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IZA DP No. 18139

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Harald Dale-Olsen
Institute for Social Research, Oslo and IZA

Henning Finseraas
Norwegian University of Science and Technology

Kristine Nergaard
Fafo, Oslo

Elin Svarstad
Fafo, Oslo

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ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Upholding Unions – How Colleagues Shape Union Membership?*

Social interactions between young and senior colleagues might have consequences for union membership uptake of young workers, thus influencing public policies on unions. We apply Norwegian administrative register data to test this claim about the influence of social interactions on unionization, while addressing threats of homophily bias, contextual, and network confounding. Leveraging exogenous spillover shocks by colleagues' siblings' unionization to colleagues' unionization, we find causal evidence supporting the notion that social interactions with close colleagues are important for unionization, mainly driven by social costs and information sharing. Our results suggest that one standard deviation increase in the union density of close colleagues, causes the uptake of union membership for young workers to grow by 20-23 percent. Our analyses thus reveal one source of additional spillover impacts from the implementation of public policies supporting unions. Furthermore, our results have important implications for unions' mobilization strategies.

JEL Classification: J50, J51, Z13

Keywords: unionization, social interaction, young workers, panel data, IV-analyses

Corresponding author:
Harald Dale-Olsen
Institute for Social Research, Oslo
P.O. Box 3233 Elisenberg
N0208-Oslo
Norway

E-mail: hdo@socialresearch.no

* We thank participants at COPE2023 for fruitful discussions and helpful suggestions. This study has benefit from support from the Research Council of Norway, grant No. 301280.

1. Introduction

Union membership rates vary across Europe, but have declined in many countries (Fitzenberger et al., 2009; Schnabel, 2013; OECD, 2019). Recently, several studies have pointed out the potential important roles unions have for the labour market, in general (OECD, 2018), for productivity and technological change (Barth et al., 2020; Garnero et al., 2020; Svarstad and Kostøl, 2022; Kostøl and Svarstad, 2023), for wages, wage inequality and dispersion (Western and Rosenfeld, 2011; Garnero et al., 2020; Dodini et al., 2022), for firm behaviour (Dodini et al., 2023b), for worker careers (Dodini et al., 2023a), for immigrants and low-pay job opportunities (Svarstad, 2023: Dodini et al., 2024), educational attainment (Kostøl, 2024) and as a gender inequality-reducing device (Kostøl and Svarstad, 2025). In the U.S., conditioning on sorting into unionization, being unionized has always been associated with improvements in job satisfaction (Arntz et al., 2022) and declining unionisation is even associated with increased opioid misuse (Chen and Islam, 2025). Thus, if these positive benefits from unionisation are true, declining union membership rates is worrisome. Changes in aggregate union density can, in some cases, be attributed to changes in "supportive" public policies, i.e., changes in social insurance schemes (Böckerman and Uusitalo, 2006; Shin and Böckerman, 2019) and changes to tax deductions (Barth et al., 2025). From a policy point of view, particularly the latter findings appear attractive, since such a supportive strategy seems easy to implement.

In this paper we address an issue potentially enforcing the impacts from supportive union policies – the importance of social interaction and peer effects. However, such effects will also aggravate negative trends, e.g., induced by changing workforce composition, but also reflect changing social interaction and/or culture. From a policy point of view, union recruitment strategies, their communication and mobilization, will have to address why people join unions. Thus, monetary considerations (rewards through wage bargaining strength vs fees), appeal and political stance, social interactions and culture all possibly contributes to unionisation.

Our key hypothesis builds on the assumption that social interaction at work is important for human behaviour. At work, colleagues affect each other, which has been supported theoretically (Granovetter, 1973, 2005; Kandel and Lazear, 1992) and empirically along a wide range of attributes and choices (Bandiera et al., 2009; Mas and Moretti, 2009; Cornelissen et al., 2017; Godøy and Dale-Olsen, 2018). However, given the rich literature linking social costs and social customs to unionisation (e.g., Booth et al., 1985; Olson, 1965), the lack of causal evidence on how colleagues affect each other w.r.t. unionisation is on the one hand rather surprising. Previous studies retort to analysing correlations, for example Ibsen et al. (2017), usually finding that colleagues' matter positively for unionisation. On the other hand, this lack of causal studies should not come as a surprise, as identifying workplace peer effects is methodologically challenging. Analyses of peer effects are susceptible to homophily bias and contextual confounding (Manski, 1993; Van der Weele and An, 2013), also known as the reflection problem, causing challenges in the analysis of networks (Angrist, 2014), which is aggravated by network confounding (Lee and Ogburn, 2020). Thus, it is far from established that peer effects in unionisation at work exist at all.[1] Currently, no paper has yet addressed these relationships causally.

In our paper, we apply Norwegian population-wide administrative register data (2010-2020) to test the assumption that colleagues affect each other's unionization, whilst handling homophily bias, contextual, and network confounding. Our analyses rest on two populations, whereof the former, young recently hired workers just finished school, do not include the latter, comprising older experienced workers. The two-population approach makes it possible to identify shocks to the latter population, that do not affect the former directly. Thus, we follow

[1] Beynon et al. (2021) convincingly show that local culture and values matter for unionisation, which might be transferred by family or peers. Areas close to the coalfields of Britain are more likely to be unionised today, long after the closures of the coal mines. Most studies of unionisation at work and social norms rest on correlations, e.g., Visser (2002) and Goerke and Panneberg (2004).

the requirements of an instrument variable (IV) approach in similar ways as Dahl et al. (2014a), Godøy and Dale-Olsen (2018), Nicoletti et al (2018) and Welteke and Wrohlich (2019).[2]

We know from a rich literature that family affects culture, leading practices and persisting attitudes to politics, welfare, work and social obligations (Degner and Dalege, 2013; Dahl et al., 2014b; Bryson and Davis, 2019; Bau and Fernández, 2023). Thus, this family link should imply that we can use variation in senior workers' siblings' unionization to create unionization shocks to our senior workers not related to our young workers' uptake of union membership. For example, we know from Bryson and Davis (2019) and Nergaard and Svarstad (2025) that unionisation among parents is transmitted to their children. Thus, siblings' unionisation should be related even if they do not work or live close by. To ensure that confounding variation between these siblings and our young workers does not affect these young workers' union uptake, *we exclude siblings working in the same occupations, same local labour markets and same industries as our young workers*. Thus, we are confident that these "shocks" affecting our senior workers, do not affect our population of young, newly hired workers at work. The scope for interaction between young workers and colleagues' siblings is extremely limited, thereby making it possible to use an IV-approach to study peer effects. Such an IV-approach is sometimes called 'peer-of-peers' (Bramoullè et al, 2009; Carlsen and Rashid, 2022). Since our data comprise a panel of workplaces and occupations over time, we even include fixed effects to remove any remaining confounding variation between occupations, workplaces, and time trends.[3]

[2] Although changes in insurance schemes and tax deductions make it possible to draw causal evidence on unionisation (e.g., Barth et al., 2025), such changes cannot be utilised to causally identify unionisation peer effects because they affect all workers.

[3] Thereby, our analyses address many of the shortcomings in the previous correlation-based literature. For example, Ibsen et al. (2017) mainly handle these issues by multi-level modelling and random effects. A multi-level random effect approach rests on an extremely strong assumption that these random effects are uncorrelated with other explanatory variables such as the focal variable workplace union density.

While the previous literature finds that workplace social culture is correlated to the unionisation decisions of newly employed workers, we find that the causal influence is from worker colleagues within the same occupational group as the newly employed workers. Our analyses, which are conducted specifically to be able to draw causal inference, suggest that unionisation decisions are driven by the influence of close colleagues, and not by an encompassing culture at the workplace. In particular, the union spillover effects seem related to social costs and to a certain degree, information sharing, but less by monetary gains or social norm transmission possibilities.

The paper proceeds as follows. We review the relevant literature in the next section and introduce hypotheses for how to empirically shed light on social interaction. In Sections 3 and 4 we describe the administrative data and the statistical approach, and how this data source allows us to address important shortcomings in the previous empirical work. Section 5 establishes the spillover effects, while Section 6 shed light and tests why we observe these spillover effects. Section 7 explores the existence of heterogeneous spillover effects. Section 8 discusses policy implications and briefly concludes.

2. Social interaction as drivers of unionisation

Several papers have documented a significant decline in union density (Visser, 2002; Ebbinghaus et al., 2011; Schnabel, 2013; OECD, 2019). Diminishing unionization is a concern for unions, as their power base erodes. As indicated in the introduction, a decline in unionization could be a concern also for other parties (OECD, 2018). While the decline is well-documented, it is contested why this drop in aggregate union density occurs. One strain of literature points to changes in workforce composition. If worker groups traditionally less unionized enter the labour market, or if traditionally strongly unionized industries diminish, then aggregate unionization drops, e.g., as we have seen in heavy industries (Bain and Elsheik, 1976; Schnabel and Wagner,

2007).Women have replaced men as the majority gender in unions (Bryson et al., 2019), and immigrants are less likely to be unionized than natives, primarily reflecting what work they do (Cools et al., 2021) but potentially also reflecting cultural differences (Fernandez, 2011).

Youth-adult differences in the demand for unionization do not appear to be very important (Bryson et al., 2005), and when present, they often indicate a more positive view on unions among youths. For example, Canadian youth had a stronger desire to have unions deal with workplace issues rather than deal with these themselves (Gomez et al., 2002). Booth et al. (2010) identify the same pattern, as US unionised workers are more likely to be in their forties or fifties, but younger workers are more likely to be union members when they get older. Several studies have addressed whether unions appeal less to young people today than before, but find little support for this notion (Vandaele, 2012, 2018; Aleks et al., 2021; Høgedahl and Møberg, 2022). Høgedahl and Møberg (2022) conclude that young workers are not more individualized; to the contrary, unorganized young workers have a growing collective mind-set, but youths tend to take jobs in parts of the labour market with weak trade union representation. Based on Norwegian register data, Nergaard and Svarstad (2021) observe that unionization of young and older workers is correlated, but no indication that these correlations are changing over time. Still, this does not exclude that cohort effects could be important. Böckerman and Uusitalo (2006) attribute part of the decline in unionisation to an unwillingness of cohorts born after the early 1960s to become union members. Kollmann et al. (2020) argue that younger workers today are more motivated by monetary rewards than before, while more experienced workers are motivated by what tasks they are conducting.

If structural changes in the workforce are not the sole reason why unionisation is on the decline, perhaps changing appeals, changes in social interactions, or changing culture at work are important causal factors? As indicated in the introduction, we know that colleagues influence each other, along a wide range of dimensions. Correlations support this notion also for

unionization. Furthermore, unionization might follow from workplace culture (Akerlof and Kranton, 2000, 2005; Graham et al., 2022) or from social costs or customs (Booth, 1985; Ibsen et al., 2017) or both. Booth (1985:255) applies Akerlof (1980)'s description of a social custom as "an act whose utility to the agent performing it in some wage depends on the beliefs or actions of other members of the community". However, if unions are strong enough, they can create what can be understood as bandwagon effects (Corneo and Jeanne, 1997). Cultural transmissions occur in the labour market and between generations (Bisin and Verdier, 2011). For unions, successful recruitment strategies are easier to implement given a strong occupational identity (Simms et al., 2018). However, as Bisin and Verdier (2001), one could treat culture as the accumulation of norms transmitted through social interactions, which of course causes challenges to empirically disentangle workplace culture from the sum of contemporaneous social interactions across workers. This also raises the issue of whether culture and social costs are transmitted by more experienced workers as a form for information sharing? Experienced workers might have internalised knowledge of social norms and customs (Chatman and O'Reilly, 2016; Greco, 2020). If this is the case, unionization could be transmitted through the relationship between newcomers and experienced colleagues in line with the correlations observed by several authors (Visser, 2002; Toubøl and Jensen, 2014; Ibsen et al., 2017).

The relationship between newcomers and experienced colleagues might potentially change with the advent of internet and social media. As Bryson et al. (2010) discuss, the growth of internet communications has arguably changed social interactions (Décieux et al., 2018; Arias, 2019; Zhong, 2021), particularly within young age cohorts, so interactions across age groups might have been influenced.

Disentangling social interaction empirically as drivers of unionisation

Even comprehensive population-wide administrative data do not usually comprise detailed direct information on how and why people (workers) interact. This is unfortunately our case as well. However, that does not mean it is not possible to derive testable hypotheses regarding if and how colleagues' unionisation influences young workers.

First, Bisin and Verdier (2001) argue that culture is the accumulation of norms transmitted through social interactions. If we find that the workplace has little importance in the transmission of union uptake from colleagues to young workers, it is hard to argue that a (time-invariant) workplace culture is important for this determination. However, from the perspective of Bisin and Verdier (2001), culture varies over time following the accumulation of norms.

Second, if any social interaction effect is independent of age differences between young workers and colleagues, it seems difficult to argue that information sharing occurs along this dimension and drives the social interaction effect.

Third, the social interaction effect also reflects the bargaining power of these groups at the workplace, and thus could express not social interaction, but just the monetary rewards associated with unionisation. If this is the case, we should observe stronger spillover as the union wage premium grows. Similarly, if more costly unions offer more and/or cheaper non-wage amenities as part of the union good (cheaper legal counsel, insurance, training), then social interactions would appear stronger for more expensive unions (i.e., higher union dues).

Fourth, we argue that union membership uptake for young workers follows from social interaction, and thus can be thought of as a social workplace norm of the descriptive kind (how colleagues behave) or as an injunctive kind (as colleagues would approve/disapprove of)(Cialdini et al., 1991). If this is the case, we argue that occupational task autonomy and occupational support among colleagues potentially influence the uptake of union membership. In occupations

where colleagues' support is common and necessary, deviating from colleagues' behaviour is more costly. On the other hand, job autonomy might make it easier to deviating from colleagues' behaviour. Langfred (2004) argue that some employees may dislike job autonomy as it requires more commitment, trust and responsibility to be deployed into the job with little support, while Eisele and Schneider (2020) argue that unions prefer Tayloristic production with little job autonomy.

Fifth, for colleagues to influence our young workers socially, they need to have extensive contact. Many union colleagues on public sick pay (sick leaves lasting 16 days or more) potentially reduce this contact (admittedly this could also reflect bad work environment). One could also argue that occupations where employees are able to work at home (remote work) should experience less contact and possibly less social interaction effects than other occupations. Similarly, high turnover occupations could possibly yield less time for workers to interact, which might weaken any social interaction effect. Thus, both remote work and worker turnover affect the transmission possibilities of social norms.

Finally, it is well known that key targets of unions are to raise the wages for the poorest paid and work for wage moderation or compression (Barth et al., 2015; Svarstad, 2023). Higher wage dispersion among non-union colleagues implies larger inequality and potentially directly sort young workers into unions if young workers share unions' preference for equality. However, larger wage dispersion among union colleagues might also potentially reflect a weaker union and thus yields a weaker social interaction effect (where union power acts as producer market power, which is a necessity for a bandwagon effect to arise as indicated by Corneo and Jeanne (1997).

3. Data

We apply Norwegian population-wide register data covering the years 2010-2021 to test how colleagues' unionization influences recently educated and newly hired workers' decision to join unions. Our data comprise of linked employer-employee data for the population of Norwegian workers, workplaces and firms. In addition, we have population-wide information covering the same period, with necessary information on family relationships (including siblings), educational qualifications, wages, occupations, sick pay, unemployment, place of residence, and whether they are unionized (back to 2000). During this period, i we observe a diminishing aggregate union density in private sector, from 51 percent in 2010 to 36 percent in 2020 (all workers included). Union density in the public sector is, in contrast, stable during this period, hovering around 77 percent.

From these data, we create our key sample of young workers, by selecting a) relatively young people, 19-39 years of age, which are b) leaving the educational system with a new educational qualification, and which c) the previous two years were without a job or only in a minor job (less than 19 weekly working hours), d) getting a new full-time position for a new employer which employs at least 4 employees[4], and e) with no siblings employed at these workplaces.

For these workers, we collect their union membership status, either the year they started or the next year.[5] Since union information is available from 2000, we are able to study the potential union involvement for these young workers in earlier part-time jobs and previous

[4] We focus on full-time positions primarily for identification purposes. Full-time employees are more embedded in the labour market than part-time employees. When students leave the educational system for good and enter the labour market, they usually target full time positions. Many schoolchildren and students have part-time jobs, some even several. We surmise that colleagues at work matter less for school children and students than friends and fellow students. Admittedly, some workers (more often women) keep part-time position permanently, and these workers we thus discard.

[5] Admittedly, this time span is chosen somewhat ad hoc, but not completely. First, we need to let young workers settle in their new job. Second, we want these young workers to experience at least one major union wage negotiation settlement, which occurs during springtime. Third, for each added year, we would lose one year of observations in the final regression data set. Fourth, as time goes by, we would expect other confounding actions or occurrences influencing union membership uptake would become more important as time goes by.

careers. For nearly all these young workers (93-95 percent), they have no union experience before they started in this fulltime job which of course reflects that most of these workers are at the beginning of their careers, although this varies slightly across the age groups.[6] We primarily let colleagues be defined as other workers at the workplace within the same 1-digit ISCO-code.[7] We furthermore measure the colleagues' unionization the year before our young workers started. This sample construction (for young workers and colleagues) implies that the regression analyses cover the period 2011-2019 (2019 is the last year a young worker might start). In some ways, the sample construction yields a series of linked cross-section data (for each cohort of starting young workers), but over the time (over cohorts), repeated panel of observations on workplaces and colleagues are observed.

However, as a robustness check, we select young workers starting 2011, defined as the 2011-cohort, and follow these young workers and their colleagues, over the years (until 2020) and potentially across workplaces. This cohort thus constitutes a linked employer-employee panel over time.[8]

Finally, to shed light on social interactions and the interplay of workers, we apply information from several sources. First, our rich population-wide register data spanning the period 2000-2020, let us construct several measures directly. Secondly, we apply information from the Norwegian Level of Living Survey 2013 (Vrålstad and Revold, 2014), utilising the worker response to questions averaged at the 3-digit ISCO08 level across workers and then linked to our

[6] For a 19-year-old student starting to work in 2011, we thus know for all practical purposes his or her complete involvement with unions (99.99 percent of the 20 year-old-students have no previous union involvement). For a 39-year-old student starting to work in 2011, we know his or her union involvement back to age 20. A 39-year-old student might have had a different career in his or her early twenties, facing adverse labour market conditions, which required reskilling in the form of formal education (83.49 percent of the 39 year-old-students have no previous union involvement)).

[7] This differentiates between ten major occupational groups and is in line with the definition used by Carlsson and Reshid (2022). However, in a robustness test, we split these groups into several by defining colleagues as those that have the same 2-digit occupational code (ISCO-08).

[8] Potentially we could have constructed a panel data set comprising all the cohorts (not only the 2011-cohort), but by construction (given our observation period and attrition), these cohorts would have been given different importance and weight in the analyses. In addition, we would have had to solve difficulties associated with mobility by the different cohorts of young workers into the control groups.

register data. Thirdly, we apply an index at the 3-digit ISCO08-level on whether the work can be conducted at home (Brugiavnini et al, 2022).[9] Fourthly, we apply data on union dues at the 3-digit ISCO08-level for 2011 from Barth et al. (2025).[10]

In Table 1 we present descriptive statistics on our young workers and their close colleagues over time. Over time, the total number of young workers starting to work fulltime after finishing school is usually around 6-7000 individuals, but it seems to increase slightly at the end of our observation period. While the gender ratio appears equal early, the share of women increases slightly towards the end of the observation period. The average age of these young workers is around 25 years of age, while aggregate union density for these workers hovers around 70 percent.

Table 1 Descriptive statistics on sample size, gender and age

	Young workers						Colleagues (median values)			
Year	Total numbers	Women	Age	Union density	Workplace size	Workplace size (p50)	Women	Age	Group size	Union density
2011	6067	0.49	25.6	0.67	377	74	0.43	41.7	31	0.75
2012	6768	0.48	25.4	0.69	734	80	0.41	41.5	32	0.75
2013	7112	0.50	25.5	0.70	647	76	0.44	41.5	31	0.77
2014	6301	0.53	25.7	0.73	657	78	0.49	42.2	34	0.80
2015	6343	0.56	25.7	0.72	752	65	0.53	42.0	29	0.82
2016	6112	0.58	25.6	0.68	489	52	0.60	41.8	25	0.82
2017	6995	0.57	25.5	0.68	451	58	0.57	41.7	27	0.80
2018	7775	0.56	25.6	0.69	419	60	0.55	41.5	27	0.78
2019	8686	0.56	25.3	0.68	489	66	0.56	41.3	28	0.78

Note: Own calculation

[9] Recently, following COVID-pandemic, in a number of papers have developed occupational indices for working at home and social interaction (Basso et al., 2020; Brugiavnini et al., 2022). However, when it comes to social interaction, these measures capture the social interaction with customers, thus making this measure unsuitable for our purpose. Note also that the pandemic induced widespread changes to the possibilities of working at home through digitalisation (Gahtmann et al., 2024), these newer indices are not perfectly suited as measures of whether work can be conducted at home during the ten years before the Pandemic.

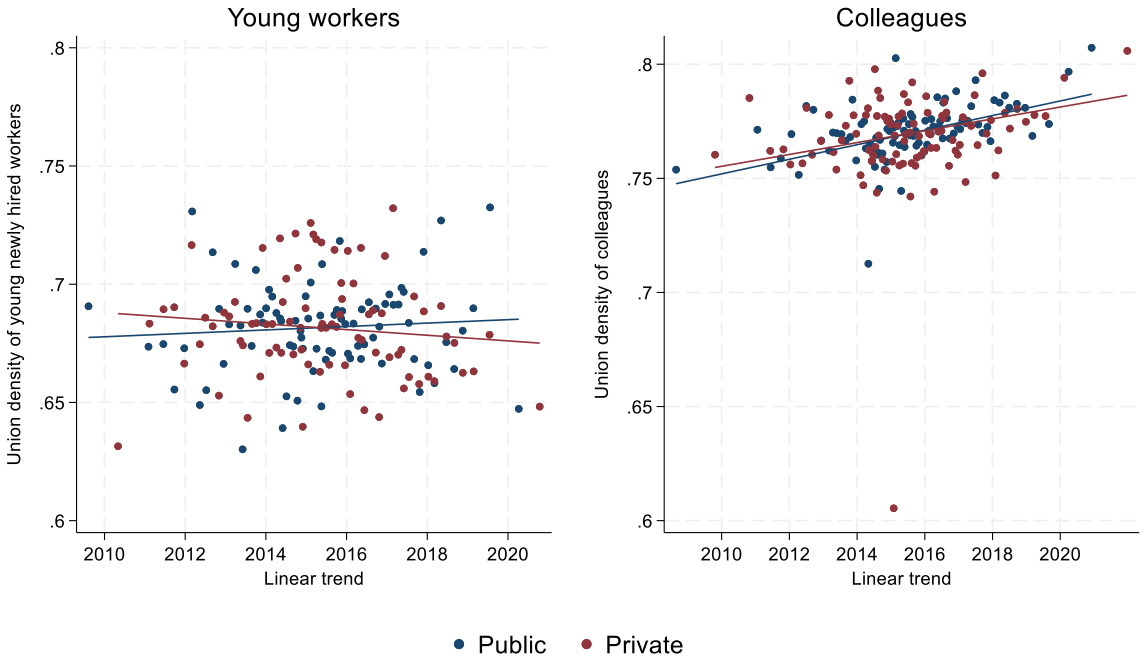
[10] We chose 2011 for several reasons. First, 2011 is the first year in our observation period. By fixing union dues to this year, we avoid potentially endogenous union price responses. Second, as is discussed in Barth et al. (2025), from 2012 tax changes (deductions) are kept fixed for the remainder of our observation period, thus such tax changes will have negligible impact (the tax subsidy increased by 20 Nok from 2011 to 2012).

However, where these young workers begin to work, it varies considerably. This is seen in the variation in average and median workplace size over time. Median and average is taken across young workers. Thus, variation in the average characteristics of close colleagues would thus be driven primarily through the inclusion of a few large workplaces and colleague-groups and how these characteristics were distributed among these. Therefore, we present median values (across young workers) instead of mean values for the close colleagues. The close colleague group changes from being dominated by men towards being dominated by women. While the close colleague group also appears to diminish in size slightly over time, the median age varies around 41-42 years of age, while the median union density varies between 75 to 82 percent. Thus, colleagues are typically more likely to be unionised than these new employees. However, although we do not show the corresponding mean union density among colleagues, one should note that this is lower, since in many small workplaces there simply are no unionised colleagues, while large workplaces are more unionised.[11]

Figure 1 shows how unionization for newly recruited workers and their colleagues develop over time in our data. Our sample is not random and thus deviate from Norwegian aggregate union density, we have selected workplaces which employ young individuals leaving school. We have split the data by public and private sector, but we see that differences between them are not very large. In both sectors, the union uptake of young workers appears lower than the average union density of their colleagues. For young workers, the aggregate union density varies around 70 percent. For their peers, the union density is around 75-80 percent.

[11] This creates an apparent inconsistency when comparing Table 1 and Table A1. The average across workplaces of very different sizes will not be equal to the average across workers. For example, let say you have three workplaces with union density of 0, 0 and 1. The two former workplaces are small, let say 10 workers, and the latter is big, 100 workers. Average union density across workers will be 0.83, but average union density across workplaces will be 0.33. In Figure A2, we show the distribution of the young workers' colleague group. We see small groups dominates, less than 30, supporting the notion that social interaction might occur. Admittedly, some of these colleague groups are so large, that one might infer that the scope for social interaction is limited.

Figure 1 Unionization over time



Note: Own calculation

Next, the availability of population-wide linked employer-employee data makes it easy to establish two pseudo-groups: i) workers with the same occupation, but employed at other workplaces that employ these occupations to a similar degree as our sampled workplaces (based on propensity score matching on the number of workers at the workplace and within occupation), and ii) workers with the same occupation, but employed at another workplace that employ these occupations to a similar degree as our sampled workplaces (based on propensity score matching on the number of workers at the workplace and within occupation) but where this pseudo-workplace is located in the same municipality and produces goods or services in the same industry as the original workplace. We use these samples for placebo analyses. The latter pseudo-sub-group is just not a sub-sample of the former due to the construction of the pseudo-samples. In the former case, it is sufficient to just have a workforce occupational structure like our sampled workplaces, but these pseudo-workplaces will be randomly located and conduct random activities. In the latter case, the sampled pseudo-workplaces additionally

will have to be in the same municipality and do business in the same industry as our original sampled workplaces.

Key variables – main effects

A colleague is defined as a worker having the same occupation o (1-digit-level or 2-digit-level as a robustness check) AND employed at the same workplace g as the young worker i. Let other workers be noted by j. Let F denote families of colleagues defined by their parents. Our focus in on colleagues' siblings, for the colleagues defined as $j \nexists i$.

U_{iogt}: Young worker i union membership (year t/ t+1) (member=1) having occupation o employed at workplace g starting at cohort year t.

$\overline{U}_{ogt|(j \nexists i)}$: Colleagues' (all workers $j \nexists i$) union density (membership measured year t-1) having occupation o employed at workplace g at cohort t.

$\overline{U}_{Fogt}$: Colleagues' (all workers $j \nexists i$) siblings' average union density (membership measured year t-1) having occupation o employed at workplace g at cohort t.

We exclude siblings working in the same industry, same occupation or in the same municipality as our young workers, to avoid capturing bias induced by unobserved industry, occupation or local labour market effects. Let s index remaining non-excluded siblings and n_{Fst} then express this number of siblings for all colleagues. Finally, then $\overline{U}_{Fogt} = \sum_{Fs} U_{Fst}/n_{Fst.}$

$\overline{W}_{Fogt}$: Colleagues' (all workers $j \nexists i$) siblings' average log yearly earnings (earnings measured year t-1) having occupation o employed at workplace g at cohort t.

See above on colleagues' siblings' union density for more detail.

Key variables – mechanisms

Monetary rewards and costs

UWP_{ogt}: Colleagues' (all workers $j \neq i$) union wage premium for occupation g employed at workplace g at cohort t. Constructed as the difference between the average log yearly earnings for union members and the average log yearly earnings for non-union members for occupation o employed at workplace g at cohort time t. Constructed from the complete population of Norwegian workers and workplaces.

$Udues_{oont}$: Average union dues 2011 for union workers in occupation oo employed in industry n. Taken from Barth et al. (2025)

Social costs

$Taskfree_{oot}$: Freedom to choose tasks for 3-digit occupation oo for cohort t. Based on the question: Can you choose your job task? Responses are given as a 5-level Likert scale averaged at the 3-digit ISCO-level.

$Support_{oot}$: Degree of support from colleagues in occupation oo at cohort time t. Based on the question: Do you receive support from your worker colleagues? Responses are given as a 5-level Likert scale and averaged at the 3-digit ISCO-level.

$Physical_{oot}$: Physical taxing job for 3-digit occupation oo for cohort t. Based on the question: How often do you experience being physically exhausted? Responses are given as a 5-level Likert scale averaged at the 3-digit ISCO-level.

$Mental_{oot}$: Mental taxing job for 3-digit occupation oo for cohort t. Based on the question: How often do you experience being mentally exhausted? Responses are given as a 5-level Likert scale averaged at the 3-digit ISCO-level.

Norm transfer possibilities

Rem_work_{oot}:Degree of whether work can be conducted at home for occupation oo at cohort time t. Based on the index of Brugiavnini et al. (2022).

$Turnover_{oot}$:The separation rate for occupation oo existing at cohort time t, where turnover is measured at t-1.

SPI_U_{ogt}: Public sick pay incidence rate for union colleagues at workplace g in occupation o at cohort t, where the sick leave incidence has been measured at t-1.

Information sharing

ΔA_{iogt}: Age difference between young workers age and colleagues' average age, i.e., $\Delta A_{iogt} = A_{iogt}$-$\bar{A}_{ogt|(j \neq i)}$. Larger difference increases the possibility of information sharing between young and more experienced workers.

$Unemploy_{oomt}$:Average outflow to unemployment t-1 from employment in municipality m for workers in occupation oo at cohort time t.

Policy – Union strength/Reduced inequality

SDW_U_{ogt}: Earnings dispersion conditional on age for union colleagues at workplace g in occupation o at cohort t, where the earnings dispersion has been calculated at t-1.

SDW_NU_{ogt}: Earnings dispersion conditional on age for non-union colleagues at workplace g in occupation o at cohort t, where the earnings dispersion has been calculated at t-1.

4. Statistical approach

We estimate linear probability models with year, occupation, and workplace fixed effects. When comparing workers within the same occupations, colleagues' unionization potentially influences new workers' decisions to join unions when they are working together. We define

the colleagues of newly hired workers as those workers with the same occupation (1-digit levels working at the same workplace), which we argue comprise rather homogeneous groups (Cornellisen et al., 2017; Carlsson and Reshid, 2022). Admittedly, by focussing on the average unionisation of colleagues, we ignore the possibility that some colleagues are more important and influential than others (Halliday and Kwak, 2012).

These linear probability models for the newly hired workers' probabilities of becoming union members can be expressed as:

(1) $U_{iogt} = \alpha_1 + \alpha_2 \overline{U}_{ogt|(j \neq i)} + \alpha_3 X_{igt} + \alpha_4 \overline{X}_{gt|(j \neq i)} + t_t + \theta_g + \Delta_o + \varepsilon_{igt}$,

where $t = 2011 - 2019$, and U_{ift} expresses a dummy for membership, X_{igt} and $\overline{X}_{gt}$ express individual, group/workplace controls, t_t, θ_g and Δ_o express cohort , workplace and occupation FEs, and ε_{igt} expresses a normal-distributed error term. Our key parameter is α_2, associated with $\overline{U}_{ogt|(j \neq i)}$, the union density of colleagues at time t (i.e., co-workers in occupation g employed at workplace g) but where union membership is measured the year before worker i was hired. Note that we do not follow these young workers over time, thus the t is a cohort indicator. Moreover, a workplace might exist for many cohorts, i.e., over time a workplace might recruit young workers several times.

The main concern is that $COV(\overline{U}_{ogt|(j \neq i)}, \varepsilon_{igt}) \neq 0$. For instance, one could argue that reflection bias arises, causing conflated estimates of the relationship between colleagues' union density and young workers' uptake of union membership. However, one can also argue the opposite; if detrimental economic development for the establishment induces high level of unionisation, then seniority rules ("last-in-first-out"- aka LIFO-rules)(Below and Thoursie, 2010; Butschek and Sauerman, 2024) make the protection ordinarily provided by union membership negligible and thus induce a negative selection and bias into union membership for young workers. A priori the direction of the bias is not clear. Few would argue that estimating Equation

1) by OLS would yield unbiased estimates (see note 10). Thus, in line with Dahl et al. (2014) we refrain from showing these.

IV-approach

Our solution to the concern above follows the rich literature documenting family spillovers, following the influence of parents, in general (Dahl et al. 2014b; Nicoletti et al., 2018; Welteke and Wrohlich, 2019; Carlsson and Reshid, 2022), and of unionisation specifically (Bryson and Davis, 2019; Nergaard and Svarstad, 2025). For over the past fifty years, evidence indicate that family is important for political views in that there exist an association between parent's sociopolitical attitudes with those of their children (Liebes and Ribak, 1992; Degner and Dalege, 2013).[12] Thus, if parents influence their children's view on unionisation, in our data these young workers' colleagues and these colleagues' siblings should have related views on unionisation. Through this family linkage, we therefore argue that colleagues' siblings' unionisation might act as an instrumental variable for the union density of colleagues. The key assumption is that colleagues' siblings do not interact with newly hired workers and therefore only influence their unionisation through the colleagues. This strategy is also based on the established concept of "peer-of-peers" in the literature (Bramoullé et al., 2009; Carlsson and Reshid, 2022).

Let F denote families of colleagues defined by their parents. We assume that through their parents, colleagues j and their siblings s share values on unionisation, i.e., COV(U_{Fjogt}, U_{Fst}) >0. As an IV for $\overline{U}_{ogt|(j\neq i)}$ we propose to use $\overline{U}_{Fogt}$, i.e., the colleagues' siblings' average union density. We assume $COV(\overline{U}_{Fogt|(s\neq j)}, \overline{U}_{ogt})$ ≠0 and $COV(\overline{U}_{Fogt}, \varepsilon_{igt})$=0. In addition, we will add in all IV-regressions $\overline{W}_{Fogt}$ as a control (see

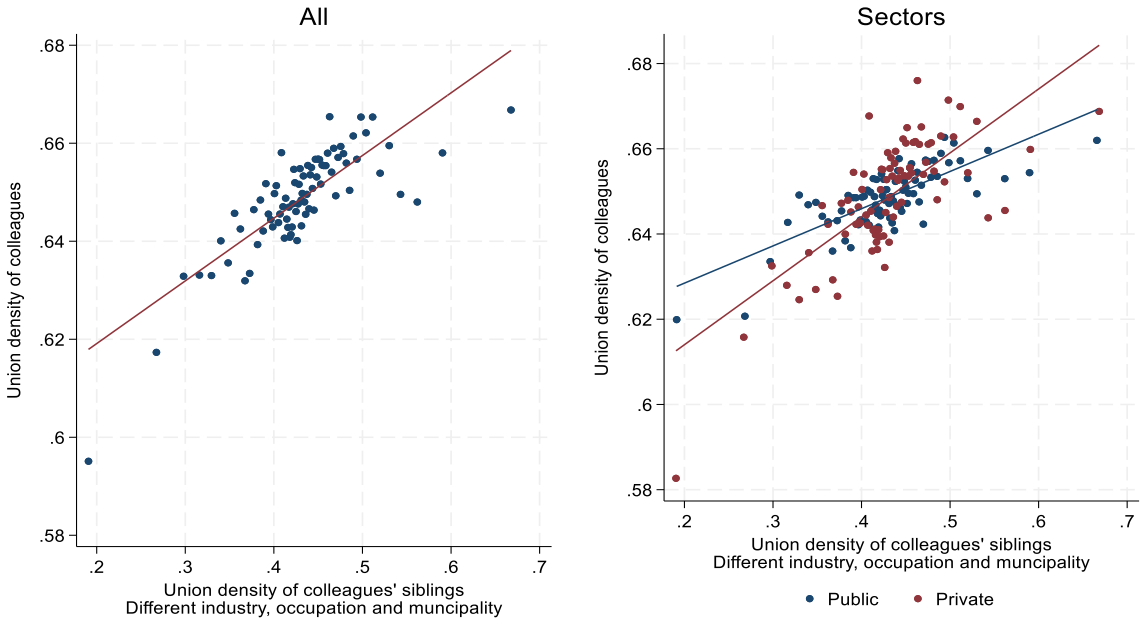
[12] Arguable parents' influence on children might also have a genetic component, as indicated by Alford et al. (2005) and Willoughby et al.(2021), but whether parents' influence on their children's view on unions are genetically or socially driven or both, do not matter for the validity of our instrument.

Section 3 on key variables for details on construction of $\overline{U}_{Fogt}$ and $\overline{W}_{Fogt}$). This can be thought of as family-time fixed effects. As pointed out by Nicoletti et al. (2018), Carlsson and Reshid (2022) and Caeyers and Fafchamps (2024), we correct for potential bias caused by unobserved characteristics of colleagues' families but also for the general exclusion bias in studies of peer effects. Our IV solves bias caused by confounding factors at the workplace influencing jointly both young workers and colleagues. The challenge to our identification strategy is whether there exists some confounding aspect associated with these siblings' unionisation that also influence our young workers. Given that we have excluded siblings working in the same industry, same occupation or in the same municipality as our young workers, we avoid bias induced by unobserved industry, occupation or local labour market effects, and we argue that any remaining bias is negligible (but this is of course not explicitly testable).

However, that our exclusion restriction valid, is not sufficient for our IV-approach to be valid. The IV-approach implies that our instrument – colleagues' siblings' unionisation – operate in the same directions for all colleague groups, i.e., it requires monotonicity. To test this, we follow the approach of Dahl et al. (2014b) and Bhuller et al. (2020). First, we predict the colleagues' union density rates from a model incorporating all the exogenous variables, i.e., a regression without our IV. Next, we split the data into four quartiles depending on the predicted colleagues' union density. Then, we finally estimate our first stage model separately for the four quartiles. In line with Dahl et al. (2014b) and Bhuller et al. (2020), we interpret the monotonicity condition to be satisfied if the parameters associated with colleagues' siblings' union density in the four quartiles do not point significantly in the opposite direction.

Figure 2 shows that the correlation between colleagues' unionization and their siblings' unionization is strong and positive. Thus, this supports the previous studies indicating that family spillover effects are present regarding unionisation, and it gives a first indication that our IV-strategy is sound.

Figure 2 The relationship between colleagues' unionization and their siblings' unionization



Note: The figures show the relationship between the union density of the siblings of colleagues and colleagues' union density for young newly recruited workers after school. Note the union density of siblings are based on siblings not employed in the same industry, nor same occupation nor in the same municipality as the colleagues.

Empirical specifications

We estimate several linear probability regression models. First, we estimate what can be interpreted as reduced form models:

(2) $U_{iogt} = \beta_1 + \beta_2 \overline{U}_{Fogt} + \beta_3 X_{igt} + \beta_4 \overline{X}_{gt|(j \neq i)} + \beta_5 \overline{W}_{Fogt} + t_t + \theta_g + \Delta_o + \varepsilon'_{igt}, t \in 2011 - 2019$,

and where the individual and colleague group control vectors X_{igt} and $\overline{X}_{gt}$ comprise in the most involved form by dummies for woman and immigrant, age *quartile* dummies, workforce size and colleague group size. t_t, θ_g, Δ_o express year FE effects, occupation (1-digit) FE, and workplace FE. $\overline{U}_{Fogt}$ and $\overline{W}_{Fogt}$ express colleagues' siblings' average union density and colleagues' siblings' average earnings. We even experiment with occupational and workplace-specific time trends, and 2-digit occupation FE.

Second, we estimate the similar linear probability IV regression models for the newly hired workers' probabilities of becoming union members for t=2011-2019:

(3) $U_{iogt} = \alpha_1 + \alpha_2 \overline{U}_{ogt|(j \neq i)} + \alpha_3 X_{igt} + \alpha_4 \overline{X}_{gt|(j \neq i)} + \alpha_5 \overline{W}_{Fogt} + t_t + \theta_g + \Delta_o + \varepsilon_{igt}$,

and where we instrument colleagues' union density $\overline{U}_{ogt|(j \neq i)}$ by colleagues' siblings' union density $\overline{U}_{Fogt}$. [13] Notation is otherwise as for Equation 2).[14]

Third, when we test or disentangle different mechanisms explaining the social interaction effect, we do this in the form of interaction effects related to (presumably) exogenous dimensions. Let this dimension be noted as Z. The linear probability regression models will in this case for t=2011-2019 be described as:

(4) $U_{iogt} = \alpha_1 + \alpha_2 \overline{U}_{ogt|(j \neq i)} + \alpha_2 Z x \overline{U}_{ogt|(j \neq i)} + \alpha_4 X_{igt} + \alpha_5 \overline{X}_{gt|(j \neq i)} + \alpha_6 \overline{W}_{Fogt} + \alpha_7 Z + t_t + \theta_g + \Delta_o + \varepsilon''_{igt}$,

and where we instrument colleagues' union density $\overline{U}_{ogt|(j \neq i)}$ and the interaction term $Z x \overline{U}_{ogt|(j \neq i)}$ by colleagues' siblings' union density $\overline{U}_{Fogt}$ and the interaction term $Z x \overline{U}_{Fogt}$. Notation is otherwise unchanged. From Section 3 on key variables expressing mechanisms, we see that Z can represent UWP_{ogt}, $Udues_{oont}$, $Taskfree_{oot}$, $Support_{oot}$, $Physical_{oot}, Mental_{oot}, Rem\_work_{oot}, Turnover_{oot}, \Delta A_{iogt}, Unemploy_{oomt}, SDW\_U_{ogt}$ or

[13] In practice, this implies a two-stage approach, where the second stage can be expressed as:
$U_{iogt} = \alpha_1 + \alpha_2 \overline{U^*}_{ogt|(j \neq i)} + \alpha_3 X_{igt} + \alpha_4 \overline{X}_{gt|(j \neq i)} + \alpha_5 \overline{W}_{Fogt} + t_t + \theta_g + \Delta_o + \varepsilon'_{igt}$, where $t = 2011 - 2019$, and where the first-stage equation predicting $\overline{U^*}_{ogt|(j \neq i)}$ can be expressed as:
$\overline{U}_{ogt|(j \neq i)} = \beta_1 + \beta_2 \overline{U}_{Fogt} + \beta_3 X_{igt} + \beta_4 \overline{X}_{gt|(j \neq i)} + \beta_5 \overline{W}_{Fogt} + t_t + \theta_g + \Delta_o + \varepsilon'_{igt}$, where $t = 2011 - 2019$.

[14] When we utilise the 2011-cohort panel data, we estimate the similar linear probability IV regression models for the newly hired 2011-workers' probabilities of becoming union members for observations over the years t=2011-2019: $U_{iogt} = \alpha_1 + \alpha_2 \overline{U}_{ogt|(j \neq i)} + \alpha_3 X_{igt} + \alpha_4 \overline{X}_{gt|(j \neq i)} + \alpha_5 \overline{W}_{Fogt} + t_t + \theta_i + \varepsilon_{igt}$,
where we instrument colleagues' union density $\overline{U}_{ogt|(j \neq i)}$ by colleagues' siblings' union density $\overline{U}_{Fogt}$. Note that t in this case no longer indexes cohort, but time, and that $\overline{U}_{ogt|(j \neq i)}$ and $\overline{U}_{Fogt}$ changes over time due to mobility by young workers (between workplaces) and by colleagues.

SDW_NU_{ogt}. Otherwise, the Z-variables can express characteristics as gender, blue-collar occupations, workplace or colleague group size, sector, period or union pre-experience.

5. Main results

Table 2 presents the main results, using the approach explained above (see Table A1 in the appendix for descriptive statistics). The first four regressions apply the instrument only, i.e. they can be interpreted as reduced form regressions. All models include a basic control vector comprising year dummies, dummies for woman and immigrants, dummies for age group, workforce and group size, colleagues' siblings' average earnings. In Model 1, we only control for occupation (1-digit), Models 2 adds workplace FEs, Model 3 adds occupational and workplace-specific time trends, while Model 4 repeats Model 3, but uses a more detailed specification of occupation (2-digit).

In all four models, our instrument influences young workers uptake of union membership positively and significantly. Except for model 1, the correlations are quite stable, hovering around 7-9 percent increase when colleagues' siblings' union density goes from zero to 1 (when all colleagues' siblings are unionized). Perhaps a more useful metric is to consider a standard deviation difference in the instrument (=0.12 (see Table A1)), whereby a one standard deviation difference in colleagues' siblings' union density yields a one percentage point increase in young workers uptake of union membership.

Next, we apply colleagues' siblings' union density as an IV for colleagues' union density in linear IV regressions models of young workers' uptake of union membership.[15] As expected, the instrument strongly predicts colleague union density in the first stage.

[15] Note we have conducted Durbin-Wu-Hausman test of the endogeneity of colleagues' union density, based on the assumption that this is exogenous. This null hypothesis is strongly rejected with p-values less than 0.001.

Table 2 The impact of colleague unionization on young workers uptake of union membership.

Dep: Union member-ship(=1) for young worker	Reduced form				IV			
	M1	M2	M3	M4	M1	M2	M3	M4
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
					SECOND STAGE			
Colleagues' union density					**0.743*****	**0.671*****	**0.715*****	**0.762*****
					(0.023)	(0.267)	(0.337)	(0.369)
					FIRST STAGE (Dep: Colleagues' union density)			
Colleagues' siblings' union density	**0.539*****	**0.074****	**0.099*****	**0.096****	**0.726*****	**0.110*****	**0.138*****	**0.127*****
	(0.019)	(0.031)	(0.048)	(0.045)	(0.027)	(0.016)	(0.025)	(0.024)
					Strength of instrument			
Kleibergen-Paap rk Wald F-statistic					744.1	69.1	44.4	39.8
Controls								
Occupation 1-digit FE	Yes	Yes	Yes		Yes	Yes	Yes	
Occupation 2-digit FE				Yes				Yes
Occ. 1-digit X time trend			Yes				Yes	
Occ. 2-digit X time trend				Yes				Yes
Workplace FE		Yes	Yes	Yes		Yes	Yes	Yes
Workplace X time trend			Yes	Yes			Yes	Yes
Additional controls:	In all models, control for year, woman, immigrant, age group, workforce and group size, colleagues' siblings' average earnings.							
N	49860	49860	49860	49860	49860	49860	49860	49860

Note: 49860 young workers newly hired after finishing school at 8100 workplaces. Linear probability regressions models of the young workers' unionisation probability. The colleagues' siblings' union density is introduced as an IV for the colleagues' union density (see text). The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, in other occupations and in other industries than our young workers. Standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.10, ** p<0.05, *** p<0.01

In Figure A1 in the Appendix, we show the first stage of IV-model 2, depicting the relationship between colleagues' unionization and their siblings' unionization as well as the density of colleagues' siblings' unionization. This figure reveals that our instrument - colleagues' siblings' unionization – varies considerably, and although a certain lumpiness exists at the ends (completely unionised or non-unionised), most observations are between these end points. In Table A2, we show that our simple test of whether the monotonicity assumption is also satisfied. Thus, from this perspective our IV-approach appears sound.

For the impact on young workers' uptake of union membership, the models yield an unambiguous answer. In all models, we see a strong and positive impact from colleagues'

unionisation on young workers uptake of union membership. Across the models, the estimated impacts are quite stable, as point estimates vary between 0.671 to 0.762 depending on specification. One standard deviation of colleagues' union density is roughly 0.3. Thus, a one standard deviation increase in colleagues' union density increases young workers' uptake of union membership by roughly 20 percentage points. This tells us that colleagues are very important for young workers' uptake of union membership. However, since the impacts vary so little when we add workplace FE, it seems that a fixed workplace culture is not important for the uptake of union membership than argued in previous work. This is probably a too strong statement; a more precise interpretation is that the unionisation culture transmitted through family relationships (through siblings) is not clustered at workplaces.

Can we be certain that our estimates are consistent and follow truly from social interaction between young workers and their colleagues? No, because there is no test for the assumption that colleagues' siblings' union density does not influence young workers in any other way (the exclusion restriction). However, we can conduct robustness checks to examine the plausibility of the assumption. First, as already noted, the IV-results of Table 2 are stable across specifications.

Second, in the first two columns of Table A3, we provide estimates of the correlations between our instrument and background characteristics of the young workers. We see that these correlations are negligible and not significant.

Third, we can use our sample of pseudo-colleagues to study the impacts arising when replacing our real colleagues with these pseudo-colleagues. Since there is no social interaction between these people, we should not find peer effects. If we do, it suggests that the previous estimates of the relationship between colleagues' union density and young workers' propensity to unionize are just artefacts of something unobservable and not related to the relationship between young workers and their colleagues. In Table 3, we follow this approach. We apply propensity score matching and sample workplaces and occupations as similar as possible to the

workplaces and occupations the young workers are employed in (based on the occupational structure of the workplaces).

Table 3 Robustness check: Young workers uptake of union membership and pseudo colleagues' unionization. IV

Dep: Union membership (=1) for young worker	Random		Random within industry and municipality	
	M1	M2	M1	M2
	b/se	b/se	b/se	b/se
Colleagues' union density	0.023	0.020	-0.055	-0.073
	(0.022)	(0.028)	(0.057)	(0.068)
	FIRST STAGE			
Colleagues' siblings' union density	**0.769*****	**0.803*****	**0.123*****	**0.127*****
	(0.035)	(0.051)	(0.019)	(0.024)
	Strength of instrument			
Kleibergen-Paap rk Wald F-statistic	496.6	285.3	43.3	27.8
Controls				
Occupation 1-digit FE	Yes	Yes	Yes	Yes
Occ. 1-digit X time trend		Yes		Yes
Workplace FE	Yes	Yes	Yes	Yes
Workplace X time trend		Yes		Yes
Additional controls:	In all models, control for year, woman, age group, workforce and group size, colleagues' siblings' average earnings			
N	49860	49860	49860	49860

Note: 49860 young workers newly hired after finishing school randomly matched to 8100 workplaces similar in occupational structure and size as their original workplace based on propensity score matching. The colleagues' siblings union density is calculated based on siblings that work at workplaces located in other municipalities and in other industries than our young workers. All standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.1, ** p<0.05, *** p<0.01

Models 1 and 2 are based on completely random matching to pseudo-workplaces, while models 3 and 4 require the pseudo-workplace to be active in the same industry and located in the same municipality as our original workplace. Next, we measure union density for these pseudo-colleagues with the same occupations as the young workers, and their siblings' average union density. Reassuringly, the estimates in Table 3 do not reproduce the peer effects we estimated above, as we do not observe any relationship between young workers' uptake of union membership and these pseudo-colleagues' unionization. Thus, the results are consistent with a peer effects interpretation of the estimates in Table 2.

The last two columns of Table A3, Model 3-4 provide results from a set of regressions of colleagues' union density on young workers' siblings' union density and average earnings. This can be interpreted as a negative control outcome test (Egami and Tchetgen Tchetgen, 2023; Egami, 2024), where we relate a different pseudo-outcome (young workers' siblings' union density and earnings) to observed controls. These regressions reveal only negligible correlations.

Finally, while the previous robustness checks (Tables 3 and A3) lend support to our analyses, one might still worry that the effects found in Table 2 follows from unobserved fixed confounding factors associated with our young workers that is somehow correlated with colleagues' unionisation. As a final robustness check, we select the young workers starting in 2011, the 2011-cohort, and follow these young workers until 2020 as they potentially move to other workplaces. Thus, variation in colleagues' unionisation (and their siblings' unionisation) might thus occur because the colleague-population might change over time (through hires and separations of colleagues) or through young workers moving to new workplaces and thereby getting new colleagues. Table A6 in the appendix presents descriptive statistics on the 2011-cohort-panel. By utilising the 2011-cohort-panel, we can take into account fixed confounding factors associated with our young workers. We conduct similar linear probability regressions, with similar controls, as those reported as Model 2 in Table 2. In addition, we estimate models adding fixed worker effects. The results from these regressions are reported in Table 4.

Table 4 shows that the reduced form regressions yield slightly weaker impact from colleagues' siblings' unionization, since this drops from 0.07 (Table 2) to 0.045-0.05 (Table 4). However, even after controlling for fixed worker effects, colleagues' siblings' unionization significantly "affects" our young workers union membership uptake. When turning to the IV-analyses, we see from the first stage regressions, that colleagues' siblings' unionization, our IV-variable, significantly and strongly affects colleagues' unionisation. More importantly, the second stage regression impact from colleagues' unionisation on young workers uptake of

union membership is very strong. When we only control for workplace and occupation fixed effects, the estimated spillover effect is over 1.06, but this estimate also includes worker mobility selection. However, even after controlling for fixed worker effects, which in this panel setting is more appropriate, we still measure a significant spillover effect of 0.43. Since one standard deviation of colleagues' union density for the 2011-cohort is also 0.3, one standard deviation increase in colleagues' union density increases young workers' uptake of union membership by roughly 13 percentage points. This is slightly weaker than what we observed in Table 2 (an increase of 20 percentage points), but it clearly supports our notion that colleagues' unionisation is important for young workers' union membership uptake.

Table 4 The impact of colleague unionization on young workers uptake of union membership. The 2011-cohort panel.

Dep: Union member-ship(=1) for young worker	Reduced form		IV	
	M1	M2	M1	M2
	b/se	b/se	b/se	b/se
			SECOND STAGE	
Colleagues' union density			**1.064****	**0.437*****
			(0.542)	(0.184)
			FIRST STAGE (Dep: Colleagues' union density)	
Colleagues' siblings' union density	**0.051****	**0.045****	**0.051*****	**0.107*****
	(0.025)	(0.019)	(0.014)	(0.016)
			Strength of instrument	
Kleibergen-Paap rk Wald F-statistic			12.5	54.2
Controls				
Occupation 1-digit FE (9)	Yes		Yes	
Workplace FE (4493)	Yes		Yes	
Worker FE (6157)		Yes		Yes
Additional controls:				
Immigrant, woman	Yes		Yes	
Additional controls:	In all models, control for year, age, seniority, workforce and group size, new job, colleagues' siblings' average earnings.			
N	38774	38774	37971	37971

Note: 6157 young workers newly hired at 2679 workplaces in 2011 after finishing school and followed until 2020. Linear probability regressions models of the young workers' unionisation probability. The colleagues' siblings' union density is introduced as an IV for the colleagues' union density (see text). The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, in other occupations and in other industries than our young workers. Standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.10, ** p<0.05, *** p<0.01

6. Why is young workers' unionisation influenced by colleagues' unionisation?

In this section, we shed light on and test why these young workers' unionization is influenced by their colleagues' unionisation. Does it reflect information sharing between young and more experienced workers? Or, instead, does it follow from social customs (and thus costs)? In Section 2, we pointed out one aspect of the union good (what unions provide to workers) that could be characterised as an experience good. Since union strength is often related to union density, monetary rewards might also drive this relationship. As is often the case with administrative register data, direct evidence is hard to derive but will have to be addressed indirectly.

Our strategy is simple and based on the IV-regressions of Model 2 in Table 2. We treat each characteristic we explore as exogenous and interact it with the union density of colleagues. Since this cross-term will be endogenous, we instrument the cross-term with the interaction of the exogenous characteristic and colleagues' siblings' union density. In Table A4, we present the second stage parameter estimates from these regressions (models M1-M13), showing that the IVs perform strongly across models.

Next, based on these estimates, we predict the estimated spillover effect at the 10^{th} and the 90^{th} percentile for the characteristic of the cross term, as well as the difference between these. Table 5 presents our results. With one exception, the spillover effects are strongly positive and significant. We have split the table in 5 parts reflecting monetary rewards and costs (Panel A), social costs (Panel B), transmission possibilities (Panel C), information sharing (Panel D) and inequality (Panel E). Arguably, age discrepancy and local unemployment rate might reflect more than information sharing (eg. power and protection), but since the union good arguably has a strong experience component, more senior workers should be able to convey this information to younger workers. Similarly, unions contribution to job security and legal help should be easier to convey as job insecurity increases locally.

Table 5 On mechanisms underlying young workers' union membership uptake.
Union spillover effects at different percentiles of the exogeneous dimension

90th percentile		10th percentile		Difference
A) Monetary rewards and costs				
Union wage premium	0.666**	Union wage premium	0.695**	-0.029
	(0.265)		(0.276)	(0.035)
Occupational union dues	0.740***	Occupational union fee	0.685**	0.055
	(0.264)		(0.307)	(0.122)
B) Social costs				
Occupational task freedom of choice	0.576**	Occupational task freedom of choice	0.829***	-0.253**
	(0.278)		(0.279)	(0.289)
Occupational support from colleagues	0.819***	Occupational support from colleagues	0.606**	0.212*
	(0.296)		(0.269)	(0.114)
Physical taxing occupation	0.656**	Physical taxing occupation	0.698**	-0.041
	(0.265)		(0.294)	(0.176)
Mental taxing occupation	0.662**	Mental taxing occupation	0.671**	-0.009
	(0.254)		(0.307)	(0.118)
C) Transmission possibilities				
Occupational remote work	0.527	Occupational remote work	0.630**	-0.103
	(0.365)		(0.262)	(0.216)
Occupational separation rate	0.696***	Occupational separation rate	0.640***	-0.059
	(0.272)		(0.262)	(0.053)
Unionised colleagues' sick pay incidence rate	0.707***	Unionised colleagues' sick pay incidence rate	0.655***	0.048
	(0.289)		(0.272)	(0.159)
D) Information sharing				
Age discrepancy	0.904***	Age discrepancy	0.684***	0.219*
	(0.303)		(0.275)	(0.121)
Local unemployment risk for occupation	0.675**	Local unemployment risk for occupation	0.673**	0.001
	(0.261)		(0.282)	(0.046)
E) Inequality				
Wage dispersion – unionised colleagues	0.637***	Wage dispersion – unionised colleagues	0.744***	-0.107**
	(0.264)		(0.275)	(0.053)
Wage dispersion – non-unionised colleagues	0.649**	Wage dispersion – non-unionised colleagues	0.690***	-0.041
	(0.267)		(0.268)	(0.048)

Note: Population: 49860 young workers newly hired after finishing school employed at 8100 workplaces. Se Table A4 for details. Table elements based on estimation of linear probability IV regression models of young workers union membership uptake. All standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.10, ** p<0.05, *** p<0.01

However, neither for monetary rewards and costs nor for transmission possibilities (Panels A and C), do we observe any significant difference between these groups. Thus, we observe small support for the notion that these spillover effects reflect monetary gains, goods

or rewards (wage premiums and union dues) nor social norm transmission possibilities (as expressed by remote work, union sick pay incidence and worker turnover rates).

For Panels B), D) and E), we observe significant differences. Panel B) reveals that social interactions appear much stronger in high support occupations and weaker in high task autonomy occupations. However, note that it does not matter whether occupations are physical or mental taxing. The key aspect is support. In Panel D), we see that large age discrepancy implies stronger social interactions, which we interpret as evidence of information sharing. Regarding an experience good such as unionisation, it should come as no surprise that more senior colleagues enforce the spillover effects. However, we see that the spillover effects are not sensitive to the outflow to unemployment, indicating that our social interaction effects do not capture union protection or job insecurity information.

Finally, in Panel E) we see that increased earnings dispersion for union colleagues acts detrimental on the spillover reflects. If larger earnings dispersion reflects a weaker union, this influences union colleagues' ability to influence young workers. However, it could also reflect larger inequality and less homogenous colleagues, which also could explain the reduced spillover effects. Panel E) also reveals that earnings dispersion and inequality among non-union colleagues has no impact on the union spillover effects.

7. Heterogenous impact between workers groups and over time?

Finally, we return to questions raised in the introduction: does the relationship between young workers and their colleague changes over time? Do we observe significant differences among other dimensions as well? In Table A5, we present the second stage-parameter estimates from these regressions (models M1-M8), showing that the IVs perform strongly across models. Table 6 presents our results regarding heterogenous spillover effects.

Once more, we see that regardless of groups, the spillover effects are positive, considerable and significant. However, mostly these spillover effects do not vary much between groups. We observe stronger effects as expressed by the point estimates in the public sector than the private sector, stronger effects in small groups than in larger groups, and similarly, stronger impacts in the early period than the later. But neither of these differences are very large and significant. For example, we see only small differences between the early and late period. This might imply that the growth in social media use during this period, particularly among young people, have not replaced social interaction at work as a union recruiting device. Table 5 reveals only one strong exception. We see no interaction effect if the young worker has any union experience before they started in this new job after leaving school. Thus, previous union experience has long-lasting impacts, reducing the importance of colleagues. Still, only a few of our young workers have such previous experience.

Table 6 Heterogenous impacts on young workers' union membership uptake.

Group 1		Group 2		Difference
Men	0.620**	Women	0.680**	-0.060
	(0.286)		(0.290)	(0.050)
Natives	0.702**	Immigrant	0.660**	0.041
	(0.267)		(0.274)	(0.082)
White collar	0.679**	Blue collar	0.547**	0.131
	(0.306)		(0.255)	(0.178)
Large workplace	0.601**	Small workplace	0.742**	0.141
	(0.253)		(0.317)	(0.205)
Large work group	0.651**	Small work group	0.718**	0.067
	(0.272)		(0.291)	(0.102)
Public sector	0.801**	Private sector	0.648**	0.154
	(0.327)		(0.262)	(0.161)
2011-2015	0.673**	2016-2019	0.624**	0.050
	(0.266)		(0.267)	(0.043)
No previous union experience	0.689**	Previous union member	-0.215	0.902**
	(0.263)		(0.262)	(0.067)

Note: Population: 49860 young workers newly hired after finishing school employed at 8100 workplaces. Each row reports estimates and cross-effects from separate linear probability IV models on young workers' union uptake, applying colleagues' siblings' union density as IV colleagues' union density. Se Table A3 for details. Difference expresses the difference between the two groups defined as Group 1 subtracted Group 2. All standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.10, ** p<0.05, *** p<0.01

8. Conclusion

In this paper, we apply the "peers of peers"-approach to identify peer effects in unionization using Norwegian administrative data. We are motivated by recent work showing that social changes at the workplace and in social interactions between old and young workers might influence unionization among newly hired workers. We find strong support for the social customs claim that social interactions are important for unionization. One standard deviation growth in colleagues' union density is across all cohorts associated with roughly 20 percentage points increase in union membership uptake for young workers. Even when we follow one cohort, the 2011-cohort, over time and across workplaces, we find that one standard deviation growth in colleagues' union density is associated with 13 percentage points increase in union membership uptake for young workers. This means that public strategies to bolster unions, such as beneficial tax treatment (Barth et al. 2025), yield additional supportive influence.

We find little support for the notion that the union membership uptake of young workers reflects an overall fixed workplace culture, or rather, any unionisation culture transmitted through family relationships is not clustered at workplaces. This is seen since adding workplace fixed effects to our analyses, yields only minor changes to our estimates. Thus, our interpretation is that social interactions between colleagues are much more important than workplace culture. One could possibly talk about workplace-occupational-specific culture, or sub-culture (Trice, 1993; Jung et al., 2015), but that deviates from the recent literature's emphasis on an encompassing workplace culture. Our claim that these effects are from close colleague peer effects and social interaction is strengthened by additional analyses. For instance, we find no effect of unionization of pseudo-colleagues with similar occupations but employed at other similar workplaces. Similarly, we find no, or only small, impacts related to union monetary rewards and social norm transmission possibilities.

On the other hand, in line with our expectations, we find that occupational colleagues' support matters positively and task freedom of choice matters negatively for the spillover effects, while more senior colleagues enforce the spillover effects. In our view, our set of results provides quite compelling support for the notion that social interactions with close colleagues are important for unionization.

From a public policy point of view, this is important since this means that establishing public policies supporting unions will have additional spillover effects in addition to the direct treatment effect from the policy. However, our analysis also shows that this added support will be diminished in good times, since young workers then enter the labour market more frequently than in bad times. We show that when the incumbent workers are young, they have had less time to experience the union benefits and have had less time to establish strong social customs. Therefore, social interactions and social customs are weakened during business cycle expansion phase and peak, when many young workers and new firms enter the labour market.

For the unions, our analyses also add policy insights. Business cycle expansion phase and peak are challenging times for unions since many new employees weaken social customs, thus demanding union attention. Senior union members, through their experience, are crucial for conveying the contents of the union good. New recruits are important to recruit early, as this has long-lasting impact. As technology redraws the boundaries of the firm and shifts the nature of employment, for example via domestic outsourcing or fissuring (Weil 2014, Goldschmidt and Schmieder 2017) unions face trouble. Automation of, and thus decline, of jobs characterized by routine tasks, and increasing inequality (Acemoglu and Autor, 2011; Acemoglu and Restrepo, 2022). On the one hand, workers employed in these routine task jobs had limited freedom of choice regarding tasks. Thus, if increased freedom of task choice diminishes the union spillover effect, so will the disappearance of routine task jobs and union focus on more Tayloristic production, appear problematic, at least with respect to recruitment. On the other hand, by

strengthening and endorsing the way colleagues support each other, unions also successfully enforce social interaction effects, and bolster union uptake. Whether work is taxing or not (as measured by occupational indexes or sick leave behaviour) matter less for the union spillover effects. We even observe evidence that the growth of social media use the last decade has not replaced the importance of social interaction at work as a union recruitment device.

Relatively big firms increasingly focus on hiring workers with the competence needed to solve the core tasks of their business, while support functions – earlier performed by in-house workers - are bought from other companies. Such a restructuring may lead to greater diversity in the quality of jobs, along dimensions such as union coverage, wages and employment security. As inequality is on the rise, sorting increases, making high wage workers more likely to work in high-wage firms, but high wage workers are also becoming more likely to work with each other (Song et al., 2019). Our results show that inequality and earnings dispersion weaken the spillover effects, though primarily driven by union colleagues. Thus, sustained focus by unions on wage inequality reduction appears essential for social effects to play out.

Our results suggest that unions need to adjust their mobilization strategies to recruit new members, by increasingly try to embed social gains to membership. The changing demands of young workers and the limited importance of encompassing workplace culture, and weaker social interaction for younger colleague groups, implies that unions need more targeted mobilization strategies. Future work should assess the success of such targeted strategies.

Appendix

Tables

Table A1 Descriptive statistics. Main data

Variable	Mean	Standard deviation
Unionisation (young workers)	0.682	0.466
Colleagues' union density	0.649	0.311
Colleagues' siblings' union density	0.431	0.126
Young workers' siblings' union density	0.143	0.334
Young workers' siblings' average earnings	355680.6	122819.3
Woman (young)	0.543	0.498
Immigrant (young)	0.072	0.258
Age (young)	25.507	3.652
Age (colleagues)	41.105	5.845
Workforce size	403.530	1005.641
Occupational size	145.005	327.837
Small workplace	0.518	0.500
Small work group	0.510	0.500
Blue collar	0.146	0.352
Previous union experience	0.058	0.235
Union wage premium	0.036	0.209
Occupational union dues (NOK)	3701.832	834.159
Occupational support among colleagues	1.630	0.166
Occupational freedom of task choice	3.824	0.148
Occupational physical taxing work	3.063	0.212
Occupational mental taxing work	3.893	0.204
Occupational remote work possibility	0.358	0.424
Occupational separation rates	0.152	0.123
Union colleagues' public sick pay incidence rate last year	0.245	0.146
Age discrepancy	0.641	0.305
Earnings dispersion among union colleagues	0.243	0.123
Earnings dispersion among non-union colleagues	0.321	0.169
Local occupational outflow to unemployment	0.021	0.032
Pseudo-Colleagues' union density	0.641	0.305
Pseudo-Colleagues' siblings' union density	0.416	0.144
Pseudo-Colleagues' union density-same occ/municipality	0.467	0.426
Pseudo-Colleagues' siblings' union density-same occ/municipality	0.409	0.321

Note: 49860 young workers newly hired after finishing school employed at 8100 workplaces.

Table A2. IV-assumption: Monotonicity

Dep: Colleagues' union density	Quartiles of the predicted colleagues' union density			
	Q1	Q2	Q3	Q4
	b/se	b/se	b/se	b/se
Colleagues' siblings' union density	**0.074*****	**0.191*****	0.039	**0.029****
	(0.021)	(0.036)	(0.024)	(0.013)
Controls				
Occupation 1-digit FE	Yes	Yes	Yes	Yes
Workplace FE	Yes	Yes	Yes	Yes
Additional controls:	In all models, control for year, woman, age group, workforce and group size, colleagues' siblings' average earnings			
N	12381	12231	12089	12307

Note: 49860 young workers newly hired after finishing school employed at 8100 workplaces. The sample is split into 4 quartiles based on the predicted colleagues' 'union density, based on a regression where all the assumed exogenous control variables are used in the prediction (this regression does not include our instrument). Except for the instrument, this regression equals IV-Model 2 in Table 2. The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, other occupations and in other industries than our young workers. All standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.1, ** p<0.05, *** p<0.01

Table A3 Robustness check: Characteristics of young workers and colleagues' siblings' unionization. IV

Dep:	Colleagues' siblings' union density		Colleagues' union density	
	M1	M2	M1	M2
	b/se	b/se	b/se	
Woman	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Immigrant	-0.001	-0.001	-0.002	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Age	-0.002	-0.001	-0.004	-0.003
	(0.002)	(0.001)	(0.003)	(0.006)
Log age	0.056	0.032	0.120	0.094
	(0.042)	(0.040)	(0.064)	(0.064)
Young workers' siblings' union density			-9.7e-10	6.2e-10
			(1.7e-9)	(1.6e-9)
Young workers' siblings' average earnings			-0.001	-0.001
			(0.001)	(0.001)
Controls				
Occupation 1-digit FE	Yes	Yes	Yes	Yes
Occ. 1-digit X time trend		Yes		Yes
Workplace FE	Yes	Yes	Yes	Yes
Workplace X time trend		Yes		Yes
Additional controls in all models: Year, workforce and group size, colleagues' siblings' average earnings				
N	49860		49860	

Note: 49860 young workers newly hired after finishing school employed at 8100 workplaces. The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities and in other industries than our young workers. All standard errors are adjusted for clustering on the occupationXworkplace level. $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

Table A4. Tests of Mechanisms. Continuous interaction terms. IV. Second stage estimates.

Dep: Union(=1)	Union wage premium	Union fee	Task freedom	Colleagues' support	Physical taxing	Mental taxing	Remote work	Separation rate	Age discrepancy	Local occupa-tional unem-ployment risk	Earnings dispersion union colleagues	Earnings dispersion non-union colleagues	Union colleagues' sick pay incidence
	M1	M2	M3	M4	M5	M6	M7	M8	M9	M10	M11	M12	M13
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Colleagues' union density	0.684**	0.710**	0.718**	0.698**	0.679**	0.667**	0.593**	0.664**	0.800***	0.674**	0.693**	0.669**	0.682**
	(0.271)	(0.282)	(0.273)	(0.276)	(0.267)	(0.278)	(0.285)	(0.265)	(0.283)	(0.271)	(0.269)	(0.271)	(0.269)
Colleagues' union density X column head	-0.075	0.001	-0.813**	0.708*	0.077	-0.018	-0.103	0.216	0.014*	0.033	-0.444**	-0.139	0.137
	(0.092)	(0.002)	(0.373)	(0.381)	(0.323)	(0.238)	(0.215)	(0.192)	(0.007)	(1.050)	(0.223)	(0.141)	(0.452)
Column head	0.060	-0.001	0.287	-0.614**	0.031	-0.054	-0.070	0.198	-0.012**	-0.236	0.227**	0.090	-0.139
	(0.062)	(0.001)	(0.226)	(0.253)	(0.222)	(0.173)	(0.128)	(0.126)	(0.005)	(0.641)	(0.109)	(0.096)	(0.310)
Kleibergen-Paap rk Wald F-statistic	33.4	29.1	34.3	32.1	30.4	35.6	26.3	26.9	33.4	33.6	34.1	34.6	35.7
Additional controls:	In all models, control for year, woman, age group, workforce and group size, colleagues' siblings' average earnings, occupation FE, and workplace FE												

Note: Population: 49860 young workers newly hired after finishing school employed at 8100 workplaces. Second stage estimates from linear probability IV regression models of young workers union membership uptake. Colleagues' union density is instrumented by colleagues' siblings' average union density, while Colleagues' union density X column head is instrumented by colleagues' siblings' average union densityXcolumn head. The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, in other occupations and in other industries than our young workers. Full regression results available from the authors upon request. All standard errors are adjusted for clustering on the occupationXworkplace level. *p<0.10, ** p<0.05, *** p<0.01

Table A5. Heterogeneity. IV. Second stage estimates.

Dep: Union(=1)	Women	Immigrant	Blue collar	Small workplace	Small group	Private	Late period	No union experience
	M1	M2	M3	M4	M5	M6	M7	M8
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Colleagues' union density	0.620**	0.702**	0.679**	0.602**	0.651**	0.801**	0.664**	0.664**
	(0.266)	(0.267)	(0.305)	(0.253)	(0.272)	(0.327)	(0.266)	(0.266)
Colleagues' union density X column head	0.060	-0.041	-0.131	0.141	0.067	-0.154	-0.070*	-0.070*
	(0.051)	(0.082)	(0.178)	(0.204)	(0.102)	(0.161)	(0.041)	(0.041)
Kleibergen-Paap rk Wald F-statistic	34.5	34.6	30.1	29.3	30.4	35.6	34.3	34.3
Additional controls:	In all models, control for year, woman, age group, workforce and group size, colleagues' siblings' average earnings, occupation FE, and workplace FE							

Note: Population: 49860 young workers newly hired after finishing school employed at 8100 workplaces. Second stage estimates from linear probability IV regression models of young workers union membership uptake. Colleagues' union density is instrumented by colleagues' siblings' average union density, while Colleagues' union density X column head is instrumented by colleagues' siblings' average union densityXcolumn head. The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, in other occupations and in other industries than our young workers. Full regression results available from the authors upon request. All standard errors are adjusted for clustering on the occupationXworkplace level. $^{*}p<0.05$, $^{**}p<0.01$, $^{***}p<0.001$

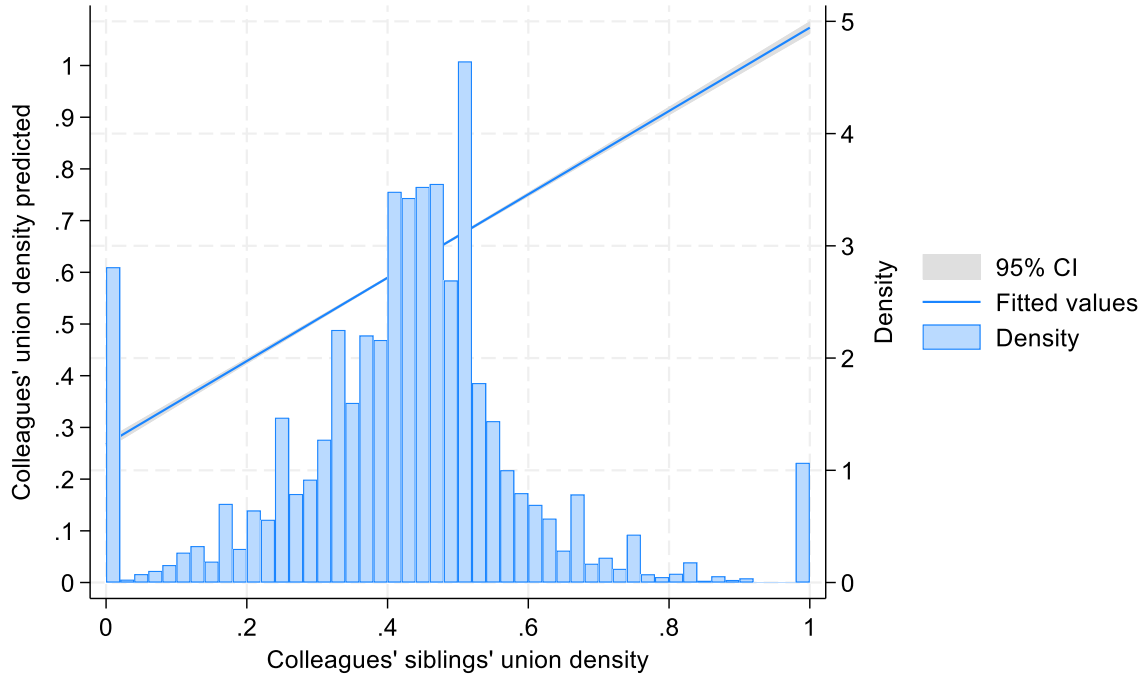
Table A6 Descriptive statistics. 2011-cohort panel data

Variable	Mean	Standard deviation
Unionisation (young workers)	0.701	0.457
Colleagues' union density	0.609	0.292
Colleagues' siblings' union density	0.314	0.130
Colleagues' siblings' earnings	358772.7	118653.7
New job	0.247	0.431
Woman (young)	0.495	0.499
Immigrant (young)	0.063	0.242
Age (young)	29.736	5.088
Seniority	4.102	2.709
Workforce size	512.699	1391.526
Work group size (median=40)	226.109	531.366

Note: 6157 young workers newly hired after finishing school at 2679/4493 workplaces in 2011, but over time are employed in 4493 workplaces. 39576 observations.

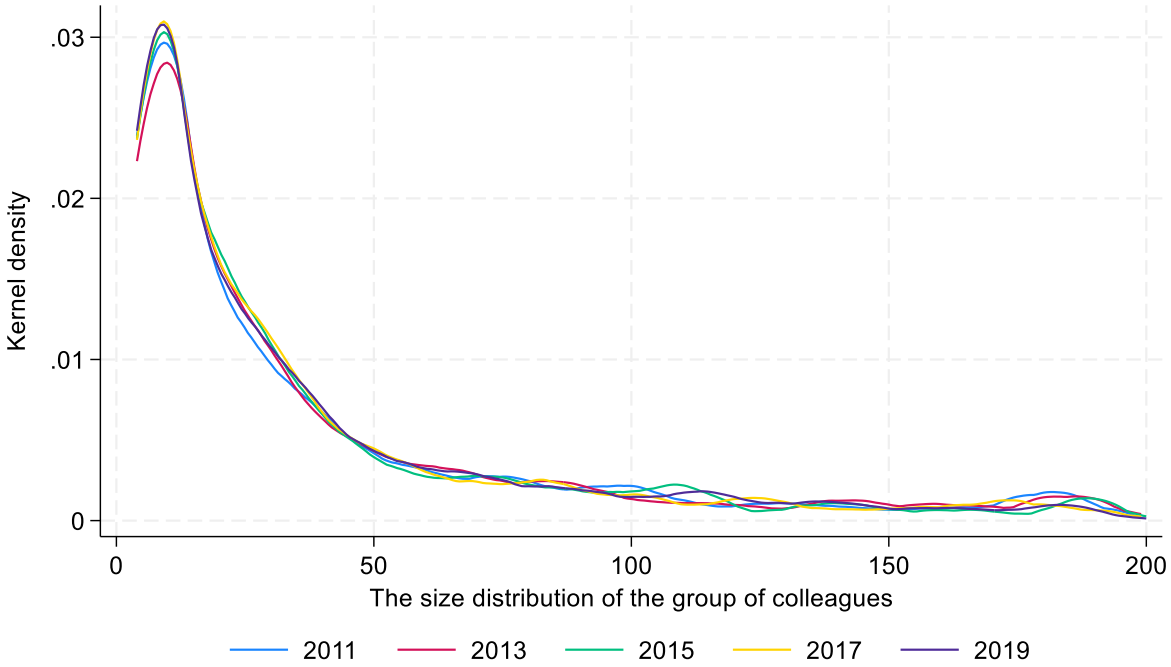
Figures

Figure A1 The relationship between colleagues' union density and colleagues' siblings' union density (first-stage in Table 2-IV-model 2) and the density of colleagues' siblings union density



Note: Population: The colleagues of 49860 young workers newly hired after finishing school employed at 8100 workplaces. The graph depicts the linear relationship between colleagues' union density and colleagues' siblings' union density from a regression controlling for year, woman, age group, workforce and group size, colleagues' siblings' average earnings, occupation FE, and workplace FEs. The colleagues' siblings' union density is calculated based on siblings that work at workplaces located in other municipalities, in other occupations and in other industries than our young workers. Figure A1 also shows the density distribution of colleagues' siblings' union density.

Figure A2 The size distribution of young workers' colleague groups



Note: Population: The colleagues of 49860 young workers newly hired after finishing school employed at 8100 workplaces. Selected cohort-years.

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