Strike, wage bargaining and the cost of conflict

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Abstract

Strike as collective action is a right embedded in both EU and Norwegian legislation, implicitly embedding the idea that participating in a strike should not affect long-term career outcomes. After a strike in 2016, rights to local bargaining were introduced in the Norwegian hotel-and restaurant sector, where bargaining previously only occurred centrally. Applying Norwegian administrative linked-employer-employee data from 2015-2019, we show that relative to workers in retail trade, where wages are set centrally, hotel-and-restaurant-workers at workplaces covered by trade union agreements experienced negligible wage changes in levels, but increased wage compression at the top. The striking workers, however, experienced lower hourly wages, weaker labour market attachment, and higher probability of receiving welfare benefits. Thus, for these workers, the strike brought long-term costs in addition to those cost experienced under the strike.

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1. Introduction

Strike as collective action is a right embedded in both EU and Norwegian legislation. For the EU, the right to strike is both ensured by the Article 28 of the Charter of Fundamental Rights, but also The European Convention on Human Rights (eg. Article 11). The European Social Charter, adopted by the Council of Europe, further emphasizes the importance of collective bargaining and the right to take collective action, including strikes. How strikes are regulated vary between the EU countries.[1] In Norway, the right to strike for union members is ensured by several laws and ILO-conventions. Arguably this is ensured by having the human rights incorporated into the National Convention, but also The Norwegian Labour Dispute Act § 3 states that unions can implement strikes. Strikes are quite common under the central wage bargaining process between unions and employer organisations, which occurs biannually. In principle, unions work under peace obligations between these negotiations. A key aspect of the right to strike is that participating in strikes should not have detrimental impact on workers' long-term careers.

However, whether this is actually the case, is not known. In the literature on strikes, this topic is close to never addressed, at least partly because public administrative registers usually do not comprise information allowing researcher to identify striking workers. To remedy this shortcoming in the literature, in this paper we exploit information on a specific strike in Norway allowing us to identify striking workers and study the long-term outcomes from the strike.

Since 2014, the blue-collar unions in the Norwegian hotel and restaurant sector fought for the right to bargaining wages locally (NHO_Fellesforbundet 2015). For workers and establishment covered by a trade union agreement in this sector, wages were completely determined through bargaining centrally, between the union and employer organisation. The employer organisation, NHO-reiseliv, argued that in an industry characterised by low unionisation and no culture for local bargaining, this would be impossible or difficult to implement. April 23, 2016, 3450 workers who

[1] https://www.europarl.europa.eu/RegData/etudes/STUD/2024/757656/IPOL_STU(2024)757656_EN.pdf

were members in Fellesforbundet – the biggest private sector labour union - went on strike in the hotel and restaurant sector, primarily for the right to bargain wages locally (Seip, 2018). However, most of the workers in the sector were non-unionised, and few businesses were forced to close. Still, as the time went, more members joined the strike, increasing the inconvenience for employers, and when the employer organisation yielded to the union demands, over 7200 members were on strike. The Norwegian strikers, however, did not primarily fight for wage gains, but the right to bargain locally, although underlying union demands, was the notion that local bargaining also would yield higher wage on average. Through the agreement following the strike, additional local bargaining would be permissible from negotiations 2017 and onwards. In the period that followed, unions successfully worked for the establishment of mandatory extensions of the collective agreement, which was put in place from January 2018, effectively ensuring minimum wages in the sector.

Thus, in this paper, we study the impact of the strike workers' careers and of changed bargaining rights in the Norwegian hotel and restaurant-sector, towards a more decentralised wage setting with local bargaining rights. Our analyses are based on difference-in-different event study design. To have a plausible control sector, we use data from retail trade, a sector recognised for centralised wage bargaining for those workplaces covered by a trade union agreement. Due to the unionisation structure in our treatment sector, where most of the workers were non-unionised, we can endorse the varying treatment intensity approach of Angrist and Imbens (1995) to identify causal effects. First, we study how wages and wage dispersion in the hotel and restaurant sector for all jobs changes as local bargaining rights are introduced, and second, for the striking workers, we study the impact of being on strike. By applying monthly population wide data from Norway, we can reveal what the union achieved by the strike at what cost.

Strikes have economic consequences for employers and workers. Work is suspended, workers receive no pay and employers do not receive whatever labour contributes to in production. Strike is "a temporary stoppage of work by a group of employees in order to express a grievance or enforce a

demand (Hyman, 1972:17). However, strikes do not occur in competitive markets. Pre-requisites for a strike are first, that a surplus exists that the workers and employers disagree on, and second, that one of the parts in the strike has private information on the size of the surplus to be divided (Kennan, 1986). Truly competitive markets are few, and usually the wage paid to workers in private sectors are less than the value of labour. Strikes could also act as a signalling or recruitment device (Hicks, 1963; Cregan, 2013; Hodder et al., 2017). In public sectors, the dynamics of strikes are different, in that the economic surplus that workers and employers bargain over, is not clear, and that different unions might have competing interests (Scheuer et al., 2016).

Strike (and the treat of strike) has been one of the key sources of union power (Card and Olson, 1995; Kimeldorf, 2013). However, the last decades in the U.S., this has seldom been successful (Rosenfeldt, 2014). Before 1980s, Massenkoff and Wilmers (2024) find that strikers enjoyed 5-10 percent wage gains, but close to no gains after 1980. In addition, for over 40 years it has been recognised that strikes and lockouts are associated with stress and detrimental mental health outcomes such as depression, anxiety and irritation (Barling and Milligan, 1987; Fowler et al., 2008). Thus, other kinds of industrial action than strikes might thus has become more attractive (Gall, 2013), although the evidence for this is not clear (Gall and Kirk, 2018). Furthermore, restrictions in some countries such as the UK Trade Union Act of 2016 (Qc and Novitz, 2016) limit how and when trade unions might apply industrial action such as strikes (Gall and Kirk, 2018). In Norway, industrial action such as strikes is limited for firms' part of trade union agreements since these firms and their workers face an embargo on strikes and lockout between biannual key negotiations (occurring every second year).[2] Participation in strikes is affected by different norms reflecting solidarity and free-rider punishment (Akkerman et al., 2013). However, industrial action and particularly strikes might also be a way to bolster union membership, potentially by showing workers what unions do. For example, Cregan (2013) and Hodder et al (2017) found that strike action induced periods of higher trade union

[2] The right to strike in Norway follows from verdicts from the European Human Rights Court, ILO-conventions, the Norwegian Labour Disputes Act and the Services Disputes Act (Moen, 2022).

membership growth. Strike preparation might be interpreted as union effectiveness, which are know to recruit workers into unions (Clark, 2009).

Higher workplace union density implies a higher strike incidence (Addison and Teixeira, 2019; 2024), while work councils are associated with lower incidence. Company-wide bargaining appears associated with lower strike incidence, but the results regarding mixed level bargaining (Addison and Teixeira, 2024) and individual bargaining are more mixed (Addison and Teixeira, 2019).

One should also note that conflicts appear associated with payment structures, and then particularly those that act at the local level. Employee share ownership appears most effective in reducing the range of conflicts, but also other forms for performance pay schemes might reduce the incidence of conflict (Fakhfakh et al., 2019). These payment forms reflect a combination of providing incentives and of providing apparent gifts (Bryson and Freeman, 2018) and thus aligning the interest of workers and management and thereby improving their relationships (Green and Heywood, 2010). In line with this, Addison and Teixeira (2019) also find that increased dissonance between the parties (management and unions) also yields higher strike incidence, while employee-focused strategies and heightened employee motivation is associated with ower strike incidence.

Since strikes are costly according to the literature, what then should the union expect to achieve from a decentralisation of wage bargaining (by introducing additional local bargaining when bargaining only were conducted centrally). From a theoretical point of view, there are several arguments why a more centralised wage bargaining regime produces higher wages and less wage dispersion than a wage bargaining regime based on local wage bargaining (Calmfors and Driffill, 1988; Moene and Wallerstein, 1997; Barth et al, 2014). Wage compression is more easily achieved with more coordination when setting the wage. The Scandinavian countries were for many years characterised by a coordinated and centralised wage bargaining regime (OECD, 2017, 2018, 2019; Bhuller et al., 2022) This wage compression in the Scandinavian countries is believed to contribute their good labour market performance. Barth et al. (2014:1) writes “wage compression fuels capitalist

investments in the process of creative destruction, increasing the average productivity and the average wage for a constant employment level".

However, the last decades have seen changes to the level of coordination, and although these countries are still considered coordinated, their level of centralisation has dropped from the previously high levels (Bhuller et al., 2022). The evidence from this decentralisation process from these countries yields mixed results. In Denmark, Dahl et al (2013) finds wage level and dispersion growth. In Sweden, a reform affecting teachers did little to change their wages, but one might observe spillover effects (Willén, 2021). In Finland, Kauhanen (2023) only finds growing wages and wage dispersion for blue collar workers in the paper industry, but in many other industries the impact appears heterogeneous but negligible (Kauhanen, 2023, Kauhanen et al., 2020).

The remainder of the paper has the following structure: Section 2 describes the econometric approach. Section 3 describes the data. Section 4 describes the empirical results. In Section 4 we map out the wage level and wage dispersion changes caused by the introduction of local bargaining rights. In Section 5, we then ask what happens to the workers that went on strike. Section 7 briefly concludes.

2. Econometric models

As pointed out in the introduction, we endorse a varying treatment intensity approach for both analyses. The first analysis addresses the impact of introducing local bargaining rights on the wage level and wage dispersion in the hotel and restaurant sector. Most of the workers in the hotel and restaurant sector are not unionised, and a considerable number of workplaces are not covered by a trade union agreement. Since mandatory extension of the trade union agreement was introduced in 2017, we discard all employment relationships not covered by trade union agreements. Next, the impact of introducing local bargaining rights is assumed to be related to union strength, which we as many others, measure by union density. Thus, the treatment, the introduction of local bargaining rights, varies across

workplaces depending on the union density measured by December 2015 (pre-strike). Higher treatment intensity implies a stronger impact. Our control jobs are employment relationships in retail trade, by those covered by trade union agreements.

We assume that the log hourly wage of worker i employed at workplace j at relative time k (year) can be described by the event-study-specification of Equation 1):

1) $lnW_{ijk} = \delta_0 + \sum_{k=0}^{k=y+4} \delta_k xI(HR)_n xUD_{fHR2015} + t_k + t_{RTk} + \delta_{pre} I(RT)_n xUD_{fRT2015} + \delta'_{pre} I(RT)_n xUD_{fRT2015} + m_m + \theta_i + \nu_{ifk}$,

where m_m expresses month fixed effects, θ_i expresses worker fixed effects. ν_{ifk} expresses a standard error term. Note this specification treat the year before the strike as the reference year, and that the δ_k 's express the yearly average wage impact relative to the wage changes occurring in for workers in retail trade. We estimate Equation 1) applying the MWFE-estimator (Corrreia, 2016, 2017) and CMMQ-estimator of Machado and Santos Silva (2019) and Rios-Avila, Siles Canavire-Bacarrez (2024). Note that while we can apply Equation 1) to an analysis of within worker wage impacts, we can repeat this analysis on workplace-level data and applying workplaceXtime fixed wage effects arising from the AKM-approach (see Section 3) instead of focussing on log hourly wages. Thus, we can study how local bargaining rights influence between firm dispersion as well.

The second analysis addresses the impact of being on strike on log hourly wages and several other outcomes, both within the labour market and outside the labour market. In this case, we focus on union members only, in hotel and restaurant-sector and in retail trade sector. Following an identification procedure described in Section3, we identify striking workers. Unfortunately, we cannot exclude the possibility of measurement errors, i.e., we classify workers as non-striking when they in reality were on strike. However, as the share of striking workers increases at the workplace, the less likely we are to wrongly classify workers. In other words, we argue that the measurement errors are declining in the workplace share of striking workers. Therefore, we endorse a varying treatment intensity approach, by letting the treatment intensity increase by diminishing measurement errors.

We measure the strike intensity by the number of striking workers relative to all unionised workers as measured by April 2016. We assume that the outcome of worker i originally employed at workplace j at relative time k (month) can be described by the event-study-specification of Equation 2):

$$2)\ Y_{ik} = \delta_0 + \sum_{k=t-4}^{k=t-2} \delta_k x Strike_i x I(Union)_i x SI_{fHR0} +$$

$$\sum_{k=0}^{k=t+4} \delta_k x Strike_i x I(Union)_i x SI_{fHR0} + t_t + t_{RTt} + \delta_c X_{ift} + \theta_i + \nu_{ift},$$

where t_t expresses relative time fixed effects, t_{RTt} expresses relative time fixed effects for the retail sector, θ_i expresses worker fixed effects. ν_{ifk} expresses a standard error term. We estimate Equation 2) applying the MWFE-estimator of Correia (2016, 2017).

3. Data

We utilise Norwegian administrative register data on the population of workers and firms during the period 2014-2019. During these years, we have monthly data on jobs, including information on union membership, occupation, work hours, hourly wage and bonus, and for workplaces, industry, workforce size, union density, and trade union agreements. Data comprise roughly 3.65 million men and 3.64 million women, with nearly 70 million monthly observations for each gender. In total, our data comprise slightly more than 133 million observations. We apply all observations to derive a measure of the employers' wage policy.

Wage policy at workplace f at time t: The wage policy at workplace f at time t is estimated based on the population-wide monthly data following Barth and Dale-Olsen (2024). We apply standard linear fixed effect regressions as they were introduced by Abowd et al. (1999) and recently extended e.g. to incorporate time-varying firm effects (Barth et al., 2021; Engbom et al., 2022; Schmieder, 2023). We start by residualizing the log hourly wage, controlling for worker age (age and age squared measured

relative to 35 years of age) and education (7 dummies) as seen in Table A1 in the appendix. Then, having added the intercept to this residualised wage, we estimate the regression given by Equation (7) for worker i employed by firm f in year y and month m:

1) $$lnW^{r}_{ifmy} = \alpha_0 + \theta_i + \Delta_{fy} + \varepsilon_{ifmy},$$

where ε_{ifmy} expresses a standard error term, θ_i expresses a worker FE. This equation identifies a standard wage premium or firm FE, Δ_{fy}, as is seen previously in the literature.

3.1 Main analyses

Our main analyses use data from hotel and restaurant sector and from retail trade only, and then only for workers between 20 and 60 years of age. We also discard observations of workers employed at workplaces not covered by trade union agreements. From this starting point, we construct two real samples and one set of pseudo-samples (for the pseudo-analyses).

Sample1

Sample 1 comprises all employment relationships given the constraints above during the period February 2015-December 2019. During our period observation, the number of jobs in the hotel and restaurant sector increases from 21000 to 24000, while the number of jobs in the retail trade sector increases from 42000 to 47000 jobs. This period is time when the Norwegian economy is growing. Table 1 show that the hourly wage and the number of workplaces grows as well, and as jobs, quite similarly in the two sectors. Figure 1 indicates that the wage dispersion in both sectors are increasing weakly. The development of unionisation differs slightly, in that while aggregate union density is increasing in retail trade, unionisation is quite stable in the hotel and restaurant sector. We see no evidence in the aggregate figures that the strike in 2016 bolstered the uptake of union membership.

[TABLE 1 AROUND HERE]

[FIGURE 1 AROUND HERE]

Table 1 also shows that the employers in the hotel and restaurant sectors pay lower wage premiums than those in the retail trade sector, but that the difference here is diminishing towards the end of our observation period.

Sample 2

Sample 2 comprises union workers on strike (treatment) and all non-unionized workers in Hotel and Restaurant sectors (control group 1) and all unionized workers in Retail Trade (control group 2) limited to employment relationships at workplaces covered by trade union agreements. We follow these workers during the period January 2016 to December 2019. We limit the analysis to workers employed in the same job January-May 2016, and which do not receive any benefits (unemployment, disability, sick leave pay) during the pre-period (January-April 2016).

Striking workers

Striking workers are not registered in the administrative registers. However, striking workers do not receive pay from their employers, they only receive compensation from their union. This compensation is not taxable, and it is not reported to the Tax Authorities. Thus, it is possible to identify striking workers by changes to their earnings from the months before the strike, under the strike, and after the strike. For this specific sector, we face an additional complication that many workers are paid on an hourly basis and face changing service schedules, which causes a natural variation in earnings. We identify striking workers as blue collar workers that experience a 30 percent drop in monthly earnings May 2016 when compared to April 2016. We are lucky in so far that Easter in 2016 was in March, thus it does not affect pay in April. Figure 2 shows the distributions of earnings growth of those that are described as strikers and non-strikers for the months preceding the strike (February-April) and the strike month (May). You observe some workers each month that experience considerable changes in earnings. However, May month differ quite considerably, and particularly (by

construction) for those that are defined as being on strike. For these the pattern over the months differs considerably compared to those that are not defined as being on strike.

[FIGURE 2 AROUND HERE]

Table 2 shows key descriptive statistics on the strikers and non-strikers in the two control groups. We see that those on strike comprise more women, deviate slightly when it comes to age and weekly working hours. The difference is larger when it comes to workplace size. The striking workers in hotel and restaurant sector are more likely to work at smaller workplaces compared to the non-unionised control workers in hotel and restaurant sector, but they are employed in much larger workplaces compared to the average workforce size in retail trade. To reduce the bias caused by unbalanced treatment and control groups, we conduct coarse exact matching, based on gender, age, work hours and workforce size. Unfortunately, data is such that to achieve complete balancing along these key dimensions, we would lose too many observations of the treated (note we start with only 400 treatment observations). After CEM-matching, the balancing improves, but particularly the difference regarding workforce size differences between hotel and restaurant sector and retail trade sector persists.

[TABLE 2 AROUND HERE]

Pseudo-samples

We construct 2 different pseudo-samples. Both these samples comprise union workers on strike (treatment) and all non-unionized workers in Hotel and Restaurant sectors (control group 1) and all unionized workers in Retail Trade (control group 2) limited to employment relationships at workplaces covered by trade union agreements. Pseudo-sample 1 assumes that the strike occurred in May 2015, i.e., the same month but the previous year. This sample follows these workers during the period January 2015 to December 2018. We limit the analysis to workers employed in the same job January-May 2015, and which do not receive any benefits (unemployment, disability, sick leave pay) during the pre-period (January-April 2015). Striking workers are defined as those that experience a 30 percent drop in

monthly earnings May 2015 when compared to April 2015. Pseudo-sample 2 assumes that the strike occurred in April 2016, i.e., the previous month but the same year. This sample follows these workers during the period January 2016 to December 2019. We limit the analysis to workers employed in the same job January-April 2016, and which do not receive any benefits (unemployment, disability, sick leave pay) during the pre-period (January-April 2016). Striking workers are defined as those that experience a 30 percent drop in monthly earnings April 2016 when compared to March 2016.

4. The impact of local bargaining rights on wages

In this section, we study the impact of changed bargaining rights, towards a more decentralised wage setting with local bargaining rights. We utilise monthly Norwegian linked employer-employee data for 2015 to 2019 and employ linear FE regressions and GMM conditional quantile regression approach of Rios-Avila et al. (2024) to study the development of log hourly wages at different quartiles in the wage distribution for employees in the hotel and restaurant sector. As a plausible control sector, we use monthly data from retail trade, a sector recognised for centralised wage bargaining for those workplaces covered by a trade union agreement. In all regressions we control for fixed worker effects, relative time effects, year and month dummies. Table 3 presents the results from our analysis.

In Model 1, we see that average yearly impact on the log hourly wage from the introduction of local bargaining rights. In t0, i.e., 2016, we see an average drop in hourly wage by 4 percent, primarily caused by the strike, but only small and insignificant effects afterwards. However, in Model 2, we see the impacts at different quantiles, or more precisely, quartiles. We see that negligible impact occurred at the first quartile or at the median, but that as the years went by, local bargaining rights implied a significant drop of 5-6 percent in hourly wages at the top. For the median workers the point estimates are non-negligible negative but insignificant, while for low wage workers no sizeable impact can be seen whatsoever. Thus, through local bargaining unions in hotel and restaurants achieved to curb wage growth for high wage workers. Thus, by having differential impact on wages

across the wage distribution, introducing local bargaining rights affected the wage dispersion in the sector. In Figure 3, we see the wage dispersion in the sector drops clearly and significantly, and then particularly at the top.

Next, in Models 3 and 4, we repeat the analysis of models 1 and 2 on workplace level data, with the wage premium as the dependent variable. Once again, in both models we strong negative impacts in 2016, but in this case the negative impacts persist longer. However, the quantile regression of Model 2 does not indicate a differential impact on wages over the wage distribution. Thus, although we had expected it, we see no sign of increased wage dispersion between workplaces following the introduction of local bargaining rights. This is confirmed by Figure 3. We see no significant sign of changing wage dispersion between workplaces. Thus, since we observe wage compression overall and no changes between workplaces, it implies that by introducing the local bargaining rights union achieved wage compression within the workplace, primarily affecting those at the top.

[TABLE 3 AROUND HERE]

[FIGURE 3 AROUND HERE]

6. The impact of strike on career outcomes

In this section, we ask whether being a striker had a detrimental impact on these workers careers for the next years. We follow a sub-sample of union workers, identified as strikers, from January 2016 to December 2019. As pointed out in Section 3, being on strike is not registered in the administrative data. Next, we establish a non-organised control group within the hotel and restaurant sector and a control group belonging to retail trade, which we follow over the same time period. To make our treatment and control group as similar as possible, we conduct Coarse-extended-matching based on the pre-period information of age, workforce size, gender, holiday entitlement, and whether the workplace practice performance pay. Since a limited number of workers at each workplace went on

strike, this allows us to apply a varying treatment-intensity design. Our analyses address several different outcomes. First, we study how the strike affected these workers log hourly fixed wage and log hourly total compensation. The latter differ from the former, by incorporating all forms for bonuses, additional payment, and monetary fringe benefits. However, since being on strike might affect the careers at a workplace, we also study outcomes such as moving to a new job, changing occupation, or changing industry. Potentially, however, we cannot exclude that being on strike affect the labour market opportunities detrimentally, thus we also study non-labour outcomes such as benefits and long-term sick leaves.

In Table 4, we start by looking at the impact on hourly wages, by estimating several difference-in-difference-event study log hourly wage regressions with varying treatment intensity (expressed by the strike intensity) on monthly data covering the period January 2016 to December 2019. Note we do not require that the workers stay at the same job they had as when the strike was conducted. To ease interpretation, we have standardized or normalized the reported estimates, so they measure 1 standard deviation increase in the strike intensity at the workplace. The table reports the average treatment intensity associated with the strike.

In Models 1-3 we study the impact on log hourly fixed wage, while Model 4 focuses on log hourly total wage, where the elements comprising the wage also included bonuses, additional monetary benefits, compensation and extra payments of different kinds. Our baseline model, Model 1, controls for fixed worker effects, calender effects relative time and relative timeXcontrol group effects. In this simple specification, we see the strike implied a wage loss of 1.9 percent. Since these workers, strikers and control workers, might be on different trajectories concerning future wage growth, and the result of Model 1 might reflect this, we add in Model 2 industry-specific linear time trends to the control vector of Model 1. When we take into account industry-specific linear time trends, we see this only enforce the detrimental impact of the strike. Participating in the strike is now associated with a drop in hourly wages of over 2 percent. Finally, in Model 3 we add controls for a

moving holiday, and we add linear time trends for workers on hourly contracts or on monthly payment schedules to acknowledge that these workers might be on have different wage trajectories. This is our preferred specification. In this case, we see the impact of the strike is further aggravated to implying a wage loss of nearly 3.5 percent.

[TABLE 4 AROUND HERE]

The strike might affect workers' future performance, and it might affect employers' perception of the workers. Both in retail trade and hotel and restaurants are performance pay not uncommon. In addition, payment also incorporate compensatory elements (for night work, shift work, rotas, weekends, public holidays). Thus, depending on the payment contract, workers might receive bonuses and additional payments on top of a fixed pay. Model 4 shows that when we study the impact of the strike on total wage, we see that the total loss caused by the strike actually was closer to 4.6 percent. Thus, for these workers participating in the strike, the strike caused a clear drop in hourly wages. In Figure 4, based on Model 3 and Model 4, we show how the development in hourly wages following the strike, by mapping each impact estimate over the relative time distribution.

[FIGURE 4 AROUND HERE]

Next, participating in the strike might influence other career outcomes than wages. We study five different outcomes: weekly work hours, move to another job (dummy), change occupation (dummy), change industry (dummy), enter the benefit rolls (from the Norwegian Labour and Welfare Administration, long-term sick leave) (dummy), and long-term sick leave (dummy). We apply the same difference-in-difference event study linear regression model as our preferred specification in Table 4. Table 5 presents the results from these regressions, expressing the average treatment effect. First, we see that for those employed, we observe minor changes in weekly working hours. Thus, for the employed worker and given the decline in hourly pay, striking workers experience diminishing earnings. Next, Table 5 shows that the striking workers are less likely to change jobs, to change

occupation or to change industry where they work. This is slightly surprising, since we would have expected that diminished career opportunities, as expressed by the drop in payment, would manifest in higher likelihood of mobility to new jobs, new occupations in potentially new industries. However, we see that one of the reasons why these workers reveal lower mobility than others, is that they enter the welfare system. Workers participating in the strike are more likely to receive benefits and to enter long-term sick leave than other workers. This is in line with other studies on strike behaviour and how strike participation has affected mental health detrimentally. Figure 5 maps the development of how the strike affected welfare benefit reception and long-term sick-leave over time.

[TABLE 5 AROUND HERE]

[FIGURE 5 AROUND HERE]

Finally, to close this section, we look closer on our pseudo-samples. We have constructed two pseudo-sample, where we assume that the strike occurred at other times than May 2016. We identify the strike through earnings drop from April 2016 to May 2016. If such earnings drop is common or associated with the month of May, then we wrongly identify striking workers. Thus, we assume that the strike might have occurred in May 2015 or in April 2016. Then we repeat the analyses in Tables 4 and 5, based on our preferred model specification. Table 6 and Figure 6 present our results.

[TABLE 6 AROUND HERE]

[FIGURE 6 AROUND HERE]

The table and figure show no significant average treatment effects whatsoever. Of course, in some cases we observe a couple significant estimates associated with some of the relative time estimates, but that is as expected.

7. Conclusion

Our study has analysed the impact of a strike in the Norwegian hotel- and restaurant sector 2016, where one of the key unions went on strike for the right to bargain wages locally. The union won this dispute, thus local bargaining in addition to central bargaining was introduced from wage negotiations 2017 and onwards. Since hotel and restaurant sector can be considered a low wage industry, the union hoped that by introducing local bargaining, workers employed at high-performing workplaces should get higher wages. Our analysis only pertains to workers employed by workplaces covered by trade union agreements. The reason for this is that the unions successfully worked for a mandatory extension of the trade union agreements in the sector, which were introduced from 2018, and thereby affected workers employed by non-covered workplaces. To contrast the wage development in the hotel and restaurant sector, we apply union workers in retail trade sector, where wages traditionally are set centrally.

The overall conclusion is that it is hard to observe positive gains from the strike for the strikers. For the union, establishment of local bargaining rights was a prerogative, but we do not observe strong evidence for that this has mattered positively in form of higher wages. However, by establishing local bargaining rights the union has compressed the wage dispersion, driven by curbing wages at the top. Although maybe not the main target for blue collar unions, wage compression is considered attractive from the union's point of view. Still, it is hard to argue that the gains from the strike and the introduction of local bargaining rights have been immense or even considerable. For striking workers, the outcome is much clearer. for these workers participating in the strike, the strike caused a clear drop in hourly wages. Even worse, they not only lost money, but striking workers were also even more likely than non-strikers to end up on welfare. Thus, the conflict had a price – an unknown cost - that at the time of the strike was not obvious for the workers.

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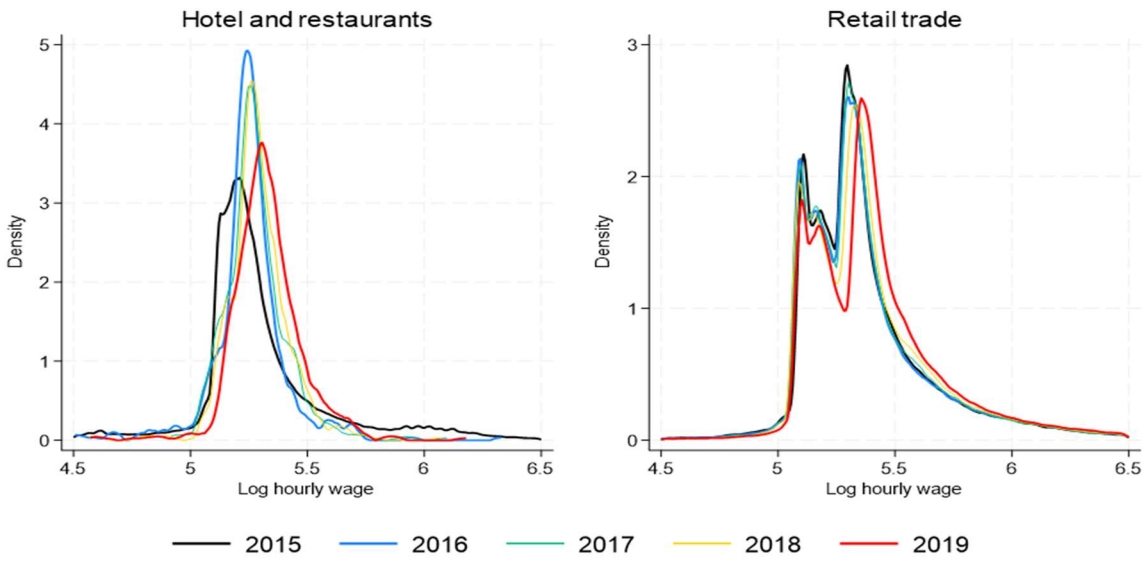
Figure 1 Wage distributions over time



Note:

Figure 2 Identification of workers on strike

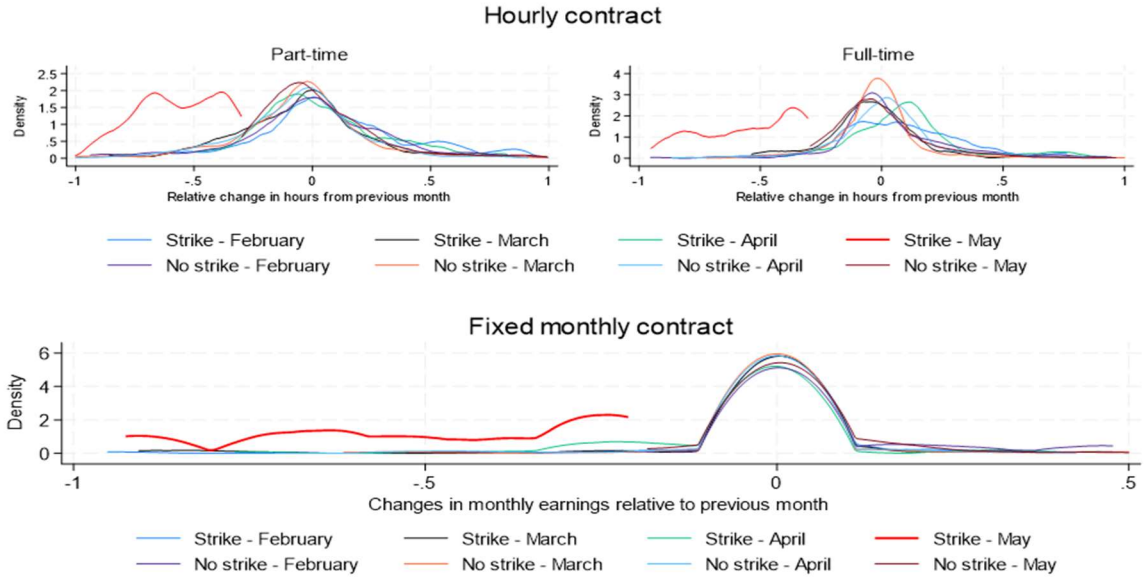


Figure 3 Local bargaining rights and wage dispersion

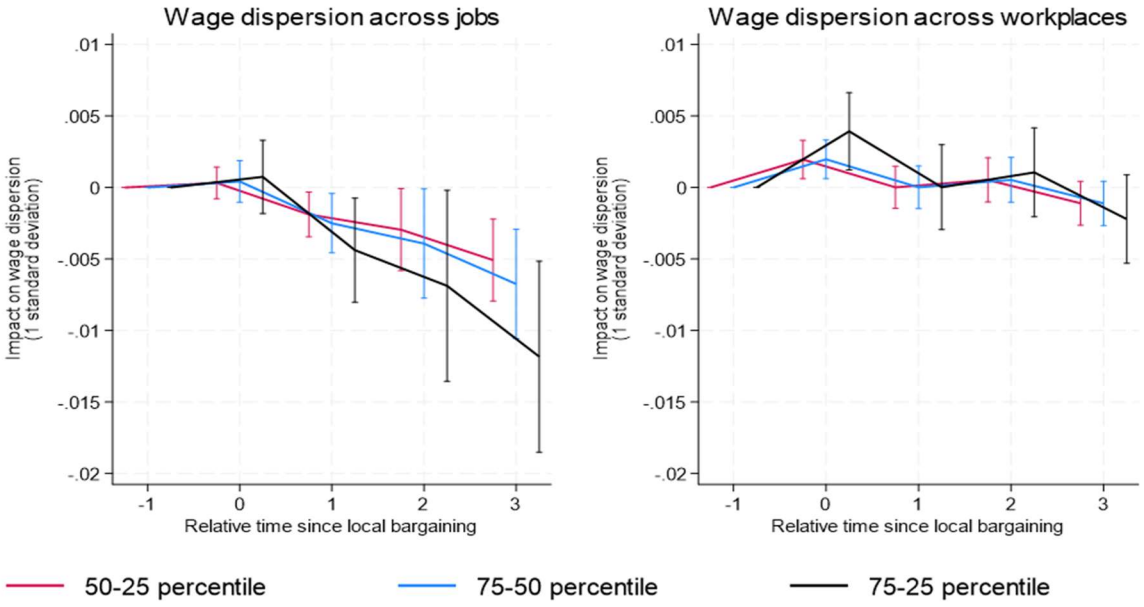


Figure 4 Impact of strike on future payment

Figure 5 Impact of the strike on outcomes outside the labour market

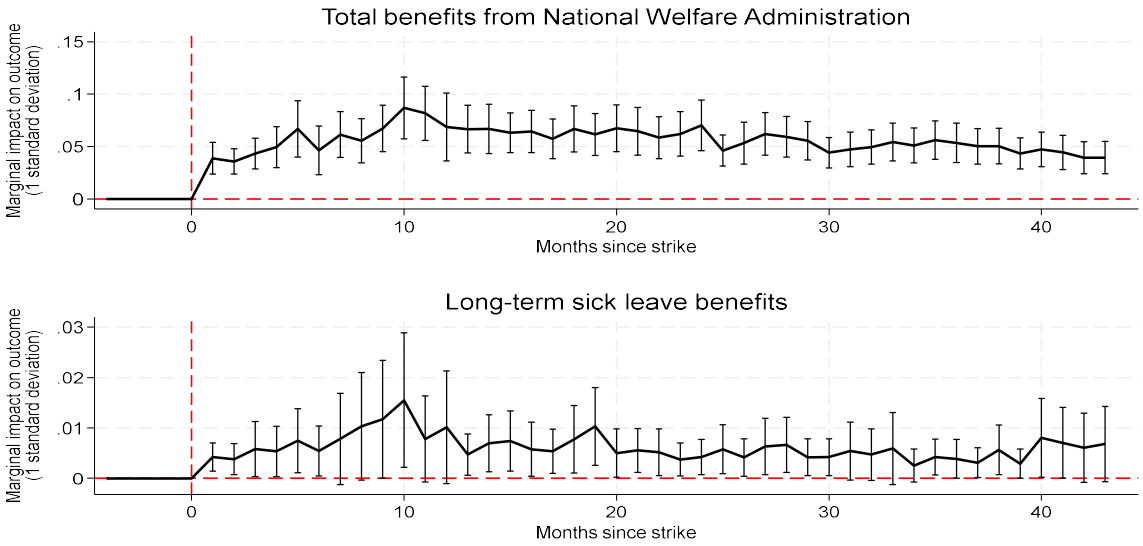


Figure 6 Impact of strike in pseudo-analyses

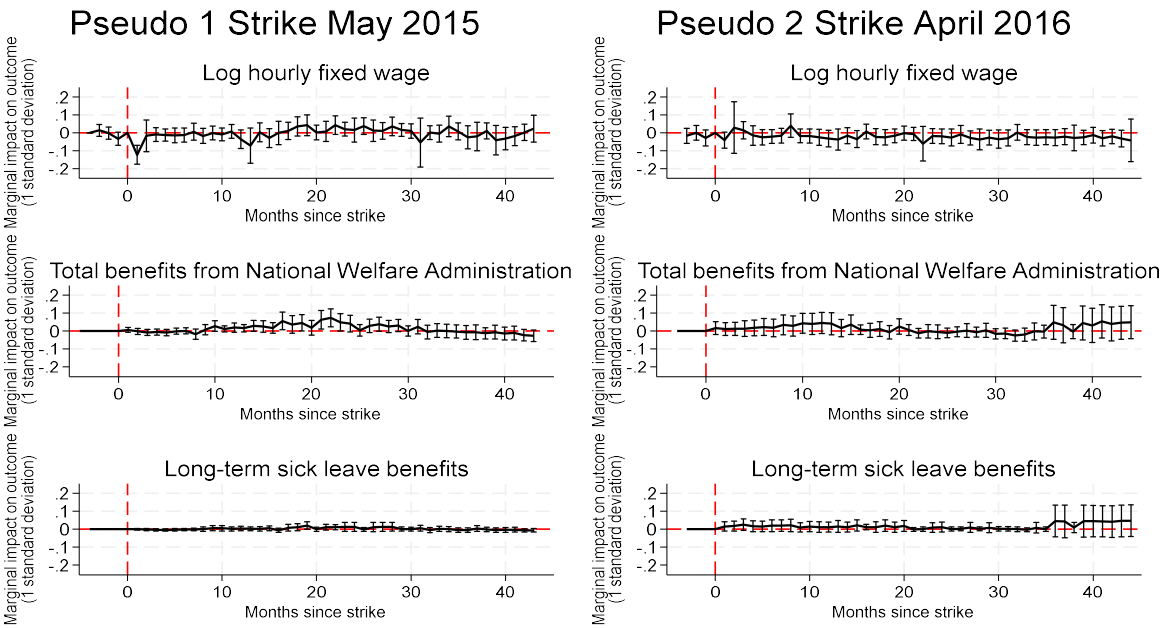


Table 1 Descriptive statistics on jobs and workplaces in the hotel and restaurant sectors and in retail trrade.

	Across jobs			Across workplaces		
	Log hourly wages	Union density	Number of jobs	Wage premium	Union density	Number of workplaces
A) Hotel and restaurants						
2015	5.297	0.283	21194	-0.012	0.418	1005
	(0.381)	(0.186)		(0.294)	(0.493)	
2016	5.282	0.281	21970	-0.056	0.440	1058
	(0.368)	(0.185)		(0.312)	(0.496)	
2017	5.307	0.260	22899	-0.054	0.423	1070
	(0.327)	(0.189)		(0.314)	(0.494)	
2018	5.327	0.281	22958	-0.049	0.436	1078
	(0.316)	(0.177)		(0.311)	(0.495)	
2019	5.366	0.277	24183	-0.030	0.440	1112
	(0.317)	(0.178)		(0.316)	(0.501)	
B) Retail trade						
2015	5.408	0.284	42103	0.007	0.462	3846
	(0.432)	(0.164)		(0.287)	(0.498)	
2016	5.408	0.284	47100	-0.015	0.475	4251
	(0.447)	(0.180)		(0.305)	(0.499)	
2017	5.414	0.274	47723	-0.012	0.469	4331
	(0.439)	(0.180)		(0.311)	(0.499)	
2018	5.427	0.295	48010	-0.012	0.478	4314
	(0.449)	(0.163)		(0.302)	(0.500)	
2019	5.461	0.303	47217	0.004	0.480	4190
	(0.452)	(0.165)		(0.299)	(0.499)	

Table 2 Descriptive statistics

	Unmatched			Post-CEM-matching		
	Strike	Control in HR	Control in RT	Strike	Control in HR	Control in RT
A) Pre-strike						
Woman[c]	0.704	0.568	0.715	0.704	0.702	0.703
	(0.457)	(0.495)	(0.451)	(0.457)	(0.457)	(0.457)
Age[c]	37.020	33.964	37.680	37.020	36.665	36.967
	(9.970)	(9.797)	(10.281)	(9.970)	(9.888)	(10.064)
Working hours[c]	29.669	27.623	31.132	29.669	28.971	28.529[*]
	(8.794)	(11.816)	(9.454)	(8.794)	(9.908)	(8.892)
Workforce size[c]	93.344	121.164	23.495	93.344	109.867[*]	70.493[*]
	(125.355)	(164.675)	(37.792)	(125.355)	(163.687)	(86.164)
Number of workers	368	4062	8699	368	3332	6776

Table 3 Changing wage structure following introducing local bargaining rights

	Log hourly wage				Workplace wage premium			
	Model 1-	Model 2-CMMQ			Model 3-	Model 4-CMMQ		
	MWFE	Q25	Q50	Q75	MWFE	Q25	Q50	Q75
TreatXudXt0	-0.0405**	-0.0427**	-0.0409**	-0.0383**	-0.0399**	-0.0513**	-0.0398**	-0.0383**
	(0.0114)	(0.0134)	(0.0115)	(0.0107)	(0.0107)	(0.0129)	(0.0107)	(0.0205)
TreatXudXt1	-0.0126	0.0008	-0.0102	-0.0250	-0.0271*	-0.0272	-0.0271*	-0.0272*
	(0.0212)	(0.0253)	(0.0215)	(0.0177)	(0.0131)	(0.0156)	(0.0131)	(0.0124)
TreatXudXt2	-0.0314	-0.0103	-0.0277	-0.0507**	-0.0382**	-0.0413*	-0.0382**	-0.0351*
	(0.0312)	(0.0414)	(0.0324)	(0.0220)	(0.0145)	(0.0170)	(0.0144)	(0.0138)
TreatXudXt3	-0.0318	0.0043	-0.0255	-0.0651**	-0.0140	-0.0075	-0.0140	-0.0206
	(0.0273)	(0.0373)	(0.0285)	(0.0189)	(0.0152)	(0.0173)	(0.0152)	(0.0149)
Controls								
All models control for treatXpre-intro union density (not models 3-4), retail tradeXpre-intro union density, relative time (years) month dummies, and worker FE								
Workplaces	6488		6497		6482		6482	
Workers	177925		178802					
N	4031197		4044670		305628		305628	

Table 4 Impact of strike on hourly wages

	Log hourly fixed wage			Log hourly total wage
	Model 1	Model 2	Model 3	Model 4
ATE	-0.0191**	-0.0205**	-0.0349**	-0.0463**
	(0.0054)	(0.0056)	(0.0064)	(0.0077)
Controls				
Worker	Yes	Yes	Yes	Yes
Relative time	Yes	Yes	Yes	Yes
Relative timeXcontrol	Yes	Yes	Yes	Yes
Industry time-trends		Yes	Yes	Yes
Easter			Yes	Yes
Hourly paid			Yes	Yes
Hourly paidXtime-trends			Yes	Yes
Workers	10514	10514	10514	10514
N(obs)	401237	401237	401237	401237

Note: ATE measures the impact normalised by 1 standard deviation increase in the workplace strike intensity. Population: Matched workers at workplaces with trade-union agreements in hotel and restaurants or in retail trade, where strike workers are selected union members in hotel and restaurant, while control workers either are non-unionised workers in hotel and restaurant or union members in retail trade. Standard errors adjusted for workplace clustering in parentheses. ** and * denote 1 and 5 percent level of significance, respectively.

Table 5 Impact of strike on other outcomes

	Working hours	New job	New occupation	New industry	Benefits	Long-term sick leave
ATE	-0.0626	-0.2374**	-0.1816**	-0.1619**	0.0563**	0.0062**
	(0.3014)	(0.0134)	(0.0147)	(0.0154)	(0.0074)	(0.0020)
Controls						
+ in all models controls for worker FE, relative time, relative timeXcontrol industry, industry-time-trends, Easter, hourly paid job, hourly paid jobXtime-trends						
Workers	10514	10514	10514	10514	10617	10617
N(obs)	401237	401237	401237	401237	506714	506714

Note: ATE measures the impact normalised by 1 standard deviation increase in the workplace strike intensity. Population: Matched workers at workplaces with trade-union agreements in hotel and restaurants or in retail trade, where strike workers are selected union members in hotel and restaurant, while control workers either are non-unionised workers in hotel and restaurant or union members in retail trade. Standard errors adjusted for workplace clustering in parentheses. ** and * denote 1 and 5 percent level of significance, respectively.

Table 6 Impact of pseudo strikes on outcomes

	Pseudo 1 Same month, previous year			Pseudo 2 Same year, previous month		
	Log hourly fixed wage	Benefits	Long-term sick leave	Log hourly fixed wage	Benefits	Long-term sick leave
ATE	-0.0092	0.0133	0.0023	-0.0121	0.0156	0.0168
	(0.0133)	(0.0113)	(0.0043)	(0.0099)	(0.0221)	(0.0171)
Controls						
+ in all models controls for worker FE, relative time, relative timeXcontrol industry, industry-time-trends, Easter, hourly paid job, hourly paid jobXtime-trends						
Workers	11264	12062	12062	10025	11353	11353
N(obs)	418506	563262	563262	467977	541393	541393

Note: ATE measures the impact normalised by 1 standard deviation increase in the workplace strike intensity. Population: Matched workers at workplaces with trade-union agreements in hotel and restaurants or in retail trade, where strike workers are selected union members in hotel and restaurant, while control workers either are non-unionised workers in hotel and restaurant or union members in retail trade. Standard errors adjusted for workplace clustering in parentheses. ** and * denote 1 and 5 percent level of significance, respectively.

Table A1 Wage premium estimation

	Residualising (age and education)	Wage policy
		$\ln W^{r}_{ifmy}$
		b/se
Constant	**5.3454*****	**5.3617*****
	(0.0001)	(0.0001)
Age-35	**0.0156*****	
	(0.0001)	
$(\text{Age-35})^2$	**-0.0005*****	
	(0.0001)	
Education FE(7)		
Controls		
Worker FE (2961791)		Yes
WorkplaceXyearFE (1068087)		Yes
N	133146893	133146893

*p<0.05, ** p<0.01, *** p<0.001 (se clustered on workers)