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Job Retention during the Covid-19 Pandemic

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Abstract
Following the outbreak of the covid-19 pandemic, countries worldwide adopted new or scaled up existing job retention schemes. This paper examines the labor market effects of the Danish wage compensation scheme for employees, which offered wage subsidies to private firms that furloughed workers instead of laying them off. Using payroll records at the monthly frequency for Danish employer-employee matches, I find that the scheme prevented at least 10,400 exits from employment. However, it also compensated workers that firms were not planning to lay off. It mainly prevented job losses for low-tenured workers (≤ 1 year), while high-tenured workers (8+ years) would, to a lesser extent, have been laid off even in the absence of the scheme. Further, the scheme led to a significant decline in furloughed workers' labor income, which may reflect that firms negotiated wage cuts with their employees before applying for wage compensation. Finally, I find that labor market mobility was basically unaffected by the scheme.

Resumé
Da covid-19 pandemien brød ud, valgte mange lande at introducere nye eller opskalere eksisterende jobfastholdelsesordninger. Dette papir analyserer arbejdsmarkedseffekterne af den danske lønkompensationsordning for lønmodtagere, som tilbød løntilskud til private virksomheder, der hjemsendte deres medarbejdere med løn i stedet for at afskedige dem. Ved brug af registerdata med månedlige lønoplysninger for danske arbejdsgiver-arbejdstager matches finder jeg, at ordningen forhindrede mindst 10.400 afgange fra beskæftigelse. Ordningen kompenserede imidlertid også lønmodtagere, som virksomhederne ikke planlagde at afskedige. Den reddede primært jobs for nyansatte (≤ 1 år), mens lønmodtagere med mange års erfaring i virksomheden (8+ år) kun i mindre omfang ville være blevet afskediget, hvis ordningen ikke var blevet indført. Ordningen førte endvidere til et fald i de hjemsendte lønmodtageres arbejdsindkomst, hvilket kan afspejle, at virksomhederne forhandlede lønnedgange med deres medarbejdere, før de ansøgte om lønkompensation. Endelig finder jeg, at mobiliteten på arbejdsmarkedet stort set var upåvirket af ordningen.

Key words
Economic activity and employment; public finances and fiscal policy.

JEL classification
H12; J24; J38; J63

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1 Introduction

A significant amount of jobs were at risk when governments worldwide enforced lockdowns of entire industries in an attempt to contain the covid-19 virus.[1] To limit job losses, many countries adopted new or scaled-up existing job retention schemes. The goal was to preserve job matches by subsidizing wage costs for firms that furloughed workers rather than laying them off. The concept of these policies is not new. Similar schemes prevented job losses during the Great Recession.[2] Yet, our knowledge about the role of job retention schemes for labor markets remains limited.

This paper analyzes the labor market effects of a temporary wage compensation scheme introduced in Denmark in March 2020. The scheme offered wage subsidies of up to 90 percent of salaries to firms that faced layoffs of at least 30 percent of their workforce, or more than 50 employees, due to the pandemic but chose to furlough workers with pay instead. For an evaluation of this scheme, there are at least four questions to be asked: *i)* How many job losses did the scheme prevent? *ii)* Did it prevent loss of firm-specific human capital? *iii)* How did it affect furloughed workers' labor income? *iv)* How did it affect labor market mobility? Using register data with monthly information on wage compensation and labor market outcomes for Danish employer-employee matches and a matched difference-in-differences estimator, the paper presents four key findings.

First, the preferred estimate indicates that the Danish wage compensation scheme prevented at least 10,400 exits from employment from March to August 2020 (8,500 exits at the lower bound of the 95% confidence interval). However, it also compensated workers that firms were not planning to lay off. This is in line with results for the US Paycheck Protection Program, which also intended to limit job losses during the pandemic but had little effect on employment since aid-taking firms would not have laid off many workers even in its absence (Chetty et al., 2020).

Second, this paper examines the role of job retention schemes for retaining firm-specific

1. The pandemic induced significant declines in hirings (Bess, Borgensgaard, and Iuel, 2020; Kahn, Lange, and Wiczer, 2020; Hensvik, Le Barbanchon, and Rathelot, 2021) and large-scale job losses (Coibion, Gorodnichenko, and Weber, 2020; Cajner et al., 2020; Bartik et al., 2020) that were partly explained by health policies such as lockdowns (Gupta et al., 2020; Kong and Prinz, 2020; Juranek et al., 2020).

2. Empirical studies find positive effects on employment of short-time work schemes during the Great Recession (Felter, 2012; Hijzen and Martin, 2013), but they also targeted jobs that would have been preserved anyway (Boeri and Bruecker, 2011).

human capital measured by workers' *ex ante* firm tenure. I find that the Danish wage compensation scheme mainly prevented job losses for low-tenured workers ($\leq$ 1 year), while furloughed workers with many years of experience in the firm (8+ years) would, to a lesser extent, have been laid off even in the absence of the scheme. This may suggest that firm-specific human capital would, to some extent, have been retained anyway. This is arguably an interesting finding, as retaining firm-specific human capital is often part of the motivation for adopting job retention schemes during recessions. It is believed to shorten the recovery, as re-developing productive worker-firm matches is time-consuming and costly (Oi, 1962; Barron, Bishop, and Dunkelberg, 1985; Blatter, Muehlemann, and Schenker, 2012). More generally, the result implies that the benefits of compensating jobs for high-tenured workers are limited. On the other hand, the scheme may have supported future potential output growth by saving jobs for low-tenured workers. A recent paper by Caggese, Cuñat, and Metzger (2019) shows that in the event of an economic contraction like the covid-19 shock, financially constrained firms are induced to fire low-tenured workers with high productivity growth prospects rather than less promising high-tenured workers because of their lower firing costs.

Third, the results point to a fall in furloughed workers' labor income in response to the scheme. In the compensation period from March to June 2020, the scheme is estimated to have reduced the average monthly labor income of furloughed workers by 2.1 percent, corresponding to a total loss in earnings of almost DKK 2,400 (USD 370) per worker over this period.[3] This may suggest that firms and their employees use the option to negotiate wage cuts before applying for wage compensation.

Fourth, and finally, this paper studies how job retention schemes affect labor market mobility. Since furloughed workers only have limited incentives to apply for new jobs when they are sent home with pay, the scheme may reduce the high degree of labor mobility which is otherwise seen as one of the strengths of the Danish labor market model (Andersen, Svarer, and Schröder, 2020). Labor mobility enables the allocation of workers from less to more productive job matches. I find that furloughed workers' probability of having a job-to-job transition is basically unaffected by the wage compensation scheme. This may reflect that job search costs were very high for both furloughed and non-compensated workers during the lockdown of the Danish economy, as vacancies declined drastically.

3. While the average monthly labor income of furloughed workers is DKK 28,258 in this period, the results indicate that it would have been approximately DKK 28,850 in the absence of the scheme.

A related study by Bennedsen et al. (2020) uses newly collected survey data on 10,642 firms to study the role of Danish relief packages for preventing job losses during the pandemic. Comparing firms' layoff decisions to their reported counterfactuals, they find that the Danish relief packages, including cost aid, fiscal aid, and the wage compensation scheme, saved 81,000 jobs. Their study is, however, not directly comparable to this paper, as it estimates the job savings of several Danish relief packages and not only the wage compensation scheme. It is also an earlier study, measuring the effect on firms' intended layoffs at the beginning of the pandemic rather than actual layoffs. It also employs a different research design. When survey data is used, a concern is that firms misreport counterfactual layoffs, leading to an upward bias in the estimated job savings. Aid-taking firms that could otherwise afford to retain furloughed workers themselves have an incentive to overstate counterfactual layoffs to be eligible for wage compensation, which they can credibly do since the eligibility rule is unobserved.[4]

This paper estimates the effects of the wage compensation scheme using a matched difference-in-differences estimator, comparing the growth rate of several labor market outcomes from before to after the introduction of the scheme for two groups of workers: those that were furloughed by wage compensated firms from March to June 2020 and their non-compensated matches. The two groups are matched on the propensity score to have similar pre-pandemic characteristics and a similar *ex ante* exposure to the covid-19 shock, exploiting the extraordinary degree of detail in the Danish registers. Exposure is measured by workers' *ex ante* industry and occupation, while the data does not allow for *ex ante* firm-level measures. Firms operating within the same industries may not be equally affected by the covid-19 shock, suggesting that the preferred estimate for the number of averted exits from employment may be understated. Importantly, however, this issue is not expected to be pronounced, as the results do not change if firms are additionally matched on the change in sales of their workplaces from March 2019 to March 2020.

The empirical strategy provides a partial equilibrium analysis of the labor market effects of the scheme, ignoring general equilibrium effects that may have influenced consumer demand, and thereby employment at all firms. In the absence of the wage compensation scheme, layoffs of furloughed workers would probably have led to a contraction in consumer demand, an additional worsening of economic conditions, and another round

4. The eligibility rule is based on the number of layoffs a firm would perform in the absence of the scheme.

of layoffs. This provides a further argument for interpreting the estimated number of averted exits from employment as a lower bound for the true effect of the scheme.

The paper is organized as follows. Section 2 presents the wage compensation scheme and a timeline of Danish covid-19 policies. Section 3 describes the data, and section 4 explains the empirical strategy. Section 5 presents the results, and section 6 concludes.

2 The Danish Wage Compensation Scheme

The wage compensation scheme for employees was introduced in March 2020, following the lockdown of the Danish economy. The scheme became effective on March 9, 2020, and was originally set to be phased out on June 8, 2020, but was later extended to August 29, 2020. By this time, approved applications for wage compensation amounted to almost DKK 12.2 billion, supporting more than 273,000 jobs, cf. table 5 in appendix A.

The scheme offered temporary wage subsidies to private firms facing layoffs of at least 30 percent of their workforce, or more than 50 employees, due to the covid-19 pandemic. For white- and blue-collar employees the compensation amounted to 75 and 90 percent of salaries, respectively, but with a monthly cap of DKK 30,000 per full-time employee.[5] Firms covered the remaining part of wages but were allowed to negotiate salary reductions with their employees before they applied for compensation. If a salary reduction was not agreed upon, employees were entitled to full pay during the entire compensation period but were required to take some holidays.[6]

During the support period, firms were not allowed to dismiss employees for reasons related to the pandemic and had to send compensated employees home on furlough. Furloughed employees, except students, were not allowed to work for the compensated firm. However, they might take on paid work in other firms during the furlough period if their contract did not preclude this.

Box 1 in appendix B provides an overview of the timing of Danish covid-19 policies surrounding the adoption and expiration of the scheme.

5. For firms that furloughed up to 25 employees, compensation was, in principle, computed based on the highest average labor income of furloughed workers in the previous three or 12 months, including add-ons to salaries. However, the salary could deviate from this rule if firms documented that it did not reflect the agreed salary. For firms that furloughed more than 25 employees, the agreed salary for all furloughed workers should be stated in the application, including pension contributions, foreseen shift differentials, etc.

6. From March 9 to July 9, 2020, compensated employees had to take 1.67 holidays per furloughed month. From July 9 to August 29, 2020, they had to take up to 15 holidays unless none were accrued.

3 Data

The empirical analysis is based on Danish administrative register data from Statistics Denmark. To identify wage compensated jobs and furlough periods, I exploit a new register with information on all approved applications for wage compensation.[7] Labor market outcomes are obtained from the register of wage earner employment, which contains monthly information on wages, hours of work, occupation, and industry for Danish employer-employee matches. This information is reported by firms to the Danish Tax Agency and tax evasion on wage income is very small (Kleven et al., 2011). The register uniquely identifies workers and workplaces over time by personal registration numbers and firm identifiers, such that worker flows in and out of every workplace may be tracked. It further enables me to link the data to various background characteristics of workers and firms from other administrative registers. For an overview of the covariate set, see table 6 in appendix C.

The baseline sample covers the period January 2018 to August 2020. It is restricted to workers employed in private sector firms since public sector firms are not eligible for wage compensation. The focus is on the cohort of workers who are 22-65 years old, have monthly labor incomes of DKK 10,000-70,000, and who are employed in February 2020. For individuals with more than one job, I consider only their main occupation, defined as the job with the highest number of reported working hours.

The empirical design of this paper relies on comparing workers who are furloughed by wage compensated firms from March to June 2020 (the treatment group) to similar workers employed by non-compensated firms in February 2020 (the control group). The two groups are matched on the propensity score to have similar pre-pandemic characteristics and a similar *ex ante* exposure to the covid-19 shock. Among the 21 possible compensation periods, the focus is only on the largest in terms of furloughed workers (March to June 2020) to ensure that different timings of treatment do not bias the results.[8] For the remainder of the paper, workers in the treatment and control groups are simply referred

7. A limitation to the data used is that it only contains information on *approved* and not *actual* compensation periods. This means that it is not observed in the data if a firm leaves the scheme before the approved compensation period ends either to lay off workers due to financial distress or to call back workers due to recovered demand.

8. Table 5 in appendix A reports the number of furloughed workers in each of the 21 compensation periods.

to as *furloughed* and *non-compensated* workers, respectively.

The treatment group does not include non-furloughed workers who are employed by wage compensated firms, even though their jobs are protected by the scheme as well, as these workers cannot credibly be identified.[9] The main issue in this context is that firms' layoff and furlough decisions may not be perfectly related. Firms may decide to furlough highly valued workers that are temporarily unproductive due to the lockdown rather than less valued workers that would otherwise have been laid off. This indicates that the estimated job savings of the scheme may be a lower bound for the true effect. To ensure that those covered by the scheme do not incorrectly end up in the control group, I discard observations for non-furloughed workers who are employed by wage compensated firms in February 2020.

This leaves an unmatched sample of 743,509 workers, of which 51,632 were furloughed from March to June 2020. After matching, there are 90,231 workers in the sample. Among these, 51,632 are furloughed (treated), and 38,599 are non-compensated (controls). Tables 7-9 in appendix D present descriptive statistics for the full sample and the subsamples of furloughed and non-compensated workers both before and after matching.

Before matching, furloughed workers are on average four years younger than non-compensated workers, have one year less tenure, and earned DKK 5,434 less labor income each month of employment in 2019. They also appear to be less educated than non-compensated workers. There is an overweight of women, foreign citizens, and part-time employees among furloughed workers compared to non-compensated workers. Further, there are substantial differences in the distributions of workers across industries and occupations for the two groups. For industries, one example is accommodation and food service, which employs 17 percent of furloughed workers but less than one percent of non-compensated workers. For occupations, it appears that while 15 percent of furloughed employees work with personal services, including waiters and hairdressers, only 2 percent of non-compensated employees do. Hence, before matching, the groups of furloughed and non-compensated workers differ in terms of potentially important confounding factors.

After matching, the two groups have very similar covariate distributions. The average

9. The wage compensation register does not contain information on these workers, and the group of non-furloughed workers that receive salaries from wage compensated firms *prior to* the scheme also includes workers who were laid off just before the firm applied for compensation. Observing only separations from employment which are affected by resignation periods and not the time of layoff, I cannot distinguish between these two groups of workers.

worker is just above 39 years old, has almost 4 years of tenure, and had an average monthly labor income of DKK 29,622 when employed in 2019. Moreover, 55 percent are full-time employed, 48 percent are men, and 10 percent have foreign citizenship. Most workers do not have children or only children older than 15 years, and the vast majority have a vocational education. The two most represented industries are wholesale and retail trade, where 36 percent of workers are employed, and accommodation and food service, where 18 percent of workers are employed. The two most represented occupations are personal services and sales, with worker shares of 14 and 17 percent, respectively.

4 Empirical Strategy

This section presents the empirical framework used to estimate the labor market effects of the Danish wage compensation scheme. Workers have been allocated to treatment and control groups based on who gets wage compensation rather than who is eligible for it, because the eligibility rule is unobserved. It is based on the number of layoffs that a firm would perform in the absence of the scheme. This gives rise to concerns about selection into the scheme. A key goal of this paper is to identify a suitable control group for furloughed workers among workers who are employed by non-compensated firms. For this purpose, a matched difference-in-differences approach is proposed.[10]

The idea is to compare the growth rate of labor market outcomes from before to after the introduction of the scheme for furloughed and non-compensated workers that are matched on the propensity score to have similar pre-pandemic characteristics and a similar *ex ante* exposure to the covid-19 shock. The key identifying assumption is that no unobserved heterogeneity across furloughed and non-compensated workers affects both participation in the wage compensation scheme and changes in labor market outcomes. Unobserved heterogeneity may induce firms that are most severely affected by the pandemic to select into the scheme. This suggests that the estimated exits from employment averted by the scheme may be a lower bound for the true effect.

The matched difference-in-differences approach is implemented in a two-step procedure. First, the propensity score is estimated using a logit model that predicts the proba-

10. Other studies using this approach include Girma and Görg (2007), Aerts and Schmidt (2008), Böckerman and Ilmakunnas (2009), Leth-Petersen (2010), Mu and Van de Walle (2011), Bentivogli and Mirenda (2017), Espinosa, Desrieux, and Ferracci (2018), Moore, Grosskurth, and Themann (2019), and Ronchetti and Terriau (2019, 2021).

bility of being furloughed by a wage compensated firm conditional on the covariate set. It is used to match each furloughed worker with the non-compensated worker that minimizes the difference in propensity scores.[11] Matching is done with replacement so that the same non-compensated worker can act as a match for more than one furloughed worker. While one-to-one matching with replacement minimizes the risk of bias, it comes at the cost of inefficiency, as many observations for the group of non-compensated workers are discarded. With population-wide data on Danish employer-employee matches, this approach does, however, yield reasonably precise estimates.

Second, using the matched sample of furloughed and non-compensated workers, I estimate a difference-in-differences specification,

$$y_{it} = \alpha + \beta D_i^{TREAT} + \sum_{\text{t=Jan-18}}^{\text{Aug-20}} \delta_t D_t^{MONTH} + \sum_{\text{t=Jan-18}}^{\text{Aug-20}} \gamma_t D_i^{TREAT} \times D_t^{MONTH} + \kappa_{jm} + \epsilon_{it}, \qquad (1)$$

where y_{it} is the labor market outcome of interest: i) a dummy for having an exit from employment, ii) log-labor income, or iii) a dummy for having a job-to-job transition for worker i in month $t \in$ {Jan-18,..., Aug-20}, where Feb-20 is omitted. D_i^{TREAT} is an indicator variable equal to one if worker i is in the treatment group of furloughed workers, and zero otherwise. D_t^{MONTH} are month dummies. κ_{jm} is industry-calendar-month fixed effects for the $j = 18$ industries and $m = 12$ calendar months, introduced to absorb differences in seasonal patterns across industries. Non-compensated workers in the control group are weighted according to the number of times they are matched.

The parameters of interest are γ_t. They measure the average change in the outcome for furloughed workers relative to matched non-compensated workers in each month t. Under the null hypothesis of parallel trends, γ_t equals zero in every pre-scheme month $t \in$ {Jan-18,..., Feb-20}, and identifies the effects of the wage compensation scheme in the post-scheme period $t \in$ {Mar-20,..., Aug-20}.

The validity of the parallel trends assumption relies crucially on the choice of covariates for the matching procedure. The covariate set must include all variables that affect both participation in the wage compensation scheme and changes in labor market outcomes. Two sources of selection are particularly important to handle. First, firms select into the scheme based on how severely they are affected by lockdown policies. This is accounted for by including *ex ante* measures of industry and occupation in the covariate set. While

11. Rosenbaum and Rubin (1983) show that if potential outcomes are independent of treatment conditional on covariates, then they are also independent of treatment conditional on the propensity score.

industry is an important predictor of taking up Danish relief packages (Bennedsen et al., 2020), occupation captures the targeting of the covid-19 shock towards jobs that require face-to-face interactions (Adams-Prassl et al., 2020; Garrote Sanchez et al., 2020; Montenovo et al., 2020). Second, since the management in firms selects which employees to furlough and lay off based on certain characteristics (Bess and Darougheh, 2021; Mattana, Smeets, and Warzynski, 2020), a series of relevant background variables are included in the covariate set. All covariates are measured in the most recently observed month prior to the introduction of the scheme.[12]

A limitation to the covariate set used is that it does not control for firms' use of other covid-19 relief packages, including compensation for fixed costs and various measures of fiscal aid such as deferral of tax obligations. However, Julin, Kuchler, and Otte (2021) find no differences in debt developments between firms receiving and those not receiving wage compensation. Moreover, the covariate set does not include any firm-level measures of exposure to the covid-19 shock, as no *ex ante* measure exists. This further indicates that the estimated exits from employment averted by the scheme may be a lower bound for the true effect. In particular, for firms operating within the same industry, the likelihood of receiving wage compensation is either unrelated to or increasing with the revenue drop experienced in response to the pandemic, depending on the combination of relief packages used (Bennedsen et al., 2020). A series of robustness checks will be performed in section 5.5 to examine the sensitivity of the results to the choice of covariates.

A further requirement for achieving identification is that the covariate distributions for furloughed and non-compensated workers overlap (Heckman, Ichimura, and Todd, 1997). In the next section, it is assessed that the common support condition is satisfied.

The empirical strategy provides a partial equilibrium analysis of the labor market effects of the Danish wage compensation scheme, ignoring general equilibrium effects that may have influenced consumer demand, and thereby employment at all firms. In the absence of the scheme, layoffs of furloughed workers would probably have led to a contraction in consumer demand, an additional worsening of economic conditions, and another round of layoffs. This provides a further argument for interpreting the estimated

12. Table 6 in appendix C presents the covariate set, which includes measures of demographics (age, gender, and children by age intervals in Jan-20, and citizenship in Jan-19), human capital (educational attainment in Sep-19 and firm tenure in Feb-20), and labor market attachment (full-time, inflow to employment, exit from employment, job-to-job transition, and several jobs in Feb-20, and average monthly labor income in employed months of 2019).

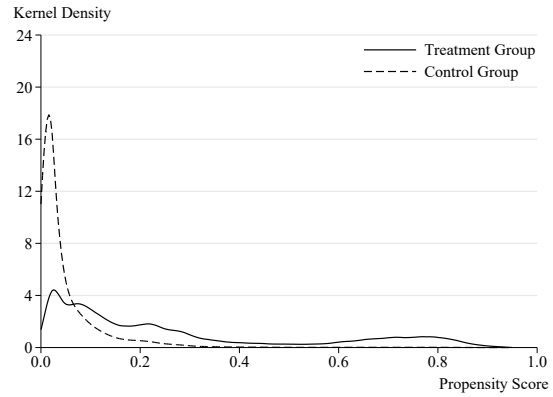
number of exits from employment averted by the scheme as a lower bound for the true effect.

4.1 Assessing the Quality of Matches

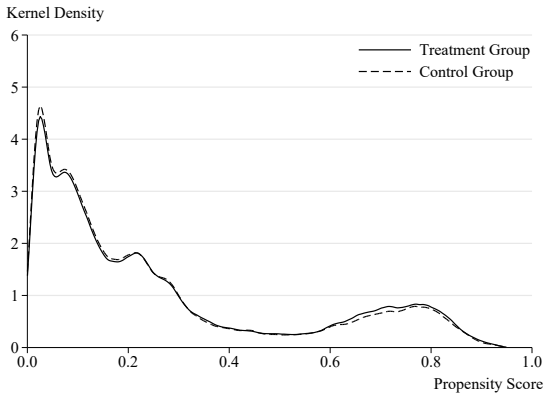
The propensity score is estimated using a logit model that predicts the probability of being furloughed by a wage compensated firm conditional on the covariate set. Tables 10-13 in appendix E present the estimation results transformed to average marginal effects. It appears that both industry and occupation are important determinants of being furloughed. Among industries, the furlough probability is highest for workers employed in accommodation and food services. Among occupations, it is highest for personal service workers. Employees with several jobs are more likely to be furloughed, while part-time and full-time employees are equally likely to be furloughed. Moreover, the probability of being furloughed is decreasing with average monthly labor income in employed months of 2019. Likewise, it is decreasing with age. Generally, men are less likely to be furloughed, while foreign citizens are more likely to be furloughed. Having young children does not appear to be an important determinant of whether an employee is furloughed. Finally, the furlough probability is predominantly decreasing with both tenure and educational attainment higher than primary school.

Figure 1: Kernel Density Estimates of Propensity Scores

(a) Before Matching



(b) After Matching



Note: Panels (1a) and (1b) show kernel density estimates of propensity scores for furloughed workers (solid) and non-compensated workers (dashed) in the baseline sample before and after matching, respectively. The kernel used is an Epanechnikov, and the bandwidth is 0.01.

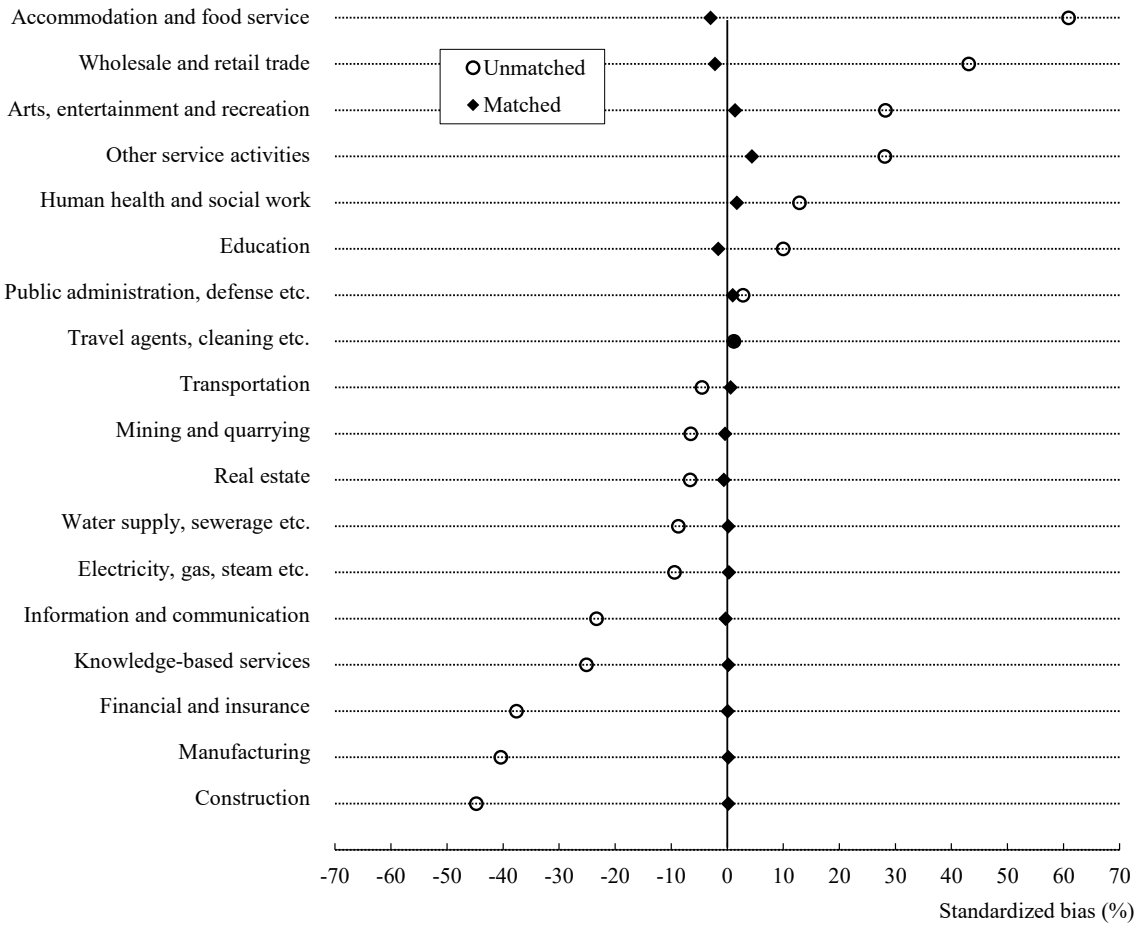
Source: Own calculations based on Danish administrative register data from Statistics Denmark.

The propensity score is used to match furloughed workers to similar non-compensated

workers. It is crucial for the validity of the matching estimator that for every furloughed worker, there exists a non-compensated worker with a similar probability of treatment. Figure 1 depicts kernel density estimates of the propensity scores and shows that there is common support for the two groups. Further, the distributions of propensity scores for furloughed and non-compensated workers are almost identical after matching.

The goal of the matching procedure is to balance covariates across the treatment and control groups. Looking at standardized biases, as suggested by Rosenbaum and Rubin (1985), the two groups appear to have very similar characteristics after matching, cf. figure 2 and figures 6-7 in appendix F. The mean standardized bias is reduced from 15.5 percent before matching to 1.1 percent after matching.

Figure 2: Standardized Biases Across Industries Before and After Matching



Note: The figure shows standardized percentage biases across industries before and after matching. Standardized biases measure, for each covariate, the difference between the means of the treatment and control groups as a percentage of the square root of the average variance across the two groups (Rosenbaum and Rubin, 1985). A positive bias means that the value of the covariate is higher in the treatment group than in the control group (and vice versa). Standardized biases across occupations and the remaining covariates are presented in figures 6-7 in appendix F.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

5 Results

This section presents the main results. First, it is examined to what extent the wage compensation scheme prevented job losses and retained firm-specific human capital. Afterwards, it is examined whether the scheme affected furloughed workers' labor income and labor market mobility.

5.1 Prevention of Job Losses

The first step in the empirical analysis is to examine to what extent the wage compensation scheme prevented job losses for furloughed workers. As a measure of job loss, I use an indicator for having an exit from employment, where an exit is defined as receiving wage income in the current but not in the subsequent month.[13]

Figure 3a shows the monthly share of workers that have an exit from employment (the exit probabilities) for the treatment and control groups. The black and gray dashed vertical lines mark the introduction of the scheme (March 2020) and the end of the compensation period (June 2020), respectively. Following the lockdown in March 2020, there is a sharp increase in the exit share for the control group to 3.2 percent, implying that many non-compensated workers lost their jobs. In the subsequent months, job losses are less pronounced, albeit the monthly exit shares are still above the level seen prior to the pandemic.[14] Among the treated workers, the exit share remains slightly above zero in March 2020, reflecting that furloughed workers' jobs are being preserved. For the remainder of the compensation period (until June), the exit share rises slowly.[15]

Figure 3b shows estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having an exit from employment, with corresponding 95% confidence bands.[16] In the months preceding the introduction of the scheme, almost all estimates are insignificant, supporting the parallel trends assumption. After the introduction of the scheme in March 2020, the exit probability for furloughed workers declines

13. This is a correlated but imperfect measure of job loss. For instance, a worker who is laid off may find a new job before wage payments cease from the previous firm. This further suggests that the estimate for the number of exits from employment averted by the scheme may be a lower bound for the true effect.

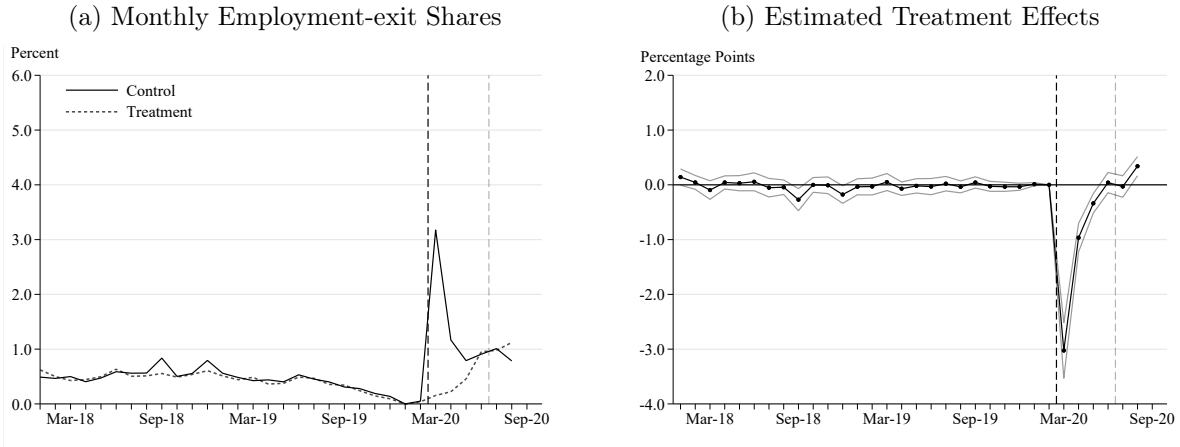
14. This either reflects that non-compensated firms enforce all their layoffs on impact or that fewer layoffs are needed, as the gradual reopening of the Danish economy improves economic conditions.

15. This reflects that firms leave the scheme before their approved compensation period ends to lay off workers and/or that workers are resigning to an increasing degree.

16. These estimates are also presented in panel (a) in table 14 in appendix H.

markedly *relative* to that for similar non-compensated workers in the control group. This suggests that the scheme successfully saved jobs, which is consistent with the findings of Bennedsen et al. (2020).

Figure 3: Monthly Effects of the Scheme on the Exit Probability



Note: Panel (a) shows the monthly shares of workers that have an exit from employment for the treatment and control groups. Panel (b) shows estimates for the average effects of the wage compensation scheme on furloughed workers' employment-exit probability. The gray lines that encircle the estimates are 95% confidence bands based on clustered standard errors at the firm level. The black and gray dashed vertical lines mark the introduction of the scheme and the end of the compensation period, respectively.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 1 panel (1) reports the preferred difference-in-differences estimate of the change in the exit share of the treatment group from the four months preceding the introduction of the scheme to the four months following it relative to the control group. It suggests that the scheme reduces the average exit probability of furloughed workers by 3.8 percentage points in the compensation period from March to June 2020. While the share of furloughed workers that have an exit from employment during this period is 1.7 percent, it would have been approximately 5.5 percent in the absence of the scheme.

Comparing this estimate to the total number of furloughed workers, regardless of compensation periods, I find that the Danish wage compensation scheme prevented approximately 10,400 exits from employment during the first wave of the pandemic (8,500 exits at the lower bound of the 95% confidence interval). However, it also compensated workers that firms were not planning to lay off.

The employed research design implies that the preferred estimate for the number of exits averted by the scheme may be a lower bound for the true effect.

Table 1: Estimated Effect of the Scheme on the Exit Probability

	Main	Robustness		
	(1)	(2)	(3)	(4)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.038***	-0.038***	-0.039***	-0.045***
	(0.004)	(0.004)	(0.004)	(0.004)
D^{POST}	0.044***	0.044***	0.047***	0.055***
	(0.003)	(0.002)	(0.002)	(0.007)
D^{TREAT}	-0.001	-0.001	-0.001	-0.001**
	(0.000)	(0.000)	(0.000)	(0.001)
Industry-time FE	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No
Firm Covariates	No	No	Yes	No
Observations	203,969	203,969	186,192	176,482

Note: The table shows estimates for the average effect of the wage compensation scheme on furloughed workers' exit probability in the compensation period, comparing changes in exit shares of the treatment and control groups from $D^{POST} = 0$ (Nov-19 to Feb-20) to $D^{POST} = 1$ (Mar-20 to Jun-20). Panel (1) presents the preferred estimate from a model with industry-time fixed effects. Panels (2)-(4) each represent a robustness test. Panel (2) includes occupation-time fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Parentheses report standard errors clustered at the firm level. * p<0.05, ** p<0.01, *** p<0.001.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

5.2 Retention of Firm-specific Human Capital

Another important aspect to explore is whether the scheme mainly prevented job losses for workers with little or a fair amount of firm-specific human capital, as this is essential for understanding its implications for both current and future productivity. As a proxy for firm-specific human capital, I use months of tenure in the firm in February 2020.[17] Table 2 shows preferred estimates for the average impact of the wage compensation scheme on furloughed workers' exit probability in the compensation period from March to June 2020 for four subgroups of workers with a) ≤ 1 year, b) $]1;4]$ years, c) $]4;8]$ years, and d) 8+ years of firm tenure, respectively.

The estimated job savings of the scheme appear to be declining with firm tenure. For furloughed workers with up to one year of tenure, the results point to a scheme-induced fall in the exit probability of 5.1 percentage points in the compensation period. For workers with $]1;4]$ years of tenure, the estimated fall is 3.5 percentage points. For workers with $]4;8]$ years of tenure, it is 1.8 percentage points, and for workers with more than eight years of tenure, the response is insignificant.[18] For all four subgroups, the exit shares of furloughed and non-compensated workers appear to follow parallel trends in the

17. Firm tenure is positively correlated with firm-specific human capital, but there are limitations to using this proxy, as workers may, for example, acquire firm-specific skills at different paces.

18. A caveat is that high-tenured workers may have longer resignation periods than low-tenured workers. That is, they may be more likely to have an unobserved layoff because wage payments continue after the sample ends.

pre-scheme period, cf. table 15 in appendix H.

Table 2: Heterogenous Effects on the Exit Probability Across Firm Tenure

	(1)	(2)	(3)	(4)
	$\leq$ 1 year	]1; 4] years	]4; 8] years	$>$ 8 years
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.051***	-0.035***	-0.018***	-0.008
	(0.005)	(0.004)	(0.004)	(0.006)
D^{POST}	0.060***	0.035***	0.030***	0.010***
	(0.006)	(0.003)	(0.002)	(0.002)
D^{TREAT}	-0.001	-0.001	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)
Industry-time FE	Yes	Yes	Yes	Yes
Occupation-time FE	No	No	No	No
Firm Covariates	No	No	No	No
Observations	85,163	76,826	33,017	8,963

Note: The table presents estimates for the average effects of the wage compensation scheme on furloughed workers' exit probability in the compensation period from March to June 2020 for four subgroups of workers with different levels of *ex ante* firm tenure. Each estimate compares the change in the exit share of the treatment group from the four months preceding the introduction of the scheme to the four months following it to the change of the control group. Panels (1)-(4) refer to workers with (1) $\leq$ 1 year, (2)]1; 4] years, (3)]4; 8] years, and (4) $>$ 8 years of tenure in February 2020, respectively. Parentheses report standard errors clustered at the firm level. * p<0.05, ** p<0.01, *** p<0.001.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

These results indicate that the wage compensation scheme mainly prevented job losses for low-tenured workers ($\leq$ 1 year), while furloughed workers with many years of firm tenure (8+ years) would, to a lesser extent, have been laid off even in the absence of the scheme. This may suggest that firm-specific human capital would, to some extent, have been retained anyway. More generally, the results indicate that the benefits of compensating jobs for high-tenured workers are limited. On the other hand, the scheme may have supported future potential output growth by saving jobs for low-tenured workers. A recent paper by Caggese, Cuñat, and Metzger (2019) shows that in the event of an economic contraction like the covid-19 shock, financially constrained firms are induced to fire low-tenured workers with high productivity growth prospects rather than less promising high-tenured workers because of their lower firing costs.

5.3 Intensive Margin Responses

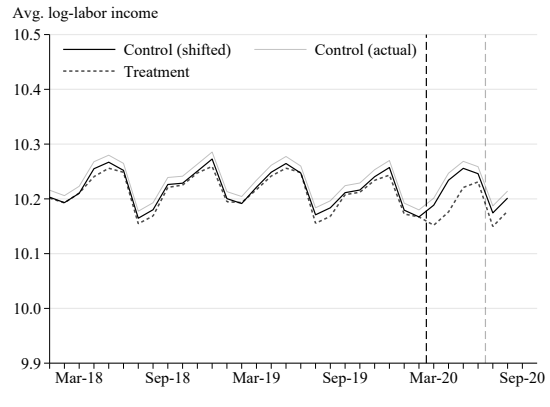
The next step in the empirical analysis is to examine how furloughed workers' labor income was affected by the wage compensation scheme. A relatively narrow measure of labor income is used, excluding mandatory pension contributions and fringe benefits.

Figure 4a shows the average monthly log-labor incomes for the treatment and control groups. The solid black line is the outcome for the control group shifted downwards from

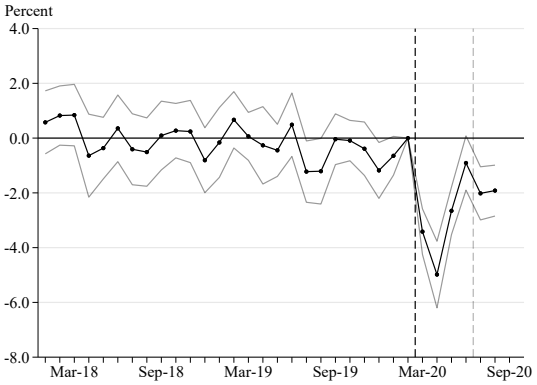
its actual position (the gray line) such that the share in February 2020 equals the one for the treatment group. This graph allows for an easier comparison of pre-trends and scheme effects. In the months preceding the introduction of the scheme, log-labor income develops almost identically for the two groups, despite non-compensated workers earning slightly more. Following the introduction of the scheme in March 2020, the average log-labor income drops for the treatment group, while for the control group, it stays on the pre-scheme path. Assuming that the two groups are equally affected by the pandemic, this indicates that furloughed workers' labor incomes fall in response to the scheme.[19]

Figure 4: Monthly Effects of the Scheme on Log-Labor Income

(a) Average Monthly Log-labor Income



(b) Estimated Treatment Effects



Note: Panel (a) shows the average monthly log-labor income for the treatment and control groups. The solid black line represents the control group shifted downwards from its actual position (the gray line) such that the share in February 2020 equals the one for the treatment group. Panel (b) shows estimates for the average effects of the wage compensation scheme on furloughed workers' log-labor income. The gray lines that encircle the estimates are 95% confidence bands based on clustered standard errors at the firm level. The black and gray dashed vertical lines mark the introduction of the scheme and the end of the compensation period, respectively.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Figure 4b shows estimates for the average effects of the wage compensation scheme on furloughed workers' labor income, with corresponding 95% confidence bands.[20] In the pre-scheme period, all estimates but three are insignificant, supporting the parallel trends assumption. From March 2020 and onward, the scheme is estimated to reduce furloughed workers' labor income. This may indicate that firms and their employees use the option to negotiate wage cuts before applying for wage compensation.[21]

19. Furloughed workers' labor income also appears to decline in most post-scheme months when comparing monthly labor income distributions for 2020 to those for the same months in the pre-scheme years 2018 and 2019, cf. figure 8 in appendix G.

20. These estimates are also presented in panel (a) in table 16 in appendix H.

21. The decline in furloughed workers' labor income is not expected to be driven by a reduction in

Table 3 panel (1) reports the preferred difference-in-differences estimate of the change in the average monthly log-labor income of the treatment group from the four months preceding the introduction of the scheme to the four months following it relative to the control group. It suggests that the scheme reduces the average monthly labor income of furloughed workers by 2.1 percent in the compensation period from March to June 2020. While the average monthly labor income for furloughed workers is DKK 28,258 in this period, it would have been approximately DKK 28,850 in the absence of the scheme. This corresponds to a total loss in earnings of almost DKK 2,400 (USD 370) in the compensation period.

Table 3: Estimated Effect of the Scheme on Log-Labor Income

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.021**	-0.021***	-0.017**	-0.018*	-0.027***
	(0.007)	(0.006)	(0.006)	(0.007)	(0.003)
D^{POST}	-0.152***	-0.038	-0.100	-0.145***	0.047*
	(0.034)	(0.054)	(0.058)	(0.041)	(0.023)
D^{TREAT}	-0.015	-0.018	-0.015	-0.017	-0.021
	(0.012)	(0.010)	(0.011)	(0.013)	(0.012)
Industry-time FE	Yes	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	203,969	203,969	186,192	176,482	183,512

Note: The table shows estimates for the average effect of the wage compensation scheme on furloughed workers' average monthly log-labor income in the compensation period, comparing changes in the average monthly log-labor incomes of the treatment and control groups from $D^{POST} = 0$ (Nov-19 to Feb-20) to $D^{POST} = 1$ (Mar-20 to Jun-20). Panel (1) presents the preferred estimate from a model with industry-time fixed effects. Panels (2)-(5) each represent a robustness test. Panel (2) includes occupation-time fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Panel (5) re-matches workers on a restricted sample that only includes workers who are employed during the compensation period. Parentheses report standard errors clustered at the firm level. * p<0.05, ** p<0.01, *** p<0.001.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

5.4 Labor Market Mobility

The final step in the empirical analysis is to examine to what extent the wage compensation scheme affected labor market mobility. As a measure of labor market mobility, I use an indicator for having a job-to-job transition, where a job-to-job transition is defined as a change in firm identifiers of primary workplaces between the current and the previous month.[22]

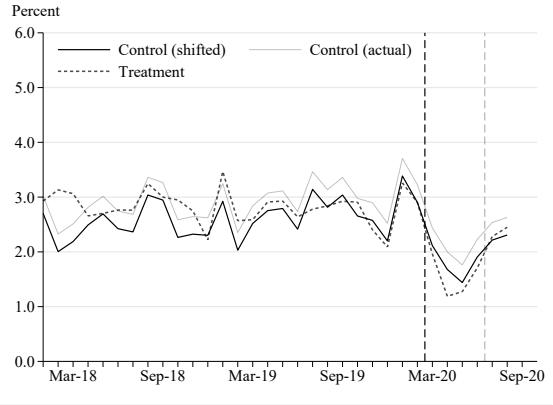
salary add-ons, as wage compensation accounted for all foreseen add-ons for planned work, such as shift differentials. It even accounted for supplements for usual overtime work, unless the firm manually corrected the salary in the application.

22. This is a highly correlated but imperfect measure of job-to-job transitions. For instance, if a workplace gets a new CVR number (firm identifier) due to changes in ownership or company type, it will be

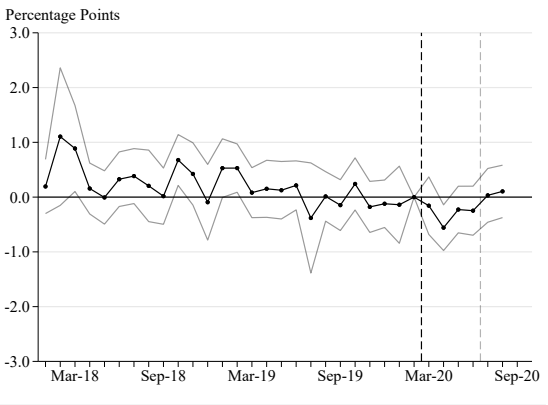
Figure 5a shows the monthly shares of workers that have a job-to-job transition for the treatment and control groups. The solid black line is the outcome for the control group shifted downwards from its actual position (the gray line) such that the share in February 2020 equals the one for the treatment group. This illustration shows that before the introduction of the scheme, the job-to-job transition shares develop fairly similarly for the treatment and control groups. Following the lockdown of the economy in March 2020, the job-to-job transition shares for both groups decline and stay below the pre-pandemic level for the remainder of the observed period. This likely reflects that vacancies fell markedly during this period (Bess, Borgensgaard, and Iuel, 2020). More interestingly, from April 2020 and until the end of the compensation period, the job-to-job transition share for the treatment group declines by relatively more than for the shifted control group. This indicates that labor market mobility was slightly reduced by the scheme.

Figure 5: Monthly Effects of the Scheme on the Job-to-Job Transition Probability

(a) Monthly Job-to-Job Transition Shares



(b) Estimated Treatment Effects



Note: Panel (a) shows the monthly shares of workers that have a job-to-job transition for the treatment and control groups. The solid black line represents the control group shifted downwards from its actual position (the gray line) such that the share in February 2020 equals the one for the treatment group. Panel (b) shows estimates for the average effects of the wage compensation scheme on furloughed workers' job-to-job transition probability. The gray lines that encircle the estimates are 95% confidence bands based on clustered standard errors at the firm level. The black and gray dashed vertical lines mark the introduction of the scheme and the end of the compensation period, respectively.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Figure 5b presents the matched difference-in-differences estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having a job-to-job transition, with corresponding 95% confidence bands.[23] In the pre-scheme pe-

incorrectly counted as a job-to-job transition.

23. These estimates are also presented in panel (a) in table 17 in appendix H.

riod, almost all estimates are insignificant, supporting the parallel trends assumption. In the compensation period, the scheme is estimated to have a small negative impact on furloughed workers' job-to-job transition probability, but only the estimate for April is statistically significant. As a result, when estimating the aggregate response for the compensation period, the estimate is very small and insignificant as well. In particular, table 4 shows that the preferred estimate of the change in furloughed workers' job-to-job transition share from the four months preceding the introduction of the scheme to the four months following it relative to the control group is -0.003. This suggests that labor market mobility was basically unaffected by the wage compensation scheme.

This result likely reflects that job search costs were extremely high during the lockdown of the Danish economy, as vacancies declined drastically. For instance, Bess, Borgensgaard, and Iuel (2020) show that from late March to late April 2020, the number of job postings in accommodation and food services was approximately 80 percent below the historical average. As a result, neither furloughed nor non-compensated workers had strong incentives to apply for new jobs.

Table 4: Estimated Effect of the Scheme on the Job-to-Job Transition Probability

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.003	-0.003	-0.010*	-0.005	-0.007
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)
D^{POST}	0.050	0.045	0.026	-0.059	0.054
	(0.029)	(0.033)	(0.045)	(0.044)	(0.049)
D^{TREAT}	-0.013**	-0.013**	-0.010*	-0.007	-0.008
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Industry-time FE	Yes	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	203,969	203,969	186,192	176,482	183,512

Note: The table shows estimates for the average effect of the wage compensation scheme on furloughed workers' job-to-job probability in the compensation period, comparing changes in job-to-job transition shares of the treatment and control groups from $D^{POST} = 0$ (Nov-19 to Feb-20) to $D^{POST} = 1$ (Mar-20 to Jun-20). Panel (1) presents the preferred estimate from a model with industry-time fixed effects. Panels (2)-(5) each represent a robustness test. Panel (2) includes occupation-time fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Panel (5) re-matches workers on a restricted sample that only includes workers who are employed during the compensation period. Parentheses report standard errors clustered at the firm level. * p<0.05, ** p<0.01, *** p<0.001.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

5.5 Robustness

To address possible caveats in the research design, panels (2)-(5) in tables 1 and 3-4 report estimates associated with relevant robustness checks.

First, occupation-time fixed effects are included in the specification. These covariates flexibly allow each of the 39 occupations to have its own time trend. Workers with occupations that are often offered as seasonal positions, e.g., waiters, are more likely to experience an increase in labor income during peak months than workers with other occupations. They are also more likely to have an exit from employment or a job-to-job transition when the temporary employment ends. Panel (2) shows that for all three outcomes, the results are very similar to those of the preferred specification.

Second, firms may be more likely to apply for wage compensation if they experienced negative growth before the pandemic (Nielsen and Hansen, 2020). To account for this, I expand the covariate set with employment growth dummies, indicating whether the workplace had negative, stable (up to 10 percent), positive (10-20 percent), or highly positive (more than 20 percent) average employment growth in 2016-2019. For firms that have not existed that long, there is a dummy indicating that it is a new firm. Another concern is that firms operating within the same industries are not equally affected by the pandemic. To account for this, the covariate set is extended with the most frequent indicator of firm-level covid-19 exposure made possible by the Danish registers, which is the change in sales from March 2019 to March 2020. Further, I include a measure of average firm wages in February 2020, as firms with higher wages may be less likely to select into the scheme due to the monthly wage compensation cap of DKK 30,000. Panel (3) shows that re-matching workers on this extended covariate set does not change the main results. For all three outcomes, the estimated effect of the scheme is close to that of the baseline model.

Third, for workers with several jobs, exits from employment and job-to-job transitions are potentially incorrectly measured because the sample is restricted to workers' main occupation. If a worker separates from one of several jobs, this will not be counted as an exit from employment, but incorrectly as a job-to-job transition, if it is the main job, or otherwise be unobserved, leading to bias in the estimated impact of the scheme. Moreover, since workers are not matched based on industry and occupation of secondary jobs, these may not be equally affected by the pandemic across treatment and control groups. As a result, the estimated decline in total labor income may simply be explained by furloughed workers being laid off from their side jobs to a larger extent than non-compensated workers. To address this concern, I exclude workers with several jobs in

February 2020 and then re-match them. Panel (4) shows that this does not change the estimated effect of the scheme significantly for any outcome.

Fourth, non-compensated workers in the control group who are laid off in the post-scheme period could be different from those that are not. If those that are laid off generally earn less, then the estimated effects on labor income would be negative even in the absence of the scheme. Likewise, the estimated decline in the job-to-job transition probability may simply be explained by fewer transitions of non-compensated workers who are laid off compared to those that are not. To account for this, I re-match workers on a restricted sample that only includes workers who are employed during the entire compensation period from March to June 2020. Panel (5) in tables 3 and 4 shows that for both labor income and the job-to-job transition probability, the estimated effect of the scheme is numerically larger. However, neither estimate differs significantly from those of the baseline model.

Fifth and finally, a concern is that the results do not carry over to furloughed workers with other compensation periods than March to June 2020. However, it does not change the main findings to define treatment as being furloughed in *at least one* post-scheme month from March to August 2020 instead, cf. figure 9 in appendix I. I still find that the scheme averted exits from employment, reduced labor income of furloughed workers, and did not affect labor market mobility noticeably.

6 Conclusion

This paper has examined the labor market effects of a temporary wage compensation scheme introduced in Denmark in March 2020 to limit job losses following the covid-19 pandemic. The scheme provided wage subsidies to private firms that faced extensive layoffs due to the pandemic but chose to furlough workers with pay instead.

The effects are estimated using detailed register data and a matched difference-in-differences estimator, comparing the growth rate of several labor market outcomes from before to after the introduction of the scheme for furloughed workers and their non-compensated matches. The two groups are matched on the propensity score to have similar pre-pandemic characteristics and similar *ex ante* exposures to the covid-19 shock.

The findings suggest that during economic contractions, job retention schemes work as

intended by mitigating job losses. The Danish wage compensation scheme did, however, also compensate workers that firms were not planning to lay off. It mainly prevented job losses for low-tenured workers ($\leq$ 1 year), while high-tenured workers (8+ years) would, to a lesser extent, have been laid off even in the absence of the scheme. This may suggest that firm-specific human capital would, to some extent, have been retained anyway. Further, the results indicate that firms and their employees might have used the option to negotiate wage cuts before applying for wage compensation and that labor market mobility was basically unaffected by the scheme. The latter suggests that job retention schemes do not prevent reallocation of workers from less to more productive job matches in periods with few vacancies.

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Appendices

Appendix A – Different Compensation Periods

Table 5: Furloughed Workers by Compensation Periods

—— Compensation period ——		— Approved applications —
Start month	End month	Furloughed workers
Mar-20	Mar-20	2,667
Mar-20	Apr-20	12,434
Mar-20	May-20	10,745
Mar-20	**Jun-20**	**133,481**
Mar-20	Jul-20	28,026
Mar-20	Aug-20	17,270
Apr-20	Apr-20	3,841
Apr-20	May-20	4,694
Apr-20	Jun-20	27,516
Apr-20	Jul-20	10,030
Apr-20	Aug-20	4,971
May-20	May-20	1,075
May-20	Jun-20	5,368
May-20	Jul-20	3,907
May-20	Aug-20	2,045
Jun-20	Jun-20	1,003
Jun-20	Jul-20	2,288
Jun-20	Aug-20	1,110
Jul-20	Jul-20	75
Jul-20	Aug-20	713
Aug-20	Aug-20	12
Total		273,271

Note: The table shows the number of workers who were furloughed in each of the 21 possible compensation periods. The bold line marks the largest group of furloughed workers who were sent home from March 2020 to June 2020. It is based on the raw wage compensation data from the register LONKOMP and not the baseline sample used in the empirical analysis. However, observations with missing worker identifiers are discarded.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Appendix B – Timeline of Danish Covid-19 Policies

Box 1. Timeline of Danish Covid-19 Policies in 2020

Feb 27: First confirmed case of covid-19 in Denmark.

Mar 11: Press conference with Danish Prime Minister Mette Frederiksen. Closure of public schools, daycare and cultural institutions. Ban on indoor gatherings of more than 100 participants.

Mar 14: Closure of Danish borders.

Mar 15: Announcement of tripartite agreement on temporary wage compensation for private-sector employees.

Mar 17: Closure of hairdressers, malls, bars, gyms, and other businesses with close customer contact. Cafes and restaurants close for guests but are allowed to deliver takeaway orders. Ban on large gatherings tightened, now prohibiting both indoor and outdoor gatherings of more than 10 people.

Mar 19: Announcement of agreement on temporary compensation for firms' fixed costs. Extension of entitlement to unemployment insurance and sickness benefits.

Mar 24: Danish Parliament passes the law on temporary wage compensation for employees in private firms.

Apr 15: Reopening of daycare institutions and schools from 0th to 5th grade.

Apr 18: Agreement on refund of paid VAT and payroll taxes as an interest-free loan.

Apr 20: Reopening of liberal professions, including hairdressers and masseurs.

May 11: Reopening of the retail sector, including shopping malls.

May 18: Reopening of cafes, restaurants, and schools for 6th to 10th grade.

May 27: Reopening of several cultural institutions, including theaters and cinemas.

Jun 08: Reopening of gyms and swimming pools.

Jun 15: Reopening of borders for tourists from Germany, Norway, and Iceland.

Jun 27: Reopening of borders for European countries that meet infection criteria.

Jul 08: Termination of the fixed costs compensation scheme for all firms but those that are still subject to covid-19 restrictions.

Aug 29: Termination of the wage compensation scheme for employees.

Aug 31: Termination of the fixed costs compensation scheme for remaining firms.

Source: IMF (2020), Stephensen and Hansen (2020), Philipsen (2020), Bloch, Holm, and Rohde (2020), Retsinformation (2020a, 2020b), Bitsch and Skinbjerg (2020), Honoré (2020), Hare (2020), Bloch, Munksgaard, and Kildegaard (2020), Randeris (2020), Berlingske (2020), Dansk Erhverv (The Danish Chamber of Commerce) (2020), and Erhvervsministeriet (Ministry of Industry, Business and Financial Affairs) (2020a, 2020b, 2020c, 2020d).

Appendix C – Description of the Covariate Set

Table 6: Description of the Covariate Set

Covariate	Description
Age	Age in years on January 1, 2020.
Man	An indicator that equals one if a worker is male, and zero otherwise.
Foreign	An indicator that equals one if a worker has non-Danish or unobserved citizenship on January 1, 2019, and zero otherwise.
Children indicators	Four indicators that equal one if a worker's youngest child is a) up to 5 years old, b) 6-10 years old, c) 11-15 years old, or if a worker d) has no children or only children older than 15 years on 1 January, 2020, respectively, and equal to zero otherwise.
Educational indicators	Indicators that equal one if a worker's highest educational attainment is a) primary school, b) high school, c) vocational education (e.g. carpenter, mechanic, nursing home assistant), d) short higher education (e.g. 2-year marketing economist), e) medium higher education (e.g. trained childcare worker), f) bachelor's degree, g) long higher education (e.g. master's degree), and h) research (e.g. PhD), respectively, in September 2020, and equal to zero otherwise. They act as measures of general human capital.
Tenure	The number of months a worker has been employed in a given firm in February 2020, censored above a threshold of 12 years and 2 months, as the data is only observed from 2008. It acts as a measure of firm-specific human capital.
Full-time	An indicator that equals one if an employee worked at least 160 hours (full time) in February 2020, and zero otherwise. This classification follows *SKAT's* reporting instructions for the Danish Income Tax Register with a minor correction. The instruction states that full-time work must always be reported as 160.33 hours per month (*SKAT* (The Danish Customs and Tax Administration), 2021). The correction is made because there are many observations, where the reported number of hours is 160, which is interpreted as incorrect reporting rather than part-time work.
Inflow to employment	An indicator that equals one if a worker has an inflow to employment in February 2020, and zero otherwise. An inflow is defined as receiving wages in the current but not in the previous month.
Exit from employment	An indicator that equals one if a worker has an exit from employment in February 2020, and zero otherwise. An exit is defined as receiving wages in the current but not in the subsequent month.
Several jobs	An indicator that equals one if a worker has several jobs in February 2020, and zero otherwise. Several jobs are defined as receiving wages from at least two workplaces with different CVR numbers.
Job-to-job transition	An indicator that equals one if a worker has a job-to-job transition in February 2020, and zero otherwise. A job-to-job transition is defined as a change in firm identifiers of workplaces between the current and the previous month.
Avg. monthly labor income in 2019	Average monthly labor income in 2019 conditional on employment. A relatively narrow measure of labor income is used, excluding mandatory pension contributions and fringe benefits.
Industry indicators	Indicators describing the industry of the workplace that employs the worker in February 2020. I distinguish between 20 industries that are defined according to the NACE classification. For a description of each industry, see Statistics Denmark.
Occupation indicators	Indicators describing a worker's occupation in February 2020. I distinguish between 38 occupations that are defined according to the DISCO-08 classification, but where one of them ("other occupations") includes all occupational codes that are shared by less than 150 non-compensated workers in February 2020. For a description of the DISCO-08 classification, see Statistics Denmark.

Note: This table describes the baseline covariate set used to estimate the propensity score for being furloughed by a wage compensated firm in section 4.1.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Appendix D – Descriptive Statistics

Table 7: Descriptive Statistics Across Selected Covariates

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Age (Years)	42.867	39.099	43.149	39.102	39.099	39.105
	(11.839)	(12.229)	(11.761)	(12.236)	(12.229)	(12.244)
Man	0.656	0.481	0.669	0.482	0.481	0.483
	(0.475)	(0.500)	(0.471)	(0.500)	(0.500)	(0.500)
Foreign	0.066	0.100	0.064	0.101	0.100	0.102
	(0.249)	(0.301)	(0.244)	(0.302)	(0.301)	(0.303)
Youngest child is $\leq$ 5 years	0.127	0.124	0.127	0.125	0.124	0.125
	(0.332)	(0.330)	(0.333)	(0.330)	(0.330)	(0.330)
Youngest child is 6-10 years	0.102	0.091	0.103	0.093	0.091	0.094
	(0.303)	(0.288)	(0.304)	(0.290)	(0.288)	(0.292)
Youngest child is 11-15 years	0.103	0.083	0.104	0.084	0.083	0.084
	(0.303)	(0.276)	(0.305)	(0.277)	(0.276)	(0.278)
High school	0.089	0.170	0.083	0.174	0.170	0.178
	(0.284)	(0.376)	(0.275)	(0.379)	(0.376)	(0.383)
Vocational training	0.434	0.435	0.434	0.430	0.435	0.426
	(0.496)	(0.496)	(0.496)	(0.495)	(0.496)	(0.495)
Short higher education	0.079	0.057	0.081	0.058	0.057	0.059
	(0.270)	(0.233)	(0.273)	(0.234)	(0.233)	(0.236)
Medium higher education	0.095	0.077	0.096	0.077	0.077	0.078
	(0.293)	(0.266)	(0.295)	(0.267)	(0.266)	(0.268)
Bachelor's degree	0.025	0.022	0.025	0.023	0.022	0.023
	(0.156)	(0.148)	(0.157)	(0.149)	(0.148)	(0.151)
Long higher education	0.109	0.058	0.113	0.057	0.058	0.056
	(0.312)	(0.233)	(0.317)	(0.232)	(0.233)	(0.230)
Research education	0.008	0.002	0.008	0.002	0.002	0.002
	(0.088)	(0.048)	(0.091)	(0.049)	(0.048)	(0.049)
Full-time	0.611	0.551	0.616	0.553	0.551	0.554
	(0.487)	(0.497)	(0.486)	(0.497)	(0.497)	(0.497)
Inflow to employment	0.009	0.009	0.008	0.009	0.009	0.009
	(0.092)	(0.094)	(0.092)	(0.095)	(0.094)	(0.095)
Exit from employment	0.006	0.000	0.006	0.000	0.000	0.001
	(0.075)	(0.020)	(0.078)	(0.021)	(0.020)	(0.022)
Several jobs	0.066	0.138	0.061	0.147	0.138	0.155
	(0.248)	(0.345)	(0.239)	(0.354)	(0.345)	(0.362)
Job-to-job transition	0.019	0.029	0.018	0.031	0.029	0.032
	(0.136)	(0.168)	(0.133)	(0.172)	(0.168)	(0.177)
Tenure (Years)	4.850	3.886	4.922	3.813	3.886	3.741
	(4.240)	(3.859)	(4.258)	(3.818)	(3.859)	(3.776)
Avg. monthly labor income in 2019 (DKK 1,000)	34.701	29.644	35.078	29.622	29.644	29.600
	(12.042)	(10.822)	(12.043)	(10.877)	(10.822)	(10.932)
Observations	743,509	51,632	691,877	90,231	51,632	38,599

Note: The table shows sample means of selected covariates for the full sample and the subsamples of furloughed (treated) and non-compensated (control) workers both before and after matching. The reference category for the three children indicators is having no children or only children older than 15 years, and for the seven human capital indicators, it is primary school. Parentheses report standard deviations.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 8: Distribution of Workers Across Industries in February 2020

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Mining and quarrying	0.003	0.001	0.003	0.001	0.001	0.001
	(0.057)	(0.024)	(0.059)	(0.026)	(0.024)	(0.027)
Manufacturing	0.220	0.086	0.230	0.085	0.086	0.085
	(0.414)	(0.280)	(0.421)	(0.279)	(0.280)	(0.279)
Electricity, gas, steam etc.	0.005	0.000	0.005	0.000	0.000	0.000
	(0.070)	(0.018)	(0.072)	(0.016)	(0.018)	(0.014)
Water supply, sewerage etc.	0.006	0.001	0.007	0.001	0.001	0.001
	(0.079)	(0.035)	(0.081)	(0.034)	(0.035)	(0.033)
Construction	0.137	0.024	0.145	0.023	0.024	0.023
	(0.344)	(0.152)	(0.352)	(0.151)	(0.152)	(0.150)
Wholesale and retail trade	0.183	0.356	0.170	0.360	0.356	0.365
	(0.387)	(0.479)	(0.376)	(0.480)	(0.479)	(0.481)
Transportation	0.058	0.049	0.059	0.048	0.049	0.047
	(0.234)	(0.215)	(0.236)	(0.214)	(0.215)	(0.213)
Accommodation and food service	0.018	0.173	0.006	0.177	0.173	0.181
	(0.133)	(0.378)	(0.080)	(0.382)	(0.378)	(0.385)
Information and communication	0.066	0.021	0.069	0.021	0.021	0.022
	(0.248)	(0.144)	(0.254)	(0.145)	(0.144)	(0.146)
Financial and insurance	0.076	0.006	0.081	0.006	0.006	0.006
	(0.265)	(0.076)	(0.273)	(0.075)	(0.076)	(0.074)
Real estate	0.023	0.015	0.024	0.015	0.015	0.016
	(0.151)	(0.121)	(0.153)	(0.123)	(0.121)	(0.124)
Knowledge-based services	0.084	0.029	0.088	0.029	0.029	0.029
	(0.277)	(0.169)	(0.283)	(0.168)	(0.169)	(0.167)
Travel agents, cleaning etc.	0.054	0.056	0.054	0.055	0.056	0.053
	(0.225)	(0.230)	(0.225)	(0.228)	(0.230)	(0.225)
Public administration, defense etc.	0.002	0.003	0.002	0.003	0.003	0.003
	(0.046)	(0.058)	(0.045)	(0.056)	(0.058)	(0.054)
Education	0.004	0.012	0.003	0.012	0.012	0.013
	(0.061)	(0.107)	(0.056)	(0.110)	(0.107)	(0.113)
Human health and social work	0.038	0.064	0.036	0.063	0.064	0.061
	(0.192)	(0.246)	(0.187)	(0.242)	(0.246)	(0.239)
Arts, entertainment and recreation	0.007	0.047	0.003	0.046	0.047	0.045
	(0.081)	(0.213)	(0.059)	(0.210)	(0.213)	(0.208)
Other service activities	0.010	0.055	0.007	0.052	0.055	0.048
	(0.101)	(0.229)	(0.083)	(0.221)	(0.229)	(0.214)
Observations	743,509	51,632	691,877	90,231	51,632	38,599

Note: The table shows sample means of the industry dummies that are included in the covariate set for the full sample and the subsamples of furloughed (treated) and non-compensated (control) workers both before and after matching. These are measured in February 2020, and the reference category is agriculture, forestry, and fishing. Parentheses report standard deviations.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 9: Distribution of Workers Across Occupations in February 2020

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Top management and legislators	0.012	0.003	0.013	0.003	0.003	0.003
	(0.110)	(0.058)	(0.113)	(0.057)	(0.058)	(0.056)
Administrative managers etc.	0.019	0.013	0.019	0.012	0.013	0.012
	(0.136)	(0.112)	(0.137)	(0.110)	(0.112)	(0.109)
Production and services managers	0.014	0.005	0.014	0.005	0.005	0.005
	(0.116)	(0.070)	(0.118)	(0.069)	(0.070)	(0.068)
Hotel, restaurant, retail managers etc.	0.008	0.017	0.007	0.017	0.017	0.017
	(0.087)	(0.128)	(0.084)	(0.128)	(0.128)	(0.128)
Science and engineering professionals	0.046	0.011	0.048	0.011	0.011	0.011
	(0.209)	(0.106)	(0.214)	(0.105)	(0.106)	(0.104)
Health professionals	0.012	0.031	0.010	0.031	0.031	0.032
	(0.108)	(0.173)	(0.102)	(0.174)	(0.173)	(0.175)
Teaching professionals	0.019	0.018	0.019	0.018	0.018	0.018
	(0.136)	(0.134)	(0.136)	(0.134)	(0.134)	(0.135)
Business and administration professionals	0.063	0.017	0.066	0.018	0.017	0.018
	(0.242)	(0.131)	(0.248)	(0.131)	(0.131)	(0.132)
Information technology professionals etc.	0.045	0.007	0.047	0.007	0.007	0.006
	(0.206)	(0.084)	(0.212)	(0.081)	(0.084)	(0.079)
Legal, social and cultural professionals	0.017	0.007	0.018	0.008	0.007	0.009
	(0.130)	(0.084)	(0.133)	(0.088)	(0.084)	(0.093)
Science and engineering ass. professionals	0.045	0.018	0.047	0.018	0.018	0.017
	(0.207)	(0.132)	(0.212)	(0.131)	(0.132)	(0.131)
Health associate professionals	0.010	0.032	0.009	0.032	0.032	0.033
	(0.101)	(0.176)	(0.092)	(0.177)	(0.176)	(0.177)
Business and admin. ass. professionals	0.088	0.068	0.089	0.068	0.068	0.069
	(0.283)	(0.252)	(0.285)	(0.252)	(0.252)	(0.253)
Legal, social and cultural ass. professionals	0.007	0.033	0.005	0.031	0.033	0.029
	(0.081)	(0.179)	(0.067)	(0.173)	(0.179)	(0.167)
Information technicians etc.	0.012	0.004	0.013	0.005	0.004	0.005
	(0.109)	(0.067)	(0.112)	(0.068)	(0.067)	(0.070)
Secretaries and office clerks	0.045	0.041	0.045	0.042	0.041	0.043
	(0.207)	(0.199)	(0.207)	(0.201)	(0.199)	(0.204)
Customer services clerks	0.014	0.034	0.012	0.032	0.034	0.030
	(0.116)	(0.181)	(0.109)	(0.176)	(0.181)	(0.170)
Numerical and material recording clerks	0.041	0.025	0.042	0.025	0.025	0.026
	(0.199)	(0.156)	(0.202)	(0.158)	(0.156)	(0.159)
Other clerical support workers	0.010	0.013	0.010	0.013	0.013	0.014
	(0.102)	(0.113)	(0.101)	(0.115)	(0.113)	(0.116)
Personal service workers	0.026	0.149	0.017	0.139	0.149	0.130
	(0.159)	(0.356)	(0.128)	(0.346)	(0.356)	(0.336)
Sales workers	0.057	0.165	0.049	0.171	0.165	0.177
	(0.233)	(0.371)	(0.217)	(0.376)	(0.371)	(0.381)
Personal care workers	0.024	0.019	0.024	0.019	0.019	0.019
	(0.153)	(0.138)	(0.154)	(0.137)	(0.138)	(0.136)
Protective services workers	0.005	0.011	0.004	0.011	0.011	0.011
	(0.068)	(0.103)	(0.065)	(0.105)	(0.103)	(0.106)
Building workers and painters	0.067	0.014	0.071	0.015	0.014	0.015
	(0.251)	(0.119)	(0.257)	(0.121)	(0.119)	(0.122)
Metal and machinery workers	0.048	0.035	0.049	0.035	0.035	0.036
	(0.213)	(0.184)	(0.215)	(0.185)	(0.184)	(0.186)
Handicraft and printing workers	0.003	0.005	0.003	0.005	0.005	0.005
	(0.058)	(0.068)	(0.057)	(0.069)	(0.068)	(0.071)
Electrical and electronic workers	0.027	0.009	0.028	0.009	0.009	0.009
	(0.162)	(0.093)	(0.166)	(0.094)	(0.093)	(0.094)
Food processing workers etc.	0.013	0.009	0.013	0.009	0.009	0.009
	(0.112)	(0.094)	(0.113)	(0.095)	(0.094)	(0.097)
Stationary plant and machine operators	0.047	0.020	0.049	0.019	0.020	0.018
	(0.211)	(0.139)	(0.215)	(0.136)	(0.139)	(0.134)
Assemblers	0.010	0.006	0.011	0.006	0.006	0.006
	(0.102)	(0.080)	(0.103)	(0.079)	(0.080)	(0.078)
Drivers and mobile plant operators	0.047	0.026	0.048	0.026	0.026	0.026
	(0.211)	(0.160)	(0.215)	(0.160)	(0.160)	(0.159)
Cleaners and helpers	0.019	0.045	0.017	0.045	0.045	0.045
	(0.136)	(0.207)	(0.129)	(0.207)	(0.207)	(0.208)
Agricultural and forestry laborers etc.	0.001	0.000	0.001	0.000	0.000	0.000
	(0.026)	(0.022)	(0.026)	(0.021)	(0.022)	(0.020)
Mining and construction laborers etc.	0.064	0.045	0.066	0.045	0.045	0.046
	(0.245)	(0.207)	(0.248)	(0.208)	(0.207)	(0.209)
Food preparation assistants	0.006	0.032	0.004	0.037	0.032	0.041
	(0.077)	(0.177)	(0.063)	(0.188)	(0.177)	(0.198)
Refuse workers etc.	0.007	0.009	0.007	0.009	0.009	0.009
	(0.086)	(0.096)	(0.085)	(0.096)	(0.096)	(0.097)
Other occupations	0.000	0.000	0.000	0.000	0.000	0.000
	(0.021)	(0.017)	(0.022)	(0.019)	(0.017)	(0.021)
Observations	743,509	51,632	691,877	90,231	51,632	38,599

Note: The table shows sample means of the occupation dummies that are included in the covariate set for the full sample and the subsamples of furloughed (treated) and non-compensated (control) workers both before and after matching. These are measured in February 2020, and the reference category is agricultural, forestry, and fishery workers. Parentheses report standard deviations.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Appendix E – Results from the Propensity Score Estimation

Table 10: Estimated Effects of Covariates on the Furlough Probability

	Logit estimates	Average marginal effects
Age (Years)	-0.010***	-0.001***
	(0.001)	(0.000)
Man	-0.347***	-0.018***
	(0.012)	(0.001)
Foreign	0.203***	0.011***
	(0.020)	(0.001)
Youngest child is $\leq$ 5 years	0.023	0.001
	(0.017)	(0.001)
Youngest child is 6-10 years	-0.019	-0.001
	(0.018)	(0.001)
Youngest child is 11-15 years	-0.052**	-0.003**
	(0.019)	(0.001)
High school	0.169***	0.009***
	(0.020)	(0.001)
Vocational training	0.094***	0.005***
	(0.015)	(0.001)
Short higher education	-0.252***	-0.012***
	(0.026)	(0.001)
Medium higher education	-0.192***	-0.009***
	(0.025)	(0.001)
Bachelor's degree	-0.205***	-0.010***
	(0.037)	(0.002)
Long higher education	-0.456***	-0.020***
	(0.027)	(0.001)
Research education	-0.900***	-0.034***
	(0.102)	(0.003)
Full-time	0.013	0.001
	(0.012)	(0.001)
Inflow to employment	-0.094	-0.005
	(0.056)	(0.003)
Exit from employment	-3.529***	-0.066***
	(0.230)	(0.001)
Several jobs	0.659***	0.039***
	(0.017)	(0.001)
Job-to-job transition	-0.269***	-0.012***
	(0.035)	(0.001)
Tenure (Years)	-0.022***	-0.001***
	(0.001)	(0.000)
Avg. monthly labor income in 2019 (DKK 1,000)	-0.015***	-0.001***
	(0.001)	(0.000)
Constant	-4.942***	
	(0.211)	
Observations	743,509	743,509

Note: The table shows estimation results from a logistic regression of the baseline covariate set on the treatment indicator. It both presents non-transformed logit estimates and average marginal effects. Average marginal effects are evaluated for discrete changes of dummy variables from their base level and one unit changes of continuous variables. Parentheses report standard errors. The reference category for the children indicators is having no children or only children older than 15 years, and for the human capital indicators, it is primary school. * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Estimation results for the industry and occupation dummies are presented in tables 11-13 in appendix E.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 11: Estimated Effects of Industries on the Furlough Probability

	Logit estimates	Average marginal effects
Mining and quarrying	0.263	0.014
	(0.230)	(0.013)
Manufacturing	0.932***	0.058***
	(0.134)	(0.010)
Electricity, gas, steam etc.	-0.881**	-0.033***
	(0.277)	(0.008)
Water supply, sewerage etc.	0.123	0.006
	(0.185)	(0.010)
Construction	0.175	0.009
	(0.137)	(0.008)
Wholesale and retail trade	2.220***	0.160***
	(0.133)	(0.015)
Transportation	1.705***	0.135***
	(0.135)	(0.015)
Accommodation and food service	4.159***	0.557***
	(0.134)	(0.024)
Information and communication	0.904***	0.059***
	(0.137)	(0.011)
Financial and insurance	-0.654***	-0.027***
	(0.145)	(0.005)
Real estate	0.640***	0.039***
	(0.138)	(0.010)
Knowledge-based services	0.841***	0.053***
	(0.135)	(0.011)
Travel agents, cleaning etc.	1.480***	0.111***
	(0.134)	(0.014)
Public administration, defense etc.	2.281***	0.221***
	(0.157)	(0.024)
Education	3.303***	0.393***
	(0.142)	(0.026)
Human health and social work	2.080***	0.184***
	(0.135)	(0.018)
Arts, entertainment and recreation	4.155***	0.551***
	(0.137)	(0.024)
Other service activities	3.359***	0.403***
	(0.135)	(0.025)
Observations	743,509	743,509

Note: The table shows estimation results from a logistic regression of the baseline covariate set on the treatment indicator. It both presents non-transformed logit estimates and average marginal effects of the industry dummies. Average marginal effects are evaluated for a discrete change from its base level, which is agriculture, forestry, and fishing. Parentheses report standard errors. * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Estimation results for the occupation dummies and the remaining covariates are presented in tables 10 and 12-13 in appendix E.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 12: Estimated Effects of Occupations on the Furlough Probability (1/2)

	Logit estimates	Average marginal effects
Top management and legislators	0.944***	0.063***
	(0.183)	(0.016)
Administrative managers etc.	1.763***	0.148***
	(0.171)	(0.021)
Production and services managers	1.461***	0.114***
	(0.178)	(0.020)
Hotel, restaurant, retail managers etc.	1.914***	0.168***
	(0.170)	(0.022)
Science and engineering professionals	1.368***	0.103***
	(0.171)	(0.018)
Health professionals	2.747***	0.292***
	(0.169)	(0.028)
Teaching professionals	1.178***	0.084***
	(0.170)	(0.016)
Business and administration professionals	1.497***	0.116***
	(0.170)	(0.019)
Information technology professionals etc.	0.867***	0.056***
	(0.175)	(0.014)
Legal, social and cultural professionals	0.754***	0.048***
	(0.175)	(0.014)
Science and engineering ass. professionals	1.608***	0.128***
	(0.169)	(0.019)
Health associate professionals	2.793***	0.301***
	(0.168)	(0.028)
Business and admin. ass. professionals	1.816***	0.146***
	(0.167)	(0.019)
Legal, social and cultural ass. professionals	3.048***	0.348***
	(0.169)	(0.030)
Information technicians etc.	1.176***	0.084***
	(0.180)	(0.017)
Secretaries and office clerks	1.753***	0.143***
	(0.167)	(0.020)
Customer services clerks	2.668***	0.280***
	(0.168)	(0.028)
Numerical and material recording clerks	1.375***	0.102***
	(0.168)	(0.017)
Other clerical support workers	2.018***	0.182***
	(0.171)	(0.023)
Observations	743,509	743,509

Note: The table shows estimation results from a logistic regression of the baseline covariate set on the treatment indicator. It both presents non-transformed logit estimates and average marginal effects of the occupation dummies. Average marginal effects are evaluated for a discrete change from its base level, which is agricultural, forestry, and fishery workers. Parentheses report standard errors. * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Estimation results for the industry dummies and the remaining covariates are presented in tables 10-11 in appendix E.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 13: Estimated Effects of Occupations on the Furlough Probability (2/2)

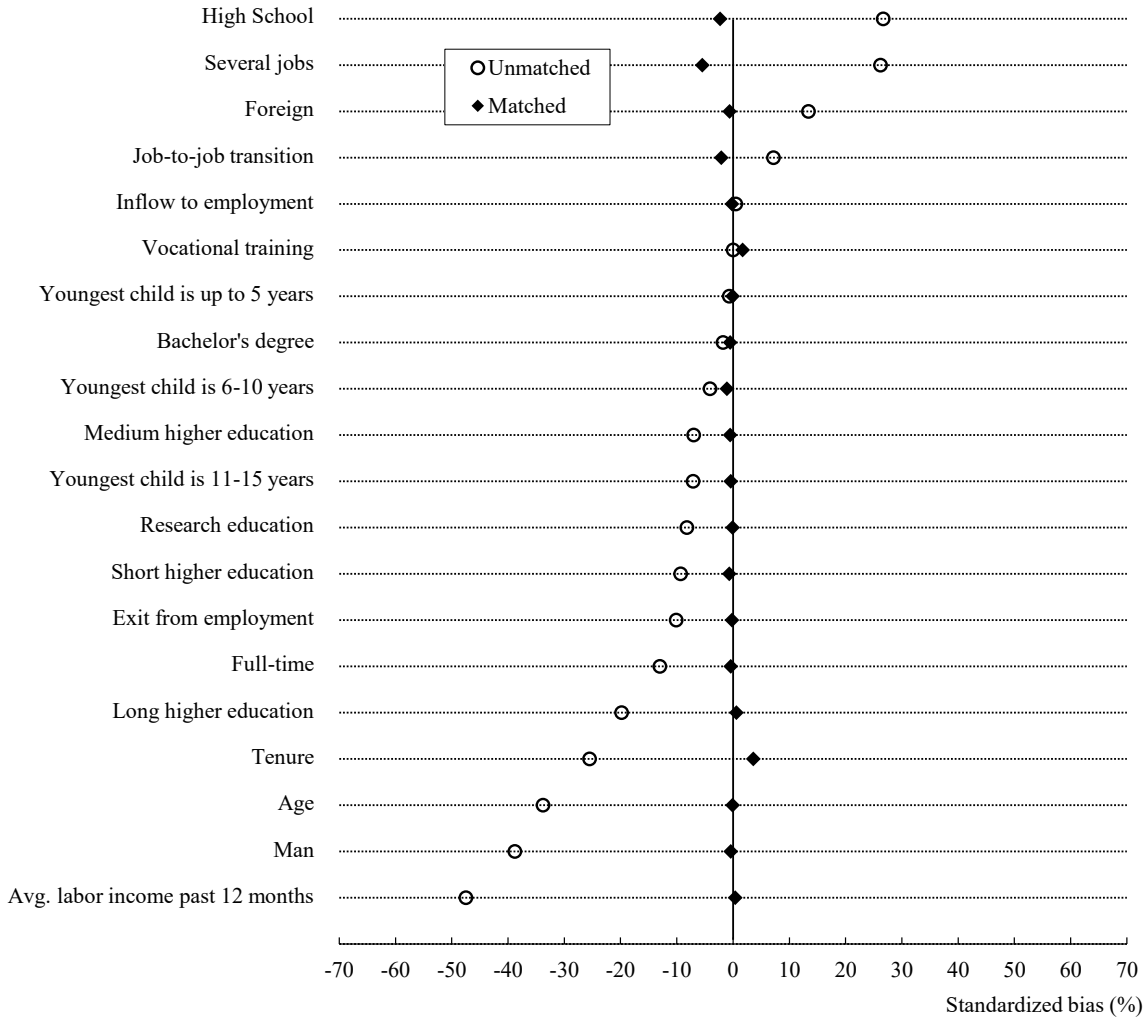
	Logit estimates	Average marginal effects
Personal service workers	3.083***	0.353***
	(0.167)	(0.029)
Sales workers	2.325***	0.212***
	(0.166)	(0.024)
Personal care workers	0.767***	0.048***
	(0.169)	(0.013)
Protective services workers	2.528***	0.259***
	(0.173)	(0.028)
Building workers and painters	1.323***	0.098***
	(0.171)	(0.017)
Metal and machinery workers	1.806***	0.149***
	(0.167)	(0.020)
Handicraft and printing workers	2.688***	0.286***
	(0.180)	(0.030)
Electrical and electronic workers	1.708***	0.142***
	(0.173)	(0.021)
Food processing workers etc.	1.537***	0.122***
	(0.173)	(0.020)
Stationary plant and machine operators	1.453***	0.111***
	(0.169)	(0.018)
Assemblers	1.903***	0.167***
	(0.175)	(0.023)
Drivers and mobile plant operators	1.377***	0.102***
	(0.169)	(0.017)
Cleaners and helpers	2.158***	0.199***
	(0.167)	(0.024)
Agricultural and forestry laborers etc.	1.356***	0.103***
	(0.277)	(0.029)
Mining and construction laborers etc.	1.694***	0.135***
	(0.167)	(0.019)
Food preparation assistants	2.168***	0.205***
	(0.170)	(0.025)
Refuse workers etc.	2.145***	0.200***
	(0.174)	(0.025)
Other occupations	1.052**	0.073*
	(0.327)	(0.030)
Observations	743,509	743,509

Note: The table shows estimation results from a logistic regression of the baseline covariate set on the treatment indicator. It both presents non-transformed logit estimates and average marginal effects of the occupation dummies. Average marginal effects are evaluated for a discrete change from its base level, which is agricultural, forestry, and fishery workers. Parentheses report standard errors. * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Estimation results for the industry dummies and the remaining covariates are presented in tables 10-11 in appendix E.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Appendix F – Standardized Biases Before and After Matching

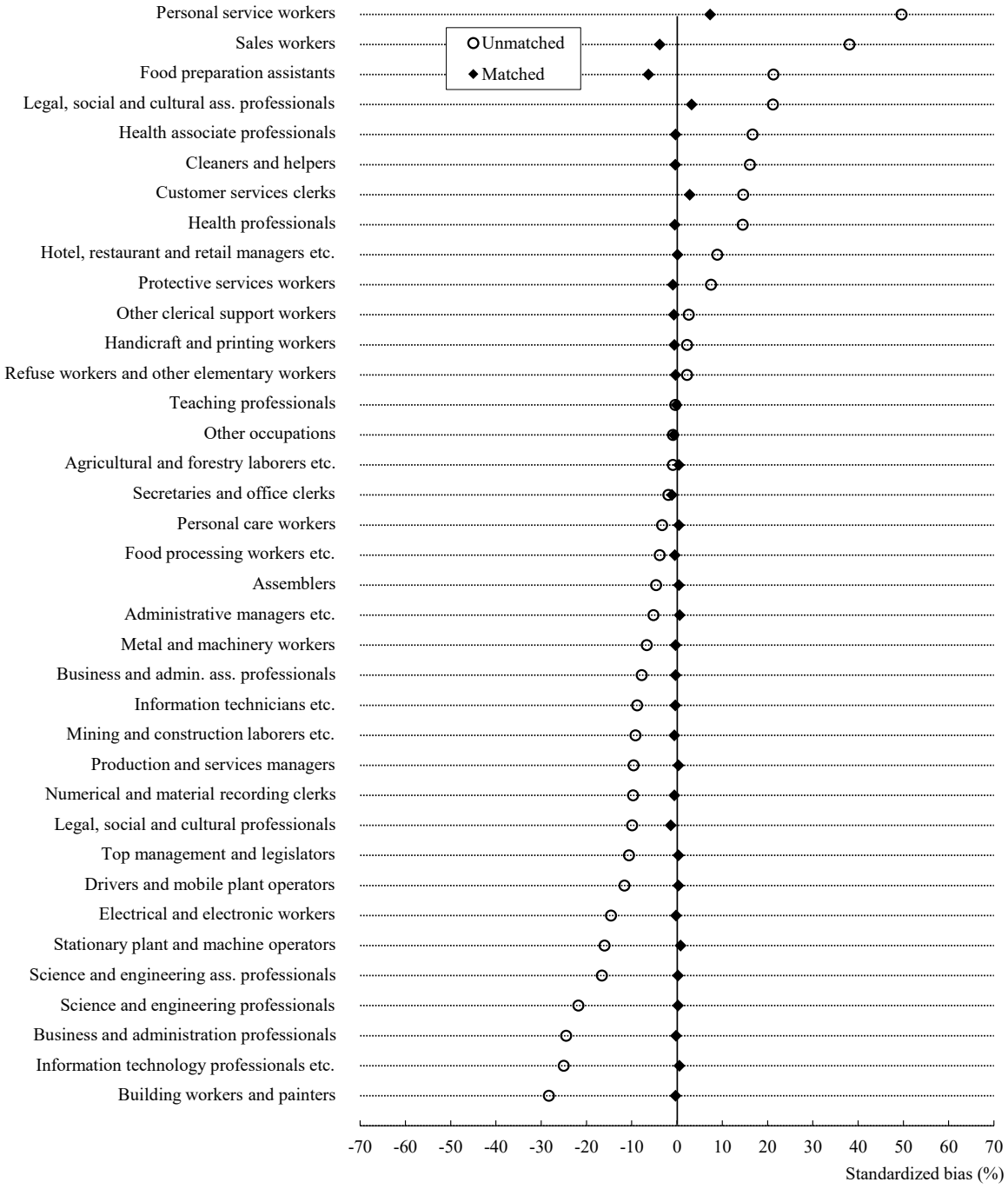
Figure 6: Standardized Biases Across Selected Covariates Before and After Matching



Note: The figure shows standardized percentage biases across covariates other than industry and occupation before and after matching. Standardized biases measure, for each covariate, the difference between the means of the treatment and control groups as a percentage of the square root of the average variance across the two groups (Rosenbaum and Rubin, 1985). A positive bias means that the value of the particular covariate is higher in the treatment group than in the control group (and vice versa). Standardized biases across industries and occupations are presented in figure 2 in section 4.1 and figure 7 in appendix F, respectively.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Figure 7: Standardized Biases Across Occupations Before and After Matching



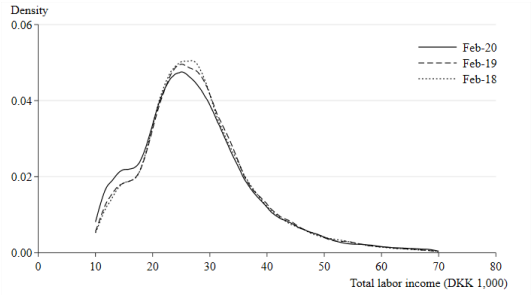
Note: The figure shows standardized percentage biases across occupations in the covariate set before and after matching. Standardized biases measure, for each covariate, the difference between the means of the treatment and control groups as a percentage of the square root of the average variance across the two groups (Rosenbaum and Rubin, 1985). A positive bias means that the value of the particular covariate is higher in the treatment group than in the control group (and vice versa). Standardized biases across industries and the remaining covariates are presented in figure 2 in section 4.1 and figure 6 in appendix F, respectively.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

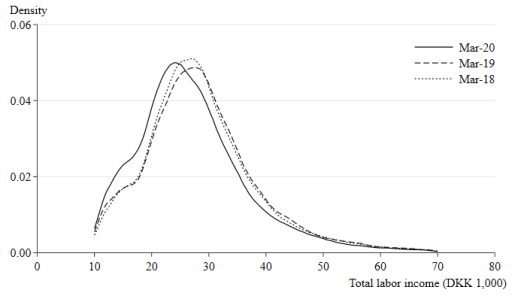
Appendix G – Labor Income Distributions for Furloughed Workers

Figure 8: Labor Income Distributions for Furloughed Workers

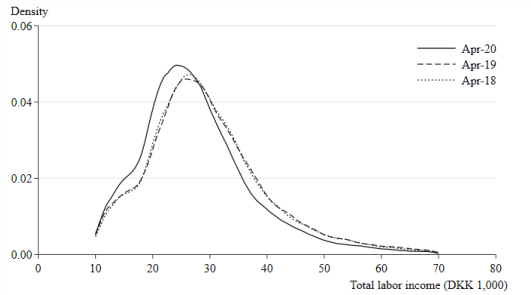
(a) February



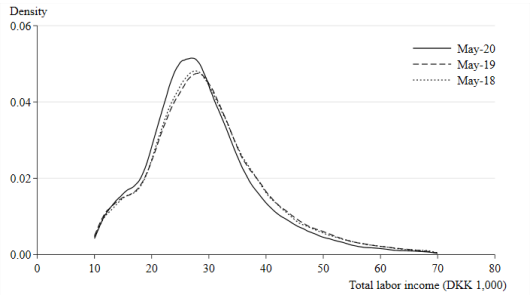
(b) March



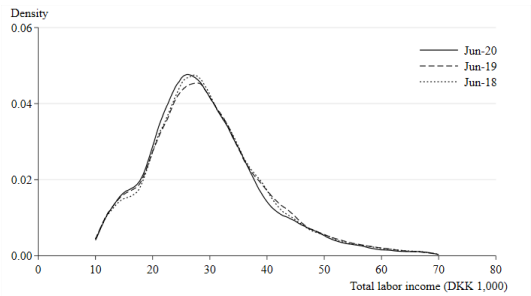
(c) April



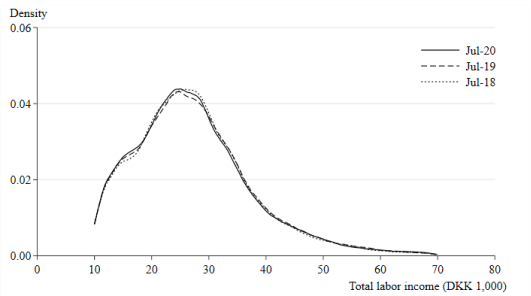
(d) May



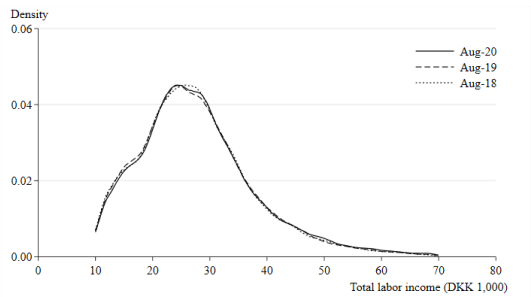
(e) June



(f) July



(g) August



Note: The figures show monthly kernel density estimates of labor income distributions for workers furloughed from March 2020 to June 2020 in the baseline sample for the years 2018-2020. The kernel used is an Epanechnikov, and the bandwidth is 1.0. Panel (8a) shows the distributions for the pre-treatment month February 2020, whereas panels (8b)-(8g) show the distributions for the post-treatment months March to August 2020.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Appendix H – Main Estimation Results

Table 14: Estimated Effects of the Scheme on the Exit Probability

	Main	Robustness		
	(1)	(2)	(3)	(4)
$D^{\text{Jan-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Feb-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Mar-19}} \times D^{\text{TREAT}}$	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Apr-19}} \times D^{\text{TREAT}}$	-0.001	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{May-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Jun-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Jul-19}} \times D^{\text{TREAT}}$	0.000	0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Aug-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Sep-19}} \times D^{\text{TREAT}}$	0.000	0.000	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Oct-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.001)
$D^{\text{Nov-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
$D^{\text{Dec-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)
$D^{\text{Jan-20}} \times D^{\text{TREAT}}$	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
$D^{\text{Mar-20}} \times D^{\text{TREAT}}$	-0.030***	-0.030***	-0.030***	-0.034***
	(0.003)	(0.003)	(0.003)	(0.003)
$D^{\text{Apr-20}} \times D^{\text{TREAT}}$	-0.010***	-0.010***	-0.011***	-0.012***
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{May-20}} \times D^{\text{TREAT}}$	-0.003***	-0.003***	-0.003***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Jun-20}} \times D^{\text{TREAT}}$	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Jul-20}} \times D^{\text{TREAT}}$	-0.000	-0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Aug-20}} \times D^{\text{TREAT}}$	0.003***	0.003***	0.002*	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Industry-month FE	Yes	Yes	Yes	Yes
Occupation-month FE	No	Yes	Yes	No
Firm Covariates	No	No	Yes	No
Observations	2,622,010	2,622,010	2,410,426	2,315,373

Note: The table shows estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having an exit from employment in the post-scheme months, March 2020 to August 2020, and some pre-scheme months, January 2019 to January 2020. Panel (1) presents the main results from the preferred matched difference-in-differences specification with industry-calendar-month fixed effects. Panels (2)-(4) each represent a robustness test. Panel (2) includes occupation-calendar-month fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Parentheses report standard errors clustered at the firm level. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 15: Heterogenous Effects on the Exit Probability Across Firm Tenure

	(1)	(2)	(3)	(4)
	$\leq$ 1 year	]1; 4] years	]4; 8] years	$>$ 8 years
$D^{\text{Jan-19}} \times D^{\text{TREAT}}$	-0.002	0.000	-0.000	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{Feb-19}} \times D^{\text{TREAT}}$	-0.002	0.001**	-0.001	0.002*
	(0.002)	(0.000)	(0.001)	(0.001)
$D^{\text{Mar-19}} \times D^{\text{TREAT}}$	-0.000	0.001	0.000	0.003*
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{Apr-19}} \times D^{\text{TREAT}}$	-0.002	0.001	-0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{May-19}} \times D^{\text{TREAT}}$	-0.001	0.000	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{Jun-19}} \times D^{\text{TREAT}}$	-0.001	-0.000	0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{Jul-19}} \times D^{\text{TREAT}}$	-0.000	-0.000	0.001*	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
$D^{\text{Aug-19}} \times D^{\text{TREAT}}$	-0.001	0.000	-0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Sep-19}} \times D^{\text{TREAT}}$	0.001	-0.001	0.000	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
$D^{\text{Oct-19}} \times D^{\text{TREAT}}$	-0.001	0.000	-0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.001)
$D^{\text{Nov-19}} \times D^{\text{TREAT}}$	-0.001	0.001	-0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)
$D^{\text{Dec-19}} \times D^{\text{TREAT}}$	-0.001	-0.000	-0.000	0.001
	(0.001)	(0.000)	(0.000)	(0.000)
$D^{\text{Jan-20}} \times D^{\text{TREAT}}$	-0.000	0.000	-0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.000)
$D^{\text{Mar-20}} \times D^{\text{TREAT}}$	-0.043***	-0.026***	-0.014***	-0.007
	(0.004)	(0.003)	(0.003)	(0.005)
$D^{\text{Apr-20}} \times D^{\text{TREAT}}$	-0.014***	-0.009***	-0.003*	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)
$D^{\text{May-20}} \times D^{\text{TREAT}}$	-0.005**	-0.003*	-0.003*	0.001
	(0.002)	(0.001)	(0.001)	(0.002)
$D^{\text{Jun-20}} \times D^{\text{TREAT}}$	0.001	-0.000	0.000	-0.000
	(0.002)	(0.001)	(0.001)	(0.002)
$D^{\text{Jul-20}} \times D^{\text{TREAT}}$	0.001	-0.002	0.000	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Aug-20}} \times D^{\text{TREAT}}$	0.003*	0.003*	0.003	0.006**
	(0.001)	(0.002)	(0.002)	(0.002)
Industry-month FE	Yes	Yes	Yes	Yes
Occupation-month FE	No	No	No	No
Firm Covariates	No	No	No	No
Observations	996,644	1,010,786	479,567	135,013

Note: The table shows the preferred matched difference-in-differences estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having an exit from employment across four subgroups of workers with different levels of *ex ante* firm tenure. Panels (1)-(4) refer to workers with (1) up to 1 year, (2)]1; 4] years, (3)]4; 8] years, and (4) more than 8 years of tenure in February 2020, respectively. Parentheses report standard errors clustered at the firm level. * p<0.05, ** p<0.01, *** p<0.001.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 16: Estimated Effects of the Scheme on Log-Labor Income

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{Jan-19}} \times D^{\text{TREAT}}$	-0.002	-0.003	-0.009	-0.005	0.001
	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)
$D^{\text{Feb-19}} \times D^{\text{TREAT}}$	0.007	0.006	0.002	0.009	0.014**
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
$D^{\text{Mar-19}} \times D^{\text{TREAT}}$	0.001	-0.002	-0.006	-0.001	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
$D^{\text{Apr-19}} \times D^{\text{TREAT}}$	-0.003	-0.004	-0.007	-0.005	-0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
$D^{\text{May-19}} \times D^{\text{TREAT}}$	-0.004	-0.005	-0.011*	-0.007	-0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{\text{Jun-19}} \times D^{\text{TREAT}}$	0.005	0.005	0.002	0.006	0.011
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
$D^{\text{Jul-19}} \times D^{\text{TREAT}}$	-0.012*	-0.012*	-0.013*	-0.015*	-0.007
	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)
$D^{\text{Aug-19}} \times D^{\text{TREAT}}$	-0.012*	-0.012*	-0.012*	-0.013*	-0.007
	(0.006)	(0.005)	(0.006)	(0.006)	(0.006)
$D^{\text{Sep-19}} \times D^{\text{TREAT}}$	-0.000	-0.001	-0.001	-0.001	0.000
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
$D^{\text{Oct-19}} \times D^{\text{TREAT}}$	-0.001	-0.001	-0.003	-0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
$D^{\text{Nov-19}} \times D^{\text{TREAT}}$	-0.004	-0.003	-0.007	-0.006	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{\text{Dec-19}} \times D^{\text{TREAT}}$	-0.012*	-0.011*	-0.013*	-0.013*	-0.013*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{\text{Jan-20}} \times D^{\text{TREAT}}$	-0.006	-0.007	-0.008*	-0.010**	-0.007
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
$D^{\text{Mar-20}} \times D^{\text{TREAT}}$	-0.034***	-0.035***	-0.032***	-0.030***	-0.045***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
$D^{\text{Apr-20}} \times D^{\text{TREAT}}$	-0.050***	-0.050***	-0.053***	-0.053***	-0.045***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
$D^{\text{May-20}} \times D^{\text{TREAT}}$	-0.027***	-0.026***	-0.024***	-0.028***	-0.017***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
$D^{\text{Jun-20}} \times D^{\text{TREAT}}$	-0.009	-0.008	-0.009	-0.013*	-0.006
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{\text{Jul-20}} \times D^{\text{TREAT}}$	-0.020***	-0.019***	-0.015**	-0.020***	-0.010*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$D^{\text{Aug-20}} \times D^{\text{TREAT}}$	-0.019***	-0.018***	-0.018***	-0.018***	-0.011*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Industry-month FE	Yes	Yes	Yes	Yes	Yes
Occupation-month FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	2,622,010	2,622,010	2,410,426	2,315,373	2,454,997

Note: The table shows estimates for the average effects of the wage compensation scheme on furloughed workers' log-labor income in the post-scheme months, March 2020 to August 2020, and some pre-scheme months, January 2019 to January 2020. Panel (1) presents the main results from the preferred matched difference-in-differences specification with industry-calendar-month fixed effects. Panels (2)-(5) each represent a robustness test. Panel (2) includes occupation-calendar-month fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Panel (5) re-matches workers on a restricted sample that only includes workers who are employed during the compensation period. Parentheses report standard errors clustered at the firm level. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

Table 17: Estimated Effects of the Scheme on the Job-to-Job Transition Probability

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{Jan-19}} \times D^{\text{TREAT}}$	0.005	0.006*	0.001	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
$D^{\text{Feb-19}} \times D^{\text{TREAT}}$	0.005*	0.006*	0.001	0.003	0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Mar-19}} \times D^{\text{TREAT}}$	0.001	0.001	-0.002	0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
$D^{\text{Apr-19}} \times D^{\text{TREAT}}$	0.002	0.002	-0.004	-0.001	-0.000
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)
$D^{\text{May-19}} \times D^{\text{TREAT}}$	0.001	0.001	-0.002	0.000	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$D^{\text{Jun-19}} \times D^{\text{TREAT}}$	0.002	0.002	-0.004	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
$D^{\text{Jul-19}} \times D^{\text{TREAT}}$	-0.004	-0.004	-0.001	-0.006	-0.004
	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)
$D^{\text{Aug-19}} \times D^{\text{TREAT}}$	0.000	0.000	-0.003	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
$D^{\text{Sep-19}} \times D^{\text{TREAT}}$	-0.001	-0.001	-0.003	-0.004*	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Oct-19}} \times D^{\text{TREAT}}$	0.002	0.002	0.000	0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
$D^{\text{Nov-19}} \times D^{\text{TREAT}}$	-0.002	-0.002	-0.003	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
$D^{\text{Dec-19}} \times D^{\text{TREAT}}$	-0.001	-0.001	-0.005*	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Jan-20}} \times D^{\text{TREAT}}$	-0.001	-0.001	-0.005	-0.003	-0.002
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
$D^{\text{Mar-20}} \times D^{\text{TREAT}}$	-0.002	-0.002	-0.008***	0.000	-0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
$D^{\text{Apr-20}} \times D^{\text{TREAT}}$	-0.006**	-0.006**	-0.010***	-0.006***	-0.006**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
$D^{\text{May-20}} \times D^{\text{TREAT}}$	-0.002	-0.002	-0.005*	-0.006***	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Jun-20}} \times D^{\text{TREAT}}$	-0.002	-0.002	-0.005**	-0.004*	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$D^{\text{Jul-20}} \times D^{\text{TREAT}}$	0.000	0.000	-0.003	-0.000	0.002
	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
$D^{\text{Aug-20}} \times D^{\text{TREAT}}$	0.001	0.001	-0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Industry-month FE	Yes	Yes	Yes	Yes	Yes
Occupation-month FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	2,622,010	2,622,010	2,410,426	2,315,373	2,454,997

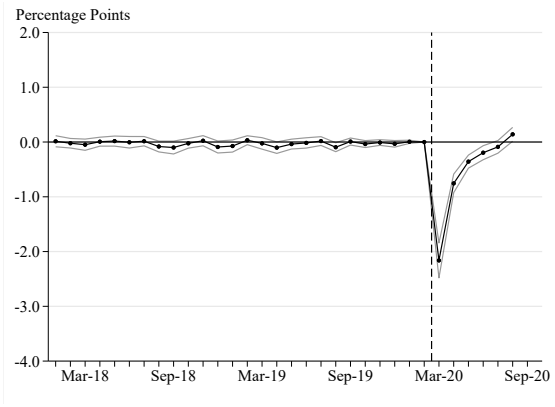
Note: The table shows estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having a job-to-job transition in the post-scheme months, March 2020 to August 2020, and some pre-scheme months, January 2019 to January 2020. Panel (1) presents the main results from the preferred matched difference-in-differences specification with industry-calendar-month fixed effects. Panels (2)-(5) each represent a robustness test. Panel (2) includes occupation-calendar-month fixed effects. Panel (3) re-matches workers on a covariate set extended with additional firm characteristics. Panel (4) excludes workers with more than one job in February 2020. Panel (5) re-matches workers on a restricted sample that only includes workers who are employed during the compensation period. Parentheses report standard errors clustered at the firm level. * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

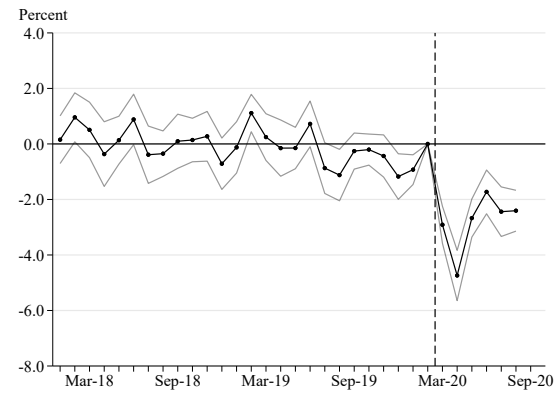
Appendix I – Estimation Results for All Furloughed Workers

Figure 9: Estimated Effects of the Scheme for All Furloughed Workers

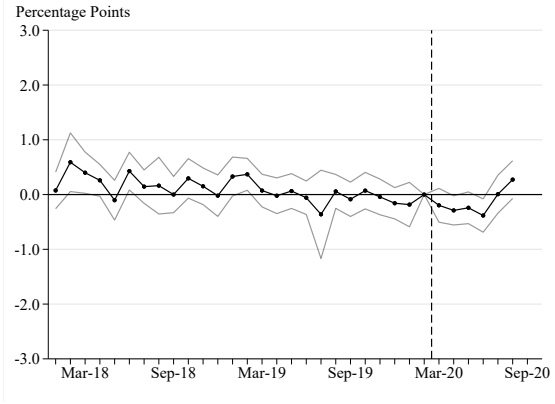
(a) Employment-exit Probability



(b) Log-labor Income



(c) Job-to-job Transition Probability



Note: The figure shows estimates for the average impact of the wage compensation scheme on the three labor market outcomes from an alternative sample selection where all furloughed workers, regardless of their compensation period, are included in the treatment group. Estimates are obtained from a regression of the preferred matched difference-in-differences specification with industry-calendar-month fixed effects. In panel (a), the outcome is the probability of having an exit from employment. In panel (b), it is total log-labor income, and in panel (c), it is the probability of having a job-to-job transition. The gray lines that encircle the estimates are 95% confidence bands based on clustered standard errors at the firm level. The dashed vertical lines mark the introduction of the wage compensation scheme.

Source: Own calculations based on Danish administrative register data from Statistics Denmark.

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