Can health promotion in the workplace save public money? Evidence from a quasi-experiment in Denmark

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ABSTRACT

This paper estimates the effects of a workplace health promotion program adopted in selected blue-collar trades in Denmark on public expenditure-related outcomes. A key policy question in many countries is whether such programs represent an effective means of reducing some of the public's financial burden. To uncover the causal effects, we employ a difference-in-differences strategy that exploits an exogenous variation in the timing of program assignment based on collective agreements. The empirical analysis is based on unique longitudinal administrative register data. We find suggestive evidence that the program has the potential to save public money through a reduction in publicly paid sick-leave compensations up to 15% in selected trades, notwithstanding the benefits accruing to companies and workers from reduced sickness-related absence. In contrast, mixed evidence is found for a reduction in use of publicly financed health care services, at least in the short- and medium-term. Considerable heterogeneity in effects is found across different settings. In particular, the effects of program assignment appear to be smaller in small companies. This suggests that these types of programs may not have the same cost-saving benefits in all settings.

I dette arbejdspapir fremlægges de første resultater af effektanalyserne af sundhedsordningen, PensionDanmark Sundhedsordning. Studiet baserer sig på et unikt longitudinalt registerbaseret datasæt og et difference-in-differences design, hvor det udnyttes, at forskellige overenskomstområder fik sundhedsordningen på forskellige tidspunkter. Analyserne viser, at for en række overenskomstområder der er blevet omfattet af sundhedsordningen, er andelen af personer på sygedagpenge reduceret i et givent år med op til 15 %. Effekterne er dog ikke ens for alle, særligt ses en betydelig variation på tværs af brancher og virksomhedsstørrelse. Derudover ses ikke en entydig effekt på de omfattedes forbrug af sundhedsydelser. Således peger analyserne på, at en målrettet indsats inden for sundhed og forebyggelse ikke kun kommer de ansatte og virksomhederne til gode, men også de offentlige kasser i form af reducerede sygedagpengeudbetalinger.

Keywords: workplace health promotion, public expenditures, difference-in-differences, intention-to-treat, quasi-experiment, sickness-related absence, health care use

JEL classification: H51, I11, I18

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Content

1. INTRODUCTION

This paper explores whether health promotion in the workplace can save public money. Workplace health promotion programs[1] are employer-sponsored initiatives that are intended to prevent the onset of disease or its progression from an early unrecognized stage to a more severe stage (Goetzel and Ozminkowski, 2008). These programs have existed for many years in the U.S. (Warner, 1990) as a popular loss prevention business strategy (Kenkel and Supina, 1992) or as a form of non-wage compensation (Woodbury, 1983). Several U.S.-based studies have reported positive financial returns to companies that invest in these programs; for a recent survey of the literature, see, for example, Baicker et al. (2010) and (Osilla et al., 2012). Some studies have also examined the effects on Medicare expenditures among senior employees (Goetzel et al., 2007). In contrast, the value of these human capital investments in employee health and well-being remain surprisingly understudied in countries with universal, tax-financed health care systems such as Denmark (Kirsten and Karch, 2011).

The purpose of this paper is to estimate the causal effects of a workplace health promotion program adopted in selected blue-collar trades in Denmark on employee sickness-related absences from work and the use of health care services. An important difference in evaluating the effects of such programs in countries with universal, tax-financed health care systems compared with countries in which employers also sponsor health insurance providing access to health care and workers' compensation lies in the sharing of the costs and potential benefits of such programs. Whereas employers only bear the after-tax costs, the public systems (or society as a whole) reap a high share of the potential benefits in terms of savings in health care and social security expenditures. Hence, from a public policy perspective, a key question is whether these programs represent an effective means of alleviating some of the financial pressure on public systems, while improving the health of the workforce.

Therefore, the main contribution of this paper is that, to the best of our knowledge, we provide the first empirical evidence of the possible effects of private investments in workplace health promotion programs on public expenditure-related outcomes. This evidence is timely, as increasing health care expenditures that are only rising with an aging workforce have prompted policy makers in many European countries to reexamine the organization of their health care systems and place a growing emphasis on health promotion and disease prevention as a possible way of achieving better health at lower costs (Kirsten and Karch, 2011).

[1] Many other terminologies have been used in the literature, such as workplace wellness programs, health enhancement, health and productivity management and workplace disease prevention programs.

Another main contribution of this paper lies in the different approach we take to measure the effects of the program. We use an intention-to-treat (ITT) approach and evaluate the effects of treatment eligibility, which mimics the anticipated effect of increasing the availability of these programs in the workforce. Most previous studies of workplace health promotion programs have focused on estimating as-treated effects (Baicker et al., 2010). However, because of the voluntary nature of most programs some eligible individuals in the target group do not actually receive treatment (in Denmark, participation levels in workplace health promotion activities have been reported to vary from 13-34% (Danish Working Environment Council, 2009)). Furthermore, those who do receive treatment initially tend to be healthier and more highly motivated to begin with than those who do not accept (or are uninformed of) the offer to receive treatment. Therefore, we believe that the ITT is a more salient effect to evaluate from a public policy perspective because policy makers can exert efforts to increase the availability of these programs, for example through the preferential tax treatment of health promotion activities in the workplace (Danish Ministry of Taxation, 1995), yet they cannot force workers to actually comply with such programs.

The program considered in the present study differs from the traditional workplace health promotion programs considered in the literature in that this program is not directly organized by the employer; rather it is organized by a labor market pension fund and assigned on the basis of collective agreements. Therefore, another important aspect of the study is that we can use the institutional feature that the program was assigned on the basis of collective agreements (and was thus compulsory to all workers who are covered by a collective agreement in a given trade) to construct a novel quasi-experimental research design, which, under explicit assumptions allows for an estimation of the causal effects of eligibility into the program. The empirical challenges in any study of workplace health promotion programs are that companies may choose to invest in these programs based on unobservable factors, such as their expected returns or social responsibility and that workers may select companies that offer such programs based on unobservable factors such as health endowments; these possibilities may bias naïve comparisons. Previous studies have relied on different control strategies to eliminate some of this bias by controlling for worker and company characteristics (Baicker et al., 2010), but the possibility for bias from unobserved variables remains. In the current study, we take advantage of the fact that the program was assigned to virtually entire trades at different times in a difference-in-differences framework, which enables us to take into account fixed unobserved heterogeneity in outcomes. Using data on several periods before the program was assigned and a set of pseudo-outcomes we provide support for the validity of our identification strategy.

The empirical analysis presented in this paper is based on unique data pertaining to program coverage from the labor market pension fund that organized the program. These data are combined with individual-level administrative register information on public health care service use and publicly paid sick-leave compensation as well as some background characteristics from the 2002-2010 period. This approach is unique in that information regarding these programs typically does not exist in any administrative registers, which represents a frequent obstacle in evaluating such effects.

To preview our main results, we find that program assignment has reduced the number of workers in selected blue-collar trades who receive any publicly paid sick-leave compensation by up to 15% and, thus, has the potential to save public money, notwithstanding the benefits accruing to companies and workers from reduced sickness-related absence. In contrast, mixed evidence is found for a reduction in publicly financed health care service use, at least in the short and medium term. We find considerable heterogeneity in the effects across target groups; thus our results suggest that such programs may not have the same cost-saving benefits in all settings. In particular, we find the largest effects in a trade dominated by a single large company and the smallest effects in trades dominated by many small companies that may not have the resources and social networks to effectively put such programs into practice. Hence, from a policy perspective, it may not be sufficient merely to increase the availability of these programs in the workforce. Attention and resources should also be devoted to the actual implementation of such programs in the workplace. This difference in capabilities should be considered when generalizing the results to other settings.

The paper proceeds as follows: Section 2 provides background on the institutional features in Denmark and the workplace health promotion program considered. Section 3 discusses our data and empirical strategy. Section 4 presents our results, which are discussed in section 5.

2. BACKGROUND

2.1 Institutional environment

In Denmark, an employee who is unable to work because of illness or injury is entitled to receive publicly paid sick-leave compensations from the health and social security administration of the municipality in which he or she lives. The employee must have been employed by the employer for the last 13 weeks and have worked at least 120 hours during this period. Publicly paid sick-leave compensations can normally be received for a maximum of 52 weeks during a period of 18 months and equals full wage compensation up to an amount that equals the maximum unemployment benefits (Johansen et al., 2008). No distinction is made between sick-leave periods due to work-related or non-work-related causes. The publicly paid sick-leave compensations is generally paid after 3 consecutive weeks of absence (before July 2008, this period was 2 weeks). In instances in which an employee receives full wages during his or her absence, the employer is reimbursed by an amount that corresponds to the compensations paid. Under certain conditions, employees suffering from chronic illness may enter into an agreement with their employer whereby the authorities reimburse the employers for the entire period of absence. Furthermore, small private companies can purchase a sick-leave insurance, whereby the authorities reimburse employers for an entire period of absence, excluding the first day. In total, more than 60% of all sickness-related absences from work in Denmark are covered by publicly paid sick-leave compensations (Danish Ministry of Employment, 2008). Public expenditures for sick-leave compensations amount to DKK 12 billion per year, which is approximately 0.8% of GDP (Danish Ministry of Employment, 2008). For more information on the Danish sickness benefits scheme see, for example Johansen et al. (2008).

The Danish health care system is a comprehensive tax-financed system with universal access. Treatment by general practitioners and specialists, outpatient ambulatory care and hospitalization are free at the point of use for all citizens. General practitioners act as gatekeepers to more specialized care. There are considerable private copayments for adult dental care, prescription medicine, physiotherapy, chiropractic care and psychological counseling (Strandberg-Larsen et al., 2007). Of particular relevance to the present study are the copayments for physiotherapy and chiropractic care. The public health insurance reimburses only 40 percent of the fee for physiotherapy (Ministry of Health, 2008). The treatment is only reimbursed if it has been prescribed by a general practitioner, which is the case for most physiotherapy use in Denmark (Association of Danish Physiotherapists, 2010). Public health insurance also reimburses part of the fee for chiropractic care. The specific amount depends on the services that are delivered and ranges from 15 to 25% of the fee. It is not necessary to be referred by a general practitioner to receive reimbursement (Ministry of Health, 2010).

2.2 The program under study

The program under study is one of the most common types of workplace health promotion programs in Denmark and is organized by a labor market pension fund. Since 2005 it has been assigned to several blue-collar trades on the basis of collective agreements since 2005; see Table 1. Because of the high physical workload and health risks, blue-collar workers assume relatively high risks of temporary or more permanent impaired work ability, thus representing an obvious target group for health promoting activities in the workplace also from the perspective of a pension fund that will ultimately assume the costs of impaired workability in terms of disability pensions. At the core, the program is a comprehensive musculoskeletal disorder prevention program that supports primary, secondary and tertiary prevention efforts by providing timely access to appropriate intervention services before, during and after a work-induced musculoskeletal event occurs with the purpose of preventing its progression into an injury. The specific treatments include manual therapy, resistance training, massage and electrotherapy, joint manipulation and other non-surgical and non-medical types of treatments such as exercise, dietary advice, ergonomic advice and soft tissue treatment. The treatments are delivered by a multidisciplinary team of physiotherapists, chiropractors, reflexologists and massage therapists at clinics located near the worksite with the exception of most dairy workers, who have actual onsite access to treatment. The clinics are managed by a large Danish private health care provider that is independent of the employer (Falck Healthcare). Studies have shown that when these intervention services are appropriately applied, they may represent an effective means of treating musculoskeletal disorders in the workforce (Beckerman et al., 1993; Ernst, 2009; Green et al., 2003; Hernandez-Reif et al., 2001; Tsao, 2007). In addition, the program provides 24-hour counseling service regarding mental health problems, such as stress, bullying or work-related accidents; an anonymous helpline for substance abuse; and elements of demand management in the form of advice on the public healthcare system on matters that include waiting lists, free choice of hospital, reimbursement of medicinal products and rehabilitation. These services are delivered by psychologists, nurses and substance-abuse counselors.

Worker alone make decisions to receive treatment and such decisions are generally not required to be communicated to employers. There are no restrictions on the number of treatment sessions. However, the interventions can be used only for the prevention and treatment of work-induced injuries and illnesses to qualify for exemption of a workplace fringe benefit; thus, such interventions must not be used to treat injuries that occur outside out of work time (Danish Ministry of Taxation, 1995). The initial treatment is always delivered by a physiotherapist who determines on the specific course of treatment. Access to treatment is available without a referral from a general practitioner and is free for workers. For acute injuries, treatment is delivered within 24 hours, whereas non-acute injury the treatment is delivered within 4 days. The underlying hypothesis is that removing barriers to access, such as copayments and other components of the “full price” including the opportunity costs of seeking treatment in terms of time lost on the job and reduced waiting times for treatment, will encourage greater (and possibly more timely and appropriate) utilization of the services that are included in the program (Cauley, 1987; Lillard et al., 1986). This outcome is likely to affect the health and well-being of the workforce and ultimately save public money.

3. MATERIAL AND METHODS

3.1 The basic idea of the study design

We use terminology from the treatment effect literature (see e.g. (Heckman et al., 1999)), and thus denote "treatment" as having used the program, and "assignment-to-treatment" as the event in which the program is included in a particular trade's collective agreement. As noted previously, our main approach contrasts workers assigned to the program who were eligible to be treated with workers without such assignment. Evaluating the effects of treatment intention rather than actual treatment received has the advantage that health-related selection issues are less likely to be a concern. Nevertheless, workers with program assignment may still differ from workers without assignment with respect to important characteristics.

To exploit a plausibly exogenous variation in the program assignment we take advantage of the fact that it is based on collective agreements and thus is compulsory for most workers (i.e. all workers covered by a collective agreement[2]) in a given trade assigned-to-treatment (see Table 1 and figure 1). This has the main advantage that program assignment may be perceived as an exogenous shock because it essentially becomes a collective choice made by trade unions and employer organizations rather than an individual choice made by the worker.

Next, we construct a quasi-experimental research design and select a comparison group of workers who were not assigned to the program by using the fact that different trades were assigned to treatment at different times. We argue that the timing of assignment-to-treatment may be perceived as random within main industries in an economic or statistical sense. This allows us to contrast the change in outcomes over time for workers who gained coverage by being assigned through the collective agreement at a particular point in time with a comparison group of workers employed in similar trades within the same main industry not yet assigned-to-treatment during our period of observation. This is the basic setup of a difference-in-differences analysis. Below we provide support for our identification strategy.

3.2 Description of the data

Our main source of data contains information on individual-level program coverage for workers in some of the major trades. Using unique personal identification numbers assigned to all persons who have permission to stay in Denmark for at least 3 months (Pedersen, 2011), we augment this data source with longitudinal information on register-based event histories in terms of publicly paid sick-leave compensations, the use of public health care services, and person- and company-level background variables from 2002-2010.

[2] In Denmark most (approximately 80%) wage earners are covered by a collective agreement.

Sample selection

Our quasi-experimental intervention group is composed of the entire collective agreement-covered population of hairdressers, fishermen, plumbers, chimney sweepers, electricians and dairy workers who had program coverage at some point before 2010 (>50,000 individuals). To exploit the program assignment based on collective agreements, we take into account the sample composition by dividing the group of covered workers according to how they gained coverage: those workers who were already employed in the above trades before they were assigned-to-treatment and those who changed into the above trades at some point after they was assigned-to-treatment. By focusing only on the former group we exclude workers who potentially self-selected into the program. This information comes from the Register-based Workforce Statistics. To avoid potential anticipatory effects, we consider only workers who were employed in the above trades for at least two years prior to assignment-to-treatment.

To investigate to what degree promising programs are transferable across subgroups of settings and target groups, we construct separate samples for each of the trades. However, we combine the group of plumbers and chimney sweepers because of the small sample size in the latter group. Previous work have primarily been restricted to the setting of a single large company, which raises the question of external validity (e.g. Bertera, 1990; Goetzel et al., 1998; Knight et al., 1994; Leigh et al., 1992; Ozminkowski et al., 1999).

From a 20% random sample of the Danish working population identified using the Register-based Workforce Statistics, we extract a comparison group for each of our target groups composed of workers employed within the same main industry (NACE) in which program assignment was absent during our period of observation. However, because the other services activities industry comprises a very heterogeneous group of trades, we extract a comparison group for the hairdressers composed of workers who are employed in the retail sale of goods in specialized stores, such as cosmetics. We follow each target group and its comparison group from three years before the program was assigned to the target group and up until five years after. If an individual, for example, dies within this period, then the individual only contributes to the mean outcome until the year in which he/she dies.

We performed an extensive review of all collective agreements to identify and exclude any workers from the comparison groups who were covered by similar collective agreement-based programs during our period of observation. These workers included painters who were assigned to a similar workplace health promotion program that was organized by another private provider during 2009 and were thus excluded from our analysis.

We restrict our attention to workers who were between 18 and 60 years of age during our study period. The upper limit on age is defined to ensure that there is a considerable likelihood that the individuals are working during the entire study period (i.e., we exclude individuals above 60 years of age because they may be eligible for retirement programs). We further restrict our sample to only wage earners (unskilled, skilled and salaried workers) because we are evaluating a collective agreement-based program. Sickness-related absence behavior and health care use of self-employed individuals is expected to differ to some extent from that of wage earners. Furthermore, self-employed may choose to enroll in similar programs for which we have no information. We observe some self-employed individuals in the comparison groups and they are excluded from the analysis. Table 2 shows the final sample compositions of our constructed samples.

Variables

Because the perspective taken in the present study is that of the public sector expenditure effect, we focus on outcomes that i) may be affected by the program and ii) affect public expenditures, as we do not have access to actual expenditure data. We study the following outcomes:

Publicly paid sick-leave compensations. Our main dependent variable is the likelihood of publicly paid sick-leave compensations during a given time period. It is defined to be unity for workers with any publicly paid sick-leave days irrespective of the cause for which they are paid and zero otherwise. These absences may include multiple periods of sick-leave and non-musculoskeletal disorder-related causes. This information is obtained from DREAM, which is a national database maintained by the Ministry of Employment that contains information on all social transfer payments for all citizens in Denmark (National Labour Market Authority, 2010). We choose this measure because the primary purpose of the program is to prevent the occurrence or progression of musculoskeletal disorders into injuries and thus potentially avert periods of publicly paid sick-leave compensations. We also construct a second outcome, which is unity for workers with sickness-related absence periods of more than three consecutive weeks for which publicly paid sick-leave compensations have been paid to investigate the effect on long-term periods of sickness-related absence.

Use of publicly financed health care services. Another possible effect of the program is the reduction of employees' use of publicly financed health care services. To investigate this, we also study a range of secondary outcomes extracted from a host of Danish administrative health registries. From the Register of Medicinal Product Statistics (Wallach Kildemoes et al., 2011), we obtain information on all prescribed medicinal product use dispensed at pharmacies. We focus on the likelihood of dispensing products that are prescribed for the musculoskeletal system (ATC M) during a given time period. Further, we study the likelihood of public hospital contacts (inpatient, outpatient or emergency room) relating to musculoskeletal disorders during a given time period defined by the ICD-10 XIII (Diseases of the musculoskeletal system and connective tissue) extracted from the National Patient Registry (Lynge et al., 2011).

We also study the likelihood of publicly reimbursed physiotherapy use during a given year (i.e. use that is not covered by the program). This information is obtained from the National Health Service Registry (Sahl Andersen et al., 2011). We expect assignment to the program to reduce the likelihood of using publicly reimbursed physiotherapy because some public reimbursed use is likely to be substituted with private use that is covered by the program. We do not consider publicly reimbursed chiropractic care as outcome because this specific program has entered into agreements with chiropractor caregivers that were also covered by the national health insurance. Therefore, we cannot distinguish the use of chiropractic care through the program from other use.

Our last outcome is the number of general practitioner contacts during a given time period (visits, phone consultations or home consultations). This is also obtained from the National Health Service Registry, but this registry does not contain information on the causes of specific contacts. Therefore, this outcome also includes non-musculoskeletal disorder-related contacts. Nevertheless, we expect the program to reduce the number of GP contacts during a given year through a reduced need for prescribing medication to treat musculoskeletal disorders or referrals to physiotherapy or public hospitals.

Control variables. In our data, we have access to a set of background characteristics pertaining to both the worker and the company levels. These data include information on gender, age, ethnicity and marital status as defined by a dummy variable that is unity for individuals who are married or cohabiting; the number of children in a household; and the region of residence in a given time period extracted from the Register of Population Statistics. We include information from the Register of Education and Training Statistics on highest completed educational attainment as defined by a dummy variable that indicates whether an individual has attained a primary or lower-secondary level of educational in a given time period. In addition, we include information on job functions as defined by dummy variables for managers, salaried workers and skilled workers as well as a dummy variable for part-time employment obtained from the Register-based Workforce Statistics. Finally, we include information on company size, which is defined by the number of employees and the number of workplaces within a company during a given time period. This information is obtained from employee-employer matched data from the Integrated Database for Labor Market Research.

Summary statistics

To describe the data available and compare the target and comparison groups, Table 3 reports the means for selected variables measured in the year prior to assignment-to-treatment. Statistics are broken down by target and comparison groups for each sample.

We observe some differences in outcomes between target and comparison groups before the program was assigned; these differences suggest that the assignment-to-treatment is not random and that such differences before assignment-to-treatment must to be considered for when estimating the causal effects of the program. However, it appears that the program was not systematically assigned to trades with the highest level of sickness-related absence and health care use. Furthermore, we observe that the target and comparison groups are fairly homogeneous with respect to several of the conditioning variables. In contrast, we observe substantial differences in the means across target groups especially with respect to outcomes and company size.

3.2 Empirical framework and estimation

We are interested in the effect of program assignment on publicly paid sick-leave compensations and health care use. As previously noted, we apply a difference-in-differences strategy to account for fixed unobserved heterogeneity in outcomes. For example, Table 3 indicates that dairy workers had slightly lower sickness-related absence than other manufacturing workers already before they were assigned to the program. It is possible that dairy workers represent an otherwise healthy group of workers compared with other manufacturing workers. The failure to consider this would suggest that the program is reducing sickness-related absence more than it actually is. Because we have longitudinal data where we follow the same individuals over time, our empirical models are estimated using a series of fixed effect model specifications. Our primary estimating equation is as follows:

$$y_{it} = \rho_t + \beta d_{it} + \delta X_{it} + a_i + \varepsilon_{it} \qquad (1)$$

where *i* indexes individuals and *t* time periods, which represent 52 weeks of observation rather than calendar years; y_{it} is the outcome of individual *i* in time period *t* (e.g. the likelihood of publicly paid sickness absence compensations); a full set of time period dummies, ρ_t, is added to control for common time shocks that might affect the outcomes even in the absence of assignment to the program; and X_{it} contains a set of standard control variables that may be unaffected by the program as described in section 3.1. The individual-specific fixed effects, a_i, refers to time-invariant unobservables such as initial health endowments, gender and (when the time horizon is not too long) also factors that include physical and psychosocial work-related factors, health risk and motivation. ε_{it} is an idiosyncratic error term assumed independent of all others terms in the equation. The policy variable of interest is the program dummy variable, d_{it}, which is unity for all time periods after program assignment for workers in the target groups. Hence, captures the ITT effect of program assignment if certain assumptions hold – the change in outcomes after program assignment relative to before and relative to the comparison group.

Despite the limited dependent variable nature of the outcomes, we use linear probability models for all outcomes: the fixed effect analysis of equation (1) is conducted via a "within" transformation. There are two practical reasons for this choice: first, linear analysis provides us with results that are easier to interpret; second, the panel estimation is able to appeal to the entire sample rather than the sharply reduced sample under, for example, the conditional fixed effects logit that respects the binary nature of the dependent variable and in which identification is based only on persons who experience changes in the dependent variable e.g. both any and no sickness absence over time (Wooldridge, 2002). Given the large mass point at zero for many of the outcomes, such nonlinear analysis would result in a sharp reduction of the sample size. Pragmatically, linear and nonlinear analyses often produce similar results when the specification is nearly saturated in the independent variables (Wooldridge, 2002).

In addition to the discrete "before/after" setup considered above, we consider an equation in which we relax the restriction that the program has the same effect in every time period after assignment-to-treatment imposed in equation (1) by replacing the single program dummy variable, d_{it}, with a series of dummy variables for each time period after program assignment. This allows us to investigate possible dynamics of the effects of the program over time. It is possible that there is no short-term effect but that the effect appears only some time after program assignment (for example, if workers need time to adapt to or learn about the existence and use of the program). In this case, the estimating equation is as follows:

$$y_{it} = \rho_t + \sum_{\tau=0}^{\mathrm{T}} \gamma_\tau D_{it,\tau} + \delta X_{it} + a_i + \varepsilon_{it} \qquad (2)$$

where the period of program assignment beginning with the week of assignment-to-treatment is normalized to zero, $\tau=0$, and $\tau=1,\ldots,\mathrm{T}$ denote the subsequent time periods after program assignment. Specifically, we include a dummy variable that is unity in the period in which the target group was assigned to the program, $D_{it,\tau=0}$. In addition, lagged dummy variables that are unity in subsequent time periods after program assignment, $D_{it,\tau}$ $\tau>0$, are included to capture any effect dynamics. Hence, γ_0 provides an estimate of the short-term effect, and the $\gamma_{>0}$ measure the medium-term effects. The augmented model allows us to investigate whether the effects, for example, accumulate over time so that γ_τ increases in τ. In contrast, if the effects are stable over time, then we should expect to find γ_τ's to be the same.

Our identification strategy is based on the assumption that the *timing* of assignment-to-treatment is plausibly random such that our target groups would have had the same trend in outcomes as the comparison groups in absence of being assigned to the program. A violation of this assumption could occur if the assignment-to-treatment was a response to a *relative* increase in sickness-related absence and health care use within industries and thus potentially overestimating our effects. Because we have access to several time periods before program assignment, it is possible to provide some insight into the credibility and robustness of our identification strategy. In Figure 2 to 6, we plot the evolution of our outcomes over the time period of observation in which the time period of program assignment is time '0'. For each outcome, we observe that the lines are perceptibly parallel for the target and comparison groups in the three years before assignment-to-treatment. This indicates that workers who are employed in trades within the same main industry (e.g. the construction industry) are likely to be exposed to similar aggregate shocks to their sickness-related absence and health care use. Thus, the figures provide suggestive evidence to support the assumption of equal time trends in absence of program assignment. Moreover, it should be kept in mind that all figures draw raw, unconditional pictures. In the parametric regressions, we additionally control for various influential factors that may lead to differential trends. To more formally assess whether the trends in outcomes are parallel before program assignment, we can augment equation (2) with a set of leads of the program dummy variable for individual *i* at time period *t*:

$$y_{it} = \rho_t + \sum_{\tau=-2}^{-3} \gamma_\tau D_{it,\tau} + \sum_{\tau=0}^{T} \gamma_\tau D_{it,\tau} + \delta X_{it} + a_i + \varepsilon_{it} \qquad (3)$$

Specifically, we include a dummy variable, $D_{it,\tau=-2}$, which is unity two time periods before program assignment and $D_{it,\tau=-3}$, which is unity three time periods before program assignment, whereas $D_{it,\tau=-1}$ is omitted from the model as a reference category. A test of whether the trends are parallel before program assignment is that $\gamma_{-\tau}=0$ for all $\tau<0$. In contrast, a nonzero effect would indicate that the trends in outcomes before program assignment are not parallel and that a common trend in absence of program assignment would thus be harder to justify.

4. RESULTS

4.1 Main results

In Table 4 and 5 we report the reduced form ITT effect of program assignment on publicly paid sick-leave compensations and health care use. Each coefficient represents the estimate from a separate regression and can be interpreted as the average yearly effect across all periods after program assignment. We present estimates both from an unconditional difference-in-differences model that controls only for fixed unobserved heterogeneity in outcomes and common time trends as well as estimates for which the full set of controls described in section 3.2 are added to the model. However, the results are not particularly sensitive to the inclusion of these control variables, which adds to the credibility of the assumption of common time trends and to the assumption that assignment of eligibility into the program is random.

Consider first the effect on the likelihood of publicly paid sick-leave compensations. The empirical results reported in Table 4 suggest that there is notable heterogeneity in the effects of program assignment across the target groups. Among dairy workers, we find that program assignment has significantly reduced the likelihood of receiving any publicly paid sick-leave compensations during a given year by approximately 0.014. The size of the effects is generally large relative to the proportion of workers with any publicly paid sick-leave compensations prior to program assignment, see Table 3. The size of the effect corresponds to a 15% reduction in the proportion of dairy workers with any publicly paid sick-leave compensations during a given year. Smaller effects are found within the construction industry. Here, program assignment has reduced the proportion of plumbers including chimney sweepers and electricians with any publicly paid sick-leave compensations by approximately 7% and 10%, respectively. In contrast, we find no effects among fishermen and hairdressers (we observe some significant effects among hairdressers, but these effects are likely to be explained by a pre-existing trend as we discuss below). These results are generally supported for both any publicly paid sick-leave compensations and periods of absence of more than three consecutive weeks, except for plumbers and chimney sweepers for whom we find no effect on periods of absence of longer than 3 weeks.

Turning to the effects on publicly financed health care service use reported in Table 5, we find that the program has substituted for some of the publicly reimbursed physiotherapy use by physiotherapy services covered by the program. In particular, we find a reduction of up to 32% (depending on trade) in the proportion of individuals who use publicly reimbursed physiotherapy during a given year. Again, the largest effect is found among dairy workers, whereas no effects are found among fishermen and hairdressers. This corresponds well to the effects on sickness-related absence outcomes and suggests that the uptake of the program may be higher among dairy workers. For the remaining health care services, we find no strong evidence of an effect in either of the target groups, at least in the short- and medium terms considered. However, the evidence appears to weakly suggest that program assignment has reduced the number of general practitioner contacts among electricians; this effect is rather small (approximately one fewer contact per five years). Overall, this finding corresponds well to the results from the literature reporting that individuals with musculoskeletal disorders frequently avoid physicians, blue-collar workers in particular. For example, Lipscomb et al. (2009) found that more than half of their sample of a fifteen-year cohort of union carpenters with back injuries did not seek medical care from either union-provided health insurance or workers' compensation. Hence, the potential for an impact on health care use may be relatively low.

As a sensitivity check we also present results in which we add a group-specific linear time trend to the list of controls. In this case, the identification of the effects is derived from whether program assignment leads to a deviation from a pre-existing trend. We observe that the effects on publicly paid sick-leave compensations remain largely unchanged for dairy workers and thus confirm the robustness of the result. For construction workers the effect becomes somewhat smaller and marginally insignificant for plumbers and chimney sweepers. In contrast, the effect estimates become insignificant among hairdressers when the group specific linear time trend is added. Regarding publicly financed health care use, the results further suggest that assignment to the program has reduced the use of medication among dairy workers corresponding to a decrease of approximately 13% in the proportion of workers who use prescription drugs for the musculoskeletal system. Next, we consider some extensions of the model to provide insight into the possible heterogeneity in the effects across the target groups.

4.2 Heterogeneity among subpopulations

First, we investigate whether there are subgroups of workers who particularly benefit from being assigned to the program. For this purpose, we report in Table 6 the effect estimates from the largest target group (i.e., electricians) separately for different subsamples defined by a selection of characteristics measured around the time of program assignment.

Considering first the results by age, we divide the sample according to the median age in the target group (40 years) in the period of program assignment. We observe that program assignment appears more beneficial for the older population. In particular, we find larger effects for the 41- to 60-years old population than for the 18- to 40-year olds. This is not surprising because the occurrence of musculoskeletal disorders reportedly increase with age (e.g. Holmström and Engholm, 2003). In particular, the older groups of workers may have been exposed to physical factors at work for a longer period of time and thus have a higher need for health-preserving activities.

Although the differences are small, it is also interesting that the effects increase in magnitude for individuals who do not live alone in the period of program assignment. This consistent with the literature on the family as a producer of health (e.g. Bolin et al., 2002).

We divide the sample according to whether the individuals have used medication for the musculoskeletal system in the period before program assignment. Those individuals may represent a particular target group with a potential for substantial health impact of program assignment. We generally observe that the effects are, to some extent, higher for the group of individuals who have used medication for the musculoskeletal system compared to individuals without such use.

We also report estimates separately for individuals who are employed in companies with more (less) than the median number of workers in the target group (i.e., 30 employees) in the period of program assignment to investigate whether similar effects of program assignment can be achieved in small companies compared to large companies, which is an important policy question (Baicker et al., 2010). Small companies may not have the resources and economies of scale to put such programs into practice (e.g., a human resource department), which may limit the diffusion of program participation and health behavior though, for example, employee social networks and corporate culture. We observe that the effects on publicly paid sick-leave compensations are nearly twice as high for individuals who are employed in companies with more than 30 employees than for individuals who are employed in companies with 30 or fewer employees. This suggests that company size is important and may explain some of the heterogeneity in effects across the trades.

Finally, we find some regional variation in the size of the effects, particularly when considering sickness-related absence. We find the largest effects in the Central Denmark Region and the Capital Region of Denmark, which is where the two largest cities in Denmark are located. Hence, the effects appear to be larger in urban populations. However, this may also be explained by that larger companies are located near the largest cities.

4.3 Effect dynamics

Next, we examine the timing in which the effects occur, which may also explain some of the heterogeneity across the target groups and is of direct policy interest. For example, evidence suggesting only little effect in the short-run may understate the impact of such programs if the effects primarily manifest a certain amount of time after the program assignment either because the effects of prevention on health take time to appear or because workers need time to learn about the existence and use of the program.

Figure 7 and 8 show how the effect on our main outcomes, the likelihood of publicly paid sick-leave compensations, evolves over time in each of the target groups. In this model, the difference in outcomes between the target and comparison groups at a given time period is contrasted with the difference in outcomes in the time period immediately before program assignment (τ=-1), which is normalized to zero in the figures. We observe that the effect occurs instantaneously among dairy workers (i.e., in the year of program assignment) and stabilizes one year after (i.e. all after-program assignment coefficients are jointly equal, $p<0.01$). In contrast, for construction workers, we find that the effects generally manifest one year after the program assignment and stabilizes two years after. This may contribute to some extent to the heterogeneity in effects between dairy and construction workers reported in Table 4. The figures suggest no effect over the full period after program assignment for fishermen, whereas for hairdressers, the figures show that a significant effect may be contributed to a difference in an underlying trend relative to the comparison group (i.e., retail sale of cosmetics workers may not be a perfect comparison group).

Another important aspect of this model is that we can estimate placebo effects to determine whether the program had any "effects" before it was actually assigned. For each outcome, we find that the before-program assignment coefficients are jointly equal to zero ($p<0.01$) supporting the assumption of a common trend in absence of program assignment. This is supported both for the sickness-related absence outcomes in Figures 6 and 7 as well as the health care utilization outcomes (not shown).

4.4 Alternative outcomes

To further assess the plausibility of our identification strategy, we extract a range of pseudo-outcomes that may be correlated with the unobservables (e.g., health endowments) but should be unaffected by the program. These outcomes include the number of specialist contacts (ophthalmology and otolaryngology), the likelihood of redeeming prescribed medicinal products unrelated to the musculoskeletal system (non ATC M) and the likelihood of hospital contacts unrelated to the musculoskeletal system (non ICD-10 XIII). Finding an "effect" on these outcomes would suggest a difference in an underlying general health trend between target and comparison groups that is not captured by our models and would question the validity of our identification strategy to uncover the causal effects. Table 7 shows that for most samples, there are no significant differences in these outcomes between the target and comparison groups.

4.5 Instrumental variable estimates of actual coverage

Using a difference-in-differences strategy, we have shown a reduced form ITT effect on outcomes. This effect is of direct policy interest and provides suggestive evidence on the potential impact of program assignment to selected trades. However, some workers leave the program by either changing or losing their jobs. From a policy perspective, it may be of interest to determine the extent to which the reduced form ITT effects differs from the effects of actual program coverage, as policy makers cannot control whether workers leave the program after they are initially assigned. This possibility may also explain some of the heterogeneity in effects across trades if the dropout rates differ across trades. For example, the lowest dropout rate is found among dairy workers (3%), whereas construction workers have higher dropout rates (up to 17%).

To estimate the effects of actual program coverage more generally, we can combine the reduced form estimates with first stage estimates of the effects of program assignment on actual coverage during a given year. This approach involves imposing the exclusion restriction that program assignment affects the reduced form outcomes only through actual coverage. Under a monotonicity assumption, Angrist et al. (1996) showed that such an effect can be interpreted as a local average treatment effect (LATE). This effect refers to the effect of program coverage for those who have coverage because they are initially assigned (i.e., the compliers).

When scaling our reduced-form estimates by our first-stage coverage estimates, we use three different first stage coverage estimates: any coverage during a given year, coverage for at least six months during a given year and coverage for the full 12 months during a given year.

Table 8 reports the LATE effects and the previously reported reduced form ITT effects. As expected, the effects increase in magnitude with the number of months that the individuals are covered by the program during a given time period. We observe that there are only slight increases in the effects among dairy workers (up to approximately 17% reduction in proportion of workers with any publicly paid sick-leave compensations). In contrast, among construction workers, the effects become notably larger and closer to the estimates of the dairy workers when program dropout is taken into account (10% and 15% reductions in the proportion of workers with any publicly paid sick-leave compensations for plumbers and electricians, respectively). Hence, it appears that higher dropout rates among construction workers also may contribute to some of the heterogeneity in the effects across the target groups reported in Table 4.

5. DISCUSSION

This study investigates the question of whether workplace health promotion programs can save public money. The study adopts an ITT approach and evaluates the effects of one of the most common types of workplace health promotion programs in Denmark. The estimates for the population indicate that program assignment has reduced the proportion of workers in selected trades who receive publicly paid sick-leave compensations by up to 15% and the number of workers who have used publicly reimbursed physiotherapy by up to 32%; thus, the program has the potential to save public money. Aside from these findings, only mixed evidence is found for a reduction in publicly financed health care service use, at least in the short and medium term.

Our results have several important policy implications. First, our study appears to be the first detailed evaluation of the effects of workplace health promotion programs on publicly compensated outcomes. The findings of this study may inform the debate regarding the role of privately provided health promotion and disease prevention in the organization of the health care sector in countries with tax-financed health care systems. However, it must be emphasized, that this study is not a full budget impact analysis (nor a full cost benefit analysis) of these programs. To capture the full effect on public budgets, the tax revenue loss that stems from a preferential tax treatment of these programs in Denmark must also be included.

Second, we find that there is considerable heterogeneity in the effects across different target groups (i.e., blue-collar trades); thus our results suggest that such programs may not have the same cost-saving benefits in all settings. The lack of an effect in some trades may be explained either by a differential effect of the actual treatment received or by a variation in the participation rates. Although the effects of the actual treatment received may differ across different trades to some extent, we find no reason to believe that this variation is the dominant explanation. Rather, low participation rates are likely to be a limiting factor for such effects. This is supported by our findings of the largest effects among dairy workers, whose trade is dominated by a single large company in which the program was implemented with onsite treatment clinics, whereas little or no were found among trades that are dominated by several small- and medium-sized companies (e.g., hairdressers) that may not have the resources and social networks necessary to effectively put such programs into practice. This result suggests that from a policy perspective it may not be sufficient merely to increase the availability of these programs to the workforce, considerable attention and resources should also be devoted to the actual implementation in the workplace, especially in small- and medium-sized companies. Therefore, a possible area for future research is to investigate what determines participation in these programs and the as-treated effects. Such research may provide insight into the heterogeneity in the effects across target groups that are reported in this study.

Some limitations warrant attention, as these limitations may affect the ability to generalize and use our results. Although evaluating the effects of a collective agreement-based program had the principal advantage that it provided us with a plausible exogenous variation in program assignment, it may be argued that the effects estimated in this study are conservative. First, because program assignment in this study is not a result of an actual employer choice, assigned workers may have insufficient awareness or knowledge of the existence of the program and thus use of the program. Second, we did not possess information on individually (i.e., company) purchased programs (as opposed to collective agreement-based programs). Hence, we were not able exclude workers who were covered by an individually purchased similar program at their company from the comparison groups. This limitation is expected to result in a conservative estimate of the ITT effects. Finally, our study was limited to only short- and medium-term effects. Given that the effects of prevention on health may take time to appear, there may be longer-term effects than those reported in this study. However, the effects on sickness-related absence appeared to stabilize within our period of observation.

As in other studies based on observational data, our study is also limited in that we cannot be certain that our target groups were not exposed to other “interventions” while they were assigned to the program. Depending on the effect of such “interventions” this will either overestimate or underestimate our effects. For example, if the program was part of a larger effort to reduce sickness-related absence in selected trades, then our effects would be overestimated. With the available data, we are not able to investigate this possibility;

thus, a causal interpretation of the estimated effects should be made with this in mind. However, we have performed a review of the collective agreements to assess whether they also included changes in other elements that may affect the sickness-related absence and health care utilization of covered workers. We found that the number of weeks of full wages during absences increased during our period of observation. This is expected to lead to an underestimation of the effects of the program.

Despite these limitations, we find evidence that suggests that the workplace health promotion program under study was able to reduce the number of workers who receive publicly paid sick-leave compensations that may affect public spending and thus produce cost-saving benefits not only to the workers and the companies, but also to society as a whole.

TABLES

Table 1. Assignment-to-treatment

Main industry (NACE)	Major covered trades	Assignment-to-treatment
Transportation	Freight transport by road Urban and suburban passenger land transport Transportation support activities	October 2005
Construction	Plumbing Electrical installation Carpenters Bricklayers Soil and Environmental workers Glaziers	July 2007 January 2008 September 2010 September 2010 September 2010 September 2010
Forestry and fishing	Fishing Landscape gardeners	March 2007 January 2011
Administration and support service activities	Chimney sweeping Cleaning	July 2007 Marts 2012
Manufacturing	Manufacture of dairy products	July 2008
Other service activities	Hairdressing	January 2006

Figure 1. The number of workers assigned to the workplace health promotion program over time

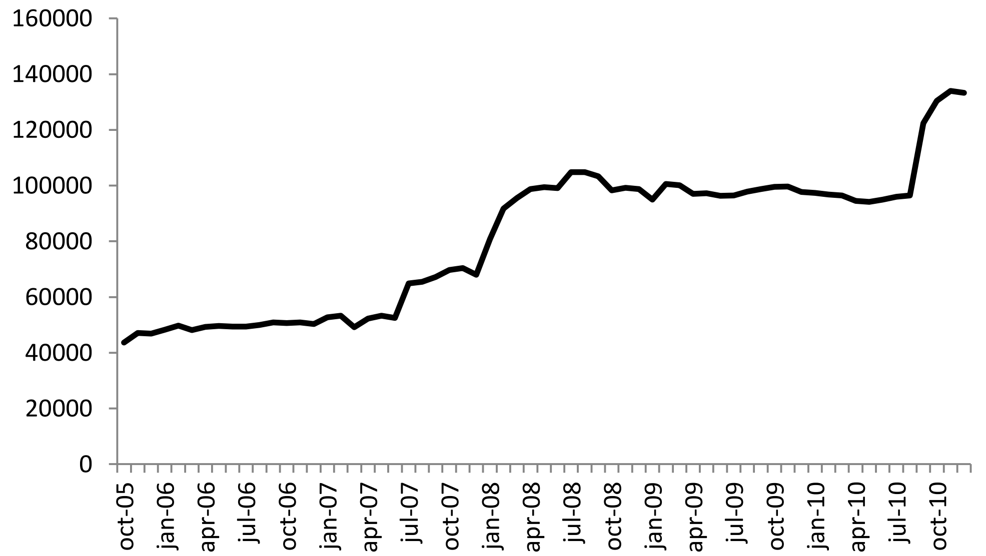


Table 2. Sample compositions

Main industry	Assignment-to-treatment	Target group	Comparison group	Before program assignment time periods (years)	After program assignment time periods (years)
Other service activities	January 2006	2,782 hairdressers	950 retail sale of cosmetics workers	3	5
Forestry and fishing	Marts 2007	334 fishermen	487 forestry and fishery workers	3	3
Construction	July 2007	5,023 plumbers and chimney sweepers	10,988 construction workers e.g. carpenters and bricklayers	3	3
Construction	January 2008	9,897 electricians	10,850 construction workers e.g. carpenters and bricklayers	3	3
Manufacturing	July 2008	3,543 dairy workers	29,970 manufacturing workers	3	2

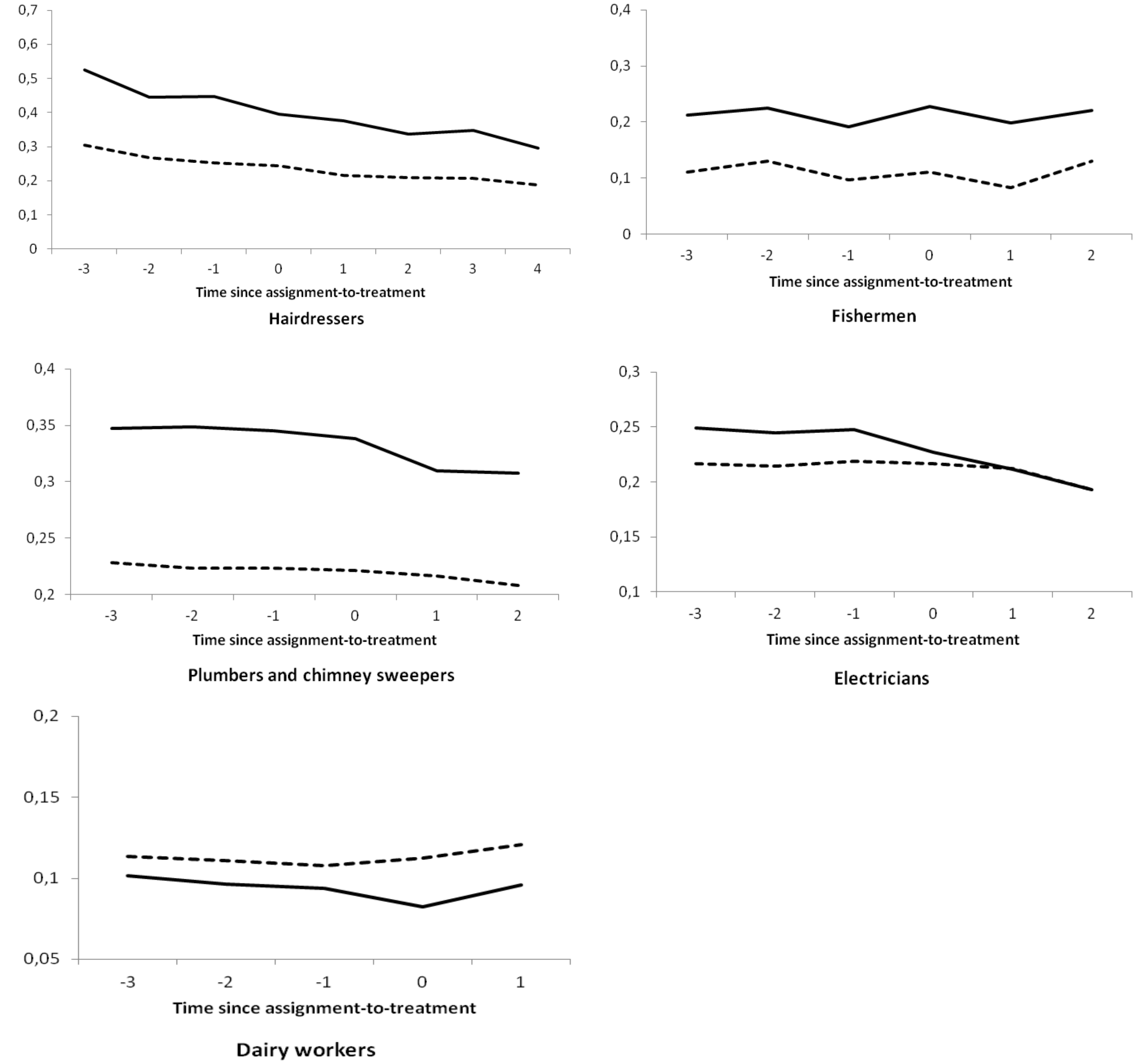
Table 3. Summary statistics measured in the time period before assignment-to-treatment

	Hairdressers	Comparison	Fishermen	Comparison	Plumbers and ch. sweepers	Comparison	Electricians	Comparison	Dairy workers	Comparison
Dependent variables										
Likelihood of publicly paid sick-leave compensations (=1 if publicly paid sick-leave compensations)	0.446	0.253	0.192	0.097	0.345	0.223	0.247	0.222	0.094	0.108
Likelihood of publicly paid sick-leave compensations>3w (=1 if publicly paid sick-leave compensations>3w)	0.069	0.058	0.111	0.039	0.075	0.068	0.057	0.069	0.053	0.046
Likelihood of publicly reimbursed physiotherapy use (=1 if use)	0.103	0.095	0.048	0.072	0.060	0.062	0.064	0.065	0.073	0.078
GP (number of contacts)	7.81	6.99	3.21	4.57	3.84	3.91	3.57	3.97	4.65	5.02
	(6.46)	(7.03)	(3.97)	(5.63)	(4.88)	(4.93)	(4.44)	(5.06)	(5.85)	(6.07)
Likelihood of medication use for the musculoskeletal system (=1 if medication use, ATC M)	0.184	0.195	0.186	0.208	0.205	0.199	0.171	0.196	0.206	0.197
Likelihood of hospital contacts for the musculoskeletal system (=1 if hospital contact, ICD-10 XIII)	0.027	0.034	0.024	0.050	0.049	0.039	0.041	0.047	0.037	0.035
Individual characteristics										
Age (yrs)	32.30	36.12	42.81	44.23	37.96	39.91	35.78	40.08	42.05	42.84
	(9.11)	(10.01)	(9.56)	(9.39)	(9.90)	(10.89)	(10.69)	(10.93)	(9.90)	(9.51)
Danish ethnicity (=1 if Danish ethnicity)	0.959	0.956	0.961	0.969	0.975	0.978	0.973	0.977	0.946	0.923
Male (=1 if male)	0.043	0.122	0.997	0.844	0.988	0.924	0.984	0.920	0.736	0.681
Basic education (1 if primary or lower secondary educational level)	0.088	0.124	0.459	0.349	0.106	0.260	0.129	0.262	0.388	0.300
Marital status (=1 if married or cohabiting)	0.743	0.781	0.722	0.781	0.705	0.759	0.733	0.760	0.751	0.763
Children (=Number of children in household)	0.858	0.906	1.02	1.06	0.970	0.936	0.895	0.929	1.01	0.977
	(0.954)	(1.00)	(1.10)	(1.15)	(1.05)	(1.06)	(1.02)	(1.05)	(1.13)	(1.08)
Capital Region of Denmark	0.252	0.270	0.174	0.117	0.330	0.191	0.265	0.194	0.015	0.162
Region Zealand	0.159	0.163	0.024	0.111	0.212	0.193	0.189	0.191	0.067	0.132
Region of Southern Denmark	0.201	0.201	0.081	0.269	0.196	0.242	0.225	0.244	0.347	0.284
Central Denmark Region	0.265	0.240	0.407	0.306	0.163	0.251	0.207	0.246	0.394	0.292
North Denmark Region	0.124	0.125	0.314	0.197	0.098	0.124	0.114	0.125	0.176	0.130
Job characteristics										
Part-time (=1 if employed part-time)	0.038	0.039	0.072	0.039	0.005	0.019	0.012	0.018	0.012	0.011
Manager (=1 if manager)	0.004	0.002	0	0	0.000	0.000	0.000	0.002	0.001	0.001
Salaried (=1 if salaried worker)	0.001	0.012	0	0.069	0.021	0.060	0.015	0.064	0.005	0.173
Skilled (=1 if skilled worker)	0.959	0.434	0.243	0.319	0.940	0.618	0.941	0.602	0.712	0.640
Unskilled (=1 if unskilled worker)	0.005	0.009	0.021	0.014	0.012	0.126	0.009	0.128	0.235	0.072

Company characteristics										
Number of employees in employed company	9.45	13.31	3.63	381.59	79.48	225.09	268.35	250.11	6,296	1,223
	(15.68)	(20.78)	(3.27)	(498.41)	(234.19)	(628.88)	(562.39)	(648.25)	(2,510)	(2,497)
Number of workplaces in employed company	1.57	1.68	1.01	14.19	1.88	3.61	4.96	3.98	38,08	4.88
	(1.93)	(1.82)	(0.097)	(21.44)	(2.95)	(11.56)	(11.78)	(11.86)	(14,65)	(11.65)
N	2,763	950	334	360	5,023	10,988	9,887	10,850	3,542	29,970

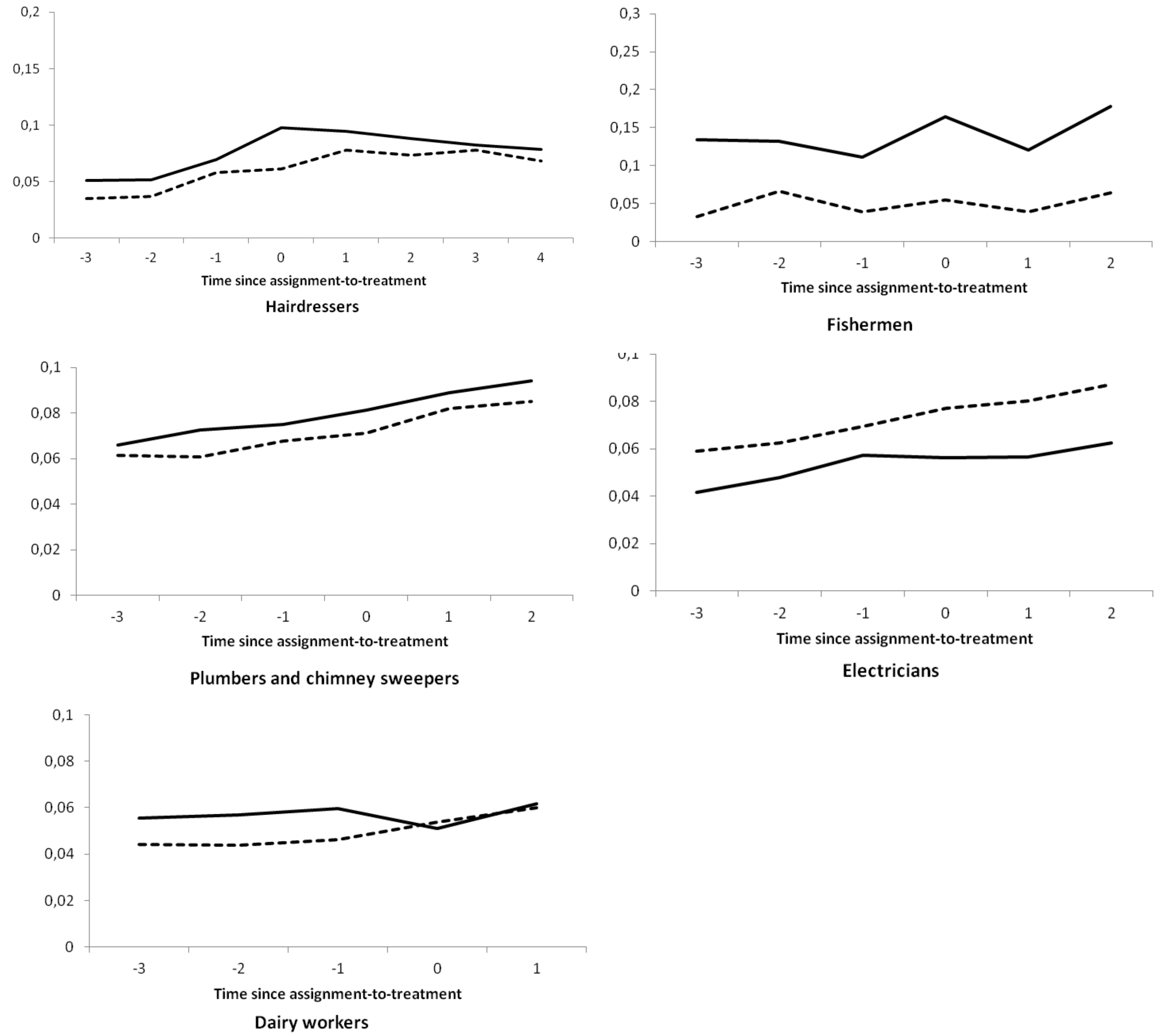
Standard deviations are given in parentheses. All variables are measured in the year before program assignment.

Figure 2: Proportion of workers with any publicly paid sick-leave compensations by intervention group and time



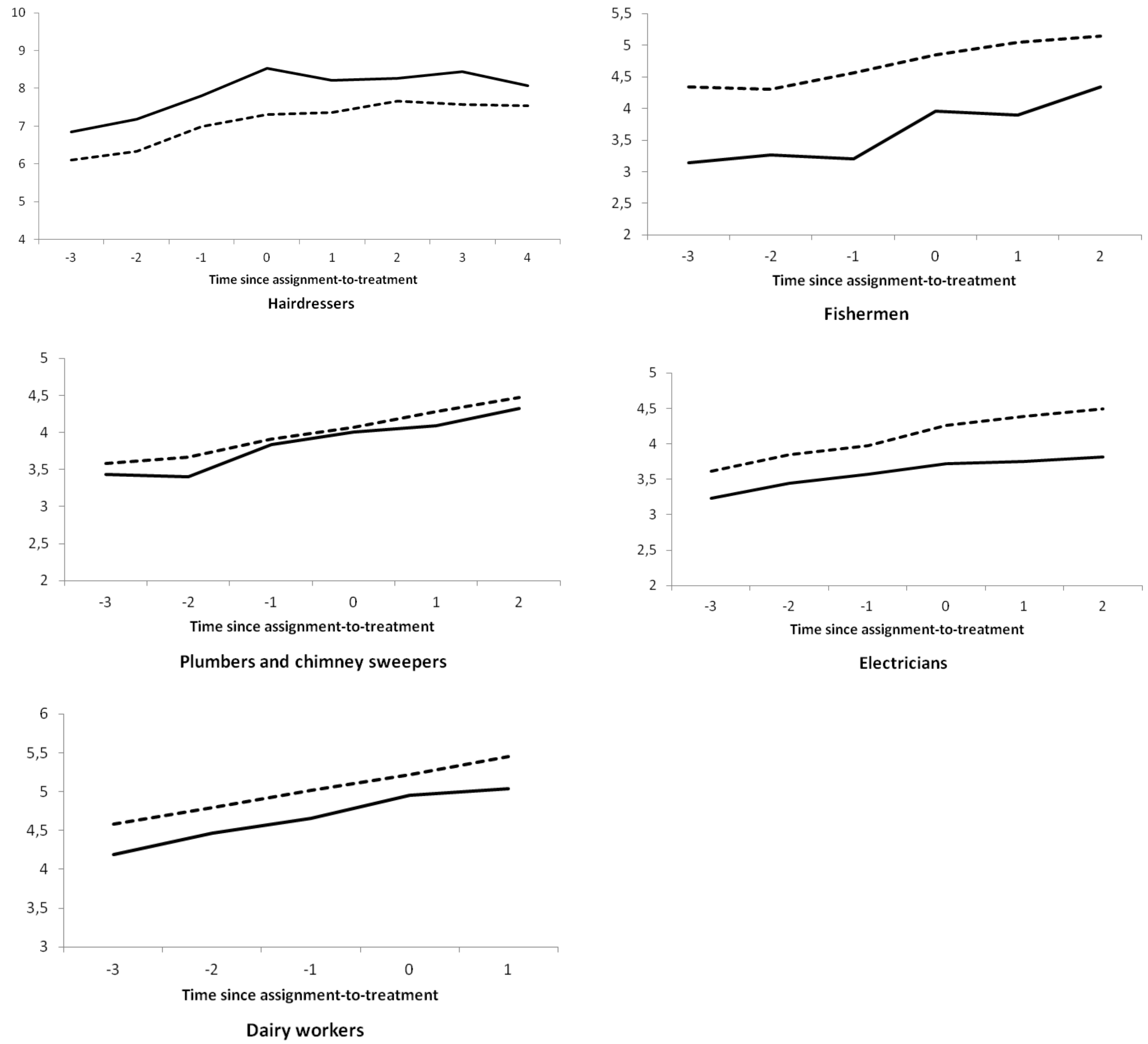
A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Figure 3. Proportion of workers with publicly paid sick-leave compensated>3 weeks by intervention group and time



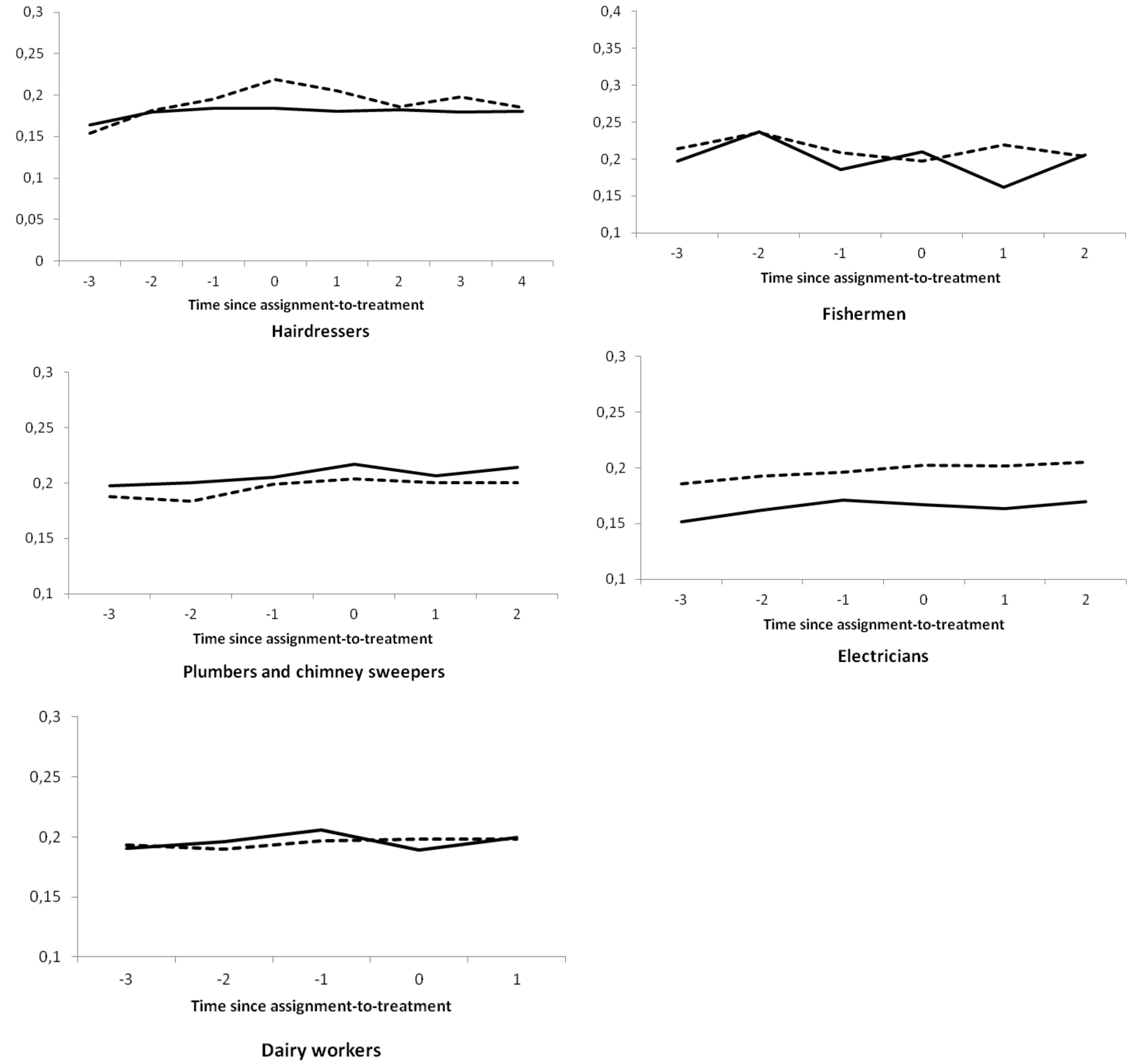
A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Figure 4. Number of general practitioner contacts by intervention group and time



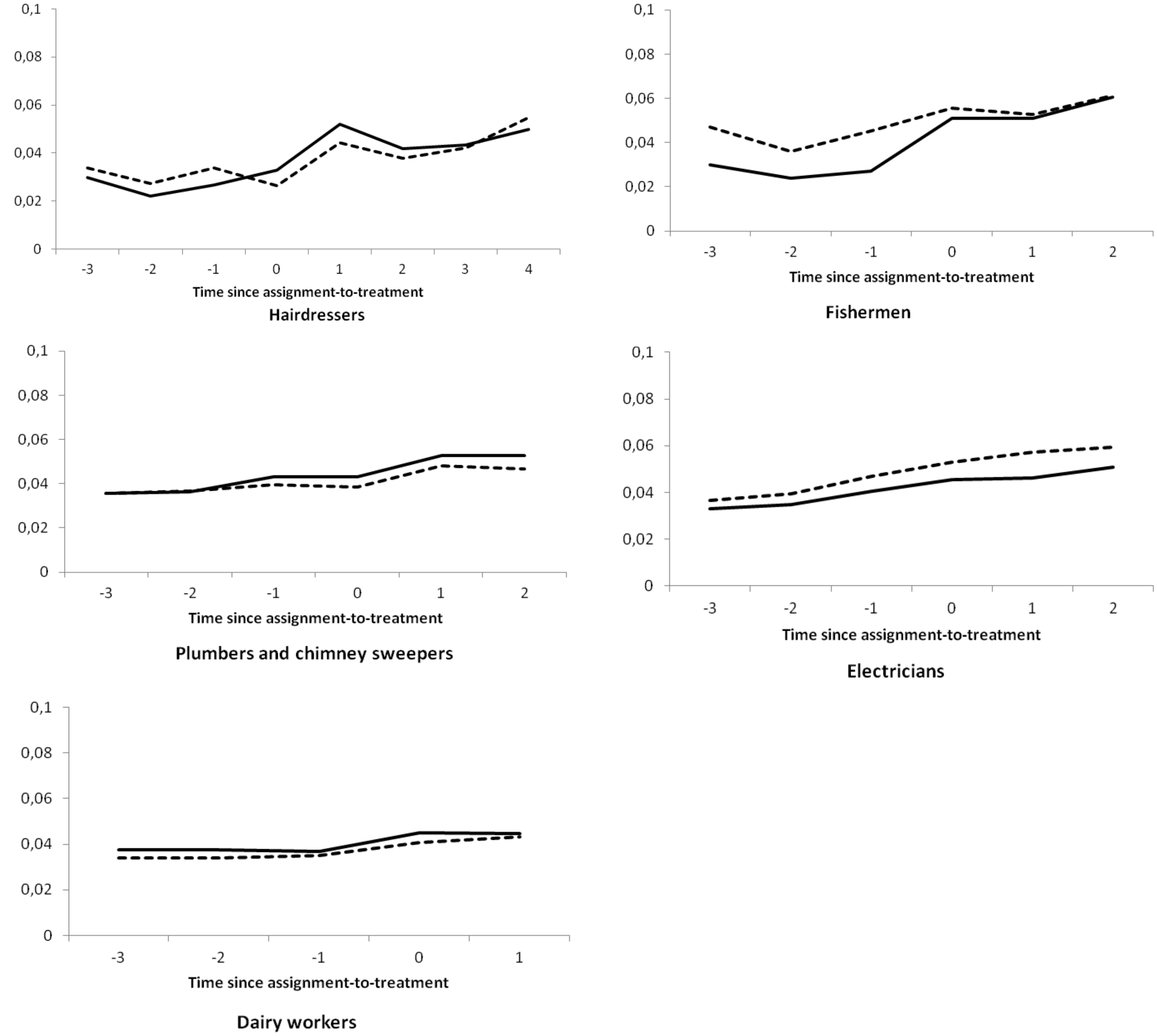
A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Figure 5. Proportion of workers with medication use for the musculoskeletal system by intervention group and time



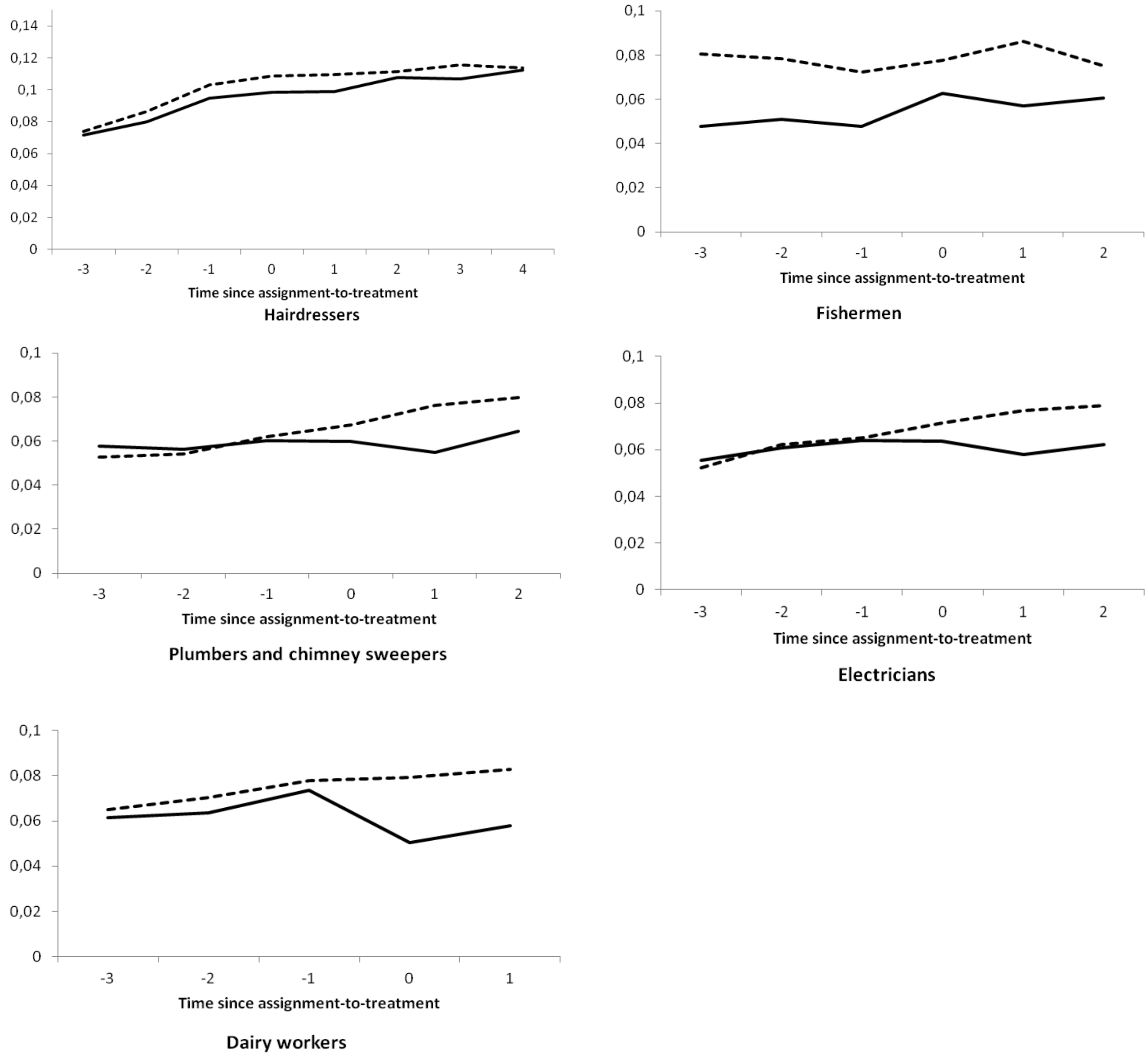
A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Figure 6. Proportion of workers with hospital contacts for the musculoskeletal system by intervention group and time



A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Figure 7. Proportion of workers with publicly reimbursed physiotherapy use by intervention group and time



A solid line indicates the target group and a dashed line represents a comparison group comprising of workers from the same main industry who have not yet been assigned to treatment.

Table 4 Marin results on publicly paid sick-leave compensations: fixed effects models by target group

	Dep.Var: Likelihood of publicly paid sick-leave compensations			Dep. Var: Likelihood of publicly paid sick-leave compensations >3w		
	FE (1)	FE (2)	FE (3)	FE (4)	FE (5)	FE (6)
Hairdressers	-0.060 (0.013)***	-0.048 (0.013)***	-0.021 (0.020)	0.026 (0.012)**	0.009 (0.007)	0.014 (0.013)
N	29,836	27,388	27,388	29,836	27,388	27,388
Fishermen	0.009 (0.021)	0.021 (0.023)	0.039 (0.041)	0.047 (0.033)	0.031 (0.019)	0.049 (0.034)
N	4,159	3,563	3,563	4,159	3,563	3,563
Plumbers and ch. sweepers	-0.020 (0.006)***	-0.024 (0.006)***	-0.014 (0.008)*	-0.001 (0.007)	-0.001 (0.004)	0.000 (0.007)
N	96,020	92,489	92,489	96,020	94,489	94,489
Electricians	-0.027 (0.004)***	-0.028 (0.004)***	-0.016 (0.007)**	-0.009 (0.005)*	-0.010 (0.003)***	-0.009 (0.005)*
N	124,423	120,596	120,596	124,423	120,596	120,596
Dairy workers	-0.014 (0.005)***	-0.014 (0.005)***	-0.015 (0.007)**	-0.012 (0.007)*	-0.011 (0.004)***	-0.013 (0.006)**
N	167,478	163,459	163,459	167,478	163,459	163,459
Full Control Var.	No	Yes	Yes	No	Yes	Yes
Linear Group-Specific Trend	No	No	Yes	No	No	Yes

Robust standard errors adjusted for within individual correlation are given in parentheses. We use all available observations for each variable. All models control for individual- and time specific fixed effects. Full control variables include marital status, the number of children, county of residence, whether an individual has completed primary or lower secondary education, job type, part-time employment and the number of employees and workplaces in the company.
* Denotes statistical significance at the 10 percent level.
**Denotes statistical significance at the 5 percent level.
***Denotes statistical significance at the 1 percent level.

Table 5 Main results on publicly financed health care use: fixed effects models by target group

	Dep.Var: Number of general practitioner contacts			Dep. Var: Likelihood of medication use for the musculoskeletal system			Dep. Var: Likelihood of hospital contact for the musculoskeletal system			Dep. Var: Likelihood of publicly reimbursed physiotherapy use		
	FE (1)	FE (2)	FE (3)	FE (4)	FE (5)	FE (6)	FE (7)	FE (8)	FE (9)	FE (10)	FE (11)	FE (12)
Hairdressers	0.014	0.120	0.364	-0.016	-0.015	-0.028	0.006	0.009	0.007	-0.007	-0.012	-0.011
	(0.155)	(0.160)	(0.254)	(0.010)	(0.011)	(0.029)	(0.005)	(0.006)	(0.009)	(0.008)	(0.009)	(0.015)
N	29,836	27,388	27,388	29,836	27,388	27,388	29,836	27,388	27,388	29,836	27,388	27,388
Fishermen	0.239	0.233	0.366	-0.001	-0.003	0.040	0.016	0.011	0.031	0.001	0.011	-0.025
	(0.304)	(0.319)	(0.440)	(0.024)	(0.027)	(0.047)	(0.013)	(0.014)	(0.027)	(0.017)	(0.018)	(0.025)
N	4,159	3,563	3,563	4,159	3,563	3,563	4,159	3,563	3,563	4,159	3,563	3,563
Plumbers and	0.031	0.037	0.023	0.000	-0.000	0.002	0.002	0.002	-0.006	-0.017	-0.018	-0.005
ch. sweepers	(0.063)	(0.062)	(0.100)	(0.005)	(0.005)	(0.010)	(0.003)	(0.003)	(0.006)	(0.003)***	(0.004)***	(0.006)
N	96,020	92,489	92,489	96,020	92,489	92,489	96,020	92,489	92,489	96,020	92,489	92,489
Electricians	-0.222	-0.200	-0.099	-0.007	-0.007	-0.013	-0.004	-0.004	-0.002	-0.015	-0.015	-0.007
	(0.047)***	(0.047)***	(0.077)	(0.004)*	(0.004)*	(0.008)*	(0.002)*	(0.002)*	(0.004)	(0.003)***	(0.003)***	(0.005)
N	124,423	120,596	120,596	124,423	120,596	120,596	124,423	120,596	120,596	124,423	120,596	120,596
Dairy workers	0.026	0.014	0.056	-0.008	-0.009	-0.026	-0.000	0.000	0.002	-0.021	-0.023	-0.023
	(0.074)	(0.074)	(0.123)	(0.006)	(0.006)	(0.011)**	(0.003)	(0.003)	(0.006)	(0.003)***	(0.004)***	(0.007)***
N	167,478	163,459	163,459	167,478	163,459	163,459	167,478	163,459	163,459	167,478	163,459	163,459
Full Control Var.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Linear Group-Specific Trend	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

Robust standard errors adjusted for within individual correlation are given in parentheses. We use all available observations for each variable. All models control for individual- and time specific fixed effects. Full control variables include marital status, the number of children, the county of residence, whether an individual has completed primary or lower secondary education, job type, part-time employment and the number of employees and workplaces in the company.

* Denotes statistical significance at the 10 percent level.
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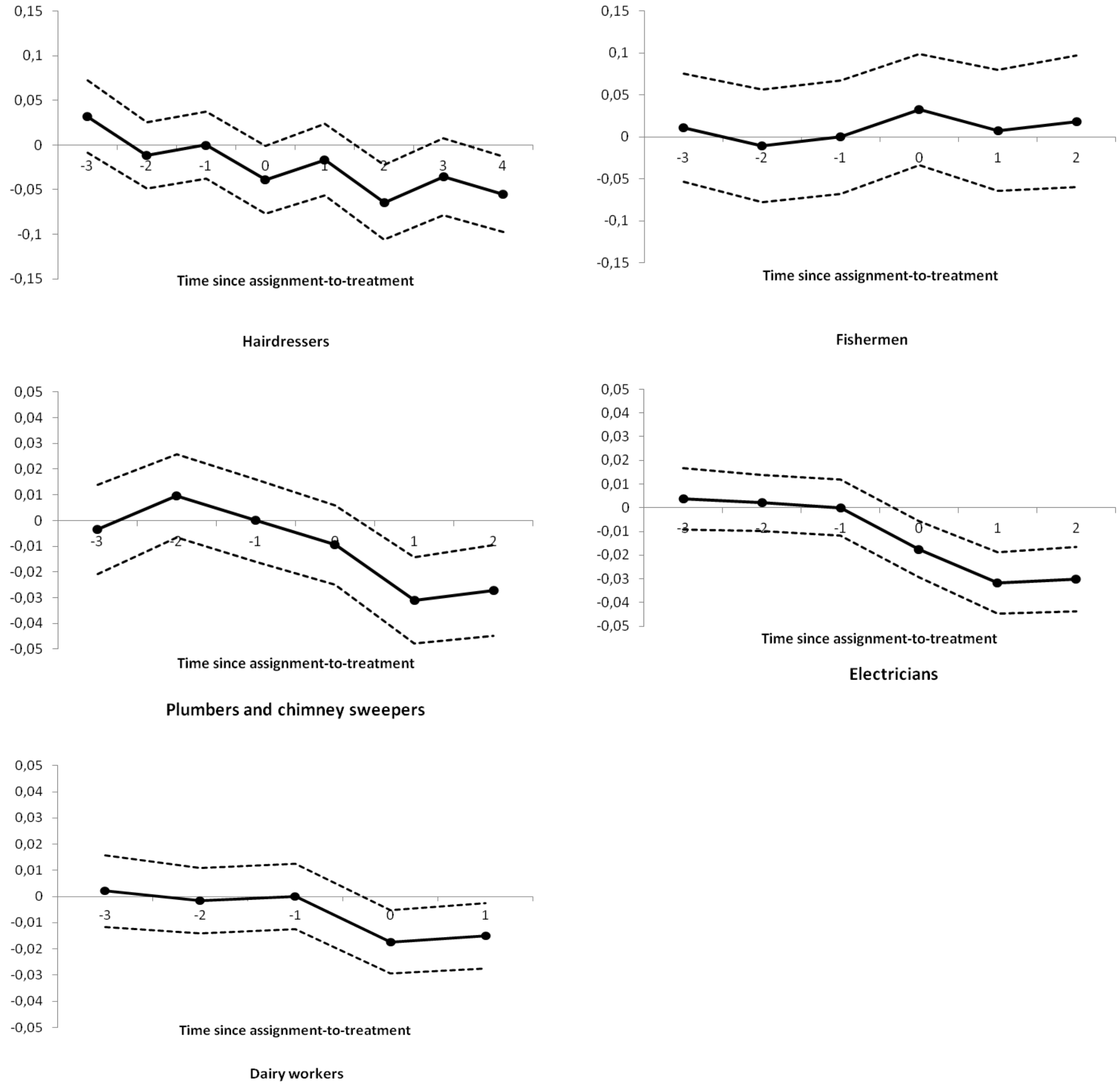
***Denotes statistical significance at the 1 percent level.

Table 6. Heterogeneity in effects among subgroups: Electricians

Dependent variable:	Likelihood of publicly paid sick-leave compensations	Likelihood of publicly paid sick-leave compensations>3w	Number of general practitioner contacts	Likelihood of medication use for the musculoskeletal system	Likelihood of hospital contacts for the musculoskeletal system	Likelihood of publicly reimbursed physiotherapy use	N
All	-0.028 (0.004)***	-0.010 (0.003)***	-0.200 (0.047)***	-0.007 (0.004)*	-0.004 (0.002)*	-0.015 (0.003)***	120,596
Males	-0.026 (0.005)***	-0.010 (0.003)***	-0.199 (0.047)***	-0.008 (0.004)***	-0.003 (0.003)	-0.015 (0.003)***	114,636
Married/cohabiting, τ=0	-0.029 (0.005)***	-0.016 (0.006)**	-0.230 (0.054)***	-0.006 (0.005)	-0.004 (0.003)	-0.014 (0.003)***	90,385
Living alone, τ=0	-0.026 (0.009)***	-0.008 (0.003)***	-0.095 (0.093)	-0.011 (0.008)	-0.004 (0.005)	-0.020 (0.005)***	30,211
Age≤40yrs, τ=0	-0.024 (0.006)***	-0.004 (0.004)	-0.043 (0.058)	-0.003 (0.005)	0.001 (0.003)	-0.012 (0.004)***	66,557
Age>40yrs, τ=0	-0.032 (0.007)***	-0.017 (0.005)***	-0.224 (0.078)***	-0.008 (0.007)	-0.008 (0.004)	-0.020 (0.004)***	54,039
Medication use for the musculoskeletal system, τ=-1	-0.025 (0.012)**	-0.012 (0.008)	-0.291 (0.0135)**	-0.026 (0.011)**	-0.005 (0.008)	-0.029 (0.008)***	22,085
No medication use for the musculoskeletal system, τ=-1	-0.029 (0.005)***	-0.010 (0.003)***	-0.185 (0.048)***	-0.010 (0.004)**	-0.004 (0.002)	-0.013 (0.003)***	98,511
Company<30 employees, τ=0	-0.019 (0.007)***	-0.007 (0.004)	-0.167 (0.064)***	-0.002 (0.006)	-0.001 (0.004)	-0.010 (0.004)**	60,312
Company≥30 employees, τ=0	-0.038 (0.005)***	-0.013 (0.004)***	-0.232 (0.068)***	-0.012 (0.006)**	-0.008 (0.004)**	-0.020 (0.004)***	60,284
North Denmark Region, τ=0	-0.008 (0.013)	-0.010 (0.008)	-0.202 (0.126)	-0.003 (0.012)	-0.013 (0.006)**	-0.019 (0.08)**	14,517
Central Denmark Region, τ=0	-0.045 (0.010)***	-0.009 (0.006)	-0.309 (0.098)***	-0.001 (0.009)	-0.004 (0.005)	-0.020 (0.006)***	27,443
Region of Southern Denmark, τ=0	-0.016 (0.009)*	-0.001 (0.006)	-0.200 (0.098)**	-0.15 (0.008)*	-0.003 (0.006)	-0.017 (0.005)***	28,213
Region Zealand, τ=0	-0.028 (0.010)***	-0.016 (0.007)**	-0.196 (0.109)*	-0.005 (0.009)	0.002 (0.005)	-0.004 (0.007)	23,043
Capital Region of Denmark, τ=0	-0.033 (0.009)***	-0.019 (0.006)***	-0.095 (0.097)	-0.017 (0.008)**	-0.007 (0.005)	-0.017 (0.006)***	27,380

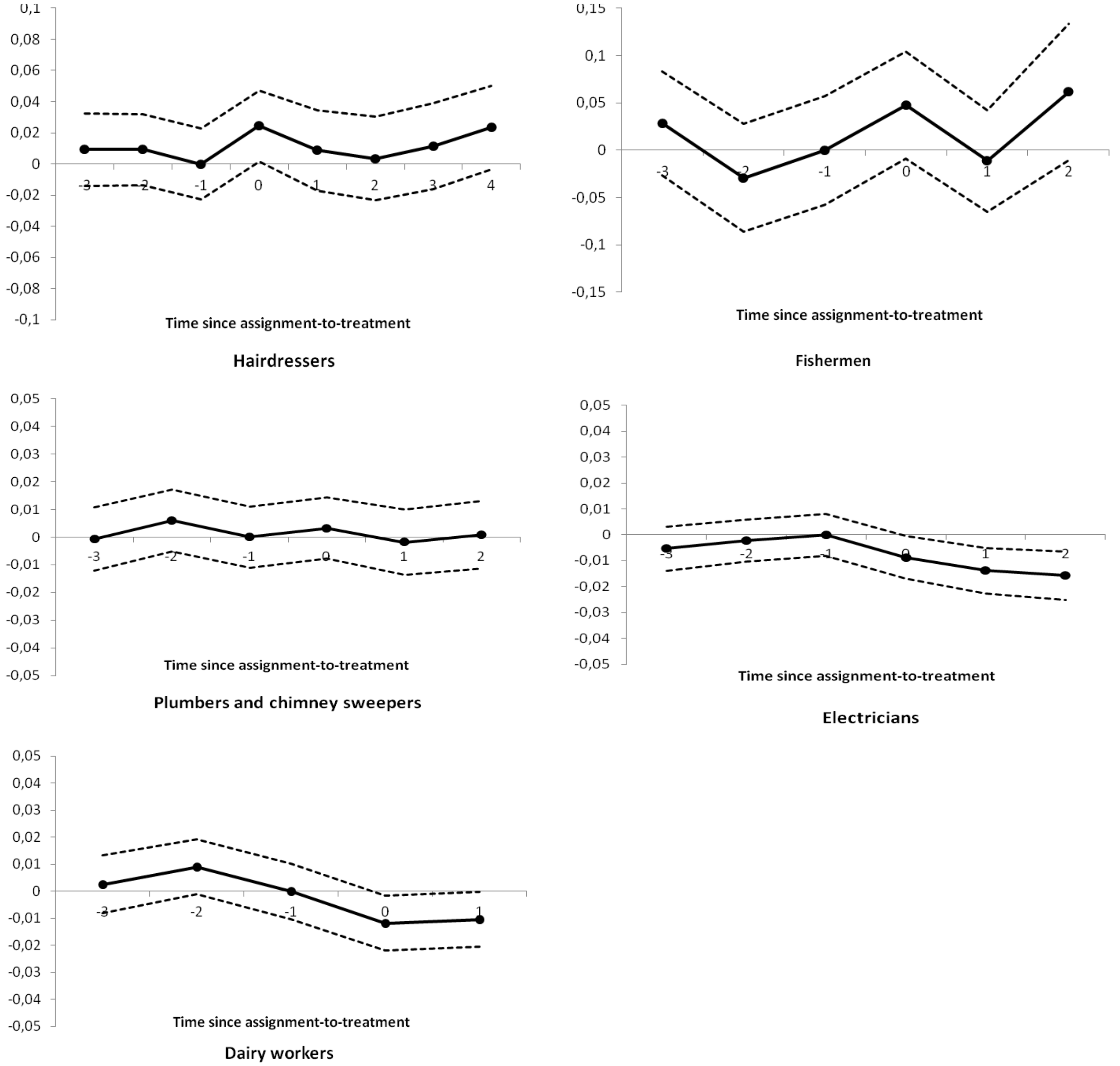
Robust standard errors adjusted for within individual correlation are given in parentheses. We use all available observations for each variable. All models control for individual- and time specific fixed effects, marital status, the number of children, the county of residence, whether an individual has completed primary or lower secondary education, job type, part-time employment and the number of employees and workplaces in the company.
* Denotes statistical significance at the 10 percent level.
**Denotes statistical significance at the 5 percent level.
***Denotes statistical significance at the 1 percent level.

Figure 8. Intention-to-treat effects by intervention group and time since assignment-to-treatment, dependent variable: likelihood of publicly paid sick-leave compensations



A solid line indicates the intention-to-treat effect and the dashed lines represent a 95% confidence interval around the ITT effect using estimated robust standard errors adjusted for within-individual correlation. All models control for marital status, the number of children, the county of residence whether an individual has a completed primary or lower secondary education, job type, part-time employment, the number of employees and workplaces in the company and individual- and time-fixed effects.

Figure 9. Intention-to-treat effects by intervention group and time since assignment to treatment, dependent variable :likelihood of publicly paid sick-leave compensations>3weeks



A solid line indicates the intention-to-treat effect and the dashed lines represent a 95% confidence interval around the ITT effect using estimated robust standard errors adjusted for within-individual correlation. All models control for marital status, the number of children, the county of residence whether an individual has a completed primary or lower secondary education, job type, part-time employment, the number of employees and workplaces in the company and individual- and time-fixed effects.

Table 7. Alternative outcomes

Dependent variable:	Number of specialist contacts (#)	Likelihood of medicine use unrelated to musculoskeletal disorders (non-ATC M)	Likelihood of hospital contacts unrelated to musculoskeletal disorders (non ICD-10 XIII)
Hairdressers (N=27,388)	0.005	-0.004	0.022
	(0.018)	(0.012)	(0.014)
Fishermen (N=3,563)	0.016	0.016	0.007
	(0.032)	(0.030)	(0.006)
Plumbers and chimney	-0.014	-0.000	0.002
sweepers (N=92,489)	(0.009)	(0.005)	(0.003)
Electricians (N=120,596)	-0.007	-0.008	-0.007
	(0.007)	(0.005)	(0.005)
Dairy workers (N=163,549)	-0.002	0.001	0.018
	(0.011)	(0.007)	(0.007)***

Robust standard errors adjusted for within individual correlation are given in parentheses. We use all available observations for each variable. All models control for individual- and time specific fixed effects, marital status, the number of children, the county of residence, whether an individual has completed primary or lower secondary education, job type, part-time employment and the number of employees and workplaces in the company.

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**Denotes statistical significance at the 5 percent level.
***Denotes statistical significance at the 1 percent level.

Table 8. Instrumental variable estimates of actual coverage

Dependent variable	Reduced form ITT effects	IV estimates Access >0 months per year	IV estimates Access ≥6 month per year	IV estimates Access 12 month per year
	A. Hairdressers (N=27,388)			
Likelihood of publicly paid sick-leave compensations	-0.048	-0.058	-0.063	-0.083
	(0.013)***	(0.016)***	(0.018)***	(0.023)***
Likelihood of publicly paid sick-leave compensations>3w	0.009	0.011	0.012	0.015
	(0.007)	(0.009)	(0.009)	(0.013)
Number of general practitioner contacts	0.120	0.146	0.159	0.210
	(0.160)	(0.194)	(0.210)	(0.278)
Likelihood of medication use for the musculoskeletal system	-0.015	-0.019	-0.020	-0.027
	(0.011)	(0.013)	(0.014)	(0.019)
Likelihood of hospital contact for the musculoskeletal system	0.009	0.011	0.012	0.016
	(0.006)	(0.075)	(0.082)	(0.010)
Likelihood of publicly reimbursed physiotherapy use		-0.015	-0.016	-0.021
		(0.011)	(0.012)	(0.015)
First stage t-stat	-	184	122	84
	B. Fishermen (N= 3,563)			
Likelihood of publicly paid sick-leave compensations	0.021	0.022	0.025	0.057
	(0.023)	(0.025)	(0.027)	(0.064)
Likelihood of publicly paid sick-leave compensations>3w	0.031	0.035	0.038	0.091
	(0.019)	(0.020)*	(0.022)*	(0.053)*
Number of general practitioner contacts	0.233	0.250	0.274	0.642
	(0.319)	(0.341)	(0.374)	(0.878)
Likelihood of medication use for the musculoskeletal system	-0.034	-0.004	-0.004	-0.009
	(0.027)	(0.029)	(0.031)	(0.074)
Likelihood of hospital contact for the musculoskeletal system	0.011	0.013	0.014	0.032
	(0.014)	(0.015)	(0.016)	(0.037)
Likelihood of publicly reimbursed physiotherapy use	0.011	0.012	0.013	0.030
	(0.018)	(0.019)	(0.021)	(0.049)
First stage t-stat	-	78	51	17
	C. Plumbers and chimney sweepers (N=92,489)			
Likelihood of publicly paid sick-leave compensations	-0.024	-0.026	-0.027	-0.034
	(0.006)***	(0.006)***	(0.006)***	(0.008)***
Likelihood of publicly paid sick-leave compensations>3w	-0.001	-0.001	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.005)
Number of general practitioner contacts	0.037	0.040	0.042	0.053
	(0.062)	(0.066)	(0.070)	(0.088)
Likelihood of medication use for the musculoskeletal system	-0.000	-0.001	-0.000	-0.001
	(0.005)	(0.006)	(0.006)	(0.007)
Likelihood of hospital contact for the musculoskeletal system	0.002	0.002	0.002	0.003
	(0.003)	(0.003)	(0.003)	(0.004)
Likelihood of publicly reimbursed physiotherapy use	-0.018	-0.019	-0.020	-0.025
	(0.004)***	(0.004)***	(0.004)***	(0.005)***
First stage t-stat	-	412	256	143
	D. Electricians (N=120,596)			
Likelihood of publicly paid sick-leave compensations	-0.028	-0.034	-0.032	-0.038
	(0.004)***	(0.005)***	(0.005)***	(0.006)***
Likelihood of publicly paid sick-leave compensations>3w	-0.010	-0.012	-0.012	-0.014
	(0.003)***	(0.003)***	(0.003)***	(0.004)***
Number of general practitioner contacts	-0.200	-0.238	-0.228	-0.272
	(0.047)***	(0.056)***	(0.054)***	(0.064)***
Likelihood of medication use for the musculoskeletal system	-0.007	-0.009	-0.008	-0.010
	(0.004)*	(0.005)*	(0.005)*	(0.006)*
Likelihood of hospital contact for the musculoskeletal system	-0.004	-0.005	-0.005	-0.006
	(0.002)*	(0.003)*	(0.003)*	(0.003)*
Likelihood of publicly reimbursed physiotherapy use	-0.015	-0.018	-0.017	-0.021
	(0.003)***	(0.003)***	(0.003)***	(0.004)***
First stage t-stat	-	373	349	211

	E. Dairy workers (N=163,459)			
Likelihood of publicly paid sick-leave compensations	-0.014 (0.005)***	-0.015 (0.005)***	-0.015 (0.005)***	-0.016 (0.005)***
Likelihood of publicly paid sick-leave compensations>3w	-0.011 (0.004)***	-0.011 (0.004)***	-0.011 (0.004)***	-0.012 (0.004)***
Number of general practitioner contacts	0.014 (0.074)	0.015 (0.075)	0.015 (0.076)	0.016 (0.082)
Likelihood of medication use for the musculoskeletal system	-0.009 (0.006)	-0.009 (0.006)	-0.009 (0.006)	-0.010 (0.006)
Likelihood of hospital contact for the musculoskeletal system	0.000 (0.003)	0.000 (0.003)	0.000 (0.003)	0.000 (0.004)
Likelihood of publicly reimbursed physiotherapy use	-0.023 (0.004)***			
First stage t-stat	-	768	480	214

Robust standard errors adjusted for within individual correlation are given in parentheses. We use all available observations for each variable. All models control for individual- and time specific fixed effects, marital status, the number of children, the county of residence, whether an individual has completed primary or lower secondary education, job type, part-time employment and the number of employees and workplaces in the company.

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