Crowding (out) the retirees? RDD application to raising effective retirement age in Poland *

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Abstract

As of 2007 increased labor force participation of the elderly has been observed in Poland. In 2009 a reform in the eligibility criteria narrowed the scope of early retirement opportunities for majority of the occupations. While labor force participation in the directly affected cohorts continued to grow, but an increase already prior to the reform hints that other factors may have been at play as well. The objective of this paper is to isolate and evaluate the causal effect of the changes in eligibility criteria on labor force participation and exit to retirement of the affected cohorts. We rely on Polish Labor Force Survey and employ regression discontinuity design to evaluate the change in participation subsequent to the eligibility reform among the treated cohorts. We find a statistically significant, but economically small discontinuity at the timing of the reform. The placebo test shows no similar effects in earlier or later quarters. Yet, the pure treatment effects are insignificant in vast majority of the specification. Our conclusions are thus as follows: the changes in the eligibility criteria were not instrumental in fostering the participation rates among the affected cohort, i.e. the immediate contribution to increased labor force participation of these cohorts is not economically large.

Key words: retirement age, early retirement, regression discontinuity, Poland

JEL Codes: J14, J26

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1 Introduction

Some of the transition economies adopted policies encouraging early exit to retirement in the early stages of shift from centrally planned to market based system. The rationale for such choice was typically twofold. First, a newly emerging market economy required a different skill composition of the workforce, whereas the training programs on a large scale seemed prohibitively expensive.[1] Early retirement schemes were applied universally (e.g. for all workers with sufficiently long overall tenure) or specifically (special privileges within professions or industries). While in the short run these instruments facilitated transition for the elderly workers providing a safety net, over the next decades the cohorts of young retirees reinforced the pressure from longevity on the pension expenditure. Moreover, in some of these countries, early retirement entitlements were prolonged for subsequent cohorts as well, reinforcing further the pressure on pension expenditure which came from the fact more numerous post-war cohorts reaching retirement age.

Poland is a fairly extreme case of these general tendencies. The minimum eligibility retirement age was 60 for women and 65 for men, but in early transition with 20/25 years of working experience one could retire as early as 55/60. Second, in some professions the minimum eligibility retirement age was not set, whereas the experience requirement was as low as 15 years, resulting in 40-year olds with entitlement to a pension benefit. While these additional privileges concerned only a small fraction of professions, they were decided on in a tripartite dialog, usually under a threat of strike. Given this situation there were numerous efforts to remove the so-called early retirement schemes in general. After a couple of failed legal attempts, as of January 1st 2009, minimum eligibility retirement age was set universally back to 60/65 for all workers in combination with a requirement of working experiencing exceeding 25 years. The only exceptions were based on the so-called medical criteria and concerned individual professions rather than industries. The final legislation was passed in 4th quarter of 2008, leaving little room to workers to adapt to the changed legal rules.

Given these legal constraints, we treat the legislative change as a natural experiment and hope to explore room for potential discontinuity in labor force participation as well as exits to retirement between the siding cohorts of workers. Using detailed individual data from the Polish Labor Force Survey we are able to identify pension eligibility depending on the state of the legislation (i.e. both pre- and post-change). We can thus compare activity and exits to retirement between eligible and ineligible at different points in time, accounting also for loosing the eligibility due to legal changes and remaining eligible despite the legal changes. Our focus remains on individuals aged 50+, i.e. those who could consider the option of early retirement at all. With all these prerequisites, the choice of regression discontinuity design seems natural. It is also well founded in the recent literature, see Van Der Klaauw (2008) as well as Battistin et al. (2009a).

With this paper we hope to contribute to the literature in three ways. First, rather than focusing on the differences in statuses, we provide an analysis of discontinuities in transitions from activity to retirement. Thanks to the panel dimension of the data we observe directly flows into retirement/inactivity, controlling for age cohort. Second, we complement a pure discontinuity in cohort analysis with a fuzzy design, because in addition to age eligibility we also analyse the effects of changes in the occupational eligibility. Finally, we also provide a benchmark for the estimates in the actual quarter of the reform. We account for confounding environment, such as the labor market status of the spouse (retired/active), individual labor market outlooks (e.g. educational level, employment in sunset industries) as well as other household level characteristics (e.g. the "double sandwich" hypothesis for women). We also perform a placebo test, i.e. compare the outcomes of the cohorts affected by the legislation to other cohorts, who faced similar choice, but were not induced by the changes in the eligibility rules.

The findings of our study point to little or effectively no treatment effect of the eligibility reform. Increased labor force participation of the elderly, observed as of 2007, does not seem to be driven by the access to early pension benefits (or lack thereof). We are thus unable to build a causal link from the legislative changes to individual choices. This is not to say, however, that reducing the scope of privileges concerning early retirement was pointless. Neither do we claim that the long run effects of these changes are negligible. Rather we argue that the cohorts directly affected by the legislative changes would have remained more active even in the absence of the reform to a similar extent. The sources of this stronger labor market attachment, thus, still remain to be explored.

[1] In addition, political arguments could have been at play, such as the lump of labor fallacy.

This paper is structured as follows. We start from a brief review of the literature. We then move to describe the details of the reform, thus giving insights into our identification strategy. We describe data in detail in section 4, emphasizing patterns that can be important for the evaluation of the treatment effects. Section 5 discusses the findings of our analyses and the counter-factual experiments. In the concluding section we try to provide some guidance to future efforts to raise the effective retirement age.

2 Experience from earlier empirical literature

Population aging is observed in the most of the western countries. It creates increasing burden on old age security and pension systems. This long run fiscal imbalance becomes acute with cyclical fiscal tensions, such as the one imposed by the global financial crisis. The necessity to reduce public expenditure frequently forces governments to limit the access to early retirement schemes, as has recently been observed in a number of advanced economies, see OECD (2012). This tendency is also projected into the future (European Commission 2012, p. 39).

Most studies suggest that excessively wide eligibility is the main reason for low labor force participation (LFP) of the elderly, e.g. Duval (2003) or Blundell et al. (2002), Quinn (1977). However, in some cases eligibility reforms in the pension system can be offset by growing take-up of alternative social security benefits, as has been argued forcefully by Lammers et al. (2013). That is why the success of pension reforms depends on the labor market decisions made by the affected individuals. Clearly, outdated skills and low employability are often cited as reasons for claiming pension benefits as early as possible, see Bosch and ter Weel (2013). This tendency may be further reinforced if the pension system is not equipped with adequate incentives. For example, if the pay off to staying longer in the labor market is not actuarially fair, leaving early is economically rational, see Hairault et al. (2010).

On the other hand, labor force participation of the older individuals can change independently of the pension system incentives and their reforms. First, cohort effects are in general difficult to be separated from other time-specific factors, see Balleer et al. (2009). The past decades have brought about substantial changes in many aspects that affect participation decisions, notably human capital has increased substantially, health status has improved and the household structure has changed. Healthier and better educated new-coming cohorts are likely to *prefer* longer labor market activity, thus affecting labor force participation of the elderly even without changes in the pension systems. Indeed, there is some evidence, that reforms have only minor effects on labor force participation if they concern persons that would be inactive anyway (Baker and Benjamin 1999).

Given the confounding of the cohort effects with the potential treatment effect of a policy change, approach based on regression discontinuity is an attractive method to evaluate the extent of direct policy impact. Indeed, the changes in the behavior of persons due to changes in eligibility pension benefits is typically sharp and unique in time, see Van Der Klaauw (2008). The variety of studies which apply regression discontinuity to assessing the effects of changes in benefits on labor force participation is large and growing, recent examples include Barrientos and Villa (2013), Chen and Klaauw (2008), Lemieux and Milligan (2008) etc.[2]

This established literature typically considers eligibility changes as "natural experiments". For example Danzer (2013) used regression discontinuity to assess the effects of the substantial increase in minimum pensions implemented in Ukraine. The method is based on the comparison of the discontinuities in the labor force participation at the retirement age before and after the change in benefits generosity. In a similar way, Puhani and Tabbert (n.d.) asses the effects of the steep and unexpected decreases in the generosity of pensions among the German emigrants.[3] Since similar adjustments affected also the disability insurance and benefits, regression discontinuity was also helpful in estimating the labor market effects.

[2]In addition to labor market effects, there is also a vast literature which considers the taking up of retirement as potential discontinuity in consumption – the so-called retirement-consumption puzzle, e.g. Battistin et al. (2009b), Moreau and Stancanelli (2013) – as well as health, Bound and Waidmann (2007), Eibich (2014), Kuhn et al. (2010). A growing body of literature tries to operationalize the value of leisure after retirement (Stancanelli and van Soest 2012), and determinants of household structure changes in the context of retirement (Edmonds et al. 2005, Stancanelli and Soest 2012). Given different objectives of this literature, we further abstract from its findings.

[3]The reform affected individuals who worked outside the Germany in 1990s.

The application that would be the most similar to our objectives in this paper concerns the increases in minimum eligibility age for retirement in Austria, see Staubli and Zweimoeller (2011). In 2000-2010 period eligibility for early retirement schemes has been increased by 2 years for men and by a bit more than 3 years for women Defining the assignment by age in the quarters when the increases occurred, Staubli and Zweimoeller (2011) find substantial effects in terms of the employment rate (6.8 pp. for men and 10.1 pp. for women).[4] Also Lalive and Staubli (n.d.) in the case of Switzerland employs regression discontinuity to assess the consequences of gradual increase in the full eligibility retirement age that started in 1997. They find that confirmed that the changes in labor force participation were correlated with the moves of the retirement age but the adjustment of the labor supply was not immediate. Yet another example includes Bozio (2008). France implemented a reform which increased the number of contribution years for eligibility to full pension. He finds delayed labor market exits among the treated cohorts.

Summarizing, the literature has long been interested in providing reliable estimates when policy change can imply a sharp behavioral adjustment. Regression discontinuity design is such a method, while many of the eligibility reforms in the pension systems constitute valid "natural experiments" for the evaluation of the direct labor market effects. Earlier literature – mostly due to data availability – focused on comparing the labor market status of individuals in the affected to fairly comparable individuals from unaffected cohorts. Most of the policy changes consisted of gradual increase in the minimum eligibility retirement age for full or early pension schemes. Studies are typically an ex-post comparison of participation rates between the affected and unaffected cohorts. Our paper hopes to offer some new insights. First, we can observe directly the flows to retirement, or – to put it bluntly – the decision to retire at the timing of the reform. Thus, our identification is direct. Second, we can compare the timing of the reform to earlier and later periods, to provide a benchmark for the evaluated effects. Finally, some of the individuals maintained the eligibility for early retirement schemes, because the reform introduced occupational eligibility in addition to age-based criteria. Thus, we can offer estimates of discontinuity with additional control for the occupational eligibility.

3 The nature of the eligibility reform and the identification strategy

Early labor market exit was a particularly acute issue in Poland. For example, in 2006 the average exit age for women fell short of 52 with the minimum eligibility retirement age set officially at 60. The reasons for this early exit comprised a multiplicity of legal and contractual arrangements which sometimes acted interactively. First, following the legislative changes from early transition, with 20/25 years of experience one could retire at 55/60. Second, many of the restructuring programs comprised bridging income support for elderly workers who accepted to leave even before reaching this threshold. Finally, in many professions dedicated legislation introduced lowered minimum eligibility retirement age - in some professions as low as 40 years of age. As a consequence, a number of both public and private instruments provided incentives to elderly workers to discontinue labor market activity.

Of the reasons listed above, exceptions based on profession combined with supremacy of experience criterion over the age criterion have proven to yield the highest number of relatively young retirees. For example, 2007 Constitutional Tribunal ruling established, that virtually all professions are entitled to eligibility age at 55/60, rather than 60/65, provided that experience condition is fulfilled.[5] In addition, according to the legislation, experience comprised certain number of years for educational attainment as well as periods of unemployment and inactivity (e.g. due to child-

[4] Blau and Goodstein (2010), Mastrobuoni (2009) analyze also the gradual increases of the minimum eligibility retirement age in USA from 1983. They complement the discontinuity approach with a cohort rather than individual counter-factual simulation, to provide estimates for the delayed effects of the reform. This study confirms that changes in MERA are only a part of the explanation for wider labor force participation of the elderly (in the case of the US it was about a half of the overall increase).

[5] See Parliament of Poland (2008). Technically, early retirement scheme was available five years before the legal retirement age: at the age of 55 for women and 60 for men was accessible for majority of persons that fulfilled the condition of sufficient age, tenure (at least 30 years for women and 35 years for men) or combination of shorter tenure (20 years for women and 25 years for men) and additional individual decision of medical expert about inability to work. These decisions were granted with low selectivity.

bearing). Thus, the number of years *effectively worked* required for the retirement eligibility was substantially lower than 20/25. With a defined benefit system with fixed replacement rate the incentives to remain active – especially in the country with a relatively high unemployment rate – were too low to encourage longer labor market activity. The reform of the pension system from a defined benefit to a defined contribution introduced in 1999 substantially altered the incentives, but the reform was phased in gradually, comprising fully only the cohorts born in 1969 or later. Consequently, in the 2000s, the incentives to leave labor market early continued to be strong. One of the numerous legislative changes – linked to the 1999 reform – attempted re-establishing the effective minimum eligibility retirement age back at 60 for women and 65 for men. However, this legislation had a clause which allowed prolonging the *status quo* in consecutive years.

Given the low activity rates among the elderly and high fiscal costs of pre-mature labor market exit, the attempts to change the regulations concerning the minimum eligibility retirement age have eventually been effective. As of January 2009, the professions with retirement eligibility 55/60 have been limited to those, for whom working conditions necessitate medically hazardous environment. In addition, actual employment in these conditions could not fall short of 15 years of experience and for individuals already economically active at the moment of reform, employment in these professions had to take place in 2008 and for at least one year prior to 1999, while the total minimum experience required to apply for early pension amounts to 20 years for women and 25 years for men.[6] These changes have substantially limited the access to early retirement schemes for 96% of professions.[7]

The political and social debate on the legislative changes were undertaken in the 3rd quarter of 2008, whereas the changes were effective as of January 1st 2009. Moreover, the list of professions eligible was the last item in the negotiations. Consequently, the ability to apply for early retirement in the anticipation of changes was rather limited.

While the room for anticipation effects are not large, it also seems that the legislative change responded largely to challenges and tendencies more relevant for early 2000s, than for 2010s. First, labor market activity of the elderly started growing as early as in 2007. The subsequent quarterly increases in the participation rates reached 2.6 pp in compound terms in the age group 45/50 till 60/65 between the 1st quarter of 2007 and the 4th quarter of 2008, with an average annual increase in participation rate of 1.1 pp. While this tendency continued in the period subsequent to the analyzed reform, reaching overall 8.8 pp by the end of 2013 - it is not granted that the overall increase could be attributed to the change in the eligibility criteria. In other words, it is possible that e.g. cohort effects were strong, making the changes in elderly participation and employment independent of reform in eligibility criteria.

Summarizing, eligibility reform introduced as of January 1st, 2009 treated differently men aged less than 60 or women aged less than 55 on the first day of 2009 when compared to similar women aged 55 or more and men aged 60 or more on the very same day. That allows to define treatment group (eligible for early pensions) and control group younger generations whose eligibility to early pension benefits was reduced only to few professions. Our outcome variables will clearly be the relevant labor market indicators: activity and – for robustness check – exit to inactivity/retirement.

3.1 Identification strategy

Given the design of the eligibility reform, potential alternative explanations for the growth in the participation and employment rates as well as relatively low risk of strong anticipation effects, this reform constitutes a case that can adequately be addressed with the means of a regression discontinuity. Regression discontinuity method relies on the assumption that agents are not able to control the value of assignment variable near the known cut off, see Imbens and Lemieux (2008), Lee and Lemieux (2009). If this condition is fulfilled the method can be considered as more credible than other methods for *quasi*-natural experiments. In our case, most workers born in January 1949 lost eligibility, whereas workers born in December 1948 were eligible and could still apply for

[6]This legislation affects only the general pension system so it does not apply to farmers and separate social security systems, e.g. armed forces, police, justice system etc. Some exceptions from the new regulations have been maintained also in general pension system. The most prominent examples are the early pension schemes for miners and teachers.

[7]The list of professions eligible to early retirement schemes is provided by Central Institute for Labor Protection, and established with reference to a unique survey on working conditions from 2002. The list may be amended if social partners recognize the changes in working conditions within professions.

early retirement schemes if only the experience criterion were fulfilled. While the birth date is fully random, effective experience could only be endogenous if the eligibility reform had gradual phase in, which it had not.

While the design fits well the purpose and the assumptions behind the regression discontinuity approach, we also need the data which permits also identification of professions which lost and professions which maintained eligibility. Labor Force Survey (LFS) in Poland provides a 4-digit identification of occupations, following ISCO.

Utilizing fully the information from LFS, we are able to identify individuals who reached the lower eligibility age in the last quarter of 2008, but no longer fulfilled the age eligibility criterion as of 2009. We can thus establish in the data eligibility based on age. In addition, we also have information on the occupation at the moment of observation, which is indicative of the eligibility based on the professions (although, admitedly, we cannot identify if an individual has held an eligible occupation for the sufficient number of years).

In addition to estimating the sharp regression discontinuity, we can also account for a number of confounding factors. Namely, we can utilize data on the household structure, thus being able to identify, if an individual lives in a household with other retirees, with small children, with other income earners. We can also control for gender as well as the educational attainment of an individual. We use these indicators to test if the treatment effect of the eligibility reform was homogenous across individuals with different confounding enviroment and thus incentives.

Finally, in general labor market exits are typically more frequent in the last quarter of each year and less frequent in the first quarter of each year. Given this pattern, identifying a clear discontinuity may require comparing the quarters encompassing the reform introduction to quarters before and after. Fortunately, LFS is a performed quarterly sice 1995 with a comparable methodology. We can thus test explicitly if discontinuity in the first quarter of 2009 is different from other first quarters in previous and subsequent years as well as whether it is different from the siding quarters. In fact, we use the data for the period 2007q1-2012q4. We begin the sample in 2007 to capture the period after the ruling of the Constitutional Tribunal. The sample ends in 2012 as the latest available data.

In the reminder of this paper we present in detail the data at disposal, the descriptives, the details of the adopted methodology. We then move to the discussion of the results as well as policy implications from this study.

4 Data

To study the effects of the eligibility reform in Poland we rely on Polish Labor Force Survey (LFS), similar to Lemieux and Milligan (2008). This is a representative survey with currently approximately 100 000 individuals participating each quarter. As it is a standardized labor force survey, it provides a reliable source of information about the labor market activity of the population. Most importantly, it is a *quasi*-panel, which permits computing quarterly transition rates. In this section we describe in detail the LFS data.

4.1 LFS sample description

Polish LFS is a representative survey administered quarterly by the Central Statistical Office as of 1993. Given our research focus, we rely on the data between 2007q1 and 2012q4. The first boundary follows from the fact that prior to 2007 early retirement eligibility were different for men and women. The end point is dictated by data availability. Thus, this sample covers two years prior to the eligibility reform and three subsequent years (the reform occurs in the 9th quarter in our study).

In 2012, LFS sampling design results in sample size of roughly 100,000 individuals per quarter, whereas in 2007 the sample size amounted to roughly 50,000 individuals. The design follows three stages (geographical, age and gender stratification). Since some of the location-age-gender group may be slightly over or under-represented, the CSO provides the importance weights for each observation.

The survey follows the 2-2-2 design, which implies that each randomly selected individual is interviewed for two consecutive quarters. After a break of additional two quarters, the individuals

participate for another two consecutive quarters. Roughly half of the sample is rotated between the quarters. Given this design, it is suitable to observe labor market transitions that constituted the output variables described in the section 4.3. Yet, potential sample attrition may bias the results, if – for example — post-retirement individuals were systematically less willing to participate in the survey. We document the scope of attrition in Table 1.

Table 1: The scope of sample attrition

Times individual is observed	**All individuals**	**%**	**Individuals aged 45 and above**	**%**
once	89,856	6.7	38,292	6.24
twice	351,252	26.19	154,975	25.27
three times	225,456	16.81	101,135	16.49
four times	674,480	50.3	318,765	51.99

Source: LFS data, 2007q1-2012q4, the pooled rotating panel comprises 1,341,044 observations.

Both in the case of total population and individuals of interest in this study more than half of the sample is observed all four times. In fact, nearly 95% of the sample is observed twice or more. The share of sample in the 6 years period that was only observed once amounts to app. 6.7%. There seems to be little evidence, however, that the problem intensifies among the older population. In fact, the percentage of individuals participating in all four rounds of the survey is the highest in this age group, whereas the share of population dropping out after the first interview is the lowest. To limit the scope for the sample attrition, we focus on quarterly flows.

The LFS data comprises information about the year of birth as well as identification if an individual already had birthday this year, prior to participating in the survey. Since we dispose of the information about the week of the interview as well as the two information available for each individual, we can compute age indicator with the precision of 0.25 year. This identification is very accurate, i.e. all cases of changing the age between the two interviews can be identified. In the case of person becoming a year older between the two interviews, we can identify if birthday were in the previous year (but after the interview) or already in the new year. This is key for the identification of the eligibility.

The full database used in this paper cover about 1.3 mln observations of persons aged 15-75. Of those individuals we can quantify the size of the subsample eligible for early retirement schemes in any of the analyzed quarters. If there was no reform, approximately 110 thousands individuals were observed for at least two consecutive quarters at the moment of reaching eligibility age (i.e. were within 5 years to reach MERA of 60 years for women and 65 years for men). For this sample we provide the descriptives in Table 2 . Indeed, prior to the reform nearly all individuals with up to 5 years to retirement age were eligible to access early pension schemes and slightly more than half of them actually did claim their pension benefits. There seems to be a small difference in the benefit take up rate prior to the reform - app. 0.9 pp higher among the ineligible than among the eligible workers, but this rate is computed on a very small sample and may thus be misleading. Because the take-up rates hover around 50% prior to the reform, there is a substantial number of individuals who fulfilled age, experience and occupation eligibility prior to the reform, but have not claimed the pension benefits. These individuals keep entitlement to early retirement schemes despite the reform, which explains why app. 20% of ineligible workers are benefit recipients post reform. Extremely low labor force participation among the pre-reform ineligible workers, seems to reflect low attachment to the labor market and possibly alternative life strategies (such as within household allocation of work, migration or non-earned income). On the other hand, relatively high participation rates among those workers who fulfilled the post-reform eligibility criteria suggests that these are occupations or workers with stronger preference for work.[8]

To provide some intuition of the data and the analyzed changes, in addition to the descriptive statistics in Table 2 we provide also computed participation rates and pension benefit take-up rates for age cohorts in Figures 1 and 2, respectively. The dashed line shows the average for the pre-reform years, whereas the colored lines express the rates in the population for 2008-2012. Visibly, labor force participation shifts towards higher values for both men and women, but the shift occurs

[8]Among the professions which maintained the early retirement scheme entitlements are for example medical doctors, fishermen, seamen, teachers and tutors.

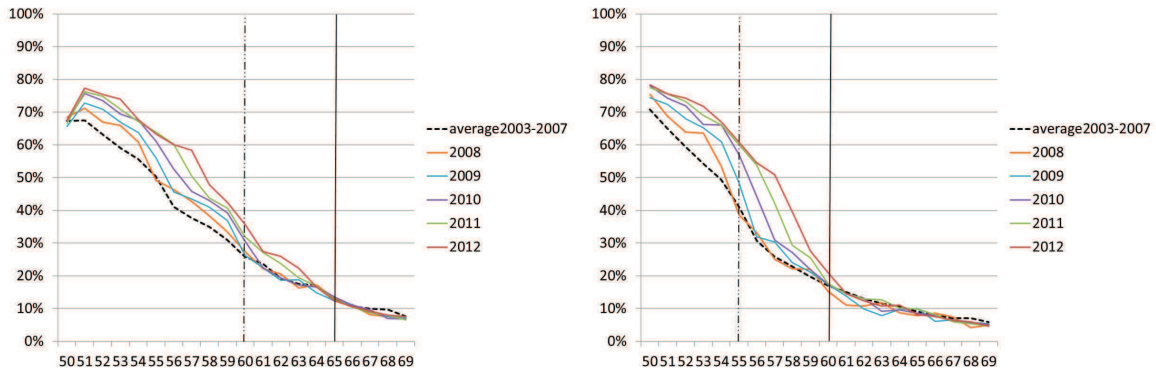
Table 2: Descriptive statistics for individuals at MERA or up to 5 years younger

	Before the reform		After the reform	
	eligible	ineligible	eligible	ineligible
Number of individuals	68,788	562	2,083	38,535
Labor force participation rate (in %)	22.1	18.3	51.1	42.9
Share of pension benefit recipients (in %)	52.7	12.6	39.4	19.3
Quarterly transition probabilities:				
Remained economically active	0.95	0.75	0.94	0.96
Transition rate to early pension	0.05	0.03	0.06	0.03
Transition to inactivity (adequate age)	0.05	0.25	0.06	0.04
Transition to inactivity (adequate tenure)	0.05	-	0.05	0.03
Characteristics of the persons in the sample (in %):				
Share of women	60.8	81.0	29.6	56.8
Tertiary education	12.5	3.9	11.5	13.2
Secondary education	37.1	26.5	32.8	35.8
Vocational	29.4	29.0	38.9	33.4
Primary (including gymnasium)	20.9	40.6	16.8	17.6
Characteristics of HH - other persons (in %):				
worker	44.1	45.6	49.4	50.9
pension benefit recipient	50.7	43.4	46.6	38.6
child	12.8	16.2	13.0	12.5

Source: LFS data, 2007q1-2012q4, the pooled rotating for individual who at each respective quarter were at MERA or up to 5 years younger.
Notes: The sample for all pooled quarterly datasets amounts to 109,968 individuals. Individuals ineligible for regular early pensions before the reform fulfilled age criteria but do not fulfilled work experience criteria. However 48% of them received already special pension or disability benefits.

for both the younger ages and in the 5-year span of their lives that was affected by the eligibility reform. In the case of women, the increases in the participation rates were the most pronounced in the post-reform period in the groups that would have been eligible to early retirement scheme if there was no reform. However, in the case of men, majority of the increased activity takes place already before reaching the early retirement eligibility and the differences attenuate the closer age gets to the minimum full pension eligibility.

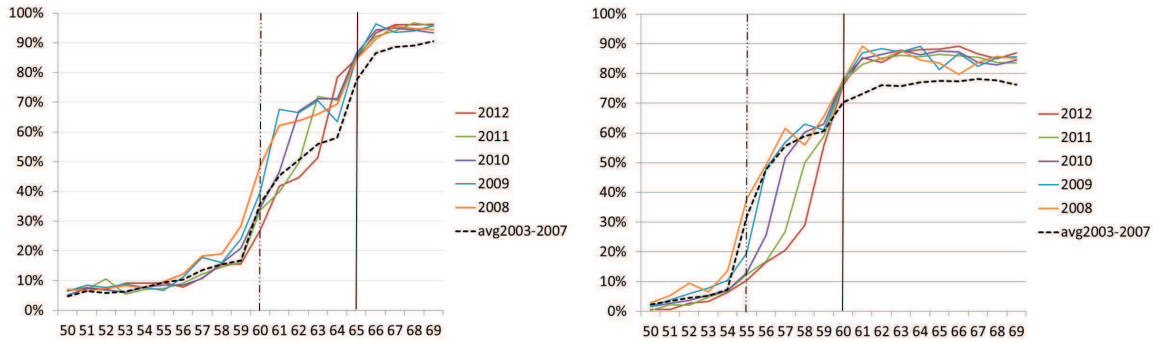
Also the picture for the pension take-up rates seems to suggest that the reform did not have much effect. First of all, already prior to the reform in 2008 there has been a substantial increase in the pension take-up rate for individuals up to 5 years younger than MERA. This increased interest in early retirement pension schemes is less visible among women, but persists among men in nearly all of the analyzed years. In the case of women, consecutive years display gradual delay of pension benefit take up. However, these delays are associated with the higher aggregate overall pension take up rate at MERA, similar as in the case of men.



(a) men (b) women

Figure 1: Changes in the labour force participation rates

Figures 1 and 2 display aggregate rates in the population observed in the data. While they clearly suggest some changes in the labor force participation as well as pension benefit take-up rates, such analysis cannot help to answer if there is a behavioral change in the decisions to retire. In the reminder of the paper we work with the individual level rather than the aggregated data



(a) men (b) women

Figure 2: Changes of the percentages of persons in pre-retirement age receiving pensions

and employ regression discontinuity design to test if the eligibility reform has introduced any star shifts in the decisions to retire.

4.2 Challenges regarding using LFS data

On top of the hazards from potential sample attrition – which we already addressed – there are two additional sources of possible measurement error when using the LFS data for the question at hand. First, we show that the risk of wrong classification of eligibility due to mismeasurement of work experience is low. Second, we demostrate that the identification of eligible occupations – if mismeasured – cannot have a large bearing on the findings of this paper.

Measuring work experience in LFS. The information about the work experience during the whole life is a self-reported variable in LFS. It can thus be treated only as a rough estimate, not necessarily aligned with the the definitions of contributory periods and non-contributory periods used for determining the pension benefit eligibility. These discrepancies may stem from a number of cases. For example, some employment episodes may involve no contributions (e.g. shadow economy or emigration), whereas other periods that involved no employment would count as contributory (e.g. formal education, child bearing, military service, etc.). Also unemployment episodes count as contributory period. Admittedly, there can be important deviations between the eligibility experience and the self-reported experience in most market economies. However, in the centrally planned economy full employment was the official state policy (shadow employment was usually coupled with official registered employment and there was no official unemployment), the measurement error is not likely to be large for older individuals.

Identification of eligible occupations. After the 2009 eligibility reform only a small fraction of occupations yields early pension entitlements. The list of occupations is a part of the legislation (with the changing working conditions this list is to be reviewed systematically). The names of occupations were fully consistent with 4-digit ISCO codes available in LFS. While creating the translation vector between the legislation and ISCO consumes time, it leaves little room for discretion. However, in about 3% of the occupations which lost eligibility, the legal definition was too narrow relative to 4-digit ISCO.

To address this problem, two solutions are possible: (a) consider all workers within a single 4-digit ISCO ineligible, if not all professions are enumerated in the legislation or (b) consider all workers within a single 4-digit ISCO eligible, if at least one profession is enumerated in the legislation. Clearly, the former is "too" narrow, whereas the latter is "too" wide. Given that we are interested in estimating the discontinuity controlling for treatment (occupational eligibility is treatment in our setting), following the second option is preferable, because it yields the most conservative estimate of the discontinuity. In addition, majority of the doubtful cases are highly skilled occupations. We thus introduce educational attainment as one of the confounding factors.

4.3 Method

We employ regression discontinuity design to observe if a change in retirement benefit eligibility (i.e. access to earlier pension schemes) can explain the increase in labor force participation of the older workers. We employ the *quasi*-panel design of LFS, to identify quarterly transitions to inactivity and retirement. Data makes it possible to compare periods prior to the eligibility reform and those already with higher minimum eligibility retirement age. Finally, we can also distinguish between occupations which maintained the right to retire early and those who lost that privilege. In this section we describe in detail the empirical strategy adopted.

Outcome measures. Given the definitional issues associated with LFS data, we consider four outcome measures. First, we observe directly the quarterly transitions between two states: receiving pension benefits in time $t+1$ and being active in time t. We call this outcome measure "transition to early retirement". Identification is based on the source of income declared by individual, which implies that a person identified as receiving pension benefits at time $t+1$ could actually be economically active (e.g. employed or self-employed). This is why we construct a second measure, which captures that a person remains active between time $t+1$ and time t. We call this outcome measure "remaining economically active". While the first of our two measures is indicative of the fiscal consequences (i.e. claiming pension benefits), the second one focuses on the labor market dimension of the analyzed eligibility reform (i.e. remaining in the activity for a longer tenure).

In addition to these two main measures, we also perform two robustness checks. On the one hand, it is possible that a person does not declare claiming pension benefits in the LFS, but is nonetheless inactive. Examples may include situations like still waiting for the pension benefit to be assigned, reaching sufficient age in the near future, but already having sufficient tenure, etc. We thus construct measures, which take the value of 1 if a person becomes inactive between time t and time $t+1$, but with either of the two additional conditions. In the first case, we impose the constraint that an individual has to have minimum eligibility age for early retirement benefits either already at time t or at the latest at the time $t+1$. In the second case, we impose a similar constraint, but instead of age, we use the minimum eligibility experience criterion.

Since we work with the LFS and not with the administrative data, the identification of outcomes is indirect and based on individual declarations. Yet, LFS has a wide coverage and is representative, which implies that any mistakes in self-reporting – so long as they are not systematic – should have little or no bearing on the interpretation of the results.

Assignment. Clearly the eligibility reform could not have affected individuals, who were already eligible for regular pension benefits or still too young. Thus, the assignment variable measures a distance between the age of a person at time $t+1$ and the eligibility age (i.e. 55 years for women and 60 years for men). In the static approach, this would capture the age of a person in the first quarter of 2009 and the respective eligibility age, thus, it takes the value of 0 if an individual *became* eligible in the first quarter of 2009 (having negative values for younger individuals and positive otherwise). Positive or null value of assignment variable mean that person was older than required age, so at least this condition of being eligible to early pension was fulfilled. Negative value of assignment variable mean that persons was covered by the new more restrictive regulations.

The static approach puts the entire emphasis on transitions in one quarter only. However, as already discussed, there are some seasonal patterns in retirement decisions. Thus, to identify the true causal effect of the eligibility reform, we construct a series of placebo tests. In the placebo tests the way to construct the assignment value is the same, but instead of anchoring all values at q1 of 2009, we do it for all prior and subsequent quarters in the sample. Please, note that there was no other eligibility reform between 2007 and 2013. Thus, in principle one should expect no discontinuities in these other 22 quarters. Should they appear, they would set the benchmark against which we could evaluate the size of the discontinuity obtained for q1 of 2009.

The placebo tests reveal also one of the limitations of our approach. Namely, it is possible that the effect in the quarter of the reform is small or negligible, but the accumulation of small effects of increased activity and lower pension benefit claims over the next years could aggregate to a noticeable long run total effect. Clearly, the regression discontinuity design we propose in this study cannot capture this effect. Yet, observing the subsequent quarters we will be able to partially address this point.

Treatment. The core of the eligibility reform was to establish which professions remain entitled to pension benefits at the age of 55/60, and which gain eligibility only at the full statutory age of 60/65 (controlling for the experience). Given the identification of occupations in LFS, we are able to identify the majority of occupations eligible to early retirement schemes after the reform. We used the list of entitled professions issued as a part of the legislation and classified ISCO 4-digit occupations into the two groups. Thus, the treatment variable takes the value of 1 if a certain occupation *lost* eligibility and 0 otherwise.

This detailed classification is sufficient for a large majority of cases. The individual exceptions concern occupations with small number of workers. For example, not all surgeons fulfill the medical criteria formulated by the new legislation, but ISCO at this level of disaggregation makes no distinction between cardiac surgeons (continue to be eligible) and orthopedic surgeons (no longer eligible). We assign surgeons to the treated group (i.e. we act as if the cardiac surgeons lost eligibility, which they did not), which makes treatment fuzzy. To address this point, in addition to the sharp regression discontinuity, we also estimate the fuzzy variant.

Control variables. Some individual characteristics could affect the decision to retire in addition to the exogenous eligibility requirements. For example, better health or better education could translate to higher preference for consumption and lower for leisure, thus leading to postponement of the retirement. Also, labor market activity and retirement decisions often internalize household level preferences in additional to the individual ones. For example, a working spouse may reduce the relative value of retirement leisure, whereas a retired spouse or small children in the household (e.g. grandchildren) could raise the relative value of leisure, this increasing the willingness to retire early. To account for heterogeneity stemming from these effects we included a series of variables that allow to explain to some extent the activity/retirement decisions. We used five categories of educational attainment, the information if there are children in the household, other pensioners in the household as well as data on other economically active individuals in a household.

Estimation. We follow the standard approach in estimating the regression discontinuity, but we estimate it in many variants. First, we have four outcome variables, each of which requires its own estimation. Second, in addition to the main, static assignment, we also perform a series of placebo tests for non-reform quarters in the sample. Third, given that the treatment variable leaves some room for overstating the actual treatment effect, we employ fuzzy in addition to the sharp estimation. Finally, we are able to control for a number of individual and household characteristics.

Consequently, for each quarter (placebo or actual reform) there is four outcome variables, with or without treatment. If treatment is specified, it can be estimated as sharp or fuzzy. Only these two dimension generate 8 estimations for each quarter. Controlling for individual and household characteristics, yields in total 80 specifications with treatment and 80 specifications without treatment in each quarter.

5 Results

We discuss the results in three substantive parts. First, we show the most basic estimations, i.e. for the individuals becoming eligible to early retirement schemes in the first quarter of 2009. We then provide the comparison of the parameters of that model with the parameters of the models with the same specifications estimated for other quarters and also for other subpopulations in order to assess the stability of the results. In the third part we show if and how these results are susceptible to individual and household level controls. The discussion of the results concludes this section.

5.1 Static analysis

We consider four outcome variables that reflect the decisions of persons regarding changes in labor market status and claiming the retirement benefits. We also include the treatment based on the self-reported occupation. Table 3 discusses the findings. The results for estimating discontinuities for all the four outcome variables indicated the significant "jump" at the eligibility age in q1 of 2009. Indeed, individuals were less likely to exit the labor market and claim benefits as a consequence of the reform. The effect is similar for estimates with and without the control for the occupations (i.e.

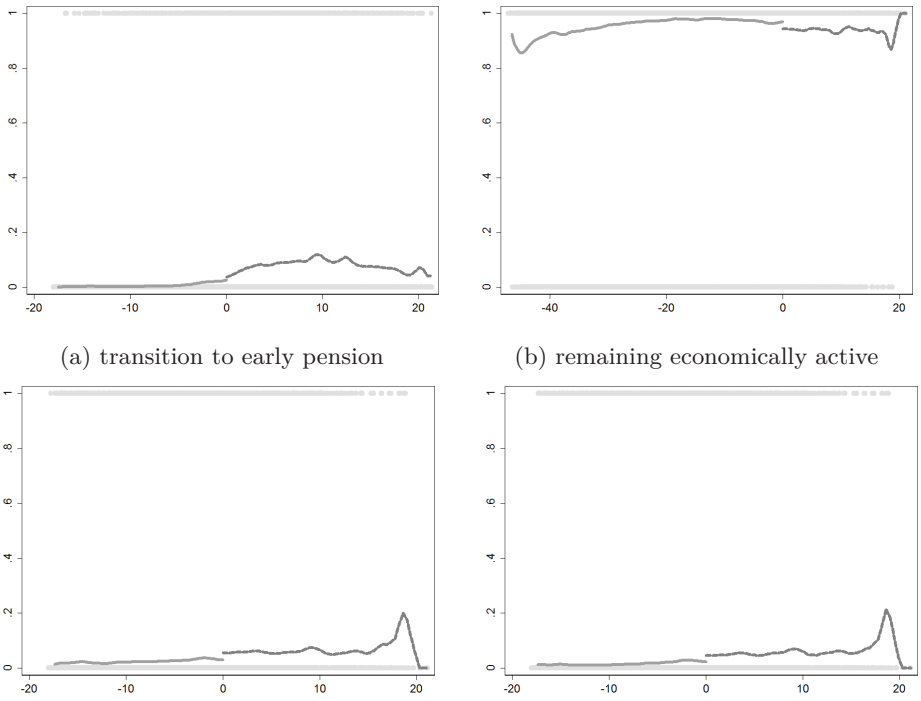
treatment) which may be due to the fact that eligibility was lost by app. 88% of occupations (91% if weighted by employment). However the estimated parameters are relatively small indicating that possible change of the probabilities amounted to about 1-3 pp.

Table 3: RD estimation results: parameter of cut-off and its significance

Model	Coefficient	(Std. Err.)	z-statistic	Significance level
(a) transition to early retirement				
- sharp	-0.0108	0.0066	1.637	0.102
- fuzzy	-0.0118	0.0072	-1.637	0.102
(b) remaining economically active				
- sharp	0.0270	0.0095	-2.836	0.005
- fuzzy	0.0308	0.0109	2.835	0.005
(c) transition to inactivity (adequate age)				
- sharp	-0.0269	0.0095	2.829	0.005
- fuzzy	-0.0308	0.0109	-2.828	0.005
(d) transition to inactivity (with sufficient tenure)				
- sharp	-0.0221	0.0095	2.324	0.020
- fuzzy	-0.0252	0.0108	-2.324	0.020

Notes: Application of -rd- syntax in STATA. Sharp design employs no control for occupational treatment, fuzzy design employs treatment control for occupational eligibility.

The tendencies observed in Table 3 are confirmed by the graphical analysis, see Figure 3. The results confirm the positive impact of the reform on the probability of remaining in the labor force and the negative effect on the probabilities of: the transition to receiving pension benefits, exit from the labor market in pre-retirement age, exit from the labor market in pre-retirement age of persons with long work tenure. However, the estimated parameters are relatively small. The implied possible change of the probabilities falls in the range of about 1 to 3 percentage points.



(a) transition to early pension

(b) remaining economically active

(c) transition to inactivity (elderly with sufficient age)

(d) transition to inactivity (elderly with sufficient experience)

Figure 3: Graphical illustration of RD models estimated for the cut-off between 2008q4 and 2009q1

Significance of the results for the single period does not necessary mean that the changes of this period were unusual in comparison to the changes in other quarters. The results of the "placebo test" on other quarters are described in the subsequent section. Furthermore, in order to consider the results as significant – the discontinuity should also remain significant in different model specifications. This, in section 5.3 we discuss the analysis for estimated models with different spec-

ifications (sharp and fuzzy) and for different subgroups that identify features that can influence the decisions about labor supply and retirement in the household.

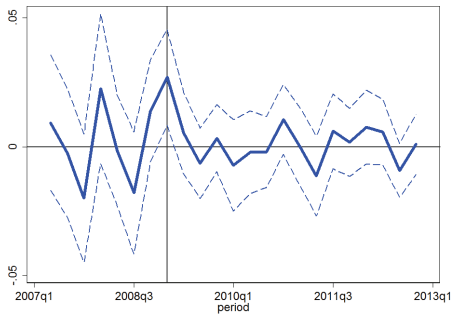
5.2 Placebo test - discontinuities in other quarters

The estimated significant results for the transitions in the quarter of interest need to be set in the context of general tendencies for the break of the year. It is in general frequent that regardless of the actual date of passing the retirement age and/or reaching the minimum work experience, workers tend to retire with the end of the year. We are thus not as much interested in the estimates of discontinuities as we are in these estimates when compared to analogue periods of previous years. We thus repeat the same calculating procedure for the other quarters. Partly, these estimations are a placebo test, but partly they provide a range of estimates at respective quarters. Please note, that while assignment variables are recalculated for every timing, the treatment variable is defined once, as only once the occupational eligibility criteria were reformed.

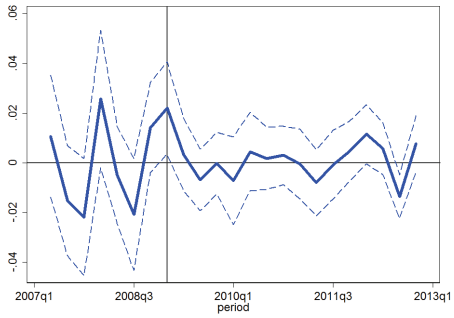
Indeed, the rationale behind our exercise is to verify the hypothesis that the changes observed in the first quarter of 2009 can be distinguished from those observed in other periods. If the estimates of the cut off "'jumps"' are stable the observed parameters of that jumps should be similar or at least should have the same signs. The results are presented in Figure 4. Clearly, the estimates point to no particular pattern. Our quarter of interest seems to be characterized by somewhat larger estimates, whereas in other quarters the discontinuities are statistically insignificant at 5% confidence level.

(a) transition to early pension

(b) remaining economically active



(c) transition to inactivity (elderly with sufficient age)



(d) transition to inactivity (elderly with sufficient experience)

Figure 4: RDD sharp estimates averaged over specifications, for each quarter between 2007q2 and 2012q4, bootstrapped standard errors

We test that notion formally, by running a series of probit and tobit regressions (we report marginal effects in Table 4). In the probit specifications, we set the explanatory variable to be equal to one if an estimate from this particular specification was significant and zero otherwise. In the case of tobit specifications we censor the insignificant estimates to zero. The reform quarter is significant in most of the probit specifications, but insignificant in predicting the size of the estimated effect. Thus, it seems that while the sizes of the estimators may have been volatile in the

analyzed period, the odds that in the reform quarter the actual point estimator is significant are approximately 30-50% higher than in any other quarter.[9]

Including treatment in the specification of the discontinuity estimation does not affect the chances of the estimator to be statistically significant (despite significant share of the population being affected by the treatment). The dummy for the specification with controlling for occupational eligibility is only marginally significant in one of the OLS specifications and insignificant in all probit estimations. Moreover, the estimated sign is negative, which implies that controlling for occupational eligibility – if it has any effect at all – yields lower estimates in the case of discontinuity of remaining active.

Table 4: The "placebo test"

	all outcome variables		**remaining active**		**early retirement**		**transition to inactivity (age criterion)**		**transition to inactivity (experience criterion)**	
	Probit	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**
Reform quarter	0.462***	-0.0010	0.541***	-0.0032	0.484***	-0.0035	0.527***	0.0033	0.268**	-0.0011
	(0.0546)	(0.0049)	(0.104)	(0.011)	(0.104)	(0.009)	(0.104)	(0.011)	(0.114)	(0.007)
Treatment	0.0012	-0.0007	0.0000	-0.0144*	0.0045	-0.0027	-0.0000	0.0123	-0.0000	0.0011
	(0.0191)	(0.0032)	(0.036)	(0.008)	(0.040)	(0.006)	(0.037)	(0.008)	(0.040)	(0.004)
Constant		0.0019		0.0072		0.0020		-0.0062		-0.0004
		(0.007)		(0.006)		(0.004)		(0.006)		(0.003)
Observations	2576	395	644	84	644	109	644	90	644	112

Notes: Marginal effects reported, ***, ** and * indicate significance at 0.1%, 1% and 5% levels.
OLS estimated for significant specifications only.

OLS specifications are of low informational value. While with lower number of observations lack of significance could stem from insufficient power, in fact it does not seem to be the case. Most of the OLS estimates are substantially lower than in the case of probit estimations, whereas the precision of estimation is high. Thus, we further present only probit estimates (OLS estimates available upon request).

5.3 Room for potential heterogeneity

For each treatment and definition of the LHS, approximately 20% of the estimates are statistically significant at all. A similar share of significant estimates does not need to necessitate that these are the same specifications or that the size of the estimated discontinuities is similar. Thus, one final attempt to capture the variation in the discontinuities is to test if the control for the additional individual and household characteristics, which maybe predictive of the retirement decisions. To test that contention we run a series of regressions on the obtained coefficients, which are reported in Table 5.

Treatment effect is insignificant in all specifications, but apparently, in all the specifications the reform quarter proves highly significant – increasing the probability of a significant result by app. 30%. These coefficients are somewhat lower than in the Table 4, which may be due to the fact that all of the specifications in Table 5 include a control for at least one additional confounding factor (in addition to the controls for the definition of the outcome variable). This would suggest that possibly the discontinuities observed in the reform quarter were not a homogenous effect, observed stronger in some group of individuals than in others.

Indeed, consitent with this contention is the observation that the estimates in Table 5 – while mostly we fail to reject the null hypothesis – if significant at all, are negative. Specifications where one controls for a small child in a household seem to produce statistically significant results with lower probability, *ceteris paribus*.

Notably, education is systematically significant: when estimates are obtained for individuals with primary or vocational education, the results are less likely to be significant. It is an interesting observation given that the change in the eligibility criteria should have affected this group the least (most of the hazardous occupations are also low skill or manual occupations).

[9]It should also be noted that the post-reform "placebo" test does not stand a fair chance in the post-reform period. Namely, once the eligibility criteria were changed, reaching the previously sufficient age was no longer sufficient to access the early retirement schemes (except for the still eligible professions, of course). Thus, one cannot reasonably expect significant discontinuities for the sharp designs without treatment, but they could actually be statistically significant in the case of the fuzzy estimates with the treatment. They are not significant, even in the case of outcome measures which involve no legal rules, i.e. transition to inactivity.

Table 5: Patterns of discontinuities – differences across specifications

	(1)	(2)	(3)	(4)
Reform quarter	0.3310***	-0.3311***	0.3309***	0.3312***
	(0.048)	(0.048)	(0.048)	(0.048)
Treatment	0.0000	0.0000	-0.0000	0.0000
	(0.016)	(0.016)	(0.016)	(0.016)
Other retired in hh	-0.0437			
	(0.028)			
Other worker in hh		-0.0331		
		(0.029)		
Kid in hh			-0.0647**	
			(0.027)	
Female				-0.0229
				(0.029)
Primary	-0.0637**	-0.0628**	-0.0619**	-0.0657**
	(0.027)	(0.027)	(0.027)	(0.027)
Gymnasium	-0.0374	-0.0363	-0.0354	-0.0395
	(0.029)	(0.029)	(0.029)	(0.029)
Vocational	-0.0784***	-0.0775***	-0.0767***	-0.0802***
	(0.026)	(0.026)	(0.026)	(0.026)
Secondary	0.0420	0.0433	0.0445	0.0392
	(0.032)	(0.033)	(0.033)	(0.032)
Tertiary	-0.0361	-0.0350	-0.0341	-0.0383
	(0.029)	(0.029)	(0.029)	(0.029)
FE for outcome variables	Yes	Yes	Yes	Yes
Observations	2,576	2,576	2,576	2,576

Notes: Marginal effects from probit estimations.
Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Summarizing, there is some evidence of discontinuity in transitions out of labor market at the quarter of the eligibility reform. On the other hand, the actual treatment effect contributes to neither size of the discontinuity, nor the probability it even exists. Given the small size of the estimates, we attempted to test it they stand out from similar estimates for other quarters (a placebo test). The results seem to suggest that they do not. In fact, the analysis of the parameters for the whole population of models leads to the conclusion that in general the significance of the discontinuities of all outcome variables in the quarter after the reform is more likely, but the estimates are not substantially larger than in the other periods.

5.4 Discussion of the results

The analyzed reform was effectively an abolishment of early retirement schemes. The new, medical criteria for occupational eligibility limited the access to pension benefits before reaching the minimum retirement age to less than 15% of the workers. The change was not gradual and could not be anticipated. Given these conditions one should expect large discontinuities. One should also expect the effects to be stronger for the professions which lost eligibility. Finally, one would also expect the discontinuities to be heterogeneous.

Given these expectations, the results provide a relatively small – though statistically significant – estimator of discontinuity. Depending on a measure, the estimates fall into the range of 1 to 3 percentage points. These effects do not seem to be particularly persistent. Moreover, the differences in participation rates do not seem to be driven by the occupational eligibility (or treatment). To be specific, exhibited willingness to retire did not seem to be substantially smaller among the the workers who lost the occupational eligibility than among those who could still access early retirement schemes.

When compared to the estimates for the other countries – e.g. much less extensive reforms in Austria or Switzerland – the estimated discontinuities were substantially smaller. They were also deprived of clear behavioral patterns. We find no evidence that individual characteristics such as gender or education affect the willingness to retire (or postpone retirement). There is also no support to claim that household level characteristics can explain individual transitions from labor market to inactivity.

Lack of clear pattern explaining the transitions out of labor force and pension benefit take up rates among the workers approaching the retirement age seems to suggest, that increase in labor force participation observed as of 2007 is rather related to other phenomena than to the legislative changes. In this paper, due to the methodological constraints, we effectively provide a tentative estimate of the treatment effect on the treated. The long-run effects of the reform may concern also

the non-treated, i.e. the cumulative effect on the future cohorts who will not be eligible for early retirement schemes.

6 Conclusions

Raising the statutory retirement age or constraining the eligibility criteria for early retirement are typically policies opposed by social actors in most economies. It is often considered that early retirement is a privilege – awarded on a basis of occupation or even simply employment in an industry. In many countries – e.g. France, Italy, Germany – attempts to make the eligibility criteria more strict resulted in general strikes and Poland was no exception from this rule. The economic rationale for such strong resistance to change would need to build on a relatively high valuation of leisure time after exiting the labor market as well as subjective valuation of the unemployment risk after passing the 50 years of age threshold. Yet, in some countries labor force participation grows among the older cohorts. Among others, such trend has started in 2007 in Poland and continues. In 2009 a reform was passed, which highly constrained the eligibility criteria for early retirement. As of 2009 q1 about 85% of professions was no longer eligible for the early retirement schemes, which automatically postponed access to retirement benefits by 5 years for both men and women. In this paper we attempted to evaluate the direct causal effect of this change on exits from the labor market and pension benefit take up rate. We thus tried to assess the immediate effects of the reform.

Following the insights from the literature, we have employed regression discontinuity. Indeed, people tend to retire at retirement age. We were thus not that much interested in one particular discontinuity at the quarter of the reform, but rather if that discontinuity differs from those observed earlier or later. We employed data from Polish Labor Force Survey and carefully developed an assignment strategy, measuring the actual age with sufficient detail to reflect the actual eligibility rules. We constructed four outcome measures: remaining active (despite gaining eligibility), leaving to retirement and exit from labor market without a pension benefit (with or without eligibility). RDD allows thus to asses the effects of treatment on the treated, which can be interpreted as immediate causal effect of the reform.

We find that there are some significant discontinuities in observed probabilities of transitions between the groups eligible and not eligible for early retirement. It confirms that at least in the short run the effect of changes introduced by the reform negatively influenced the probabilities of leaving to retirement and pre-mature labor market exit for elderly workers. There is also a positive causal effect on remaining economically active. However, the estimated effects (discontinuities) are low. More importantly, including the control for actually constrained eligibility has no significant effect on the estimates of discontinuities. In other words, there was some discontinuity in q1 of 2009, but it can hardly be related to the actual legal changes. We thus tested for the patterns in estimates of similar discontinuities over the period 2007q1-2012q4. This can be treated as a placebo test, but also exits to retirement are less likely between q4 and q1 in most of the analyzed years. This exercise reveals that while discontinuity is more likely at the period of the reform, treatment effect is not statistically significant and neither is the role of the confounding environment.

What our results effectively imply is that a large fraction of individuals who remained active in the labor market, would have done so also in the absence of the legislative changes. These results do not imply, however, that in the long run there is little or no room for the labor market effects of constraining the eligibility for early retirement schemes. Thus, our paper offers some interesting policy insights. First, this analysis confirms that the instant effect of the increase of the reform on labor force participation was positive and significant but the estimate was relatively small, which implies also small instantaneous fiscal savings. This may explain why – given the social resistance – in many countries eligibility reforms are delayed or narrowed in scope. Second, in Poland these are the strong cohort effects that lie behind the continuous increase in the labor force participation among the elderly. The reasons for these cohort effects are yet to be determined. We tested a variety of confounding factors, such as education, gender and household structure and they do not seem to be powerful in explaining the observed patterns. This topic calls for further empirical research, especially given the relatively low take up rates of the early retirement scheme among the eligible. In sum, despite the strong opposition of trade unions and social actors, individual willingness to continue labor market activity seems to be relatively high and eligibility reforms only

reinforce these trends.

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