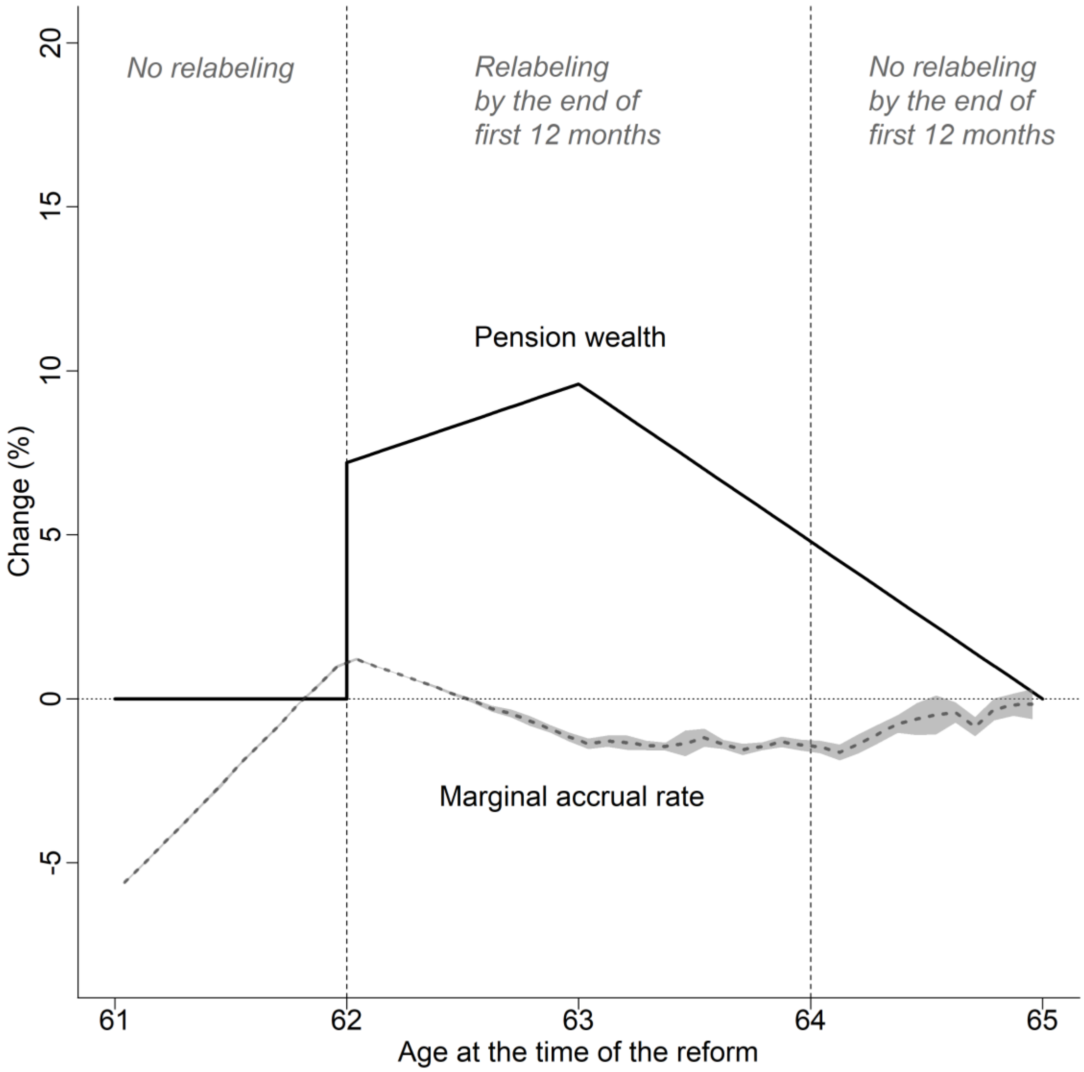
Notes. The y-axis depicts the conditional labor force exit rate at early retirement and normal retirement as a multiple of the average of the retirement rate in the year before and after. Data source: Gruber, J. and David W. (1999).

Figure 2. Nonworking elderly and disincentives to work by country.



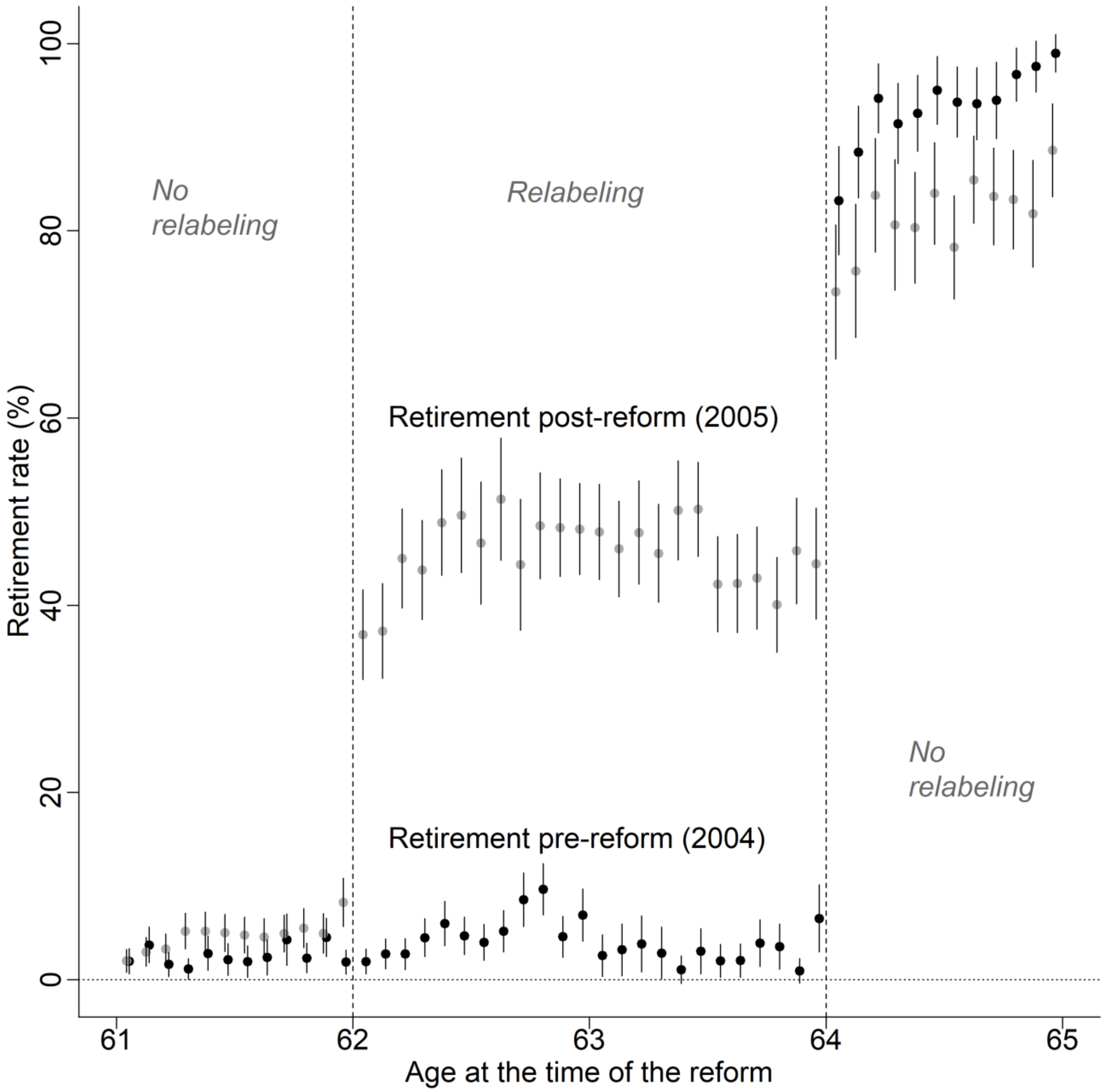
Notes. Nonworking elderly (vertical axis) is measured as the percentage of the population that is not working from ages 55 to 65, on average. The disincentive to work (horizontal axis) is measured as the natural logarithm of the sum of implicit taxes on work at all ages from the early retirement age to age 69. Source. Gruber, J. and David W. (1999); Gruber (2019).

Figure 3: The effect of the reform on pension incentives and labeling



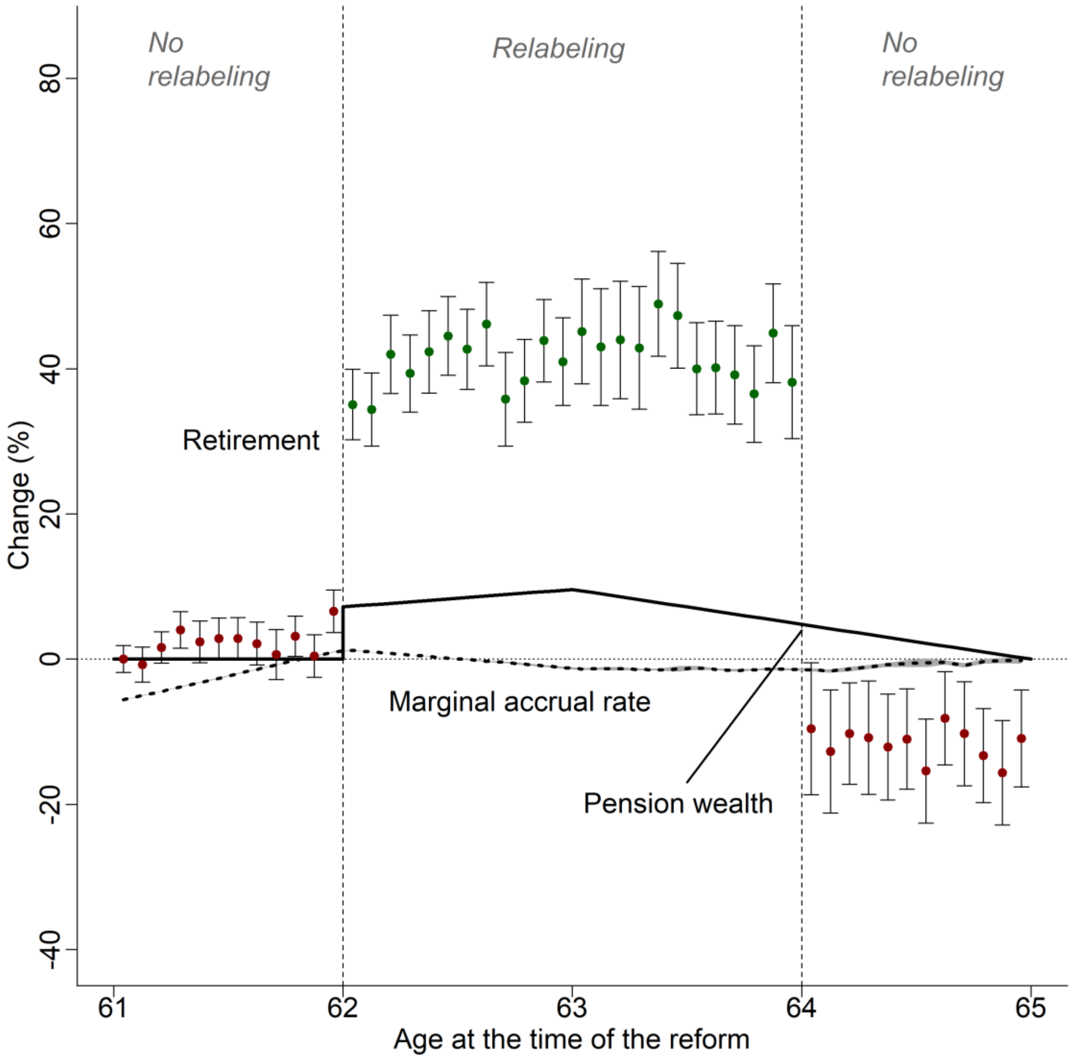
Notes. Pension wealth, if retired immediately, increased on January 1, 2005 due to the reform as a function of age. Marginal accrual rate as a proportion of accrued pension calculated for a 12-month period changed due to the reform as a function of age, earnings and accrued pension. Relabeling is defined as reaching full retirement age due to the 2005 reform in 12 months. The means are estimated for bimonthly birth bins. The 95% confidence intervals are shown in the shaded area. The cohorts represented in the x-axis are 1940–1943.

Figure 4: Retirement rates by bimonthly birth bins in 2004 (pre-reform) and 2005 (post-reform).



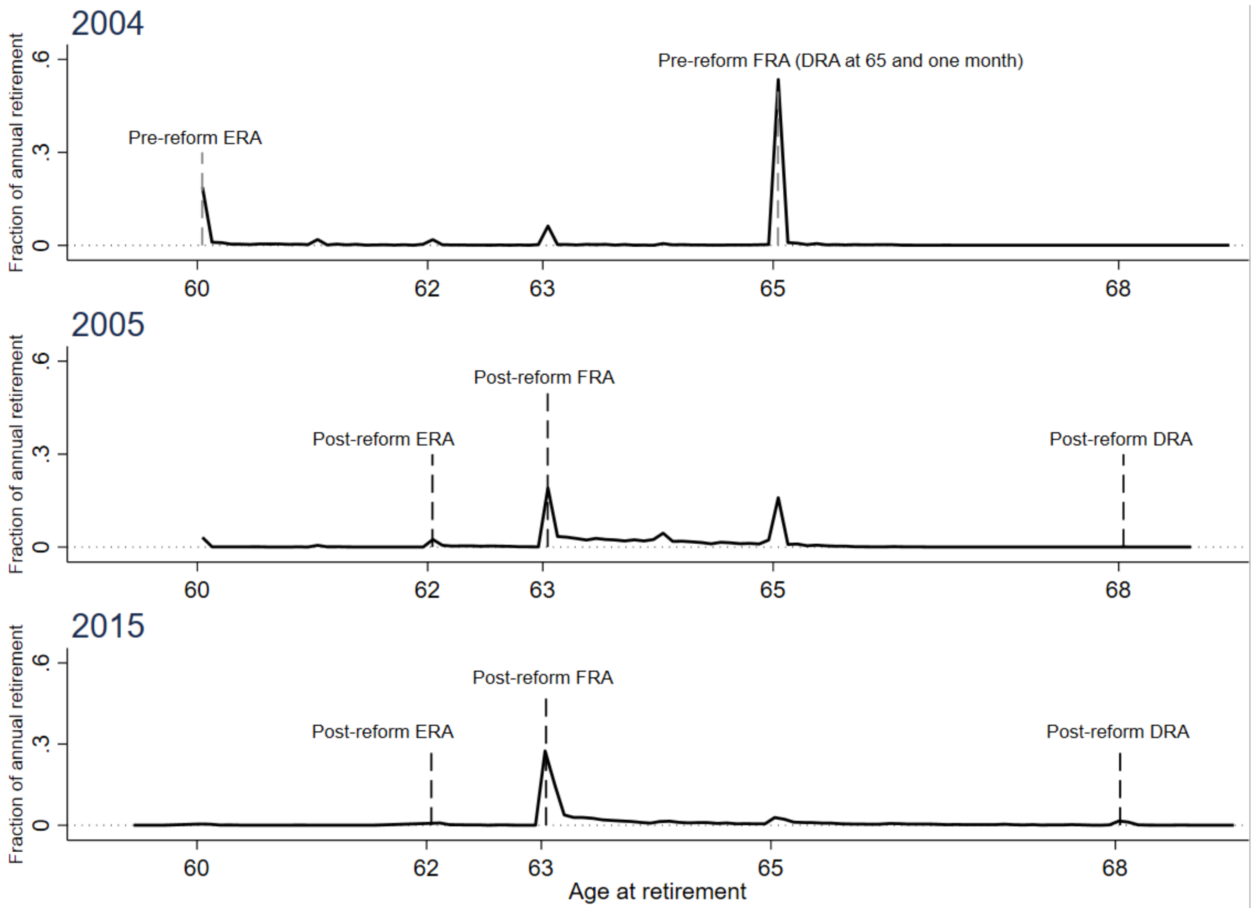
Notes. Retirement is measured as claiming old-age pension. Relabeling is defined as reaching full retirement age due to the 2005 reform in 12 months. The means are estimated for bimonthly birth bins. The 95% confidence intervals are shown by the vertical bars. The grey dots depict retirement rates for the cohorts 1940–1943 (in 2005), black dots for the cohorts 1939–1942 (in 2004).

Figure 5: Pension incentives, labeling and retirement rates in 2005 vs 2004.



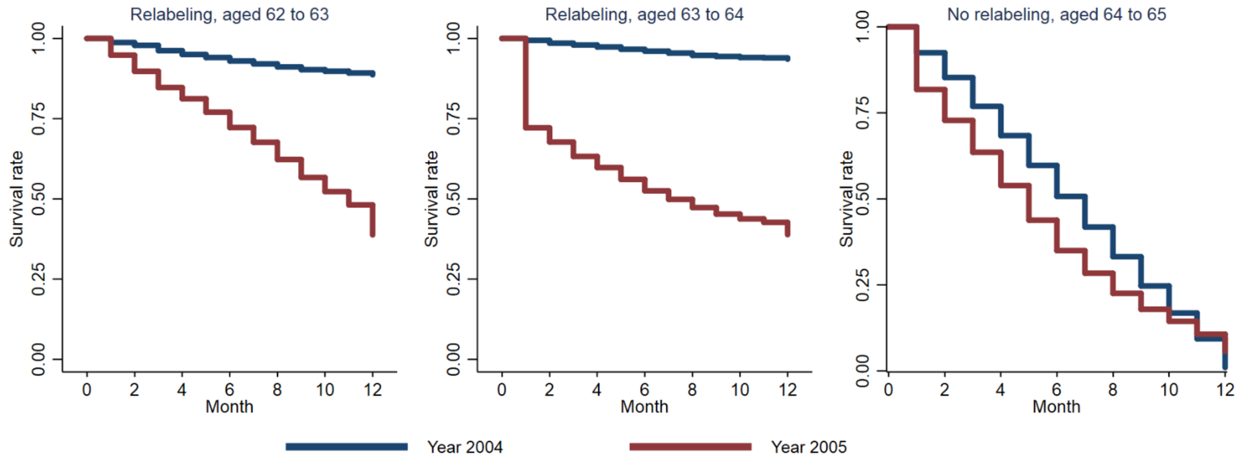
Notes. Pension wealth increased on January 1, 2005 due to the reform as a function of age. Marginal accrual rate as a proportion of accrued pension calculated for a 12-month period changed due to the reform as a function of age, earnings and accrued pension. Relabeling is defined as reaching full retirement age due to the 2005 reform in 12 months. The means are estimated for bimonthly age bins. Retirement is measured as claiming old-age pension and estimated as a t-test of the difference in 2005 and 2004 for monthly birth bins. The 95% confidence intervals are shown by the error bars.

Figure 6: Retirement fractions at different ages.



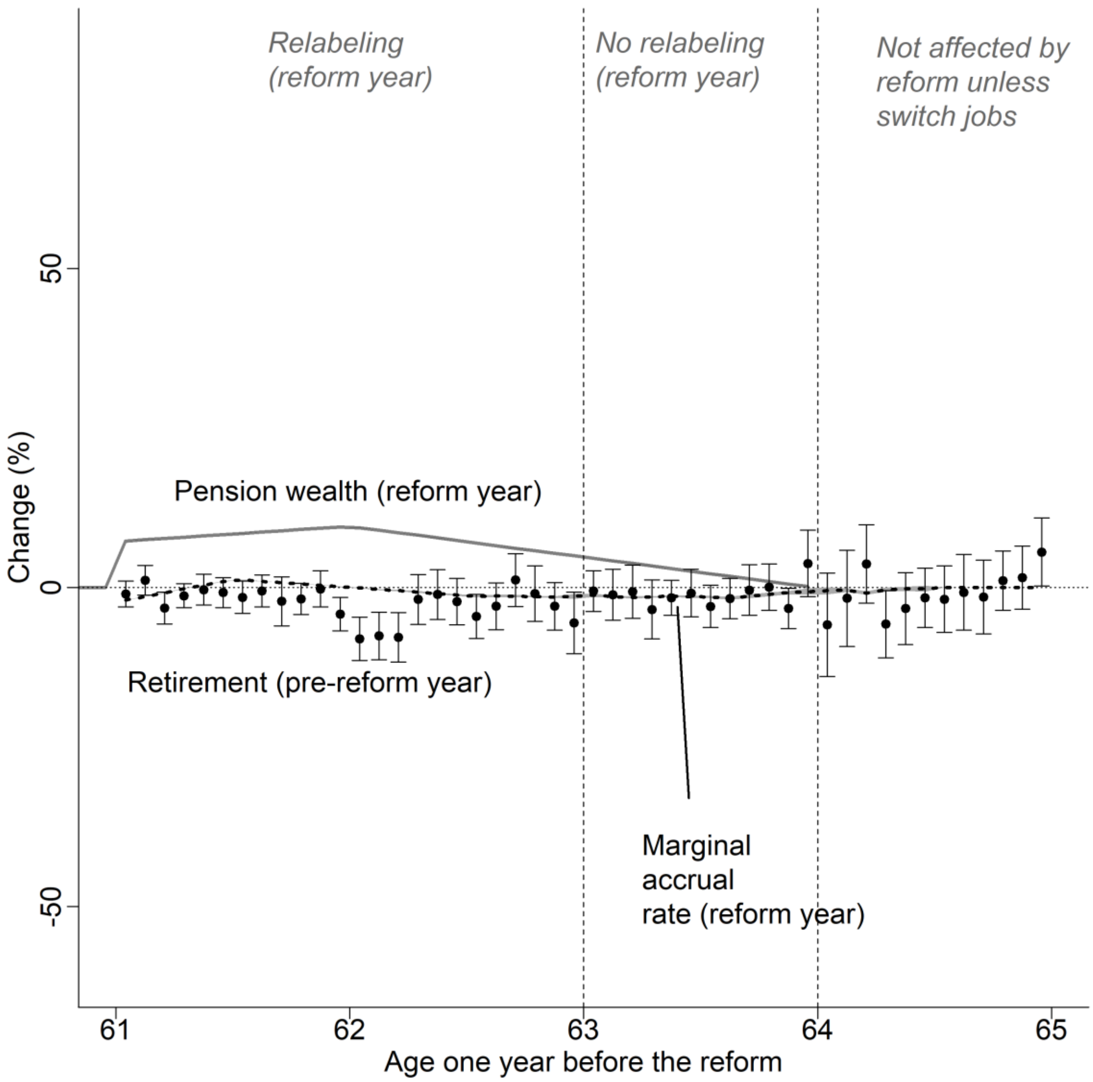
Notes. Fraction of those who retired (claimed old-age pension) during each year by monthly age bins in 2004, 2005 (the year of the reform) and 2015. The reform in 2005 raised early retirement age (ERA) from 60 to 62 and lowered full retirement age (FRA) from 65 to 63. Delayed retirement age (DRA) was raised from 65 years and one month to 68 years and one month. No significant reforms for old-age pension took place between 2005 and 2015.

Figure 7: Kaplan-Maier survival estimates by relabeling status and year.



Notes. The sample is working population at each age. Survival is measured as not retiring (claiming old-age pension) during the calendar year. The reform came into effect in 2005, lowering full retirement age (FRA) from 65 to 63. ***First panel:*** *In 2005, the depicted age bracket reached FRA as they turned 63.* ***Second panel:*** *In 2005, the depicted age bracket reached FRA in January.* ***Third panel:*** *In 2005, the depicted age bracket would have reached FRA even without the reform. However, the reform affected the timing of reaching FRA. In 2004, they reached FRA as they turned 65. In 2005, they reached FRA in January.*

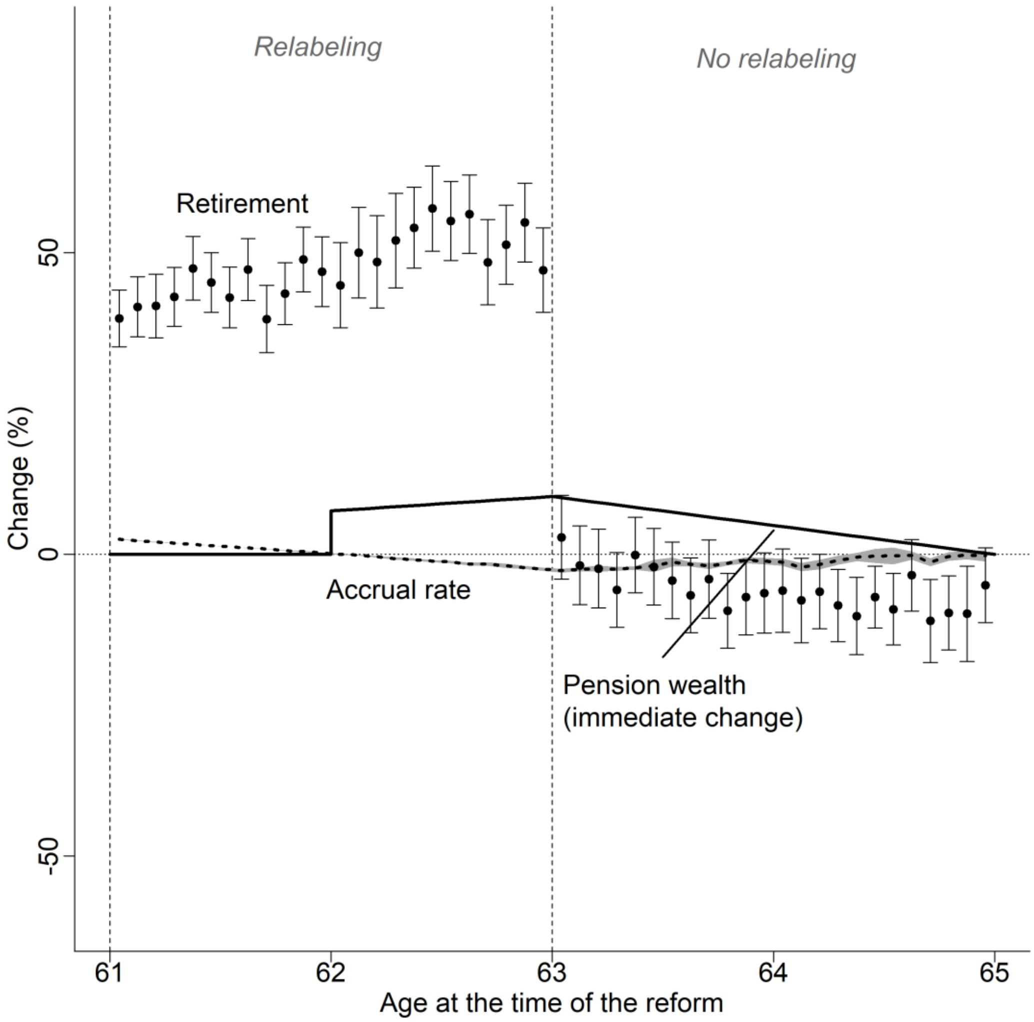
Figure 8. *The anticipation effect – Pension incentives, labeling and retirement rates in 2004 vs 2003.*



Notes. The figure depicts changes in retirement patterns one year before the reform (2004) and changes in incentives and labeling in the reform (2005). Pension wealth increased on January 1, 2005 due to the reform as a function of age. Marginal accrual rate as a proportion of accrued pension calculated for a 12-month period changed due to the reform as a function of age, earnings and accrued pension. Relabeling is defined as reaching full retirement age due to the 2005 reform in 12 months. The means are estimated for bimonthly age bins. Retirement is measured as claiming old-age pension and estimated as a t-test of the difference in 2004 and 2003 for monthly birth bins. The 95% confidence intervals are shown by the error bars.

Appendix Tables and Figures

Figure A1: The effect of the reform on pension incentives, labeling and retirement rates by monthly birth bins in 2005–2006 vs 2003–2004.



Notes. Pension wealth increased on January 1, 2005 due to the reform as a function of age. Marginal accrual rate as a proportion of accrued pension calculated for a 24-month period changed due to the reform as a function of age, earnings and accrued pension. The means are estimated for monthly age bins. The 95% confidence intervals are shown in the shaded area. Retirement is estimated as a t-test of the difference in 2005–2006 and 2003–2004 for monthly birth bins. The 95% confidence intervals are shown by the error bars.

Table A1: Pre-reform regression for selection correction prediction.

Dependent variable: Returning to labor market	Only year 2003
	(1)
Sickness absences in past 3 years	-0.380*** (0.109)
Primary education	-0.056 (0.115)
Tertiary education	0.361*** (0.124)
First net wealth tercile	0.289*** (0.109)
Third net wealth tercile	0.355*** (0.110)
Female	-0.052 (0.110)
Has spouse	0.071 (0.109)
Spouse not retired	0.011 (0.109)
Monthly age controls	Yes
N	3,573

*Notes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Years covered: 2003. The estimated model is the Cox proportional hazard model regression (Equation 1). The dependent variable is returning to labor market in the next 3 years at 25% of maximum earnings of the past three years. Control variables are monthly age, non-pension wealth tercile, having been on sick leave in the three prior years, having a spouse, having a non-retired spouse, primary and tertiary education and female.*