Correcting vs. Preventing the Gender Pension Gap*

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March 23, 2026

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Abstract

Gender gaps in lifetime earnings and employment translate into substantial disparities in pension benefits. This paper studies two policy approaches to reducing the gender pension gap: preventing future gaps through subsidized childcare and correcting existing gaps through pension credits for unpaid care work. Using Norwegian administrative data, I compare the long-run effects of expanding subsidized childcare and a retroactive extension of childcare-related pension credits. The childcare expansion had limited short-run employment effects but generated larger cumulative gains upon retirement. Despite the increase in lifetime earnings, I find no evidence of an earlier claiming age. Finally, I find positive fiscal externalities of the policy through higher tax revenue. On the other hand, childcare-related pension credits increased annual pension benefits by 2.1 percent on average. I find small effects on retirement and increased disability insurance take-up among cohorts not yet eligible to retire at the time of treatment. The comparison highlights the trade-offs between correcting and preventing gender inequality and informs the welfare evaluation of policies targeting gender gaps over the life cycle.

*I declare no conflicts of interest. I thank Aline Bütikofer, Andreas Haller, Hilary Hoynes, Charlie Rafkin, seminar participants at NHH and UC Berkeley, and conference participants at the 2025 World Labor Conference, the 2025 Congress of the International Institute of Public Finance, the RFBerlin Workshop on the Economics of Aging, the 2025 Nebraska Labor Summit and the 2024 Nordic Public Policy Symposium for useful comments and suggestions. All errors are my own.

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1 Introduction

Gender gaps over the working life are well documented in the literature (for a review of the study of gender in the labor market, see Olivetti, Pan, & Petrongolo, 2024). Because pension benefits depend on lifetime earnings and employment, these disparities generate a substantial gender pension gap. On average, women retire with fewer contribution years and lower earnings during the years in which they contributed, partly due to employment and earnings penalties associated with childcare. The gender pension gap, therefore, reflects the *cumulative penalty* in earnings and employment over the working life. In 2021, the average gender wage gap in OECD countries was 11.4%, compared with a 25.6% gender pension gap (OECD, 2021). Although rising female labor force participation and declining earnings gaps suggest future convergence in pension benefits, labor market gender disparities remain. As a result, the scope for the pension gap to "close" naturally in the next decades is limited, leaving room for policy intervention.

If the policy goal is to reduce this cumulative penalty and thus the gender pension gap, there are two main approaches available to policymakers: to *prevent* higher future pension gaps by encouraging female labor force participation during working life, and to *correct* existing gaps by ensuring a certain level of pension entitlements during women's career breaks. Both have potential advantages and drawbacks, and the cost-effectiveness of each is ultimately an empirical question.

Preventive policies - for example, childcare subsidies - take decades to materialize into higher pension benefits and will not have any direct effect on women who have already completed fertility by the time of implementation. On the other hand, there are positive fiscal externalities from increasing lifetime earnings, such as higher tax revenue from the policy beneficiaries. Importantly, this approach relies on public policy being able to influence female labor force participation, and little is known about the long-run effects of childcare policies on future pension benefits or cumulative earnings (for a review of the existing literature, see Albanesi, Olivetti, & Petrongolo, 2023).

According to a survey of the legal framework in 190 countries, 109 countries account for periods of absence due to childcare in the final calculation of pension benefits (World Bank, 2024); that is, they implement corrective policies. This corresponds to 81% of high-income countries and 47% of low- and middle-income countries. When applied retroactively, these policies can target cohorts close to retirement, at the cost of potentially reducing labor market attachment during old age. Evaluating these reforms provides insight into the welfare implications of *ex post* correcting the consequences of gender inequality in the labor market through the pension system. The results also inform the study of compensatory or reparatory policies beyond the existing social safety net. Moreover, variation in the timing and design of these policies allows us to analize how beneficiaries—and their partners or children—respond to positive shocks to lifetime income.

Assessing the potential of corrective and preventive policies to reduce the gender pension gap poses several challenges. Studying the effect of increased pension wealth on retirement requires an exogenous shock that targets women, and evaluating preventive policies requires them to have occurred

far enough in the past for retirement outcomes to be observed, yet recently enough for reliable data to be available. Finally, the same cohorts must be affected by each policy at different points in time to ensure that comparisons are not biased by differences in sample composition, institutional setting or social norms.

This paper compares the effects of preventive and corrective policies on women upon retirement: the 1975 expansion of subsidized childcare and the 2010 retroactive extension of childcare-related pension credits in Norway. The beneficiaries of each policy partly overlap, making it possible to study the interaction of the two.

The retroactive extension of pension accrual for unpaid care work in Norway, introduced in 2010 for mothers who gave birth between 1967 and 1991, allows me to identify the employment, earnings, and consumption response to a pure change in pension wealth, as well as the costs and benefits of a corrective policy. Pension accrual for unpaid care work ensures that mothers have their pension rights calculated relative to a fixed amount whenever it would have been lower if calculated relative to their actual income. For the older treated cohorts, this reform is akin to a windfall gain, its magnitude determined by each individual's past employment and earnings history. I estimate the retroactive extension of pension accrual for unpaid care work to account for an average 2.1% (USD 881,[1] a present value of USD 17,004) increase in pension benefits for the eligible mothers for whom the policy had a non-zero impact.

I find that pension accrual for unpaid care work led to a 0.48 pp reduction in the gender gap in lifetime pension benefits, with modest employment effects. Beneficiaries anticipate pension claiming and reduce their labor supply, which fully explains the subsequent decrease in take-up of unemployment benefits and sick leave. I find an increase in take-up of disability insurance for the cohorts that were not yet eligible to retire at the time of treatment. Finally, following Eika, Mogstad, and Vestad (2020), I approximate consumption expenditure to estimate the marginal propensity to consume. My analysis documents the response to the retroactive extension of pension credits, and allows me to quantify the cost of the policy, including the mechanical effect, at NOK 125,595 (USD 15,624) per beneficiary between 2010 and 2021.

The introduction of the Kindergarten Act in 1975 triggered a sharp increase in funding to widen childcare coverage in the following years, from below 10 to 30% between 1976 and 1979. Havnes and Mogstad (2011) find that the policy led to a relatively small increase in mothers' employment during this period. I exploit geographic variation in treatment intensity and compare, across municipalities, mothers with a 3-year-old child at the time of the reform to mothers with a 7-year-old child, who could therefore not benefit from the increase in subsidized childcare. I find that, despite the relatively small short-run employment effects, the effect of the policy accumulates over time and reaches a total increase of NOK 388,858 (USD 48,374) by age 62. I aim to further analyze the increase in earnings and how it affects retirement, and compare the costs and benefits of both policies through

[1] All monetary values are in 2015 NOK. 1 NOK = 0.1244 USD, average exchange rate for 2015.

an adaptation of the Marginal Value of Public Funds (MVPF) framework (Hendren & Sprung-Keyser, 2020).

This paper makes four contributions. First, it provides comprehensive evidence on the effectiveness of childcare-related pension credits as a tool to reduce the gender pension gap. Exploiting a retroactive extension of pension credits in Norway, I quantify both the mechanical reduction in the gender gap in lifetime pension benefits and the associated behavioral responses. While existing studies document that pension credits can narrow gender pension gaps (see, e.g., Cordova, Grabka, & Sierminska, 2022; Bonnet & Rapoport, 2020; Halvorsen & Pedersen, 2019), they typically abstract from labor supply, consumption, and household responses. By jointly analyzing employment, earnings, pension claiming, and consumption expenditure, this paper evaluates the consequences of correcting gender inequality through the pension system for individuals and households. While the behavioral response is small at the individual level, it substantially increases the aggregate cost of the policy.

Second, the paper contributes to the literature on labor supply responses to pension reforms by isolating a pure, realized change in pension wealth (see, e.g., Thiemann, 2015; Ye, 2022; Becker, Buslei, Geyer, & Haan, 2023; Pesner, 2024; Malavasi & Ye, 2024). The retroactive design of the reform generates an unexpected increase in pension wealth for women close to retirement, for whom working versus claiming is the relevant margin of adjustment. This setting allows me to identify labor supply and claiming responses to pension wealth shocks, abstracting from changes in incentives or eligibility rules. The estimated responses are comparable in magnitude to those found in studies of windfall income (see, e.g., Imbens, Rubin, & Sacerdote, 2001; Cesarini, Lindqvist, Notowidigdo, & Östling, 2017; Picchio, Suetens, & van Ours, 2018; Golosov, Graber, Mogstad, & Novgorodsky, 2023), suggesting that the "earned" nature of pension credits does not substantially alter labor supply reactions to wealth shocks. Artmann, Fuchs-Schündeln, and Giupponi (2024) use a similar policy change in Germany to study forward-looking female labor supply responses to a change in pension wealth. By exploiting the variation in how much the pension credits matter for final pension benefits, I am able to identify any retirement effects and provide evidence of the beneficiaries' response once the future income shock is realized.

Third, this paper contributes to the literature on the long-term effects of childcare policies. While previous research has documented mostly positive effects of childcare policies on maternal employment during preschool years (see, e.g. Fitzpatrick, 2010; Havnes & Mogstad, 2011; Herbst, 2017; Carta & Rizzica, 2018; Eckhoff Andresen & Havnes, 2019; Hojman & Lopez Boo, 2022), fewer studies examine whether these employment or earnings gains persist over time. Notable examples are Humphries, Neilson, Ye, and Zimmerman (2024) and Silliman and Mäkinen (2026), who incorporate long-term effects of public childcare on mother's earnings to reassess cost-benefit analyses of these policies. I extend this literature by studying the effects of childcare expansion on earnings until retirement, how it shapes retirement behavior including alternative retirement pathways, and its fiscal externalities. These results address a different cost-effectiveness question, which focuses

specifically on reducing the gender pension gap.

Fourth, the paper contributes to the welfare evaluation of gender-equality policies over the life cycle by benchmarking corrective pension credits against a preventive policy. Using the same cohorts, I compare the effects of retroactive pension credits to the consequences at the time of retirement of a subsidized childcare expansion that had relatively small short-run effects on female employment and earnings during working life. This comparison highlights the trade-offs between correcting and preventing gender inequality and informs the design of policies aimed at reducing gender gaps over the life cycle.

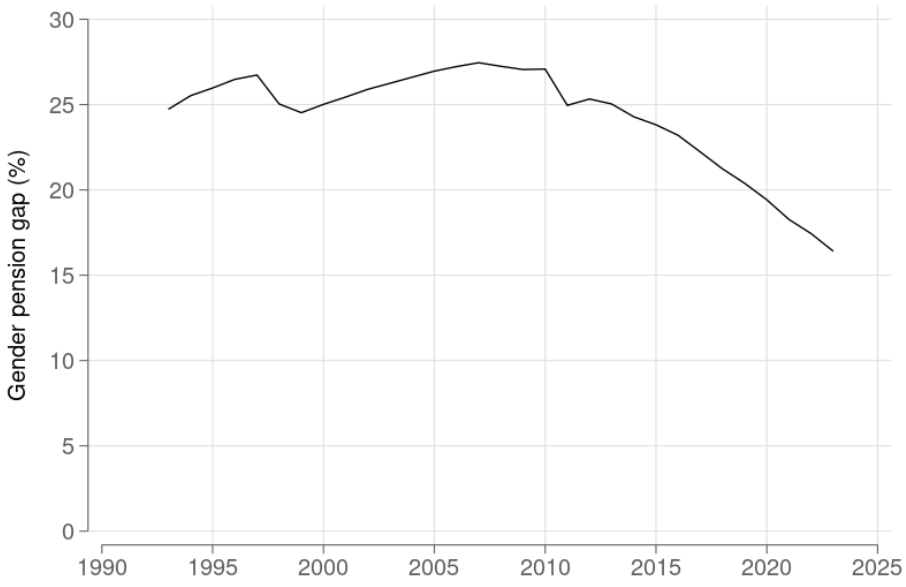
The paper is structured as follows. Section 3 studies an example of a corrective policy: the 2010 retroactive extension of childcare-related pension credits. I first outline the key aspects of the reform, then describe the data used for the analysis and discuss my identification strategy. Finally, I present the effects of an increase in pension wealth on employment, earnings, and retirement behavior. Section 4 studies an example of a preventive policy: the 1975 expansion of subsidized childcare. I summarize the details of the Kindergarten Act and its implications for childcare coverage, the data used for the analysis, and my identification strategy. I present the long-run effects of access to childcare on earnings and retirement behavior. Section 5 compares the fiscal externalities and sets out a cost-benefit analysis of both policies. Section 6 concludes.

2 The Gender Pension Gap in Norway

The gender pension gap is the difference in pension benefits between men and women, relative to men's pension benefits. Using administrative data for Norway, I calculate the gender pension gap between 1993 and 2023 (see Figure 1). The gap remained above 25% until 2010, after which it began to decrease steadily. A comprehensive pension reform was introduced in 2011, allowing for more flexible work and claiming behavior. However, the reform was implemented gradually and will only reach its full effect when cohorts born in or after 1963 retire. At the same time, cohorts born in the 1950s experienced a strong increase in female labor force participation from the 1970s to the 1980s, placing Norway at the forefront of female employment worldwide. As a result, part of the post-2010 trend also reflects compositional changes in the workforce now reaching retirement age. This highlights that the gender pension gap reflects both past gender differences in labor market outcomes and the gendered impact of the current pension system

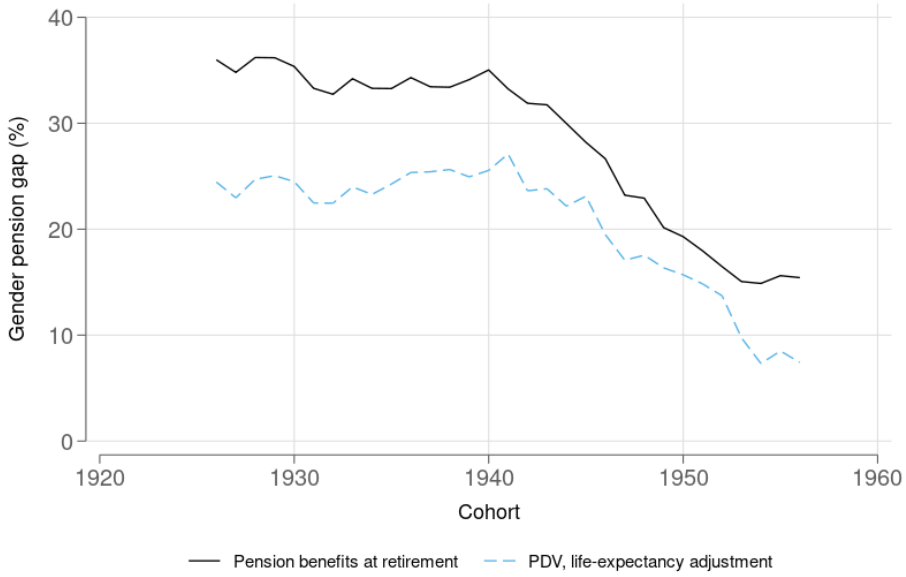
Figure 2 shows the gender pension gap by cohort, emphasizing how changes in female labor force participation over time combine to produce the cross-sectional gender pension gap. While younger cohorts face a substantially lower gap, it remains sizable, even when accounting for the fact that, on average, women live longer than men. For cohorts born between 1948 and 1953, four components determine pension benefits in the Norwegian system: the total number of years contributed to the National Insurance Scheme, the average pension points from the highest contribution years, the occupational pension scheme an individual is registered to, and the choice of retirement age once

Figure 1: Evolution of the Gender Pension Gap



Notes: This figure shows the evolution of the gender pension gap in Norway over time. Here, the gender pension gap is measured as the difference between men and women's average pension benefits relative to men. All individuals who are claiming any pension benefits in a given year are included in the calculations regardless of when they retired.

Figure 2: Gender Pension Gap by Birth Cohort



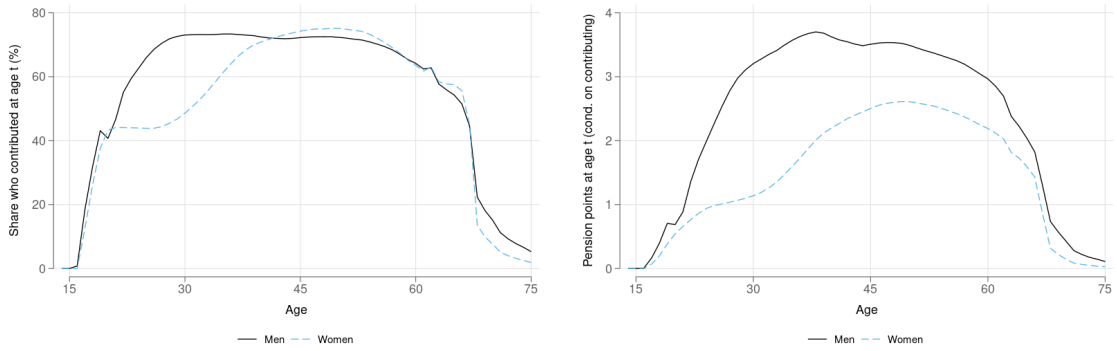
Notes: This figure shows the evolution of the gender pension gap by birth cohort. In this case, I compare men and women born in the same year, regardless of when they retired. For the gap in pension benefits at retirement (black), I calculate the real full-year equivalent starting pension. In this way, I obtain an annual value for individuals who retired after January in a given year. The present discounted value (PDV) at retirement of all payments until death (blue) accounts for gender differences in life expectancy. For individuals who are alive by 2021, the last year where I observe them, I add their cohort- and gender-specific life expectancy from 2021 as future payments.

eligible. How these components map onto gender differences in the labor market shapes the gender pension gap.

Contributions to the pension system are based on earnings and certain government transfers, including sickness and parental leave. In practice, the total number of credited years closely mirrors the total number of years employed. Employment breaks for childcare taken outside of parental leave translate into years without contributions. Figure 3a shows the share of women and men who contributed to the pension system by age. A large gender gap in contributions opens up early on, coinciding with childbearing years, and closes around age 40. By retirement, women have partly caught up to men in total contributed years, with over 50% having contributed enough to retire with their full pension.

Pension points, on the other hand, represent the "intensive margin" of contributions. They are a function of earnings and determine additional benefits above the baseline employment-based amount. For these cohorts, only the 20 years with the highest pension points are considered in the calculation of final pension benefits. Women start with lower pension points than men, and although the gap narrows over time, it never fully closes (see Figure 3b). Consequently, the gender gap in final pension points is larger than the gap in years contributed, even conditional on employment. The average difference in final pension points is around 1.2, which approximately corresponds to NOK 107,402 (USD 13,370) per year.

Figure 3: Gender Differences in Contribution Years and Pension Points by Age



(a) Positive Contributions

(b) Points Obtained (cond. on Contributing)

Notes: This figure shows the contribution behavior of men and women from birth cohorts 1948 to 1953 over the life cycle. Panel (a) shows the share of individuals who had positive contributions at age t, that is, who obtained non-zero pension points. Labor income and some government benefits, including sickness or parental leave, are used to compute annual pension points. The 20 years with the highest pension points partly determine final pension benefits at retirement. Panel (b) shows the pension-point equivalent to men and women's earnings over the life cycle, conditional on contributing.

Occupational pension schemes are determined by the firm or sector of employment. In Norway, public sector employees are automatically enrolled in the public early retirement scheme, while private sector employees may belong to the early retirement scheme for the private sector or have

firm-specific plans. Therefore, gender differences in sector and occupation translate into differences in occupational pension schemes. Panel A in Table 1 shows that a larger share of women are registered in the public sector scheme, whereas men are more represented in the private sector. Occupational schemes differ in how they increase old-age pension benefits.

Finally, retirement age determines when an individual stops accruing pension benefits. Delaying retirement is rewarded with higher benefits, and gender differences in retirement timing influence the overall pension received. Panel B in Table 1 shows that, for these cohorts, women tend to start claiming slightly later than men, potentially reflecting the time needed to accumulate enough contributed years for a full pension.

Table 1: Gender Differences in Pension Scheme and Claiming Age

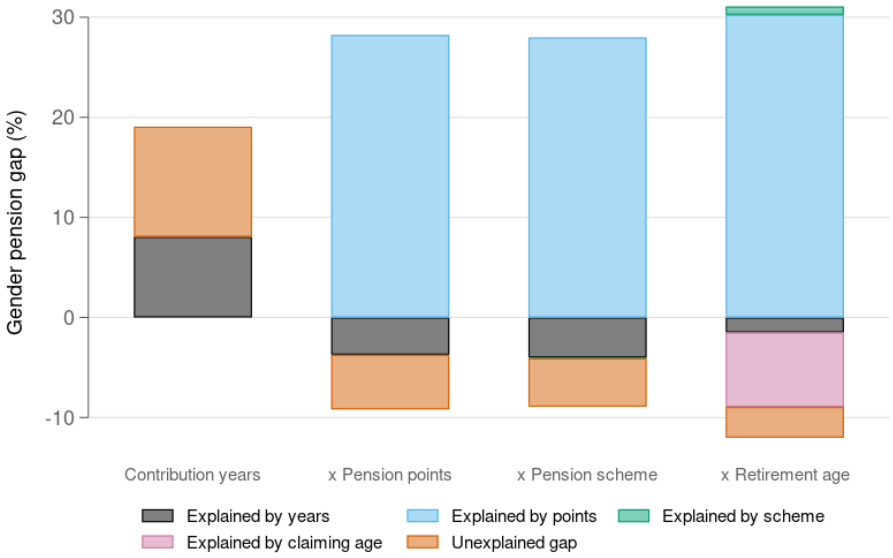
	Men	Women	Difference
Panel A: Occupational pension scheme			
Early retirement scheme, public sector	10.65	21.60	-10.94***
	[30.85]	[41.15]	(0.14)
Early retirement scheme, private sector	27.54	11.45	16.08***
	[44.67]	[31.85]	(0.15)
Other occupational scheme, private sector	61.81	66.95	-5.14***
	[48.59]	[47.04]	(0.19)
N	136,907	124,190	261,097
Panel B: Claiming age			
Claiming age, early retirement benefits	62.99	63.39	-0.40***
	[1.51]	[1.62]	(0.01)
Claiming age, old-age pension	64.63	66.09	-1.46***
	[2.29]	[1.82]	(0.01)
First claiming age	64.29	65.45	-1.16***
	[2.26]	[2.12]	(0.01)
N	152,089	141,424	293,513

Notes: This table reports the choice of occupational scheme and claiming age for men and women born between 1948 and 1953. The claiming age is defined as the age when an individual claims early retirement or old-age pension benefits for the first time. In Norway, it is possible to claim while working. As a result, the claiming age is not necessarily equivalent to the retirement age. Standard deviations in brackets. Standard errors in parentheses. *** $p < 0.001$.

I combine these four elements in a Blinder-Oaxaca decomposition to examine their relative importance for the gender pension gap (see Figure 4). Final pension points appear to be the primary driver of the large gender pension gap. In fact, if the gap were determined solely by differences in pension points, it would be around 10 pp (50%) larger. Other elements, especially the employment-based

baseline amount and differences in claiming age, help reducing earnings-based disparities. This suggests that policies aimed solely at correcting or preventing differences in employment, without addressing earnings differences, are unlikely to reduce the gender pension gap. Equivalently, policies that lead women to claim earlier can mitigate the negative effect of claiming age on the gap.

Figure 4: Decomposition of the Gender Pension Gap



Notes: This figure shows a twofold Blinder-Oaxaca decomposition using coefficients from a pooled regression of the gender pension gap for birth cohorts 1948 to 1953. I sequentially add each of the four elements that determine pension benefits at retirement. The graph reports the explained gap by each component and the residual unexplained gap. Note that negative values of the unexplained gap imply that, based on the differences in characteristics, the gender pension gap would be larger than its value.

3 A Corrective Policy: The Retroactive Extension of Childcare Pension Credits

3.1 Institutional Setting

As women often have lower labor force attachment and earnings after childbirth, their pension accrual during this period - and thus their future pension benefits - are also lower. Childcare-related pension credits, known in Norway as "pension accrual for unpaid care work," top up a woman's pension entitlements to a set minimum in the years following childbirth. For women whose labor income would grant higher pension accrual than the minimum, their pension rights are calculated as usual.

Pension accrual for unpaid care work was introduced in 1992,[2] and applied to all births from that date. From January 1st, 2010, pension accrual for unpaid care work was retroactively granted to eligible individuals who gave birth between 1967 and 1991.[3] This retroactive extension is the reform of interest in this paper. The older cohorts affected were between 57 and 62 years old at the time of implementation, meaning that fertility and most employment decisions, especially those that would be pension-relevant, had already been made. Therefore, the reform can only influence behavior around retirement.

The policy applies for up to 5 years after birth and is granted to the lower earner of the parents. Individuals must be members of the Norwegian National Insurance scheme[4] to become eligible, there are no specific requirements to document the time spent on childcare, and the policy is compatible with being employed. Although gender neutral in legal terms - in principle, men who fulfill both eligibility criteria could receive pension accrual -, women are far more likely to be both below the income threshold and the lower earner, so the policy primarily affects women and thus indirectly reduces gender gaps in pension outcomes.

The Norwegian pension system consists of three pillars: (1) the National Insurance scheme, (2) occupational pension schemes, the largest of which is AFP, and (3) private pension savings. The policy applies to women born between 1948 and 1953 only through private-sector AFP, and gradually through the National Insurance scheme and AFP for women born from 1954. To study retirement behavior where the policy is akin to a windfall gain, the treated population is therefore restricted to women employed in AFP-covered private sector firms.

At the time of the reform, old-age pension benefits combined a fixed component and an earnings-related supplement based on the 20 years with the highest earnings. As part of AFP, an additional fixed percentage of annual earnings was accrued and incorporated into old-age pension benefits at retirement. Since final pension benefits take into account the entire earnings history, the monetary

[2] See FOR-1993-06-09-439

[3] See Section II, Chapter 3, article 3-16 and Section VI, Chapter 20, article 20-21 of the National Insurance law, LOV-1997-02-28-19. These modifications were introduced by LOV-2009-06-05-32.

[4] In Norway, membership of the National Insurance scheme is defined by employment or residence.

impact of the policy varies across individuals. It depends on whether the years covered by the policy are among the highest 20 earnings years and on the difference between actual income and the policy threshold in each of them. To quantify the shock to pension wealth, I compute counterfactual pension benefits in the absence of the reform using the detailed calculation rules of the first and second pillars described in Appendix B.

3.2 Data and Sample Definition

I combine population-wide administrative data from different registries at Statistics Norway between 1967 and 2023. First, I use the population registry to identify all women born between 1948 and 1956 who gave birth to at least one child between 1967 and 1991. This dataset contains information on their country of birth, marital status, education level, and place of residence. Second, I use the matched employer-employee registry to obtain information on employment, work hours, occupation, and industry. Third, I use the different income and social security registries to obtain information on their earnings, date and degree of early retirement, date and degree of old-age retirement, and the total amount of pension benefits received. I can identify individuals who retire through AFP in the public and private sector, as well as distinguish them from those who receive an occupational pension but are not members of AFP, in the income register. To facilitate the analysis, I apply the categories of Eika et al. (2020) to the income and wealth registers, distinguishing between components of disposable income, wealth, and capital gains.

Using the unique family identifier, I can link all married or cohabiting individuals to their partners. As a result, I am able to build a panel dataset with comprehensive information on the mothers' earnings, employment, retirement behavior, and demographic characteristics, together with that of their spouses. Note that, to identify any spillovers of the policy beyond its beneficiaries, I refer to "spouse" as the partner in each year, regardless of whether they are the same as the father of the child for which pension accrual is granted.

The sample is restricted to the 1948–1953 cohorts due to data availability and a later pension reform. First, to study retirement behavior, individuals must turn 62 before 2021, with older cohorts providing more information post-retirement. Second, the 2011 pension reform changed the pension calculation rules and increased work incentives by allowing retirees to work and claim flexibly. Pension benefits for the 1948-1953 cohorts are entirely calculated according to the old rules, and I estimate the effect on labor supply within the groups defined by Hernæs, Markussen, Piggott, and Røed (2016) to ensure that incentives to postpone retirement are the same for treatment and control. For women in AFP in the private sector, the relevant criterion is whether they are entitled to a full public pension at age 62 after the reform. While this ensures that the impact of the 2011 reform does not confound my estimates, it reduces both the sample size and the magnitude of the wealth shock.

Using the pension rules described in Appendix B, I estimate the counterfactual pension benefits had the policy not been implemented for all mothers in my sample. Since the retirement age, and thus

pension benefits at retirement, are endogenous, I estimate the pension benefits including the policy coverage, and define the impact of the policy as the difference between these two values measured in 2009. Figure C.1 in Appendix C shows how the old-age pensions estimated following the rules outlined in Appendix B map to the pension benefits perceived by the individuals in the sample.

Descriptive Statistics

There are 230,809 women born between 1948 and 1953 in the Norwegian population registry. I remove from my analysis women who (1) were not alive by 2010, (2) did not give birth at least once between 1967 and 1991, and/or (3) had a child after 1992 and were thus affected by the original introduction of pension accrual for unpaid care work. This reduces the sample to 159,716 women, 154,438 (96.7%) of whom had claimed pension benefits by 2021. Finally, 21,023 women from this sample retire through AFP in the private sector and hence are potentially eligible for the retroactive extension of pension accrual for unpaid care work. Note that membership in the AFP scheme requires being employed at an affiliated firm for at least 7 years between age 53 and 62. Therefore, it is not possible for the 1948-1953 cohorts to fulfill the AFP eligibility requirements if they were to sort into an AFP firm as a response to the reform.

Most of the women who could be policy beneficiaries satisfy the eligibility criteria: 96.5% of the sample are both the lower earner of the couple and below the income threshold of the policy for at least one year when policy support applies. In fact, 60.0% of the sample were eligible for pension credits during all years of policy coverage, up to five after each birth. These values are very similar for the women in the sample regardless of their occupational pension group.

Impact of the Policy

The impact of the policy at the individual level is measured by the difference between the estimated pension benefits with and without the policy. Following this approach, the policy leads to an average NOK 6,296 (USD 881), or a 2.1% increase in annual pension benefits at the time of retirement. For the women in the sample who retired by 2021, the policy had a non-zero impact for 39.6% of them.

Tables C.5, C.6 and C.7 in Appendix C show the impact of the policy by country of origin, education level, and marital status at retirement, respectively. Since the retirement age is potentially endogenous, I also report the same figure measured at age 62. The changes in magnitude between the two can be explained by (1) the accumulation of years of membership in the Social Insurance scheme, (2) the yearly updates of the National Insurance basic amount, and (3) the fact that the annuity divisors change with the retirement age.

Finally, Table 2 shows a selection of demographic characteristics for the mothers in the sample, split between mothers who were never eligible for the policy, mothers who were eligible for the policy and in the relevant occupational pension group but for whom it had no impact on their final pension benefits (eligible with zero impact), and mothers in the relevant occupational pension group who

received higher pension benefits as a result of the policy (eligible with non-zero impact). The table shows that the women who benefit from the policy are substantially different in most dimensions from those who do not. Note that, by definition, the policy only impacts women eligible to retire through AFP in the private sector. Among other criteria, the AFP scheme requires a certain degree of attachment to the labor force prior to retirement. This explains the high employment rate in the sample. Table C.2 in Appendix C compares eligible mothers to the general female population.

Table 2: Descriptive Statistics

	Never eligible	Eligible		
		Zero impact	Non-zero impact	Difference
Share born in Norway (%)	91.79	90.73	92.59	-1.86***
	[27.47]	[29.01]	[26.19]	(0.40)
Share married (%)	59.60	65.90	74.83	-8.93***
	[49.10]	[47.41]	[43.40]	(0.66)
Share with college education (%)	60.00	32.93	11.95	20.98***
	[49.02]	[47.00]	[32.44]	(0.60)
Number of children	1.58	2.08	2.33	-0.25***
	[0.68]	[0.77]	[0.87]	(0.01)
Share working (%)	88.61	87.70	80.81	6.88***
	[31.79]	[32.85]	[39.38]	(0.51)
Share working full-time (%)	85.96	82.84	40.57	42.28***
	[34.76]	[37.70]	[49.10]	(0.61)
Annual earnings (NOK)	596,843	471,490	293,090	178,400***
	[295,838]	[185,035]	[119,997]	(2,332)
Earnings difference with partner (%)	24.27	29.57	43.84	-14.27***
	[67.48]	[59.07]	[46.97]	(0.78)
Age difference with partner	-1.65	-2.16	-2.16	-0.01
	[4.53]	[3.93]	[3.64]	(0.06)
Share whose partner is retired (%)	10.31	11.73	12.52	-0.79
	[30.44]	[32.18]	[33.10]	(0.53)
N	755	12,101	8,167	20,268

Notes: This table reports pre-reform demographic characteristics for the birth cohorts 1948–1953 retiring through AFP in the private sector. Eligibility is defined as having been the lower earner of the couple and below the income threshold for at least one year during the period covered by the policy. Policy impact is defined as the difference between the estimated pension benefits with and without the policy. Being employed is defined as having annual labor earnings above the basic income level, and full-time employment requires annual labor earnings above four times the basic income level. All monetary values are in 2015 Norwegian kroner (NOK). Standard deviations are in brackets, standard errors in parentheses. Significance stars refer to the Difference column: *** $p < 0.001$.

Present Value of Pension Accrual for Unpaid Care Work

The beneficiaries for whom the policy had a non-zero impact will receive a higher pension every year until death. I calculate the present value of the pension increase as follows

$$PV = WP \times \frac{1 - \frac{1}{(1+r)^n}}{r} \tag{1}$$

where WP is the difference in pension benefits with and without the policy measured at age 62 (at the age of retirement). n is the difference between age 62 (age of retirement) and the age of death, when applicable. Otherwise, it is the difference between age 62 (age of retirement) and the life expectancy measured at that point in time for the individual's birth cohort, gender, and education level. Life expectancy data was obtained from the Norwegian Institute of Public Health. Finally, r is the net interest rate and takes value 1.96%, following common practice for calculating present values of contributions and benefits in Norway (Fredriksen & Stølen, 2017).

Table 3 shows the present value of policy impact, for the whole sample of beneficiaries and for those beneficiaries for whom the policy had a non-zero impact. At the age of retirement, childcare-related pension credits amount to a discounted value of NOK 121,514 (USD 17,004). For comparison, this is equivalent to 2.5% of the average lifetime pension benefits.

Table 3: Present Value of the Final Increase in Pension Benefits as a Result of the Policy

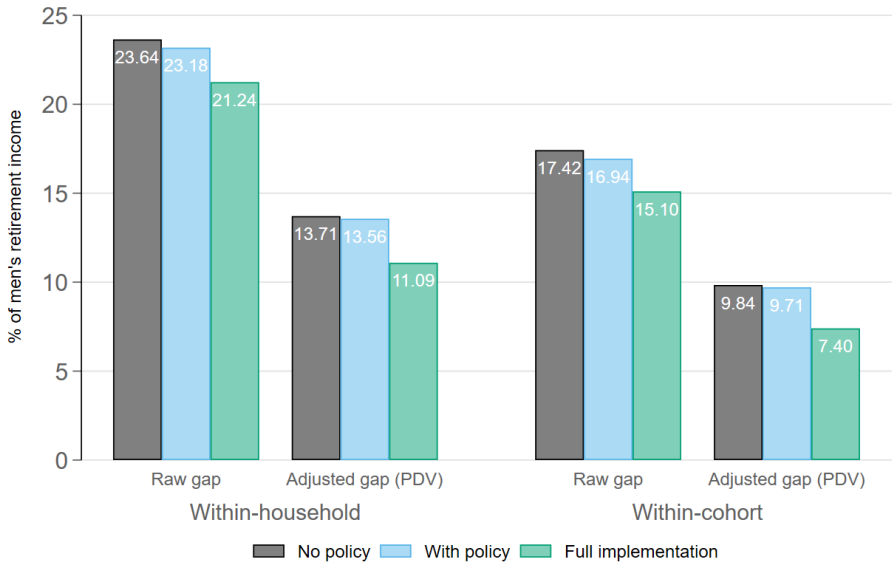
	All eligible	Eligible with non-zero impact
At age 62 (NOK)	42,114	98,591
	(84,533)	(105,645)
At retirement (NOK)	51,798	121,514
	(105,947)	(133,648)
N	20,921	8,918

Notes: These table reports the present value of the increase in pension benefits induced by the policy. The column "all eligible" includes all women from birth cohorts 1948–1953 retiring through AFP in the private sector. Policy impact is defined as the difference between the estimated pension benefits with and without the policy. Note that annuity divisors exist only from age 62. This is also the earliest possible age to retire. Standard deviations in parentheses.

Counterfactual vs. Realized Gender Pension Gap

To assess whether the policy reduced the gender pension gap, I first assume that men's pension benefits were not affected by the reform. Among individuals born between 1948 and 1953 who retired by 2023, the gender gap in combined old-age and occupational pension benefits is 16.94%. On average, men receive NOK 54,121 (USD 6,733) higher benefits than women (see Table C.8 in Appendix C).

Figure 5: Counterfactual vs. Realized Gender Pension Gap, Summary



Notes: This figure shows the impact of the increase in pension benefits as a result of the policy on the gender pension gap for birth cohorts 1948 to 1953. The raw gap corresponds to the difference in full-year equivalent pension benefits at the time of retirement. The adjusted gap is measured as the PDV of lifetime pension benefits, imputing life expectancy for individuals still alive by 2021. The "full implementation" scenario computes the counterfactual aggregate impact of the policy had it applied to women in other occupational schemes.

Removing the policy-induced increase in benefits for each beneficiary, I compute a counterfactual gender pension gap of 17.42%. The policy therefore reduced the gender pension gap by 0.48 pp. This corresponds to an average increase in women's pensions of NOK 1,518 (USD 189).

The gender gap in lifetime instead of annual pension benefits is smaller because women have higher life expectancy. I calculate the gender gap in lifetime pension benefits as the difference between the present discounted value of pension benefits, at 9.71% (see Figure 5). In this case, the policy reduces the gender gap in lifetime pension benefits by 0.13 pp. This corresponds to an average present cost of NOK 6,624 (USD 824) per woman, amounting to a total cost of NOK 1.05 billion (USD 131 million).[5]

I use the calculation rules of the first pillar to calculate how much childcare-related pension credits could reduce the gender pension gap if the policy fully applied to the old-age pensions of the 1948-1953 cohorts. Specifically, I compute counterfactual pension benefits for mothers in these cohorts as if the policy entered their final pension benefits in the same way as for the mothers born after 1963. Figure 5 presents the results, with more detail in Table C.9 in the Appendix. Under this "full

[5]This figure is the sum of the increase in lifetime pension benefits for all beneficiaries, including any behavioral response in employment or earnings. For now, I abstract from the impact of the policy on tax revenue: on the one hand, there is a mechanical increase in tax revenue from the taxation of pension benefits. On the other hand, pensions are taxed on average less compared to wages. If beneficiaries retire early as a result of the policy, the behavioral response may lead to a decrease in tax revenue

implementation" scenario, pension accrual for unpaid care work could reduce the gender pension gap by 1.94 pp, and the gender gap in lifetime pension benefits by 2.31 pp. However, this would require an average additional expenditure of NOK 113,810 (USD 14,158) per woman, a total cost of NOK 18.66 billion (USD 2.32 billion).

Finally, I compare annual and lifetime pension benefits of women born between 1948 and 1953 with those of their partners in 2009. Although this restricts the sample to 72.68% of the population, the resulting "within-household gender pension gap" is informative from a household bargaining perspective. The within-household gap is substantially larger than the within-cohort gap across all scenarios, implying that the relative impact of the policy in narrowing the pension gap is smaller (see Figure 5 and Tables C.10 and C.11 in the Appendix for more detail).

3.3 Identification Strategy

I want to identify the effect of the increase in pension wealth on the beneficiaries' behavior around retirement. Identification will rely on the possibility that the policy has a zero effect for some of its beneficiaries due to their employment and earnings history outside the period covered by the policy and prior to the announcement of the retroactive extension. This is possible because only the 20 years with highest contributions are used to determine the final amount of pension benefits upon retirement.

For illustration, consider two women, A and B, born in 1950, who gave birth in 1980 and were both below the income threshold and the lower earner in their respective couple between 1980 and 1985. In 2010, they are 60 years old, covered by AFP in the private sector, and will be able to retire from age 62. Throughout her working life, Woman A earned above the income threshold for 15 years after 1985. As a result, the 5 years of policy support count towards her 20 best income years, and thus determine (increase) her pension benefits. On the other hand, Woman B earned above the income threshold for 22 years after 1985. Then, the 5 years of policy support will not enter the equation to determine her final pension points: she is a beneficiary of pension accrual for the childcare she provided between 1980 and 1985, but this will not translate into higher final pension benefits. This means that, to study the employment effects caused by the policy, what matters is not whether the individual is a beneficiary, but whether the policy had a non-zero impact on their pension wealth. This is determined by where the years with policy support are located in the ranking of contribution years.

To ensure that the treatment and control groups follow similar earnings paths after childbirth, I exploit how the years with policy support rank among individuals' contribution years. Women whose supported years are within the top 20 contribution years constitute the treatment group, while the control group includes women whose supported years rank between positions 21 and 28. Table 4 shows that the further the supported years fall below the top 20, the more different the groups are from each other. Figure C.5 in Appendix C shows that earnings trajectories are very similar for the 24- and 30-year cutoffs. Choosing a bandwidth between these values improves precision while

keeping both groups comparable.

Finally, earnings after the reform but before retirement are included in the ranking of contribution years. If the policy affects earnings, this mechanically changes which years will fall within the top 20 at retirement. Then, treatment status partly reflects the effects of the policy and is thus endogenous. To avoid this problem, I assign treatment based only on the pre-reform ranking of contribution years. In practice, this requires individuals in the control group to have already accumulated at least 20 years with contributions above the policy threshold by 2009.

Table 4: Descriptive Statistics Across Potential Control Groups

	Treatment	Control		
		24 yrs	30 yrs	36 yrs
Share born in Norway (%)	92.58	91.71	93.23	93.72
	[26.21]	[27.58]	[25.12]	[24.26]
Share married (%)	74.82	66.45	67.32	66.99
	[43.41]	[47.23]	[46.91]	[47.03]
Share with college education (%)	11.97	18.18	24.23	30.27
	[32.46]	[38.58]	[42.85]	[45.95]
Number of children	2.33	2.14	2.16	2.13
	[0.87]	[0.75]	[0.74]	[0.74]
Share working (%)	80.81	85.97	88.27	89.53
	[39.38]	[34.74]	[32.19]	[30.62]
Share working full-time (%)	40.58	78.64	84.44	87.00
	[49.11]	[41.00]	[36.25]	[33.63]
Annual earnings (2015 NOK)	293,132	386,469	430,747	467,102
	[120,011]	[105,047]	[116,427]	[140,886]
Earnings difference with partner (%)	43.84	35.08	31.61	28.41
	[46.97]	[54.08]	[55.10]	[57.26]
Age difference with partner	-2.16	-2.13	-2.11	-2.09
	[3.65]	[4.01]	[3.80]	[3.80]
Share whose partner is retired (%)	12.53	12.00	11.68	10.98
	[33.11]	[32.51]	[32.12]	[31.27]
N	8,170	1,568	4,994	8,453

Notes: This table reports the pre-reform demographic characteristics for the birth cohorts 1948–1953 retiring through AFP in the private sector. The treatment group is defined as those eligible for whom the impact of the policy is non-zero. The control groups are defined by restricting the years where the policy support would apply to a certain range in the ranking of all contribution years. Being employed is defined as having annual labor earnings above the basic income level, and full-time employment requires annual labor earnings above four times the basic income level. All monetary values are in 2015 Norwegian Standard deviations are in brackets.

The main specification is as follows:

$$Y_{it} = \beta_0 + \beta_1 Treated_i \cdot Post_t + \beta_2 Treated_i + \lambda_c + \delta_t + \boldsymbol{X_{it}'}\boldsymbol{\gamma} + \varepsilon_{it}$$

where β_1 is the effect of the policy on outcome Y_{it}. $Treated_i$ is a dummy variable that takes value 1 if the individual is a policy beneficiary with non-zero impact on their pension benefits, and 0 if the individual is a policy beneficiary with (1) zero impact on their pension benefits, and (2) years of policy support ranked above 28 in their pre-reform ranking of contribution years. λ_c are cohort fixed effects, δ_t are year fixed effects, and $\boldsymbol{X_i'}$ includes country of origin, education level, marital status, municipality of residence, full vs. part-time employment, number of children, total household income, partner's employment status and partner's age. For all time-varying characteristics, the value for 2009 is the one that enters the regression.

And I retrieve the dynamic treatment effects from

$$Y_{ict} = \beta_0 + (\sum_{t=2003[t\neq 2009]}^{t=2023} \beta_t \cdot Treated_i) + \lambda_c + \delta_t + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_{it} \tag{2}$$

where the estimates for β_{2003}, β_{2004}, ..., and β_{2008} will allow me to evaluate the presence of pre-trends, and the estimates for β_{2010}, β_{2011}, ..., β_{2023} are the relevant treatment effects. I include the same controls as in the main specification.

Alternative control groups could, in principle, be considered. First, women born before 1948 and retiring through AFP in the private sector have no access to the policy. However, they are subject to different pension calculation rules and may have a very different retirement behavior. Second, women without children are, by definition, never treated, but have very different employment and earnings trajectories in the years leading up to retirement. Figure C.6 in Appendix C shows that this is the case, but after the reform the mothers with no positive impact from the policy behave very similarly to the women without children. Finally, while men born in the same years are not a suitable control group, I can apply the same procedure to assign treatment status and assume they are the lower earner of the couple to test my identification strategy. Panel A from Figure C.6 in Appendix C shows that the earnings difference between the men who would be assigned to treatment and control is the same before and after the reform. Note that there can be spillovers from treated women to their partners. However, there are more men than women retiring through AFP in the private sector. This implies that a substantial share of the men in this comparison will have a partner outside AFP, and therefore not treated. Additionally, focusing on men born in 1952 limits any potential spillovers, since there is an average age gap of 2 years.

My main specification is limited to study retirement behavior, since no individuals in the sample were eligible to retire before 2010. I want to identify the effect of the policy, that is, of a change in

pension wealth, on the beneficiaries' claiming age. Pension benefits are endogenous to the claiming age, but the policy-induced variation is exogenous. We can formalize the relationship between claiming age and pension wealth as follows:

$$\begin{aligned} Y_i &= \alpha + \beta P_{it} + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_i \\ &= \alpha + \beta(NP_{it} + WP_{it}) + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_i \end{aligned} \tag{3}$$

where Y_i is the claiming age for individual i , $\boldsymbol{X_i'}$ is a vector of individual characteristics and ε_i is the error term. P_{it} are the pension benefits individual i is entitled to at time t. Note that P_{it} can be decomposed into NP_{it}, the pension benefits that individual i would receive had the policy not been implemented, and WP_{it}, the amount in final pension benefits from the policy. I instrument P_{it} with its exogenous component, WP_{it}, for the sample of policy beneficiaries. Counterfactual pension benefits and the impact of the policy are both calculated pre-reform, so the t subscript drops. Then, I estimate β using two-stage least squares (2SLS). The reduced-form specification for 2SLS is then:

$$Y_{ic} = \alpha + \beta WP_{ic} + \theta FP_{ic} + \boldsymbol{X_{ic}'}\boldsymbol{\gamma} + \lambda_c + \varepsilon_{ic} \tag{4}$$

where WP_{ic} is the increase from the policy measured at age 62 and FP_{ic} are estimated old-age pension benefits (the part of NP_{it} not affected by the policy). The variables in $\boldsymbol{X_{ic}'}$ are the same as in the difference-in-differences (DiD) specification. For the individual characteristics that are time-varying, the regression includes the value for 2009.

For the estimate to have a causal interpretation, my instrument must satisfy the relevance and exclusion restrictions. Note that the relevance of the instrument is equivalent to the impact of the policy on final pension benefits, and the exclusion restriction requires that the impact of the policy is not correlated with the change in pension wealth from the broader 2011 pension reform. I discussed in Section 3.2 how the cohorts for whom the policy has the highest impact (i.e., more relevance) are also the cohorts that are more exposed to the 2011 reform (which does not in principle, but may violate the exclusion restriction). As a result, the trade-off identified previously is, in fact, a trade-off between relevance and exclusion. To the extent that the 1948-1953 cohorts, for whom the old pension rules apply, are sufficiently affected by the policy, as shown previously, both restrictions are satisfied.

Next, I use the 2SLS and DiD strategies to estimate the effect of the reform on employment, retirement and labor earnings at time t. I define employment as having non-zero labor earnings. To consider the effect on employment in the intensive margin, I use both the registered working hours and an earnings-based measure of full vs part-time employment. Note that, under this definition, not being employed is not equivalent to dropping out of the labor force. This is measured instead

as a binary variable that takes value 1 in the time periods after the last employment spell. Finally, I define retirement as being registered as an AFP or an old-age pension claimant at time t.

3.4 Results

To evaluate behavioral responses to the policy, I estimate the effect of the increase in pension wealth on retirement and labor supply. In the Norwegian context, where it is possible to work while claiming pension benefits, retirement is defined more broadly. Table 5 shows the 2SLS estimates for claiming age and full retirement age, defined as the time when an individual claims their full pension and does not receive any labor income. For the average increase in pension wealth, beneficiaries anticipate pension claiming by 1.8 months, with no substantial differences by income or education. This implies an additional average fiscal cost of NOK 43,094 (USD 5,361) per capita.

Table 5: Retirement Age: IV Estimates

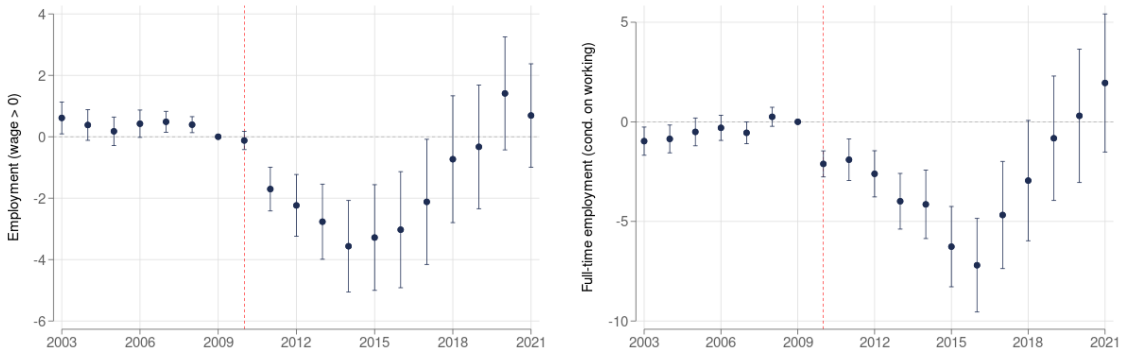
	Age at which claimed benefits for the first time (months)			Age at which claimed 100% retirement (months)		
Increase in pension benefits	-1.70***	-1.80***	-3.55**	-1.80***	-1.91***	-3.80**
	(0.17)	(0.19)	(1.18)	(0.18)	(0.21)	(1.27)
Cohort FE	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
N	19,954	19,954	14,611	19,954	19,954	14,611
Dep. var. mean (years)	64.47	64.47	64.35	64.89	64.89	64.80
First-stage R-squared	0.45	0.46	0.49	0.45	0.46	0.49
First-stage F-statistic	104.48	89.31	8.65	104.48	89.31	8.65
First-stage p-value	0.000	0.000	0.003	0.000	0.000	0.003

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the 2SLS estimates, with the calculated first-pillar pension and the increase in pension benefits from the policy as instruments for total (first and second-pillar) pension benefits. Controls include country of birth, education level, marital status, number of children, household income, municipality of residence, hours worked, partner's age and partner's employment status in 2009.

Figure 6 summarizes the effects on employment at the extensive and intensive margins. Employment declines by 1.9%, and full-time employment by 3.2%, corresponding to a labor supply elasticity of 0.55. The employment response is present only once beneficiaries are eligible to retire and atenuates after 2014, when all cohorts in the sample have reached retirement age. Moreover, employment responses are smaller for cohorts treated further from retirement, whereas reductions in full-time work are similar across cohorts. This pattern suggests that, conditional on remaining employed, beneficiaries adjust hours similarly, but treated cohorts differ in their decision to remain in the labor force.

Disability insurance represents an additional margin of adjustment. When the reform was implemented, disability benefits were calculated using the rules for pension benefits, and childcare-related

Figure 6: Impact of the Policy on Employment



(a) Employment (wage > 0)

(b) Full-time employment (cond. on working)

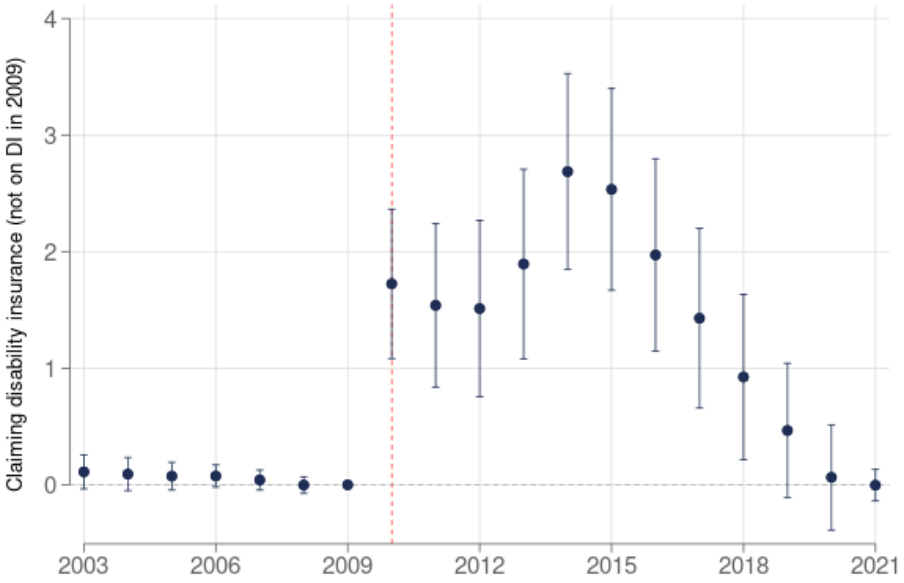
Notes: This figure shows event-study plots from the specification in Equation (2). I estimate the average treatment effect of an increase in pension benefits on employment, comparing eligible women who experienced an increase in pension benefits to eligible women who did not benefit but whose years of policy coverage are between top 20 and top 28 in their contribution years ranking. All cohorts are pooled for the analysis, and figures by cohort are reported in Figure C.9 Appendix C.

pension credits applied whenever favorable. As a result, the policy also increased the generosity of disability insurance for treated mothers. Figure 7 shows a 0.4 pp (6.8%) increase in disability take-up, which declines as women reach age 67 and transition from disability to old-age pension. Unlike employment, disability take-up responses begin immediately after treatment and persist until age 67, with stronger effects for younger cohorts. This suggests that younger cohorts, who cannot access higher pension benefits at the time of treatment, maximize income instead through disability insurance, an incentive absent for older cohorts who are eligible to retire shortly after treatment. This policy interaction may explain the absence of forward-looking labor supply responses prior to retirement, in contrast to findings for younger mothers in Germany (Artmann et al., 2024). The smaller magnitude relative to employment likely reflects health eligibility constraints.

The employment response is concentrated among married women, especially for those whose partners were already retired in 2009 (see Figure 8). This is consistent with leisure complementarities within couples. However, there is only a very small employment effect of the reform on the beneficiaries' partners, and it is only significant in the first year after the reform (see Figure C.10 in Appendix C). This effect does not translate into lower partner earnings, implying no additional negative effect on household income. This result is in line with previous evidence that spousal responses are present mainly when shocks affect the household's primary earner.

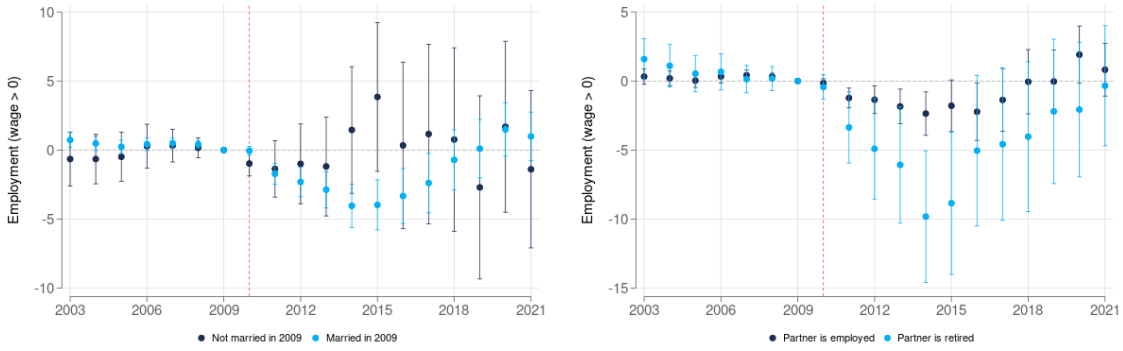
Table 6 reports average DiD estimates for a selection of outcomes. I adjust my previous calculations for the PDV of policy-induced pension wealth from 2010 until death to 2010-2021, so I match the time period for which I estimate the decrease in wages. The ratio of the two yields a marginal propensity to earn (MPE) of 0.24

Figure 7: Take-up of Disability Insurance



Notes: This figure shows event-study plots from the specification in Equation (2). I estimate the average treatment effect of an increase in pension benefits on take-up of disability insurance, comparing eligible women who experienced an increase in pension benefits to eligible women who did not benefit but whose years of policy coverage are between top 20 and top 28 in their contribution years ranking. All cohorts are pooled for the analysis, and figures by cohort are reported in Figure C.9 in Appendix C.

Figure 8: Heterogeneity in Employment Response



(a) By Marital Status

(b) By Partner's Employment Status

Notes: This figure shows event-study plots from a heterogeneity analysis based on the specification in Equation (2). I estimate the average treatment effect of an increase in pension benefits on employment, comparing eligible women who experienced an increase in pension benefits to eligible women who did not benefit but whose years of policy coverage are between top 20 and top 28 in their contribution years ranking. Panel(a) compares women who were married pre-reform to non-married women. This includes women who never married, women who are divorced / separated, and widows. Cohabiting women are classified as married. Panel (b) compares, among women who have a partner pre-reform, those whose partner is still in the labor force to those whose partner is retired. All cohorts are pooled for the analysis.

Table 6: Main Results: Difference-in-Differences Estimates

Panel A: Employment and earnings			
	Employment	Full-time employment	Annual labor earnings
Treatment	-1.49**	-2.68***	-30,602***
	(0.51)	(0.49)	(1,983)
Controls	Yes	Yes	Yes
Dep. var. mean	76.54	83.66	317,605
N	260,449	199,341	140,412
Panel B: Spillovers to the partner			
	Partner's employment	Partner's income	Household income
Treatment	0.07	-5,125	-8,388
	(0.68)	(6,716)	(4,700)
Controls	Yes	Yes	Yes
Dep. var. mean	72.27	545,234	634,670
N	204,276	201,434	244,969
Panel C: Benefits payments			
	DI	Sick leave	UB
Treatment	1,357**	-750*	-1,135***
	(429)	(301)	(178)
Controls	Yes	Yes	Yes
Dep. var. mean	8,221	7,323	1,665
N	258,595	258,595	258,595

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the difference-in-differences estimates obtained using the specification from Equation 1. I estimate the average treatment effects of an increase in pension benefits on the beneficiaries' employment and transfers received, and on the partner's employment and earnings. All regression models include cohort and year fixed effects. Controls include country of birth, education level, marital status, number of children, household income, municipality of residence, hours worked, partner's age and partner's employment status in 2009. All monetary values are in 2015 Norwegian kroner (NOK).

Finally, Table 7 summarizes the impact of the policy on selected income and wealth components. The NOK 8,462 (USD 1,053) reduction in taxes paid represents an additional fiscal cost and reflects lower tax rates for pensioners. A preliminary estimate of consumption expenditure, using the same adjustment as for the policy's PDV, implies a marginal propensity to consume (MPC) of 0.36—closely aligned with estimates for comparable age and income groups in Golosov et al. (2023).

4 A Preventive Policy: The Expansion of Subsidized Childcare

Pension accrual for unpaid care work acknowledges that childcare-related employment and earnings losses have negative consequences for future pension benefits, and aims at mitigating the latter. An alternative approach is to directly address childcare-related employment and earnings losses, which will then mechanically improve women's future pension benefits. Both arrive to the same outcome, but operate through different channels, and their relative cost-effectiveness may differ.

In late 1975, the Norwegian government approved a large-scale expansion of subsidized childcare for children ages 3 to 6, to take place in the coming years. This is an example of a policy that, if successful in increasing female employment and earnings, could lead to a lower gender pension gap for the cohorts affected. Havnes and Mogstad (2011) study the impact of the policy on affected mothers up to 3 years after the reform and find a 1.1 pp increase in employment. This is a very small effect relative to its substantial cost: federal funding for childcare increased by NOK 392 (USD 48.7) million in only one year. However, the employment and earnings effects may persist over time, and even accumulate. To study the impact of expanding childcare on pension benefits, it is necessary to consider how it affects women's whole career.

4.1 Institutional Setting

The Kindergarten act (*barnehageloven*, LOV-1975-06-06-30) was introduced in 1975 and constituted a major reform of the kindergarten sector. The law defined kindergarten as educational institutions rather than child welfare institutions, which, in the Norwegian system, implied that it was to be universally offered. It defined quality standards for kindergartens and their staff, as well as a financing framework to ensure that kindergartens were accessible. The introduction of the Kindergarten Act took place after a long public consultation period, and there was wide public support for more available kindergarten spots to replace informal care arrangements.

In 1976, the federal budget for kindergartens increased from NOK 269 (USD 33) million to NOK 661 (USD 82) million, and reached NOK 828 (USD 120) million by 1979. This is equivalent to NOK 4,179 (USD 520) per child aged 3 to 6 in 1979. The allocated budget was intended to increase childcare coverage in the country, which was below 10% prior to the reform. In only three years, coverage increased to 30%, and it continued to grow gradually in the following decades. Focusing on the rapid increase in childcare availability after the reform is useful to isolate the effect of an increase in supply, absent any demand-side adjustments in the long run. Finally, the administrative

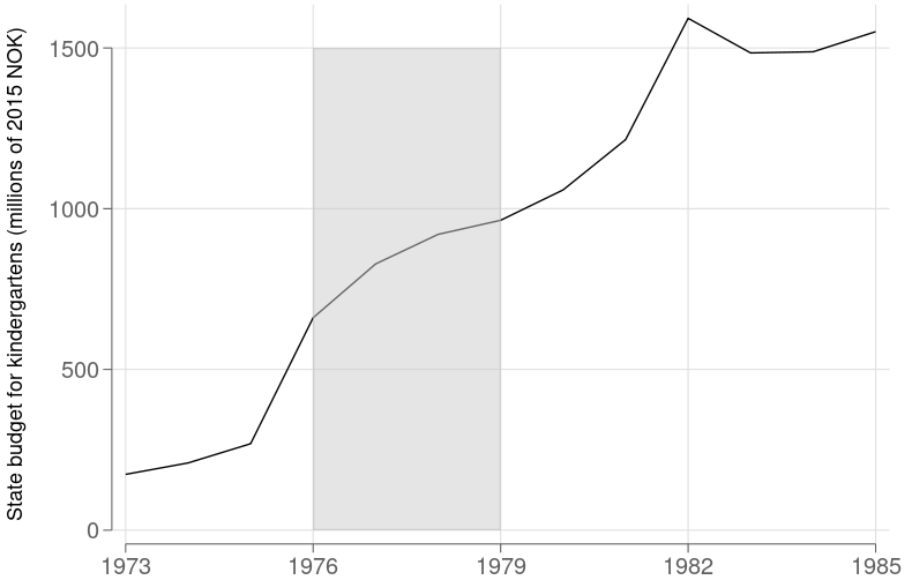
Table 7: Consumption Expenditure: Difference-in-Differences Estimates

Panel A: Disposable income components I			
	Labor income	Government transfers	Capital income
Treatment	-31,665***	1,811*	-2,005
	(1,976)	(786)	(1,570)
Controls	Yes	Yes	Yes
Dep. var. mean	324,033	13,357	-76
N	140,405	140,405	231,874
Panel B: Disposable income components II			
	Other positive transfers	Other negative transfers	Taxes
Treatment	25,931	117**	-8,462***
	(25,166)	(38)	(1,012)
Controls	Yes	Yes	Yes
Dep. var. mean	19,833	2,009	89,992
N	219,750	231,874	231,874
Panel C: Capital gains and wealth components I			
	Capital gains	Other real capital	Securities
Treatment	861	-1,000	-4,589
	(886)	(899)	(14,737)
Controls	Yes	Yes	Yes
Dep. var. mean	3,048	25,565	98,120
N	231,874	232,932	232,932
Panel D: Wealth components II			
	Other financial assets	Liquid assets	Liabilities
Treatment	-4,577	-33,527**	-15,732*
	(2,438)	(11,029)	(7,386)
Controls	Yes	Yes	Yes
Dep. var. mean	18,568	303,067	270,996
N	232,932	232,932	232,932

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the difference-in-differences estimates obtained using the specification from Equation 1. I estimate the average treatment effects of an increase in pension benefits on a selection of individual income and wealth components. All regression models include cohort and year fixed effects. Controls include country of birth, education level, marital status, number of children, household income, municipality of residence, hours worked, partner's age and partner's employment status in 2009. All monetary values are in 2015 Norwegian kroner (NOK).

competence over kindergartens belonged to the municipalities, so federal funding was distributed to the local governments. Importantly, the municipalities with lower pre-reform childcare coverage rates were awarded relatively higher subsidies.

Figure 9: State Budget Allocated to Kindergartens



Notes: This figure shows the total funding allocated to kindergartens in the Norwegian state budget between 1973 and 1985. The state budget for a given year is proposed and voted on by the Parliament late in the year prior. The gray area corresponds to the years used to determine treatment intensity.

4.2 Data and Sample Definition

To study the long-run effects of the reform, I focus on the first group of mothers who were fully exposed to the childcare expansion, that is, those who gave birth in 1973. As a comparison, I build the same panel for mothers who gave birth in 1969. Since I am able to link mothers to their children and partners, I have data on the country of origin, year of birth, education level of both parents, as well as total household size by 1973. I use information on earnings between 1968 and 2023 from the social security registries, and combine data on taxes paid, take-up of benefits, and retirement behavior between 1993 and 2023 to compare the fiscal externalities of the policy and its effect on pension benefits with the corrective example. I am, however, unable to identify whether a given child in the sample attended kindergarten and where. All estimates obtained through the approach outlined in Subsection 4.3 should therefore be interpreted as intention-to-treat effects (ITT).

Among the 64,432 women who gave birth in 1973, 34,685 did not have more children between 1973 and 1979, ensuring that all women in the sample were exposed to the same number of years of subsidized childcare as a result of the policy. After excluding mothers and children who were not alive or residing in Norway at the time of the reform, the sample consists of 33,596 women.

Restricting the sample to women aged 20 to 55 leaves 33,525 observations, of whom 29,934 were born between 1948 and 1953 — the cohorts affected by the retroactive extension of pension accrual for unpaid care work. To avoid single mothers driving potential employment and earnings effects documented in prior literature, the sample is further restricted to married women, resulting in 29,921 women. For comparison, among the 70,460 women who gave birth in 1969, 31,067 did not have additional children between 1969 and 1979; applying the same sample restrictions yields a total of 26,691 women.

Table 8 shows some demographic characteristics of the mothers in the sample, two years after birth. Only 15.54% of mothers who gave birth in 1973 were employed by 1975, and the share of mothers employed is even lower for those who gave birth in 1969. Earnings are substantially higher for women who gave birth in 1973, which is partly explained by the higher share of women working full-time and by an overall higher level of education.

Table 8: Descriptive Statistics for Mothers in 1973 vs. 1969

	Mothers in 1973	Mothers in 1969	Difference
Age	30.03	31.50	1.47***
	[5.23]	[5.91]	(0.05)
Immigrant (%)	3.09	3.32	0.23
	[17.31]	[17.92]	(0.15)
Post-secondary education (%)	13.04	9.76	-3.28***
	[33.67]	[29.68]	(0.27)
Employed (%)	15.54	9.07	-6.47***
	[36.23]	[28.72]	(0.28)
Employed full-time (%)	5.59	3.45	-2.13***
	[22.97]	[18.26]	(0.18)
Earnings (2015 NOK)	44,478	26,069	-18,409***
	[81,754]	[65,285]	(627)
N	29,921	26,691	56,612

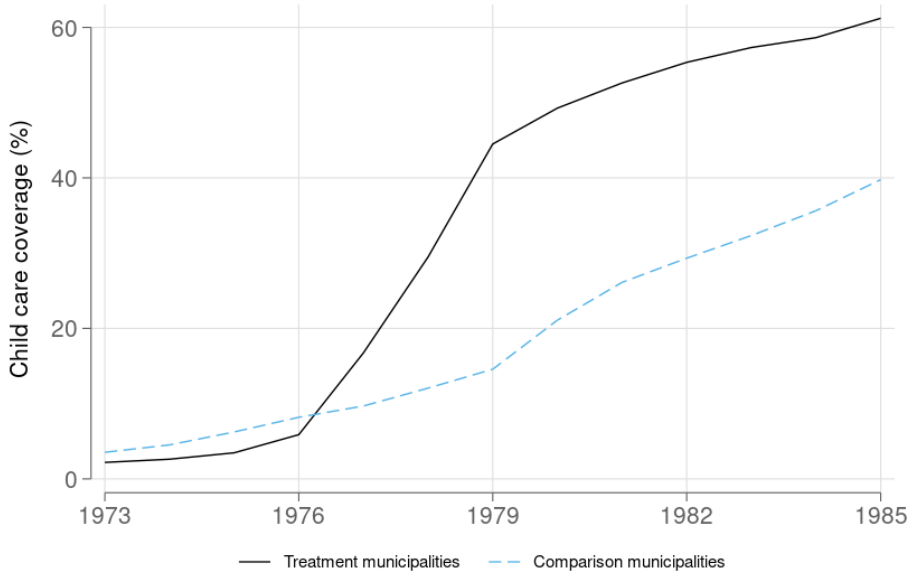
Notes: This table reports demographic characteristics at birth for women who had their youngest child in 1973 vs. 1969. In this case, employment is defined as earning at least twice the basic amount, and full-time employment is defined as earning at least four times the basic amount. Standard deviations in brackets. Standard errors in parentheses. *** $p < 0.001$.

4.3 Identification Strategy

Funding subsidies for kindergartens were allocated differently across municipalities, such that those with pre-reform lower coverage would receive more support. Additionally, municipalities performed differently in turning the subsidies into childcare spots. I follow Havnes and Mogstad (2011) and exploit the variation in the increase in childcare coverage across municipalities to create a treatment and control group, defining as treated those municipalities with a percentage point increase in

childcare coverage between 1976 and 1979 above the median (see Figure 10). I use aggregate data on kindergarten spots and the number of children ages 3 to 6 by municipality from Kommunedatabasen to determine treatment assignment. Importantly, the evolution of childcare coverage rates is similar across groups before the reform, and there is a notable kink in 1976 for treated municipalities.

Figure 10: Increase in Subsidized Childcare



Notes: This figure shows the evolution of childcare coverage in treatment vs. control municipalities. A municipality is assigned to treatment if the increase in childcare coverage between 1976 and 1979 was above the median of all municipalities. Childcare coverage is measured as the number of kindergarten spots available over the total number of children ages 3 to 6.

The main specification is as follows:

$$Y_{it} = \beta_0 + \beta_1 Treated_i \cdot Post_t + \beta_2 Treated_i + \delta_t + \boldsymbol{X_{it}'}\boldsymbol{\gamma} + \varepsilon_{it} \tag{5}$$

where β_1 is the relevant treatment effect. $Treated_i$ is a dummy variable equal to 1 if the mother lives in a treated municipality, and 0 otherwise. λ_c are cohort fixed effects, δ_t are year fixed effects, and $\boldsymbol{X_{it}'}$ includes whether the mother and father were born in Norway, the mother and father's pre-reform education levels, the mother and father's year of birth, the mother's municipality of residence, the number of children before 1973, and male employment in the municipality.

Standard errors are clustered at the municipality level. Since municipality boundaries in Norway have changed over time due to administrative mergers and splits, I construct time-invariant geographical units that serve as municipalities in the analysis for all purposes except determining treatment status, where the boundaries at the time of treatment (1976) are respected.

Note that the main identification strategy in Havnes and Mogstad (2011) compares mothers with

children ages 3 to 6 in 1976 vs. mothers with children ages 3 to 6 in 1979, in treatment vs. control municipalities. In other words, their DiD strategy compares employment for the cross-section of exposed mothers. Here, the identifying assumption is that employment of childcare-eligible mothers would have evolved the same in treatment and control municipalities had the policy never been implemented. On the other hand, I focus on the panel of women whose youngest child was three years old at the time of the reform. Therefore, I compare the same mothers before and after the policy, and follow them over time. As a result, I can redefine the post-period to include all years after 1976 and until retirement. The identifying assumption now is that employment of women who gave birth in 1973 would have evolved the same in treatment and control municipalities had the policy never been introduced.

I retrieve the dynamic treatment effects from

$$Y_{ict} = \beta_0 + (\sum_{t=1968[t\neq 1975]}^{t=2023} \beta_t \cdot Treated_i) + \lambda_c + \delta_t + \boldsymbol{X_{it}'}\boldsymbol{\gamma} + \varepsilon_{it} \tag{6}$$

where the estimates for β_{1968}, β_{1969}, ..., and β_{1974} will allow me to evaluate the presence of pre-trends, and the estimates for β_{1976}, β_{1977}, ..., and β_{2023} are the relevant treatment effects. The set of controls included is the same as for the main specification, and standard errors are clustered at the municipality level.

Table 9 shows a selection of demographic characteristics for the mothers in treatment vs. control municipalities. Women in the treatment group are more educated, participate more in the labor market and work more hours compared to the mothers in the control group. While this is not a problem for identification as long as the evolution of earnings and employment in each group is similar pre-reform, it raises a question of whether the increase in childcare coverage is correlated with pre-reform female employment, local labor market characteristics or other underlying features that may drive any post-reform differences between treatment and control.

Figure 11 shows the correlation between the increase in childcare coverage between 1976 and 1979 (my measure of treatment intensity) and other pre-reform municipality characteristics. It appears that the reform was successful in encouraging municipalities with lower childcare coverage in 1975 to increase the number of kindergarten spots (see Figure 11a). However, I find no clear correlation between the amount of state funding received to cover kindergarten's operating costs and the increase in childcare coverage (see Figure 11c). This is likely due to price differences across municipalities and varying strategies for allocating the additional funding. Importantly, I find no correlation between the average employment of the women with three-year-old children in 1972 - never exposed to the reform - and treatment intensity (see Figure 11b). Note that this does not mean that mother's employment is not constrained by local childcare availability, but that municipalities with higher pre-childbirth female employment are not systematically more or less likely to be assigned to treatment. The weak correlations highlight that the variation in treatment intensity is neither purely

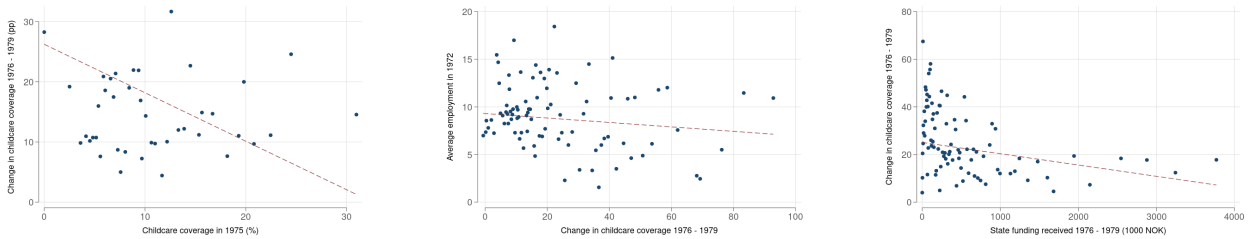
Table 9: Descriptive Statistics for Mothers who Gave Birth in 1973, by Treatment Group

	Treatment	Control	Difference
Age	30.19	29.88	-0.31***
	[5.18]	[5.27]	(0.06)
Immigrant (%)	3.71	2.51	-1.20***
	[18.90]	[15.65]	(0.20)
Post-secondary education (%)	14.75	11.43	-3.32***
	[35.46]	[31.82]	(0.39)
Employed (%)	16.36	14.78	-1.57***
	[36.99]	[35.49]	(0.42)
Employed full-time (%)	6.29	4.93	-1.35***
	[24.27]	[21.66]	(0.27)
Earnings (2015 NOK)	47,292	41,840	-5,451***
	[85,244]	[78,252]	(945)
N	14,478	15,443	29,921

Notes: This table reports a selection of pre-reform demographic characteristics for women who had their youngest child in 1973 in treatment vs. control municipalities. A municipality is assigned to treatment if the increase in childcare coverage between 1976 and 1979 was above the median of all municipalities. A mother is assigned to treatment or control based on her municipality of residence in 1976. Standard deviations in brackets. Standard errors in parentheses. *** $p < 0.001$.

mechanical nor driven by fast-growing or more efficient municipalities. Table D.2 in Appendix D compares treatment and control municipalities along a number of dimensions unrelated to childcare and shows that there are no substantial differences between the groups.

Figure 11: Selection Into Treatment



(a) Childcare coverage (b) Female employment (c) State funding received

Notes: This figure shows the relation between treatment intensity and pre-reform municipality characteristics: (a) childcare coverage on the year the reform was implemented, (b) the employment rate of treated mothers before giving birth (1972), and (c) total grants received from the state to operate kindergartens. Note that there is no direct correspondence between the funding received and the increase in childcare coverage, as the cost of a new childcare spot and how funding is allocated vary across municipalities.

To study the impact of the policy on long-term career outcomes and retirement behavior, I need to deviate from Havnes and Mogstad (2011) and incorporate an additional comparison. I compare mothers who gave birth in 1973 to mothers who gave birth in 1969 in treatment vs. control municipalities. The latter had a child who was of school age by the time the reform was introduced, and therefore did not directly benefit from it regardless of whether they lived in a treatment or control municipality at that time. Note that there is universal access to primary school in Norway, so women move from an environment with restricted childcare availability to one with full coverage once their child turns 7; this may have an impact on their careers in and of itself. To avoid these mothers being exposed to the policy later on by having another child, I restrict my comparison group to women with completed fertility by 1969. Table D.1 in Appendix D compares mothers who gave birth in 1969 in treatment and control municipalities, and shows that they are different in very similar ways compared to the 1973 group. In this case, the main outcomes of interest are accumulated earnings during working life (ages 18 to 62), total taxes paid between 1993 and 2023, total transfers received excluding pension benefits, annual pension benefits at the time of retirement, and their claiming age. The specification is now:

$$Y_{ic} = \beta_0 + \beta_1 Treated_{ic} \cdot Exposed_{ic} + \beta_2 Treated_{ic} + \beta_3 Exposed_{ic} + \lambda_c + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_{it} \tag{7}$$

where $Exposed_i$ is a dummy variable equal to 1 if the mother gave birth in 1973, and 0 otherwise. The set of controls is the same as in the main specification, and standard errors are clustered at the municipality level.

Because of the 2011 pension reform, the age at which an individual is eligible to retire and how their pension benefits are calculated depend on the mother's year of birth. To isolate the effect of the childcare expansion, it is therefore necessary to include cohort fixed effects. This implies that the comparison is between mothers born in the same year who gave birth in 1969 vs. 1973 in treatment vs. control municipalities. While younger mothers may have a very different earnings trajectory compared to those who gave birth later in life, it is the difference across municipalities that identifies the effect of the policy.

This approach can be transformed into a triple-difference specification, centered around the time of birth, to compare the evolution of earnings and employment across groups. This additional comparison is helpful if career interruptions have different long-run consequences across municipalities. The estimating equation is

$$\begin{aligned} Y_{ict} =& \alpha_0 + \Big(\sum_{t=-1[t\neq 2]}^{t=40} \beta_t \cdot Treated_{ic} \cdot Exposed_{ic}\Big) + \alpha_1 Treated_{ic} + \alpha_2 Exposed_{ic} + \\ & \alpha_3 Treated_{ic} \cdot Exposed_{ic} + \sum_{t=-1}^{t=40} \zeta_t \cdot Treated_{ic} + \sum_{t=-1}^{t=40} \iota_t \cdot Exposed_{ic} + \\ & \lambda_c + \delta_t + \boldsymbol{X_i'}\boldsymbol{\gamma} + \varepsilon_{it} \end{aligned} \tag{8}$$

where the relevant treatment effects are the estimates for β_3, β_4, ..., and β_{40}. The set of controls is the same as in the main specification, and standard errors are clustered at the municipality level.

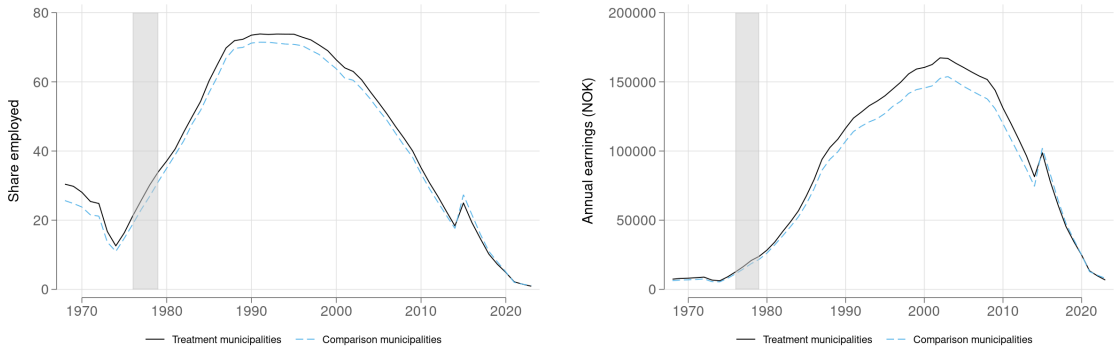
It is important to note that this specification relies on the absence of a treatment effect for mothers who could not directly benefit from the childcare extension. However, it is possible that the childcare expansion generated employment opportunities for women in treated municipalities. Figure D.1 in Appendix D shows that, indeed, total kindergarten staff increased more in treatment municipalities relative to control. However, the total increase in employment is moderately small, at only around 5,000 additional employees between 1976 and 1979. Alternatively, other women who were providing informal care to compensate for the lack of kindergarten spots could now switch to formal, better-paid care jobs. This mechanism is unlikely to be at play for women similar in age who have a child three years older.

4.4 Results

I begin by presenting descriptive evidence on the evolution of employment and earnings for mothers exposed to the 1975 reform and up to retirement (see Figure 12). Employment trends are very similar in control and treatment municipalities, and the small initial gap in employment closes by the time these mothers reach retirement. On the other hand, the gap in earnings between treatment and control widens very little in the three years immediately following the reform, but it continues

to grow. By the time treated mothers reach their peak earnings, the earnings difference between treatment and control municipalities is substantially larger.

Figure 12: Long-run Evolution of Main Outcomes



(a) Employment

(b) Earnings

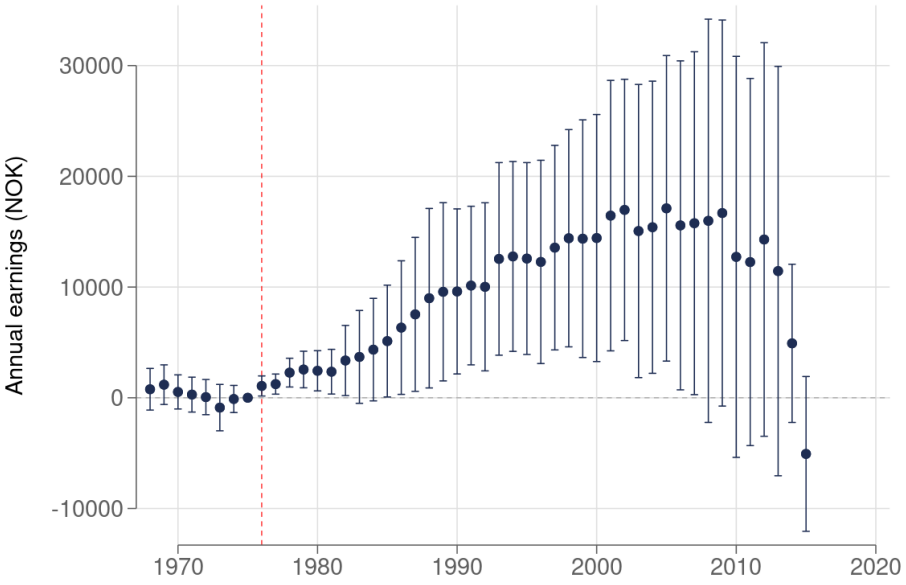
Notes: This figure shows the labor market outcomes over time of mothers who gave birth in 1973 in treatment vs. control municipalities. The gray area corresponds to 1976 - 1979, the years used to determine treatment intensity. These correspond to the years when their child would be in kindergarten. Employment is defined as earning above twice the basic amount. Note that the average mother in the sample is around 30 years old in 1973, and is therefore eligible to retire from 2010 approximately.

Figure 13 is the event-study transformation of the main identification strategy in Havnes and Mogstad (2011), using all periods between 1968 and 2013, when most individuals in the sample reach retirement age. The total effect on earnings amounts to NOK 388,858 (USD 48,374), which is roughly equivalent to having worked one more year at the time of retirement. For the mothers born between 1948 and 1953, the sample of interest, there is a NOK 467,658 (USD 58,177) increase in earnings.

This effect is still small relative to the cost of the policy, and does not change the conclusion from the original paper that subsidized childcare cannot be the main driver of the sharp increase in female employment that took place in Norway in the 1980s. However, it is large enough to translate into an increase in pension benefits, especially considering the weight that the pension system assigns to the years with highest earnings when determining the final pension points.

Table 10 reports average DiD estimates for a selection of outcomes. I estimate the effect of the policy on employment and earnings in 1979 compared to 1976, similar to Havnes and Mogstad (2011), in Panel A. My results are close in magnitude and direction to theirs. Next, I estimate the effect of the policy on employment and earnings using all pre- and post-periods between 1968 and 2023. Here is when the cumulative effect on long-run earnings is visible: living in a municipality with a higher increase in childcare coverage raised mothers' annual earnings by NOK 11,142 (USD 1,386). This positive effect on earnings does not translate to employment in the extensive margin.

Figure 13: Impact of the Policy on Earnings



Notes: This figure shows the event-study plot from the specification in Equation (6). I estimate the average treatment effect of expanding public childcare on earnings, comparing women from municipalities above to those below the median increase in childcare coverage between 1976 and 1979.

A persistent increase in annual earnings, which shows both in the event study and in the DiD estimates for lifetime earnings, would translate into an increase in the total amount of taxes paid, a positive fiscal externality of the policy. Compared to women who gave birth in 1969, women who gave birth in 1973 in treatment municipalities paid NOK 52,234 (USD 6,498) more taxes during their working life. Equivalently, higher earnings and attachment to the labor force could lead to lower government transfers received, especially later in life. I find a negative but insignificant effect on total transfers received between 1993 and retirement, the period for which data are available. Finally, to narrow the gender pension gap, the increase in lifetime earnings should result in higher pension benefits upon retirement. However, I find no significant increase along this margin. A potential reason for this is that the average pension benefits in the sample are very close to the minimum pension level. If lower-income women benefit most from the expansion of subsidized childcare, but the increase in earnings is not sufficient to push them above the minimum pension, there will be no effect on final benefits despite an increase in pension accrual.

Table 10: Main Results: Difference-in-Differences Estimates

Panel A: Labor market outcomes in 1976 vs. 1979				
	Employment		Annual earnings	
Treatment	0.65	0.84	935*	961
	(0.52)	(0.69)	(462)	(628)
Controls	No	Yes	No	Yes
Cohort FE	No	Yes	No	Yes
Municipality FE	No	Yes	No	Yes
Dep. var. mean	26.35	26.34	17,372	17,364
N	59,583	59,513	59,583	59,513
Panel B: Labor market outcomes (all periods)				
	Employment		Annual earnings	
Treatment	-0.59	-0.40	10,267***	11,142**
	(1.30)	(1.06)	(3,034)	(3,401)
Controls	No	Yes	No	Yes
Cohort FE	No	Yes	No	Yes
Municipality FE	No	Yes	No	Yes
Dep. var. mean	50.73	50.71	95,522	94,657
N	1,298,402	1,288,418	1,298,402	1,288,418
Panel C: Fiscal externalities				
	Taxes paid		Government transfers (excl. pensions)	
Treatment	44,709	52,234***	-28,688*	-20,063
	(27,044)	(14,719)	(12,415)	(12,179)
Controls	No	Yes	No	Yes
Cohort FE	No	Yes	No	Yes
Municipality FE	No	Yes	No	Yes
Dep. var. mean	1,485,172	1,484,947	510,454	509,881
N	54,431	54,360	54,431	54,360
Panel D: Retirement benefits and timing				
	Lifetime earnings (ages 18 to 62)		Pension benefits	
Treatment	82,851	107,770*	1,725	1,624
	(84,745)	(46,327)	(2,392)	(1,763)
Controls	No	Yes	No	Yes
Cohort FE	No	Yes	No	Yes
Municipality FE	No	Yes	No	Yes
Dep. var. mean	5,146,895	5,146,347	195,050	195,040
N	54,431	54,360	48,828	48,777

Notes: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the difference-in-differences estimates obtained using the specification from Equations (5) and (7). I estimate the average treatment effect of extending public childcare on mothers' labor market outcomes, taxes paid and transfers received, and retirement benefits. Panel (A) corresponds to the estimates I obtain applying the identification strategy from Havnes and Mogstad (2011), whereas Panel (B) reports the estimates from using all available pre- and post-periods.

5 Welfare Analysis

This section evaluates both policies from a fiscal and welfare perspective. The goal is to understand how effective each policy is in reducing the gender pension gap. First, I quantify the total cost per beneficiary. This includes the mechanical cost of the policy and any behavioral response that generates fiscal externalities. For my analysis, I consider the impact of the policy on taxes paid by the beneficiaries, together with additional public spending in the form of government transfers.

The corrective policy has a direct cost of NOK 72,684 (USD 9,042) over the period 2010 - 2021. While I adjust the mechanical cost to fit the time frame for which I can observe the behavioral response, the expected total payments are substantially higher, given that most women in the sample are expected to live at least ten more years. In addition, the small anticipation of pension claiming requires an extra payment of NOK 43,094 (USD 5,361) per beneficiary. While the employment response per se does not constitute a cost to the government, the corresponding decrease in taxes paid of NOK 8,462 (USD 1,053) is a negative fiscal externality. Finally, the increase in take-up of disability insurance represents an additional cost of NOK 1,357 (USD 169) per beneficiary. Summing up these components gives a total of NOK 125,597 (USD 15,624) per beneficiary.

Over 40% of the total cost to the government can be attributed to the behavioral response. This highlights that women around retirement age are a sensitive population group, reacting even to relatively small increases in pension wealth. It is important to consider whether these estimates could be generalized to the whole female population. Given that women in my sample are more attached to the labor market than the average Norwegian woman of the same age, the costs to the government of a transition from work to retirement are higher. At the same time, women in the general population may be more willing to leave the labor force when facing an increase in pension benefits. These channels go in opposite directions and cancel each other to some extent.

For the preventive policy, a mother who gave birth in 1973 is exposed to the childcare spending in 1976, 1977 and 1978 - a total of NOK 2,409 (USD 300) million. This budget covers a total of around 76,000 kids aged 3 to 6 in each year who get a kindergarten spot. Therefore, the government spends a total of NOK 32,339 (USD 4,023) per child born in 1973, assuming equal spending by age of the child. Since women in my sample have an average of 2.8 kids, full exposure to childcare would translate into NOK 90,549 (USD 11,264), using the same budget values as in 1976 - 1979. The childcare budget continued to increase after the reform, so these cost values are to be interpreted as a lower bound. This implies that, in principle, the mechanical cost of the preventive policy is larger than the corrective example. In terms of the behavioral policy, I find that the increase in earnings leads to a positive fiscal externality of NOK 52,234 (USD 6,498) in taxes paid. There is no significant effect of the policy on transfers received, nor on the retirement margin. Summing up these components gives a total cost of NOK 38,315 (USD 4,766) per beneficiary. The behavioral response substantially reduces the *a priori* large cost of the policy.

Next, I compute the Marginal Value of Public Funds (MVPF), defined as the ratio of the beneficiaries' willingness to pay (WTP) for the policy to the net fiscal cost to the government. For the corrective policy, as it is a pure wealth shock, I start by assuming that beneficiaries value the effects of the policy at exactly the amount received. Under this assumption, WTP equals the direct cost of the policy, and the implied MVPF is 0.58: the policy generates 0.58 NOK in direct benefits to the recipient per NOK of fiscal cost. This value is lower compared to what Artmann et al. (2024) finds, suggesting that behavioral responses are larger for older beneficiaries at the time of treatment. Importantly, there is a substantial cost attached to a small retirement response, which is largely responsible for the cost-benefit gap in the corrective policy. However, if anticipating pension claiming shows a revealed preference for leisure, and the policy is thus allowing beneficiaries to adjust their labor supply in line with their preferences, the true WTP for the policy would exceed the direct cost. The fact that the employment response is larger for married women, and within that group for women whose partner is retired, supports the leisure channel.

For the preventive policy, I focus on the effect of the childcare expansion on mothers' earnings. Silliman and Mäkinen (2026) finds statistically and economically insignificant effects of a similar reform on children in Finland, and thus calculate the WTP based only on parental outcomes. In this case, I am interested in evaluating the efficiency of each policy in improving women's retirement outcomes, so I abstract from any potential effects on the children. I start by assuming that mothers value the increase in lifetime earnings one-to-one and discount them at a 1.96% rate. This yields a total WTP of NOK 79,447 (USD 9,983) and implies an MVPF of 2.07, which means that the policy pays for itself. Note that, from the event study plot in Figure 13, the increase in earnings is not even over time and is largest when the beneficiaries are past age 50. As a result, the preventive policy only started "paying for itself" over 10 years after it was introduced.

However, I find no significant effect of the childcare expansion on retirement benefits despite the increase in earnings. Assuming no or a smaller effect of the policy on father's earnings, the expansion of subsidized childcare is thus an example of preventive policy that succeeds in reducing the *cumulative* gender gap in earnings, but does not change the gender pension gap. Going back to the four components that determine pension benefits in the Norwegian system can be useful to rationalize why higher earnings do not translate into higher pension benefits. First, I find no significant effect of the policy on employment in the extensive margin. This implies that there is no significant difference in the number of contribution years between treated and control mothers. Second, I find an increase in annual earnings that is not evenly spread over the life cycle. For the policy to have an effect on final pension points, the increase in earnings should be concentrated in the years ranked within the top 20, which also means that only part of the lifetime earnings increase, if any, would enter the final pension points calculations. Despite the fact that the earnings increase is highest in the top 20 years, it may not have been large enough to significantly raise pension points. Third, I find no significant effect of the policy on the claiming age of treated mothers. On the one hand, if the effect on earnings reflects a higher attachment to the labor force, then treated mothers could

choose to continue working past retirement eligibility age. On the other hand, to the extent to which higher income translates to higher pension accrual, this could induce treated mothers to retire earlier. The absence of any detectable response suggests that, while the policy affected earnings trajectories, the effect was not large enough to substantially shift retirement incentives.

Next steps include extending the analysis of both policies to identify which groups of women benefited the most and if the childcare expansion has an effect on retirement benefits for any of them. Using pension points instead of earnings as an outcome can shed light on whether the null effect is the result of how pension benefits are calculated. In addition, if there is an increase in savings in response to the increase in lifetime earnings, this can be an alternative channel through which subsidizing childcare can improve old-age outcomes for women. I can provide empirical evidence on this mechanism by extending the analysis to post-retirement income and wealth. Finally, the fact that part of the 1948-1953 cohorts gave birth in 1973 allows me to study the interaction of both policies. Since the earnings effect is very small for the five years after birth, it is unlikely to shift women away from receiving childcare-related pension credits. However, higher future earnings may push the years with policy coverage down in the ranking, hence reducing the relative impact of pension credits on total benefits. From the policymaker's perspective, this implies that investing in a preventive policy reduces the direct cost of a corrective policy. Since I only find the corrective policy to reduce the gender pension gap, the preventive policy could then indirectly *widen* the gender pension gap for the women treated by both.

6 Conclusion

This paper studies two policy approaches to reduce the gender pension gap: correcting existing gaps through childcare-related pension credits and preventing future gaps through subsidized childcare. Using Norwegian administrative data, I compare the long-run effects of a large childcare expansion in Norway with the retroactive extension of childcare-related pension credits, both policies with partly overlapping treatment groups. I provide evidence on how each shapes earnings and pension benefits upon retirement, as well as their fiscal externalities. My results highlight how the timing of policy interventions over the life cycle determines fiscal costs, even when addressing the same goal.

The corrective reform mechanically reduces the gender gap in lifetime pension benefits by 0.48 pp, a 2.8% reduction at an aggregate cost of NOK 1.05 billion (USD 131 million). Although behavioral responses to the pension wealth shock are modest, adjustments in labor supply, claiming behavior, and benefit take-up increase the total cost of the policy beyond its mechanical effect. For an average increase in annual pension benefits of NOK 6,296 (USD 881), the beneficiaries anticipate pension claiming by 1.8 months. Since the retirement decision is one of the very few margins of adjustment for the women in my sample, the negative effects on employment and take-up of benefits are driven by the change in pension claiming behavior. My findings emphasize that women are responsive to even small changes in pension wealth when close to retirement. Nevertheless, I do not find any

evidence of higher income translating to other non-monetary outcomes, including marriage stability.

In contrast, the preventive policy generated a persistent earnings increase for treated mothers that accumulates over their working life. The childcare expansion led to positive fiscal externalities through higher tax revenue, which reduced the net cost to the government to less than half the original spending. The policy thus becomes self-financing around 10 years after being introduced. However, I find no change in retirement behavior, nor an increase in pension benefits upon retirement. This suggests that the earnings effect was muted once it entered the benefit calculation formula and points to the importance of pension system design in reducing, perpetuating, or magnifying gender differences in employment and earnings during working life.

Taken together, the comparison highlights a fundamental trade-off between corrective and preventive approaches to gender inequality. Corrective policies can deliver immediate reductions in pension gaps for older cohorts, while preventive policies generate broader earnings gains and are more cost-effective, but may not change behavior enough to bridge the gap in retirement income.

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