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Empirical evidence from retirement age hikes in Hungary during the 2000s

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Abstract

This paper provides empirical evidence on the effect of changing the retirement age on employment. Base on individual data from Hungary, a country where a number of hikes increased the retirement age between 1997 and 2009, this analysis benefits from substantial variation in pension eligibility during a relatively short time. It is based on a difference-in-difference approach and supported by independent variation in the age-based eligibility rule contributing to the causal identification of the effect. Results suggest that the effect of the changes in early retirement age is substantial, amounting to 5-7.4 percentage point increase in the 45 per cent employment rate at the retirement age for women. Changes in the normal retirement age do not seem to have such employment effect because increases in disability pension claims have counteracted them.

Keywords: retirement age, older workers, employment

JEL classification: H31, H55, J14, J26

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Milyen hatással van a nyugdíjkorhatár emelése az idősebb nők foglalkoztatási rátájára?

Empirikus eredmények a 2000-es évek magyarországi nyugdíjkorhatár emelései alapján

Cseres-Gergely Zsombor

Összefoglaló

Ez a tanulmány empirikus eredményekkel szolgál a nyugdíjkorhatár emelésének hatásáról. Az elemzés magyarországi egyéni adatokat használ egy olyan időszakból, amikor a korhatár többszöri, 1997 és 2009 közötti emelése jelenetős változásokat hozott viszonylag rövid idő alatt. A hatást a „különbségek különbsége” módszerrel vizsgálom, amihez a korhatár kohorszokhoz kötődő és egyik évről a másikra élesen változó emelkedése járul független variabilitással, lehetővé téve az oksági kapcsolat vizsgálatát. Az eredmények azt mutatják, hogy a változások jelentőst hatást gyakorolnak a női foglalkoztatásra az előrehozott nyugdíjkorhatárnál, mintegy 5-7,4 százalékponttal megemelve az átlagosan 45 százalékos foglalkoztatási arányt. A korbetöltött nyugdíjazás esetében ilyen hatás nem figyelhető meg, főként azért, mert a rokkantnyugdíj igénybevételének növekedése ellensúlyozta az öregségi nyugdíj igénybevételének csökkenését.

Tárgyszavak: nyugdíjkorhatár, idősebb munkavállalók, foglalkoztatás

JEL kódok: H31, H55, J14, J26

Köszönetnyilvánítás:

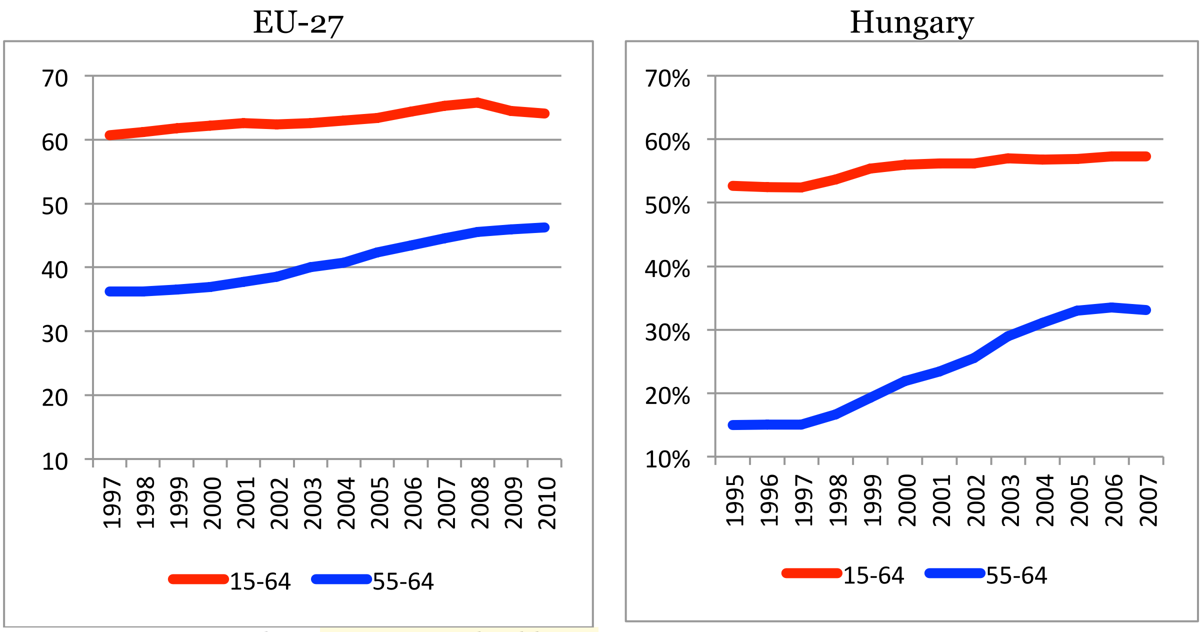
A kutatás a 101803 számú OTKA kutatás és a Bod Péter alapítvány anyagi támogatásával jött létre. Ezúton köszönöm a KRTK Adatbank munkatársainak segítségét, valamint Gál Róbert és Simonovits András, valamint a KTI szeminárium résztvevőinek segítő megjegyzéseit. Természetesen minden fennmaradó hiba engem terhel.

1. INTRODUCTION

Ageing and its interaction with the labour market is a major interest to policymakers wanting to boost economic activity of the population and lighten the load of the state budget. One of the ways to achieve both goals at the same time is to increase the pension age, a main parameter of pension systems. This is an idea that seems quite appealing as the state pension, at least many European countries, is a major exit route from the labour market.

Figure 1

Employment rate of the 15-64 and 55-64 year old population in the EU-27 and in Hungary



Source: EUROSTAT data, tps00180 and tsdde100

Hungary is a EU member state where one of the main direct causes of low employment was early retirement for two decades. Also because early retirement was used extensively in the eastern part of the EU (Vanhuysse 2006), increasing the retirement age was started in Hungary relatively early, in the late-middle 1990s. In a period when similar trends were unfolding within the EU, employment of the 55-64 year olds in Hungary has increased from 40 per cent to around 70 per cent of that in the EU (see Figure 1).

From 1995 to 2009, retirement age for women increased from 55 to 62, and from 60 to 62 years of age for men. In a comprehensive account of early retirement and labour market participation (Monostori 2008) emphasises the connection between these changes and the increased employment rate of older people. Decomposing the increases of economic activity by various subgroups (Kátay and Nobilis 2009) showed that during the beginning of the 2000s, changes in pension regulation had an important contribution to the activity of older people. Analysing incentives for claiming state pension shows that besides financial incentives, availability of the state pension was a decisive factor in the timing of retirement in Hungary (Cseres-Gergely 2008).

There is good reason to believe that rising the eligibility age for a major pension benefit will effectively delay retirement from the labour market. (Stock and Wise 1990) model retirement incentives in the US, (Börsch-Supan, Kohnz, and Schnabel 2002) in Germany to look at the effect of financial incentives. Both find that beside those, the time remaining to reaching pension age has an effect on retirement and before that, on claiming the company and state pensions, respectively. Summarising the results of a comprehensive cross-country research, (Gruber and Wise 2004) reports similar overall evidence.

The connection between the retirement age and labour market activity was an important research interest for some time, but only recent years brought methods that employ techniques that do not rely on out of sample projections. A notable first example of these is (Mastrobuoni 2009), who stresses the importance of norms, life-cycle- and other effects that are unlikely to be identified from the behaviour of populations not actually affected by the intervention. He exploits the increase of the normal retirement age in the US for cohorts born in 1938 and thereafter, identifying the effect of the intervention with discontinuities in the controlled cohort profile. His results are fairly robust and show that the response to the reform is rather strong, implying one month delay in retirement for a two month increase in the retirement. The idea of (Mastrobuoni 2009) is taken further by (Staubli and Zweimüller 2011) and more recently (Cribb, Emmerson, and Tetlow 2013). The former used a difference in difference framework and fine administrative data from Austria on all retirees to look at the effect of increasing the early retirement age by 26 months. Considering multiple outcomes, the study finds that the intervention was followed by a drop of 19 and 25 percentage points in the rate of pension claims for men and women respectively, as well as a 7 and 10 percentage point increase in employment rates. Unemployment has increased too by 10 and 11 per cent respectively, but not the inflow to disability pension. (Cribb, Emmerson, and Tetlow 2013) use a similar identification strategy but survey data to look at the effect of a normal retirement age (state pension age) rise from 60 to 61 between 2010 and 2012, the first steps of a rise up to 65 between 2010 and 2020. The authors have found that this increase had an effect of 7 percentage points. Using survey data allowed them to look at the effect of spouse's behaviour and have found that increases in women's retirement ages had a "knock-on" effect on their husbands' employment rates.

There are only two studies quantifying the effect of increased retirement ages in Hungary, both using a non-econometric refinement to go beyond out of sample projections. The first numerical results comes from the general-equilibrium microsimulation model of (Benczúr, Kátay, and Kiss 2012). Considering adjustment on both the external and internal margin, interaction of income received at the household-level and feedback through wages and market-level adjustment, this study has simulated a one-year increase in the effective retirement age. According to the estimates, this leads to a 4.26 percentage point long-run increase in the employment rate of the 55-65 population. (Major and Varga 2013) adopt a different approach, building a optimising life-cycle model with trade-offs between consumption and labour supply in different parts of the life-cycle. Calibration of the model yields a 3.9-4.1 percentage point increase in the employment rate of the 55-65 year old male population, somewhat smaller than the other estimate. This smaller figure can be attributed to considering the disincentive effects of stronger discounting of the now more distant consumption as a pensioner as well as the decreased probability of reaching this state, both are getting stronger with age.

Even though available evidence makes it very likely that the increase in retirement ages played a major role in increasing the employment rate of older people, neither this claim nor the magnitude of the effect was supported by empirical evidence relying on within-sample econometric estimates so far. This paper aims at providing reliable estimates for the immediate effect of increasing the retirement age for women in Hungary between 1999 and 2006. I use a difference in difference framework for estimating the impact of the increase in retirement ages supported by sharp changes in the age-criterion of the eligibility rule. Results show that increases in the normal retirement age had no effect on employment rates despite being numerous and spanning a wide range of ages. As a consequence, only effects for women are expected. A one-year increase in the early retirement age estimated to yield a 5-7.4 per cent increase of employment rates for all and by around 9.4 per cent for married women. An important additional insight is that the cohorts affected theoretically by the rise of retirement ages are the ones directly affected by a dramatic expansion of elementary and later by that of vocational schooling.

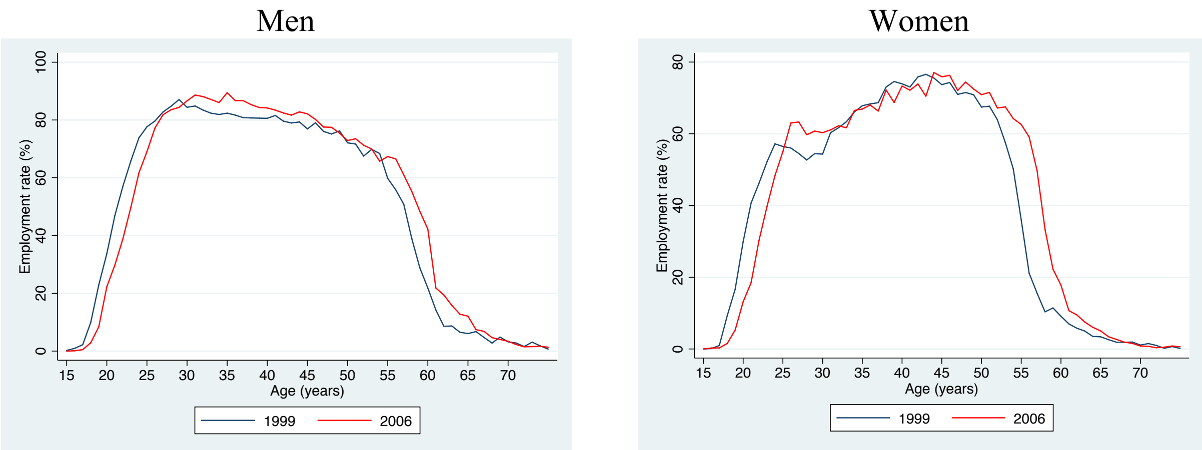
The plan of the paper is the following. Section two describes the institutional framework in Hungary governing retirement into state pension and provides some relevant stylised facts. Section three lays out the model behind the empirical analysis and discusses the identification strategy. Section four provides estimation results for employment of the population as a whole and also discusses the heterogeneity in the effects. Section five provides sensitivity analysis and discussion. Section six concludes.

2. INSTITUTIONAL FRAMEWORK, DATA AND STYLISED FACTS

This section looks at the drivers behind the fact that the number of employed people among the 55-64 year olds grew well above average during the first half or the 2000s. Whereas employment growth was only 117 thousand among the 15-64 year olds, 194 thousand 55-64 year old new employed persons were registered during the same period, by far the largest addition to the workforce among all – see Figure 2 for the contribution of different age groups. In order to account for the role of the change in retirement ages in this, I first introduce the institutional framework of employment-related social security for older people and look at aggregate outcomes. Then I discuss the data used in the analysis and provide stylised facts on retirement behaviour and its interaction with labour market outcomes on a more disaggregated basis.

Figure 2

Age-employment profiles of men and women in 1999 and 2006



Source: own calculations from the Labour Force Survey of the Hungarian Central Statistics Office

INSTITUTIONAL FRAMEWORK

The legal framework for retirement and pensions was modified frequently and to a great extent. The state pension system was reformed from the ground up in 1997 and later in 2011 by first creating, then practically eliminating its private pillar. The main legal texts[1] were modified many times, a process which I do not follow here, but look at the changes ex-post, the regulations prevailing at the time when they become legally binding. Because of the dramatic effect of the 2009 financial crisis on the labour market and the large number of changes to early retirement in the previous years as well as the special circumstances of introducing the pension reform, I am focussing on the years between 1999 and 2006.

From the perspective of those retiring during the 1990s and the 2000s, Hungary operated a pay as you go, defined contribution pension system.[2] In this period, all employed persons must pay a contribution to the pension fund as a defined portion of their gross salary. Pension claimants are paid from this fund which is nevertheless topped up should it run at a deficit. These contributions are used to pay the same year's pension benefits, therefore a direct connection between individual pension contribution payment over the life-cycle and benefit received does not exists. The indirect connection is based on wages earned since 1988, which are later "valorised" according to a formula to determine starting pension levels.

The colourful event history of the pension system was described in a number of publications, such as (Monostori 2008) and I do not aim to reproduce these here, but only give an outline of the most important changes. Apart from the introduction of a third pillar which does not affect those retiring during the 2000s, these were parametric. In the beginning of the 1990s, there was only a single normal retirement age (NRA) for men and women set at age 60 and 55, respectively. From 1999 on, the NRA started to increase and the early retirement age (ERA) was introduced for both women and men.

Table 1 shows the NRA and the ERA (subheading "Age") by female and male birth cohorts as defined by the law. In order to facilitate analysis, it also includes the implied calendar years (subheading "Implied year"), shown on Figure 3 too in a more graphic way. It is apparent that in the case of both sexes, there were much more changes to the NRA than the ERA. In the case of women, the latter has changed twice, while in the case of men, it has actually remained flat at the previous level of the NRA.

[1] Until 31 December 1997, act II of 1975 regulated the availability of state pension in Hungary. As of 1 January 1998, it was replaced by act LXXXI. of 1997.

[2] During the 2000s, the pension system in Hungary also had a privately funded „pillar", introduced with the 1997 pension reform. Members of this however did not become old enough to actually claim pension during the same period. This pillar was later practically abolished by the government having taken office in 2010 by creating incentives that made most of the members exit the system.

Table 1

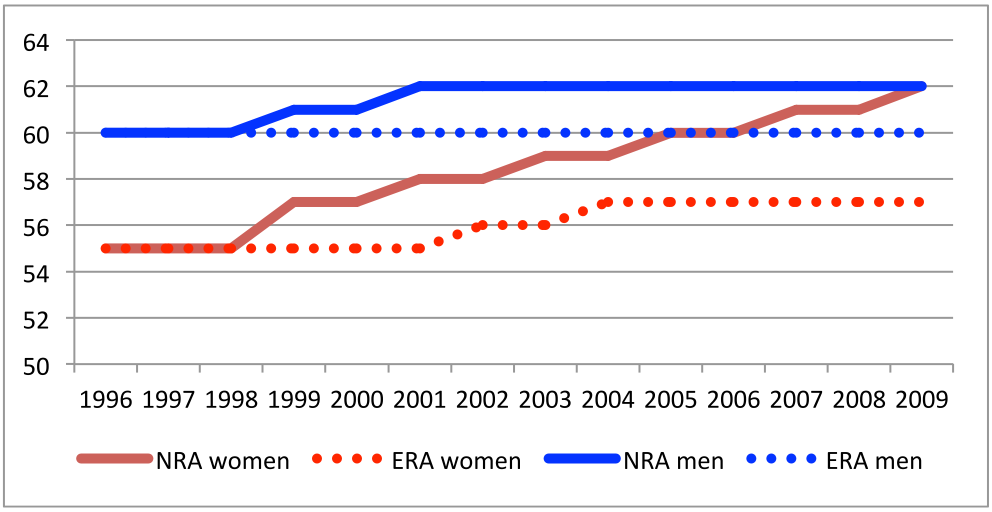
Normal retirement ages (NRA) and early retirement ages (ERA) in Hungary between 1995 and 2012

Birth cohort	Women				Men			
	NRA		ERA		NRA		ERA	
	Age	Implied year	Age	Implied year	Age	Implied year	Age	Implied year
1937	55	1992	-	-	60	1997	-	-
1938	55	1993	-	-	61	1999	60	1998
1939	55	1994	-	-	62	2001	60	1999
1940	55	1995	-	-	62	2002	60	2000
1941	55	1996	-	-	62	2003	60	2001
1942	57	1999	55	1997	62	2004	60	2002
1943	58	2001	55	1998	62	2005	60	2003
1944	59	2003	55	1999	62	2006	60	2004
1945	60	2005	55	2000	62	2007	60	2005
1946	61	2007	56	2002	62	2008	60	2006
1947	62	2009	57	2004	62	2009	60	2007

Source: retirement ages: act LXXXI. of 1997. and act LXXXI. of 1997 on social security pensions. The acts specify eligibility based on birth-year and age, not based on year and age.

Figure 3

Normal and early retirement ages for women and men between 1996 and 2009



Eligibility for pension benefit depends not only on age, but also on the number of “service” years. These are principally based on the number of years in which the individual has worked or paid social security contribution. Service years are gained also with higher education participation and early years of childrearing. Set at 10 years in 1975, the number of service years required for a full normal pension was increased to 20 from 1991 on. For those having accumulated only 10 service years, a partial pension benefit was made available with lower payment levels after this change. A full early retirement pension required much more service years, set at 37-38 for men and 34-38 for women. The timing of the change in required service years mimicked that of the rise in the ERA. Provided that other conditions hold, a partial early retirement was made available too with lower levels of payment. During the period we are looking at, old-age pensions are not differentiated by the means through which a claimant has entered it, the legal status is the same and the determined initial pension is increased by the legally set amount in each year.

There are special types of old-age pensions too, including pensions to workers in particular professions, such as miners, performers, members of armed forces etc. They can retire with much less service years and also earlier than others. Another type of old-age pension is retirement with age-concession ("korengedményes nyugdíj"), which was available from 1990 to workers who were at most 5 years younger than the NRA and whose employee was willing to fund the pension payment completely until reaching the NRA. Yet another option was pre-retirement ("előnyugdíj"), available from 1991 as a pension-type unemployment benefit. These do not affect the discussion of rules to the normal old-age pension, but because they provided relatively esy exit routes before 1999, we start the analysis with this year.

Older people do not receive only old-age pension, but also other pension-type benefits. The less important of these is widowers' pension, a mere 5 per cent of newly determined pensions, paid at a significantly lower level than own old-age pensions. A more important alternative is disability pension, amounting to about 25 per cent of all newly determined pensions during the 2000s, but reaching around 40% in some years. This pension comes in three slightly different varieties, depending on the degree of disability. Apart from being another important social transfer, they also constitute an important exit route from the labour market (Scharle 2008). Rules of disability retirement were changed in 1997-1998 as a first attempt to get disability pension awards in line with actual health conditions. The major difference compared to the previous regime was that eligibility to disability benefit was made temporary by default, requiring a more in-depth analysis of health conditions after a period of rehabilitation. Only those found to be disabled also after this period could obtain a permanent disability pension. Some changes in disability pension regulations and its potential effects overlap with the period analysed here, but we shall see that the particular empirical strategy I apply is not affected by it.

STYLISED FACTS ON EMPLOYMENT OF OLDER PEOPLE

The number and magnitude of changes to the old-age pension system suggests that they have ample potential to affect behaviour related to pension claims. As data in Table 2 show, this is not necessarily so. The beginning of the period shows a substantial drop in the number of claims before 1998, very much in line with the new restrictions on both old-age and disability pensions. After the transition year of 1999, the inflow to both types of pension grew, but to a different extent. Later changes in old-age pension reflect the bi-annual pattern of rising retirement ages: very few men retired in 1998 and 2000, and so did women in 1998, 2000, 2002, 2004 and 2006 at the normal age. It is important to note that very few have retired at the NRA after the ERA was made available: 70-90 per cent of the inflow to old-age type pension happened below the NRA.

When gauging the potential impact of the rise in retirement ages, the figure to start from is its potential effect, bounded by the effective size of the cohorts for whom old-age pension is available. According to figures derived from the 2001 Census and the 2005 Microcensus, the size of the 55-64 age group has grown from 1.1 million to 1.2 million over five years. 40-60 per cent of women in the relevant cohorts were not retired before the ERA and 10-20 per cent not retired before the NRA according to data from the Labour Force Survey (LFS). The same figures are 20-25 and 10-20 per cent for men for the ERA and the NRA respectively. Considering an average cohort size of 65 and 45 thousand in case of the two sexes, this means that increasing the NRA by one year can keep about 6,5-13 thousand women and 5-10 thousand men in the labour market on average. Increasing the ERA can retain about 26-39 thousand women and 9-11 thousand men on average. Comparing the 1999 and 2006 cross sections, there are one male (gain of 7.5 thousand) and three female (gain of about 30 thousand) cohorts affected by the rise in the NRA, but no male and two female (gain of about 65) cohorts are affected by the rise in the ERA. This implies a possible gain of about 102 thousand, much less than the observed 180 thousand individuals.

Table 2

Newly determined disability pension claims and detailed data on the number of newly determined old-age pension claims

Year	Disability and accident-related disability pensions	Old-age and old-age type pensions[a]			From the total: at the NRA			From the total: below the NRA		
	Total	Male	Female	Together	Male	Female	Together	Male	Female	Together
1996	59 967	31 770	59 939	91 709	9 893	20 073	29 966	18 681	31 857	50 538
1997	48 262	37 886	32 614	70 500	10 630	1 138	11 768	24 308	28 154	52 462
1998	42 975	12 908	17 841	30 749	385	882	1 267	11 461	15 244	26 705
1999	46 701	15 181	24 418	39 599	2 601	5 808	8 409	11 494	16 922	28 416
2000	55 558	18 071	29 526	47 597	613	813	1 426	16 089	26 859	42 948
2001	54 645	28 759	14 267	43 026	2 200	4 882	7 082	25 175	7 396	32 571
2002	52 211	30 209	25 719	55 928	2 593	646	3 239	26 346	23 503	49 849
2003	48 078	32 574	13 574	46 148	3 058	5 098	8 156	28 064	6 537	34 601
2004	44 196	35 940	36 684	72 624	3 842	989	4 831	30 234	33 817	64 051
2005	41 057	33 175	48 771	81 946	4 035	6 721	10 756	27 719	40 142	67 861
2006	36 904	34 207	47 531	81 738	4 013	732	4 745	29 025	45 675	74 700
2007	34 991	51 037	62 168	113 205	3 722	6 660	10 382	45 731	54 177	99 908

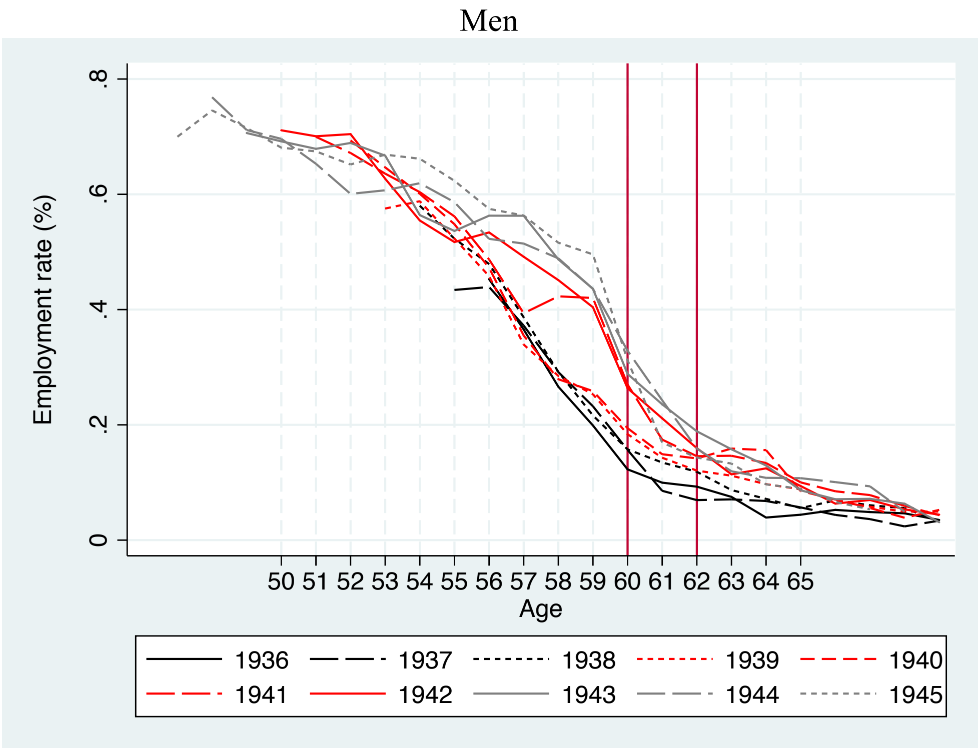
[a] Old-age type pensions include: old-age pensions given with a retirement age threshold allowance (early retirement), artists' pensions, pre-pension up until 1997, miners' pensions.

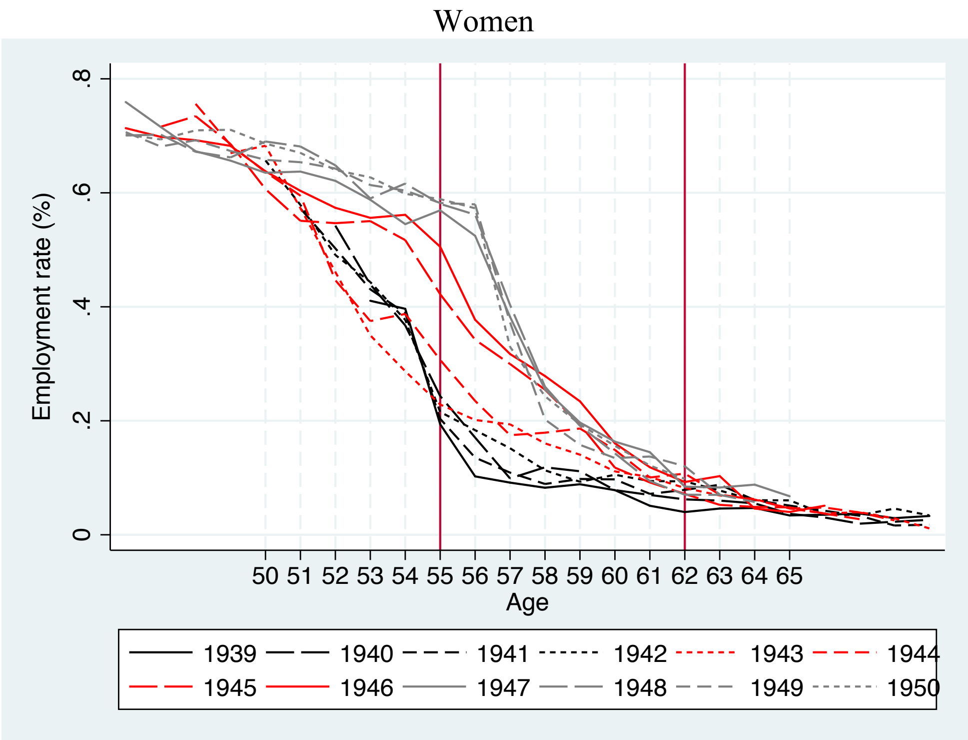
[b] Note: Pensions disbursed in the given year (determined according to the given year's rules). The source of these statistics is data from the pension determination system of the ONYF (NYUGDMEG), so these do not include the data for the armed forces and the police. Source: ONYF.

Source: http://www.bpdata.eu/mpt/2013ent11_07

The pattern of changes in the pension regulation makes it clear that if these changes do have an effect on employment, they do so on a cohort-specific way. Figure 4 shows the true life-cycle employment-age profile of the cohorts gaining eligibility to retirement between 1997 and 2007, calculated from the successive waves of the LFS (yearly weighted averages). The colour-coded profiles show a distinct pattern of transition between what seems to be two distinct equilibria. In the case of men, the change takes place between the ages 56-62. Cohorts 1936-1940 appear to be in an initial equilibrium, the change starting with cohort 1940. Cohort 1941 is within transition, while 1942 already seem to complete it. Later cohorts appear to blend into a second equilibrium. In the case of women, the transition is more interesting. The 1942 and older cohorts seem to be in an initial equilibrium. The transition starts with cohort 1943, cohorts 1944-1945 are within transition and cohort 1946 completes it.

Cohort-specific age-employment profiles of men and women reaching the retirement age between 1997 and 2007





Observe that the cohorts making the transition between the two equilibria are not necessarily the ones hit by the shock of changing retirement ages. In the case of men, the NRA was increased for those born in 1938 and 1939, cohorts whose profile blends well into the first equilibrium. Nothing was however changed for the two cohorts that do make the transition, providing strong evidence *against* the apparently clear role of pension regulations. In the case of women however, the first cohorts starting the transition already experience a change in the NRA and changes in later cohorts' profiles coincide with changes in the ERA. Here we cannot rule out the connection between regulation and employment behaviour easily.

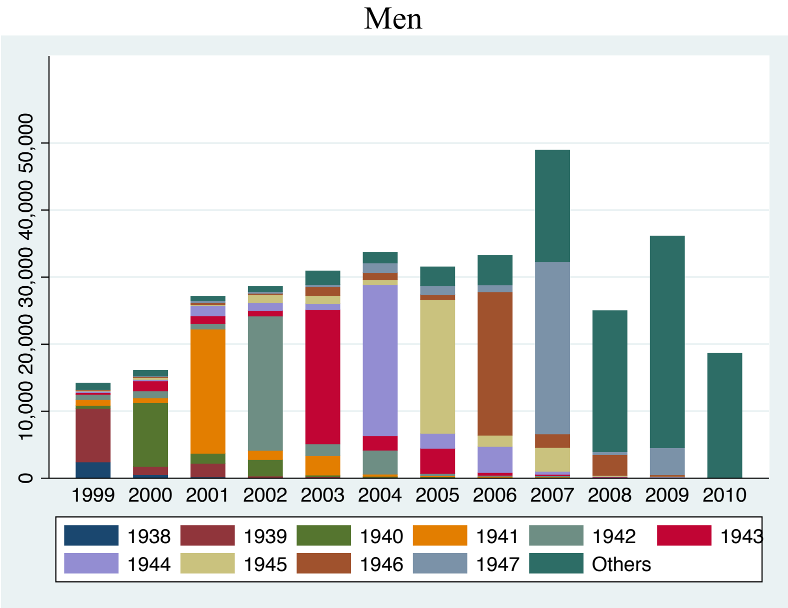
Employment rate being the main focus of interest here, Figure 8 and Figure 9 in the Appendix gives a bit more background detail by showing the same profiles for old-age pension and disability pension. Changes around the appropriate ERA for both men and women reflect what we have seen above in the case of employment rates, but a little more heterogeneity appears before the NRA, especially for women. Indeed, Figure 9 shows that changes in disability pension receipt might be a reason for this. While there appears to be no strong trend behind the changes in men's disability claim profiles, those of women shift to the right with for later born cohorts. Affecting mostly the second part of the period between the ERA and the NRA, this shift brings about 2-3-fold increases in disability claims.

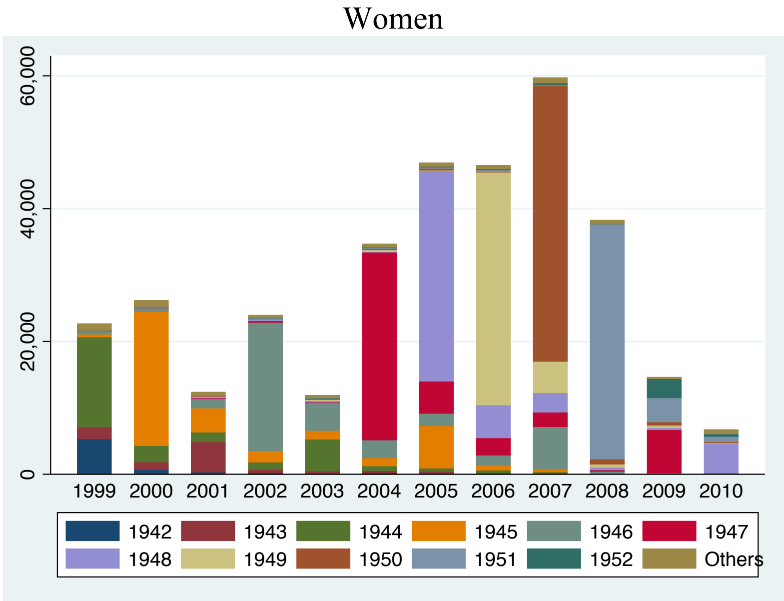
Key to our discussion is the way members of each cohort are spread out in time at the time they claim pension. In particular, the distribution of claims around and between the ERA and the NRA is decisive in relation to the potential effect of the hikes. Figure 5 shows the number of persons having successfully claimed old-age pension in each year. Comparing this to the information in Table 1, one finds that almost all claimants in all cohorts chose the first year allowed by regulation, which is at the ERA.[3] The first we can observe in full is the 1939 male cohort, being also the first with the NRA set at 62 years and the ERA at 60. Most of this cohort claims pension in 1999 at the ERA, just a fraction in 2001 at the NRA, only a few persons thereafter. This pattern repeats itself throughout the period we are looking at, in line with the fact that there was no change in regulation for the NRA later and none to the ERA at all. In the case of women, there is a much larger gap between the NRA and the ERA, claims being more spread out in time. The first cohort we follow in full is the one born in 1944, showing a pattern similar to that of men but with a larger response at the NRA. From the 1945 cohort on, the effect of the increase in retirement ages appears visibly, inflows appearin in every other year. The large share of the outflow at the first possible exit date strongly suggests that only changes in regulation affecting the ERA can have a sizeable effect on pension claiming behaviour and thus on employment.

[3] Very few can and actually do so even earlier due to easier rules for some professions.

Figure 5

The number of old-age pension claims by cohorts





Source: calculations from the official pension claims statistics of the National Pension Directorate

The employment effect of a change in regulation depends greatly on the employment potential of the affected population. Looking at the value of pension claims of the 1945 birth cohort as it ages (see this as an example Table 3 – the pattern is similar for other cohorts), the differences between initial pensions are substantial. The value of initial pensions attached to the large number of claims made at the ERA (a little more than 50 per cent in the case of both men and women) are relative high, second to only those working well after the NRA (about 10 per cent for men and about 16 per cent for women). Average service years are long too for this group. This suggests that those retiring at this age had and possibly have good labour market position on average at the point of claiming pension.

The initial pension increases somewhat with age but drops to a much lower value at the NRA. Around 90 per cent of women, but only 66 per cent of men retire at or between the ERA and the NRA with increasingly favourable initial pension and the minimum required service years. Initial pensions are lower (in the case of men: much lower) at the NRA and so are service years, suggesting that this group is rather different from the earlier ones, having a weak labour market position, particularly short service years and low levels of contribution payments. The few retiring before the NRA are very different in the case of men and women, the former appear to be enjoying more favourable conditions, probably because of the larger share of professions with special retirement options (such as miners and members of armed forces).

Even though this paper does not seek to provide a general explanation to the rise in employment rates of older people, it is instructive to look at alternative explanations beside changes in pension regulations. The first and most important explanation to consider is education, in particular the dramatically rising education level of the affected cohorts (Augusztinovics and Köllő 2007). The difference between the 55-64 year olds in 1999 made up by the 1935-1944 cohorts and those in 2006 made up by the 1942-1951 cohorts is substantial in terms of education attainment. In 1940, the parliament has adopted an act that introduced 8 grades of mandatory elementary schooling. This reform was initiated by Kuno Klébelsberg, the education minister between 1922 and 1931, but postponed in 1929 due to the economic crisis. The communist governments that took power in 1949 in Hungary have carried on with implementing the reform and later increased the mandatory schooling age to 16 (Kazuska 2012). Census data from 1960 show that the proportion of 10 year olds learned reading at most decreased from 9.3 per cent in 1941 to 3 per cent in 1960, while that of 15 year olds having completed at least primary school has increased 12.9 to 32.8 per cent (see (KSH 1962) page 28). Much of these differences prevail in the 2000s too: we find that in the 2002 cross-section, the share of those having completed at most 8 years of primary schooling is 54.5 per cent for the 1941 cohort, but only half of this, a mere 27 per cent for the 1950 cohort. The share of those having completed lower vocational education has increased from 3.2 to 30 per cent for the same cohorts.

Table 3

Statistics of initial pensions of the 1945 birth cohort by age at the initial pension payment

	Men		Initial pension (at 2000 prices)	Service years			Women		Initial pension (at 2000 prices)	Service years	
	N	%		Mean	Std. D.		N	%		Mean	Std. D.
52	1	0%	25473	27,0	0,0	52	2	0%	19525	34,5	2,1
53	384	1%	24781	32,3	6,0	53	492	1%	23873	31,5	7,2
54	1504	4%	32893	34,3	5,2	54	1376	4%	27582	32,0	6,9
55	1964	5%	36376	35,3	5,1	**55**	**20971**	**56%**	**35479**	**36,8**	**3,0**
56	1863	5%	38975	36,3	5,0	56	4101	11%	37504	35,0	4,5
57	263	1%	49026	38,3	4,7	57	2094	6%	37238	34,4	5,4
58	2435	7%	53469	38,7	5,1	58	157	0%	39977	34,3	5,4
59	1601	4%	59541	39,3	5,3	59	141	0%	49355	35,0	6,5
60	**20274**	**56%**	**66082**	**41,5**	**3,3**	**60**	**6478**	**17%**	**34912**	**27,9**	**6,8**
61	1821	5%	72988	39,3	5,0	61	711	2%	58749	33,5	9,5
62	**3555**	**10%**	**49754**	**30,5**	**7,4**	62	454	1%	63035	31,9	10,6
63	170	0%	37431	26,8	9,9	63	79	0%	33015	23,4	9,4
64	113	0%	50727	28,9	10,5	64	54	0%	36911	22,8	9,8
65	30	0%	71080	32,1	12,8	65	16	0%	47651	25,3	13,5
∑	35978	100%				∑	37126	100%			

Source: calculations using the NYUGDMEG database on pension claims and payments

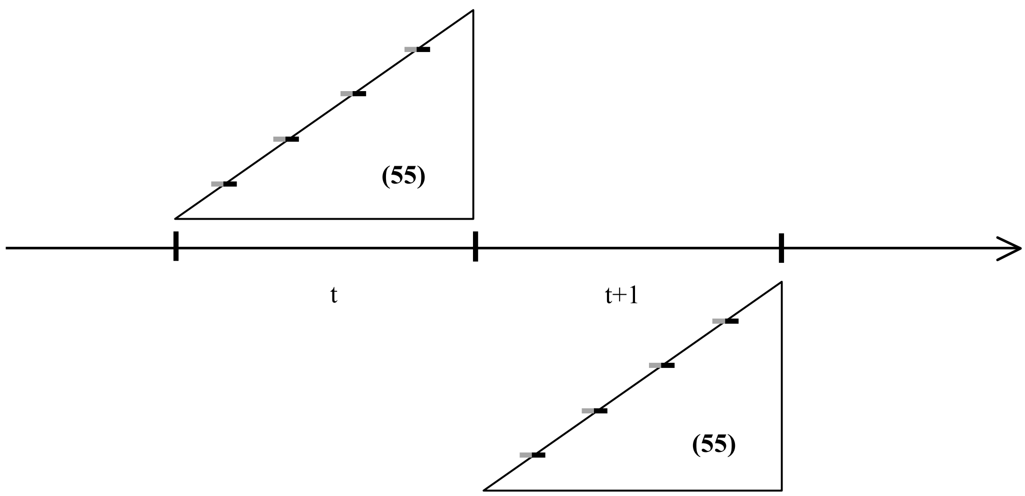
The changes in school attendance of persons born between 1940 and 1950 is rather dramatic, comparable only to the decrease of zero school attendance from the end of the 1920s through the 1930s and the expansion of higher education during the 2000s. Given that the employment rate of 55-64 year old people with at most primary education was 12 per cent in 1999, whereas that of those with completed lower vocational secondary education was 32 per cent, such a shift implies a substantial increase in employment rates in itself. Indeed, if we hold schooling-specific employment rates constant at their 1999 level, we see that such a change in composition can generate a rise of the employment rate from 19.3 to 25.5, a gain of 75 thousand employees. This is about 44 per of the observed gain of 173 thousand – the remainder comes from the increase in employment rates to be explained by factors other than changes in schooling attendance. It is reassuring that the implied remaining gain of about 100 thousand is almost exactly equal to the potential effect of rising the retirement age calculated earlier.

3. IDENTIFICATION, ESTIMATION AND DATA

The problem of estimating the causal effect of the rising retirement age on employment is the one familiar from program evaluation: estimation results from comparing individuals who self-select to being claimants and non-claimants on the basis of characteristics potentially correlated with the intervention can be very inaccurate. In order to overcome this difficulty, I follow the difference-in-difference approach of (Staubli and Zweimüller 2011) and (Cribb, Emmerson, and Tetlow 2013). The idea behind applying this workhorse of program evaluation is that we can exploit the sharp difference between cohorts who are eligible to pension at a certain age before a rise and those who fall short of this eligibility after the rise. Thinking about the rise in retirement ages as a treatment, the former is the control, the latter is the treatment group. Comparing changes in employment rates of one cohort before and after the retirement age gives an estimate contaminated with composition and life-cycle effects. Comparing this change of two adjacent cohorts, the control and the treated before and after the change gets rid of all of these effects and uses the variation introduced by the rising retirement age. The fact that legislation makes this difference sharp over time helps a great deal to make this comparison accurate, closely mimicking a comparison with an outcome in case the treatment was not in fact administered to members of the treatment group.

Figure 6

Difference-in-difference estimation of the effect of rising retirement ages



To see how this works, consider the following example illustrated in Figure 6. We have two cohorts born in 1945 and 1946. Members of the first one turn 55 during year t and do so one by one, indicated by steep side of the triangle. Members of the second cohort turn 55 a year later in t+1 the same way. We can compare behaviour "just before" and "just after" being exactly 55 to obtain estimates. Note that the difference between employment rates "just before" and "just after" the retirement age is the slope of the age-employment profile for a cohort and the difference between these differences for the two cohorts in consideration is the difference between the slopes. In the current case we are computing averages over individuals within a cohort who are connected by their position on their personal timeline of age, being just before and after 55, respectively – these are the averages represented by each side of the small horizontal lines. Births are almost uniformly distributed over the year, therefore we have a full coverage of the whole year with simple averaging. Any time-invariant feature that is correlated with time within the year is controlled for this way.

Time being continuous, we can freely define and change the concept of "just before" and "just after" by using a narrower or broader windows around that age (making the small lines on Figure 6 shorter or longer, respectively). Using a smaller window gathers less observations leading to less precision, but more homogeneous groups – there is no rule of thumb for selecting an optimal window. One also has to make a decision about the discretisation of the continuous time that is about the units in which employment rate averages are made. Again, there is no rule of thumb for doing this, but one should respect the familiar consideration between bias and variance as well as practical considerations, such as the sampling scheme of the underlying data.

Both the DiD method and the window used in it emphasize that estimates produced here are local and immediate effects. They are immediate as the logic can in no way take into account effects of the impulse that do not manifest themselves in a slope change within the vicinity of the impulse, that is a change in the slope of the age-employment-rate profile at the formerly prevailing ERA. In the extreme case when there is no slope change, but only a parallel shift of the profiles, we will measure no effect and for good reason: whatever change has happened can no be attributed directly to the rule change we are looking at. The difference will of course visible in the cleaned difference between the treated and the control cohorts, but because this can also be a cohort-effect, we cannot identify it with the impact of the treatment. Varying the window size and looking at the estimated effects can give an idea of the actual size of the effect in case the adjustment does not take place close to the focal age. Finding no significant impact estimates with narrow windows sizes but some with broader ones is consistent with a delayed impact. The opposite pattern, that is having a large and/or significant effect with smaller window size and small/insignificant with a broader one on the other hand is consistent with a substantial local, but no significant change in the slope further away from the point of change.

The treatment we consider in this exercise is revoking of retirement option from a certain cohort at a point in time. Figure 7 shows all such treatments during the period between 1999 and 2007. Consider for example the 1945 and the 1946 female cohorts. Members of the former could claim early retirement at the age of 55, but the latter will be able to do so only when they turn 56. The orange mark for cohort 1946 at age 55 indicates that they receive a treatment of *not* being able to claim pension at this age. In this case, we would compare outcomes for the two cohorts just before and after their members turn 55. This implies that we are looking at the years 2000 and 2001.

Figure 7

Treatments on pension claiming opportunities between 1999 and 2007 (years in columns, cohorts in rows, interventions shown with colour shading: blue = NRA rise for men, red = NRA rise for women, orange = ERA rise for women)

	1999	2000	2001	2002	2003	2004	2005	*2006*	2007
1935	*64*	65	66	67	68	69	70	*71*	72
1936	*63*	64	65	66	67	68	69	*70*	71
1937	*62*	63	64	65	66	67	68	*69*	70
1938	61	62	63	64	65	66	67	*68*	69
1939	*60*	61	62	63	64	65	66	*67*	68
1940	*59*	60	61	62	63	64	65	*66*	67
1941	*58*	59	60	61	62	63	64	*65*	66
1942	57	58	59	60	61	62	63	*64*	65
1943	*56*	57	58	59	60	61	62	*63*	64
1944	*55*	56	57	58	59	60	61	*62*	63
1945	*54*	55	56	57	58	59	*60*	*61*	62
1946	*53*	54	55	56	57	58	59	60	61
1947	*52*	53	54	55	56	57	58	*59*	60
1948	*51*	52	53	54	55	56	57	*58*	59
1949	*50*	51	52	53	54	55	56	*57*	58
1950	*49*	50	51	52	53	54	55	*56*	57
1951	*48*	49	50	51	52	53	54	*55*	56
1952	*47*	48	49	50	51	52	53	*54*	55

Source: act II. of 1975 and act LXXXI. of 1997

Between-cohort differences coming from genuine cohort effects and from the fact that cohorts are observed in different points in calendar time can be controlled for by standard regression techniques with an equation such as the following:

$$\phi(y_i) = \alpha + (\delta p_{it} + \gamma l_c p_{it} + \theta l_c) + (\xi b_{tci} + \eta_t + q_t + y_c) + \beta_1 X_{1tci} + \beta_2 X_{1tci} + \epsilon_{\mathrm{tci}}\,, \quad (1)$$

where $\phi(y_i)$ is an appropriate function of the labour-market outcome we are interested in, l_c is an indicator of cohort membership and p_{it} is an indicator of being treated. Using a sample period that has an appropriate span before and after the treatment and including the above terms as well as their interaction (in the first bracket in equation (1)) is the simplest estimation method. The coefficient on l_c gives us the difference in the outcome between the two cohorts, while the coefficient on p_{it} gives us the difference in outcomes before and after the retirement age, averaged over the whole sample. The coefficient of the interaction term gives us the difference between the two, which we can under the set circumstances identify with the program effect. If we look at a wider time span or stack periods for greater efficiency, we have to take into account cohort and time-effects – these are represented by terms in the second bracket. Finally, there might be differences across individuals, which change over time and are connected to individual characteristics we observe. We can enter these as another set of terms, shown in the third bracket. The term ϵ_{ti} is an individual- and time-specific stochastic driver unrelated to any of the previous observed characteristics.

Equation (1) assumes that the impact, that is the change in being under or below the retirement age depends only on age and on the actual time period through the retirement rule in effect: $p_i = \pi_t(a_{it})$. Let us model this effect by a multiplier m_{it} to get $p'_{it} = p'_{it} * m_i = \pi_t(a_{it}) m_{it}$. The estimating equation now becomes

$$y_{it} = \alpha + (\delta p_{it} m_i + \gamma l_c p_{it} m_i + \theta l_c) + (\xi b_{tci} + \eta_t + q_t + y_c) + \beta_1 X_{1tci} + \beta_2 X_{1tci} + \epsilon_{\mathrm{tci}}. \quad (2)$$

Unfortunately it is not possible to observe m_i directly, but using the results of (Augusztinovics and Köllő 2007) on the effect of education on work experience, we can reasonably approximate it with a set of education indicators.

I shall estimate equation (1) and (2) on microdata from the quarterly waves of the Hungarian Labour Force Survey (LFS). The LFS is a survey conducted by the Hungarian Central Statistics Office to characterise the Hungarian labour market using concepts standardised and suggested by the International Labour Organisation. It provides individual-level data on around 80 thousand individuals of all ages, but focuses on those in “active age”, defined as the 15-74 population.

Detailed demographic information is available for everyone in this age group, complemented by data on employment or unemployment, as appropriate. Although individuals can be followed across quarters, they are retained only for 6 months and thus the data are not suitable to build longer panel data set. Because there are practically no intervention hitting men, the estimation sample is constrained to women only. The actual estimating sample is constrained to those below and above the pensionable age prevailing before the treatment, defined by the size of the window around this age. Because of the sampling scheme of the LFS, I am using quarters as units to define the window around a particular age, using a window size of 1 to 4.

The choice of the outcome variable expresses how deep we would like to dwell into the structure of the relationship of retirement and employment. Available studies on the effect of retirement age rise in Hungary rely on rich model structure making the interaction between old-age retirement, employment and possibly between activity and other exit routes from the economy explicit. Although it is impractical to combine this richness with the econometric approach set out above, introducing the related outcome variables and estimating the effect of retirement age rise on them can shed light on the transmission process between them. The immediate effect of a rise in retirement age affects the timing of old-age pension claims. If the correlation is strong enough, it also affects activity, which in turn affects employment with a strength depending on the interaction of individual characteristics and labour market conditions. Using old-age pension claims, activity and employment rate provides us with an implicit characterisation of this transmission process. Given that there are other exit routes form the labour market such as disability pension, a decrease in pension claims can be correlated with an increase in disability pension claims, in particular if there is no effect on the employment outcome.

I use a probit estimator to control for observed heterogeneity remaining in the control and the treatment groups because in some cases the mean of the outcome variable is much smaller than 50 per cent. Unfortunately the probit cannot properly use weights and does not literally implement a DiD estimator, therefore I also use linear ordinary least squares in the sensitivity analysis to look at the robustness of the results. Given that the mean of the binary outcome variable is 46.2 for the most relevant scenario (NRA, full sample), a linear probability model is safe to use in that case. Because the LFS is a rotating panel with overlaps between quarters, the calculation of standard errors takes into account the clustering of unobserved heterogeneity due to repeated sampling of the same individuals over time.

I estimate the effect of raising the NRA and the ERA separately as they affect very different populations, I pool the relevant data for efficiency reasons. This means that calendar times are forgone and only treatment status as well as before- and after ages are considered. Appropriately, indicators for months and years are included to control for the effect of calendar times.

4. ESTIMATION RESULTS

I consider estimates from a number of specifications. I refer to the first set based on equation (1) as "homogeneous impact" estimates. The first (1) specification contains only the variables necessary to define a DiD estimator. The second (2) one introduces variables with impact at the macro level: indicators for years, quarters and counties of residence in order to control for differences between the two periods over which the two cohorts are measured. The third (3) specification adds individual-specific variables, education and family status. We have seen the importance of the former earlier and discussed the potential role of the latter. Specification (4) is based on specification (3), but the "Impact" indicator is interacted with indicators of education attainment, a proxy for labour market history to produce heterogeneous impact estimates. Specification (5) goes back to specification (3) but is estimated on a smaller sample of those living with a partner. In addition to this, it includes indicators on the partner's employment and unemployment status, education attainment and a linear age variable. In order to keep homogeneity of groups but gain in sample size, I settled with a window size of 4 quarters (see the sensitivity analysis in the next section for a discussion).

Estimates of the impact of four pooled NRA hikes appear in general as relatively precise zeroes in Table 4. The single consistently significant coefficient is the positive employment advantage of the younger cohorts, estimated to be between 2.5-2.9 percentage points – note that if there was any effect of the NRA hike unfolding earlier than the eligibility age, it might be subsumed in these effects. Indeed, going back to Figure 2, no difference in slopes, but a clear difference in levels can be observed, with greater differences between older than between younger cohorts. Temporal aggregate variables do not matter much but spatial ones do, introducing an employment penalty relative to the capital (neither are shown here). While none of the family status indicators have significant coefficients, both the significance and the magnitude of those of schooling are stable across specifications. Among those who live with a partner, employment of the partner increases the chance of employment too. These results indicate that increasing the NRA has likely to had no measurable immediate effect on employment chances except when we separate those with higher education level. In that case, we find that the increases have no significant effect on the employment rates on them, but have on the rest of the population. Looking at the evidence presented in Table 3, this is no wonder: those with good labour market chances have already claimed pension by the time they reach the NRA or will do so years after.

Table 4

Estimation results for the NRA hike episodes with employment as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	-0.00678	0.0246	0.0206	0.0605*	-0.0133
	(0.0155)	(0.0209)	(0.0203)	(0.0310)	(0.0205)
Impact*education: lower secondary				0.00821	
				(0.0457)	
Impact*education: upper secondary				-0.0374	
				(0.0274)	
Impact*education: higher				-0.0908***	
				(0.0203)	
Treated (cohort with increased NRA)	0.0311***	0.0283***	0.0289***	0.0291***	0.0266***
	(0.00828)	(0.00840)	(0.00813)	(0.00817)	(0.00944)
Above (the NRA before the treatment)	-0.0280***	-0.0317***	-0.0263***	-0.0263***	-0.0142*
	(0.00761)	(0.00773)	(0.00753)	(0.00757)	(0.00861)
Education: lower secondary			0.0654***	0.0624***	0.0301
			(0.0188)	(0.0192)	(0.0201)
Education: upper secondary			0.117***	0.120***	0.0649***
			(0.0129)	(0.0132)	(0.0161)
Education: higher			0.396***	0.411***	0.304***
			(0.0215)	(0.0221)	(0.0344)
Partner works					0.170***
					(0.0168)
Observations	28,861	28,861	28,861	28,861	19,700

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Repeating the above estimates for activity as an outcome yields a very similar result (see Table 7 in the Appendix), but switching to pension claims helps opening up the reasons behind the lack of effect for people with lower than higher education. Table 8 and Table 9 in the Appendix show estimates with old-age- and disability pensioner status on the left-hand side of the estimating equation. Contrary to the results for employment, the increase of retirement ages have a significant negative effect on old-age pension claims at the order between -4.4 to -7 percentage points. Those with higher education are again an exception with a 3 percentage point effect. The answer to why a significant effect on old-age pensions does not translate to activity and employment improvement lies in the estimates for disability pension claims: these are about the same size as the effects on old-age pension claims, but have the opposite sign. This means that while the increase in retirement ages did decrease old-age pension claims, they were absorbed by disability pension claims. The former negative effect on employment of the higher educated is explained by the fact that there is no such balancing effect in their case. Note that despite the significant effects, these results concern only a small fraction of the population: according to Table 3, only about 30 per cent of a cohort has not claimed old-age pension at ages before the NRA.

Turning to the two hikes in the ERA, we see much more action and a significant effect on employment. The structure of Table 5 is identical to the previous one showing five specifications, four of which are estimated for all individuals in the sample and the last for only those living with a partner. The most significant difference from the previous results is that increasing the ERA appears to have had a significant effect on employment rates in all but one specifications. Specification (1) already shows that younger cohorts have significantly higher employment rates and so do the younger irrespective of which cohort they belong to. Moving from (1) to (2), aggregate effects take away the confounding difference between time periods and regions. As in the case of the NRA, differences between cohorts are a mixture of cohort-effects and potential indirect effects of the retirement age hike that unfold earlier and appear as a level shift. They are much stronger than in the case of the NRA and their magnitude are comparable to that of the impact estimate itself.

Education has a profound effect on employment rates, sometimes even larger than around the NRA, in particular for those with upper secondary or higher education – see specification (3). Notice that despite of the sizeable effect of education, it does not affect the estimate of the impact directly due to the good separation of control and treatment groups through the age-based eligibility rule. Moving to specification (4) with heterogeneous-effects, the significance of the impact estimate is lost. Its magnitude increases significantly as we turn to those living with partners. Similarly to results for the NRA, a working partner increases the chance of being at work significantly and irrespectively from age. It is interesting to note that the partner's higher education attainment has an independent positive effect (at about 7 percentage point) on employment rates even after controlling for the actual employment of the partner. Marriage appears to command a significant premium as opposed to cohabitation at 13 percentage points (coefficient not included in the table). Using activity on the left-hand side of the equation yields estimates that are not significantly different from what we have obtained here, the only difference being a significant estimate for the heterogeneous case too (see Table 10 in the Appendix). This implies that the effect on employment is not only significant, but is transmitted without resulting in a notable increase in unemployment.

Estimates with old-age pension and disability pension on their left-hand side explain why we observe positive employment effects in this case. There is a strong negative effect on claiming old-age pension in all specifications, but none on disability pension. Estimates are between 7 and

8.8 percentage point, comparable to the employment and activity gains seen earlier. There is remarkable heterogeneity behind these average estimates: those with higher education appear to have reduced old-age and increased disability pension claims at a very high level of about 15 and 20 percentage points respectively.

Table 5

Estimation results for the two ERA episodes with employment as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	0.0481**	0.0639**	0.0690**	0.0493	0.0947***
	(0.0225)	(0.0263)	(0.0274)	(0.0350)	(0.0336)
Impact*education: lower secondary				0.0611	
				(0.0565)	
Impact*education: upper secondary				0.0442	
				(0.0454)	
Impact*education: higher				-0.0662	
				(0.0702)	
Treated (cohort with increased NRA)	0.0443***	0.0507***	0.0592***	0.0594***	0.0660***
	(0.0162)	(0.0177)	(0.0181)	(0.0181)	(0.0219)
Above (the NRA before the treatment)	-0.0997***	-0.105***	-0.110***	-0.110***	-0.132***
	(0.0144)	(0.0160)	(0.0166)	(0.0166)	(0.0198)
Education: lower secondary			0.129***	0.119***	0.118***
			(0.0249)	(0.0271)	(0.0307)
Education: upper secondary			0.243***	0.236***	0.199***
			(0.0191)	(0.0205)	(0.0263)
Education: higher			0.455***	0.459***	0.391***
			(0.0185)	(0.0191)	(0.0312)
Partner works					0.169***
					(0.0222)
Observations	15,624	15,624	15,624	15,624	10,971

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

5. SENSITIVITY ANALYSIS AND DISCUSSION

To assess the sensitivity of the above results to different assumptions, I have performed a number of sensitivity checks. The first of these is varying windows sizes used in the DiD estimation because as already mentioned, there is no rule of thumb to guide our choice of the window size around the retirement age. I have chosen a 4-quarter window because estimates from that on showed the least change across all models and specifications, but this choice is not the only one possible. Table 6 shows estimates of the "Impact" parameter shown earlier for the NRA hikes in all specifications for employment. Sample sizes vary from 4428 to 22.318 as we move from a window of 1 to 6 quarters. Using one or two quarters only appears to be too small of a sample size, producing estimates that are not significantly different from zero except for specification (5) and a window of 1 quarter. The effect of increasing the window size is exactly what we expect with a temporally local impact: larger windows sizes yield larger and more significant point estimates. A 4 quarter window does not only seem to be a good compromise in the stability of estimates while staying close to the focal age, but also yields stable results in the case of NRA-related estimates (not shown here).

Table 6

Sensitivity of impact estimates to the size of the window around the retirement age (ERA, employment on the left-hand side)

Window size	(1)	(2)	(3)	(4)	(5)
1	0.0048	0.0246	0.0248	0.0165	0.0512**
2	0.0042	0.0201	0.0202	0.0162	0.0409
3	0.0310	0.0488**	0.0499**	0.0335	0.0722**
4	0.0481**	0.0639**	0.0690**	0.0493	0.0947***
5	0.0554**	0.0850***	0.0935***	0.0796**	0.1130***
6	0.0533**	0.1070***	0.1170***	0.1070***	0.1230***

Remark: see remarks for Table 5.

The estimates included in the previous section come from a probit model to accommodate the unbalanced share of outcomes in some cases, especially in the NRA hike episodes. Because probit estimates are inconsistent when weighted, these estiamates are missing the weights that come with the LFS required to restore the desired sample structure. Except for the quarter 1 estimate for specification (5), both the pattern and the magnitude of the estimates are the same as before.

All of the above use the full sample to estimate the impact of the intervention. One can argue that a sharper local estimate obtains if we focus on the exact time when the policy change took place. Because such changes are tied to calendar years that is from quarter 4 in year t to quarter 1 in year t+1, this obtains when constraining estimation to the winter quarters only. I have repeated the estimates with this restriction for all specifications and have found estimates similar in magnitude and significance to those from the full sample.

I have also performed a so-called placebo check for all of the specifications showed above. If the effect estimated is to be trusted, we have to make sure that it is not a product of simple coincidence. To look at this, I have defined the treatment variable to pick different cohorts than the ones having actually received the treatment: the 1945 and 1948 cohorts became treated, the 1944 and 1947 became controls with ages 55 and 56 remaining the former retirement ages respectively. In both cases I have run the same regressions as before and obtained similar patterns in terms of explanatory power and the significance of additional variables, but the impact variable was not significantly different from zero in all cases.

Based on the above results, we can be fairly certain that the immediate effect of increasing the effective retirement age by one year increases the employment rate of women at least by 5-7 per cent for all and by around 9.5 per cent for married women. Making looser identifying assumptions, we might want to increase these estimates to 10.7-11.7 and 12.3 per cent respectively. This estimated impact is coming entirely from changes in ERA rules and is somewhat smaller than the available estimates relating to much broader populations and concepts. The intervention appears to affect old-age pension claims directly, but does not seem to have an effect on disability benefit claims except for higher educated persons, which is difficult to explain. The pattern is very different for the NRA, where disability seems to have an important role in diverting the affected individuals away from the labour market and yielding ultimately to an employment effect, which is not significantly different from zero.

The strength of the applied method is its precision and clear focus on those immediately affected by the change. The same feature is a limitation too, as concentrating only on the immediately affected groups is likely to miss spillover- and life-cycle effects, captured by the model of (Major and Varga 2013) among others. If these exist and are net positive, then the total effect is underestimated here, the strategy missing important groups affected by the change for example those not close to the cut-off age. Indeed, the estimating results being sensitive to the choice of the window size is an indication of room for improvement in this regard.

6. CONCLUSIONS

The recent rise in normal retirement ages in several countries has triggered a surge of interest in the precise labour market effect of these measures. Following earlier estimates of the role of other parameters of the pension system in shaping labour market outcomes, a literature on the effect of increasing pension age started to develop recently. This paper contributes to this literature by estimating the effect of various episodes of increasing the normal retirement age (NRA) and early retirement age (ERA) for women in Hungary between 1999 and 2006. The effect is estimated for both types of interventions on survey data that enables looking at different outcomes and the effect of partners' characteristics. The method used is difference in differences defining control and treatment groups based on the changes in the sharp age-based eligibility rules of old-age pension, providing a precise source of identification.

The applied procedure did not aim at and is also incapable of explaining the complete change in employment rates of older people over the observed period, but the analysis highlighted the importance to consider cohort-related confounding factors. The most important of this is schooling, responsible for half of the increase in employment rates. Indeed, the reform appears to be targeted at cohorts with vastly improved labour market chances compared to previous ones due to the introduction of mandatory schooling until age 14 and later 16 as well as the mainstreaming of lower vocational schooling during the 1940s and 50s.

Estimates reveal an interplay of claiming old-age and disability pension as well as labour market status and put existing estimates in perspective. A one year increase in the NRA induces at least a 4.4-7 percentage point decrease in claiming old-age pension, which is however absorbed by a similar increase of disability pension claims. This translates to an ultimately zero effect on employment and activity. Estimates relating to the ERA confirm earlier findings, that increases in the retirement age have had a positive effect on the employment rate of women in the age groups becoming ineligible to state pension after the reform. A one-year increase in the retirement age estimated to yield a 5-7.4 per cent increase of employment rates for all and by around 9.4 per cent for married women. Considering that the employment rate of the affected cohorts is around 45 per cent prior and around 38 per cent after the ERA, this is a significant impact.

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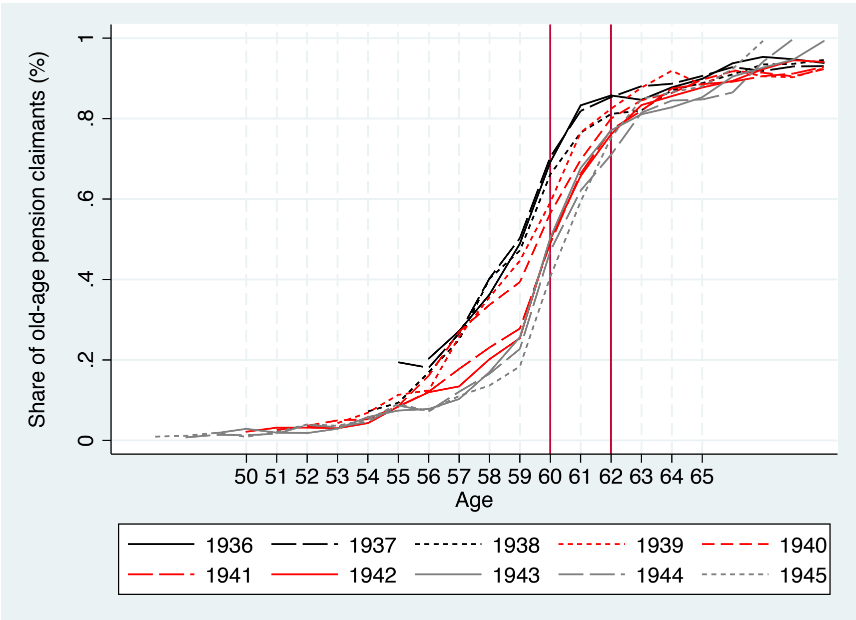
APPENDIX

Figure 8

Cohort-specific age-old-age pensioner profiles of men and women reaching the retirement age between 1997 and 2007

Men



Women

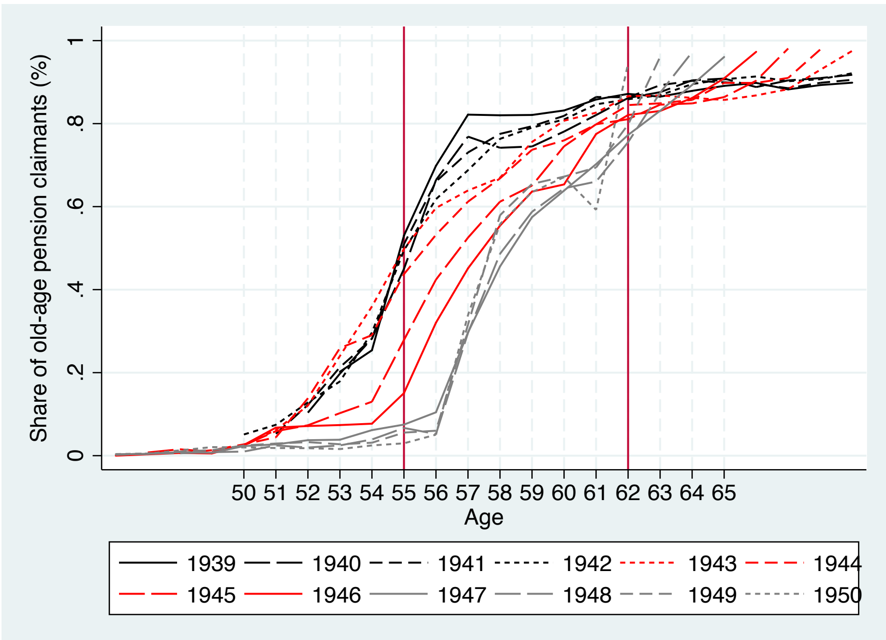


Figure 9

Cohort-specific age-disability pensioner profiles of men and women reaching the retirement age between 1997 and 2007

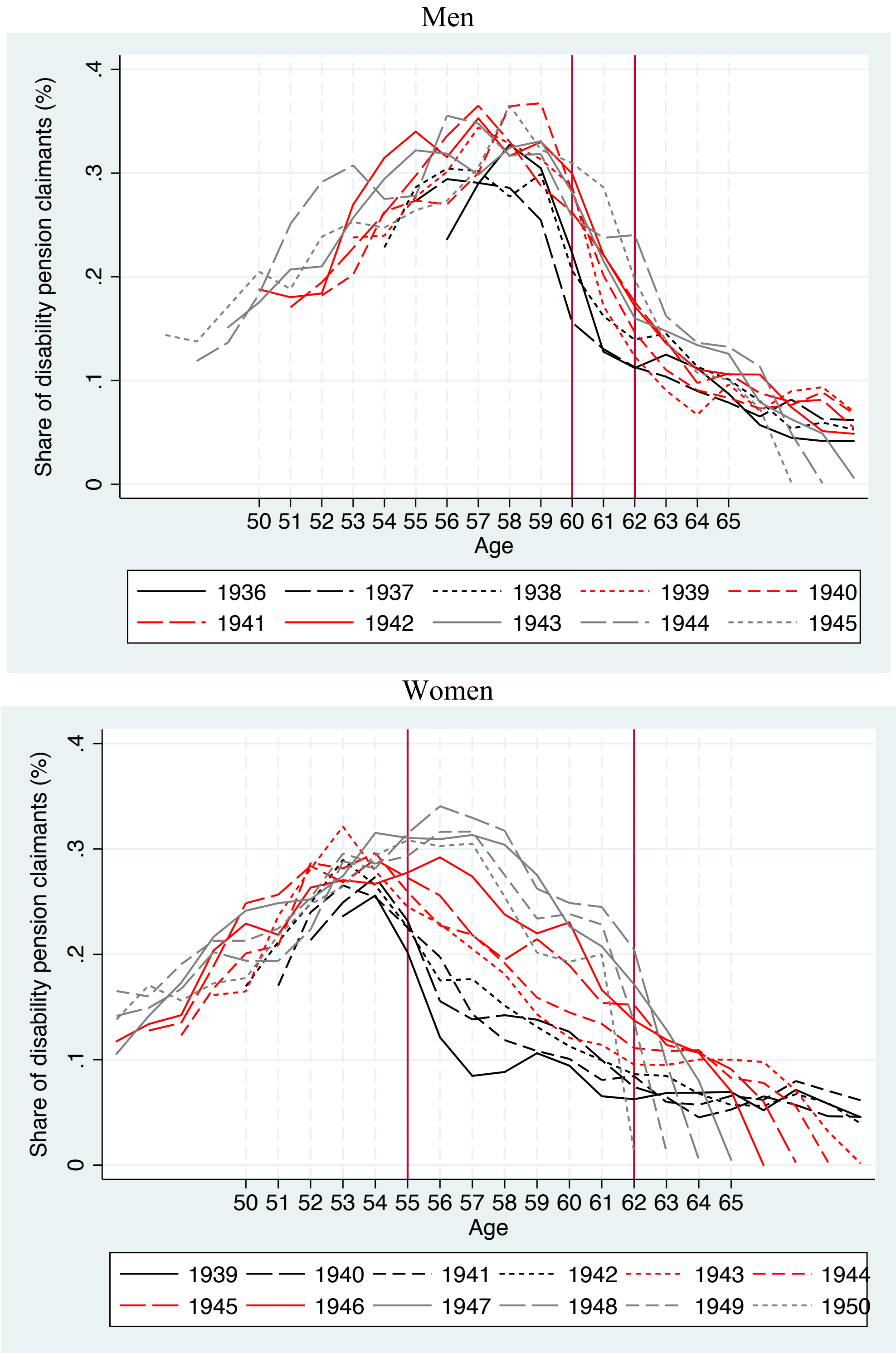


Table 7

Estimation results for the NRA hike episodes with activity as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	-0.00423	0.0249	0.0209	0.0612**	-0.00451
	(0.0158)	(0.0210)	(0.0204)	(0.0306)	(0.0215)
Impact*education: lower secondary				0.00302	
				(0.0452)	
Impact*education: upper secondary				-0.0376	
				(0.0277)	
Impact*education: higher				-0.0917***	
				(0.0210)	
Treated (cohort with increased NRA)	0.0333***	0.0310***	0.0318***	0.0319***	0.0304***
	(0.00835)	(0.00850)	(0.00825)	(0.00828)	(0.00959)
Above (the NRA before the treatment)	-0.0277***	-0.0311***	-0.0258***	-0.0258***	-0.0146*
	(0.00768)	(0.00782)	(0.00765)	(0.00768)	(0.00874)
Education: lower secondary			0.0613***	0.0590***	0.0260
			(0.0188)	(0.0192)	(0.0201)
Education: upper secondary			0.116***	0.119***	0.0643***
			(0.0129)	(0.0132)	(0.0161)
Education: higher			0.397***	0.412***	0.310***
			(0.0215)	(0.0221)	(0.0345)
Partner works					0.172***
					(0.0169)
Observations	28,861	28,861	28,861	28,861	19,700

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Table 8

Estimation results for the NRA hike episodes with claiming old-age pension as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	-0.0222	-0.0442***	-0.0436***	-0.0694***	-0.0591***
	(0.0143)	(0.0153)	(0.0154)	(0.0183)	(0.0190)
Impact*education: lower secondary				0.0330	
				(0.0343)	
Impact*education: upper secondary				0.0349	
				(0.0250)	
Impact*education: higher				0.104***	
				(0.0294)	
Treated (cohort with increased NRA)	-0.0353***	-0.0401***	-0.0457***	-0.0454***	-0.0328**
	(0.0113)	(0.0115)	(0.0116)	(0.0116)	(0.0141)
Above (the NRA before the treatment)	0.0486***	0.0483***	0.0506***	0.0506***	0.0521***
	(0.00918)	(0.00948)	(0.00954)	(0.00954)	(0.0116)
Education: lower secondary			0.112***	0.108***	0.122***
			(0.0206)	(0.0211)	(0.0244)
Education: upper secondary			0.146***	0.142***	0.166***
			(0.0148)	(0.0152)	(0.0200)
Education: higher			0.0309	0.0151	0.0459
			(0.0222)	(0.0230)	(0.0336)
Partner works					-0.0759***
					(0.0214)
Observations	23,324	23,324	23,324	23,324	15,884

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Table 9

Estimation results for the NRA hike episodes with claiming disability pension as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	0.0347*	0.0407*	0.0406*	0.0594*	0.0760**
	(0.0189)	(0.0233)	(0.0231)	(0.0305)	(0.0302)
Impact*education: lower secondary				-0.0528	
				(0.0426)	
Impact*education: upper secondary				-0.00991	
				(0.0357)	
Impact*education: higher				-0.0785	
				(0.0496)	
Treated (cohort with increased NRA)	0.0241***	0.0181**	0.0207**	0.0207**	0.0109
	(0.00892)	(0.00915)	(0.00910)	(0.00910)	(0.0110)
Above (the NRA before the treatment)	-0.0257***	-0.0302***	-0.0332***	-0.0331***	-0.0352***
	(0.00840)	(0.00840)	(0.00834)	(0.00833)	(0.0100)
Education: lower secondary			-0.0676***	-0.0634***	-0.0619***
			(0.0147)	(0.0155)	(0.0178)
Education: upper secondary			-0.106***	-0.106***	-0.101***
			(0.0105)	(0.0109)	(0.0143)
Education: higher			-0.163***	-0.159***	-0.136***
			(0.0105)	(0.0111)	(0.0172)
Partner works					-0.0450***
					(0.0152)
Observations	28,861	28,861	28,861	28,861	19,700

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Table 10

Estimation results for the ERA hike episodes with activity as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	0.0575**	0.0698***	0.0752***	0.0605*	0.0952***
	(0.0225)	(0.0262)	(0.0273)	(0.0347)	(0.0333)
Impact*education: lower secondary				0.0557	
				(0.0562)	
Impact*education: upper secondary				0.0346	
				(0.0455)	
Impact*education: higher				-0.0759	
				(0.0710)	
Treated (cohort with increased NRA)	0.0425***	0.0546***	0.0638***	0.0641***	0.0743***
	(0.0163)	(0.0177)	(0.0182)	(0.0182)	(0.0218)
Above (the NRA before the treatment)	-0.105***	-0.105***	-0.110***	-0.110***	-0.126***
	(0.0144)	(0.0161)	(0.0166)	(0.0166)	(0.0199)
Education: lower secondary			0.117***	0.107***	0.101***
			(0.0248)	(0.0270)	(0.0306)
Education: upper secondary			0.244***	0.239***	0.191***
			(0.0188)	(0.0202)	(0.0261)
Education: higher			0.441***	0.447***	0.374***
			(0.0184)	(0.0189)	(0.0315)
Partner works					0.168***
					(0.0221)
Observations	15,624	15,624	15,624	15,624	10,971

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Table 11

Estimation results for the ERA hike episodes with claiming old-age pension as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	-0.0669***	-0.0770***	-0.0782***	-0.0524***	-0.0879***
	(0.0122)	(0.0129)	(0.0127)	(0.0187)	(0.0138)
Impact*education: lower secondary				-0.0113	
				(0.0357)	
Impact*education: upper secondary				-0.0654***	
				(0.0199)	
Impact*education: higher				-0.0902***	
				(0.0218)	
Treated (cohort with increased NRA)	-0.0663***	-0.0781***	-0.0784***	-0.0780***	-0.0759***
	(0.0119)	(0.0126)	(0.0126)	(0.0125)	(0.0147)
Above (the NRA before the treatment)	0.119***	0.103***	0.103***	0.103***	0.105***
	(0.00994)	(0.0108)	(0.0107)	(0.0107)	(0.0128)
Education: lower secondary			0.0213	0.0222	0.0214
			(0.0174)	(0.0183)	(0.0204)
Education: upper secondary			0.0290**	0.0404***	0.0316*
			(0.0135)	(0.0146)	(0.0184)
Education: higher			-0.0220	-0.0109	-0.00831
			(0.0160)	(0.0176)	(0.0251)
Partner works					-0.0417***
					(0.0137)
Observations	15,624	15,624	15,624	15,624	10,971

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)

Table 12

Estimation results for the ERA hike episodes with claiming disability pension as the outcome (probability contributions calculated from unweighted probit estimates; window = 4 quarter)

	(1)	(2)	(3)	(4)	(5)
Impact (Treated*Above)	0.0322	0.00133	0.00518	-0.0106	0.0134
	(0.0213)	(0.0242)	(0.0248)	(0.0299)	(0.0304)
Impact*education: lower secondary				-0.0119	
				(0.0496)	
Impact*education: upper secondary				0.0193	
				(0.0417)	
Impact*education: higher				0.201***	
				(0.0760)	
Treated (cohort with increased NRA)	0.00463	0.00101	-0.00304	-0.00341	-0.0154
	(0.0152)	(0.0163)	(0.0162)	(0.0163)	(0.0193)
Above (the NRA before the treatment)	-0.00369	0.00103	-0.00213	-0.00214	0.0168
	(0.0135)	(0.0148)	(0.0148)	(0.0149)	(0.0174)
Education: lower secondary			-0.0598***	-0.0571**	-0.0452*
			(0.0209)	(0.0225)	(0.0253)
Education: upper secondary			-0.150***	-0.152***	-0.112***
			(0.0162)	(0.0174)	(0.0228)
Education: higher			-0.275***	-0.288***	-0.219***
			(0.0147)	(0.0142)	(0.0253)
Partner works					-0.0853***
					(0.0202)
Observations	15,624	15,624	15,624	15,624	10,971

Robust (clustered) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1
Specifications include the following variables beside those shown: (2) a full set of year, month and county dummies (for place of residence), (3) in addition to (2): indicators of family status, (4) same as in (3)