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ABSTRACT

A Regression Discontinuity Evaluation of Reducing Early Retirement Eligibility in Poland*

The reform introduced in Poland in 2009 substantially and abruptly reduced the number of workers eligible for early retirement. This paper evaluates the causal effects of this reform on labor force participation and exit to retirement. We use rich rotating panel from the Polish Labor Force Survey and exploit the discontinuity imposed by this reform. 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, but in a vast majority of specifications the discontinuity is not larger for the treated individuals, i.e. those whose occupation lost eligibility. We interpret these results 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.

JEL Classification: J14, J26

Keywords: retirement age, early retirement, regression discontinuity, Poland

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1 Introduction

Some of the transition economies adopted policies encouraging early exit to retirement in the early stages of the shift from a centrally planned to a market based system. Typically, a newly emerging market economy requires a different skill composition of the workforce, whereas the training programs on a large scale seemed prohibitively expensive, thus serving as a rationale for implementing early retirement schemes. Such schemes were applied either 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 provided a safety net for the elderly workers, over the next decades the cohorts of young retirees reinforced the pressure from longevity on the pension expenditure.

Poland is a fairly extreme case of these general tendencies. The minimum eligibility retirement age was originally 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 full entitlement to a pension benefit. Gradually, more occupational groups were able to negotiate preferential retirement arrangements. As a result, in the mid 2000s nearly everybody was entitled to claim benefits as early as 55/60 and in some occupations it was even 50/55.

This situation changed with a bill passed in December 2008 and effective as of January 2009. Minimum eligibility age of 60/65 was restored universally, with full eligibility requiring 25 years of work experience. Roughly 9% of workers were eligible to retire earlier, with a very specific list of occupations providing early retirement entitlement. Moreover, this exception applied only to those workers who were employed in one of the few occupations prior to January 2008. Given this design of the reform, no anticipation effects were possible, while the eligibility reform introduced a large discontinuity in pension eligibility. We exploit this discontinuity using detailed individual rotating panel data from the Polish Labor Force Survey. We focus attention on early exits from the labor market among individuals aged 50/55+. The way the reform was implemented fits well the requirements of the regression discontinuity (see Van Der Klaauw 2008, Battistin et al. 2009a).

We contribute to the literature in three ways. First, we provide an analysis of discontinuities in transitions from activity to retirement, rather than focusing on the labor market status. The panel dimension of the data permits to observe directly the flows into retirement/inactivity, controlling for age and birth cohort. Second, we complement a pure discontinuity in cohort analysis with a fuzzy design, because in addition to age eligibility we also analyze the effects of changes in occupational eligibility. Third, we provide a benchmark for the estimates in the actual quarter of the reform by a series of placebo and conditional specifications. This allows to evaluate the (immediate) size and heterogeneity of the treatment effects. We find small effects of age eligibility reduction and effectively no effects of occupational eligibility. Hence, increased labor force participation of the elderly, observed even prior to the reform, seems to be driven by factors unrelated to early pension eligibility.

This paper is structured as follows. We start with 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 the data in section 4 and the identification strategy in section 5. Main findings along with the placebo tests and heterogeneity analysis are reported in section 6. The paper is concluded by policy implications of our study.

2 Insights from earlier empirical literature

Most studies suggest that too broad pension eligibility is the main reason for low labor force participation (LFP) of the elderly (e.g. Duval 2003, Quinn 1977, Blundell et al. 2002). However, in some cases eligibility reforms in the pension system can be offset by growing take-up of alternative social security benefits (Lammers et al. 2013). For this reason, the success of eligibility 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 (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 (Hairault et al. 2010).

However, labor force participation of older workers 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 (Balleer et al. 2009). The past decades have brought about changes in many aspects that affect participation decisions; notably, human capital has increased, 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).

Labor force participation at eligibility for pension benefits is typically sharp and unique in time, which permits identifying the causal effect of the eligibility reform via regression discontinuity (RD, see Van Der Klaauw 2008). Indeed, there is a growing body of literature exploiting RD to assess the effects of changes in benefits on labor force participation (Lemieux and Milligan 2008, Chen and Klaauw 2008, Barrientos and Villa 2015).[1] For the transition countries, Danzer (2013) used regression discontinuity to assess the effects of the substantial change of the minimum pensions generosity in Ukraine. Puhani and Tabbert (2016) asses the effects of the steep and unexpected decreases in the generosity of pensions among repatriated ethnic German workers.

[1] In addition to labor market effects, there is also 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 (2015) – as well as health, Bound and Waidmann (2007), Kuhn et al. (2010), Eibich (2014). 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).

The Polish reform concerned eligibility rather than generosity, though. Hence our study is more similar to the literature analyzing similar reforms in advanced market economies. In the USA, eligibility for retirement was raised as early as 1983. This reform was exploited in the literature (see Mastrobuoni 2009, Blau and Goodstein 2010) and, apparently, changes in the eligibility age can explain only a part of the rise in elderly labor force participation. In Austria, between 2000 and 2010 the eligibility for early retirement schemes has been increased by 2 years for men and by a bit more than 3 years for women. This reform was analyzed by Staubli and Zweimoeller (2011), who find substantial effects in terms of the employment rate (6.8 percentage points for men and 10.1 for women). In Switzerland, a gradual increase in the eligibility age was started in 1997, as analyzed by Lalive and Staubli (2014). 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. In France, the eligibility reform concerned the years of working experience rather than age and was analyzed by Bozio (2008). He finds delayed labor market exits among the treated cohorts.

Summarizing, most of the previously analyzed eligibility age reforms consisted of a gradual increase in the minimum eligibility age for full or early pension schemes. Meanwhile, Poland introduced an immediate and large change in eligibility age: 5 years difference for two adjacent birth cohorts. Our paper studies the effects of this reform. Against the earlier literature, we offer several novel insights. First, rather than stocks of active/retired we observe the flows from activity, i.e. the decision to retire around the reform. Second, we use general tendency to retire at eligibility as a benchmark for the estimated treatment effects. The placebo tests we perform permit judging the magnitude of the observed changes. Third, we exploit the heterogeneity of these treatment effects: some of the individuals maintained the eligibility for early retirement schemes, whereas individual characteristics could have played a role in the decision to retire.

3 The situation in Poland and the 2008 eligibility reform

Early labor market exits were increasingly prevalent in Poland. By the mid 2000s, the average exit age for women fell short of 52, despite the minimum eligibility age at 60. This discrepancy occurred due to several exceptions in the legislation, of which the most common was the early retirement eligibility at the age of 55 for women and 60 for men, encompassing virtually all occupations as of 2007. Early retirement was allowed if tenure of 35 years for men or 30 years for women was documented. In addition, with at least 20/25 years of documented tenure, full eligibility could be obtained conditional on medical evaluation. Moreover, in many professions dedicated legislation introduced a lowered minimum eligibility retirement age - in some professions as low as 40 years of age. Workers in these occupations could retire up to 5 years prior to the legal retirement age, if they documented sufficiently long tenure. Finally, tenure comprised years of education, years of child bearing and rearing as well as periods of unemployment in the case of some occupations. Thus, the number of years *effectively worked* required for the retirement eligibility was in many cases lower than 30/35. In sum, there was a variety of publicly provided instruments for elderly workers, securing their access to retirement benefit ahead of legal retirement age.[2]

The reform in the eligibility age regulations was presented to the parliament in the 4th quarter of 2008 and became effective as of January 2009. The reform removed access to early retirement schemes for 91% of workers.[3] The eligibility age was set universally for women at 60 and for men at 65. The only early retirement exception concerned occupations for which working conditions impose a medically hazardous environment.[4] The reform was universal (only police, judges and military were exempt from the legislation). It was also immediate in a sense that the change in the eligibility age was introduced abruptly, with no transition periods, nor step-wise increases in the pension eligibility age, see Table 1.

Table 1: Eligibility criteria for early pension before and after the reform

	Before the reform		**After the reform**	
	age	**tenure**	**age**	**tenure**
Men born until 31.12.1948	60	35 (25)	60	35 (25)
Men born after 31.12.1948				
not eligible for early retirement	60	35 (25*)	65	25
eligible for early retirement	60	35 (25*)	60	25(15**)
Women born until 31.12.1953	55	30 (20*)	55	20
Women born after 31.12.1953				
not eligible for early retirement	55	30 (20*)	60	20
eligible for early retirement	55	30 (20*)	55	20 (15**)

Notes: *The minimum required tenure was shorter for persons with an additional individual decision of a medical expert about inability to work. **Minimum tenure in occupations hazardous to health listed in the new regulations.

The reform created ideal conditions for a regression discontinuity analysis, all the more so given that no anticipation effects could blur the results. However, it appears that the reform responded largely to challenges and tendencies more relevant for the early 2000s, than for the 2010s. First, labor market activity of the elderly started growing as early as 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. Figure 1 displays aggregate rates observed in the data.

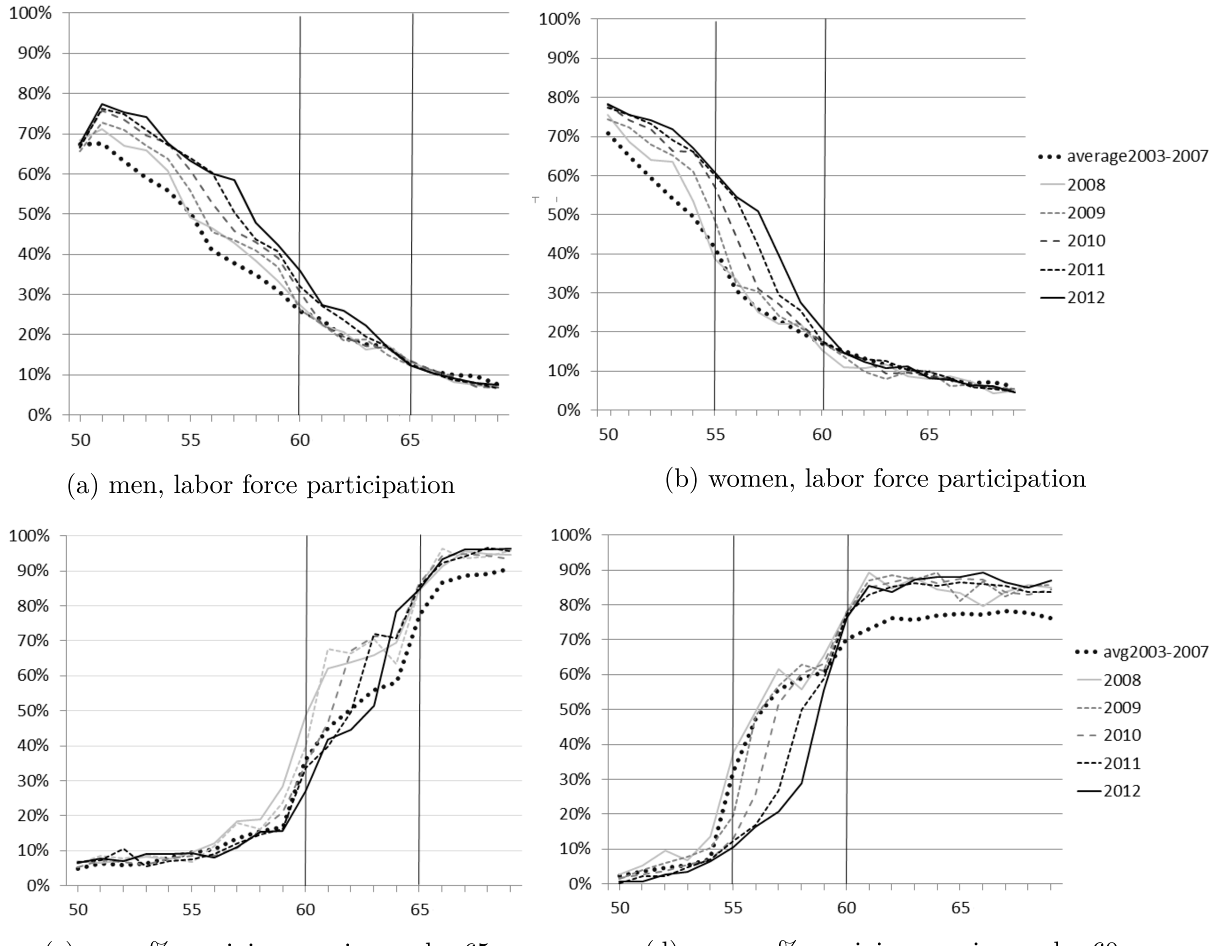
In the case of women, the increases in the participation rates were most pronounced in the post-reform period in the groups that would have been eligible to the early retirement scheme if there had been no reform. In the case of men, the majority of the increased activity takes place before reaching the early retirement eligibility already and the differences attenuate the closer

[2] Many of these exceptions are documented in: Parliament of Poland (2008). Admittedly, changing a defined benefit system to a defined contribution one, as implemented in 1999, provided incentives to prolong labor market activity for both men and women, but this reform affected cohorts born in 1969 or later.

[3] The list of professions eligible to early retirement schemes is provided by the 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 changes in working conditions.

[4] In addition, employment in medically hazardous conditions could not fall short of 15 years of actual working tenure and employment in these professions had to be recorded in 2008 and for at least one year prior to 1999. Additionally, the total minimum actual working tenure was set at 20 years for women and 25 years for men.

Figure 1: Aggregate changes in activity and retirement



(a) men, labor force participation

(b) women, labor force participation

(c) men, % receiving pension under 65

(d) women % receiving pension under 60

Source: data from the Polish Labor Force Survey, quarterly samples combined for 2003q1-2012q4, with population weights.

age gets to the minimum full pension eligibility. Also the changes in the pension take-up rates suggest that the reform did not have much effect for men. 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 eligibility age, similar as in the case of men. While many tendencies concerning increased participation continued in the period subsequent the eligibility reform, it is possible that e.g. cohort effects were strong, making the changes in elderly participation and employment independent of reform in eligibility criteria.

Clearly, the differences in the aggregate participation rates and pension take-up rates cannot be used as causal estimates for the effects of the eligibility reform. The reform introduced substantially different eligibility conditions for 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. This discontinuity on January 1st of 2009 provides a direct identification for the RD estimation. Note also that some workers from the younger birth cohorts remained eligible for early retirement.

4 Data

In parallel to earlier studies, we use data from the Labor Force Survey (LFS, see Lemieux and Milligan 2008). The Polish LFS is a representative, standardized quarterly survey. In 2007 roughly 50 000 individuals participated in each wave, this number grew gradually to 100 000 in 2012.

4.1 LFS sample description

We use data between 2007q1 and 2012q4, covering two years prior to the eligibility reform and three subsequent years. The reform occurs in the 9th quarter in our study.

The Polish LFS is a rotating panel, which permits observing quarterly labor market flows. The survey follows the 2-2-2 design, hence 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. Yet, potential sample attrition may bias the results. We document that attrition is not a problem in our sample, i.e. distributions are remarkably close to theoretical, given the sample design, see Table 2.

Table 2: The scope of sample attrition

Times individual is observed	**All individual**	%	**Individual within 5 years to retirement age**	%
once	84,027	6.5	8,107	5.9
twice	333,603	25.6	33,693	24.6
three times	219,328	16.8	22,567	16.5
four times	666,024	51.1	72,787	53.0

Source: LFS data, 2007q1-2012q4, the pooled rotating panel comprises 1,302,982 observations.

The Polish LFS 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, we can compute the specific age of an individual at the moment of the interview. This identification is accurate in a sense that all cases of changing the age between the two interviews can be identified. In the case of a person becoming a year older between the two interviews, we can identify if the birthday was in the previous year (but after the interview) or already in the new year. This is key for the identification of eligibility.

The LFS reports also sources of income for an individual, including a category identified as pension or early pension benefits. Thanks to this variable, we can identify pension take-up rates independently of the reported labor market status. Combining professional activity with claiming pension benefits is allowed throughout the period of our study.

In the study we use observations on all those individuals for whom a quarterly flow could be computed, i.e. the individual was observed at least twice. Hence, we are able to utilize roughly 93% of the 1.3 million observations. Of those, 12,9047 observations satisfy the age criterion, i.e. concern individuals at legal retirement age or up to 5 years prior that threshold. This is a large sample, which is important, because transitions in general are rare. Approximately 129 thousand individuals were observed for at least two consecutive quarters at the moment of reaching eligibility age (e.g. were within 5 years to reach eligibility age of 60 years for women and 65 years for men after the 2009 reform within majority of occupations). For this sample we provide the descriptive statistics in Table 3.

Table 3: Individuals within 5 years of reaching the early retirement eligibility

	Before the reform		**After the reform**	
	eligible	**ineligible**	**eligible**	**ineligible**
Number of individuals	83,372	4,975	3,941	36,759
Labor force participation rate (in %)	21.3	8.1	53.0	42.7
Share of pension benefit recipients (in %)	56.7	58.7	34.4	18.8
Quarterly transition probabilities:				
Remained economically active	0.95	0.65	0.95	0.96
Transition rate to early pension	0.06	0.03	0.06	0.03
Transition to inactivity (adequate age)	0.05	0.35	0.05	0.04
Transition to inactivity (adequate tenure)	0.05	-	0.04	0.03
Individual characteristics (in %):				
Share of women	61.8	61.7	25.0	56.7
Tertiary education	12.8	9.3	12.3	13.2
Secondary education	37.0	21.9	28.3	36.4
Vocational education	28.7	31.1	43.1	32.7
Less than vocational education	21.5	27.7	16.1	17.7
Households with:				
other worker (in %)	42.7	41.4	47.7	51.1
other pension benefit recipient (in %)	52.2	48.0	46.9	38.1
child (in %)	12.7	13.0	13.0	12.5

Source: Polish LFS data, 2007q1-2012q4, the pooled panel, subsample for individuals with $age \in < legal - 5, legal >$, where $legal$ denotes the legal retirement age in a given period. Individuals ineligible for early pension schemes before the reform satisfied the age criterion, but not the experience criterion.

Prior to the reform, nearly all individuals with up to 5 years to retirement age would have been eligible to access early pension schemes and slightly more than half of them actually did claim pension benefits, which hints that there was a substantial number of individuals who fulfilled the eligibility criteria, but did not claim the pension benefits even prior to the reform. These individuals keep entitlement to early retirement schemes despite the reform, which explains why approximately 20% of ineligible workers are benefit recipients post reform. A relatively high participation rate among those workers who fulfilled the post-reform eligibility criteria suggests that these are occupations are held by workers with stronger preference for work.

While sample attrition does not seem to bring about a source of bias, tenure is measured with an error. First, in the LFS it is typically a self-reported variable, hence it is rounded up and typically imprecise. Moreover, it does not have to be aligned with the legal definitions of tenure. Pre-reform, education years and periods of maternity leave were included, which people intuitively do not report as working experience for the most part, post-reform they were no longer relevant. Furthermore, some employment episodes may involve no contributions (e.g. shadow economy or emigration). 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 individuals who spent more than half of their professional life in central planning, as is the case of our sample.

Finally, after the 2009 eligibility reform only workers in specific occupations remained eligible for early pensions. The list of occupations was part of the legislation. The names of occupations were fully consistent with 4-digit ISCO codes available in the LFS. Occupation by occupation, we created mapping between the list of eligible occupations and ISCO codes in the LFS. However, in about 3% of the occupations which lost eligibility, the legal definition was too narrow relative to 4-digit ISCO. 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). To address this problem, two solutions arise: (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. Notably, nearly all doubtful cases are highly skilled occupations.

5 Method

The 2008 eligibility reform in Poland constitutes a case that can adequately be addressed with the means of a regression discontinuity. The regression discontinuity method relies on the assumption that agents are not able to control the value of the assignment variable near the known cut off, see (Imbens and Lemieux 2008, ?). In our case, most workers born in January 1949 lost eligibility, whereas workers born in December 1948 were eligible for early retirement, conditional on a relatively lax experience criterion. While the birth date is fully random, effective experience could only be endogenous if the eligibility reform had a gradual phase in, which it had not.

Eligibility for pension benefits depended on the age of the person at the moment of the reform, i.e. January 1st, 2009. We seek treatment effect, τ, at the cutoff c of the assignment variable X while $Y_i(1)$ and $Y_i(0)$ represent the values of the outcome variable in the sub-samples with and without treatment respectively. Utilizing fully the information from the LFS, we are able to identify individuals who reached the 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 the sharp variant of the regression discontinuity design (Lee and Lemieux 2010) it can be described as follows:

$$\tau = E[Y_i(1) - Y_i(0)|X = c]$$

In addition, we also have information on the occupation at the moment of observation, which is indicative of the eligibility based on hazardous working conditions. Hence, we can also identify eligibility based on occupations, i.e. identify individuals who can retire at an age younger than the legal retirement age also after the reform, we can further estimate the RD in fuzzy variant. Notably, the LFS reports tenure as one number rather than an entire work history, hence we cannot be sure that a person working currently in a given occupation has held a job with similarly hazardous conditions for a sufficient number of years. We assume conservatively that it was the case.

Outcome measures. 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 for individuals below the legal retirement age in a given period. 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 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 experience criterion.

Assignment variable and treatment. The assignment variable measures a distance between the age of a person at time $t+1$ and the eligibility age. In the static approach, this would capture the gender-specific age of a person in the first quarter of 2009. The assignment variable 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). There is only one assignment value for each individual.

The static approach puts the entire weight on transitions in one quarter only. However, as already discussed, there are some seasonal patterns in retirement decisions. Thus, 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. 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 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 issue.

Treatment, sharp and fuzzy approach. 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 even 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 the vast majority of cases. The individual exceptions concern occupations with a small number of workers. 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 preference for leisure, thus leading to postponement of the retirement. Also, labor market activity and retirement decisions often internalize household level preferences in addition 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, thus 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 estimate RD in many variants. First, we have four outcome variables, each of which requires its own estimation. Second, 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 seasonal pattern, identifying a clear discontinuity may require comparing the quarters encompassing the reform introduction to quarters before and after. Thus, in addition to the main, static assignment, we also perform a series of placebo tests for non-reform quarters in the sample. These placebo specifications 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. 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. These two dimensions alone 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.

6 Results

6.1 Static analysis

Table 4 reports the estimates of the discontinuity. Effectively, we report a change in the intensity of a given flow, subsequent the reform. For example, on average 5% of individuals leave for early retirement per quarter, as reported in Table 3. With the eligibility reform as of the 1st quarter of 2009, this number is reduced by 1.1-1.2%, with $p - value \approx 0.10$. We present also a result of a simulation exercise, denoted in table as 'theoretical maximum': if all the persons followed the law (e.g. retired at earliest eligibility, etc), the treatment effect of the eligibility reform would have amounted to roughly 3.7%, hence the materialized reduction in early retirement exits was roughly 30% of the theoretically possible reduction.

We consider four outcome variables that reflect the decisions of persons regarding labor market status and claiming the pension benefits. Indeed individuals were less likely to exit the labor market and claim benefits as a consequence of the reform, but these effects are relatively small. The effects are similar for estimates without (sharp) and with (fuzzy) control for the occupations, which may be due to the fact that occupational eligibility was lost by approximately 91% of workers.

Figure 2 presents graphically the result of the sharp regression discontinuity. In the point zero we have the youngest persons eligible for early pensions after the reform. On the positive side are the observations for older persons that remained eligible for early pensions. Persons covered by the reform due to their age have negative values. The aim of the method is to measure the discontinuity in the point zero between two separate mathematical functions fitted to the observations for persons treated and not treated by the eligibility reform. Clearly, even eligible cohorts would intensify leaving labor market at much later age, with outflows to retirement peaking at 4 years after the early eligibility (i.e. 1 year prior to regular eligibility). Particularly noteworthy are the early exits to inactivity, without pension benefits, which *intensify* as a result of the eligibility reform. These exits are either supported within household or funded from alternative public sources, such as e.g. disability insurance.

Table 4: RD estimation results: treatment effect and its significance

Model	Coefficient	(Std. Err.)	z-statistic	Significance level
(a) transition to early retirement				
- sharp	-0.01	0.01	-1.64	0.102
- fuzzy	-0.01	0.01	-1.64	0.102
- theoretical maximum	-0.04	0.01	-6.53	0.000
(b) remaining economically active				
- sharp	0.03	0.01	2.84	0.005
- fuzzy	0.03	0.01	2.84	0.005
- theoretical maximum	0.05	0.01	3.73	0.000
(c) transition to inactivity (adequate age)				
- sharp	-0.03	0.01	-2.83	0.005
- fuzzy	-0.03	0.01	-2.83	0.005
- theoretical maximum	-0.05	0.01	-3.73	0.000
(d) transition to inactivity (with sufficient tenure)				
- sharp	-0.02	0.01	-2.32	0.020
- fuzzy	-0.03	0.01	-2.32	0.020
theoretical maximum	-0.04	0.01	-3.87	0.000

Notes: Sharp design employs no control for occupational treatment, fuzzy design employs control for occupational eligibility. Theoretical maximum computed on a simulated sample, where instead of actual behavior of the individuals, we impose the eligibility (i.e. all eligible to retire, claim pension benefits, nobody ineligible quits the labor market, etc.)

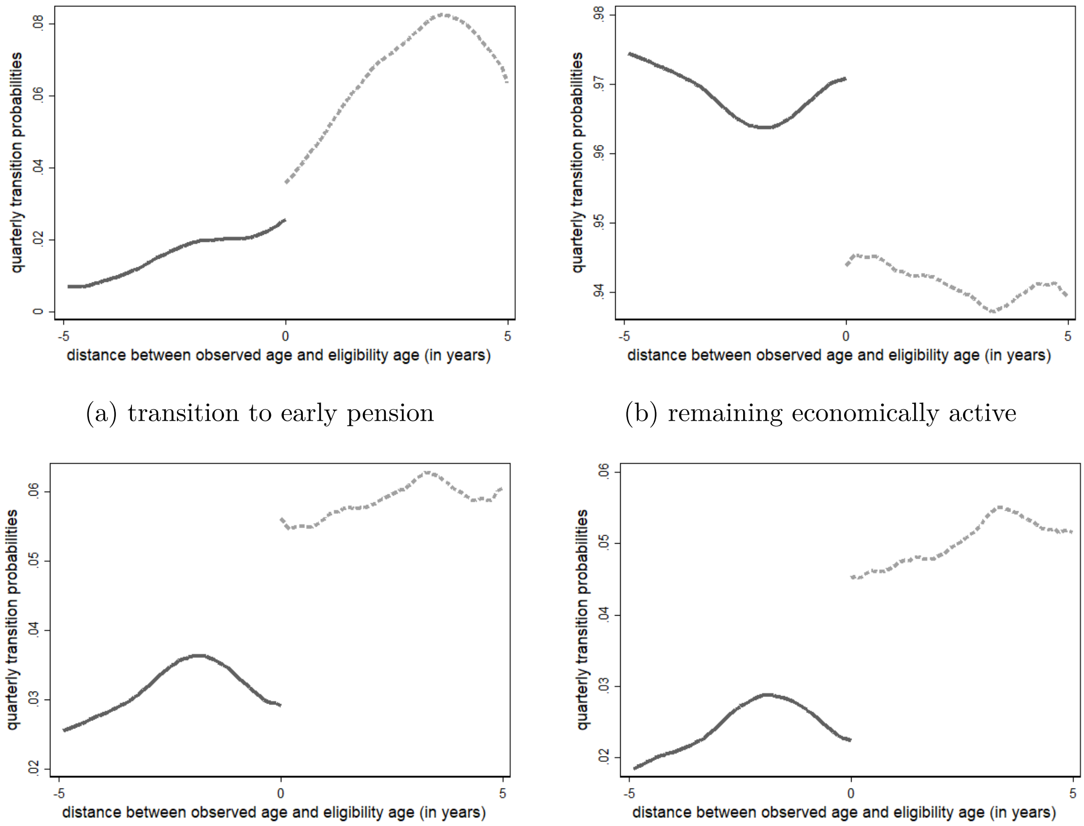
6.2 Placebo tests - discontinuities in other quarters

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. There appear seasonal regularities in labor market exits, regardless of the eligibility age. 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 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. 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. The results are presented in Figure 3. 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.

We test that notion formally, by running a series of probit and tobit regressions (we report marginal effects in Table 5). 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 OLS specifications we only use the significant estimates. 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.[5]

[5]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

Figure 2: Effects of the eligibility reform: RD estimates, cut-off at 2009q1



(a) transition to early pension

(b) remaining economically active

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

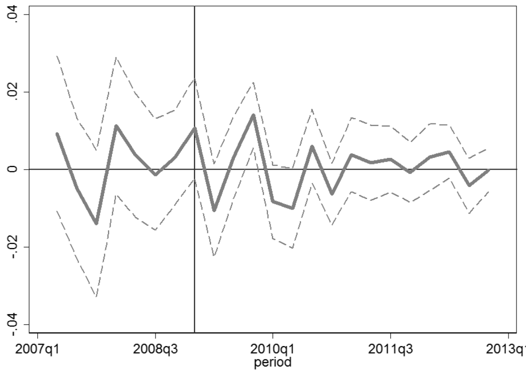
(d) transition to inactivity (elderly with sufficient experience)

Note: Dark grey denotes cohorts which lost early eligibility, light gray denotes cohort which maintained eligibility, point 0 denotes original early retirement eligibility.

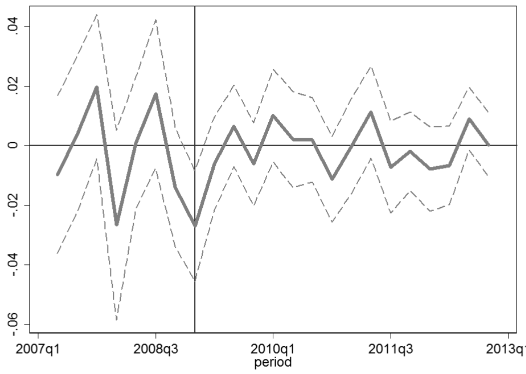
Including treatment in the specification of the discontinuity estimation does not affect the chances of the estimator to be statistically significant (despite a significant share of the population being affected by the treatment). The dummy for the specification with controlling for occupational eligibility 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.

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 if the reform had a sizable effect, they should 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 conditions, such as transitions to inactivity.

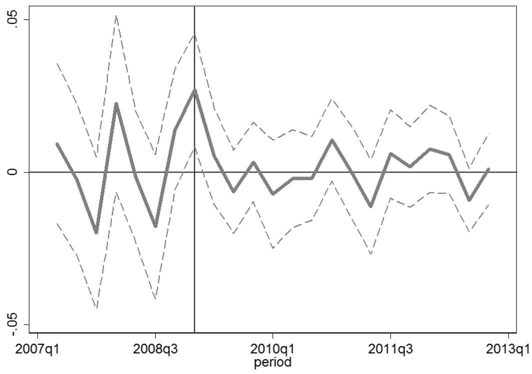
Figure 3: Placebo tests: RD sharp estimates, for each quarter between 2007q2 and 2012q4



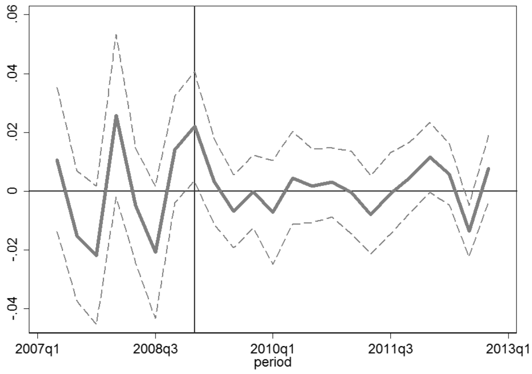
(a) transition to early pension



(b) remaining economically active



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



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

Notes: While assignment variables are recalculated for every timing, the occupational eligibility is not relevant for the pre-reform periods. Hence, we present sharp estimates.

6.3 Heterogeneity of the treatment effects

The reform could have affected women stronger than men, the education and household situation could also play a role. Since we estimated RD in each sub-population, we can analyze if the treatment effects of the reform are larger in specifications e.g. for women only. The results are reported in Table 6. The estimates for the reform quarter are somewhat lower than in Table 5, which may be due to the fact that all of the specifications in Table 6 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 homogeneous effect, i.e. they could have been stronger in some group of individuals than in others. However, the only dimension of heterogeneity which appears to play an important role is education: specifications for the low skilled individuals were less likely to exhibit significant RD estimates. 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). Specifications where one controls for a small child

Table 5: The "placebo test"

	all outcome variables		remaining active		early retirement		transition to inactivity (age)		(experience)	
	Probit	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**	**Probit**	**OLS**
2009q1	0.462***	-0.001	0.541***	-0.003	0.484***	-0.004	0.527***	0.003	0.268**	-0.001
	(0.06)	(0.01)	(0.10)	(0.01)	(0.10)	(0.01)	(0.10)	(0.01)	(0.11)	(0.01)
Treatment	0.001	-0.001	0.000	-0.014*	0.005	-0.003	-0.000	0.012	-0.000	0.001
	(0.02)	(0.00)	(0.04)	(0.01)	(0.04)	(0.01)	(0.08)	(0.01)	(0.04)	(0.00)
Constant		0.002		0.007		0.002		-0.006		-0.004
		(0.01)		(0.01)		(0.00)		(0.01)		(0.00)
N	2,576	395	644	84	644	109	644	90	644	112

Notes: Marginal effects reported, ***, ** and * indicate significance at 0.1%, 1% and 5% levels. 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. OLS estimated for significant specifications only. All specifications control for model, controls and year dummies.

in a household seem to produce statistically significant results with lower probability, *ceteris paribus*.

Table 6: Heterogeneity of the treatment effects

	(1)	(2)	(3)	(4)
2009q1	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)
Less than vocational education	-0.0637**	-0.0628**	-0.0619**	-0.0657**
	(0.027)	(0.027)	(0.027)	(0.027)
Vocational education	-0.0784***	-0.0775***	-0.0767***	-0.0802***
	(0.026)	(0.026)	(0.026)	(0.026)
Secondary education	0.0420	0.0433	0.0445	0.0392
	(0.032)	(0.033)	(0.033)	(0.032)
Tertiary education	-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 (estimations)	2,576	2,576	2,576	2,576

Notes: Marginal effects from probit estimations with a given specification yielding significant discontinuity denoted by 1 and 0 otherwise. 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 the size of the discontinuity, nor to the probability it even exists. Given the small size of the estimates, we attempted to test if 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.

7 Discussion of the results and policy implications

The analyzed eligibility reform in Poland effectively abolished early retirement for a vast majority of workers. The change was not gradual and could not be anticipated. Hence, one should expect large discontinuities. One should also expect the effects to be stronger for the professions which lost eligibility. Finally, one could expect the discontinuities to be heterogeneous across various groups of workers. Given these expectations, the results provide a relatively small – though statistically significant – estimator of discontinuity.

We find that there are some significant discontinuities in observed intensity of the early exits from the labor market at the reform date. Depending on a the outcome measure, the estimates fall into the range of 1 to 3 percentage points, typically less than half of the potential effect. We find virtually no effects for the early pension benefit take-up rates, which suggests that even prior to the reform, the eligibility alone was not the only criterion for the decision to claim early pension benefits. Moreover, the differences in employment measures do not seem to be driven by the occupational eligibility: revealed willingness to retire did not seem to be different among the workers who lost the occupational eligibility than among those who could still access early retirement schemes. Naturally, the cumulative effect on the future cohorts may be larger than our instantaneous estimates.

When compared to the estimates for other countries – e.g. much less extensive reforms in the USA, France, Austria or Switzerland – the discontinuities in Poland were substantially smaller. In fact, it appears that the increase in labor force participation observed as of 2007 is related to other phenomena rather than to this specific reform. What our results effectively imply is that a large fraction of individuals who remained active in the labor market following the introduction of the reform, would have done so also in the absence of the legislative changes.

There are two main policy implications of our study. First, constraining the pension eligibility criteria for retirement are frequently opposed by social actors. 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. If treatment effects of the large and radical eligibility reform are small in participation rates and pension take-up rates, then immediate fiscal effects are bound to be small as well, even if in the desirable direction. This may explain why – given the strong social resistance – in many countries eligibility reforms are delayed or narrowed in scope.

Second, the economic rationale for strong social resistance to eligibility reforms builds on assuming either a relatively high valuation of leisure time after exiting the labor market or a relatively high subjective valuation of the unemployment risk after passing the early retirement age threshold. If leisure preference is overstated, reducing eligibility may be opposed as such, but eligibility alone is irrelevant for household decision making. Meanwhile, unemployment risk may be mitigated via alternative instruments, such as employment protection legislation, as is the case in Poland. Depending on a specific composition of the two factors in a given country, the effects of the eligibility reforms may be as high as in Switzerland or as low as in Poland.

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