

## ORIGINAL ARTICLE

# Union decline through extension of collective agreements?

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## Abstract

This study investigates the effects of the mandatory extension of collective agreements on union membership. This relationship is investigated using a difference-in-differences framework on the staggered industry- and county-wide introduction of mandatory extension in Norway from 2005 to 2011. The introduction of mandatory extension was championed by labour and social partners and motivated by the EU enlargement of 2004 in response to increasing labour immigration. However, mandatory extension could lead to lower union membership through free-rider behaviour. It is therefore a question of whether the cure is worse than the problem it is supposed to solve. The results show that mandatory extension had an overall negative effect on union density in the affected industries of about  $-2.7$  percentage points. The effect varies between the different industries but is never positive. These findings give credence to the free-rider hypothesis, where mandatory extension is expected to lead to lower union density because there is less incentive to become a union member when workers can obtain the benefits of the collective agreement without paying the union dues. In line with the theoretical expectations, the effect is stronger where the gap between collective wage and non-union wage is largest. However, negative effects are also present where this

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wage gap is small, showing that the results are valid in different contexts.

## 1 | INTRODUCTION

Union membership rates have fallen in most Western countries in the last few decades, though union density levels and the rate of decline vary between countries (Schnabel, 2020). This begs the question, why are unions in some countries able to organize over half of the working population, and why have some countries experienced a steep fall in union membership while membership is relatively stable in other countries? The different development of union membership in various countries cannot be explained exclusively using long-term structural or short-term cyclical observations (Ebbinghaus & Visser, 1999). Union density and the evolution of union density over time vary across countries with similar structural and cyclical economic situations. This variation is often attributed to institutional factors in the labour market, such as the degree of wage centralization, corporatism, union-controlled unemployment insurance, workplace representation of unions, minimum wage and employment protection legislation (Cecchi & Lucifora, 2002; Cecchi & Visser, 2005; Ebbinghaus & Visser, 1999; Visser, 2002; Western, 1995).

In this article, the relationship between union membership and one very widespread (Hayter & Visser, 2018), but not thoroughly researched,<sup>1</sup> labour market institution is analysed: mandatory extension of collective agreements. Mandatory extension of collective agreements is when the collective agreement between labour unions and employer organizations in an industry is made statutory for every worker in the industry, regardless of union membership for the worker or employer membership in an employer organization. Mandatory extension is used in many countries across the world to secure better terms for unorganized workers and to prevent competitive advantages for companies without collective agreements. It has also been proposed as a solution to wage pressures created by increasing immigration from low-wage countries. However, mandatory extension has been argued to reduce the incentives to join a union, as it creates free-rider incentives (Cecchi & Lucifora, 2002; Eldring, 2010).

In this article, I investigate the effects of the mandatory extension of collective agreements on union membership by analysing the industry-by-industry introduction of mandatory extension in Norway from 2005 to 2011 (six mandatory extension events affecting 7 per cent of the Norwegian workforce). Most countries that have leveraged mandatory extension introduced it in the interwar period, or right after the second World War (Hayter & Visser, 2018), periods during which detailed, relevant data were typically not available. The Norwegian introduction of mandatory extension was the first instance of mandatory extension in a non-formerly communist OECD country<sup>2</sup> since Iceland in 1980. The Norwegian case is therefore uniquely well-suited for the analysis of the effect of mandatory extension of collective agreements on union membership because we can leverage detailed individual-level administrative data from before and after the introduction to investigate its effects. Mandatory extension was introduced in Norway because of the fear of downward wage pressure caused by labour immigration after the EU enlargement in 2004 (Eldring, 2010). The reform was therefore inherently tied to labour immigration and was contained to industries experiencing a large increase in labour immigrants during the period. This fact makes the Norwegian case uniquely relevant because it is an example of utilizing an old institution (mandatory extension) to solve modern problems caused by globalization and labour immigration.

Part of the reason mandatory extension was not introduced in Norway prior to EU enlargement was the fear that it would lead to free-rider behaviour and thus lower union density; if a worker

is entitled to the terms of a collective agreement without being a member of a union, they have less incentive to become a union member because of the costs of union membership (Eldring, 2010). This is also the explanation given in the small body of comparative literature on mandatory extension and union density (Blaschke, 2000; Checchi & Lucifora, 2002). However, these studies did not use good measures of the presence of mandatory extension, meaning these measures are therefore not suitable for investigating the relationship between mandatory extension and union density.<sup>3</sup> A qualitative study of the Norwegian case (Eldring & Hansen, 2009) suggested that in certain cases, mandatory extension could have a positive effect on union density. Theoretically, there exist mechanisms that go in the opposite direction. Because mandatory extension reduces the wage gap between union members and non-members, it could lead to lower employer hostility towards unions (Blanchflower & Freeman, 1992; Rasmussen, 2017; Western, 1999).

To analyse this relationship, I use a difference-in-differences with variation in treatment timing framework (Callaway & Sant'Anna, 2021). Mandatory extension was rolled out industry by industry over several years. Therefore, I utilize the variation in the timing of the introduction of the reform in different industries to estimate its effects on union membership.

The findings show that mandatory extension had a negative overall effect on union membership in the affected industries of about  $-2.7$  percentage points. The results give credence to the free-rider hypothesis and are in line with extant comparative literature but go against the previous studies on the Norwegian case. These findings are restricted to certain industries and geographical areas, while in other industries there was a small and insignificant effect or no effect at all. In accordance with the free-rider theory, the negative effect seems to be larger where there is a larger wage difference between union members and non-organized workers. However, there are also negative effects where this wage gap is smaller, indicating that the negative relationship is valid across different contexts. The effects are clearer when analysing only the low-skill workers in the sample, which are the workers expected to be most affected by mandatory extension.

## 2 | LITERATURE REVIEW

This article builds on the literature on institutional determinants of union density. In the following section, I provide a brief overview of the main findings in this literature before focusing on the literature specifically on the mandatory extension of collective agreements and union density.

### 2.1 | Institutional determinants of union density

The most robust findings in the literature on institutional determinants of union density are that union-administered unemployment benefits (Ghent system), union access to the workplace and wage centralization are labour market institutions that are positively linked with union density (Checchi & Lucifora, 2002; Checchi & Visser, 2005; Ebbinghaus & Visser, 1999; Visser, 2002; Western, 1995). In Ghent systems, trade unions have traditionally administered unemployment benefits, leading to higher union density. Union access to the workplace (for instance, in the form of legal consultation rights) increases the day-to-day relevance of trade unions for the worker, which can boost recruitment. Centralized wage bargaining has a positive impact on union density through three channels: less employer hostility towards trade unions, less inter-union competition for the same members and the fact that trade unions, in a centralized wage bargaining setting, can move employment to industries with higher union density.

## 2.2 | Mandatory extension of collective agreements and union density

There has not been much comparative research on the effects of mandatory extension of collective agreements on union membership. However, Checchi and Lucifora (2002) found a negative association between mandatory extension and union density in their analysis of 20 different labour market institutions sampled from 13 European countries from 1960 to 2000. They attributed this negative association to free-rider incentives. However, their operationalization of mandatory extension of collective agreements was questionable. They assigned Norway, Sweden and Denmark the highest scores on the mandatory extension variable because the agreements were 'extended through centralization' (Checchi & Lucifora, 2002, Appendix 3). The fact that Norway, Sweden and Denmark did not use mandatory extension of collective agreements in this time period shows that this is not a good measure of the extent of mandatory extension in a country. Their measure was also time-invariant, which makes it difficult to establish the direction of the effect. It is theoretically equally possible that the effect works in the other direction: countries with low union density could use mandatory extension to offer better wages to more workers. Blaschke (2000) did not have a separate variable for mandatory extension in their analysis, but they found a negative association between union density and the variable 'Statutory Works Council plus Pervasive Extension Practice'. This could indicate that the mandatory extension of collective agreements can have a more negative impact on union density if there is a channel for employee influence in the workplace outside of labour unions.<sup>4</sup>

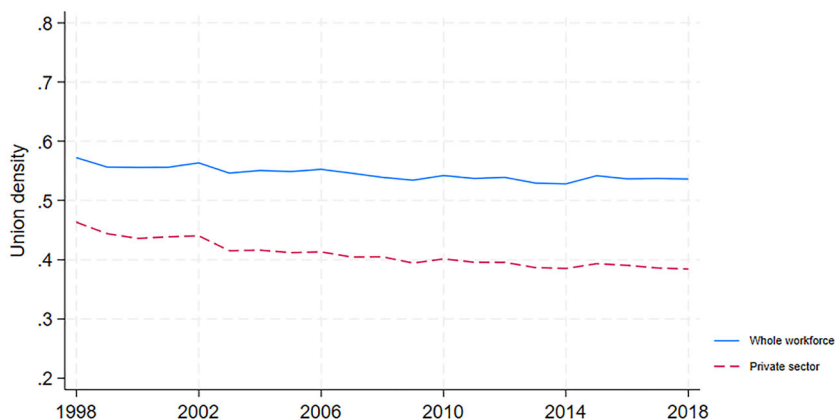
We can see from this review that there is a need for more research that leverages more direct measures of mandatory extension. This article addresses this gap by investigating the causal relationship between Norway's introduction of mandatory extension and union membership. However, there is also room for more comparative studies using more precise measurements of mandatory extension, such as those found in the OECD/AIAS ICTWSS database (OECD and AIAS, 2023).

### 2.2.1 | The Norwegian case

Research has been conducted on the Norwegian case of mandatory extension. First, several studies have found that the reform had a positive impact on the wages of the lowest paid workers (Benedictow et al., 2021; Bjørnstad et al., 2015).

In a qualitative study using interviews with union representatives, Eldring and Hansen (2009) proposed that one of the reasons as to why the labour movement in Norway has been more successful in recruiting labour immigrants than that in Denmark is the fact that Norway enforces mandatory extension of collective agreements. The reasoning of the union representatives is that it is easier for unions to get in touch with and earn the trust of, labour immigrants when unions help them with obtaining their statutory right to the collective wage. Mandatory extension is thus perceived to lower the tensions between employers and workers, making it easier for unions to recruit conflict-shy foreign workers.

Benedictow et al. (2021) found little effect of mandatory extension on union density either way in a difference-in-differences analysis, where each treatment group was analysed separately using the never treated as the control group. As will be explained in the empirical setup, the analysis in this article differs from the analysis carried out by Benedictow et al. (2021) by utilizing more advanced analytical tools, making it possible to use the 'not-yet-treated' as a control group. Addi-



**FIGURE 1** Union density. The graph shows the trends in union density (union membership rate) in the whole workforce and in the private sector in Norway between 1998 and 2018. Individual union membership is automatically tax deductible and therefore part of the tax register.

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.

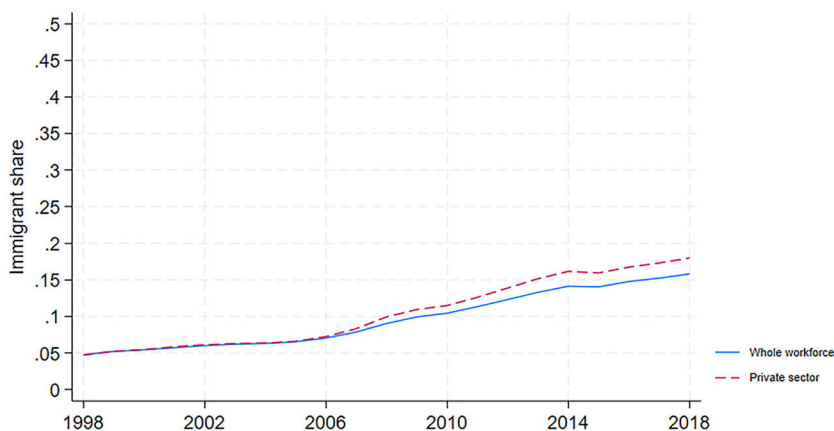
tionally, this study's analysis generated aggregated (unbiased) treatment effects, allowing me to dive deeper into treatment effect heterogeneity.

### 3 | CONTEXT

In this section, I explain the important aspects of the context surrounding the mandatory extension reform, such as the structure of Norwegian wage bargaining, the impact of the 2004 EU enlargement on labour immigration, the development of wage inequality and the prevalence of alternative work arrangements.

#### 3.1 | Labour unions and wage formation

The Norwegian labour market is distinguished by a relatively high, but declining, level of union density. Figure 1 shows that national union density fell from about 57 per cent to 53 per cent from 1998 to 2018 and from 44 per cent to 38 per cent in the private sector. According to Alsos et al. (2021), it is estimated that 64 per cent of workers in Norway are governed by a collective agreement (45 per cent in the private sector). This estimation excludes the workers who are subject to the mandatory extension of collective agreements and the fact that the negotiated wage often serves as the standard for determining the wage offered by companies not governed by collective agreements. Centralized and coordinated wage formation means that the collective wage in an industry is often seen as the industry standard and the norm for workers for whom a collective agreement does not govern their workplace. Alsos et al. (2021) showed that this practice is widespread across industries in Norway. However, the share of uncovered workers who receive the collective wage has been decreasing from 2002 to 2018. Centralized wage formation gives rise to another key aspect of the Norwegian labour market: a compressed wage structure. In 2004, Norway had the sixth lowest Gini coefficient in the OECD (OECD, 2024). General income inequality



**FIGURE 2** Immigrant share. The graph shows the share of foreign-born workers in the whole workforce and in the private sector in Norway from 1998 to 2018.

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.

grew slowly over the research period (Dale-Olsen & Østbakken, 2016). In general, a compressed wage structure means that low-skill workers earn more than they normally would, and high-skill workers earn less than they would in another institutional context (Barth & Moene, 2013).

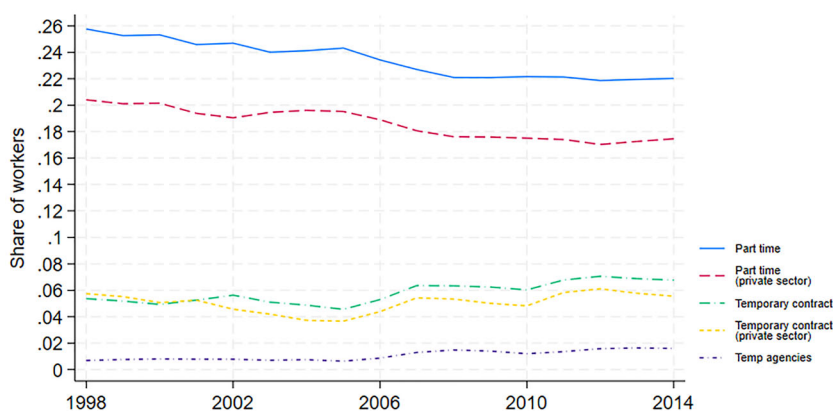
### 3.2 | Labour immigration

The catalyst for the introduction of the mandatory extension of collective agreements was the enlargement of the EU in 2004 and the fear of downwards wage pressure caused by labour immigration from the new EU countries. Therefore, it is important to understand the impact of this event on labour immigration to Norway. As we can see from Figure 2, the share of foreign-born workers in the Norwegian labour force increased sharply after 2004, when workers born in Estonia, Latvia, Lithuania, Poland, Hungary, Czechia, Slovakia, Slovenia, Malta and Cyprus were allowed to apply for work visas in the EU and the European Economic Area (including Norway). The share of foreign-born workers in the Norwegian workforce increased from 6 per cent in 2004 to 15 per cent in 2018. Workers from Poland and Lithuania constitute the two largest immigrant groups in Norway (Steinkellner et al., 2023). The 2004 EU enlargement was a big immigration shock for the Norwegian labour market, and as will be discussed in detail later in the article, this shock did not affect the labour market similarly across all industries: the industries that experienced the largest increase in foreign workers were also the industries that received mandatory extension.

### 3.3 | Labour market affiliation

Traditionally, the Norwegian labour market has been characterized by the ‘full-time permanent employment’ norm. Even though this is still the norm, other types of employment relations gained prevalence over the research period. Figure 3 shows that there has been a small increase in the





**FIGURE 3** Share of workers in non-standard work. The graph shows the share of workers in three types of non-standard work in the whole workforce and in the private sector from 1998 to 2014. Part-time is defined as working under 30 hours per week. A temporary job is defined as a job lasting for less than a year. Temp agency work is defined as a worker working for a company that has the industry codes for temp agencies in the administrative data (74.502 and 78.200).

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.

share of workers on temporary contracts<sup>5</sup> over the period but a reduction in the share of workers working part-time. We also observed an increase in the share of workers employed by temp agencies. In general, there are small signs of an increasing fragmentation of the types of affiliations between employers and employees.

From the sections above, we see that Norway, compared to other OECD countries, has high union membership rates, high collective agreement coverage rates and low wage inequality, and the open-ended employment contract is the predominant form of affiliation between employer and employee. However, we also see that over the research period, the union density and collective bargaining coverage rates declined, alternative forms of employer/employee affiliation were on the rise and there was increasing immigration from countries with lower wages. These are all developments that point towards a more fragmented labour market, albeit from a 'strong starting position'. This is the context that ushered in the introduction of mandatory extension of collective agreements in Norway. Importantly, different industries and geographical areas were not impacted in the same way based on these general trends. We will see in the later sections that the industries that received mandatory extensions were more strongly affected by these developments than other industries.

#### 4 | MANDATORY EXTENSION OF COLLECTIVE AGREEMENTS

The most clear-cut definition of mandatory extension of collective agreements is 'a statutory extension of the coverage of collective agreements' (Stokke, 2010, p. 9). Common for all extended agreements is that the normative provisions regarding wages and working conditions are extended to workers in companies that were originally not covered by the collective agreement. In most countries, collective agreements are carried out on an industry basis. The provisions of a collective agreement between an employer organization and an employee organization are then a

**TABLE 1** The treatment year for the different industries.

	Treatment year	Industry
Excluded from analysis	2004	<ul style="list-style-type: none"> <li>Seven onshore petroleum facilities (terminated in 2010)</li> </ul>
	2005	<ul style="list-style-type: none"> <li>Electric installation in Oslo, Akershus (terminated in 2010)</li> </ul>
First wave (treatment and control group)	2006	<ul style="list-style-type: none"> <li>Construction in Oslo, Akershus, Østfold, Buskerud, Vestfold</li> </ul>
	2007	<ul style="list-style-type: none"> <li>Construction in Hordaland</li> </ul>
	2008	<ul style="list-style-type: none"> <li>Construction in rest of Norway</li> </ul>
	2010	<ul style="list-style-type: none"> <li>Shipbuilding</li> </ul>
	2011	<ul style="list-style-type: none"> <li>Agriculture</li> <li>Cleaning</li> </ul>
Second wave (control group)	2015	<ul style="list-style-type: none"> <li>Fishing industry</li> <li>Electrical installation</li> <li>Road freight transport</li> <li>Bus drivers</li> </ul>
	2018	<ul style="list-style-type: none"> <li>Accommodation</li> <li>Serving</li> <li>Catering</li> <li>Bars</li> </ul>

*Note:* This table shows the rollout of the mandatory extension of collective agreements to the different industries and counties. The extension agreements for seven on-shore petroleum facilities and electric installation in the Oslo and Akershus counties were terminated in 2010 and thus removed from the analysis. The analysis focuses on the first wave extension events while the industries in the second wave are used as control groups (together with the industries in the first wave that have “not yet” received mandatory extension). *Source:* Eldring et al. (2011) and Bergsli (2016).

statutory minimum standard for wages and working conditions in the industry. Once a collective agreement has been extended in an industry, the provisions in the agreement are legally binding for the employer (Stokke, 2010, pp. 18–19). Mandatory extension must not be confused with the minimum wage.<sup>6</sup>

Most European countries use mandatory extension, whether to a smaller or larger extent. In some countries, such as France, collective agreements are extended automatically, leading in France to a collective agreement coverage rate of 98 per cent from a union density of 9 per cent. In other countries, such as Estonia, collective agreements are only extended occasionally with very strict criteria. Most countries, including Norway, are somewhere in between. The only EU countries that do not engage in mandatory extension of collective agreements in any capacity are Lithuania, Poland, Latvia, Cyprus, Greece, Denmark and Sweden (OECD and AIAS, 2023).

#### 4.1 | Mandatory extension in Norway

With the eastward enlargement of the EU in 2004, mandatory extension was actualized in response to the fear of foreign workers undercutting Norwegian wages. LO (the main trade union federation) requested mandatory extension for seven land-based petroleum plants for the first time in 2004. As seen in Table 1, mandatory extension was introduced in the construction sector in the Greater Oslo Area<sup>7</sup> and in electrical installation in the Oslo and Akershus counties in 2005. The construction industry in Hordaland county got mandatory extension in 2006. In 2007, mandatory extension was given to the construction industry in the rest of Norway. The shipbuilding agreement was extended nationwide in 2008, agriculture and horticulture was extended in 2010, and the cleaning agreement was extended nationwide in 2011. This marked the end of the



first ‘wave’ of extensions, where extension was introduced mostly in one industry per year. The second extension wave started in 2015. What separates this wave from the first, in addition to the time gap, is that several nationwide industries received extensions at the same time. In 2015, the agreements for the fishing industry, electrical installation, freight transport by car and passenger transport by bus were extended. In 2018, the agreements for accommodation, catering, restaurants and bars were extended. By this time, over 300,000 workers had their main employment in industries covered by mandatory extension. This constituted about 13 per cent of the total workforce in Norway. The industries in the second wave are used as controls when analysing the effects of the first wave on union membership.<sup>8</sup> Even though the first three extension decisions were geographically limited, all extended collective agreements are now nationwide (Eldring et al., 2011, pp. 76–77). Requests for extension have been declined three times: the nationwide extension for electric installation in 2008 and the requests for continuing extension practices in the seven land-based petroleum plants and in electrical installation in the Oslo and Akershus counties, both in 2010.

The decision to implement mandatory extension of collective agreements in an industry is made by a government-appointed committee, consisting of one representative from the employee organizations, one representative from the employer organizations, and three neutral members. For the committee to make a decision about extension, there must be documentation that foreign workers are, or could in the future be, receiving lower wages than Norwegians for comparable work.

To address the concern of free-riding behaviour, only the minimum individual provisions of the collective agreement were extended.<sup>9</sup> Most other countries extend the complete agreement, including collective provisions, such as pension and bargaining rights, as well as the standard individual provisions (Eldring et al., 2011, pp. 76–77). The gap between the extended agreement and the agreement a worker in a unionized workplace gets is therefore wider in Norway than in other countries. This could possibly mitigate the free-rider effects associated with mandatory extension in Norway compared to other countries. However, if there is an effect in Norway, one would assume that the effect is even larger in other countries, where the difference between the extended agreement and the actual collective agreement is smaller.

## 5 | THEORY

In this section, I first outline the free-rider theory and its theoretical implications for the relationship between mandatory extension of collective agreements and union density. Then, I propose different mechanisms that could pull in the opposite direction.

### 5.1 | Free-rider incentives

Checchi and Lucifora (2002) explained the negative relationship they found between mandatory extension and union density resulting from free-rider behaviour. Olson (1965) is credited as the contributor to the modern understanding of the free-rider problem. He used labour unions in the workplace as an example of how free-rider incentives work because the collective union wage becomes a common good in the workplace. The non-union workers in the workplace also benefit from the wages the union bargains for. Workers will then have incentives not to become a

union member. This way, they can enjoy the benefits of union membership (higher wages) without paying union dues and without direct conflict with their employer.

Mandatory extension of collective agreements invokes the same mechanism, but for a whole industry, not only one company. Specifically, mandatory extension makes the collective agreement a common good in the industry because no one can be excluded from enjoying the benefits once the agreement is extended. Once a collective agreement is in place, there is less incentive to be a union member. Taking union dues into account, the worker have a disincentive to be a union member because they would essentially have to pay for something they can get for free. This presupposes that there are enough union members in an industry for a collective agreement to take place. The number of union members in an industry is still significant enough that the abstention of one member does not affect the bargaining power of the union.

According to this logic, no rational agents would choose to join a union if there is already an extended collective agreement in the industry. We would of course expect that some union members would nevertheless join because there are other factors aside from the purely economic that influence the decision to become a union member.<sup>10</sup> Mancur Olson's solution to the free-rider problem for unions is to offer private goods, such as insurance and pension deals (which most unions do). The fact that unions in the workplace can negotiate higher wages than the extended wage also provides an incentive for union membership, even when there is already an extended agreement. Therefore, the free-rider theory predicts that workers will have less incentive to join a union if there is an extended collective agreement in an industry, thus negatively impacting union membership.

## 5.2 | Other possible mechanisms

There are also other possible mechanisms that could have positive effects on union membership. One of these can be borrowed from the literature on wage centralization and union density and pertains to employer hostility towards unions (Rasmussen, 2017; Blanchflower & Freeman, 1992; Western, 1999). In the same way as centralized bargaining, mandatory extension shrinks the wage gap between union members and non-members. When the wage gap is smaller, the employer has less incentive to fight unions because the loss of profits from unionization would decrease. This could have a positive impact on unionization. This mechanism is the flip side of the free-rider mechanism: when the wage gap is larger, the workers have more incentive for union membership and the employer has more incentive to fight unions.

As mentioned by the informant in Eldring and Hansen's (2009) study, mandatory extension can lower the tensions between employers and workers because workers no longer need to fight employers to obtain collective agreements. It could be easier for unions to get in touch with labour immigrants to help them realize the rights they have by law through mandatory extension than it is to get them to fight their employers to obtain a collective agreement 'the old-fashioned way'. This could make it easier for unions to recruit foreign workers (or other 'outsider workers') because they tend to not want to enter into conflict with their employers (Eldring & Hansen, 2009).

There are also reasons aside from the purely economic ones for joining a union. The social norm model (Akerlöf, 1980; Booth, 1985; Naylor, 1989, 1990; Toubøl & Jensen, 2014; Visser, 2002) posits that in every community, there are rules and customs that individuals follow to avoid the loss of reputation (social sanctions) in the community. In a workplace, being a member of a union could be one such norm that workers follow to avoid social sanctions. Other explanations highlight the attitudes and normative beliefs of the worker, such as left-wing attitudes (Riley, 1997; Schnabel &

Wagner, 2007; Toubøl & Jensen, 2014), the degree of frustration/aggression (Klandermans, 1986) or rational choice models incorporating a broader view than only the economic (Crouch, 1982).

I do not seek to disprove any of these explanations with this article, and this article does not claim that these non-economic factors play a negligible role in individuals' decisions to join a union. As previously mentioned, the existence of union members, even when facing strong free-rider incentives, proves that there are other considerations than the purely economic. However, the introduction of mandatory extension of collective agreements is an economic reform (giving the collective wage to non-union members). Therefore, I theorize that this reform first and foremost affects the economic calculus of union membership and that the non-economic factors are not significantly affected by the reform. The effects of mandatory extension on union density should therefore primarily manifest via the economic calculus mechanism, either for the worker (free riding) or the employer (lower hostility towards unions).

### 5.3 | Theoretical expectations regarding differences in the effects across industries

There are theoretical reasons to believe that the effects of mandatory extension on union membership are not necessarily the same across every industry. The different characteristics of various industries could lead to different effects.

Following directly from free-rider theory, free-rider incentives for workers should be higher in industries where *the gap between the collective wage and the non-union wage is larger before the introduction of mandatory extension*. The reasoning for this is that the places with the largest gaps have the highest incentives for union membership in the first place. The incentive reduction is larger when the initial gap is larger. In industries with smaller wage gaps between the collective wage and the non-union wage, the mandatory extension 'shock' should be smaller.

There are also other industry characteristics that could shape how workers or employers react to mandatory extension. It is possible that mandatory extension could work differently in contexts where the connection between employer and worker is looser. For instance, the incentive structures for both employer and employee could be very different if the worker is employed through a temporary work agency, works for a subcontractor or is employed on a temporary contract. 'Domestic outsourcing' is a term used to describe the hiring of subcontractors and members of temporary work agencies, and the literature on this topic highlights the fact that many companies use domestic outsourcing to reduce wage costs and avoid accountability for labour and employment laws, such as laws concerning wages, working hours, health and safety, pensions and antidiscrimination (Bernhardt et al., 2016; Weil, 2014). If a company has a large union presence and a collective agreement, outsourcing of parts of the production to subcontractors (or temporary work agencies) could reduce costs if the union presence is weaker among the subcontractors. This way, the workers who have a looser connection to their employer/main contractor would derive more benefits from mandatory extension than others because they are not able to reap the benefits of the 'normal' labour market, where the collective wage sets a benchmark for a worker's wage regardless of their union membership. The condition for this is that the firms that are outsourced to comply with the mandatory extension provisions.

### 5.4 | Hypotheses

Based on the mechanisms outlined in the sections above, the main hypothesis is as follows:

1. Mandatory extension of collective agreements will have a negative impact on union membership.

We can also develop hypotheses about treatment heterogeneity:

1. The effect will be larger where the wage gap between the collective wage and the non-union wage is largest.
2. The effect will be larger in labour market contexts where the 'normal' relationship between the employer and the worker, based on permanent full-time contracts, is weakest.

## 6 | DATA

The data used in this study are administrative data from Statistics Norway. The employer/employee register includes individual-level information on wages, working time, workplace and industry for all employment relations in Norway. Other administrative data sources are linked with this register to provide information about gender, age, immigrant status, education, and union membership. In Norway, union membership is automatically tax deductible. Therefore, we have information about every individual's union membership status from the tax register. This is a very precise measurement of individual union membership, which makes it possible to estimate the precise union density of the industries.

To investigate the degree of domestic outsourcing, I retrieved information from the accounting registries on the money spent by each company on the budget item 'contracted labour and subcontracting'. From there, I created a variable that measures the share of 'contracted labour and subcontracting' from the total 'labour costs (wages + hired labour)' in the industry. This is a measure for what is called 'domestic outsourcing' in the literature: the delegation of tasks by one company to workers not employed by the company (temporary agency workers) or to other companies (subcontractors).

The dataset contains data from 1998 to 2018 for all variables, except for data from the accounting registries, as the reporting on these variables started in 2005. The advantage of administrative data is that they are very detailed, providing information about a range of relevant variables for every individual in Norway. This makes it possible to run analyses on subgroups of the population. The challenge with administrative data is that they are not designed for researchers, so one has to be very careful with how a variable is operationalized and be aware of how reporting practices have changed over time. The Norwegian employer/employee register went through a major overhaul in 2015.<sup>11</sup> To address the difficulties in comparing the variables before and after this change, I decided to make 2014 the endpoint of the analysis. For the outcome variable (union membership), the reporting practices were stable over the research period. For identifying treatment status, the industry code for the company where the individual works, is used.<sup>12</sup> Over the research period, three different industry classifications have been used in the registers.<sup>13</sup> I converted the industry codes to NACE Rev. 2 for all years.

To summarize, even though administrative data offer many advantages, they bring some specific challenges concerning operationalization and changes in reporting practices. I addressed these challenges by ending the analysis before the overhaul in 2015 and by recoding variables to make sure the analysis was comparing the same variables over time.

**TABLE 2** The number of individuals in the treatment groups the year before the first treatment.

Treatment groups	Industries	N_2004	N_treatment_time
Never	Rest of the private sector	919,406	.
2005	Construction in Oslo, Akershus, Østfold, Buskerud, Vestfold	37,400	31,736
2006	Construction in Hordaland	7,051	5,470
2007	Construction rest of Norway	39,064	27,259
2008	Shipbuilding	27,168	16,639
2010	Agriculture	13,286	3,535
2011	Cleaning	16,200	4,283
2015	Fishing industry, electrical installation, road freight transport, bus drivers	51,824	16,164
2018	Accommodation, serving, catering	52,233	7,277

Note: This table shows the number of workers who worked in the industry the year before the first extension event and the number of these workers who still worked in the same industry (and county) the year of the mandatory extension. Source: Administrative data from Statistics Norway.

## 7 | EMPIRICAL SETUP

Mandatory extension of collective agreements was introduced in different industries (and counties<sup>14</sup>) in different years. Grouping together the industries that received mandatory extension the same year results in six treatment groups for the years 2005, 2006, 2007, 2008, 2010 and 2011, as well as the second-wave groups used as the control groups. The composition and size of the different treatment groups are shown in Table 2. I use a difference-in-differences framework to assess the impact mandatory extension had on union membership by comparing changes in union density between the treatment and control groups after the extension to before the extension. This way, I can estimate what the union membership rate would have been in the treated industries if they had not received mandatory extension. This applies under the conditions of parallel trends, stable unit treatment value, limited anticipation, and irreversibility of treatment. These assumptions will be discussed in detail in Section 7.3.

### 7.1 | Difference-in-differences with variation in treatment timing

The canonical way to explore the treatment effect in a difference-in-differences setting would be the two-way fixed effects (TWFE) linear regression. The TWFE specification would look like this<sup>15</sup>:

$$Union_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 X_{it} + v_i + \delta_t + \varepsilon_{it}.$$

However, to address the problems with this estimator in settings with different treatment timings (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021), I utilize the Callaway and Sant'Anna (CS) estimator for difference in differences with different

treatment timings (Callaway & Sant'Anna, 2021). This is the estimator that the literature cites as being most fitting for my case. It works when the treatment is binary and staggered, it allows for dynamic effects, for comparisons with the not-yet-treated (and not only the never-treated), and for conditioning on time-invariant controls (de Chaisemartin & D'Haultfoeuille, 2022, p. 20). For full transparency, I report results using the classical TWFE estimator, in addition to the Roth and Sant'Anna (2023) estimator and the Sun and Abraham (2021) estimators, in Online Appendix Section A.9.<sup>16</sup>

## 7.2 | Empirical specification

The main specification in the analysis is the following: a difference-in-differences analysis with different treatment timings using the CS estimator for the first wave of industry- and geography-based introductions of mandatory extension of collective agreements in Norway from 2005 to 2011. If an individual worked in construction in the Greater Oslo Area, they were categorized in the 2005 group (G2005) (i.e., these workers received treatment in 2005). If they worked in cleaning, they were in the 2011 group (G2011). For each of these treatment groups, the group time average treatment effect  $ATT(g, t)$  of mandatory extension on union density is calculated based on the difference in the change in union membership between the treatment and control groups. The control group in each case consists of the not-yet-treated groups in addition to the 2015 group that received extension after the research period. For the 2005 group, all industries that eventually received the treatment are categorized in the control group until the point where each group is itself treated. For the 2011 group, only the industries that received mandatory extension in 2015 are categorized in the control group, as all the other groups have already been treated.

The  $ATT(g, t)$  is the average treatment effect for group  $g$  at time  $t$ . The parameter is estimated for every period where  $t \geq g$  and  $t + g \leq U$ , where  $U$  is the last period when at least one group has not received treatment. To estimate the treatment effect at time  $t$  for the group treated at time  $g$ , the development in the outcome for group  $G$  from  $t-1$  to  $t$  was compared to the development of the outcome among the not-yet-treated groups  $n$  in the same period. Using general potential outcomes notation, this estimator can be expressed as follows:

$$ATT(g, t) = E [Y_t(g) - Y_t(0) | G_g = 1], \text{ for } t \geq g.$$

This group-time average treatment effect is then aggregated into different summary measures. Three summary measures are reported: event study aggregation ( $\theta_D^{event}(e)$ ), the average treatment effect across groups ( $\theta_{sel}(\bar{g})$ ) and the aggregation into an overall treatment effect ( $\theta_{sel}^O$ ). This parameter can be interpreted the same way as the average treatment effect on the treated in the classical difference-in-differences setup with two groups. How these aggregations are calculated can be found in Online Appendix Section B. To estimate the causal parameters, I follow the recommendations of Callaway and Sant'Anna in using the doubly robust estimator (Callaway & Sant'Anna, 2021, p. 13).

### 7.2.1 | Variables

The dependent variable is individual union membership, a dummy that equals 1 when the individual is a union member and 0 otherwise. The independent variable is the presence of mandatory



extension of collective agreements in the industry and county where the individual worked. When the industry (and county) in which the individual worked in 2004 received mandatory extension, this individual was coded 1 for the remainder of the time periods. The covariates are age, gender, immigrant status, and education.<sup>17</sup>

### 7.3 | Assumptions

For the CS estimator to be appropriate for the case, certain assumptions must be met: irreversibility of treatment, stable unit treatment value, limited treatment anticipation, and parallel trends. *Irreversibility of treatment*, also known as staggered adoption, means that once a unit gets treated, it will remain treated for the entire analysis period. In my analysis, there are two cases of an industry removing mandatory extension: electric installation in the Oslo and Akershus counties and seven on-shore petroleum plants. These workers are removed from the analysis to avoid violating this assumption. In all other instances, once an industry had introduced mandatory extension, they had mandatory extension for the remainder of the research period. A specification including electric installation in Oslo and Akershus, and therefore stopped in 2010, is reported in Online Appendix Section A.2.

As with every difference-in-differences setup, the *stable unit treatment value assumption* (SUTVA) must be met. For this to be the case, there can be no spillover effects, and the intervention and comparison groups must be stable for the whole time period. Regarding spillover effects, I do not see a clear link between one group receiving mandatory extension and the union density of other groups. Only the other extension industries are used as controls, and even though they are similar in many ways, they are not ‘connected industries’.<sup>18</sup> The stable group assumption does not hold by itself because workers can change industries and move to other parts of the country, thereby basically going in and out of treatment. To address this, I follow standard practice and assign every worker to the industry and county they belonged to in 2004, the year before the first treatment. This is done so that every worker receives treatment only once. Everyone is assigned to their treatment group from 2004 because if people moved into, or out of, one of the early treated groups, it could be considered an effect of the first treatment. Because of this, the assignment into treatment groups should be interpreted as an ‘intention to treat’.<sup>19</sup>

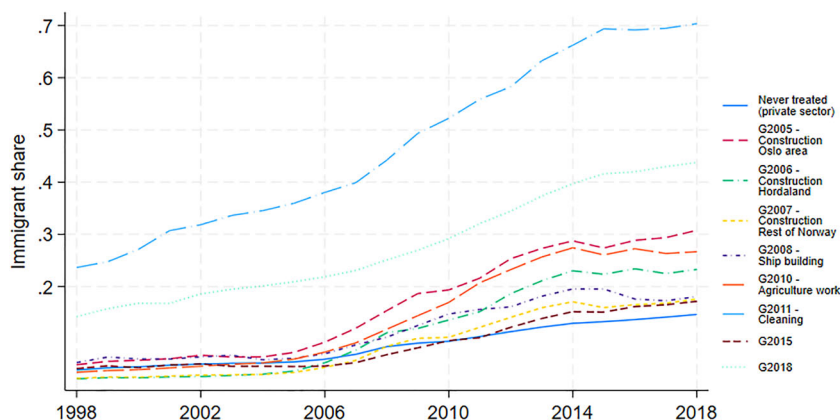
The *limited treatment anticipation* assumption is not fully met, as there was some time between the extension decision request from the union to the extension decision and from the decision to the implementation. We know the request and implementation dates, and the time between request and implementation ranges from about 2–6 months. Since we know the anticipation horizon, we can model it by setting the comparison period from one to 2 years before treatment. These results are reported in Online Appendix Section A.4.

#### 7.3.1 | Parallel trends

The CS estimator assumes parallel trends (possibly conditioned on covariates) between the treated groups and either the ‘never-treated’ or the ‘not-yet-treated’ groups as control groups. For this assumption to be met, it must be credible that the treated and control groups would have shown the same trend for the outcome variable after the time of treatment, if the treatment had not occurred. The first step is to check if the treated and control groups had parallel trends on the outcome variable before treatment, which is discussed in the Results section.

If there is no reason to believe that the ‘never-treated’ units would behave differently after treatment than the ‘eventually treated’ units, the ‘never-treated’ units are a good choice as the control





**FIGURE 4** Immigrant share by treatment group. The graph shows the share of foreign-born workers in the different treatment groups between 1998 and 2018. G2015 is the fishing industry, electrical installation, freight transport by car and passenger transport by bus. G2018 is accommodation, catering, restaurants and bars. [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.

group. However, if the ‘eventually treated’ units could be expected to behave differently than the ‘never-treated’ units, using only the ‘not-yet-treated’ units as the control group is the best option (Callaway & Sant’Anna, 2021, p. 6). In our case, there was a significant difference between the treated and never-treated industries in how they were impacted by immigration after EU enlargement in 2004. As we can see from Figure 4, all the groups that eventually received treatment had significantly steeper growth in foreign-born workers than the never-treated group. This development began around the same time as the first extension event (2005). Therefore, it could be difficult to separate the effects of labour immigration and the effects of mandatory extension since the comparison groups were not impacted by immigration in the same way.

There is reason to believe that the considerable increase in the immigrant share, mostly from ‘low-wage’ countries, in these industries influenced the conditions for the workers employed in these industries before 2004, including union membership.<sup>20</sup> Therefore, the units that eventually will receive treatment but have not yet received treatment (the ‘not-yet-treated’ group) are the control group that most credibly would have had parallel trends in union membership if they had never received treatment. For robustness, a specification with the whole private sector as the control group is reported in Online Appendix Section A.5.

## 8 | DESCRIPTIVE STATISTICS

Table 3 shows that in 2004, the industries that eventually receive treatment generally had lower union density, lower wages, less education, more men, more immigrants (with exceptions) and lower worker age compared with the rest of the private sector. The big outlier among the treatment groups was the 2008 group (shipbuilding), where the union density, mean wage and age were higher than for the never-treated units.

Table 4 shows that the wage gap between union members and non-union members was not significantly different between the never-treated and most of the treatment groups, except for construction in the Greater Oslo Area, where non-members earned significantly less than union

TABLE 3 Descriptive statistics by treatment group.

Year treated	Union density	Hours per week	Hourly wage	Low education	Middle education	High education	Students	Men	Immigrants	Age
Never	0.436	32.869	199.953	0.372	0.375	0.244	0.083	0.621	0.053	40.747
2005	0.372	36.441	187.514	0.38	0.495	0.107	0.05	0.917	0.065	40.396
2006	0.244	36.289	156.783	0.386	0.562	0.042	0.081	0.929	0.032	37.456
2007	0.28	36.219	159.477	0.412	0.522	0.058	0.059	0.934	0.032	38.304
2008	0.689	36.759	211.520	0.297	0.502	0.194	0.032	0.888	0.06	42.392
2010	0.113	21.695	113.398	0.573	0.322	0.093	0.212	0.649	0.054	37.02
2011	0.37	27.592	139.488	0.591	0.249	0.078	0.07	0.41	0.345	38.492
2015	0.378	35.61	167.571	0.424	0.491	0.072	0.066	0.852	0.047	39.163
2018	0.209	25.714	141.080	0.44	0.384	0.129	0.222	0.387	0.203	32.872

Note: This table shows the mean values for relevant variables in the different extension groups the year before the first extension event (2004). G2005 is construction in the Greater Oslo Area. G2006 is construction in Hordaland county. G2007 is construction in the rest of the country. G2008 is shipbuilding. G2010 is agricultural work. G2011 is cleaning. G2015 is the fishing industry, electrical installation, road freight transport, and bus drivers. G2018 is accommodation, serving, and catering. Source: Administrative data from Statistics Norway.

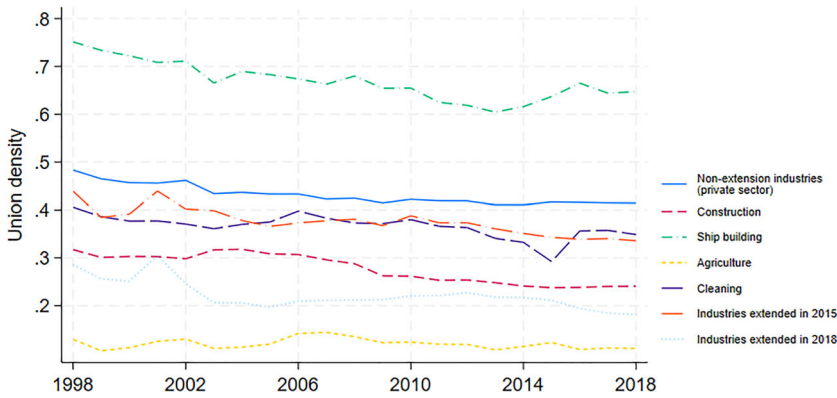
TABLE 4 Wage inequalities, by treatment group descriptive statistics – by (year treated), year = 2004.

Year treated	Hourly wage	Wage native	Wage immigrant	Wage union	Wage non-union	P50/P10	GINI
Never	199.953	200.147	196.498	194.19	192.286	0.612	0.273
2005	187.514	189.161	163.889	198.6	180.942	0.514	0.247
2006	156.783	156.953	151.678	158.671	156.174	0.476	0.216
2007	159.477	159.779	150.218	163.651	157.857	0.486	0.233
2008	211.520	211.797	207.140	206.611	222.418	0.592	0.243
2010	113.398	112.855	122.954	136.867	110.404	0.209	0.388
2011	139.488	140.077	138.371	136.995	140.952	0.291	0.330
2015	167.571	168.462	149.627	170.037	166.071	0.539	0.235
2018	141.080	142.966	133.666	162.088	135.538	0.484	0.207

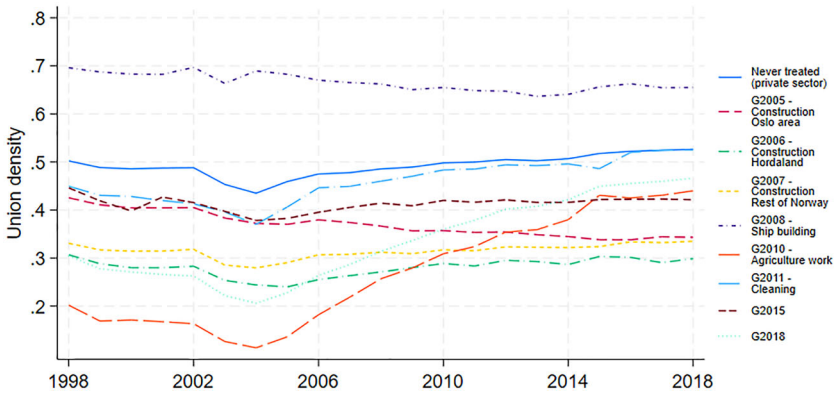
Note: This table shows the mean values for relevant wage and inequality measures in the different extension groups the year before the first extension event (2004). G2005 is construction in the Greater Oslo Area. G2006 is construction in Hordaland county. G2007 is construction in the rest of the country. G2008 is shipbuilding. G2010 is agricultural work. G2011 is cleaning. G2015 is the fishing industry, electrical installation, road freight transport, and bus drivers. G2018 is accommodation, serving, and catering. Source: Administrative data from Statistics Norway.

members, and in shipbuilding, where non-members had a higher mean wage than the union members. This pattern also held when controlling for age.

As Figure 5 shows, all industries that received treatment, except shipbuilding, had significantly lower union density than the rest of the private sector. The trend is a slow decline in union density for almost every industry. This figure reflects the actual union density in the whole population in these industries. However, since every individual is coded with their industry in 2004 for the whole sample, the evolution in union density that was used in the analysis can be seen in Figure 6. Here, we follow the same individuals from 2004 onward. Since union membership tends to increase with age, we actually observe a modest rise in union density in most of the treatment groups. An eventual negative effect we measure from mandatory extension on union membership should



**FIGURE 5** Union density by industry. The graph shows the trends in the union density (union membership rate) in the industries with mandatory extension and in the rest of the private sector in Norway between 1998 and 2018. Individual union membership is automatically tax deductible and therefore part of the tax register. The industries extended in 2015 are the fishing industry, electrical installation, freight transport by car and passenger transport by bus. The industries extended in 2018 were accommodation, catering, restaurants and bars. [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)] *Source:* Administrative data from Statistics Norway.



**FIGURE 6** Union density by treatment group. The graph shows the trend in the union density (union membership rate) in the different treatment groups (the individuals receiving treatment in the same year) between 1998 and 2018. Individual union membership is automatically tax deductible and therefore part of the tax register. G2015 is the fishing industry, electrical installation, freight transport by car and passenger transport by bus. G2018 is accommodation, catering, restaurants and bars. We followed the same individuals from 2004 onward. Since union membership tends to increase with age, we observed a different development for this sample than for the full population sample. [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)] *Source:* Administrative data from Statistics Norway.

therefore not be interpreted as a decrease in union membership but as a relative decline in the tendency to join a union with age. The results do not necessarily reveal that people left unions, but that fewer people joined unions.

TABLE 5 Mandatory extension aggregated treatment effect estimates.

All skill-levels						
Simple weighted average (ATT)	−0.027* (0.009)					
Group-specific effects	g = 2005	g = 2006	g = 2007	g = 2008	g = 2010	g = 2011
	−0.047* (0.011)	−0.002 (0.005)	−0.011 (0.006)	−0.029* (0.006)	0.005 (0.004)	−0.006 (0.003)
Event study	e = 0	e = 1	e = 2	e = 3	e = 4	e = 5
	−0.006* (0.001)	−0.013* (0.003)	−0.018* (0.004)	−0.028* (0.008)	−0.031* (0.010)	−0.035* (0.015)
Low skill						
Simple weighted average (ATT)	−0.026* (0.006)					
Group-specific effects	g = 2005	g = 2006	g = 2007	g = 2008	g = 2010	g = 2011
	−0.042* (0.008)	−0.003 (0.004)	−0.014 (0.006)	−0.036* (0.005)	0.001 (0.006)	−0.009 (0.004)
Event study	e = 0	e = 1	e = 2	e = 3	e = 4	e = 5
	−0.007* (0.002)	−0.013* (0.003)	−0.017* (0.003)	−0.026* (0.007)	−0.031* (0.007)	−0.037* (0.008)

Note: The table shows the values for the different aggregated treatment effect parameters. The “simple weighted average” shows the average of all the group-time average treatment effects weighted by group size. The “group-specific effects” row summarizes the average treatment effects for the different treatment groups, where  $g$  indicates the year the group was first treated. The “event study” row shows the average treatment effects across all treatment groups according to the length of exposure to mandatory extension, where  $e$  indicates the number of years after the introduction of mandatory extension (Callaway & Sant’Anna, 2021, p. 20). The estimates use the doubly robust estimator. The table layout is borrowed from Callaway and Sant’Anna (2021, p. 20).

\*Indicates 5 per cent significance level.

## 9 | RESULTS

The results are presented in the following order: first, the effect estimates for the three aggregations are presented to provide a general overview of the effects. Then, the event plot based on the  $\theta_D^{event}(e)$  is presented, with which we can assess the pre-trends and the extent of dynamic effects *on average*. Lastly, the plots for the ATT ( $g, t$ ) for each group are presented to highlight the difference in the treatment effect across industries and geographical location.

Table 5 shows that the overall effect of mandatory extension of collective agreements on union density is negative and significant at −2.7 percentage points. The effect is similar for both skill groups. However, the estimates for the low-skill group have smaller confidence intervals.

We see the same information in the aggregated event plots (Figure 7), where there is little difference in the pretreatment trends between the treatment and control groups before treatment and a clear effect after treatment. These findings support the free-rider hypothesis.

### 9.1 | Group-time average treatment effect

When we look at the coefficients and plots of the group-time average treatment effects for each treatment group (see Figure 8), the picture is more nuanced. Two treatment groups show a clear

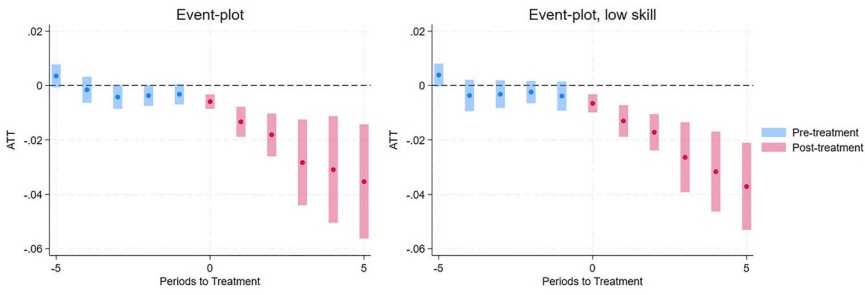


FIGURE 7 Aggregated event plots, all skills and low skill. The figure shows the event-study aggregations of the average treatment effects across all treatment groups by the length of exposure to mandatory extension. [Colour figure can be viewed at wileyonlinelibrary.com]

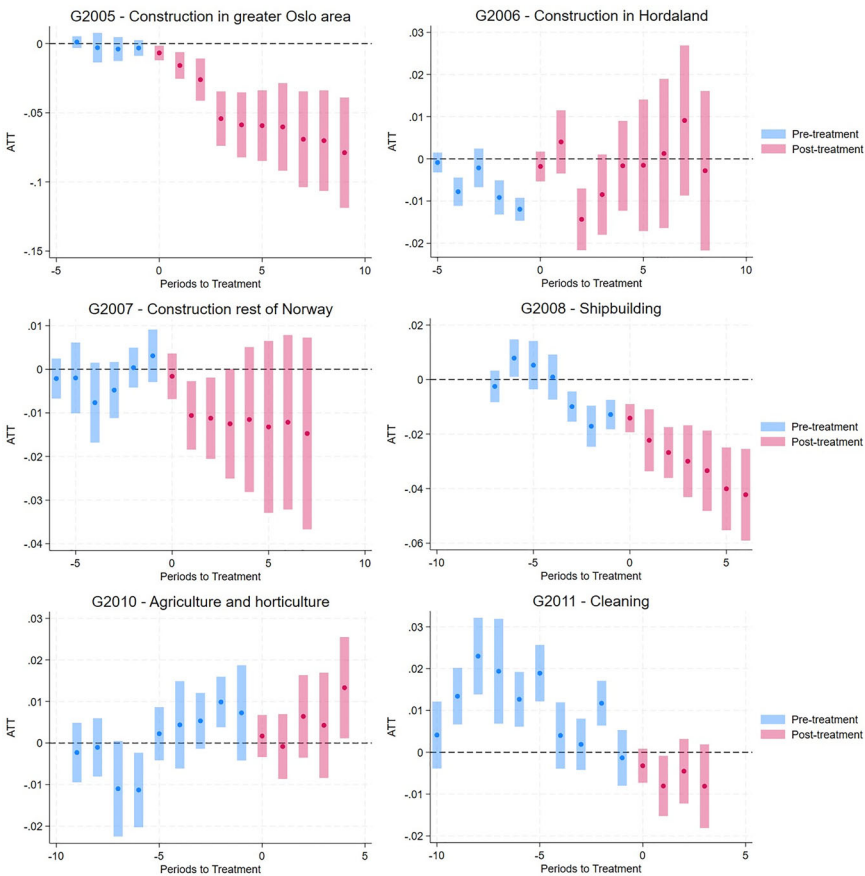


FIGURE 8 (a) The figure shows the group-time average treatment effect plots by treatment group and by the length of exposure to mandatory extension, where  $e$  indicates number of years after the introduction of mandatory extension for all skill levels. (b) The figure shows the group-time average treatment effect plots by treatment group and by the length of exposure to mandatory extension, where  $e$  indicates number of years after the introduction of mandatory extension for the low-skill sample. [Colour figure can be viewed at wileyonlinelibrary.com]

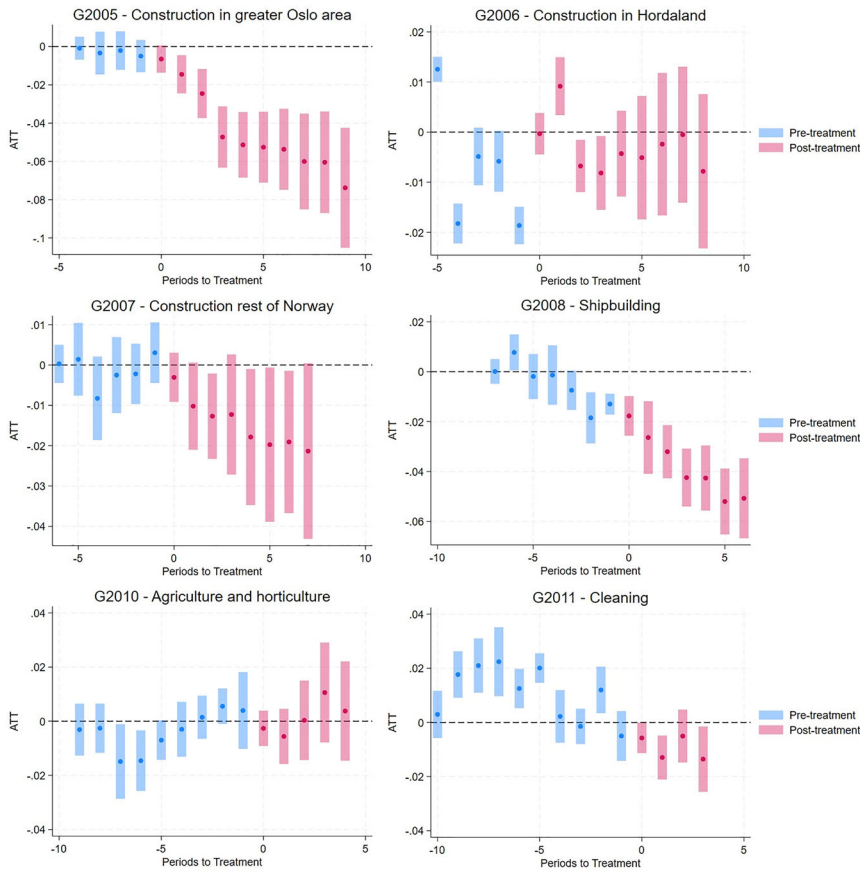


FIGURE 8 Continued

negative effect: construction in the Greater Oslo Area and shipbuilding. The overall effect in G2005 (construction in the Greater Oslo Area) is estimated at  $-4.7$  percentage points, and for G2008 (shipbuilding) the effect is  $-2.9$  percentage points. There is a small but not significant negative effect in G2007 (construction, rest of Norway) as well. This is clearer in the low-skill estimation but not significant here either. The rest of the groups show no significant effect either way. This indicates treatment heterogeneity: the effects of the treatment varied across different units.<sup>21</sup>

## 10 | DISCUSSION

The average effect of the introduction of mandatory extension of collective agreements on union membership rates is estimated to be negative at  $-2.7$  percentage points. These findings are in line with the literature and support the free-rider theory of how workers react to mandatory extension. In other words, the findings indicate that the free-rider mechanism has a stronger negative effect than the possible positive effects of less employer hostility or union access to immigrants and other precarious workers. This effect is estimated based on the workers who were already employed in these industries before the first introduction of mandatory extension in 2005. The effect is therefore not estimated based on most of the foreign workers who came to Norway after

the eastwards expansion of the EU in 2004 because these two events happened simultaneously. The results should therefore be interpreted as the effects of mandatory extension on the union density of the workers who worked in these industries before the EU expansion. How it affected the unionization patterns of the new labour immigrants is therefore not known. However, because of this, we can be sure that the results are not caused by the fact that immigrants have a lower probability of being union members in the first place (compositional effects).

Because we are dealing with control groups and potential outcomes, the interpretation of the results is that the union membership rate among the workers who worked in the treated industries before 2005 is 2.7 percentage points lower than it would have been if mandatory extension was never introduced. As noted in Section 8, this effect should not be interpreted as workers leaving unions but as fewer workers joining the unions than would have been the case if mandatory extension had never been introduced.

A negative effect of  $-2.7$  percentage points is a substantial effect size when you consider the historic stability of the union membership rate in Norway, even in the private sector and the industries that received extension (as seen in Figures 1 and 5). A negative effect of 2.7 percentage points in the extension industries constitutes a 0.4 per cent reduction in the private sector as a whole (15 per cent of the total union membership decline of 2.2 percentage points in the private sector between 2004 and 2014) and 61.3 per cent of the total decline in union membership rates in the extended industries (4.4 percentage points). The question is therefore whether the cure was worse than the problem it tried to solve. If mandatory extension leads to lower union membership rates and lower union membership leads to worse wages for the lowest paid, the workers who are supposed to benefit from this institution could be worse off in the long run. We see that there is a trade-off involved and that policymakers and social partners should be aware of this when discussing further expansion of mandatory extension. It is also important not to think about this institution in isolation, as the negative effects of some institutions can be offset by the beneficial effects of others, such as the pro-union effects of the Ghent systems (Böckerman & Uusitalo, 2006; Rasmussen & Pontusson, 2018).

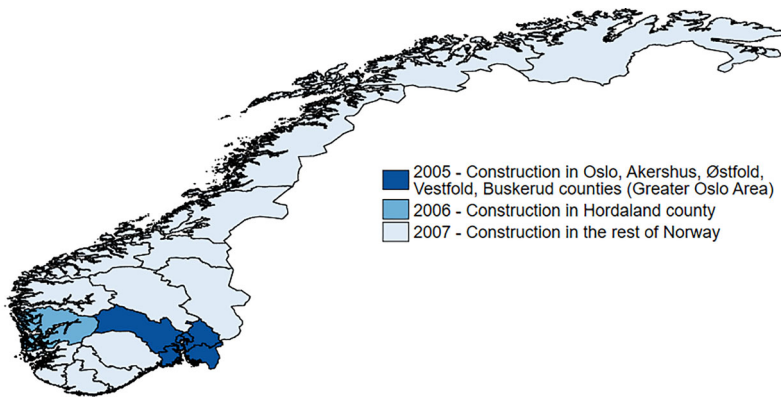
## 10.1 | Differences in the effects across industries and geographical locations

Even though the average treatment effect is negative, there are differences between the treatment groups in terms of the group-specific effects. In the following section, I discuss the differences in the treatment effects across groups based on the expectations derived from the theory.

In the analysis, individuals were coded with their treatment group in 2004, the year before the first treatment. As time went on, the treatment groups are less precisely estimated because individuals could move to another industry between 2004 and the treatment year. Therefore, the early treated groups are the ones that are most precisely estimated. We see from the individual group-time treatment effect plots that the pre-trends are more parallel and that the confidence intervals are smaller for the first treated group, compared with the other groups. If the later-treated groups are less precisely estimated, it would be less likely to find a significant effect. It is therefore possible that some of the heterogeneity stems from the fact that the earlier-treated groups are more precisely measured.

Perhaps the most striking difference in treatment effect is that between construction in the Greater Oslo Area (G2005) and construction in the rest of the country (both G2006 and G2007). The county-by-county rollout is illustrated in Figure 9. There is no negative effect to be found





**FIGURE 9** The county-wide introduction of mandatory extension in construction. The map shows the county-wide introduction of mandatory extension of collective agreements in the construction industry. After this, all other introductions of mandatory extension were nationwide.

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

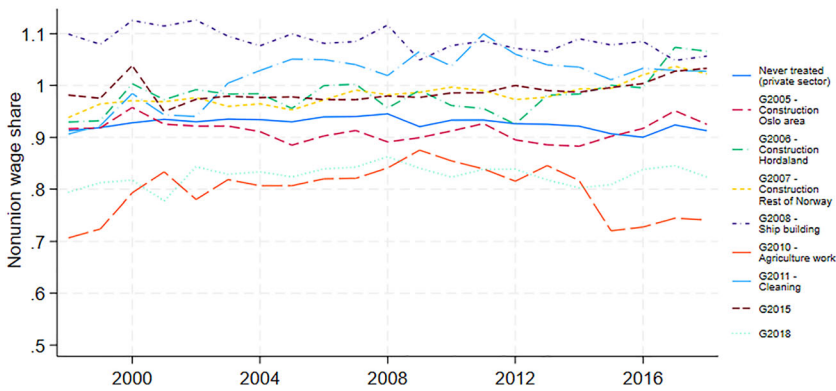
Source: Eldring et al. (2011) and Bergsli (2016).

in G2006, and a small but not significant negative effect is to be found in G2007. These are all workers in the construction industry, and what separates them is geography and 1 or 2 years of treatment timing. Understanding the differences between these groups that should be very similar, and why the effect was different in these groups could elucidate the mechanisms behind the effects of mandatory extension on union density. The treatment timing was very close, which reduce the differences stemming from calendar time effects. Since these are the earliest treated groups, these are also the most precisely estimated groups. The fact that these groups were in the same industry and distinguished only by being in different parts of the country reduces the possible industry-specific treatment effects.

Online Appendix Sections A.6 and A.7 report a split sample specification where every treated industry is estimated separately for the Greater Oslo Area and the rest of the country. The results are similar to those reported above, indicating that there is no systematic difference between the Greater Oslo Area and the rest of the country across all industries. The differences in the treatment effects between the construction groups must therefore stem from characteristics of the construction industry in the different geographical areas.

The first thing to note is the wage gap between union members and non-members. As we can see from Table 4 and Figure 10, the wage gap between union members and non-members was significantly larger in G2005 than in G2006 and G2007. In G2005, the non-union wage was 91 per cent of the union wage. In G2006, it was 98 per cent, and in G2007, it was 96 per cent. This pattern holds for both the whole industry and for only the low-skilled sample as well as when controlling for age. Figure 10 shows that the non-union wage as a share of the union wage was markedly lower in G2005 than G2006 and G2007 over the whole period and that the fall in the non-union wage share stopped in 2005, the treatment year. This is in line with the expectations based on the free-rider theory. If we treat the mean union wage as a proxy for the collective wage, a larger wage gap before the treatment would lead to a larger incentive reduction for union membership after treatment.

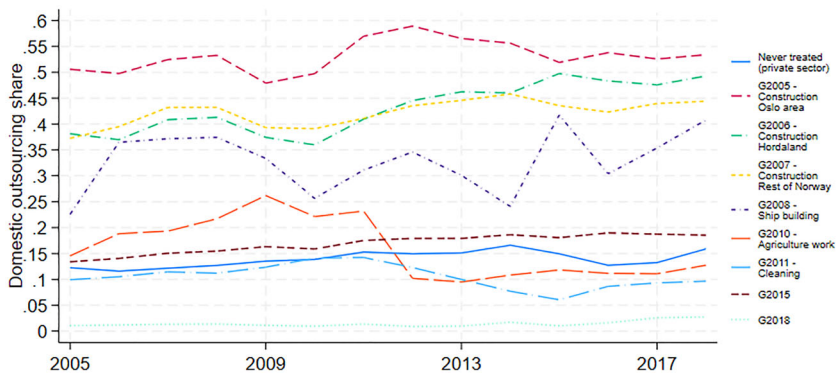
When it comes to the degree of dualization in the labour market, Figure 11 shows that the three construction groups had the highest share of domestic outsourcing costs from total production



**FIGURE 10** Non-union wage share by treatment group. The graph shows the mean hourly wage for non-union members as a share of the mean hourly wage for union members in the different treatment groups between 1998 and 2018. Individual union membership is automatically tax deductible and therefore part of the tax register. G2015 is the fishing industry, electrical installation, freight transport by car and passenger transport by bus. G2018 is accommodation, catering, restaurants and bars.

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.



**FIGURE 11** Domestic outsourcing share by treatment group. The graph shows the spending on domestic outsourcing (subcontractors and temp agency workers) as a share of total labour costs (domestic outsourcing costs + wage costs) in the different treatment groups between 2005 and 2018. G2015 is the fishing industry, electrical installation, freight transport by car and passenger transport by bus. G2018 is accommodation, catering, restaurants and bars. Specifically, 2005 was the first year with information about firm accounts.

[Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

Source: Administrative data from Statistics Norway.

costs<sup>22</sup> in 2005 (the first year for this statistic). This is to be expected because the construction industry in general engages in a lot of subcontracting. However, we can also see that the construction industry in the Greater Oslo Area had a markedly higher domestic outsourcing share than in the two other groups. Thus, a higher share of the economic activity occurred among subcontractors and temporary work agencies in construction in the Greater Oslo Area than in the other groups. This indicates that a higher share of workers worked as subcontractors or as employees of temporary work agencies. If these workers to a larger extent were not a part of the 'normal' labour

market, they would have been disproportionately positively affected by mandatory extension of collective agreements.

We can, therefore, observe a clear difference in the degree of domestic outsourcing and in the wage gap between union members and non-members. This indicates that the construction industry in the Greater Oslo Area is a more divided labour market context than the construction industry in the rest of the country. That the treatment effect is stronger in construction in the Greater Oslo Area than in construction in the rest of the country is therefore in accordance with the theoretical expectations.

Lastly, there is the case of the shipbuilding industry, where we observe a large negative effect on union density. This treatment group is somewhat of an outlier compared to the other treated industries. It had higher wages and higher union density than the average in the whole private sector. Non-union members also earned more than union members on average. This tells us that the effects are present also in contexts where there is theoretical reason to believe that the effect would not be so large.

## 10.2 | Generalization

The difference-in-differences framework generally has strong internal validity. External validity, however, must be argued on a case-by-case basis. As for how these findings could be generalized to other countries or settings, it is important to bear in mind that wage negotiations at the industry level are a precondition for the use of mandatory extension (Hayter & Visser, 2018, p. 1). Regarding the effects of the introduction of mandatory extension in a country, only two OECD countries that engage in substantial industry-level wage bargaining do not use mandatory extension: Sweden and Denmark (OECD and AIAS, 2023). The wage formation processes in these countries are very similar to that of Norway (except for union-administered unemployment benefits). Therefore, these results should be relevant for Sweden and Denmark, as introducing mandatory extension has been discussed in both countries (Bruun, 2018, p. 119) but also in countries that are considering expanding mandatory extension to new industries.

One of the key findings of this analysis is that the effects are larger in more unequal labour market contexts. This will strengthen the case for the results being generalizable to more unequal labour markets than that of Norway. In addition, the Norwegian extended collective agreement is also 'weaker' than the normal collective contract, while in most other countries, the extended contract and the union contract are the same (as explained in Section 4.1). This reduces free-rider incentives because there are still benefits with the collective contract workers do not get from the extended contract. Therefore, this effect could be regarded as the lower bound and should be expected to be more significant in other countries.

## 11 | CONCLUSION

Mandatory extension of collective agreements is a very common labour market institution in large parts of the world and is a policy mechanism used to secure adequate working conditions for workers not governed by a collective agreement. Even though mandatory extension is very widespread and has been perceived to affect union density negatively, there has not been much research on the relationship between mandatory extension and union membership. One of the reasons for this is probably that most countries introduced mandatory extension before 1960. In addition to the

advantages of the late adoption of mandatory extension in Norway in 2004 in terms of data quality, this reform is especially interesting because it is explicitly related to challenges arising from the increased labour immigration caused by EU enlargement in 2004. This article is therefore a study of the consequences of the institutional response to labour immigration from the government and social partners. It is also an example of an 'old' institution being utilized to solve 'modern' problems and can tell us a lot about the relevance, and consequences, of mandatory extension in a modern labour market context. This study is therefore theoretically relevant in understanding the relationship between labour market institutions and unionization in the modern context. Additionally, it is politically relevant because the Norwegian experience should be relevant to other similar countries, such as Sweden and Denmark, that have not (yet) introduced mandatory extension. By using a difference-in-differences with variable treatment timing framework, I was able to leverage the variation in treatment status and treatment timing to estimate the effects of the Norwegian introduction of mandatory extension on the union density in the treated industries. The findings showed an overall negative effect of mandatory extension on union density of about -2.7 percentage points, lending credence to the free-rider theory. This finding is in line with the previous comparative literature on the relationship (Blaschke, 2000; Checchi & Lucifora, 2002) but contradicts research on the Norwegian case, where Benedictow et al. (2021) found no effect. These findings do not necessarily contradict the study proposing that mandatory extension could have a positive impact on the unionization of labour immigrants in Norway (Eldring & Hansen, 2009) because most of the labour immigrants in Norway could not be included in the analysis, as they came to Norway after the first extension event. Even though the overall effect was negative, there were differences in the treatment effect across the treatment groups. In some groups, the effect was minimal, and in some groups, the effects were negative and significant. The evidence points to the negative effect being larger where the inequalities between non-union members and the collective wage were more significant before the introduction of extension. This is in line with the theoretical expectations based on the free-rider theory. The negative effects could also be found in other labour market contexts than this one, indicating that the negative relationship is valid in different circumstances. Policymakers and social partners should therefore be aware of the specific conditions in various industries when assessing the possible introduction of mandatory extension of collective agreements. If not, they could risk the cure being worse than the problem it is meant to solve.

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### CONFLICT OF INTEREST STATEMENT

The author declares no conflicts of interest.

## DATA AVAILABILITY STATEMENT

The data supporting this study cannot be made available due to privacy concerns, as they are individual-level administrative data provided by Statistics Norway on the condition that the data will not be shared.

## ETHICS STATEMENT

The study uses anonymized individual-level administrative data, provided by Statistics Norway. The data have been stored in accordance with the Norwegian Agency for Shared Services in Education and Research guidelines, and only aggregate data are presented in the article so that it is not possible to identify individuals.

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## ENDNOTES

- <sup>1</sup>See Checchi and Lucifora (2002) and Blaschke (2000) for comparative research on the relationship between mandatory extension and union membership and Benedictow et al. (2021) and Eldring and Hansen (2009) for research on the Norwegian case.
- <sup>2</sup>Mandatory extension was introduced in Czechia, Estonia, Hungary, Poland, Slovakia and Slovenia in the 1990s as a part of the transition towards a market economy (Hayter & Visser 2018).
- <sup>3</sup>Checchi and Lucifora (2002) used a measure of mandatory extension that assigned Norway, Sweden and Denmark with the highest scores on the variable because 'agreements are extended through centralization'. These three countries did not use mandatory extension during the research period, so this is clearly not a good measure of the presence of mandatory extension in a country. Blaschke (2000) did not include a separate variable for mandatory extension, instead opting for a combined variable called 'Statutory Works Council plus Pervasive Extension Practice'.
- <sup>4</sup>If there is an institution for employee influence outside of unions, such as the work councils in Germany, the pure economic calculus of a union membership could be more important for workers. This could possibly lead to a larger negative association between mandatory extension and union density than in settings where the unions are the main channel for worker influence in the workplace (i.e., Norway).
- <sup>5</sup>Measured by share of 'short jobs' under 1 year.
- <sup>6</sup>A minimum wage is usually decided by politicians and is the same rate for the whole country (or state) and all industries. However, mandatory extension is when the collective agreement between labour unions and employer organizations in an industry is made statutory. It is only statutory in that industry and can also be geographically restricted. The minimum wage is also in most cases significantly lower than the extended collective wage. There is no minimum wage in Norway.
- <sup>7</sup>'The Greater Oslo Area' refers to the counties Oslo, Akershus, Østfold, Buskerud and Vestfold.
- <sup>8</sup>The fact that these extensions happened a long time after the first extensions, that the nature of the extensions are different and that the reporting practises for the administrative data changed from 2015 makes 2014 a natural end point for the analysis. This is further elaborated in Section 6.
- <sup>9</sup>This is not specified in the law but has been established as a custom by the Extension Committee.
- <sup>10</sup>This is discussed in Section 5.2.
- <sup>11</sup>In 2015, the reporting regime for employers to the tax authorities changed. The reporting is now done monthly, and employers now must report jobs shorter than seven days or with fewer than four contracted hours per week. This increases the number of jobs in the data and makes it difficult to compare levels in many of these variables before and after 2015.
- <sup>12</sup>Combined with the county of the company for the extensions in construction.
- <sup>13</sup>NACE Rev. 1 through 2001, NACE Rev 1.1 from 2002 through 2008, and NACE Rev. 2 from 2009.
- <sup>14</sup>The two first extensions were county- and industry-specific. From 2007 onward, all extensions were nationwide.

- <sup>15</sup> Where  $Union_{it}$  is the union membership status of individual  $i$  in year  $t$ .  $D_{it}$  is the variable indicating the status of mandatory extension.  $D_{it} = treatment_i * post_{it}$ , and  $treatment_i = 1$  if the individual worked in an industry that received mandatory extension during the sample period and 0 otherwise.  $post_{it}$  is a post-treatment indicator, taking a value of 1 if  $t \geq t_{i0}$ , where  $t_{i0}$  is the time when the individual first got treated. Otherwise, it is 0.  $X_{it}$  is a set of time-varying covariates.  $\nu_i$  and  $\delta_t$  are individual and year-fixed effects, respectively. The error term is  $\varepsilon_{it}$ .
- <sup>16</sup> For transparency, in Online Appendix Section A.9, results for other estimators are reported: the classical TWFE estimator, the Roth and Sant'Anna 'efficient estimator for staggered rollout designs' (Roth & Sant'Anna 2023), and the Sun and Abraham (2021) estimator. The Callaway and Sant'Anna estimator is more suitable to our case than these estimators because the TWFE estimator is not reliable when analysing staggered rollouts with heterogeneous treatment effects (because it utilizes 'illegal comparisons') (Goodman-Bacon 2021). The Roth and Sant'Anna estimator imposes a stricter assumption of 'random treatment timing', which is not necessarily applicable in our case. Lastly, the Sun and Abraham estimator can only compare the never-treated and not the 'not-yet-treated', which is an important feature of the Callaway and Sant'Anna estimator that is used in the article.
- <sup>17</sup> The specification with covariates is reported in Online Appendix Section A.1. Gender is represented by a dummy that was 1 if the individual was registered as male and 0 if registered as female. Immigrant status also took the form of a dummy, where 1 meant the individual was born in a different country than Norway. Education was represented by dummies indicating low, medium, high or ongoing education. The individual was coded with low education if they only finished primary school, medium education if they only finished high school, and high education if they finished three or more years of higher education.
- <sup>18</sup> One possible spillover scenario could be that expectation about future adoption increased also in non-treated industries, and workers and unions started adapting accordingly. This can be seen as a version of treatment anticipation, which is tested for in Section A.4 in the Online Appendix. Another is spatial spillovers: mandatory extension in construction in the Greater Oslo Area could have affected union density in nearby counties that had not yet received treatment. In Online Appendix Section A.8, a specification where the neighboring counties are assigned treatment in 2005 and 2006 is reported. No significant effect was found, reducing the fear of spatial spillovers in the construction industry.
- <sup>19</sup> This necessary, and standard, practice creates some problems on its own. For every year that passes, fewer and fewer workers would be coded with the industry in which they actually work. This could make the assignment of the treatment less precise, especially for the groups that are treated late, as can be seen in Table 2. The assignment into treatment groups should therefore be interpreted as an intention to treat. Estimations where every worker is assigned to the treatment group the year before their own treatment time are reported in Online Appendix Section A.3.
- <sup>20</sup> This analysis does not capture the direct effect of immigration caused by the EU expansion on union membership because workers that came to Norway after 2004 were not a part of the analysis.
- <sup>21</sup> This shows the importance of the disaggregated measures in the Callaway and Sant'Anna approach.
- <sup>22</sup> We do not know to which industry or company workers employed in temp agencies were posted. Therefore, the domestic outsourcing share is used as a measure of the extent of 'hired labour' (temp agencies and subcontracting) in the treatment groups.

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## SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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