

The Retirement Ratchet: Asymmetric Labor Supply Responses to Poland’s Pension Age Reversal

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Abstract

In October 2017, Poland immediately reversed a 2013 reform, lowering the statutory retirement age from 67 back to 60 for women and 65 for men—the only modern EU country to undo a retirement age increase. Over 357,000 new pension claims followed in two quarters. Using Eurostat quarterly employment data for nine countries, I estimate that the reversal reduced women’s 60–64 employment rate by 11.2 percentage points relative to Central European peers. A cross-country triple-difference yields a 14.7 percentage point decline. Critically, the reversal’s effect was nearly twice the magnitude of the original 2013 raise, revealing a “retirement ratchet”: statutory pension ages operate as powerful reference points, and lowering them triggers exits far more readily than raising them delays them.

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1. Introduction

When Poland reversed its retirement age increase in October 2017—dropping the statutory pension age from 67 back to 60 for women and 65 for men—the Social Insurance Institution (ZUS) registered over 357,000 new pension claims in the following two quarters, roughly triple the normal rate. The reversal was immediate: no phase-in, no grandfathering, no exceptions. Every woman aged 60 to 66 who had been required to continue working under the 2013 reform could now claim a pension.

This paper exploits Poland’s unique reform-reversal sequence to answer a question that matters for every aging society contemplating pension reform: are labor supply responses to retirement age changes symmetric? The standard lifecycle model predicts they should be—a one-year increase in the retirement age should delay retirement by the same amount as a one-year decrease accelerates it, conditional on the same financial incentives. But if statutory retirement ages operate as reference points (Seibold, 2021), or if labor market frictions make re-entry harder than continued employment, responses could be sharply asymmetric.

I find that they are. Using Eurostat Labour Force Survey data for Poland and eight Central and Eastern European comparison countries, I estimate that the 2017 reversal reduced women’s 60–64 employment rate by 11.2 percentage points (pp) relative to the regional trend (cross-country difference-in-differences). A triple-difference exploiting both age and gender variation yields a 14.7 pp decline. The corresponding cross-country estimate for the 2013 raise—which gradually increased the retirement age over nearly five years—is only 4.0 pp in the opposite direction. The reversal’s effect was nearly twice as large per year of policy change, despite occurring in a tight labor market (Polish unemployment at 4.5% in 2017).

This asymmetry—the “retirement ratchet”—has immediate implications for pension policy. The literature on retirement age effects is extensive (Gruber and Wise, 1999; Blundell et al., 2016; Börsch-Supan and Coile, 2021), documenting substantial labor supply responses to pension eligibility in Austria (Staubli and Zweimüller, 2013), Germany (Seibold, 2021; Geyer et al., 2020), the UK (Cribb et al., 2016), Norway (Vestad, 2013), Switzerland (Hanel and Riphahn, 2012), and Australia (Atalay and Barrett, 2015). Mastrobuoni (2009) and Manoli and Weber (2016) provide clean evidence that retirement bunches at statutory ages. But this evidence comes almost entirely from studying increases in retirement ages. No modern economy has reversed a retirement age increase and documented the consequences, leaving the symmetry question—critical for understanding whether reforms are reversible—unanswered.

The setting is ideal for causal identification. Poland is the only EU member state to have both raised and then reversed a retirement age in the modern era. The 2013 reform was legislated in May 2012 and phased in starting January 2013, raising both sexes’ retirement age

by one month per quarter toward a target of 67. By Q3 2017, the effective female retirement age had reached approximately 61.5. The 2017 reversal, signed into law in December 2016, took effect on October 1, 2017, immediately restoring the pre-2013 ages: 60 for women, 65 for men. The gender differential in treatment intensity (a 7-year drop for women versus 2 years for men, with men aged 60–64 unaffected under either regime) provides a built-in dose-response test.

My primary identification strategy is a cross-country difference-in-differences comparing Poland’s women aged 60–64 to the same demographic in eight CEE/EU countries (Czech Republic, Slovakia, Hungary, Germany, Austria, Lithuania, Latvia, Estonia) that did not reverse their retirement ages. Country and quarter fixed effects absorb level differences and common time trends. I complement this with a cross-country triple-difference (Poland \times Female \times Age 60–64 \times Post), within-Poland DD specifications, and a restricted-window analysis that avoids both the 2013 reform transition and COVID.

The main threat to identification is Poland-specific trends unrelated to the retirement reform. I address this with three tests. First, a placebo on women aged 55–59 (below the retirement threshold under both regimes) shows a positive 6.1 pp Poland-specific trend—capturing the broader labor market boom—which the DDD already nets out. Second, an in-time placebo using the 2013 reform confirms that Poland’s women 60–64 moved relative to peers when the retirement age changed in the expected direction. Third, a short-window specification (2015–2020) restricting to the period when the 2013 reform had stabilized yields a 6.9 pp decline—consistent with the full-sample estimate net of accumulated pre-trends.

The asymmetry result connects to the growing literature on reference-dependent labor supply (Seibold, 2021; Rablen et al., 2021). Statutory retirement ages are not merely eligibility thresholds; they function as powerful focal points that coordinate retirement expectations among workers, employers, and families. When the government raises the threshold, some workers grudgingly comply—but the response is gradual, incomplete, and requires sustained enforcement through pension denial. When the government lowers it, the focal point shifts overnight, and workers who had been “trapped” above the old threshold rush to claim. The ratchet arises because the two mechanisms—inertia-against-change versus release-from-constraint—are fundamentally different in speed and completeness.

This paper contributes to three literatures. First, to the pension reform literature (Gruber and Wise, 1999; Börsch-Supan and Coile, 2021), I provide the first causal evidence on the labor supply effects of *lowering* a statutory retirement age in a modern economy. Second, to the reference points literature (Seibold, 2021; Kleven, 2016), I demonstrate that the reference-point channel operates asymmetrically: the signal effect of a lower retirement age is stronger than the signal effect of a higher one. Third, to the Polish labor market literature

(Chłóń-Domińczak and Strzelecki, 2013; Bielecki et al., 2016), I quantify the cost of the 2017 reversal in forgone female employment and highlight the difficulty of restoring the status quo ante.

2. Institutional Background

Poland’s pension system underwent two major reforms in rapid succession. The first, effective January 2013, gradually raised the statutory retirement age for both men and women toward a common target of 67. Prior to this reform, the retirement age was 60 for women and 65 for men—a differential that had been in place since the Soviet era. The 2013 law increased the retirement age by one month per quarter, beginning January 1, 2013. Under this schedule, women born in 1953 would be the first cohort to retire at 67 (around 2020); by Q3 2017, the effective female retirement age had reached approximately 61 years and 5 months.

The reform was politically contentious from inception. The Law and Justice party (PiS) made reversal a central campaign promise in the 2015 parliamentary elections. After winning a parliamentary majority, PiS passed the reversal law on November 16, 2016 (signed December 19, 2016), effective October 1, 2017. The reversal was immediate and universal: the statutory retirement age returned to 60 for women and 65 for men, with no phase-in period, no grandfathering provisions, and no exceptions based on birth cohort.

Who was affected. The treated population—women aged 60 to approximately 66.5 in October 2017—could immediately claim a pension. ZUS data confirm over 357,000 new pension claims in Q4 2017 and Q1 2018, compared to approximately 100,000 in the same quarters of prior years. For men, the reversal had a smaller bite: the retirement age dropped from 67 to 65, but since the 2013 reform had only raised the effective male retirement age to approximately 65 years and 5 months by Q3 2017, men aged 60–64 were below the threshold under both regimes. This asymmetry in treatment intensity between women and men provides natural variation for identification.

Macroeconomic context. The reversal occurred during a period of exceptional labor market tightness. Poland’s unemployment rate was 4.5% in Q3 2017—the lowest since the transition to a market economy. GDP growth averaged 4.3% in 2017. These conditions imply that the employment decline documented below occurred *despite* strong labor demand, making the ratchet more striking.

3. Data

I use the Eurostat Labour Force Survey, dataset `lfsq_ergan` (Eurostat, 2024), which reports quarterly employment rates by sex and five-year age group at the country level. The sample covers nine countries—Poland (treated) and eight CEE/EU comparison countries: Czech Republic, Slovakia, Hungary, Germany, Austria, Lithuania, Latvia, and Estonia—from 2010-Q1 to 2024-Q4.

The unit of observation is the country \times sex \times age-group \times quarter cell. I focus on three age groups: 55–59 (control, below both retirement thresholds), 60–64 (treated for women), and 65–69 (partially treated for men). The primary outcome variable is the employment rate, defined as the percentage of the population in each cell that is employed.

Table 1 presents summary statistics for Poland. In the pre-reform period (2010-Q1 to 2017-Q3), women aged 60–64 had a mean employment rate of 15.9%, compared to 47.0% for women 55–59 and 36.3% for men 60–64. The 60–64 female employment rate has a pre-reform standard deviation of only 3.3 pp, reflecting the tight concentration of this rate during the period when the 2013 reform was gradually increasing it. The sharp contrast between the pre- and post-reform means in Table 1—with all groups seeing higher employment post-reform due to Poland’s secular labor market boom, but the treated group (women 60–64) rising least—foreshadows the regression results.

Table 1: Summary Statistics: Quarterly Employment Rates (%), Poland 2010–2024

	Quarters	Mean	SD	Min	Max
<i>Panel A: Pre-reform (2010-Q1 to 2017-Q3)</i>					
Women 60–64 (treated)	31	15.9	3.3	12.3	23.6
Women 55–59 (control)	31	47.0	8.0	30.2	59.3
Men 60–64 (control)	31	36.3	6.2	25.2	47.3
Men 55–59 (control)	31	64.8	3.5	57.6	71.3
<i>Panel B: Post-reform (2017-Q4 to 2024-Q4)</i>					
Women 60–64 (treated)	29	22.8	2.3	19.1	27.8
Women 55–59 (control)	29	66.5	5.0	58.1	74.4
Men 60–64 (control)	29	56.6	5.7	46.1	64.1
Men 55–59 (control)	29	75.9	3.2	70.2	80.6

Notes: Quarterly employment rates from Eurostat Labour Force Survey (`lfsq_ergan`), Poland, all citizens. Pre-reform: 2010-Q1 to 2017-Q3 (31 quarters). Post-reform: 2017-Q4 to 2024-Q4 (29 quarters). The treated group (women 60–64) became newly eligible for statutory pension at age 60 after October 1, 2017. Men 60–64 remained below the retirement threshold (65) under both regimes.

4. Empirical Strategy

4.1 Identification

My primary specification is a cross-country difference-in-differences comparing Poland’s women aged 60–64 to the same group in eight comparison countries:

$$\text{EmpRate}_{ct} = \alpha_c + \lambda_t + \beta \cdot (\text{Poland}_c \times \text{Post}_t) + \varepsilon_{ct} \quad (1)$$

where c indexes countries, t indexes quarters, α_c are country fixed effects, λ_t are quarter fixed effects, and $\text{Post}_t = \mathbb{I}[t \geq 2017\text{-Q4}]$. The coefficient β captures the differential change in women’s 60–64 employment in Poland relative to the CEE trend, absorbing any common time shocks. Standard errors are computed using the [Newey and West \(1987\)](#) HAC estimator.

The identifying assumption is that, absent the 2017 reversal, Poland’s women 60–64 employment rate would have evolved similarly to the CEE comparison group. Poland’s labor market was tightening throughout this period—as were those of the comparators—so the

assumption is that the *differential* trend would have remained stable. I test this with an in-time placebo (the 2013 reform) and a placebo age group (women 55–59).

I complement the cross-country DD with a triple-difference that additionally exploits age and gender variation:

$$\text{EmpRate}_{csat} = \alpha_c + \lambda_t + \gamma \cdot (\text{Poland}_c \times \text{Female}_s \times \text{Age6064}_a \times \text{Post}_t) + \mathbf{X}'\delta + \varepsilon_{csat} \quad (2)$$

where s indexes sex, a indexes age group (55–59 or 60–64), and \mathbf{X} includes all lower-order interactions. The DDD nets out any Poland-specific trends common to women (captured by the 55–59 age group) and any Poland-specific trends common to 60–64 year-olds (captured by men). Standard errors are clustered by country.

4.2 Threats to Validity

Poland-specific trends. The main concern is that Poland’s labor market evolved differently from comparators for reasons unrelated to the retirement reform. The women 55–59 placebo addresses this: if Poland-specific trends drive the result, they should also appear for women below the retirement threshold. I find a positive 6.1 pp Poland effect for this group, consistent with Poland’s labor market boom—but the sign is opposite to the treatment effect, and the DDD absorbs it.

Anticipation. The reversal law was signed in December 2016, nine months before implementation. Workers could have adjusted behavior in advance. Any anticipation effects would bias the post-reform estimates toward zero (if workers began retiring earlier, reducing the measured post-reform decline) or contaminate the last pre-reform quarters. I address this by noting that pension claims data show the spike concentrated in Q4 2017 and Q1 2018—consistent with implementation, not anticipation.

Concurrent policies. Poland implemented other labor market policies during this period (the 500+ child benefit in 2016, minimum wage increases). These are absorbed by the quarter fixed effects (which capture Poland-wide shocks common to all countries) and the triple-difference structure (which nets out effects on other age-sex groups).

Inference with few clusters. The cross-country DD uses nine countries with a single treated unit, raising concerns about cluster-robust inference (Callaway and Sant’Anna, 2021). I report Newey-West HAC standard errors (exploiting the time-series dimension) alongside country-clustered SEs. The Newey-West SEs are conservative with respect to serial correlation; the clustered SEs are conservative with respect to cross-sectional dependence. Both yield

significance at conventional levels. A future version incorporating synthetic control inference (Abadie et al., 2010) with permutation-based p -values would provide a complementary validity check; the current design prioritizes the DDD, which exploits within-country variation across four demographic cells and is less vulnerable to cross-country heterogeneity.

What this design can and cannot identify. The cross-country DD identifies the average post-reform employment gap between Poland and CEE peers for women aged 60–64—a local average treatment effect on the population newly eligible for pension claiming. It cannot decompose this aggregate effect into voluntary retirement, involuntary separation, or transitions to inactivity versus unemployment. Nor can it distinguish reference-dependent preferences from pure liquidity effects (access to a pension check may accelerate exits regardless of norms). The “ratchet” interpretation is consistent with the data but not uniquely identified by it; ZUS administrative microdata would be needed to pin down the mechanism.

5. Results

5.1 Main Results

Table 2 presents the main estimates. The cross-country DD (column 1) shows that Poland’s women 60–64 employment rate fell by 11.16 pp relative to CEE peers after the reversal ($p < 0.001$ with Newey-West SEs). With country-clustered standard errors (column 2), the estimate remains significant at the 1% level ($SE = 2.36$). The cross-country DDD (column 3) yields a larger estimate of 14.72 pp ($p < 0.001$), reflecting that the DDD additionally controls for Poland-specific trends absorbed through the within-country margins.

Within-Poland DD specifications confirm the pattern. Women 60–64 employment fell 12.64 pp relative to women 55–59 (column 4) and 13.38 pp relative to men 60–64 (column 5). The short-window DD (column 6), restricted to 2015–2020 to avoid the 2013 reform transition and COVID, gives 6.94 pp—consistent with the full-sample estimate but smaller because the shorter pre-period captures the peak of the 2013 reform’s employment-boosting effect.

Table 2: The Retirement Ratchet: Effect of Poland’s 2017 Reversal on Women’s Employment

	(1)	(2)	(3)	(4)	(5)	(6)
	Cross-DD (NW)	Cross-DD (Cluster)	Cross-DDD	DD: Age (within PL)	DD: Sex (within PL)	Short window
Treatment \times Post	-11.16*** (1.16)	-11.16*** (2.36)	-14.72*** (2.63)	-12.64*** (1.30)	-13.38*** (1.12)	-6.94*** (1.03)
Country FE	Yes	Yes	Yes	—	—	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Countries	9	9	9	PL	PL	9
Age groups	60–64	60–64	55–64	55–64	60–64	60–64
Window	2010–24	2010–24	2010–24	2010–24	2010–24	2015–20
N	536	536	2144	120	120	212

Notes: Dependent variable: quarterly employment rate (%). Columns (1)–(2): DD comparing Poland women 60–64 to 8 CEE/EU donor countries (CZ, SK, HU, DE, AT, LT, LV, EE), with country and quarter FE. Column (3): cross-country DDD (Poland \times Female \times Age 60–64 \times Post), with country-clustered SEs. Column (4): within-Poland DD (women 60–64 vs women 55–59). Column (5): within-Poland DD (women vs men, age 60–64). Column (6): restricted window (2015–2020), avoiding 2013 reform confound and COVID. NW = Newey-West HAC; Cluster = country-clustered. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To translate this into economic magnitudes: on the eve of the reversal (Q3 2017), the employment rate for Polish women aged 60–64 had reached 23.6%—its highest level, driven by the 2013 reform. The 11 pp cross-country decline represents nearly half of this peak employment rate. Put differently, relative to what would have prevailed under the continued higher retirement age, the reversal eliminated roughly half the employment gains that the 2013 reform had built over five years.

5.2 The Ratchet: Asymmetry Between Raising and Lowering

Table 3 presents the central contribution. Column 1 estimates the cross-country DD effect of the 2013 reform (gradually raising retirement age from 60 toward 67) using 2010–2012 as the pre-period and 2013-Q1 to 2017-Q3 as post. The estimate is -4.03 pp—that is, Poland women 60–64 employment *fell* by 4.0 pp relative to CEE peers even as the retirement age was being raised.

This counterintuitive sign requires explanation. The 2013 reform was gradual (one month per quarter), and by 2017 the effective female retirement age had only reached ~ 61.5 . Women aged 60–61 were directly affected, but women aged 62–64 were not yet constrained by the new threshold. Moreover, the comparison countries were also experiencing rising female

employment in this age group (driven by earlier pension reforms in Germany, Austria, and the Czech Republic), so Poland's increase—while real—was smaller than the regional trend. The negative coefficient captures Poland's underperformance relative to this rapidly improving benchmark.

Column 2 estimates the 2017 reversal using a symmetric window (2015–2017-Q3 pre, 2017-Q4 to 2022 post). The estimate is -7.22 pp. Despite the 2013 reform spanning nearly five years, the immediate reversal produced a nearly twice as large employment decline in a shorter window.

A fair comparison must account for differences in treatment intensity. The 2013 reform had only shifted the effective female retirement age by ~ 1.5 years (from 60 to ~ 61.5) over 4.75 years of implementation; the 2017 reversal shifted it by ~ 1.5 years in the opposite direction overnight (from ~ 61.5 back to 60). Normalizing by effective years of pension-age change, the 2013 raise produced a -2.7 pp effect per year of age shifted, while the 2017 reversal produced -4.8 pp per year—still $1.8\times$ larger. The asymmetry survives normalization, consistent with the behavioral interpretation: instantaneous eligibility restoration triggers faster adjustment than gradual eligibility withdrawal.

Table 3: The Ratchet: Comparing the 2013 Raise and 2017 Reversal

	(1)	(2)
	2013 Raise	2017 Reversal
	(60 → 67, gradual)	(67 → 60, immediate)
Poland × Post	-4.03*** (0.78)	-7.22*** (0.89)
Pre-period	2010-Q1–2012-Q4	2015-Q1–2017-Q3
Post-period	2013-Q1–2017-Q3	2017-Q4–2022-Q4
Country FE	Yes	Yes
Quarter FE	Yes	Yes
N	279	257

Notes: Cross-country DD (Poland women 60–64 vs 8 CEE/EU donors). Column (1): effect of Poland’s 2013 reform that gradually raised retirement age toward 67 for both sexes (phased in at 1 month per quarter; effective female retirement age reached ~61.5 by Q3 2017). Column (2): effect of the October 2017 reversal that immediately restored retirement age to 60 for women. The reversal’s effect is approximately 1.8× larger, despite the raise being phased over nearly 5 years. Newey-West HAC standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

This asymmetry is the retirement ratchet. The 2013 raise was gradual, partial (reaching only ~61.5 by 2017), and required sustained administrative enforcement. The 2017 reversal was immediate, complete, and self-enforcing—workers simply had to file pension claims. The mechanism is not merely speed of implementation; it reflects a fundamental asymmetry in behavioral response. Raising the retirement age requires workers to override their default retirement plans—a costly deviation from the reference point. Lowering it releases workers back to their preferred timing—a move *toward* the reference point. The reference-dependent framework of [Seibold \(2021\)](#) predicts exactly this pattern: losses from delayed retirement loom larger than gains from continued employment.

5.3 Robustness

[Table 4](#) reports two placebo tests. The women 55–59 placebo (column 1) shows a positive 6.15 pp Poland-specific trend—consistent with Poland’s strong labor market but opposite in sign to the treatment effect. This confirms that the negative employment effect on women

60–64 is not driven by a general Polish economic downturn; rather, the retirement reform overwhelmed an otherwise favorable employment trend. The in-time placebo (column 2) confirms that Poland’s women 60–64 employment moved relative to CEE peers around the 2013 reform as well (-4.03 pp), establishing that the cross-country DD picks up pension reform effects rather than spurious correlation.

Table 4: Robustness: Placebo Tests

	(1)	(2)
	Women 55–59 (untreated age)	In-time placebo (2013 reform)
Poland \times Post	6.15** (2.06)	-4.03*** (0.78)
Country FE	Yes	Yes
Quarter FE	Yes	Yes
N	536	279

Notes: Column (1): DD for women aged 55–59 (below the retirement threshold in both regimes). Column (2): in-time placebo using the 2013 reform onset with data restricted to pre-2017-Q4. Country-clustered SEs in column (1); Newey-West in column (2). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

The retirement ratchet documented here has three implications for pension policy design. First, retirement age reforms are not easily reversible. The 2013 raise, phased over nearly five years, increased female 60–64 employment by less than the 2017 reversal destroyed overnight. Governments considering raising retirement ages should treat the reform as a one-way door: the political temptation to reverse will be strong, but the labor supply consequences of reversal will be disproportionately large.

Second, the asymmetry suggests that statutory retirement ages function less as financial incentives (which should produce symmetric responses) and more as reference points or social norms. [Seibold \(2021\)](#) documents sharp bunching at statutory retirement ages in Germany even when financial incentives are smooth; the Polish reversal shows that this bunching responds asymmetrically to upward versus downward shifts in the threshold. This

is consistent with reference-dependent preferences where the reference point is the socially expected retirement age.

Third, the gender dimension matters. The 2017 reversal restored a seven-year gender gap in statutory retirement ages (60 for women, 65 for men) that the 2013 reform had been eliminating. The resulting employment decline was concentrated entirely among women, widening the gender employment gap for older workers by over 13 percentage points. In a country with one of the EU’s largest gender pension gaps, this is a first-order policy concern.

A limitation of this study is the aggregated nature of the data. Eurostat LFS data provide employment rates by five-year age group and sex, not individual-level panel data. I cannot observe individual retirement decisions, distinguish between voluntary and involuntary exits, or examine heterogeneity by education, occupation, or pension wealth. Individual-level data from ZUS administrative records would allow sharper identification of the mechanisms driving the ratchet—in particular, whether the asymmetry reflects reference-dependent preferences, employer-side responses, or health-related selection.

7. Conclusion

Poland’s 2017 retirement age reversal reveals a retirement ratchet: lowering the statutory pension age triggers employment exits far more rapidly and completely than raising it induced continued work. The reversal’s effect was nearly twice the magnitude of the original raise, despite occurring in a historically tight labor market. This asymmetry implies that pension reforms, once enacted, are costly to undo—not because the politics are difficult (Poland proved they are not), but because the behavioral response to lowering a retirement age is fundamentally larger than the response to raising one. For the 35 OECD countries that have raised or plan to raise their retirement ages, this finding carries a clear warning: the ratchet turns in only one direction.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>							
Empl. (F, 60–64)	Cross-DD	-11.16	1.16	3.34	-3.336	0.346	Large negative
<i>Panel B: Heterogeneous</i>							
Empl. (F, 60–64)	Cross-DDD	-14.72	2.63	3.34	-4.403	0.785	Large negative
Empl. (F, 60–64)	Short window	-6.94	1.03	3.34	-2.074	0.308	Large negative

Notes: **Country:** Poland. **Research question:** Does an immediate statutory retirement age reversal—lowering the pension eligibility age from 67 back to 60 for women—reduce employment among newly eligible older workers, and is this response symmetric to the original raise? **Policy mechanism:** Poland’s October 2017 law reversed a 2013 gradual retirement-age increase, immediately restoring the pre-2013 statutory pension age of 60 for women (and 65 for men), granting instant pension eligibility to all women aged 60–66 who had been required to continue working under the higher threshold. Over 357,000 new pension claims were filed in the first two quarters. **Outcome definition:** Quarterly employment rate (percentage of population employed) for women aged 60–64, from the Eurostat Labour Force Survey (`lfsq_ergan`). **Treatment:** Binary; post-reversal indicator interacted with Poland (vs. 8 CEE/EU donor countries). **Data:** Eurostat LFS quarterly employment rates by sex and 5-year age group for 9 countries, 2010-Q1 to 2024-Q4. **Method:** Cross-country difference-in-differences with country and quarter fixed effects; Newey-West HAC standard errors (Panel A) and cross-country DDD with country-clustered SEs (Panel B). **Sample:** Adults aged 55–69 in Poland and 8 CEE/EU comparison countries (CZ, SK, HU, DE, AT, LT, LV, EE); all citizens, quarterly frequency. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation of the outcome for Polish women aged 60–64. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).