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Gender Equality Bargaining: Evidence from Finland

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Gender Equality Bargaining: Evidence from Finland*

Abstract

This paper studies whether collective bargaining can reduce gender inequality through targeted wage increases. We examine the Finnish equality allowance, a small contractual wage component corresponding to a wage increase of up to 0.5 percent, introduced in the 2005 comprehensive agreement and implemented in collective agreements in 2006. Using worker-level earnings data linked to collective agreements, we estimate its effects with difference-in-differences methods robust to heterogeneous treatment effects. Equality allowances were more commonly applied in more female-dominated sectors and occupations. Our preferred analysis focuses on the private-sector agreements, where some agreements introduced an equality allowance and others did not. We find no statistically significant effects on hourly earnings, working hours, or employer mobility. However, subgroup analyses provide some evidence of reduced working hours among low-wage women. The findings suggest that a small equality-oriented wage component was insufficient to produce detectable wage gains for women or narrow the gender wage gap, with point estimates consistent with partial offsetting through other components of the contractual settlement.

JEL classification

J52, J16, J31

Keywords

collective bargaining, gender wage gap, equality allowance, targeted wage increases, Finland

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1 Introduction

The gender wage gap has narrowed only gradually in advanced economies, raising the question of whether collective bargaining can play a more active role in reducing gender pay inequality (see, e.g., [Blau and Kahn, 2017](#)). Centralized collective bargaining is widely recognized as an effective tool for reducing the gender wage gap, primarily through wage compression. [Blau and Kahn \(2003\)](#) show that centralized wage-setting institutions and strong union density lift the wage floor, which disproportionately benefits women who have historically been over-represented in the lower half of the wage distribution. Furthermore, strict collective agreements have been shown to be successful at substantially reducing the within-job wage gap, ensuring similar pay for the same roles within firms ([Milgrom et al., 2001](#)). This structured approach may protect female workers from the penalties associated with decentralized, individual negotiations, where empirical evidence shows that women may capture a significantly smaller fraction of firm-specific rents compared to their male counterparts ([Card et al., 2016](#)).

However, the modern Nordic labor market reveals a structural paradox where the very collective bargaining mechanisms that compress wages also act as a ceiling preventing true gender wage parity. The persistent, remaining wage gap in these egalitarian countries is largely driven by severe occupational segregation intersecting with the institutional rigidity of "pattern bargaining" ([Calmfors, 2025](#)). To protect national economic competitiveness, the male-dominated, export-driven manufacturing sector negotiates first, establishing a strict normative wage ceiling that female-dominated sectors cannot exceed. Consequently, while the Nordic model provides a robust wage floor that reduces inequality, its reliance on pattern bargaining inadvertently locks historical pay disparities into place across gendered sectors, making it difficult for female-dominated professions to negotiate the catch-up wage growth necessary to close the gap entirely.

One possible solution for this challenge in centralized bargaining is gender equality bargaining ([Williamson and Baird, 2014](#)): the use of collective bargaining to pursue explicit equality objectives through contractual provisions, including targeted wage components. Such components may take the form of equality allowances, special wage pots for female-dominated sectors, or minimum increases directed at low-paid groups whose relative pay has lagged behind.

However, recent evidence points to several constraints on the effectiveness of equality bargaining. In France, equality-oriented bargaining has often generated substantial formal compliance without much substantive change, highlighting the importance of enforcement, information, and bargaining capacity ([Milner et al., 2019](#)). Qualitative evidence from Finland suggests that coordinated labour market institutions tend to preserve existing relative wages, which may limit the scope for targeted pay adjustments ([Koskinen Sandberg and Saari, 2019](#); [Saari et al., 2021](#)). More broadly, industrial-relations research emphasizes that when wage-setting is shaped by competitiveness concerns and implementation is decentralized, negotiated provisions may be challenging to implement in practice ([Colling and Dickens, 1998](#)), while equality concerns may remain peripheral within broader bargaining agendas ([Kirtton, 2021](#)). Against this background, whether targeted wage components can generate measurable improvements in gender inequality remains an open empirical question.

Despite the policy relevance of such instruments, there is limited causal evidence on the effects of gender-equality-oriented bargaining components. In Sweden, the Gender Equality

Pot (GEP) was introduced in the 2007 bargaining round to raise wages in female-dominated sectors, but proved contentious and has not been widely used since (Erikson, 2021). Eliasson and Nordström Skans (2014) find that wage increases for low-paid, female-dominated groups in the Swedish public sector led to persistent wage gains, alongside adjustments in separations, hiring, and working hours. Bustos (2024) reports similar persistent earnings gains for assistant nurses following a targeted wage increase in Sweden, with little evidence of effects on separations, hours, or benefit take-up. More broadly, Olsson and Skans (2024) show that gender pay gaps are smaller in agreements that guarantee minimum annual pay increases, highlighting the role of bargaining institutions in shaping within-job inequality.

In this paper, we evaluate the impact of an equality allowance in Finnish private-sector and municipal collective agreements in 2006. The allowance constituted a wage increase of 0–0.5 percent and was included in some, but not all, private-sector agreements and in all municipal-sector agreements. We combine worker-level earnings data from the Finnish Harmonized Structure of Earnings with detailed information on collective agreements and estimate effects using the DID_M estimator of De Chaisemartin and d’Haultfoeuille (2020), which is robust to treatment effect heterogeneity across groups and over time. Our identification exploits variation across agreements in the incidence of equality allowances, and we assess the plausibility of the identifying assumptions using placebo tests. Descriptive evidence shows that equality allowances were disproportionately applied in more female-dominated sectors and occupations, particularly clerical and service work. However, we find no statistically significant effects on total or regular hourly earnings, working hours, or employer mobility in the full private-sector sample or in analyses conducted separately for women and men. We do, however, find some evidence of a modest reduction in working hours among low-wage women, while we find no statistically significant effects on their earnings or employer mobility. Placebo estimates provide no indication of strong pre-trends. Taken together, the results suggest that this relatively small targeted bargaining component did not generate detectable short-run effects on hourly earnings, although some estimates point to modest reductions in working hours.

This paper contributes to the literature in two respects. First, while the closest related evidence on targeted equality-oriented wage increases comes from the Swedish public sector (Eliasson and Nordström Skans, 2014; Bustos, 2024), we provide evidence from the Finnish private and public sectors, where bargaining structures, implementation mechanisms, and employer responses differ in important ways. Second, by using difference-in-differences estimators that are robust to treatment heterogeneity and dynamic effects (De Chaisemartin and d’Haultfoeuille, 2020; De Chaisemartin and d’Haultfoeuille, 2024), we provide a more credible assessment of whether a negotiated equality-oriented wage component translated into measurable changes in worker outcomes. More broadly, the paper contributes to debates in industrial relations on the scope for collective bargaining to advance gender equality, showing that the effects of such provisions depend not only on their formal inclusion in agreements but also on the institutional conditions through which they are implemented.

2 Literature

A growing literature examines how collective bargaining shapes wage inequality and gender pay gaps, but the implications of explicitly equality-oriented bargaining provisions remain less well understood. Research on labour-market institutions shows that wage-setting arrangements can compress wage distributions and alter wage growth across firms and sectors, with important implications for gender inequality where women are over-represented in lower-paid jobs or occupations (DiNardo et al., 1996; Dustmann et al., 2022). Related work further shows that wage differences across firms and establishments account for a substantial share of overall wage inequality and gender wage gaps, suggesting that bargaining institutions may matter not only through wage floors but also through their effects on sorting, pay premia and wage dispersion (Card et al., 2013, 2016; Barth et al., 2016). Evidence on minimum wages likewise indicates that institutional wage floors can raise earnings at the bottom of the wage distribution with limited employment effects, reinforcing the broader view that wage-setting institutions can shape distributive outcomes in consequential ways (Cengiz et al., 2019).

This paper relates most directly to the literature on gender equality bargaining, which considers how collective bargaining can be used to advance workplace gender equality through targeted provisions and gender-aware bargaining strategies more broadly (Williamson and Baird, 2014). The central premise of this literature is that negotiated wage-setting may provide a route to correcting gendered pay inequalities that decentralized market processes have failed to eliminate. The closest causal evidence comes from Sweden. Eliasson and Nordström Skans (2014) study a Swedish municipal-sector agreement from the 2007 bargaining round, where establishment-level wage increases were tied to the share of low-paid women, and find persistent wage effects together with adjustments in hiring, hours, and worker retention. Using population-wide administrative data on public-sector workers (2003–2011), they show that these effects persisted for at least four years and were accompanied by reduced hiring and lower separations among relatively high-productivity (younger, higher-grade) workers. Recent evidence by Bustos (2024) examines a later Swedish local-government agreement (2016–2018) that introduced targeted wage increases for assistant nurses, a predominantly female, low-wage occupation. Using administrative data and a difference-in-differences design comparing assistant nurses to similar care workers, the paper finds persistent earnings gains of around 2–3 percent over several years. In a related vein, Olsson and Skans (2024) show that bargaining rules guaranteeing minimum annual pay increases are associated with smaller within-job gender pay gaps among Swedish blue-collar workers, underscoring the importance of institutional design within collective bargaining itself.

At the same time, a parallel literature emphasizes the limits of equality-oriented bargaining in coordinated wage-setting systems. In France, equality bargaining has often generated substantial formal compliance without corresponding substantive change, in part because workplace agreements frequently lack systematic measurement and monitorable implementation mechanisms (Milner et al., 2019). Qualitative research from Finland points to a different but related constraint: in coordinated labour markets, bargaining institutions may preserve existing wage relativities across occupations even where equality goals are formally acknowledged. Koskinen Sandberg and Saari (2019) and Saari et al. (2021), for example, argue that corporatist

wage-setting can absorb or dilute attempts to improve the relative pay of female-dominated groups. More broadly, industrial-relations scholarship has long stressed that equality provisions may remain secondary to competitiveness concerns and that their implementation is mediated by bargaining structures that limit redistributive change ([Colling and Dickens, 1998](#); [Kirton, 2021](#)).

3 Institutional Background

3.1 Collective bargaining in Finland

Finland has a long tradition of centralized bargaining in its labour market (see, e.g. [Andersen et al., 2015](#)). Between 1968 and 2007, industrial relations were defined by tripartite, centralised collective agreements, known as incomes policy or TUPO. Central organisations would negotiate these agreements first, followed by sectoral bodies, which could choose to adopt or reject the centrally negotiated contract. Thus, although central organisations negotiated the overall framework, binding contracts were signed at the sectoral level between employers' federations and trade unions. Government tax and social policies often depended on whether these agreements were widely adopted. As a result of centralized bargaining, wage increases tended to be similar across most sectors, though occasional sector-specific negotiations occurred when some industries opted out of central agreements to bargain independently. Contracts typically lasted about two years.

Collective bargaining is highly influential in Finland's labour market because of its broad coverage (around 90%), frequent extension to non-signatories, and scope ([Jonker-Hoffrén, 2019](#)). Each employee's applicable contract is set by their employer's federation or, if not affiliated, by industry through extension. This means that employers have limited flexibility in selecting contracts.

Collective agreements govern matters such as wages, working hours, holidays, social benefits, and parental leave ([Jonker-Hoffrén, 2019](#)). Different employment groups—blue-collar, white-collar, and sometimes upper-white-collar—have distinct contracts. Blue-collar workers receive hourly pay determined by time, piece rates, or reward rates, with supplements like shift allowances also playing a role. White-collar workers are paid monthly. Both groups may get performance-related pay, though this forms only a small part of earnings, and bonuses average about 5% for blue-collar and 8% for white-collar employees ([Kauhanen and Napari, 2012](#)).

In Finnish collective agreements, wage adjustments have historically been built around three principal components: a general increase, a sectoral allowance, and an equality-oriented component targeted at low-paid workers and, later, gender equality (for more details, see [Asplund, 2007](#)). The general increase has been the central element of negotiated pay settlements, typically accounting for the bulk of contractual wage growth, while the sectoral allowance has provided scope for branch-specific adjustments, including revisions to pay scales and, increasingly, locally negotiated changes in remuneration systems. Alongside these, collective agreements have often incorporated a low-pay supplement that later evolved into a broader equality allowance combining low-pay and gender-equality objectives in 1989, with implementation partly negotiable at sectoral or company level. The gender equality objective was proposed by a female-dominated

central organization for white-collar labor unions (Martikainen, 2000). In practice, actual wage increases often exceed those stipulated in contracts, a phenomenon termed wage drift. Wage drift is due to structural changes, market-based wage increases, variation in incentive pay, and for blue-collar workers, e.g., changes in the structure of hours (for example, shift allowances may vary over the business cycle as may the share of piece-rate or reward-rate hours). Wage drift has accounted for roughly one third of actual wage increases in Finland during our observation period (Asplund, 2007).

3.2 Equality allowance

Our interest focuses on the equality allowance, which emerged when the earlier low-pay bonus and a newer gender-equality bonus were merged into a single contractual pay component (Asplund, 2007). In practice, sector-specific low-pay and gender-equality amounts were calculated and combined into an equality allowance whose average size was agreed in the comprehensive settlement, while its allocation was negotiated at sectoral level. Although the equality allowance was introduced through collective bargaining, it has remained controversial in Finland. According to Martikainen (2000), critics have pointed to its limited size, its equal distribution within sectors rather than its targeting toward women, and the possibility that it may weaken women's wage demands. Despite these criticisms, the allowance has been considered symbolically important because it keeps gender equality on the labor market policy agenda (Martikainen, 2000).

In the 2005 TUPO agreement, which governed wage increases in 2005-2007, the part concerning the calculation of the equality allowance read as follows: "The relative share of women among all wage earners covered by the collective agreement is multiplied by 0.45. To the figure thus obtained is added the relative share—among all wage earners in the sector—of those earning less than €10.67 per hour or €1,782 per month, multiplied by 0.15." This means that the maximum equality allowance is $0.45+0.15=0.6$. In practice, it varied between 0 and 0.5.

The 2005 TUPO agreement continues in the following way about the use of the equality allowance: "When agreeing on the use of the equality allowance, attention must be paid to the criteria on which the allowance is determined. With this allowance, the contracting parties aim to improve the pay position of women whose wages are not in line with the demands of the work and their level of education, and to correct the sector's relative low pay." This language urges sectoral parties to allocate allowances that raise wages more for female-dominated or low-pay sectors, and specifically benefit women in those areas.

To give a concrete example of the different wage increase components, for the year 2006 the TUPO agreement stipulated the following increases: general increase 1.4%, sectoral allowance 0.4%, and equality allowance, which varied between 0 and 0.5%. Although the equality allowance was intended to raise wages in female-dominated and low-paid sectors, its effect on observed earnings is not obvious. Because the allowance constituted only one component of collectively negotiated pay increases, its impact may have been attenuated if employers offset it through lower wage drift or smaller locally negotiated increases. In addition, higher negotiated wages may have affected other adjustment margins, such as working hours, hiring, and worker retention. For example, employers may have responded to higher labor costs by reducing working hours rather than employment, although improved retention and reduced turnover could

also have increased hours worked among incumbent employees. Previous evidence suggests that targeted collectively bargained wage increases can affect both wages and other labor-market outcomes (Eliasson and Nordström Skans, 2014; Bustos, 2024). Consequently, the effects of the equality allowance may have extended beyond wages alone, motivating our examination of both hourly earnings and working hours.

4 Data

4.1 Data sources and sample construction

We use two data sources for the analysis, primarily the Harmonized Structure of Earnings (HSES) data from Statistics Finland. This data features consistent wage measures and variable classifications, enabling panel analysis across years and sectors. Harmonization is crucial as it accounts for changes in collective agreements and compensation components. The dataset also includes individual and employer identifiers, allowing us to track them over time.

For the private sector, the HSES data are drawn from payroll records of employer federation member firms and supplemented with surveys for other firms by Statistics Finland. Available annually since 1995, our study uses data from 2003–2010, covering 55%–75% of employees based on year and industry. Coverage excludes firms with fewer than five employees, certain industries (agriculture, forestry, fisheries), household and international organization employers, top management, owners, their relatives, and employees whose contracts started or ended during data collection. Small, unorganized firms are also only partially represented. For the public sector, the coverage of the HSES is 100%.

The dependent variables in the analyses come from the HSES. We have four dependent variables: two earnings measures, hours worked and employer change. HSES data report earnings from either the fourth quarter or October, depending on industry and employee group. Two earnings measures are included: regular hourly earnings ¹ (all regularly paid components divided by contractual hours, excluding overtime but including regular supplements) and total hourly earnings ² (which also covers overtime and bonuses, divided by all hours worked). We measure hours worked by the sum of monthly regular (contractual) and overtime hours. Employer change is measured as a change in the employer identifier between years t and $t + 1$. The variable is set to missing if the employer identifier is missing in either year.

We match Kotilainen’s (2018) private sector collective agreement data, which detail wage increase magnitudes and timing. The dataset includes 776 contracts, about 80% generally bind-

¹Regular gross hourly earnings are gross pay per hour for standard contractual monthly working time. They are calculated as regular gross monthly earnings divided by regular contractual hours in the reference month. For each pay period, the concept includes base pay; duty/skill/seniority supplements; supplements for inconvenient hours, workplace location or conditions; regularly paid individual performance bonuses (e.g., sales commissions, piece rates); and the taxable value of fringe benefits. It also includes pay for normal hours not worked (e.g., paid sick leave, public holidays). It excludes additional and overtime pay; compensation for on-call or urgent work and similar allowances; and one-off items such as holiday pay and irregular performance bonuses not paid each period.

²This measure includes additional and overtime pay and all bonuses and allowances paid in every pay period (even if the amounts vary). It also includes regular monthly performance pay (e.g., piece-rate pay for blue-collar workers) but excludes bonuses and allowances not paid each period (e.g., year-end company bonuses based largely on collective criteria). Standby pay and benefits in kind (or their taxable value) are included.

ing. Our main interest is whether an equality allowance was present in each year’s agreements. In addition to these data, we have collected the collective agreements of municipalities for the years 2005-2006. The variables we use from these data include total contractual increase, general increase, other contractual increases (such as industry allowance that is allocated as a general increase), industry allowance (when not allocated as a general increase), equality allowance and local allowance. These variables cover the contractual wage increase elements stipulated in the collective agreements.

HSES data lack individual collective agreement details, but can be matched using Kotilainen’s (2018) mapping based on industry and occupation. About 17% of employees could be linked to multiple agreements; these cases defaulted to the generally binding agreement. If all agreements were equally binding or non-binding, the contract with the most employees was selected.

We drop observations that are identified in the data as having invalid hourly wage or monthly wage ³. We also drop observations without a valid employer identifier, persons younger than 18 years old or older than 68 years old, those for whom a collective agreement cannot be identified, and persons working in agriculture. Finally, we drop the persons in private sector collective agreements, where contractual increases are not percentage increases, but either euro or cent increases or a combination of increase types. This is because in these cases we cannot decompose the contractual increases in the way done in Table 1. This drops about 15% of observations in the private sector.

4.2 Institutional variation in equality allowances

Figure 1 plots average wage growth and contractual increases over the period 2005–2010 separately for workers covered by agreements with an equality allowance (EA) and those without. Several patterns emerge. First, wage growth closely follows contractual increases in both groups, but is consistently higher, reflecting the role of wage drift. Second, in 2005 and 2006, contractual increases are larger in the agreements with equality allowance in 2006, but actual wage growth is larger for persons covered by contracts that do not have equality allowance in 2006. Third, for both groups the contractual increases and realized wage growth reach their peak in 2008 and decline afterwards due to the financial crisis.

³BASIS1==0 and BASIS2==0, respectively

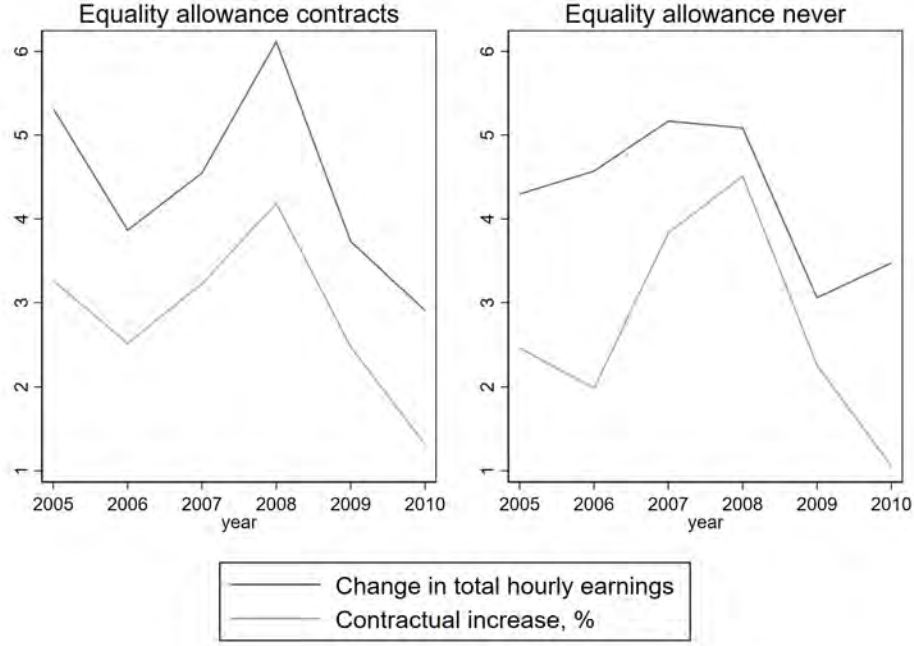


Figure 1: Wage increases in 2005-2010

Notes: Average wage change and contractual increase (%), 2005-2010.

Table 1 provides a decomposition of contractual wage increases by sector and year for 2005 and 2006, distinguishing between collective agreements that received an equality allowance in 2006 and those that did not. In the private sector, agreements with an equality allowance in 2006 had slightly higher total contractual increases in both 2005 and 2006, despite somewhat lower general increases. The difference is explained by larger fallback increases and locally distributed industry and equality allowances. In the municipal sector, all agreements included an equality allowance component in 2006, preventing comparisons between agreements with and without equality allowances. Contractual increases in the municipal sector were larger than in the private sector, particularly in 2005, and a substantial share of these increases came through the fallback component rather than general increases.

4.3 Descriptive statistics

Table 2 presents descriptive statistics for our sample employees in 2006 separately for the private and municipal sectors. The estimation sample consists of 956,663 worker-year observations, providing broad coverage across occupations and education groups in both sectors. The equality allowance averages 0.24 percent overall, but is more common in the municipal sector than in the private sector, reflecting differences in the adoption of equality-oriented bargaining components across collective agreements. In the municipal sector, all collective agreements included an equality allowance component, whereas adoption in the private sector was substantially more limited. Consistent with the institutional setting, equality allowances therefore varied primarily across private-sector agreements. Identification consequently relies on variation across agreements in whether the agreement includes an equality allowance component.

Table 1: Contractual wage increases by year, sector, and equality allowance status

	2005		2006	
	No EA in 2006	EA in 2006	No EA in 2006	EA in 2006
<i>Panel A: All</i>				
Change in hourly earnings	4.30	5.32	4.57	3.87
Total contractual increase	2.46	3.26	1.98	2.55
General increase	2.40	2.36	1.82	1.58
Other contractual increases, fallback	0.06	0.85	0.16	0.85
Industry allowance, locally distributed	0.01	0.05	0.00	0.04
Equality allowance, local	0.00	0.00	0.00	0.07
Local allowance	0.00	0.00	0.00	0.00
<i>Panel B: Private sector</i>				
Change in hourly earnings	4.30	4.59	4.57	4.28
Total contractual increase	2.46	2.53	1.98	2.09
General increase	2.40	2.23	1.82	1.47
Other contractual increases, fallback	0.06	0.14	0.16	0.28
Industry allowance, locally distributed	0.01	0.15	0.00	0.12
Equality allowance, local	0.00	0.00	0.00	0.21
Local allowance	0.00	0.01	0.00	0.00
<i>Panel C: Public sector</i>				
Change in hourly earnings	.	5.57	.	3.70
Total contractual increase	.	3.58	.	2.77
General increase	.	2.41	.	1.64
Other contractual increases, fallback	.	1.17	.	1.13
Industry allowance, locally distributed	.	0.00	.	0.00
Equality allowance, local	.	0.00	.	0.00
Local allowance	.	0.00	.	0.00

Notes: EA denotes agreements with an equality allowance component in 2006. All public-sector agreements included an equality allowance component in 2006, so there are no public-sector observations in the “No EA in 2006” category.

Appendix Table A1 reports descriptive statistics for workers in 2006 by equality allowance (EA) status. About 60% of workers are covered by EA, with an average allowance of 0.384 percent among the treated. Average hourly earnings are slightly lower for EA workers (14.6 vs. 15.4 euros per hour), although dispersion is substantial in both groups. EA coverage is clearly non-random and aligns with institutional differences across agreements. It is more prevalent among older workers, with a higher share in age groups above 45. Occupational differences are pronounced: EA workers are concentrated in clerical support and technician occupations, while craft and machine-related occupations are strongly underrepresented.

There are also clear differences in education and field of study. Workers covered by EA are less likely to have backgrounds in technology and more likely to be in social sciences, services, and health-related fields. Consistent with this, they are relatively more likely to hold short-cycle tertiary or higher degrees and less likely to have only upper secondary education. Part-time work is also slightly more common among EA workers. Overall, these patterns indicate that EA exposure is systematically related to worker and job characteristics.

5 Empirical strategy

We estimate the effects of equality allowances using the DID_M estimator developed by De Chaisemartin and d’Haultfoeuille (2020), which compares the evolution of outcomes between treated and comparison groups. This estimator is well suited to our setting because treatment varies across collective agreements, and treatment effects may be heterogeneous across agreements. In such settings, standard two-way fixed-effects estimators may recover weighted averages of

Table 2: Descriptive statistics by sector

	Private sector	Municipal sector	Total
Hourly earnings	15.259 (5.555)	14.445 (6.391)	14.913 (5.938)
Regular hourly earnings	15.125 (5.500)	13.963 (5.412)	14.631 (5.493)
Monthly working hours	156.139 (28.302)	158.246 (26.053)	157.034 (27.389)
Employer change	0.162 (0.368)	0.086 (0.280)	0.128 (0.334)
Equality allowance (%)	0.098 (0.149)	0.424 (0.127)	0.237 (0.214)
<i>Gender</i>			
Male	296,656 (53.9%)	82,838 (20.4%)	379,494 (39.7%)
Female	253,646 (46.1%)	323,523 (79.6%)	577,169 (60.3%)
<i>Age</i>			
Under 25 years	68,345 (12.4%)	16,237 (4.0%)	84,582 (8.8%)
26–35	135,839 (24.7%)	67,926 (16.7%)	203,765 (21.3%)
36–45	143,918 (26.2%)	109,953 (27.1%)	253,871 (26.5%)
46–55	135,780 (24.7%)	137,873 (33.9%)	273,653 (28.6%)
56–	66,420 (12.1%)	74,372 (18.3%)	140,792 (14.7%)
<i>Occupation</i>			
Professionals	61,139 (11.1%)	120,001 (29.5%)	181,140 (18.9%)
Technicians and associate professionals	119,081 (21.6%)	91,945 (22.6%)	211,026 (22.1%)
Clerical support workers	66,125 (12.0%)	23,896 (5.9%)	90,021 (9.4%)
Service and sales workers	114,215 (20.8%)	118,101 (29.1%)	232,316 (24.3%)
Craft and related trades workers	73,430 (13.3%)	8,198 (2.0%)	81,628 (8.5%)
Plant and machine operators	70,215 (12.8%)	3,535 (0.9%)	73,750 (7.7%)
Elementary occupations	45,852 (8.3%)	40,435 (10.0%)	86,287 (9.0%)
<i>Field of education</i>			
General	150,946 (27.4%)	48,867 (12.0%)	199,813 (20.9%)
Educational science	2,761 (0.5%)	35,602 (8.8%)	38,363 (4.0%)
Humanities	12,965 (2.4%)	20,426 (5.0%)	33,391 (3.5%)
Social science	101,897 (18.5%)	46,956 (11.6%)	148,853 (15.6%)
Natural science	10,238 (1.9%)	9,181 (2.3%)	19,419 (2.0%)
Technology	183,097 (33.3%)	37,788 (9.3%)	220,885 (23.1%)
Agriculture	9,934 (1.8%)	4,736 (1.2%)	14,670 (1.5%)
Health and welfare	30,430 (5.5%)	155,263 (38.2%)	185,693 (19.4%)
Service	47,921 (8.7%)	47,464 (11.7%)	95,385 (10.0%)
Other	113 (0.0%)	78 (0.0%)	191 (0.0%)
<i>Level of education</i>			
Lower secondary or unknown	100,014 (18.2%)	39,303 (9.7%)	139,317 (14.6%)
Upper secondary	263,184 (47.8%)	162,391 (40.0%)	425,575 (44.5%)
Short-cycle tertiary	84,898 (15.4%)	83,527 (20.6%)	168,425 (17.6%)
Bachelor's or equivalent	62,029 (11.3%)	52,483 (12.9%)	114,512 (12.0%)
Master's or equivalent	38,244 (6.9%)	64,392 (15.8%)	102,636 (10.7%)
Doctoral or equivalent	1,933 (0.4%)	4,265 (1.0%)	6,198 (0.6%)
<i>Part-time contract</i>			
No	479,427 (87.1%)	353,838 (87.1%)	833,265 (87.1%)
Yes	70,875 (12.9%)	52,523 (12.9%)	123,398 (12.9%)
Number of observations	550,302 (57.5%)	406,361 (42.5%)	956,663 (100.0%)

Notes: The table reports descriptive statistics for the worker-level estimation sample separately for the private and municipal sectors in 2006. The sample consists of 956,663 worker observations. Continuous variables are reported as means with standard deviations in parentheses. Categorical variables are reported as counts with percentages in parentheses. Equality allowance denotes the contractual wage increase component defined at the collective agreement level.

treatment effects with negative weights, making their causal interpretation problematic under heterogeneity. DID_M avoids this issue by identifying the average treatment effect for the cells whose treatment status changes between $t - 1$ and t .

Treatment is defined at the collective-agreement level. In the baseline specifications, treatment is binary: $D_{at} = 1$ if collective agreement a includes any positive equality allowance in year t , and zero otherwise. A worker is therefore considered treated in year t if the collective agreement governing their employment contains an equality allowance in that year. Treatment adoption occurs when a collective agreement introduces an equality allowance between periods $t - 1$

and t . The comparison group consists of workers covered by agreements that do not introduce an equality allowance over the same period and remain untreated in $t - 1$. Identification therefore relies on variation across agreements in the introduction of equality allowances. The DID_M estimator compares changes in outcomes among workers in agreements that switch treatment status with changes among workers in agreements whose treatment status remains unchanged over the same interval. The magnitude of the allowance is not used as the treatment variable in the baseline specifications. We examine heterogeneity by allowance size separately in Appendix Figure A1, where treated agreements are divided into lower- and higher-intensity groups. All models are estimated at the worker-year level, with standard errors clustered at the collective-agreement level. The estimations use the Stata command *did_multiplt_dyn* (De Chaisemartin and d’Haultfoeuille, 2024).

The identifying assumption is that, absent treatment, average outcome trends would have evolved similarly across switching and comparison agreements. Following De Chaisemartin and d’Haultfoeuille (2020), we assess the plausibility of this assumption using placebo tests that shift treatment backward by one period. These placebo estimates provide a direct check for differential pre-trends between future treated and comparison agreements before the equality allowance is introduced.

To allow for dynamic treatment effects, we also use the extension developed by De Chaisemartin and d’Haultfoeuille (2024). For each post-treatment horizon, the estimator compares outcome changes among “switchers”, units whose treatment status changes for the first time in a given period, with outcome changes among appropriate control units. The control group consists of units that have not yet changed treatment status over the relevant comparison window. In our case, this means comparing units that first become treated with units that remain untreated up to the same horizon. The estimator then averages these horizon-specific comparisons across switchers, allowing treatment effects to vary with time since treatment. The key identifying assumption is a dynamic version of parallel trends: absent the treatment change, switchers and the corresponding control units would have experienced the same average outcome evolution over the relevant horizon. As in the static DID_M framework, this assumption can be partly assessed using pre-treatment placebo comparisons.

6 Results

6.1 Private sector workers

6.1.1 Equality allowances and other contractual wage components

Our preferred analysis focuses on private-sector collective agreements. Before examining realized earnings, we investigate how the introduction of an equality allowance affected the overall contractual wage settlement and its components. This matters because the allowance was negotiated as part of a broader wage settlement and may therefore have been accompanied by adjustments in other contractual increases.

Table 3 reports DID_M estimates using contractual wage components as outcomes for private sector workers. The estimated effect on the total contractual increase is -0.009 percentage points

and statistically insignificant. The point estimate for the general increase is -0.137 percentage points, while those for fallback increases and locally distributed industry allowances are -0.068 and -0.048 percentage points, respectively. None of these effects is statistically significant.

Table 3: Estimated impacts of equality allowance on contractual wage components

	Total contractual increase	General increase	Other contractual increases, fallback	Industry allowance, locally distributed
ATE	-0.009 (0.078)	-0.137 (0.141)	-0.068 (0.097)	-0.048 (0.081)
N	451883	451883	451883	451883

Notes: The table reports DID_M estimates following [De Chaisemartin and d’Haultfoeuille \(2020\)](#) for private-sector workers. The dependent variables are the contractual wage increase components indicated in the column headings and are measured in percentage points. ATE denotes the average treatment effect. Standard errors, clustered at the collective-agreement level, are reported in parentheses. N denotes the number of worker-year observations.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Although imprecise, the estimates are consistent with partial substitution between the equality allowance and other components of the settlement: introducing an equality allowance did not translate one-for-one into a larger total contractual wage increase. These results affect the interpretation of the earnings estimates. A zero effect on realized earnings need not reflect only incomplete pass-through or offsetting wage drift; attenuation may already occur within the contractual settlement if the equality allowance partly replaces other negotiated components. Our estimates should therefore be interpreted as capturing the consequences of settlements containing an equality allowance rather than the ceteris paribus effect of the allowance alone.

6.1.2 Effects on earnings, working hours, and employer change

Table 4 reports the baseline estimates for private-sector workers. Treatment is defined at the collective-agreement level: the treatment variable is an indicator equal to one if the collective agreement covering the worker in 2006 included an equality allowance, and zero otherwise. The equality allowance has no statistically significant effects on hourly earnings, working hours, or employer mobility. The estimated effects on total and regular hourly earnings are small, at 6.3 and 6.7 cents per hour, respectively. The estimate for monthly working hours is -0.55 hours, while the estimated effect on the probability of changing employer is -3.4 percentage points. None of these estimates is statistically distinguishable from zero. The placebo estimates are also statistically insignificant and provide no evidence of differential pre-treatment trends.

Table 5 examines heterogeneity by gender and wage level in the private sector. Low-wage women are defined as those earning less than €10.70 per hour, a cutoff corresponding to the bottom quartile of the overall hourly wage distribution. For both women and men, the estimated effects remain statistically insignificant across all outcomes. For low-wage women, the equality allowance is associated with a statistically significant reduction in monthly working hours of about 2.4 hours. In contrast, the estimated effects on total and regular hourly earnings and employer mobility are small and statistically insignificant. The placebo estimates are also statistically insignificant, providing no evidence of differential pre-treatment trends. Overall, the results provide little evidence that the equality allowance affected wage growth or employer mobility in the private sector, although the reduction in working hours among low-wage women

Table 4: Estimated impacts of equality allowance for private sector

	Total hourly earnings	Regular hourly earnings	Working hours	Employer change
ATE	0.063 (0.103)	0.067 (0.101)	-0.554 (0.575)	-0.034 (0.030)
ATE (placebo)	0.145 (0.204)	0.139 (0.202)	1.133 (2.222)	0.042 (0.047)
N	451883	451883	451883	397919

Notes: The table reports DID_M estimates following [De Chaisemartin and d'Haultfoeuille \(2020\)](#), with standard errors in parentheses for private sector workers in the sample. ATE denotes the average treatment effect. ATE (placebo) reports a placebo estimate obtained by assigning treatment to period $t - 1$ rather than period t . The data are at the worker-year level, and N denotes the number of worker-year observations. All specifications include individual fixed effects. Standard errors are clustered at the collective agreement level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

suggests a possible adjustment along the intensive margin.

Table 5: Estimated impacts by gender and for low-wage women for private sector

	Total hourly earnings	Regular hourly earnings	Working hours	Employer change
<i>Panel A: Women</i>				
ATE	0.070 (0.088)	0.073 (0.087)	-0.345 (0.558)	-0.023 (0.024)
ATE (placebo)	0.060 (0.170)	0.060 (0.168)	0.618 (2.052)	-0.001 (0.036)
N	225542	225542	225542	197797
<i>Panel B: Men</i>				
ATE	0.024 (0.093)	0.027 (0.091)	-0.896 (0.739)	-0.035 (0.037)
ATE (placebo)	0.291 (0.275)	0.277 (0.273)	2.295 (2.547)	0.099 (0.077)
N	226340	226340	226340	200122
<i>Panel C: Low-Wage Women</i>				
ATE	-0.032 (0.032)	-0.031 (0.032)	-2.373** (1.186)	0.003 (0.012)
ATE (placebo)	0.146 (0.107)	0.150 (0.105)	1.387 (4.969)	0.097 (0.100)
N	54831	54831	54831	45225

Notes: See Table 4 for table notes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6 reports dynamic treatment effects for the private sector. The estimates provide no evidence of statistically significant effects on total or regular hourly earnings, working hours, or employer mobility at any horizon. The point estimates for working hours become increasingly negative over time, from -0.55 hours at $t+1$ to -1.52 hours at $t+3$, but they remain imprecisely estimated. The earnings effects are small and positive throughout, while the estimates for employer changes remain close to zero. Overall, the results provide no evidence that the equality allowance generated persistent changes in wages, working hours, or labor-market mobility.

Appendix Figure A1 examines whether the effects of the equality allowance differ by the size of the allowance in the private sector by conducting separate analyses for agreements with equality allowances below and above 0.3 percent. The left-hand graphs in each subfigure include agreements with equality allowances of 0.1 or 0.2 percent, while the right-hand graphs

Table 6: Dynamic effects of equality allowance for private sector

	Total hourly earnings	Regular hourly earnings	Working hours	Employer change
ATE ($t + 1$)	0.063 (0.103)	0.067 (0.101)	-0.554 (0.575)	-0.034 (0.030)
ATE ($t + 2$)	0.120 (0.188)	0.062 (0.194)	-0.679 (0.823)	-0.013 (0.015)
ATE ($t + 3$)	0.173 (0.348)	0.131 (0.355)	-1.523 (1.245)	0.015 (0.018)
N	451883	451883	451883	397919

Notes: The table reports dynamic DID_M estimates following [De Chaisemartin and d'Haultfoeuille \(2024\)](#). ATE ($t + k$) denotes the effect k years after treatment. Standard errors are in parentheses and clustered at the collective agreement level. See Table 4 for further details. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

include agreements with equality allowances of 0.3, 0.4, or 0.5 percent. Across all outcomes, the estimates are small, imprecisely estimated, and broadly similar for both groups. Overall, the figure provides little evidence that the effects differed systematically by the size of the equality allowance.

6.2 Private and municipal sector workers

As a supplementary analysis, we pool private- and municipal-sector workers. These estimates require particular caution. All municipal-sector collective agreements included an equality allowance, so there is no untreated comparison group within the municipal sector. Moreover, municipal contractual wage increases were already substantially different from private-sector increases in 2005. The pooled estimates may therefore reflect broader differences between public- and private-sector wage setting in addition to the introduction of the equality allowance. For this reason, we do not regard the pooled estimates as identifying a separate causal effect for municipal workers.⁴ Table 7 reports estimates for the pooled sample of private- and municipal-sector workers. The estimated effects on total and regular hourly earnings are close to zero and statistically insignificant. In contrast, the equality allowance is associated with a reduction in monthly working hours of approximately 0.85 hours, although the estimate is only marginally statistically significant at the 10 percent level. The placebo estimates are statistically insignificant across outcomes, providing no evidence of differential pre-treatment trends.

Table 7: Estimated impacts of equality allowance

	Total hourly earnings	Regular hourly earnings	Working hours
ATE	-0.024 (0.085)	-0.020 (0.084)	-0.845* (0.458)
ATE (placebo)	-0.190 (0.147)	-0.173 (0.135)	-0.060 (0.921)
N	858244	858244	858244

Notes: The table reports DID_M estimates following [De Chaisemartin and d'Haultfoeuille \(2020\)](#), with standard errors in parentheses for the pooled sample of private and municipal sector workers. See Table 4 for further table notes. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

⁴We do not report estimates for employer change in the pooled sample because employer identifiers contain coding inconsistencies that make mobility measures unreliable for public-sector workers.

6.3 Discussion of the results

Overall, the results indicate that the equality allowance did not measurably reduce the gender wage gap in Finland. Although the allowance was disproportionately applied in female-dominated sectors and occupations, it produced no statistically significant increase in hourly earnings, either overall or separately for women and men. In the preferred private-sector analysis, the estimated effects on total hourly earnings were small and statistically insignificant for both women and men, and there was likewise no earnings effect among low-wage women, one of the groups the policy was intended to benefit. Because the allowance did not raise women's wages relative to men's, the results provide no evidence that it narrowed the gender wage gap. One plausible explanation is that the allowance was too small, at no more than approximately 0.5 per cent, and partly substituted for other elements of the negotiated wage settlement rather than increasing total contractual wage growth. The findings should therefore be interpreted as showing no detectable effect on the gender wage gap, rather than ruling out effects too small to be identified precisely.

Our results stand in clear contrast to the Swedish evidence. While we find that the equality allowance was more common in female-dominated sectors and occupations but had no statistically significant effects on hourly earnings, the Swedish studies report persistent earnings gains following more substantial targeted interventions. In [Eliasson and Nordström Skans \(2014\)](#), the targeted agreement raised wages and had lasting effects alongside adjustments in hours growth and hiring, while [Bustos \(2024\)](#) shows sustained labor-income gains for assistant nurses without robust effects on separations or contracted working time.

One potential explanation for the differences in the results is the magnitude of the equality components. Whereas the Finnish equality allowance was small, being around 0.1–0.5 per cent, the Swedish equality-oriented components were institutionally more substantial, taking the form of explicit monthly additions such as SEK 400/200 per low-paid woman in the case studied by [Eliasson and Nordström Skans \(2014\)](#) and an extra SEK 500 per month for assistant nurses in [Bustos \(2024\)](#). These translate to 1.1–2.2% of average base wage in [Eliasson and Nordström Skans \(2014\)](#) and 3.8% for assistant nurses versus 2.0 % for attendants in [Bustos \(2024\)](#).

The Finnish and Swedish equality allowances were also embedded in different bargaining logics. In Finland, the equality allowance was introduced through the centralized TUPO settlement as a single component combining low-pay and gender-equality objectives, with its average size set centrally but its allocation left to sectoral bargaining. By contrast, the Swedish arrangements were more explicitly specified targeting devices within public-sector bargaining: in [Eliasson and Nordström Skans \(2014\)](#), the agreement linked additional wage space to the number of low-paid women at the establishment level, whereas in [Bustos \(2024\)](#) the agreement singled out assistant nurses for larger centrally bargained increases than other covered workers. From an industrial-relations perspective, the contrast is therefore between a sectorally mediated equality component nested in a broad corporatist settlement in Finland and more direct, tightly specified forms of targeting in Sweden.

Taken together, the comparison suggests that equality bargaining is more likely to matter when the intervention is large and clearly transmitted to pay-setting outcomes, rather than

embedded as a relatively modest component within a system that tends to preserve existing relative wages.

7 Conclusions

This paper has examined whether collective bargaining can reduce gender inequality through a targeted wage component. Although the Finnish equality allowance was more common in female-dominated sectors and occupations, we find no statistically significant effects on hourly earnings, either overall or separately for women and men. The contractual-component results provide one potential explanation: the point estimates are consistent with some offset of the equality allowance through other components of the negotiated settlement. However, some estimates point to modest reductions in working hours. Placebo estimates likewise do not point to strong pre-trends. The findings therefore suggest that directing a small contractual wage component towards equality objectives was not, in itself, sufficient to produce detectable short-run changes in worker outcomes in the Finnish private sector and municipalities. Our findings are consistent with qualitative evidence from Finland that equality-oriented wage policies may fail to translate into relative gains when implementation is filtered through corporatist bargaining institutions that preserve existing relative wages ([Saari et al., 2021](#)).

Our results stand in clear contrast to the Swedish evidence, where studies have shown that gender equality bargaining has led to larger wage increases for women ([Eliasson and Nordström Skans, 2014](#); [Bustos, 2024](#)). These differences may reflect institutional differences. In Finland, the equality allowance was introduced through the centralized TUPO settlement as a combined low-pay and gender-equality component whose average size was set centrally but whose allocation was left to sectoral bargaining. By contrast, the Swedish arrangements took the form of more explicit targeting devices within public-sector bargaining, either by linking additional wage space to the number of low-paid women at the establishment level or by singling out assistant nurses for larger centrally bargained increases than other covered workers.

More broadly, the results point to an important limit of equality-oriented bargaining in coordinated wage-setting systems. Formal equality provisions may signal distributive intent, but their substantive effects are likely to depend on the scale of the intervention and on whether bargaining institutions permit meaningful changes in relative wages across sectors and groups. In that sense, the Finnish equality allowance appears to have been institutionally noteworthy but economically modest: it recognized gender equality as a bargaining objective without materially altering the wage-setting outcomes that sustain inequality.

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A Additional tables and figures

Table A1: Summary statistics by equality allowance in 2006

	No EA	EA	Total
Observations	367,280 (38.4%)	589,383 (61.6%)	956,663 (100.0%)
Hourly earnings	15.410 (5.494)	14.604 (6.179)	14.913 (5.938)
Regular hourly earnings	15.250 (5.432)	14.246 (5.496)	14.631 (5.493)
Monthly working hours	157.272 (27.509)	156.885 (27.314)	157.034 (27.389)
Employer change	0.174 (0.379)	0.101 (0.301)	0.128 (0.334)
Equality allowance (%)	0.000 (0.000)	0.384 (0.132)	0.237 (0.214)
<i>Gender</i>			
Male	221,647 (60.3%)	157,847 (26.8%)	379,494 (39.7%)
Female	145,633 (39.7%)	431,536 (73.2%)	577,169 (60.3%)
<i>Age</i>			
Under 25 years	47,935 (13.1%)	36,647 (6.2%)	84,582 (8.8%)
26–35	95,640 (26.0%)	108,125 (18.3%)	203,765 (21.3%)
36–45	95,214 (25.9%)	158,657 (26.9%)	253,871 (26.5%)
46–55	86,670 (23.6%)	186,983 (31.7%)	273,653 (28.6%)
56–	41,821 (11.4%)	98,971 (16.8%)	140,792 (14.7%)
<i>Occupation</i>			
Professionals	39,685 (10.8%)	141,455 (24.0%)	181,140 (18.9%)
Technicians and associate professionals	69,959 (19.1%)	141,067 (23.9%)	211,026 (22.1%)
Clerical support workers	21,880 (6.0%)	68,141 (11.6%)	90,021 (9.4%)
Service and sales workers	80,515 (21.9%)	151,801 (25.8%)	232,316 (24.3%)
Craft and related trades workers	71,167 (19.4%)	10,461 (1.8%)	81,628 (8.5%)
Plant and machine operators	55,265 (15.1%)	18,485 (3.1%)	73,750 (7.7%)
Elementary occupations	28,625 (7.8%)	57,662 (9.8%)	86,287 (9.0%)
<i>Field of education</i>			
General	101,781 (27.7%)	98,032 (16.6%)	199,813 (20.9%)
Educational science	1,101 (0.3%)	37,262 (6.3%)	38,363 (4.0%)
Humanities	7,127 (1.9%)	26,264 (4.5%)	33,391 (3.5%)
Social science	60,894 (16.6%)	87,959 (14.9%)	148,853 (15.6%)
Natural science	7,352 (2.0%)	12,067 (2.0%)	19,419 (2.0%)
Technology	143,031 (38.9%)	77,854 (13.2%)	220,885 (23.1%)
Agriculture	6,190 (1.7%)	8,480 (1.4%)	14,670 (1.5%)
Health and welfare	15,002 (4.1%)	170,691 (29.0%)	185,693 (19.4%)
Service	24,732 (6.7%)	70,653 (12.0%)	95,385 (10.0%)
Other	70 (0.0%)	121 (0.0%)	191 (0.0%)
<i>Level of education</i>			
Lower secondary or unknown	69,201 (18.8%)	70,116 (11.9%)	139,317 (14.6%)
Upper secondary	187,245 (51.0%)	238,330 (40.4%)	425,575 (44.5%)
Short-cycle tertiary	47,264 (12.9%)	121,161 (20.6%)	168,425 (17.6%)
Bachelor's or equivalent	39,343 (10.7%)	75,169 (12.8%)	114,512 (12.0%)
Master's or equivalent	23,234 (6.3%)	79,402 (13.5%)	102,636 (10.7%)
Doctoral or equivalent	993 (0.3%)	5,205 (0.9%)	6,198 (0.6%)
<i>Part-time contract</i>			
No	323,145 (88.0%)	510,120 (86.6%)	833,265 (87.1%)
Yes	44,135 (12.0%)	79,263 (13.4%)	123,398 (12.9%)
<i>Employer sector</i>			
Private sector	367,280 (100.0%)	183,022 (31.1%)	550,302 (57.5%)
Municipal sector	0 (0.0%)	406,361 (68.9%)	406,361 (42.5%)

Notes: EA denotes agreements with an equality allowance component in 2006. The table reports summary statistics separately for workers covered by agreements with and without an equality allowance.

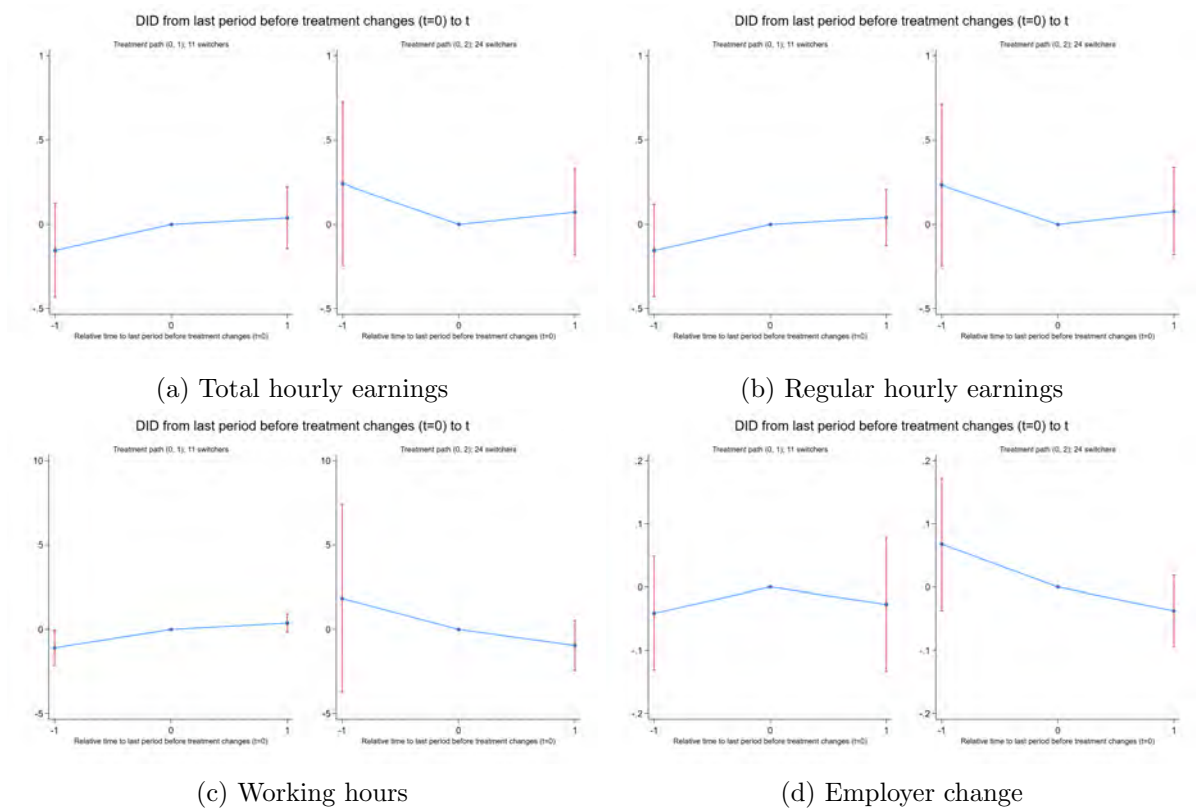


Figure A1: Effects by equality allowance intensity for private sector

Notes: The figure reports dynamic DID_M estimates. Error bars denote 95 percent confidence intervals. Relative time is measured from the last period before treatment changes ($t = 0$). In each panel, the left-hand graph shows estimates for collective agreements with equality allowances of 0.1 or 0.2 percent, while the right-hand graph shows estimates for agreements with equality allowances of 0.3, 0.4, or 0.5 percent.