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DISCUSSION PAPER SERIES

IZA DP No. 10839

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ABSTRACT

Long-Term Effects of Extended Unemployment Benefits for Older Workers*

This paper examines the long-term effects of extended unemployment benefits that older unemployed can collect until retirement in Finland. We consider a reform that increased the age threshold of this scheme from 55 to 57 for people born in 1950 or later. Our regression discontinuity estimates show that postponing eligibility by two years increased employment over the remaining working career by seven months. Despite the corresponding reduction in unemployment, we find no evidence of significant effects on mortality or receipt of disability and sickness benefits, nor on the spouse's labor supply. We also compute the fiscal impact of the reform taking into account income taxes and social security contributions paid and benefits received. The reform increased net income transfers by 15,000 Euros over the 10-year period for an average individual.

JEL Classification: J26, J63, J64, J65

Keywords: unemployment insurance, early retirement, layoffs

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1 Introduction

In many European countries, longer entitlement periods of unemployment insurance (UI) benefits are provided for older unemployed. These benefit extensions can contribute to high levels of unemployment in the oldest groups in two ways. First, employers tend to target dismissals at those employees who qualify for extended benefits. Some eligible workers may also leave their job voluntarily to obtain more leisure time if the benefit scheme is sufficiently generous. As a result, the oldest workers eligible for extended benefits are found to enter unemployment at a high rate (Winter-Ebmer, 2003, Kyyrä and Wilke, 2007, Tuit and van Ours, 2010, and Baguelin and Remillon, 2014). Second, among unemployed workers eligibility for extended benefits discourages job search and thereby prolongs unemployment spells, and such benefits are often used to bridge the time until retirement (Hunt, 1995, Lalive and Zweimüller, 2004, Kyyrä and Ollikainen, 2008, Lalive, 2008, Tatsiramos, 2010, Baguelin and Remillon, 2014, and Inderbitzin et al., 2016). While the effects of extended benefits on the unemployment inflow and the duration of unemployment spells are well documented, their long-term effects, fiscal impact and effects on health and spouse's behavior have been studied less.

In this paper, we study a reform in 2005 that postponed access to extended UI benefits by two years from age 55 to 57 in Finland. The Finnish scheme is rather generous in the sense that the extended benefits can be received until retirement. We find that the two-year increase in the eligibility age increased employment by 7 months between the ages of 54 and 63 among private-sector workers. Despite a large reduction in the time spent in unemployment, we find no evidence of health effects as measured by the amount of disability and sickness benefits received, and mortality by the end of 2015. Moreover, the spouse's employment months and retirement age do not appear to be affected. As such, the only effect seems to be higher employment for the group that was directly affected by the reform. This employment effect amounts to an increase of 22,000 Euros in wage income and a decrease of 11,000 Euros in unemployment-related benefits on average over the 10-year period. Higher earnings also reduce the need to rely on other benefits, such as housing allowance and social assistance, while increasing taxes and social security contributions paid to the society. Taking all benefits, income taxes and social security contributions into account we estimate that the fiscal impact of the reform was around 15,000 Euros over the 10-year period for an average individual. This is a large effect given that the counterfactual level of average net transfers over the same period is about 26,000 Euros.

We reach these conclusions using a sharp regression discontinuity design (RDD) and rich register data on all private-sector employees born in 1949 and 1950. The 2005 reform only affected individuals who were born in 1950 or later. As a result, the 1950 cohort

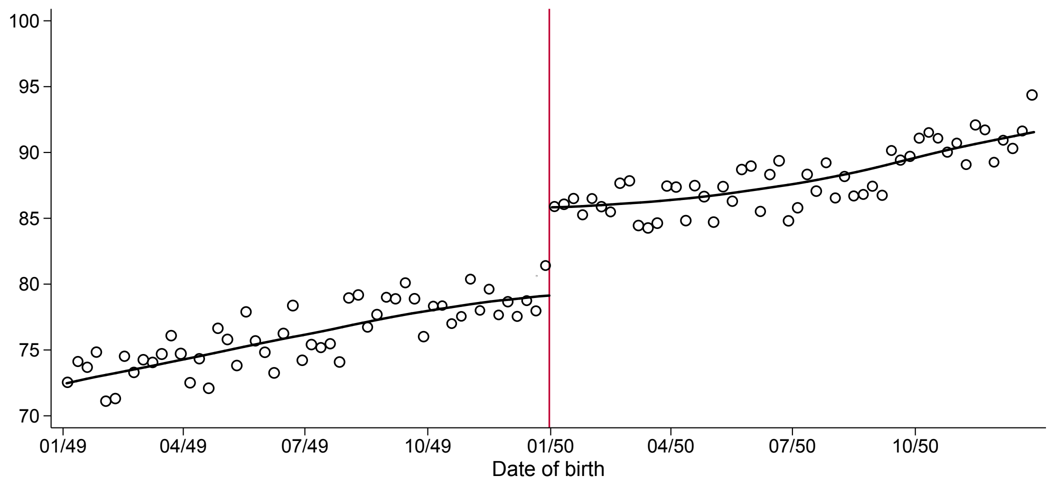

Figure 1: Months employed in 2004–2013 by birth week along with a local linear smoother

became entitled to extended benefits two years older than the 1949 cohort at the age of 57. Our research design and one of the main results are illustrated in figure 1 which depicts the average employment months over the period 2004 to 2013 for each 1-week birth cohort around the cutoff value of January 1, 1950. The reform provides an ideal setting for RDD analysis given that the individuals cannot manipulate their eligibility status which is based on the birth date. We also show that the individuals on both sides of the cutoff are highly similar in observable characteristics, and therefore it is likely that they do not differ significantly in terms of unobservables either. Despite this homogeneity, those born in 1950 worked on average notably more than those born in 1949 over the 10-year period. The jump of 7 months at the cutoff value is quite large in relation to the only two-year difference in the minimum eligibility age for extended benefits. This implies that the extended benefit scheme as a whole must have a large negative impact on employment of the oldest worker groups.

Our results are in line with previous research showing that the risk of job loss is higher for workers who can qualify for extended benefits and that those unemployed who are entitled to extended benefits experience much longer unemployment spells.[1] Our findings complement the existing studies by providing evidence of the full impact of these employment effects in the form of changes in employment months and wages over the remaining working career. We also provide evidence of the extent to which various income transfers counteract the wage losses. The previous literature has not been able to assess

[1]In the Finnish context, Kyyrä and Wilke (2007) show that the unemployment risk of private-sector workers at least doubles at the minimum eligibility age, and Kyyrä and Ollikainen (2008) estimate that approximately one half of unemployed workers eligible for the benefit extension withdraw from job search entirely.

fiscal impacts at such a detailed level. In addition, we contribute to the literature by examining broader impacts of extended benefits in terms of health, program substitution and spouse's labor supply.

Some previous studies have found a positive association between unemployment and mortality (e.g. Sullivan and von Wachter, 2009, and Eliason and Storrie, 2009), but to the best of our knowledge the effects on older unemployed with access to extended benefits have not been studied. Since extended benefits essentially secure the income for an unemployed person until retirement and extended benefits can be regarded as an early retirement scheme, it may be that many of the negative effects of unemployment on an individual's health may be absent. This could explain why we find no change in mortality despite the large drop in unemployment months.

Unlike some other studies (e.g. Inderbitzin et al., 2016), we find that eligibility for extended UI benefits has no effect on receipt of sickness and disability benefits. Furthermore, although several studies have found that the labor supply decisions of older couples are interrelated (e.g. Blau, 1998, Michaud, 2003, Zweimuller et al., 1996, and Lalive and Parrotta, 2016), we find no effect on spouse's behavior even though the extended benefits act as a pathway to early retirement for many unemployed workers. It could be that the involuntary nature of unemployment for most affected workers causes differences compared to the setting in which both spouses' decisions concern regular retirement.

The rest of the paper proceeds as follows. In the next section we describe the unemployment-related benefits for older workers in Finland. Section 3 presents our research design and section 4 describes our data. Section 5 reports our estimation results and in section 6 we discuss the robustness of these results. The final section concludes.

2 Institutional setting

In Finland, UI benefits are paid by unemployment funds. Membership is voluntary, but as many as 90% of all workers are enrolled in unemployment funds. The benefit level is determined by the average earnings over the past 34 weeks of employment. Unlike in most other countries, there is no cap in the benefit level, but the replacement rate declines rapidly with the past earnings. Those who are not eligible for UI benefits may qualify for a flat-rate labor market subsidy paid by the Social Insurance Institution. This benefit is means-tested but available indefinitely for the needy.

During our observation period, unemployment fund members with sufficient work history who lost their job were entitled to 100 weeks of UI benefits provided that they had registered as an unemployed job seeker at the public employment service. However, older unemployed constitute an exception as those above a certain age threshold on the day when their regular benefits expire (and who have been working for at least 5 years in the past 20 years) qualify for extended benefits which can be received until retirement. The age threshold for the benefit extension has been raised gradually over time. It was increased from 55 to 57 in 1997 for all workers, from 57 to 59 years in 2005 for those born in or after 1950, from 59 to 60 in 2010 for those born in or after 1955, and from 60 to the current 61 in 2014 for those born in or after 1957.

In addition, long-term unemployed individuals born before 1950 were entitled to an unemployment pension between the ages of 60 and 64, which was then followed by conventional old-age pension at the age of 65. The unemployment pension was abolished in 2005 but only from later cohorts. The combination of regular and extended UI benefits (and unemployment pension for those born before 1950) is known as the "unemployment tunnel" (UT) scheme. The changes in eligibility for this scheme over time by cohort and age are illustrated in figure 2.

In this study, we focus solely on workers born in 1949 and 1950, of whom those born in the latter year were affected by the 2005 reform. Unemployed workers born in 1949 who reached the age of 57 before running out of their regular benefits were allowed to collect extended benefits until the age of 60, after which they could claim unemployment pension. This means that a newly unemployed individual had to be at least 55 years and 1 month of age when becoming unemployed to be eligible for the UT scheme. As a consequence of the 2005 reform, the individuals born in 1950 had to be at least 57 and 1 month at the time of unemployment entry in order to be able to receive UI benefits until old-age pension. These individuals could move freely into old-age pension between the ages of 62 and 65. The two-year increase in the age threshold was the major change in the 2005 reform, albeit two other changes may have played some role as well.

First, as pointed out above, the unemployment pension was abolished and replaced by additional weeks of extended UI benefits for the individuals born in 1950 or later. The compensation level of unemployment pension was determined by previous earnings but over a longer period than that used for determining the UI benefit. As a result, the unemployment pension could be either higher or lower than the preceding UI benefit depending on the individual's earnings profile. In most cases, the levels of these benefits were close to each other but the unemployment pension was on average somewhat higher than the UI benefit. The UT scheme thus provided a slightly lower average benefit level for the 1950 cohort than for the 1949 cohort, which may have reduced attractiveness of the scheme as an early exit pathway among individuals born in 1950.

Second, large employers are partially liable for the costs of extra benefits received by their former employees under the UT scheme, and the way these liabilities were determined differs between individuals born in 1949 and 1950. When an extended benefit is granted

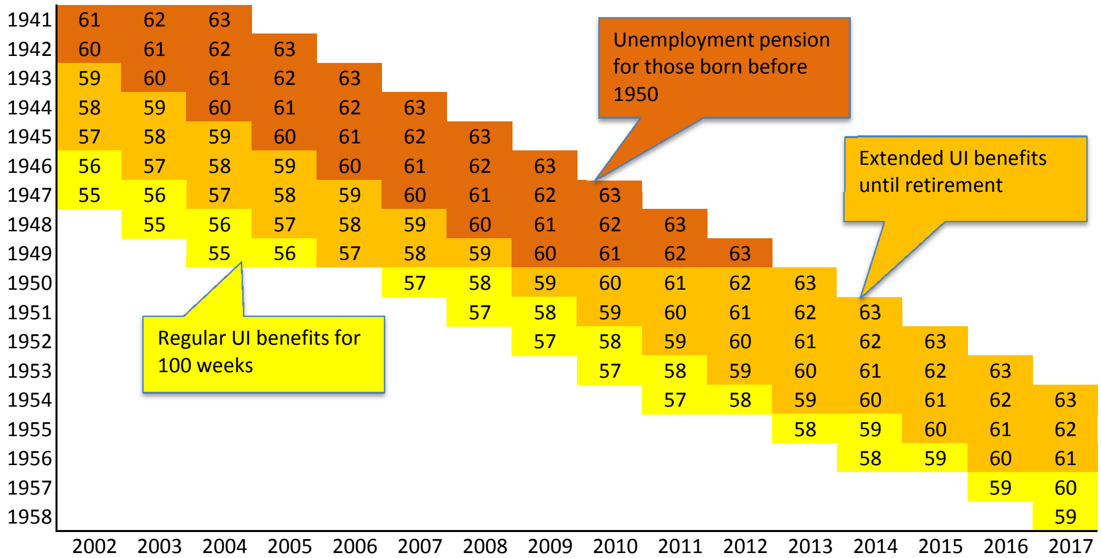

Figure 2: Eligibility for the unemployment tunnel scheme by cohort and year

to the worker born in 1950 or later, the former employer may have to pay a given share of the extended benefit costs as a lump sum payment to the Unemployment Insurance Fund. This cost share increases linearly from 0% to 80% as a function of firm size.[2] In the case of the long-term unemployed born before 1950, the former employer had to pay a similarly defined share of the unemployment pension costs to the pension provider. The reason for the change in the employer liabilities was purely technical and dictated by the fact that the unemployment pension was abolished from the later cohorts. However, the employer's costs from their former employees drawing benefits under the UT scheme have been slightly lower on average for individuals born in 1950 than for those born in 1949. This may have induced large employers to lay off their older workers born in 1950 more easily than those born in 1949.[3]

3 Research design

Given that the 2005 reform only affected individuals born in 1950 or later, we adopt a sharp regression discontinuity design around the birth time cutoff of January 1, 1950 (e.g.

[2]The cost of extended benefits is calculated assuming the worker will collect them until age 63 (the minimum old-age retirement age since 2005) irrespective of the actual behavior. In the case of a worker who qualifies for an old-age pension before the regular benefits expire, the former employer is liable for a share of the costs of regular benefits actually paid to the worker.

[3]Hakola and Uusitalo (2005) analyze a reform that changed the premium rates in 2000 and find that the experience rating of the unemployment pension costs reduced early exits from work among older workers.

Lee and Lemieux, 2010). In particular, we estimate nonparametric local linear models of the form

$$y_i = \alpha + \gamma \mathbf{1}\left[d_i \geq d_0\right] + \beta_1(d_i - d_0) + \beta_2(d_i - d_0)\mathbf{1}\left[d_i \geq d_0\right] + \varepsilon_i, \qquad (1)$$

where y_i is an outcome for individual i (e.g. months employed or earnings), d_i is birth date, and $\mathbf{1}\left[d_i \geq d_0\right]$ is an indicator for those born on January 1, 1950 ($= d_0$) or later. Under the identifying assumption that the disturbance term ε_i does not have a discontinuity at d_0, γ provides an estimate of the local average treatment effect for the 2005 reform, i.e. the effect of becoming eligible for the UT scheme at age 57 instead of age 55 (with the unemployment pension replaced by additional days of extended UI benefits) on individuals born in the first day of 1950. Control variables for background characteristics of the individuals are not required for identification but their inclusion may improve precision. We report results both with and without control variables.

To implement the RDD approach we need to choose the kernel function for weighting the observations and the bandwidth to determine the sample size around the cutoff. The choice of the kernel function makes little difference in practice. We use the triangular kernel that is widely used in applied work. Calonico et al. (2014) argue that commonly used bandwidth selectors tend to yield bandwidths that are too large to ensure the validity of the underlying distributional approximations, potentially leading to non-negligible bias. They propose an alternative method where the RDD point estimate is corrected by an estimated bias term, and the standard error estimates are adjusted for additional variability that results from the estimation of the bias correction term. We report both the conventional and bias-corrected point estimates. We select the bandwidth such that the point estimator for the bias-corrected estimate is mean square error (MSE) optimal using a procedure developed by Calonico et al. (2016). We also study the sensitivity of our results to the chosen bandwidth and polynomial order.

4 Data and descriptive statistics

We use the Finnish Linked Employer-Employee Data (FLEED) of Statistics Finland for years 2001 to 2013. This database combines information from several administrative registers, and it covers the entire Finnish population between the ages of 15 to 70 as well as the universe of private-sector firms. The data provides information on earnings, employment, unemployment, education and some characteristics of the employer. We supplement this data by merging additional information from other databases of Statistics Finland. We obtain information on deaths up to the end of 2015 from Population Statistics and Cause-of-death Statistics. This enables us to consider mortality as an outcome. In addition we merge information on various benefits received and taxes paid by individuals and households from Total Statistics on Income Distribution. These data are based on information from different authorities, including the Social Insurance Institution, the Financial Supervisory Authority, the Finnish Center for Pensions and the Tax Administration. The benefit records include unemployment benefits, pension benefits, sickness and disability benefits, housing allowance and social assistance, and the tax records include labor and capital taxes and social security contributions paid by employees. With this information we can assess how the 2005 reform affected public finances through increased taxes and social security contributions and reduced benefit payments caused by higher employment. The resulting net transfers variable does not however account for social security contributions that are paid by employers.

The FLEED also includes an identifier for each individual's spouse, which we use to link spouse's records from the above registers to the individuals in our sample.[4] This enables us to analyze spillover effects on spouse's behavior and overall effects at the household level. We consider two measures of spouse's labor supply: the retirement age and months worked in the years 2004 to 2013. For couples we compute household earned income by taking the sum of both spouses' earned income (i.e. all taxable income except capital income). This measure ignores the earned income of children and other people possibly living in the same household. In addition, we consider household disposable income which captures the role of the tax and income transfer system. Unlike the household earned income, this measure includes the incomes of all individuals living in the same household.

We limit our analysis to individuals born in 1949 or 1950 who worked in the private sector in 2001, that is, 3 or 5 years prior to eligibility to the UT scheme. This sample includes 62% of both cohorts. The unemployment risk in the public sector was very low for these cohorts and the UT scheme is mainly used by large private-sector firms when downsizing. We also run our main analyses using data on workers employed in the public sector in 2001, but did not find evidence of statistically significant effects of the 2005 reform for them. We do not report these results but they are available on request.

Figure 3 displays the number of observations by birth week. Seasonal variation in birth dates shows up in the frequencies. However, there is no bunching of observations around January 1, 1950, which is not surprising given that the manipulation of the birth date in the administrative register data is impossible (our research design also passes a formal McCrary density test).

Table 1 displays sample statistics by birth year. Our sample includes 67,352 individuals, of whom 33,941 were born in 1949 and 33,411 in 1950. In the analysis of long-term

[4]Spouses in our data set are defined as couples who are cohabiting, married or have a registered civil partnership (i.e. same sex marriage).

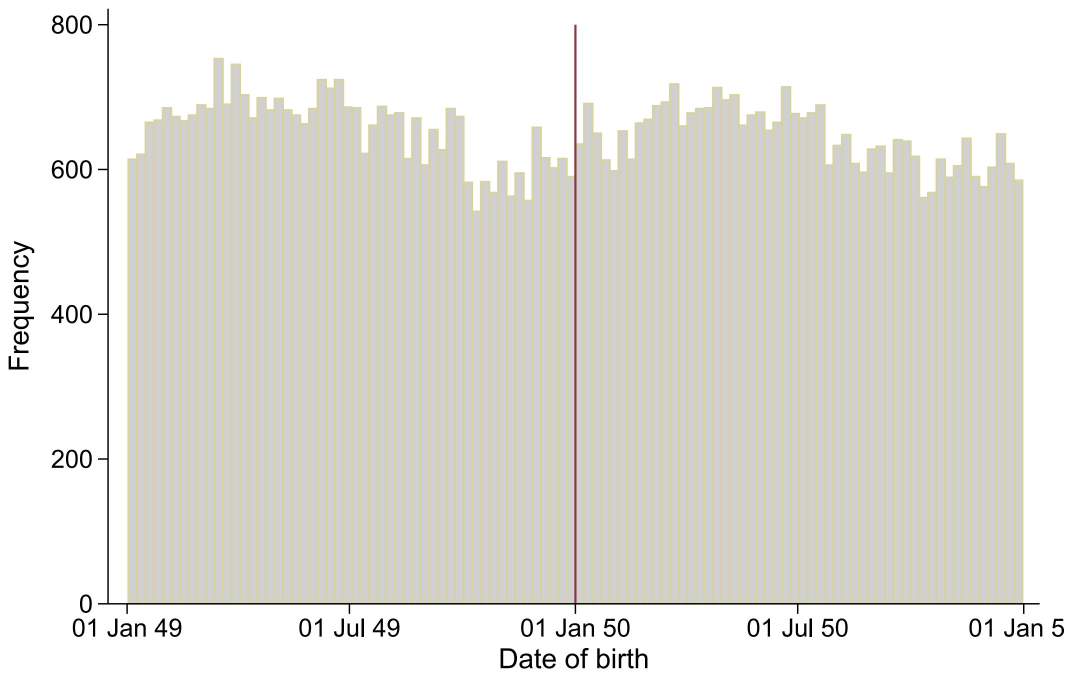

Figure 3: Number of observations per week by birth week

effects we only include those for whom we observe the outcomes in all years between 2004 and 2013, which reduces the sample size by 5.3%. As seen in panel A, individual characteristics such as gender, geographical area and education which are correlated with earnings and other outcomes are almost identical for the two cohorts. In order to assess this more rigorously we also show RDD estimates for the jump at the beginning of 1950 using a six months bandwidth. The point estimates for the discontinuity are all statistically insignificant for the afore mentioned characteristics. Since large firms in particular use the UT scheme as a soft way to get rid off workers when they downsize (Kyyrä and Wilke, 2007), we also examine the share of individuals working in firms with over 250 employees. This share appears to be almost the same for the two cohorts and no jump is detected.

A nice feature of our research design is that we can examine outcomes prior to the reform in order to check whether other discontinuities at the birth date threshold could cause discontinuities in the outcomes, although we are not aware of any other reform affecting only one of the two cohorts. Panel B of table 1 displays means for our outcome variables in 2003, when neither cohort was eligible for the UT scheme. These sample means are remarkably similar across the cohorts. Earned income, which is basically all taxable income except capital income, as well as annual wages have similar means in the two cohorts and the RDD estimates indicate no discontinuity at the threshold. The same is true for different types of benefits. We also consider net transfers, i.e. the difference

Table 1: Descriptive statistics by birth year

	Born in 1949		Born in 1950		Discontinuity at January 1, 1950	
	Mean	SD	Mean	SD	Coefficient	SE
A. Covariates in 2003						
Share female, %	42.9	49.5	43.1	49.5	0.20	1.20
Share living in major cities, %	34.8	47.6	35.0	47.7	-1.38	1.08
Share with tertiary education, %	25.8	43.8	25.7	43.7	0.97	1.19
Share working in large firms, %	32.5	46.8	32.1	46.7	1.31	1.23
B. Individual outcomes in 2003						
Months employed	10.7	3.3	10.8	3.2	-0.10	0.07
Wages (annual)	35,806	24,635	36,056	24,455	13	705
Months unemployed	0.6	2.1	0.6	2.0	0.02	0.05
Unemployment benefits	810	2,885	806	2,854	78	76
Earned income (annual)	40,686	23,373	40,767	23,206	279	659
Sickness and disability benefits	980	3,747	899	3,618	84	103
Net transfers	10,710	13,160	10,752	12,966	-534	362
Observations	33,941		33,411		33,170	
C. Household outcomes in 2003						
Spouse's age	53.8	4.4	52.9	4.4	-0.09	0.14
Spouse's months employed	9.3	4.7	9.5	4.5	0.19	0.14
Household earned income	77,501	33,823	77,675	33,225	653	1,147
Household disposable income	63,238	28,078	63,257	27,535	309	864
Observations	22,325		21,990		21,187	
D. Individual outcomes in 2004–2013						
Months employed	76.1	39.1	87.9	35.8	6.70***	0.73
Wages	233,191	206,015	271,095	214,479	22,957***	4,120
Unemployment months	19.5	31.4	13.2	22.5	-7.90***	0.53
Unemployment benefits	27,336	43,951	18,091	29,787	-11,052***	737
Earned income	344,440	203,913	358,532	207,615	9,892**	4,049
Sickness and disability benefits	20,640	48,712	22,755	49,213	449	950
Net transfers	19,429	121,523	47,087	121,239	13,488***	2,377
Share deceased by 2015, %	6.9	25.4	6.3	24.4	-0.45	0.57
Observations	32,048		31,709		31,404	
E. Household outcomes in 2004–2013						
Spouse's retirement age	59.3	5.5	59.1	5.7	-0.64	0.44
Spouse's months employed	73.5	44.8	79.0	43.9	0.11	1.08
Household earned income	668,468	300,616	688,987	302,882	8,657	7,370
Household disposable income	567,698	250,482	579,937	251,541	7,585	6,071
Observations	22,325		21,990		21,187	

Notes: All incomes are in 2013 Euros. Unemployment benefits also include unemployment pension. Earned income is all taxable income except capital income, i.e. it includes also all taxable benefits. Net transfers are calculated as the difference between income taxes and social security contributions paid and benefits received, with the tax component including all labor and capital taxes and the benefit component consisting of unemployment benefits, pension benefits, sickness and disability benefits, housing allowance and social assistance. The share deceased is calculated based on the sample observed in 2003. The sample for the spouse's retirement age only includes the spouses who retired by 2014. This includes 16,525 spouses for the 1949 cohort and 14,688 spouses for the 1950 cohort. The last two columns show RDD estimates for the discontinuity at the cutoff based on the triangular kernel and a six month bandwidth on both sides of the cutoff. Significance levels: *** 1%, ** 5% and * 10%.

between income taxes and social security contributions paid and benefits received as an outcome. Taxes paid include all labor and capital taxes, and social security contributions consist of health insurance, unemployment insurance and pension contributions. Benefits received include unemployment benefits, pension benefits, sickness and disability benefits, housing allowance and social assistance. The means of net transfers are similar in the two cohorts and no discontinuity is detected at the threshold.

Previous research for other countries has shown that educational attainment and, consequently, labor market outcomes can be affected by season of birth through compulsory schooling laws or relative age effects (e.g. Angrist and Krueger, 1991, and Plug, 2001).[5] This is a cause of concern because our analysis relies on comparing individuals born at the very end of 1949 to individuals born early in 1950. However, the lack of significant discontinuities in educational level (panel A) and pre-reform outcomes (panel B) should mitigate such concerns.

As we will also study spouses' outcomes, table 1 also shows the age of the spouse in 2003 (panel C). There could plausibly be selection such that the spouse is more likely to be from the same cohort and thereby affected by the 2005 reform in the same way. This may indirectly influence the outcomes of the sample members if the labor market outcomes of the spouses are interrelated. However, no discontinuity in the spouse's age is observed at the cutoff. Also the pre-reform outcomes for the spouses and at the household level in 2003 appear similar in the two cohorts with no significant discontinuities at the cutoff (panel C). It should be noted that in the analysis of spouse and household outcomes we only include individuals who are observed with the same spouse during our whole observation period from 2004 to 2013. This reduces the sample size for both the cohorts by about one third.

The outcomes over the years 2004 to 2013 at the individual and household levels are reported in panels D and E. There are notable differences in unemployment and employment months, wages, unemployment benefits and net transfers between individuals born in 1949 and 1950. However, none of differences in the household-level outcomes in the subsample of the couples is statistically significant at the conventional risk levels. We will return to these differences in the next section where we discuss them at length.

Table 2 shows characteristics and outcomes for individuals in each cohort conditional on whether they experienced any unemployment during the years 2004 to 2013. In both cohorts 49% of the individuals experienced at least one spell of unemployment during the 10-year period. The key difference is that job losers in the 1949 cohort remained unemployed for about one year longer than those born in 1950 (39 vs. 27 months). These

[5] Kaila (2017) studies short-term effects of the relative school starting age in Finland and finds that those who are born early in the calendar year and therefore start school at a relatively older age are more likely to be admitted to and graduate from academic track high school.

Table 2: Sample means conditional on any unemployment during 2004–2013 by birth year

	1949		1950	
Experienced unemployment in 2004–2013:	No	Yes	No	Yes
A. Covariates in 2003 (%)				
Share female	43.6	42.2	43.8	42.3
Share living in major cities	38.8	30.6	38.7	31.1
Share with tertiary education	30.8	20.6	30.3	20.9
Share working in large firms	29.9	35.2	30.1	34.2
Industry:				
Agriculture, hunting, forestry and fishing	1.9	1.7	1.6	1.6
Mining and quarrying	0.3	0.2	0.3	0.2
Manufacturing	21.7	37.4	22.8	38.0
Utilities	0.7	0.4	0.5	0.4
Construction	7.3	7.5	7.7	7.4
Wholesale and retail trade, repairs	17.2	12.5	17.0	11.7
Hotels and restaurants	1.9	2.4	2.2	2.4
Transport and communication	8.5	5.5	8.4	5.3
Financial intermediation	6.0	2.5	5.9	2.0
Real estate, business services	12.2	7.6	12.2	8.1
Public administration and defense	0.8	0.3	0.8	0.3
Education, health and social work	7.8	4.2	7.7	4.1
Other community, social and personal service activities	7.0	2.8	7.1	3.1
Other	1.2	0.8	1.1	0.9
Observations	17,330	16,611	16,983	16,428
B. Individual outcomes in 2004–2013				
Months employed	91.4	60.5	98.5	77.2
Months unemployed	0.0	39.4	0.0	26.6
Wages	281,037	184,373	307,617	234,162
Earned income	391,194	296,737	400,429	316,165
Unemployment benefits	0	55,227	0	36,385
Sickness and disability benefits	27,485	13,655	27,227	18,232
Net transfers	61,253	-23,245	79,834	13,973
Share deceased by 2015 (%)	7.8	6.0	7.1	5.5
Observations	16,185	15,863	15,943	15,766
C. Household outcomes in 2004–2013				
Spouse's retirement age	59.5	59.1	59.2	58.9
Spouse's months employed	76.2	70.4	81.6	76.3
Household earned income	728,722	603,761	742,437	632,670
Household disposable income	622,498	508,847	630,311	526,862
Observations	11,540	10,785	11,268	10,722

longer unemployment periods are of course reflected in the amount of unemployment and employment-related incomes received over the years 2004 to 2013, leading to a substantial gap in the net transfers. In particular, the unemployed in the 1949 cohort received on average 23,000 Euros more in various benefits than they paid in income taxes and social security contributions whereas those born in 1950 paid 14,000 Euros more in taxes and social security contributions than they received in benefits.

In panel A of table 2, differences in the background characteristics in 2003 between individuals who ended up in unemployment at some point and those who did not are highly similar for the two cohorts. This suggests that the selection process into unemployment is similar except that those born in 1949 became unemployed at younger ages. The higher share of manufacturing employees among those with at least one unemployment spell is related to the more extensive use of the UT scheme in the large manufacturing firms. In both cohorts the share deceased is slightly smaller for those with unemployment experiences, which is a somewhat unexpected observation.

5 Results

5.1 Individual outcomes

Figure 4 displays how employment and unemployment evolve from 2003 to 2011 for individuals born in 1949 and 1950. The dots represent the average of employment and unemployment months for each 1-week birth cohort around the cutoff value of January 1, 1950 and the solid lines show local linear smoothers. In 2003, when neither cohort was eligible for the extended benefits, there is no difference between those born in different years. In other years there are notable differences between individuals born at the end of 1949 and those born at the beginning of 1950, and these increase from 2005 to 2008 by which time the 1950 cohort had become eligible. In relative terms, the gap in employment between the cohorts increases over all years as the employment level declines with age. In 2008, for example, the individuals born in January 1950 worked roughly one month more (10% more) and spent the same amount of time less in unemployment (about 50% less) than those born in December 1949. In 2011 many of those born in 1949 were on unemployment pension and the average of the sum of months unemployed and months on unemployment pension is shown separately. It is quite striking that an average worker in the 1949 cohort spent almost one third of the year collecting unemployment-related benefits.

We examine the long-run effects of the 2005 reform graphically in figure 5 and report the RDD estimates for the same outcomes in table 3. Figure 5a shows the total amount of months employed during the 10-year period from 2004 to 2013. As would be expected

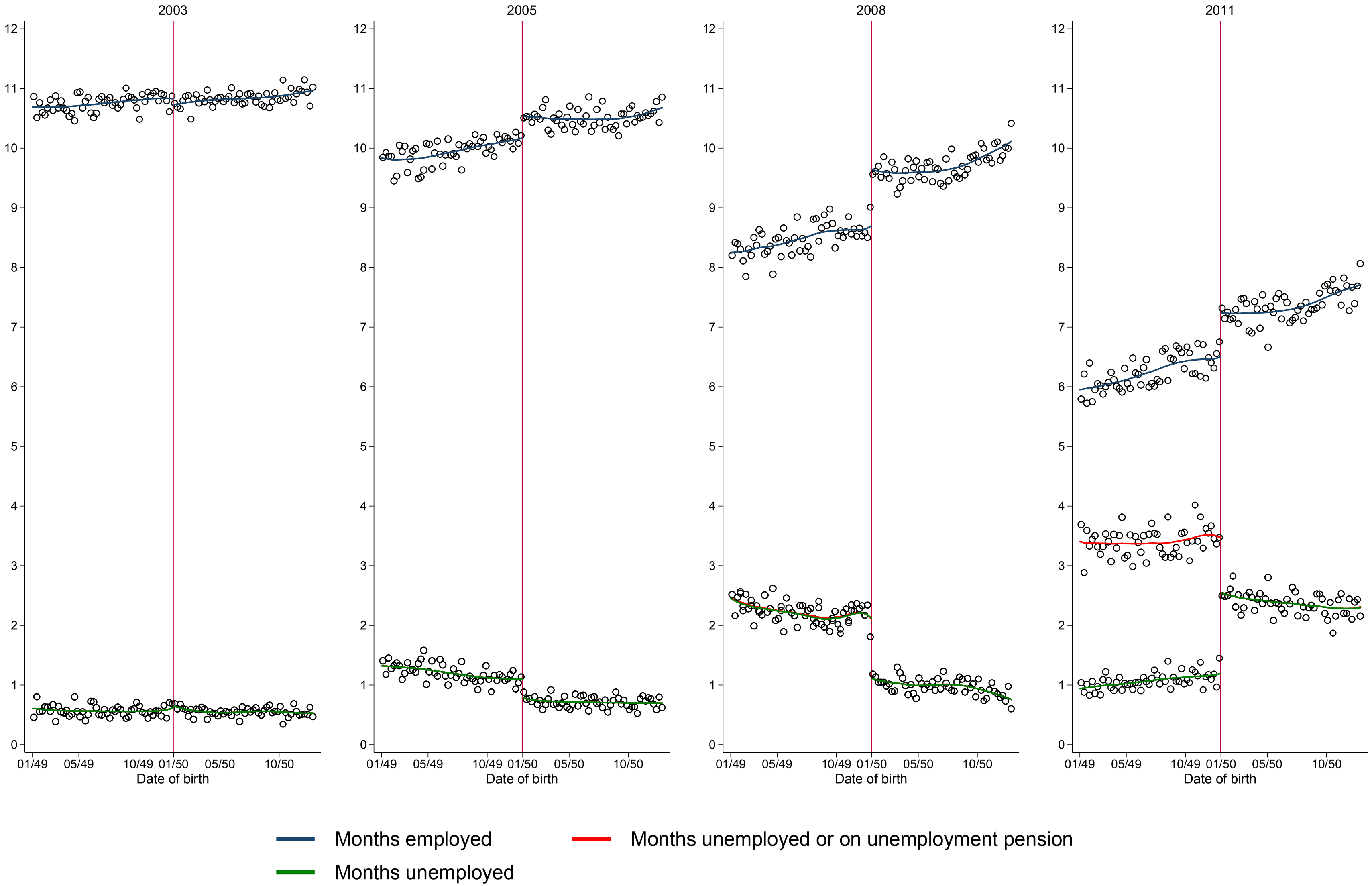

Figure 4: Average employment and unemployment months within a year by birth week along with a local linear smoother

from the yearly graphs in figure 4, there is a clear jump in total employment months at the cutoff and the estimates in column 1 of table 3 imply that the two-year increase in the age limit for extended benefits leads to those born in early 1950 working approximately 7 months (9%) more during the 10-year period. These estimates are robust to the inclusion of covariates. In line with the increase in months employed the reform also increases total wages over the 10-year period by about 9% (figure 5b and column 2 of table 3). Correspondingly, those born after the cutoff spend over 8 months less time unemployed (figure 5c and column 3 of table 3), which implies 40% lower receipts of unemployment-related benefits (figure 5d and column 4 of table 3).[6]

The difference in total earned income at the cutoff is less clear in figure 5e but the estimates in column 5 of table 3 are significant and imply that the reform led to an increase of just under 4% in earned income over the 10-year period. As earned income includes both labor income and unemployment benefits, the opposite effects of the reform on these two outcomes counteract each other and lead to a more subdued effect on earned income.

The UT scheme acts as an early retirement scheme for many unemployed in the sense that only a small share of job losers entitled to extended benefits return to employment before old-age pension. One concern is that the scheme is a close substitute for other early retirement options, mainly for disability benefits that are payable to all working age individuals with a diagnosed disability.[7] If this is the case, restrictions in the access to the UT scheme can increase the disability inflow, mitigating the employment effect of such changes. Another possibility is, of course, that those not entitled to extended benefits end up taking more sick leave. Uusitalo and Nivalainen (2013) and Kyyrä (2015) do not find evidence that the past increases in the age threshold would have had notable spillover effects on the inflow to disability benefits in Finland. Our analysis is consistent with these previous findings given that the point estimates for the effects on combined sickness and disability benefits are small and not statistically significant in column 6 of table 3.

As our data include comprehensive information on income taxes and social security contributions paid and all benefits received, we are able to assess how the increases in employment caused by the reform affect the net amount that individuals contribute to public finances.[8] Figure 5g and column 7 of table 3 indicate that raising the eligibility

[6] The cumulative unemployment benefits displayed here also include unemployment pension.

[7] Inderbitzin et al. (2016) study the interaction between extended UI benefits and take-up of disability benefits and retirement benefits in Austria. They find program complementarity (i.e. increased take-up of UI followed by higher disability or retirement benefits) for one age group and program substitution (i.e. higher take-up of UI but lower take-up of disability insurance) for older age groups. Lammers et al. (2013) find evidence of a higher outflow to sickness and disability insurance schemes following stricter search requirements for older unemployed in the Netherlands.

[8] Some benefits, e.g. housing allowance, are paid at the household level. In our data such benefits are registered for the individual who applied for the benefit. This influences benefit amounts on both sides of the cutoff similarly and should therefore not pose a problem in our analysis.

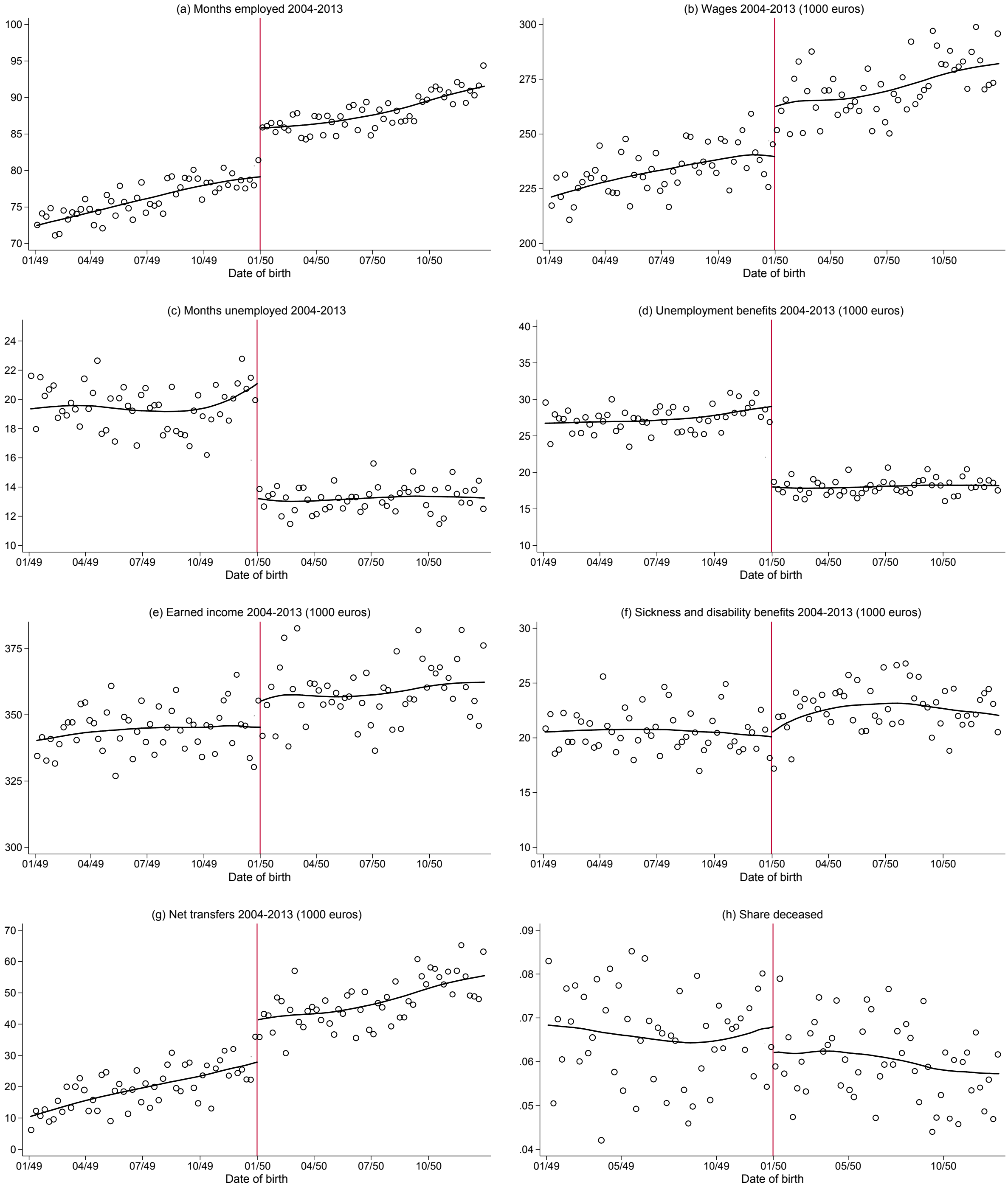

Figure 5: Average outcomes by birth week along with a local linear smoother

Table 3: Linear regression discontinuity estimates of the 2005 reform on cumulative individual outcomes over the years 2003–2014

	Months employed	Wages	Months unemployed	Unemployment benefits	Earned income	Sickness and disability benefits	Net transfers	Share deceased
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of dependent variable	78.5	240,617	19.9	28,547	346,348	20,626	25,614	
A. No covariates								
Conventional estimates	6.8***	22,983***	-8.1***	-11,048***	11,000**	85	13,468***	-0.004
	[1.0]	[5,220]	[0.8]	[855]	[4,699]	[1,252]	[3,077]	[0.005]
Bias-corrected estimates	7.1***	24,218***	-8.3***	-11,169***	11,217**	-270	13,035***	-0.005
	[1.1]	[6,165]	[0.9]	[1,025]	[5,652]	[1,463]	[3,683]	[0.006]
Bandwidth	170.4	178.9	141.3	226.4	215.9	157.2	175.5	290.1
Observations	29,598	31,019	24,368	39,743	37,794	27,265	30,494	53,669
B. With covariates								
Conventional estimates	7.1***	21,628***	-8.3***	-11,225***	12,425***	-110	14,866***	-0.003
	[0.9]	[3,563]	[0.8]	[902]	[3,686]	[1,251]	[2,628]	[0.005]
Bias-corrected estimates	7.5***	22,323***	-8.6***	-11,334***	13,211***	-504	15,345***	-0.005
	[1.0]	[4,235]	[1.0]	[1,080]	[4,380]	[1,451]	[3,133]	[0.006]
Bandwidth	173.8	302.9	131.2	198.6	262.8	153.7	192.5	294.7
Observations	30,138	53,136	22,581	34,761	46,095	26,510	33,608	54,449

Notes: Outcome variables are total amounts for years 2004–2013. Estimation and bandwidth selection are based on procedures discussed in Calonico et al. (2014) and Calonico et al. (2016). Polynomial order for bias correction is quadratic. Covariates include indicators for gender, for having a university degree, for living in major city and for being employed by a large firm in 2001. Bandwidths are mean square error optimal for the bias-corrected estimator and symmetric on both sides of the cutoff. Mean of the dependent variable is for individuals born in the last quarter of 1949. Robust standard errors reported for bias-corrected estimates. Significance levels: *** 1%, ** 5% and * 10%.

age of the UT scheme caused individuals born in early 1950 to pay 13,000 to 15,000 Euros (50% to 60%) more in net transfers to the society during the period from 2004 to 2013 than those born in late 1949. It should be noted that transfers here also include unemployment pensions and other pension payments.

Previous research has linked unemployment to increases in mortality (e.g. Sullivan and von Wachter, 2009, and Eliason and Storrie, 2009). To the extent that eligibility to extended benefits implies an increase in the incidence and length of unemployment, this could also have implications in terms of mortality. In the case of unemployed workers with access to extended benefits, the psychological and financial stress of unemployment may, however, be somewhat lower than for those not entitled to these benefits in earlier stages of working life. Furthermore, it is possible that some of the workers eligible for the UT scheme leave employment voluntarily through this scheme, i.e. the layoff decision may be based on a mutual agreement with the employer. Figure 5h and column 8 of table 3 indicate no significant effect of the 2005 reform on mortality, with the share deceased by 2015 displaying no statistically significant discontinuity at the cutoff. As potential other outcomes affected by the reform we also examined the probability that an individual migrates within the country or emigrates but the effects on these outcomes are not statistically significant (results not shown, available on request).

These findings confirm the observations in previous studies that those entitled to extended benefits are more likely to become unemployed and less likely to subsequently return to employment. The full impact of the two-year increase of the age limit for extended benefits on labor market outcomes is manifested in the substantially higher amounts of employment months and wages for those subject to the higher age limit. The fact that earned income is affected to a much lesser extent implies that the social transfer system compensates substantially for lower employment in the group who become entitled to extended benefits at a younger age. This can also be seen in the difference in net transfers over the 10-year period.

5.2 Spouse and household outcomes

Some studies, such as Blau (1998), Zweimuller et al. (1996), Michaud (2003), and Lalive and Parrotta (2016), indicate that couples' retirement decisions may depend on each other. It may be the case that when an individual becomes unemployed and is entitled to extended benefits and unemployment pension, this affects the labor supply of the spouse. Yet it is not clear, in which direction the effect would go. On the one hand, the decision may be associated with shared tastes for leisure and the spouse is more inclined to reduce

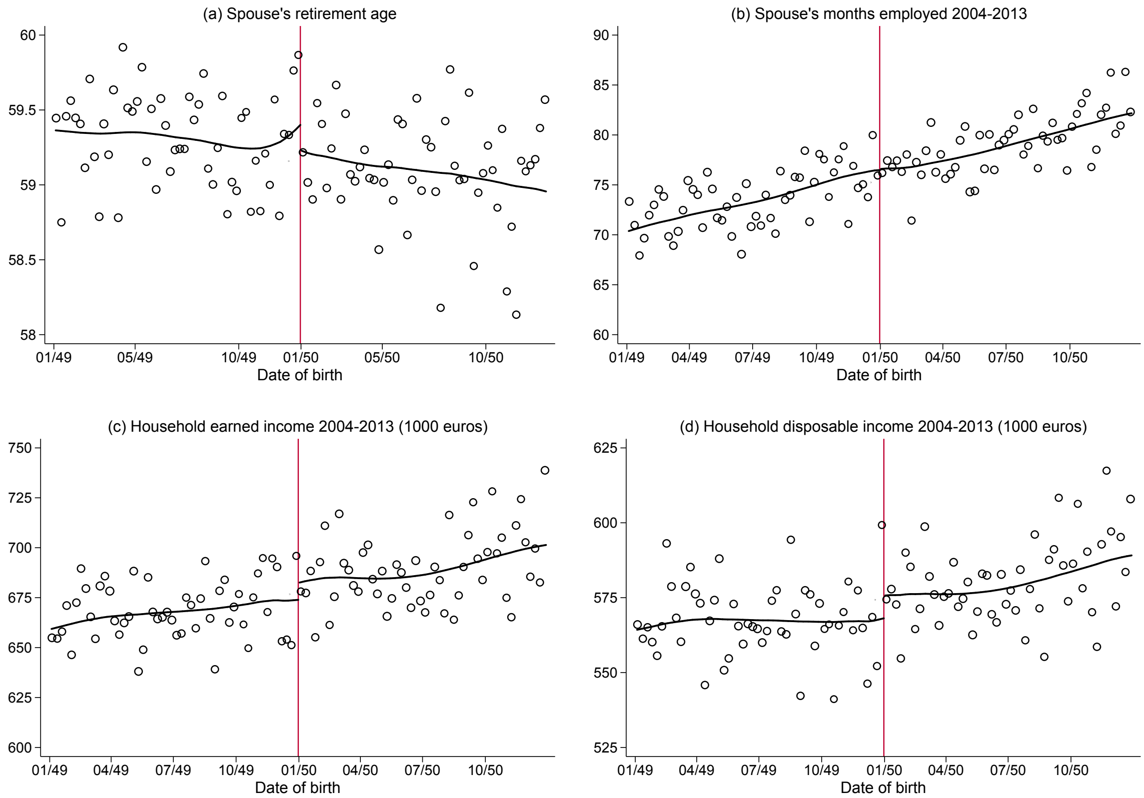

Figure 6: Average spouse and household outcomes by birth week along with a local linear smoother

labor supply when their partner gains access to the UT scheme.[9] On the other hand, financial concerns may induce the spouse to maintain or increase his or her labor supply when the partner is unemployed. For the older unemployed finding a new job can be particularly difficult and hence the risk of long-term unemployment would be high even in the absence of disincentive effects due to the extended benefit period.

Figure 6 and table 4 analyze outcomes for the spouses of our sample members and outcomes at the household level. The spouse's average retirement age in figure 6a shows no clear pattern and the estimates in column 1 of table 4 indicate that there is no statistically significant discontinuity at the cutoff. In line with this finding, the spouse's months employed do not appear to be affected by the increase in the age limit of extended benefits (figure 6b and column 2 of table 4). The household earned income in figure 6c shows a slight jump at the cutoff as would be expected from the estimates for the earned income of our target population, but the effect is muted by the earnings of the spouse and the point estimates for the discontinuity in column 3 of table 4 are not statistically significant. Yet

[9]For example, Lalive and Parrotta (2016) find that women reduce their labor force participation when their partner reaches pension eligibility.

the size of the effect is very close to the effect on the individual's earned income in column 5 of table 3, even though the estimates are obtained from somewhat different samples.

Our data also enable us to examine household disposable income, which is total household income net of all transfer payments. This measure therefore includes all income of the members of the household and all transfers that are determined at the household level, like housing allowance and social assistance. As such, it provides a measure of the full financial impact of the reform at the household level when taking into consideration all transfers received and paid. There is a slight discontinuity in household disposable income at the threshold, with households with a family member born in early 1950 displaying slightly higher disposable income than households of the 1949 cohort in figure 6d. However, the point estimates for the effect are not statistically significant in column 4 of table 4. It should be noted that the analysis of household outcomes only includes those who are observed with the same spouse during our whole observation period. This obviously reduces the sample and therefore statistical power.

We also considered as an outcome the probability that the spouse changes or is missing between 2004 and 2013 for those who had a spouse in 2003. This probability does not appear to be affected by the increase in the age limit of extended benefits and subsequent decrease in the incidence and length of unemployment (results not shown, available on request).

It is possible that there are gender differences in how extended benefits as an early retirement scheme affect the labor supply of the spouse. In addition, the relative age of the spouse can influence the extent to which the spouse can and will adjust his or her labor supply. We have examined these possibilities by running the spouse and household level analyses presented above separately for men and women as well as separately for couples where the spouse is younger/older than the individual in our analysis sample. The relative age of the spouse does not appear to affect the results and also in the case of gender the results remain largely unchanged. There is a decrease in the retirement age at the cutoff for female spouses, but the effect is not statistically significant and does not show up in the spouse's cumulative months employed. The effect of the two-year increase in the age limit for extended benefits has a positive and significant effect on household disposable income when the affected individual is male (results not shown, available on request). This can be related to men's higher wages and to the fact that the use of extended benefits as a soft way of downsizing is prevalent in male dominated occupations in the manufacturing sector.

Table 4: Linear regression discontinuity estimates of the 2005 reform on spouses' cumulative outcomes over the years 2004–2013

	Spouse's retirement age (1)	Spouse's months employed (2)	Household earned income (3)	Household disposable income (4)
Mean of dependent variable	59.3	76.0	673,069	566,107
A. No covariates				
Conventional estimates	-0.1	-0.1	9,152	7,792
	[0.2]	[1.3]	[9,083]	[7,779]
Bias-corrected estimates	-0.2	0.0	9,478	7,705
	[0.2]	[1.6]	[10,951]	[9,441]
Bandwidth	205.2	197.3	195.2	187.4
Observations	17,522	22,826	22,559	21,556
B. With covariates				
Conventional estimates	-0.1	-0.4	7,760	6,921
	[0.2]	[1.3]	[7100]	[6466]
Bias-corrected estimates	-0.2	-0.3	8,388	7,306
	[0.2]	[1.5]	[8566]	[7856]
Bandwidth	214.6	204.7	245.1	216.4
Observations	18,306	23,656	28,429	25,065

Notes: Outcome variables are total amounts for years 2004–2013. Estimation and bandwidth selection are based on procedures discussed in Calonico et al. (2014) and Calonico et al. (2016). Polynomial order for bias correction is quadratic. Covariates include indicators for gender, for having a university degree, for living in major city and for being employed by a large firm in 2001. Bandwidths are mean square error optimal for the bias-corrected estimator and symmetric on both sides of the cutoff. Mean of the dependent variable is for individuals born in the last quarter of 1949. Robust standard errors reported for bias-corrected estimates. Significance levels: *** 1%, ** 5% and * 10%.

6 Robustness

In order to examine the robustness of the results presented in the previous section, we perform a number of sensitivity analyses. Figure 7 displays bias-corrected local linear estimates including control variables for a range of bandwidths. The vertical red line in each graph depicts the MSE optimal bandwidth of Calonico et al. (2016). The graphs indicate that the results for months employed, wages, months unemployed and unemployment benefits are robust to the chosen bandwidth. The results for the effect on earned income are relatively stable after bandwidths of roughly 3 months and become more precise at wider bandwidths. This is in line with the observation that the effect on earned income is roughly the combination of the effects on wages and unemployment-related transfers. The point estimates for combined sickness and disability benefits are relatively stable close to zero at a wide range of bandwidths. The results for the effect on net transfers are stable and also statistically significant for a wide range of bandwidths. The point estimate for mortality is stable around zero for all but the very narrowest bandwidths.

Figure 8 shows the sensitivity of the estimates for spouse and household outcomes to varying the bandwidth used in estimation. The point estimate of the effect on the retirement age of the spouse is negative for all bandwidths, and it is statistically significant at the 5% risk level for a small interval of the bandwidths around 16 weeks. By contrast, the effect on spouse's months employed is stable around zero for the whole range of bandwidths, strengthening the view that the labor supply of the spouse is not affected. The household earned income and disposable income do not differ statistically significantly from zero at any bandwidth. It should be kept in mind that the sample used for analysing spouses' outcomes is smaller than our base sample because we only include those who are observed with the same spouse during our whole observation period. This obviously reduces statistical power somewhat.

We further analyze the validity of our research design by examining whether there are discontinuities at placebo cutoffs. Figure 9 shows the estimated discontinuities at cutoffs every week for 12 months before and 12 months after the actual threshold at January 1, 1950. The results are from local linear estimations including covariates and using the MSE optimal bandwidth proposed by Calonico et al. (2016). The results provide evidence in support of our main results, as there are hardly any discontinuities of similar magnitude at placebo cutoff points for months employed, wages, months unemployed, unemployment benefits and net transfers. The effect at the true cutoff for the earned income in the years 2004–2013 is also larger than at most placebo cutoffs, but not as clearly discernible. As would be expected from our main results, for sickness and disability benefits as well as for the share deceased the effects at the true cutoff are indistinguishable from those at alternative cutoffs.

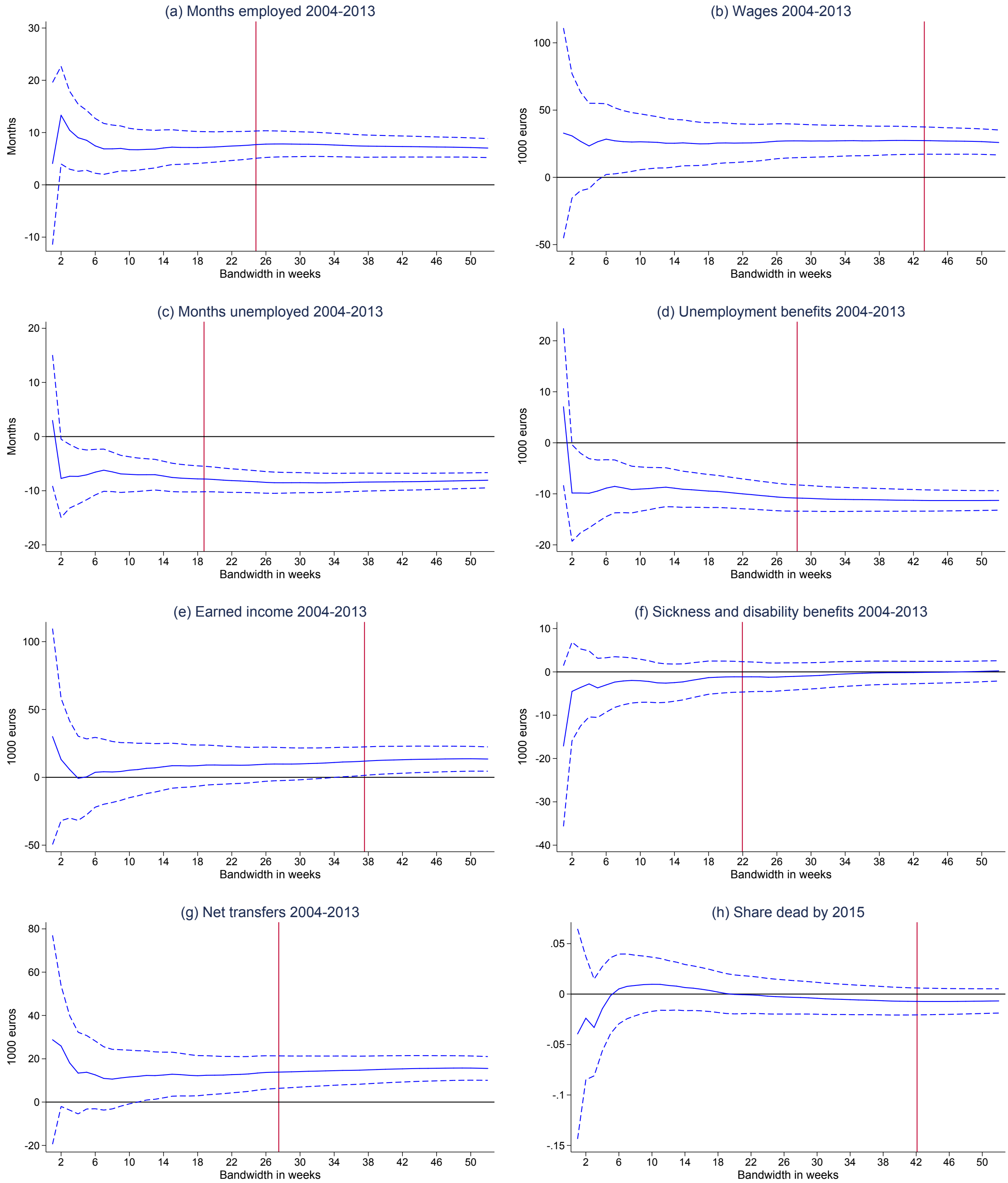

Figure 7: Sensitivity of the results for individual outcomes to bandwidth

Notes: Solid lines represent bias-corrected estimates using alternative bandwidths. Dashed lines depict the 95% confidence interval and the vertical red lines denote the MSE optimal bandwidth of Calonico et al. (2016).

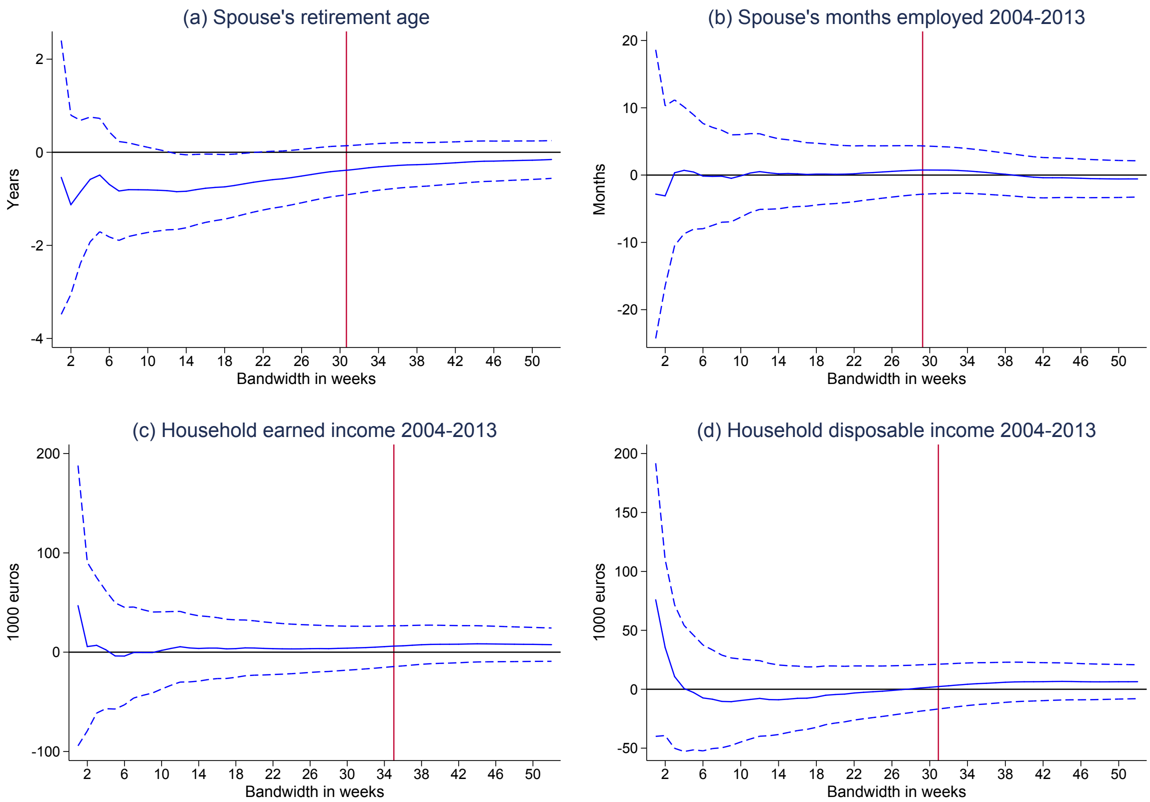

Figure 8: Sensitivity of the results for spouse and household outcomes to bandwidth

Notes: Solid lines represent bias-corrected estimates using alternative bandwidths. Dashed lines depict 95% confidence interval and vertical red lines denote the MSE optimal bandwidth of Calonico et al. (2016).

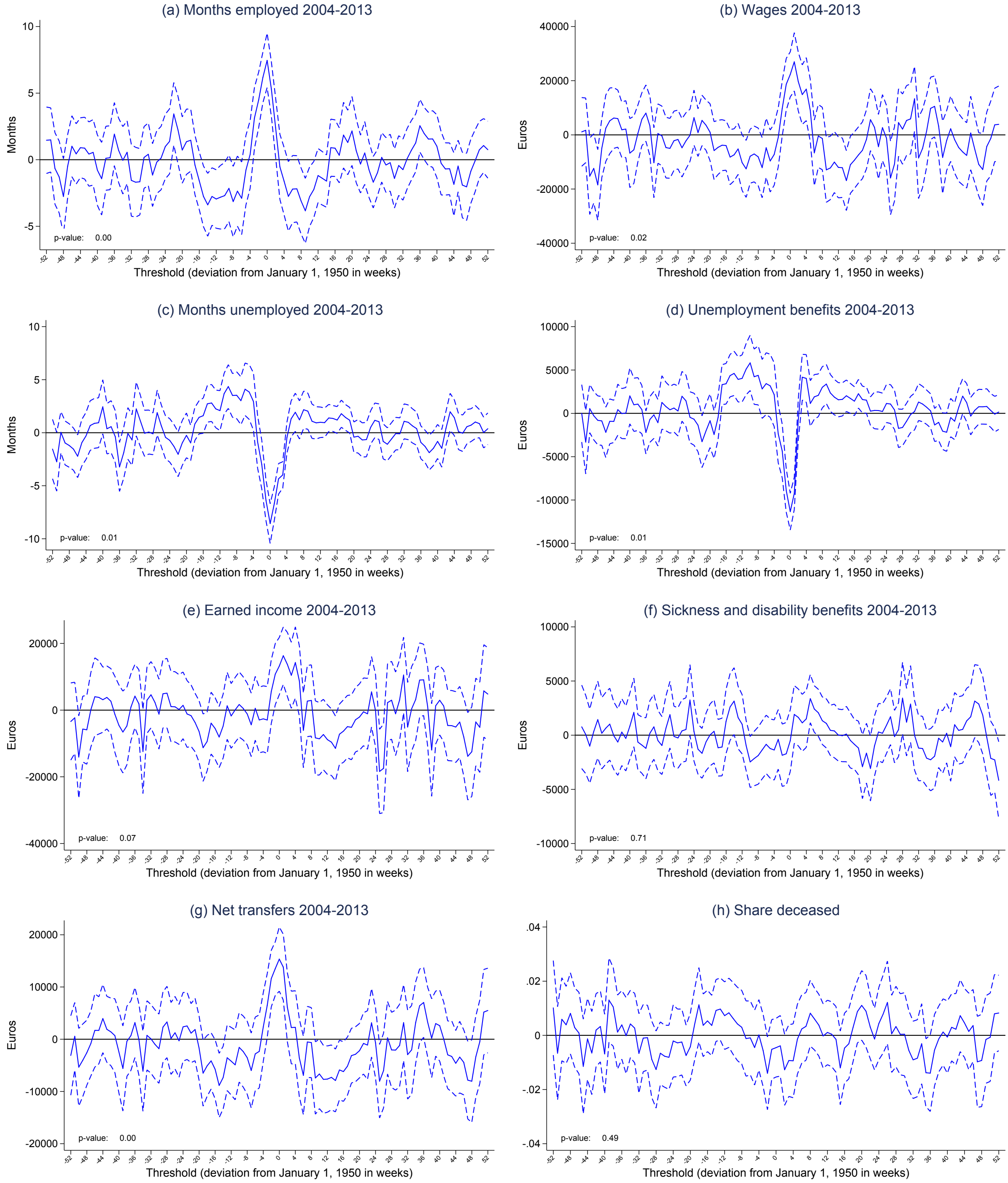

Figure 9: Placebo thresholds for individual outcomes

Notes: Solid lines represent bias-corrected estimates using the MSE optimal bandwidth of Calonico et al. (2016) at alternative cutoffs of the birth date. Dashed lines depict the 95% confidence interval. The p-values refer to the share of estimates that are larger in absolute value than the estimate at the true cutoff. Data on individuals born in 1948 and 1951 are included to enable the placebo analysis over the whole two years around the true cutoff.

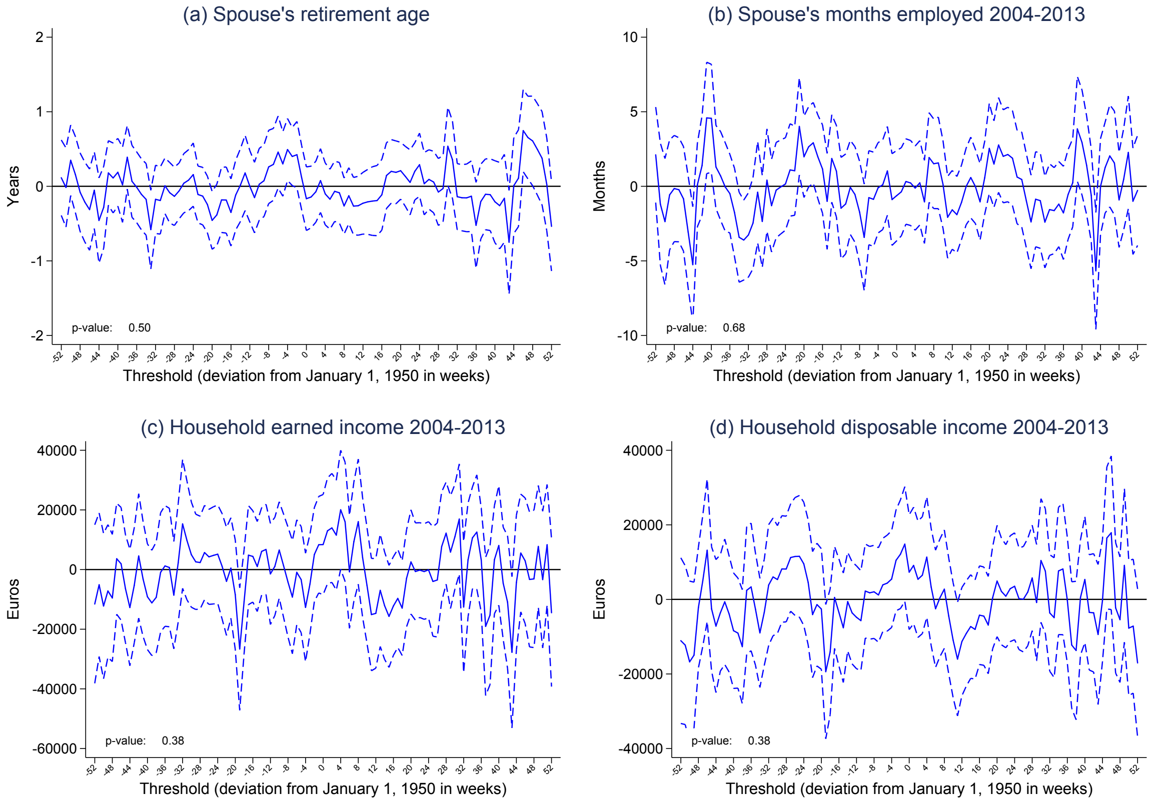

Figure 10: Placebo thresholds for spouse and household outcomes

Notes: Solid lines represent bias-corrected estimates using the MSE optimal bandwidth of Calonico et al. (2016) at alternative cutoffs of the birth date. Dashed lines depict the 95% confidence interval. The p-values refer to the share of estimates that are larger in absolute value than the estimate at the true cutoff. Data on individuals born in 1948 and 1951 are included to enable the placebo analysis over the whole two years around the true cutoff.

Figure 10 shows similar placebo analyses for the spouse and household outcomes. Considering our imprecise results from the main analysis for these outcomes, it is unsurprising that the effects at the true cutoff are not discernible in these graphs but similar sized discontinuities are found at various thresholds from January 1949 to December 1950.

In addition to alternative bandwidths and placebo cutoffs we have also considered the sensitivity of our results to the polynomial order of our specification. We estimated models both with and without additional controls using a quadratic specification for the point estimate and a cubic specification for the bias correction term. These results are very close to those from our linear specification (results not shown, available on request).

7 Conclusions

This paper presents evidence that the age at which the individual becomes eligible for extended unemployment benefits has a substantial impact on labor market outcomes over the last years of the working career. We find that a two-year increase in the age limit for extended benefits increases total months in employment over a 10-year period by 7 months (9%) and wage income by about 22,000 Euros (9%). The corresponding decrease in unemployment-related benefits leads to a smaller effect on total earned income which increases by 13,000 Euros (4%). The spouse's labor force participation does not appear to be affected and the effects on household disposable income are subdued. Our results indicate that higher levels of unemployment at older ages do not affect mortality during our observation period. Disability and sickness benefits do not appear to be affected either, which is in line with previous Finnish studies and indicates that spillover effects into these benefit types may not be a major concern.

Since our data include information on all transfers that individuals residing in Finland have received and paid, we can perform a back of the envelope analysis of the total fiscal impact of the reform. The improved labor market outcomes caused by the increase in the age limit of extended benefits led to an increase of 15,000 Euros (60%) on average in net transfers paid during our 10-year observation period. Considering the cohort size of 33,000 individuals employed in the private sector at the beginning of the period, this implies that the total fiscal impact of increasing the age limit of extended benefits by two years was an increase of up to 500 million Euros in net payments to the society during the 10-year period for a single birth cohort. Taking into account the fact that the reform affected all subsequent cohorts, this can be viewed as a substantial impact on public finances.

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