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. Usi ng payroll records at the monthly frequency for Danish employer-employee matches, I estimate that the scheme prevented 11,165 exits from employment. How- ever, 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 greater extent, have retained their jobs 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 slightly reduced by the scheme.

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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. 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. This analysis explores the scheme's effectiveness in preserving job matches and maintaining firm-specific human capital, but it also examines its impact on furloughed workers' labor income and 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 11,165 exits from employment from March to August 2020 (9,374 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 human capital measured by workers' ex ante firm tenure. I find that the Danish wage

1. The pandemic induced significant declines in vacancies (Bess, Borgensgaard, and Iuel, 2020; Forsythe et al., 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., 2022; 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 (Hijzen and Martin, 2013), but they also targeted jobs that would have been preserved anyway (Boeri and Bruecker, 2011).

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 greater extent, have retained their jobs 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 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, 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 1.6 percent, corresponding to a total loss in earnings of just over DKK 1,900 ($\approx$ USD 280) 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, but it could also reflect a reduction in salary add-ons, e.g., overtime pay and bonuses. Bertheau et al. (2022) show that Danish firms made use of both reductions in hourly base pay and salary add-ons during the pandemic.

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 only slightly reduced 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.

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

3. While the average monthly labor income of furloughed workers is DKK 29,088 in this period, the results indicate that it would have been approximately DKK 29,574 in the absence of the scheme.

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 who 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' industry and occupation, while pre-pandemic characteristics encompass both worker and firm attributes. For workers, this includes demographics, firm tenure, and education, among other factors. For firms, it includes size, mean wages, mean tenure, and employment growth indicators. Moreover, the results remain robust when adding debt ratios, liquidity per worker, value added per worker, and mean exit rates of employees to non-employment to the covariate set.

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. The results should be interpreted within the limits of this assumption. While the two groups appear to have followed parallel trends before the pandemic, the covid-19 shock may have affected them differently in ways not captured by observable characteristics. For instance, firms operating within the same industries may not be equally affected by the covid-19 shock. Nonetheless, the robustness of the results

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

lends support to the insights presented in this paper.

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 of layoffs. This suggests that the preferred estimate for the number of averted exits from employment may be 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 Institutional Framework

This section provides an overview of the Danish labor market and describes the Danish wage compensation scheme for employees. It also provides a comparison to the approach taken in other countries with a focus on the US.

2.1 The Danish Labor Market

The Danish labor market combines comprehensive active and passive labor market policies with flexible hiring and firing rules for firms. For instance, unemployment insurance benefits have a maximum duration of two years and cover up to 90 percent of previous salaries. This places Denmark among the OECD countries with the most generous unemployment insurance benefits.[5] Denmark also invests extensively in active labor market policies, including assistance for job search, retraining, etc., with compulsory participation for recipients of unemployment benefits. In fact, the country has the highest spending on active labor market programs among OECD countries. In contrast, Denmark ranks among the countries with the laxest employment protection policies, in part because severance pay is rarely used, and notice periods can be relatively short. For white-collar employees, the notice period varies between one and six months, increasing with firm tenure, while

5. A comparison of macroeconomic performance and policy parameters of Denmark and the US relative to other OECD countries can be found in table 1 in Kreiner and Svarer (2022). The paper also provides more details on the Danish labor market model.

it is typically shorter for blue-collar employees.[6]

The Danish labor market is also characterized by a high level of organization on both the employer and worker sides, with four different wage-setting systems.[7] Wage-setting typically combines collective negotiations of wage floors at the industry level with final wage-determination at the firm level. However, for some workers, wages are determined solely at either the industry or firm level.

In recent years, the Danish labor market has featured a high employment rate, a relatively low incidence of long-term unemployment, and a high level of labor market turnover, which is similar to the experience in the US (Kreiner and Svarer, 2022). The two countries also share relatively flexible employment protection policies, but they differ significantly in terms of income security. Where Denmark ranks among the OECD countries with the most generous unemployment insurance benefits, the US is on the other end of the spectrum. Additionally, Denmark invests more extensively in active labor market policies.

2.2 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.[8]

6. The notice period for white-collar employees with less than six months of employment in a firm is one month. It is three months for employment of six months to three years, four months for employment of three to six years, and five months for employment of six to nine years. It is six months for workers with more than nine years of employment in the firm. Note that the length of notice period is included when calculating firm tenure.

7. More details on the Danish wage-setting systems can be found in Dahl, Le Maire, and Munch (2013).

8. 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.

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.[9]

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.

Just like Denmark and many other countries, the US introduced programs intended to limit job losses during the covid-19 pandemic. Among these was the US Paycheck Protection Program (PPP), which provided forgivable loans to firms with up to 500 employees (Chetty et al., 2020). To be eligible for full loan forgiveness, firms had to use at least 60 percent of the loan on payroll costs and maintain employee and compensation levels. If firms instead laid off workers during the pandemic and did not try to rehire them after the compensation period, then the forgivable amount of the loan was reduced.

There are several differences between the US Paycheck Protection Program and the Danish wage compensation scheme for employees. While the Danish scheme offered wage subsidies of up to 90 percent of salaries, the US scheme was a loan program offering full forgiveness. Moreover, the Danish scheme focused exclusively on job retention, whereas the US program provided broader financial support to firms.

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.[10] Labor

9. 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.

10. 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.

market outcomes are obtained from the register of wage earner employment, which contains monthly information on wages, hours worked, 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 tables 6 and 7 in appendix C.

The baseline sample covers the period from 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. Additionally, it is confined to corporations under domestic control. 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 meet these criteria for at least 12 months over the pre-pandemic period. Additionally, the focus is on workers employed in February 2020 in industries and occupations with at least ten furloughed workers. 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.[11] For the remainder of the paper, workers in the treatment and control groups are simply referred 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.[12] The main issue in this context is that firms'

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

12. The wage compensation register does not contain information on these workers, and the group of non-furloughed workers who received salaries from wage compensated firms *prior to* the scheme also includes

layoff and furlough decisions may not be perfectly related. Firms may decide to furlough highly valued workers who are temporarily unproductive due to the lockdown rather than less valued workers who would otherwise have been laid off. 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 were employed by wage compensated firms in February 2020.

This leaves an unmatched sample of 730,001 workers, of which 47,752 were furloughed from March to June 2020. After matching, there are 82,952 workers in the sample. Among these, 47,752 are furloughed (treated), and 35,200 are non-compensated (controls). Tables 8-10 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, three years younger than non-compensated workers, have one year less tenure, and earn around DKK 6,800 less labor income each month over the past 12 months of employment. 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. Furloughed workers also tend to be employed in smaller and lower-paying firms with a less tenured workforce compared to non-compensated workers. 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 worker is almost 41 years old, has around four years of tenure, and has an average monthly labor income of around DKK 29,000 over the past 12 months of employment. Moreover, 57 percent are full-time employed, 49 percent are men, and 14 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

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.

wholesale and retail trade, where 35 percent of workers are employed, and accommodation and food service, where 17 percent of workers are employed. The two most represented occupations are personal services and sales, with worker shares of 14 and 15 percent, respectively. Furthermore, the average worker is employed in a firm with an employee count of 463, mean earnings amounting to just over DKK 24,500, and an average firm tenure of almost four years. Most work in firms with a stable average employment growth from 2016 and 2019, and fewest work in high-growth firms.

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.[13] The idea is to compare the growth rate of several labor market outcomes from before to after the introduction of the scheme for furloughed and non-compensated workers who 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. Whereas the Danish register data enables matching on a broad set of worker and firm characteristics, the results should be interpreted within the limits of this assumption.

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 probability 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

13. Other studies using a similar approach include Girma and Görg (2007), Aerts and Schmidt (2008), 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).

the difference in propensity scores.[14] 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 $j = 32$ 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. Three 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 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

14. 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.

face-to-face interactions (Adams-Prassl et al., 2020; Garrote Sanchez et al., 2020; Montenovo et al., 2022). Second, it is possible that firms select into the scheme based on their pre-pandemic performance. To account for this, I include 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 with unobserved growth rates, there is a dummy indicating that the firm was established after 2016 or had no employees at the end of that year. Further, I include measures of firm size, average monthly wages, and mean firm tenure of employees in the covariate set. Third, 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 for workers are included in the covariate set. All covariates are measured in the most recently observed month prior to the introduction of the scheme.[15]

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. Moreover, the covariate set does not include any firm-level measures of exposure to the covid-19 shock, as no ex ante measure is observed. 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 suggests that the estimated number of exits from employment averted by the scheme may be a lower bound for the true effect.

15. Tables 6 and 7 in appendix C present the covariate set, which includes measures of demographics (age, gender, citizenship, and children by age intervals in Dec-19), human capital (educational attainment in Sep-19 and firm tenure in Feb-20), firm characteristics (industry, size, mean earnings, and mean firm tenure in Feb-20, and indicators for employment growth in 2016-2019), and labor market attachment (occupation, full-time, inflow to employment, exit from employment, job-to-job transition, several jobs, and average monthly labor income over the past 12 months of employment in Feb-20.)

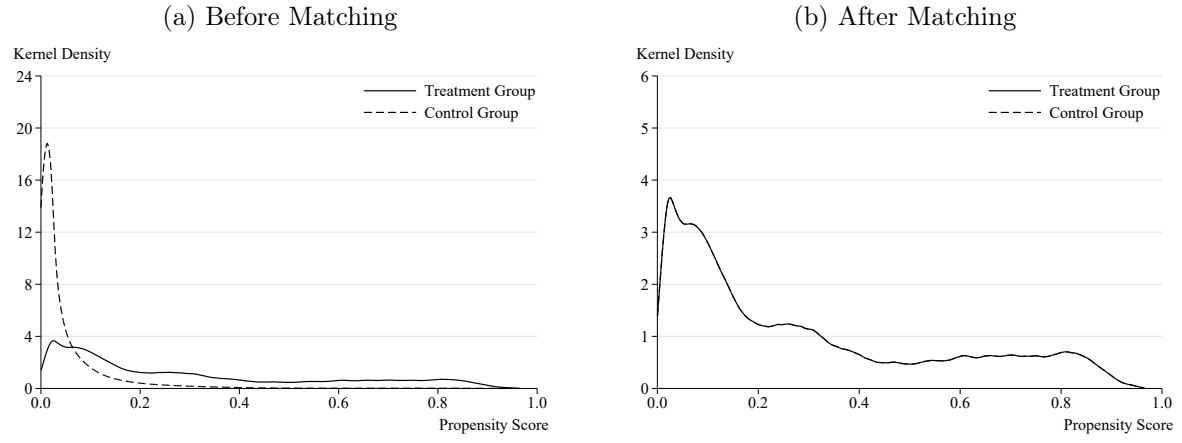
4.1 Assessing the Quality of Matches

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 11-14 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. Full-time employees and employees with several jobs are more likely to be furloughed, while higher-earning employees are less likely to be furloughed. Moreover, the probability of being furloughed is predominantly decreasing with both firm tenure and educational attainment higher than primary school. It is also decreasing with age, while having young children does not appear to be an important determinant of whether an employee is furloughed. Generally, men are less likely to be furloughed, while foreign citizens are more likely to be furloughed. Moreover, employees are less likely to be furloughed if they are employed in large, high-paying firms, where workers have more firm-specific experience on average. Finally, the furlough probability appears to be influenced by the average employment growth experienced by firms before the pandemic.

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 15.1 percent before matching to 1.2 percent after matching.

Figure 1: Kernel Density Estimates of Propensity Scores



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.

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. Afterward, it is examined whether the scheme affected furloughed workers' labor income and labor market mobility.

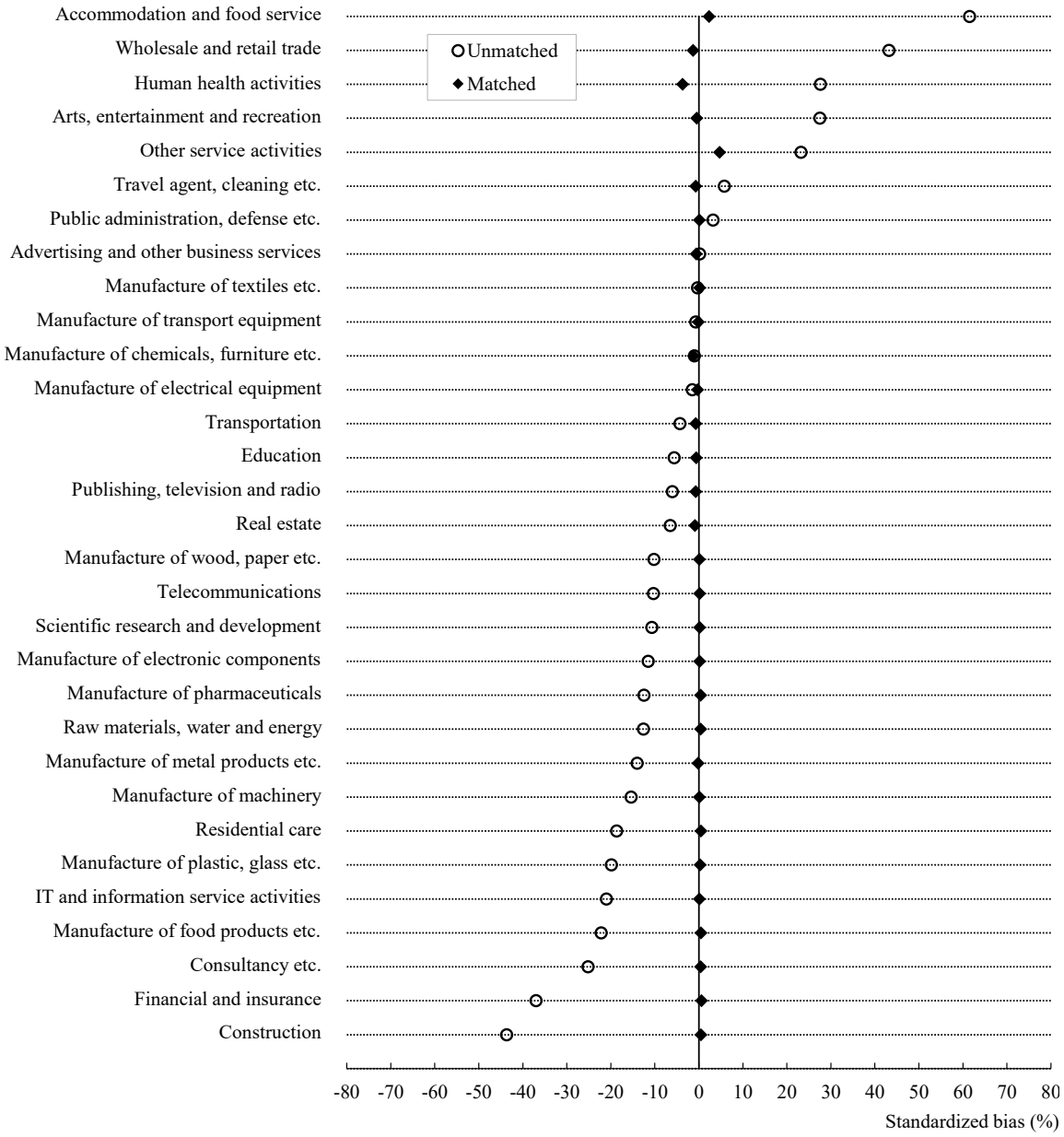
5.1 Prevention of Job Losses

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.[16]

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 almost 3.2 percent, implying that many non-compensated workers lost their jobs. In the subsequent months,

16. 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.

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.

job losses are less pronounced, albeit the monthly exit shares are still above the level seen prior to the pandemic.[17] Among the treated workers, the exit share remains slightly above zero in March 2020, reflecting that furloughed workers' jobs are being preserved.

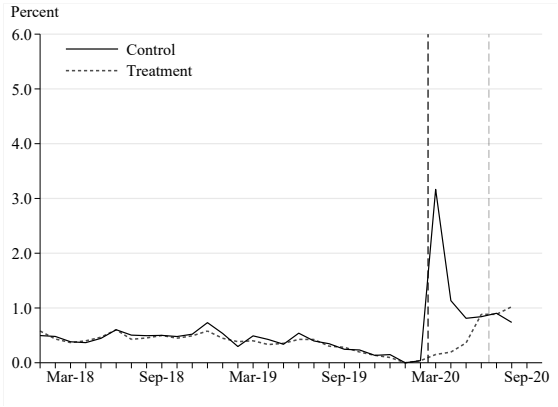
17. This either reflects that non-compensated firms enforce all their layoffs on impact or that fewer layoffs are needed, as the gradual reopening of the Danish economy improves economic conditions.

For the remainder of the compensation period (until June), the exit share rises slowly.[18]

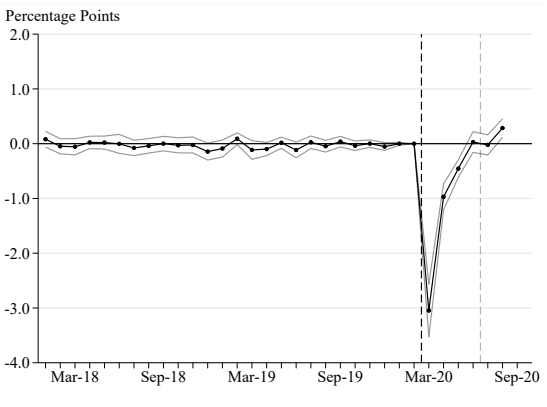
Figure 3b shows estimates for the average effects of the wage compensation scheme on furloughed workers' probability of having an exit from employment, with corresponding 95% confidence bands. In the months preceding the introduction of the scheme, all estimates are insignificant, supporting the parallel trends assumption. After the introduction of the scheme in March 2020, the exit probability for furloughed workers declines markedly *relative* to that for similar non-compensated workers in the control group. This suggests that the scheme prevented job losses, which is consistent with the findings of Bennedsen et al. (2020).

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

(a) Monthly Employment-exit Shares



(b) Estimated Treatment Effects



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.

However, in August 2020, when the scheme ended, there appears to be a minor reversal in exit rates of furloughed and non-compensated workers. This could indicate that layoffs or quits were simply postponed, though this estimate should be interpreted with caution. It is beyond the scope of this paper to analyze the longer-run effects of the scheme, as it was followed by other relief measures that could affect the two groups differently. For instance, at the end of August, the existing short-time work scheme was expanded, and in December 2020, the wage compensation scheme was re-introduced.

18. 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.

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 4.1 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 1.5 percent, it would have been approximately 5.6 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 11,165 exits from employment during the first wave of the pandemic (9,374 exits at the lower bound of the 95% confidence interval). However, it also compensated workers that firms were not planning to lay off.

A back-of-the-envelope calculation indicates that the average expenditure per saved job match was approximately DKK 287,100 (≈ USD 41,200) per month. This suggests that saving a job match under the Danish wage compensation scheme implied a net extra cost to the Danish government of around DKK 268,400 (≈ USD 38,500) per month net of taxes relative to paying unemployment insurance, see appendix G for computational details.

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

	Main	Robustness		
	(1)	(2)	(3)	(4)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.041***	-0.045***	-0.044***	-0.043***
	(0.003)	(0.004)	(0.004)	(0.004)
D^{POST}	0.043***	0.051***	0.046***	0.048***
	(0.002)	(0.002)	(0.004)	(0.002)
D^{TREAT}	-0.000	-0.001	-0.001*	-0.000
	(0.000)	(0.001)	(0.001)	(0.000)
Industry-time FE	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No
Firm Covariates	No	No	Yes	No
Observations	189,895	189,895	127,823	165,965

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 represents 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.[19] 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 5.6 percentage points in the compensation period. For workers with $]1;4]$ years of tenure, the estimated fall is 3.9 percentage points. For workers with $]4;8]$ years of tenure, it is 2.1 percentage points, and for workers with more than eight years of tenure, it is 1.3 percentage points.[20] A caveat is that high-tenured workers may have longer resignation periods than low-tenured workers, meaning that they may be more likely to have an unobserved layoff because wage payments continue after the sample ends.

Nonetheless, the results indicate that the wage compensation scheme mainly prevented job losses for low-tenured workers (≤ 1 year), while furloughed workers with many years of firm tenure (8+ years) would, to a greater extent, have retained their jobs 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, 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.

19. 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.

20. For all four subgroups, the exit shares of furloughed and non-compensated workers appear to follow parallel trends in the pre-scheme period.

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
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.056***	-0.039***	-0.021***	-0.013*
	(0.005)	(0.004)	(0.004)	(0.007)
D^{POST}	0.055***	0.038***	0.038***	0.016***
	(0.002)	(0.002)	(0.002)	(0.004)
D^{TREAT}	-0.000	-0.001	0.000	-0.001
	(0.001)	(0.001)	(0.000)	(0.001)
Industry-time FE	Yes	Yes	Yes	Yes
Occupation-time FE	No	No	No	No
Firm Covariates	No	No	No	No
Observations	69,321	76,967	34,482	9,125

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.

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 but including salary add-ons, e.g., overtime pay and bonuses. In what follows, the estimator will be comparing changes in log-labor for workers in the treatment and control groups, thus abstracting from workers who were laid off during the pandemic. This means that estimates could reflect differences in wages between dismissed and preserved workers rather than effects of the scheme. Importantly, however, selection bias of this kind does not seem to drive the results, see section 5.5.[21]

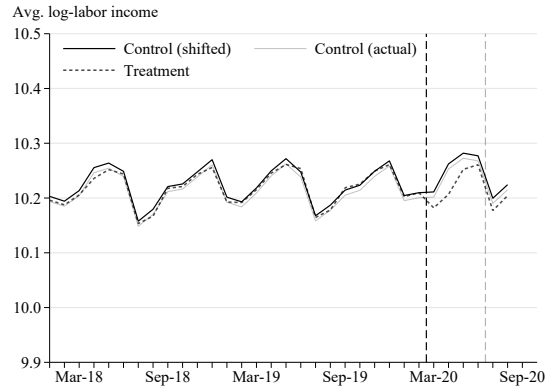
Figure 4a shows the average monthly log-labor incomes for the treatment and control groups. The solid black line is the outcome for the control group shifted upwards from its actual position (the gray line) such that the share in February 2020 equals the one for the treatment group. This graph allows 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. 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

21. The results do not change significantly when re-matching workers on a restricted sample, only including those who are employed throughout the compensation period.

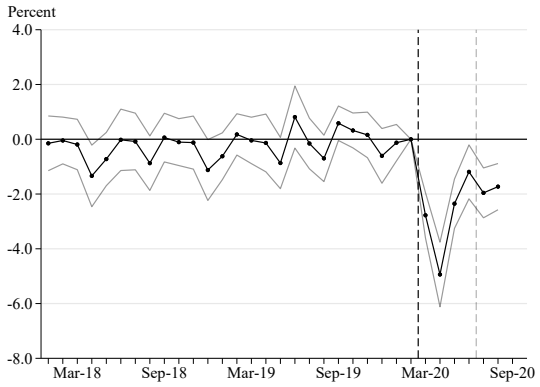
affected by the pandemic, this indicates that furloughed workers' labor incomes fall in response to the scheme.[22]

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. In the pre-scheme period, almost all estimates are insignificant, supporting the parallel trends assumption. From March 2020 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, but it could also reflect a reduction in salary add-ons. For many firms, wage compensation was computed based on workers' previous history of labor income from the firm, including add-ons such as overtime pay. However, the firm could correct the salary in the application if it documented that previous wage payments did not reflect the agreed salary, see section 2.2. Bertheau et al. (2022) show that Danish firms made use of both reductions in base pay and overtime pay during the pandemic.

Table 3 panel (1) reports the preferred difference-in-differences estimate of the change

22. 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 H.

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 1.6 percent in the compensation period from March to June 2020. While the average monthly labor income for furloughed workers is approximately DKK 29,100 in this period, it would have been almost DKK 29,600 in the absence of the scheme. This corresponds to a total loss in earnings of more than DKK 1,900 ($\approx$ USD 280) over the compensation period.

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

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.016***	-0.014**	-0.015**	-0.019***	-0.027***
	(0.004)	(0.004)	(0.006)	(0.004)	(0.003)
D^{POST}	-0.310***	-0.375***	-0.440***	-0.296***	-0.002
	(0.043)	(0.053)	(0.057)	(0.044)	(0.019)
D^{TREAT}	0.010	0.013*	0.017*	0.010	0.004
	(0.007)	(0.006)	(0.008)	(0.007)	(0.007)
Industry-time FE	Yes	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	189,895	189,895	127,823	165,965	174,952

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 represents 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

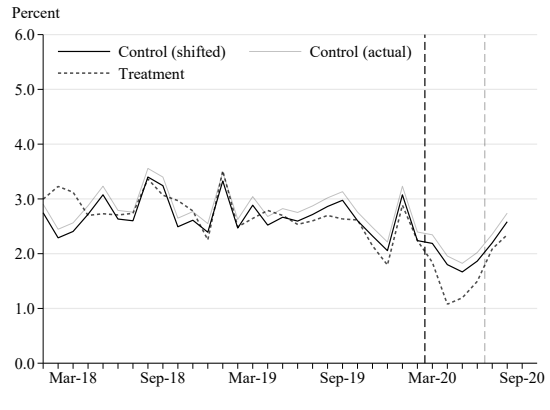
The final step in the empirical analysis is to examine to what extent the wage compensation scheme affected labor market mobility. As a measure of labor market mobility, I use an indicator for having a job-to-job transition, where a job-to-job transition is defined as a change in firm identifiers of primary workplaces between the current and the previous month.[23]

23. 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 incorrectly counted as a job-to-job transition. However, it does not change the results to only count transitions where the establishment number also changes between the current and the previous month. In contrast to CVR numbers, establishment numbers are consistent across time when firms change ownership.

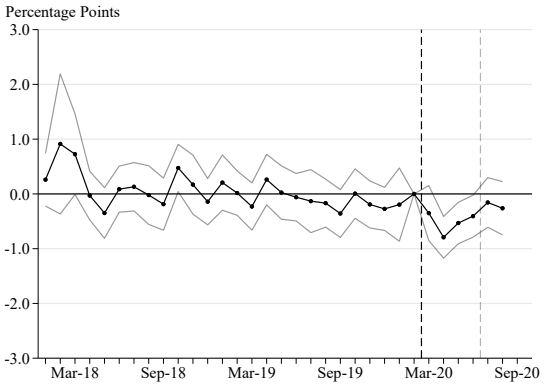
Figure 5a shows the monthly shares of workers that have a job-to-job transition for the treatment and control groups. The solid black line is the outcome for the control group shifted downwards from its actual position (the gray line) such that the share in February 2020 equals the one for the treatment 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 a couple of months. This may reflect that vacancies fell markedly during this period, so that neither furloughed nor non-compensated workers had strong incentives to apply for new jobs.[24] More interestingly, from March 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 ef-

24. 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.

fects of the wage compensation scheme on furloughed workers' probability of having a job-to-job transition, with corresponding 95% confidence bands. In the pre-scheme period, 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. In particular, table 4 panel (1) 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 -1 percentage points. This suggests that labor market mobility was slightly reduced by the wage compensation scheme.

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

	Main	Robustness			
	(1)	(2)	(3)	(4)	(5)
$D^{\text{POST}} \times D^{\text{TREAT}}$	-0.010*	-0.009*	-0.011**	-0.008	-0.012*
	(0.005)	(0.004)	(0.004)	(0.004)	(0.005)
D^{POST}	0.085***	0.094***	0.061	0.033	0.001
	(0.019)	(0.025)	(0.052)	(0.023)	(0.026)
D^{TREAT}	-0.009*	-0.012***	-0.010**	-0.005	-0.006
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Industry-time FE	Yes	Yes	Yes	Yes	Yes
Occupation-time FE	No	Yes	Yes	No	No
Firm Covariates	No	No	Yes	No	No
Observations	189,895	189,895	127,823	165,965	174,952

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 represents 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 108 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 similar to those of the preferred specification.

Second, firms may select into the scheme based on their financial situation before the pandemic. It is possible that firms with high ex ante debt ratios or limited liquidity have a lower take-up rate because they are not able to pay the part of wages that are not subsidized by the government. In particular, Friedrich and Zator (2023) show the importance of financial leverage in shaping how firms adjust employment when a demand shock occurs. To account for this, I extend the covariate set with measures of firms' debt ratio, value added per worker, and liquid assets per worker in 2019, as well as revenue growth from 2018 to 2019. Further, I include a measure of the average monthly share of employees who leave the firm for unemployment or non-participation over 2019. Panel (3) shows that re-matching workers on this extended covariate set does not change the main results significantly. 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 occupations. 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 who are not. If those who 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 who 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 the labor income of furloughed workers, and reduced labor market mobility slightly.

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 greater extent, have retained their jobs 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 only slightly reduced by the scheme.

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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 (International Monetary Fund) (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 Variables

Table 6: Description of Variables (1/2)

Variable	Description	Register
Outcome variables		
Exit from employment	An indicator that equals one if a worker has an exit from employment, and zero otherwise. An exit is defined as receiving wages in the current but not in the subsequent month (AJO_SMALT_LOENBELOEB).	BFL
Log-labor income	A relatively narrow measure of labor income is used, excluding mandatory pension contributions and fringe benefits but including salary add-ons, e.g., overtime pay and bonuses (AJO_SMALT_LOENBELOEB).	BFL
Job-to-job transition	An indicator that equals one if a worker has a job-to-job transition, and zero otherwise. A job-to-job transition is defined as a change in firm identifiers of primary workplaces between the current and the previous month (AJO_CVR_NR_FRA_PROD_JOB). This measure does not allow for any periods of unemployment or non-participation between the previous and the new job, i.e., months without wage payments.	BFL
Firm characteristics		
Size	Number of employees in February 2020 (AJO_SMALT_LOENBELOEB).	BFL
Employment growth indicators	Indicators describing whether a firm had negative, stable (up to 10 percent), positive (10-20 percent), or highly positive (more than 20 percent) average employment growth from December 2016 to December 2019. For firms with unobserved growth rates, there is a dummy indicating that the firm was established after 2016 or had no employees at the end of that year.	BFL
Exit rate	Average monthly share of a firm's employees who leave the firm for unemployment or non-participation over 2019. An exit is defined similarly to the outcome variable.	BFL
Mean earnings	Average earnings of employees in February 2020, with earnings being defined similarly to the outcome variable for labor income.	BFL
Mean firm tenure	Months of employment in the firm for the average employee in February 2020, censored above a threshold of 12 years and 2 months (AJO_CVR_NR_FRA_PROD_JOB).	BFL
Revenue growth	Percentage change in revenue from 2018 to 2019 (OMS).	FIRE
Value added per worker	Sales and other operating revenue minus intermediate costs in 2019 (XVT) over firm size in February 2020. Intermediate costs include purchases of energy, materials, subcontracted services, and goods for resale, as well as expenses for rent, leasing, temporary staffing agencies, etc.	FIRE
Liquid assets per worker	The monetary value of cash, receivables, bonds, and shares in 2019 (AT-ATT-UVBT) over firm size in February 2020.	FIRE
Debt ratio	Liabilities over assets in 2019 ((PAST-EGUL)/AT).	FIRE
Industry indicators	Indicators describing the industry of the firm in February 2020 (AJO_BRANCHE07). I distinguish between 32 industries that are defined according to the NACE 36-grouping classification, except for two differences. First, "Mining and quarrying", "Electricity, gas, steam etc.", and "Water supply, sewerage etc." are combined into "Raw materials, water and energy". Second, "Manufacture of chemicals" and "Manufacture of furniture etc." are combined into "Manufacture of chemicals, furniture etc." For a description of each industry, see Statistics Denmark.	BFL

Note: This table describes the variables used in this analysis. It distinguishes between outcome variables and firm- and worker characteristics in the covariate set.

Table 7: Description of Variables (2/2)

Variable	Description	Register
Worker characteristics		
Age	Age in years on December 31, 2019 (ALDER).	BEF
Man	An indicator that equals one if a worker is male, and zero otherwise (KOEN).	BEF
Foreign	An indicator that equals one if a worker has non-Danish or unobserved citizenship on December 31, 2019, and zero otherwise (STATSB).	BEF
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 December 31, 2019, respectively, and equal to zero otherwise (MOR_ID, FAR_ID, ALDER).	BEF
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., Ph.D.), respectively, in September 2019, and equal to zero otherwise (HFAUDD). They act as measures of general human capital.	UDDA
Firm tenure	Months of primary employment in the firm in February 2020, censored above a threshold of 12 years and 2 months, as the data is only observed from 2008 (AJO_CVR_NR_FRA_PROD_JOB). It acts as a measure of firm-specific human capital.	BFL
Full-time	An indicator that equals one if an employee worked at least 160 hours (full time) in February 2020, and zero otherwise (AJO_LOENTIMER). 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.	BFL
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.	BFL
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 similarly to the outcome variable.	BFL
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 (AJO_CVR_NR_FRA_PROD_JOB).	BFL
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 similarly to the outcome variable.	BFL
Avg. monthly labor income	Average monthly labor income over the past 12 months of employment in February 2020, with labor income being defined similarly to the outcome variable. The average is computed over months when individuals meet the sample restrictions.	BFL
Occupation indicators	Indicators describing a worker's occupation in February 2020 (VMO_DISCO_KODE). In the matching procedure, I distinguish between 38 occupations defined according to the 2-digit DISCO-08 classification. In the robustness analysis with occupation-time fixed effects, I distinguish between 108 occupations defined according to the 3-digit DISCO-08 classification, but where one of them ("other occupations") includes all occupational codes that are shared by less than ten furloughed in February 2020. For a description of the DISCO-08 classification, see Statistics Denmark.	BFL

Note: This table describes the variables used in this analysis. It distinguishes between outcome variables and firm- and worker characteristics in the covariate set.

Appendix D – Descriptive Statistics

Table 8: Descriptive Statistics Across Selected Covariates

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Worker characteristics						
Age (Years)	43.511 (11.395)	40.390 (11.721)	43.730 (11.339)	40.610 (11.736)	40.390 (11.721)	40.830 (11.748)
Man	0.653 (0.476)	0.493 (0.500)	0.664 (0.472)	0.492 (0.500)	0.493 (0.500)	0.491 (0.500)
Foreign	0.087 (0.282)	0.138 (0.345)	0.084 (0.277)	0.137 (0.344)	0.138 (0.345)	0.136 (0.342)
Youngest child is ≤ 5 years	0.190 (0.392)	0.184 (0.388)	0.190 (0.392)	0.182 (0.386)	0.184 (0.388)	0.180 (0.384)
Youngest child is 6-10 years	0.109 (0.312)	0.098 (0.298)	0.110 (0.313)	0.099 (0.299)	0.098 (0.298)	0.100 (0.300)
Youngest child is 11-15 years	0.105 (0.307)	0.088 (0.284)	0.107 (0.309)	0.089 (0.285)	0.088 (0.284)	0.091 (0.287)
High school	0.063 (0.243)	0.113 (0.317)	0.060 (0.237)	0.114 (0.318)	0.113 (0.317)	0.115 (0.319)
Vocational training	0.441 (0.496)	0.471 (0.499)	0.439 (0.496)	0.466 (0.499)	0.471 (0.499)	0.461 (0.498)
Short higher education	0.082 (0.275)	0.066 (0.248)	0.083 (0.277)	0.066 (0.247)	0.066 (0.248)	0.065 (0.247)
Medium higher education	0.122 (0.327)	0.108 (0.310)	0.123 (0.328)	0.111 (0.314)	0.108 (0.310)	0.113 (0.317)
Bachelor's degree	0.019 (0.136)	0.018 (0.132)	0.019 (0.136)	0.018 (0.132)	0.018 (0.132)	0.017 (0.131)
Long higher education	0.120 (0.325)	0.063 (0.244)	0.124 (0.329)	0.063 (0.242)	0.063 (0.244)	0.062 (0.241)
Research education	0.008 (0.089)	0.002 (0.049)	0.008 (0.091)	0.003 (0.050)	0.002 (0.049)	0.003 (0.051)
Full-time	0.628 (0.483)	0.576 (0.494)	0.631 (0.482)	0.574 (0.495)	0.576 (0.494)	0.571 (0.495)
Inflow to employment	0.005 (0.067)	0.005 (0.071)	0.004 (0.067)	0.005 (0.073)	0.005 (0.071)	0.006 (0.076)
Exit from employment	0.005 (0.068)	0.000 (0.019)	0.005 (0.071)	0.000 (0.019)	0.000 (0.019)	0.000 (0.019)
Several jobs	0.060 (0.238)	0.127 (0.333)	0.056 (0.229)	0.135 (0.342)	0.127 (0.333)	0.143 (0.350)
Job-to-job transition	0.014 (0.119)	0.022 (0.148)	0.014 (0.117)	0.023 (0.150)	0.022 (0.148)	0.024 (0.153)
Tenure (Years)	5.156 (4.212)	4.246 (3.892)	5.219 (4.226)	4.215 (3.875)	4.246 (3.892)	4.185 (3.857)
Avg. monthly labor income (DKK 1,000)	35.471 (10.897)	29.131 (8.817)	35.915 (10.890)	29.038 (8.910)	29.131 (8.817)	28.946 (9.000)
Firm characteristics						
Size (1,000 persons)	1.320 (4.658)	0.510 (1.579)	1.377 (4.795)	0.463 (1.567)	0.510 (1.579)	0.415 (1.554)
Negative empl. growth	0.286 (0.452)	0.264 (0.441)	0.287 (0.452)	0.262 (0.439)	0.264 (0.441)	0.259 (0.438)
Stable empl. growth	0.445 (0.497)	0.434 (0.496)	0.446 (0.497)	0.427 (0.495)	0.434 (0.496)	0.420 (0.494)
Positive empl. growth	0.118 (0.322)	0.108 (0.310)	0.118 (0.323)	0.110 (0.313)	0.108 (0.310)	0.112 (0.316)
Highly positive empl. growth	0.085 (0.279)	0.089 (0.285)	0.085 (0.278)	0.090 (0.287)	0.089 (0.285)	0.091 (0.288)
Mean earnings	33.793 (14.836)	24.687 (10.063)	34.430 (14.907)	24.542 (10.756)	24.687 (10.063)	24.398 (11.406)
Mean tenure in firm	4.750 (2.095)	3.936 (2.120)	4.807 (2.081)	3.924 (2.161)	3.936 (2.120)	3.911 (2.202)
Observations	730,001	47,752	682,249	95,504	47,752	47,752

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. For the seven human capital indicators, it is primary school, and for the four employment growth indicators, it is being employed in a firm that was established after 2016 or had no employees at the end of that year. Parentheses report standard deviations.

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

Table 9: Distribution of Workers Across Industries in February 2020

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Raw materials, water and energy	0.012	0.002	0.013	0.002	0.002	0.002
	(0.109)	(0.045)	(0.112)	(0.043)	(0.045)	(0.041)
Construction	0.134	0.024	0.141	0.023	0.024	0.022
	(0.340)	(0.153)	(0.348)	(0.150)	(0.153)	(0.148)
Wholesale and retail trade	0.175	0.347	0.163	0.349	0.347	0.352
	(0.380)	(0.476)	(0.369)	(0.477)	(0.476)	(0.478)
Transportation	0.057	0.048	0.058	0.049	0.048	0.050
	(0.232)	(0.215)	(0.234)	(0.216)	(0.215)	(0.218)
Accommodation and food service	0.016	0.173	0.005	0.170	0.173	0.167
	(0.127)	(0.378)	(0.073)	(0.375)	(0.378)	(0.373)
Financial and insurance	0.074	0.006	0.079	0.005	0.006	0.005
	(0.262)	(0.076)	(0.270)	(0.072)	(0.076)	(0.067)
Real estate	0.022	0.014	0.023	0.015	0.014	0.015
	(0.148)	(0.118)	(0.150)	(0.121)	(0.118)	(0.123)
Travel agents, cleaning etc.	0.048	0.060	0.047	0.061	0.060	0.062
	(0.214)	(0.238)	(0.212)	(0.239)	(0.238)	(0.241)
Public administration, defense etc.	0.002	0.003	0.002	0.003	0.003	0.003
	(0.044)	(0.059)	(0.042)	(0.059)	(0.059)	(0.059)
Education	0.027	0.019	0.028	0.019	0.019	0.020
	(0.162)	(0.137)	(0.164)	(0.138)	(0.137)	(0.140)
Arts, entertainment and recreation	0.008	0.050	0.005	0.050	0.050	0.050
	(0.089)	(0.217)	(0.072)	(0.218)	(0.217)	(0.219)
Other service activities	0.017	0.057	0.014	0.053	0.057	0.048
	(0.130)	(0.232)	(0.119)	(0.223)	(0.232)	(0.214)
Manufacture of food products etc.	0.044	0.010	0.047	0.010	0.010	0.009
	(0.206)	(0.100)	(0.211)	(0.097)	(0.100)	(0.095)
Manufacture of textiles etc.	0.002	0.002	0.002	0.002	0.002	0.002
	(0.049)	(0.048)	(0.049)	(0.048)	(0.048)	(0.047)
Manufacture of wood, paper etc.	0.016	0.006	0.017	0.006	0.006	0.006
	(0.125)	(0.076)	(0.127)	(0.076)	(0.076)	(0.076)
Manufacture of chemicals, furniture etc.	0.032	0.030	0.032	0.031	0.030	0.032
	(0.175)	(0.170)	(0.175)	(0.173)	(0.170)	(0.176)
Manufacture of pharmaceuticals	0.009	0.001	0.010	0.001	0.001	0.001
	(0.097)	(0.030)	(0.100)	(0.027)	(0.030)	(0.025)
Manufacture of plastic, glass etc.	0.025	0.003	0.027	0.003	0.003	0.002
	(0.156)	(0.052)	(0.161)	(0.050)	(0.052)	(0.049)
Manufacture of metal products etc.	0.030	0.011	0.031	0.011	0.011	0.011
	(0.169)	(0.104)	(0.173)	(0.105)	(0.104)	(0.105)
Manufacture of electronic components	0.013	0.003	0.014	0.003	0.003	0.003
	(0.113)	(0.056)	(0.116)	(0.055)	(0.056)	(0.054)
Manufacture of electrical equipment	0.006	0.005	0.006	0.005	0.005	0.005
	(0.078)	(0.072)	(0.079)	(0.072)	(0.072)	(0.073)
Manufacture of machinery	0.042	0.017	0.044	0.017	0.017	0.017
	(0.200)	(0.130)	(0.204)	(0.129)	(0.130)	(0.129)
Manufacture of transport equipment	0.003	0.003	0.003	0.003	0.003	0.003
	(0.057)	(0.054)	(0.057)	(0.054)	(0.054)	(0.054)
Publishing, television and radio	0.014	0.008	0.015	0.009	0.008	0.009
	(0.119)	(0.091)	(0.121)	(0.093)	(0.091)	(0.095)
Telecommunications	0.010	0.003	0.011	0.003	0.003	0.002
	(0.102)	(0.051)	(0.105)	(0.050)	(0.051)	(0.050)
IT and information service activities	0.038	0.008	0.041	0.008	0.008	0.008
	(0.192)	(0.091)	(0.197)	(0.090)	(0.091)	(0.090)
Consultancy etc.	0.062	0.016	0.065	0.015	0.016	0.015
	(0.240)	(0.124)	(0.246)	(0.123)	(0.124)	(0.121)
Scientific research and development	0.008	0.001	0.008	0.001	0.001	0.001
	(0.088)	(0.031)	(0.090)	(0.030)	(0.031)	(0.029)
Advertising and other business services	0.011	0.011	0.011	0.011	0.011	0.011
	(0.103)	(0.103)	(0.103)	(0.105)	(0.103)	(0.106)
Human health activities	0.010	0.054	0.007	0.057	0.054	0.061
	(0.100)	(0.226)	(0.084)	(0.232)	(0.226)	(0.238)
Residential care	0.026	0.004	0.028	0.004	0.004	0.004
	(0.160)	(0.066)	(0.164)	(0.064)	(0.066)	(0.062)
Observations	730,001	47,752	682,249	95,504	47,752	47,752

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 10: Distribution of Workers Across Occupations in February 2020

	Unmatched			Matched		
	Full sample	Treatment	Control	Full sample	Treatment	Control
Military work below officer rank	0.000	0.001	0.000	0.001	0.001	0.000
	(0.021)	(0.024)	(0.021)	(0.023)	(0.024)	(0.021)
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	0.014	0.019	0.013	0.014	0.013
	(0.136)	(0.116)	(0.137)	(0.115)	(0.116)	(0.114)
Production and services managers	0.015	0.005	0.016	0.005	0.005	0.005
	(0.122)	(0.073)	(0.124)	(0.071)	(0.073)	(0.070)
Hotel, restaurant, retail managers etc.	0.008	0.017	0.007	0.017	0.017	0.016
	(0.087)	(0.128)	(0.083)	(0.127)	(0.128)	(0.127)
Science and engineering professionals	0.047	0.012	0.049	0.012	0.012	0.012
	(0.211)	(0.110)	(0.216)	(0.109)	(0.110)	(0.109)
Health professionals	0.010	0.028	0.009	0.031	0.028	0.034
	(0.101)	(0.164)	(0.095)	(0.173)	(0.164)	(0.181)
Teaching professionals	0.033	0.020	0.034	0.021	0.020	0.022
	(0.179)	(0.141)	(0.181)	(0.144)	(0.141)	(0.147)
Business and administration professionals	0.064	0.018	0.067	0.017	0.018	0.017
	(0.245)	(0.132)	(0.250)	(0.130)	(0.132)	(0.128)
Information technology professionals etc.	0.046	0.007	0.049	0.007	0.007	0.006
	(0.209)	(0.085)	(0.215)	(0.082)	(0.085)	(0.080)
Legal, social and cultural professionals	0.018	0.007	0.019	0.008	0.007	0.008
	(0.132)	(0.085)	(0.135)	(0.087)	(0.085)	(0.089)
Science and engineering ass. professionals	0.044	0.019	0.046	0.018	0.019	0.018
	(0.205)	(0.136)	(0.209)	(0.134)	(0.136)	(0.133)
Health associate professionals	0.010	0.032	0.008	0.029	0.032	0.026
	(0.097)	(0.175)	(0.089)	(0.168)	(0.175)	(0.160)
Business and admin. ass. professionals	0.087	0.071	0.088	0.072	0.071	0.073
	(0.282)	(0.256)	(0.284)	(0.259)	(0.256)	(0.261)
Legal, social and cultural ass. professionals	0.007	0.034	0.005	0.033	0.034	0.032
	(0.084)	(0.182)	(0.072)	(0.180)	(0.182)	(0.177)
Information technicians etc.	0.012	0.005	0.012	0.005	0.005	0.004
	(0.107)	(0.068)	(0.109)	(0.067)	(0.068)	(0.066)
Secretaries and office clerks	0.044	0.041	0.044	0.042	0.041	0.044
	(0.206)	(0.198)	(0.206)	(0.201)	(0.198)	(0.204)
Customer services clerks	0.012	0.032	0.011	0.029	0.032	0.027
	(0.110)	(0.176)	(0.103)	(0.169)	(0.176)	(0.161)
Numerical and material recording clerks	0.041	0.025	0.042	0.026	0.025	0.026
	(0.198)	(0.158)	(0.200)	(0.159)	(0.158)	(0.160)
Other clerical support workers	0.010	0.012	0.010	0.013	0.012	0.013
	(0.099)	(0.109)	(0.098)	(0.112)	(0.109)	(0.115)
Personal service workers	0.026	0.152	0.017	0.142	0.152	0.131
	(0.159)	(0.359)	(0.129)	(0.349)	(0.359)	(0.338)
Sales workers	0.050	0.150	0.043	0.152	0.150	0.154
	(0.218)	(0.357)	(0.203)	(0.359)	(0.357)	(0.361)
Personal care workers	0.019	0.013	0.019	0.013	0.013	0.013
	(0.137)	(0.113)	(0.138)	(0.113)	(0.113)	(0.112)
Protective services workers	0.004	0.011	0.004	0.012	0.011	0.013
	(0.066)	(0.103)	(0.062)	(0.109)	(0.103)	(0.114)
Building workers and painters	0.066	0.015	0.070	0.015	0.015	0.014
	(0.249)	(0.122)	(0.255)	(0.121)	(0.122)	(0.119)
Metal and machinery workers	0.048	0.036	0.049	0.036	0.036	0.037
	(0.214)	(0.186)	(0.215)	(0.187)	(0.186)	(0.189)
Handicraft and printing workers	0.003	0.005	0.003	0.005	0.005	0.005
	(0.058)	(0.071)	(0.057)	(0.071)	(0.071)	(0.071)
Electrical and electronic workers	0.026	0.009	0.028	0.009	0.009	0.008
	(0.160)	(0.094)	(0.164)	(0.093)	(0.094)	(0.092)
Food processing workers etc.	0.013	0.009	0.013	0.009	0.009	0.010
	(0.111)	(0.097)	(0.112)	(0.097)	(0.097)	(0.097)
Stationary plant and machine operators	0.049	0.022	0.050	0.021	0.022	0.020
	(0.215)	(0.145)	(0.219)	(0.143)	(0.145)	(0.141)
Assemblers	0.010	0.007	0.011	0.007	0.007	0.007
	(0.101)	(0.082)	(0.102)	(0.083)	(0.082)	(0.083)
Drivers and mobile plant operators	0.045	0.027	0.047	0.027	0.027	0.027
	(0.208)	(0.161)	(0.211)	(0.162)	(0.161)	(0.163)
Cleaners and helpers	0.020	0.054	0.018	0.055	0.054	0.057
	(0.141)	(0.225)	(0.133)	(0.228)	(0.225)	(0.231)
Agricultural and forestry laborers etc.	0.001	0.000	0.001	0.000	0.000	0.000
	(0.027)	(0.020)	(0.027)	(0.020)	(0.020)	(0.020)
Mining and construction laborers etc.	0.063	0.046	0.065	0.047	0.046	0.049
	(0.244)	(0.209)	(0.246)	(0.212)	(0.209)	(0.216)
Food preparation assistants	0.006	0.033	0.004	0.038	0.033	0.043
	(0.078)	(0.179)	(0.064)	(0.191)	(0.179)	(0.203)
Refuse workers etc.	0.007	0.008	0.007	0.008	0.008	0.009
	(0.083)	(0.091)	(0.083)	(0.092)	(0.091)	(0.093)
Observations	730,001	47,752	682,249	95,504	47,752	47,752

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 and horticultural workers, excluding assistants. 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 11: Estimated Effects of Covariates on the Furlough Probability

	Logit estimates	Average marginal effects
Age (Years)	-0.011***	-0.001***
	(0.001)	(0.000)
Man	-0.269***	-0.013***
	(0.013)	(0.001)
Foreign	0.161***	0.008***
	(0.019)	(0.001)
Youngest child is $\leq$ 5 years	0.012	0.001
	(0.015)	(0.001)
Youngest child is 6-10 years	-0.007	-0.000
	(0.019)	(0.001)
Youngest child is 11-15 years	-0.027	-0.001
	(0.019)	(0.001)
High school	0.177***	0.009***
	(0.023)	(0.001)
Vocational training	0.095***	0.004***
	(0.016)	(0.001)
Short higher education	-0.222***	-0.010***
	(0.026)	(0.001)
Medium higher education	-0.108***	-0.005***
	(0.024)	(0.001)
Bachelor's degree	-0.132**	-0.006**
	(0.044)	(0.002)
Long higher education	-0.337***	-0.014***
	(0.028)	(0.001)
Research education	-0.591***	-0.023***
	(0.105)	(0.003)
Full-time	0.120***	0.005***
	(0.013)	(0.001)
Inflow to employment	-0.107	-0.005
	(0.078)	(0.003)
Exit from employment	-3.574***	-0.062***
	(0.244)	(0.001)
Several jobs	0.672***	0.037***
	(0.019)	(0.001)
Job-to-job transition	-0.211***	-0.009***
	(0.041)	(0.002)
Tenure (Years)	-0.008***	-0.000***
	(0.002)	(0.000)
Avg. monthly labor income (DKK 1,000)	-0.024***	-0.001***
	(0.001)	(0.000)
Size (1,000 persons)	-0.101***	-0.005***
	(0.003)	(0.000)
Negative empl. growth	-0.152***	-0.007***
	(0.026)	(0.001)
Stable empl. growth	-0.030	-0.001
	(0.024)	(0.001)
Positive empl. growth	-0.245***	-0.011***
	(0.026)	(0.001)
Highly positive empl. growth	0.064*	0.003*
	(0.027)	(0.001)
Mean earnings	-0.025***	-0.001***
	(0.001)	(0.000)
Mean tenure in the firm	-0.010*	-0.000*
	(0.004)	(0.000)
Constant	-4.226***	
	(0.215)	
Observations	730,001	730,001

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 three children indicators is having no children or only children older than 15 years. For the seven human capital indicators, it is primary school, and for the four employment growth indicators, it is being employed in a firm that was established after 2016 or had no employees at the end of that year. * $p<0.05$, ** $p<0.01$, *** $p<0.001$. Estimation results for the industry and occupation dummies are presented in tables 12-14 in appendix E.

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

Table 12: Estimated Effects of Industries on the Furlough Probability

	Logit estimates	Average marginal effects
Raw materials, water and energy	0.461**	0.024*
	(0.178)	(0.011)
Construction	0.474**	0.025**
	(0.148)	(0.009)
Wholesale and retail trade	2.599***	0.189***
	(0.144)	(0.017)
Transportation	1.932***	0.147***
	(0.146)	(0.016)
Accommodation and food service	4.146***	0.513***
	(0.146)	(0.026)
Financial and insurance	0.209	0.010
	(0.157)	(0.008)
Real estate	0.758***	0.044***
	(0.150)	(0.011)
Travel agents, cleaning etc.	1.715***	0.124***
	(0.146)	(0.015)
Public administration, defense etc.	3.038***	0.314***
	(0.168)	(0.027)
Education	1.399***	0.096***
	(0.149)	(0.014)
Arts, entertainment and recreation	3.788***	0.443***
	(0.147)	(0.026)
Other service activities	2.959***	0.297***
	(0.146)	(0.023)
Manufacture of food products etc.	0.675***	0.038***
	(0.152)	(0.010)
Manufacture of textiles etc.	1.961***	0.159***
	(0.176)	(0.021)
Manufacture of wood, paper etc.	0.892***	0.054***
	(0.158)	(0.012)
Manufacture of chemicals, furniture etc.	2.335***	0.200***
	(0.147)	(0.019)
Manufacture of pharmaceuticals	0.674**	0.038**
	(0.213)	(0.014)
Manufacture of plastic, glass etc.	-0.068	-0.003
	(0.169)	(0.007)
Manufacture of metal products etc.	1.120***	0.071***
	(0.151)	(0.013)
Manufacture of electronic components	0.993***	0.062***
	(0.167)	(0.013)
Manufacture of electrical equipment	2.035***	0.167***
	(0.159)	(0.020)
Manufacture of machinery	1.529***	0.108***
	(0.149)	(0.015)
Manufacture of transport equipment	2.071***	0.173***
	(0.170)	(0.021)
Publishing, television and radio	1.693***	0.127***
	(0.154)	(0.017)
Telecommunications	0.973***	0.060***
	(0.172)	(0.014)
IT and information service activities	1.185***	0.077***
	(0.154)	(0.013)
Consultancy etc.	1.030***	0.064***
	(0.149)	(0.012)
Scientific research and development	0.696***	0.040**
	(0.207)	(0.014)
Advertising and other business services	1.876***	0.148***
	(0.152)	(0.018)
Human health activities	3.492***	0.389***
	(0.147)	(0.025)
Residential care	-0.043	-0.002
	(0.161)	(0.007)
Observations	730,001	730,001

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 11 and 13-14 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 (1/2)

	Logit estimates	Average marginal effects
Military work below officer rank	1.526***	0.110***
	(0.272)	(0.028)
Top management and legislators	0.729***	0.042***
	(0.179)	(0.012)
Administrative managers etc.	1.969***	0.158***
	(0.165)	(0.020)
Production and services managers	1.575***	0.115***
	(0.172)	(0.018)
Hotel, restaurant, retail managers etc.	2.285***	0.200***
	(0.165)	(0.022)
Science and engineering professionals	1.639***	0.120***
	(0.165)	(0.017)
Health professionals	2.366***	0.210***
	(0.164)	(0.023)
Teaching professionals	1.543***	0.110***
	(0.164)	(0.017)
Business and administration professionals	1.694***	0.126***
	(0.163)	(0.017)
Information technology professionals etc.	1.190***	0.078***
	(0.169)	(0.015)
Legal, social and cultural professionals	0.849***	0.050***
	(0.170)	(0.013)
Science and engineering ass. professionals	1.828***	0.140***
	(0.163)	(0.018)
Health associate professionals	2.336***	0.206***
	(0.163)	(0.022)
Business and admin. ass. professionals	1.908***	0.142***
	(0.160)	(0.017)
Legal, social and cultural ass. professionals	3.096***	0.323***
	(0.163)	(0.027)
Information technicians etc.	1.498***	0.107***
	(0.175)	(0.018)
Secretaries and office clerks	1.808***	0.136***
	(0.161)	(0.018)
Customer services clerks	2.752***	0.267***
	(0.163)	(0.025)
Numerical and material recording clerks	1.396***	0.095***
	(0.162)	(0.015)
Observations	730,001	730,001

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 and horticultural workers, excluding assistants. 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 11-12 in appendix E.

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

Table 14: Estimated Effects of Occupations on the Furlough Probability (2/2)

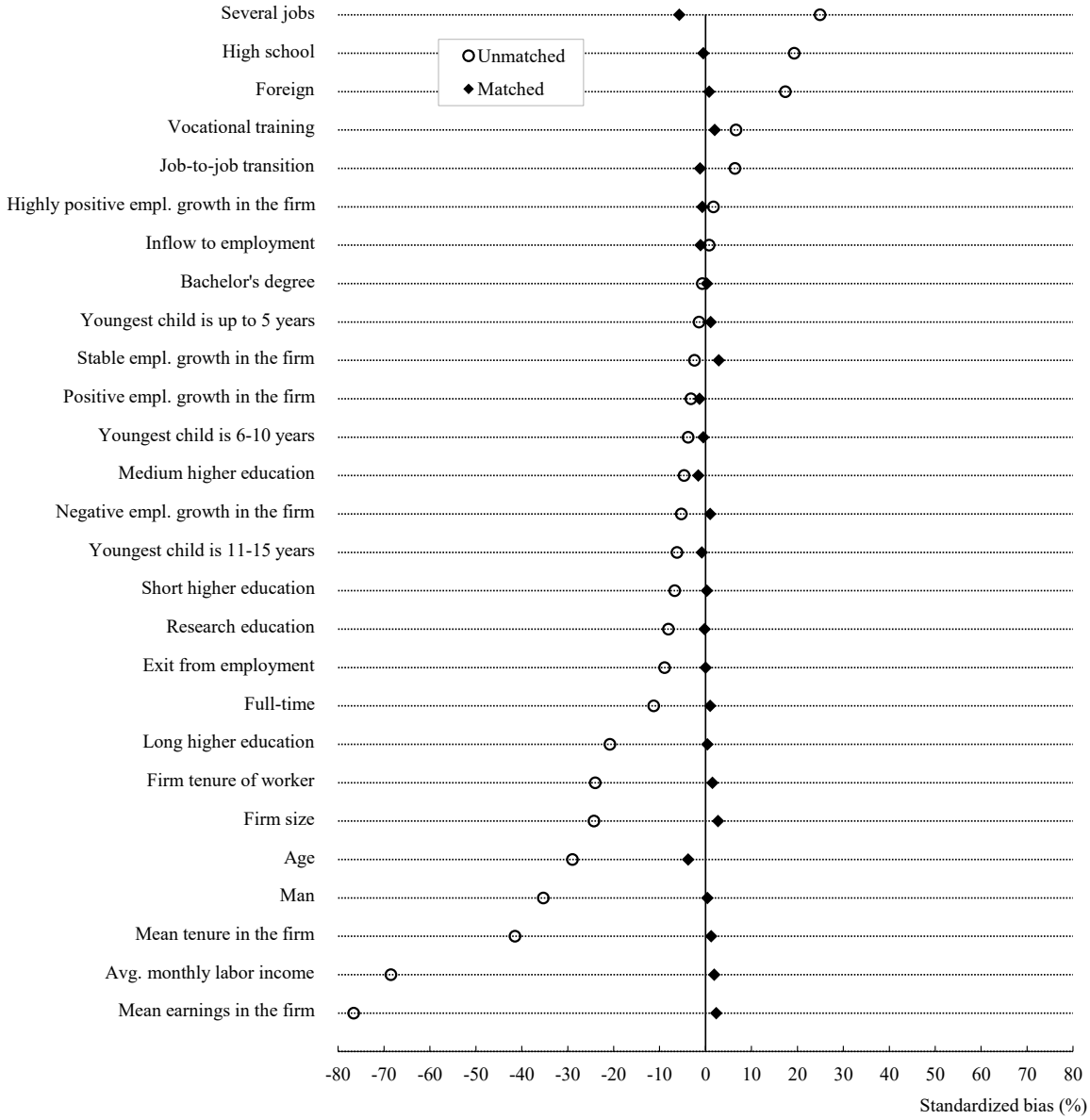
	Logit estimates	Average marginal effects
Other clerical support workers	2.067***	0.171***
	(0.166)	(0.021)
Personal service workers	3.201***	0.339***
	(0.160)	(0.027)
Sales workers	2.245***	0.184***
	(0.160)	(0.020)
Personal care workers	1.090***	0.069***
	(0.165)	(0.014)
Protective services workers	2.538***	0.236***
	(0.167)	(0.024)
Building workers and painters	1.265***	0.084***
	(0.164)	(0.015)
Metal and machinery workers	1.682***	0.122***
	(0.161)	(0.017)
Handicraft and printing workers	2.720***	0.263***
	(0.176)	(0.027)
Electrical and electronic workers	1.702***	0.128***
	(0.167)	(0.018)
Food processing workers etc.	1.690***	0.127***
	(0.167)	(0.018)
Stationary plant and machine operators	1.700***	0.126***
	(0.163)	(0.017)
Assemblers	1.786***	0.138***
	(0.170)	(0.019)
Drivers and mobile plant operators	1.368***	0.092***
	(0.163)	(0.015)
Cleaners and helpers	2.152***	0.179***
	(0.161)	(0.020)
Agricultural and forestry laborers etc.	1.010***	0.063**
	(0.289)	(0.023)
Mining and construction laborers etc.	1.671***	0.120***
	(0.160)	(0.016)
Food preparation assistants	2.367***	0.212***
	(0.164)	(0.023)
Refuse workers etc.	1.955***	0.158***
	(0.169)	(0.020)
Observations	730,001	730,001

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 and horticultural workers, excluding assistants. 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 11-12 in appendix E.

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

Appendix F – Standardized Biases Before and After Matching

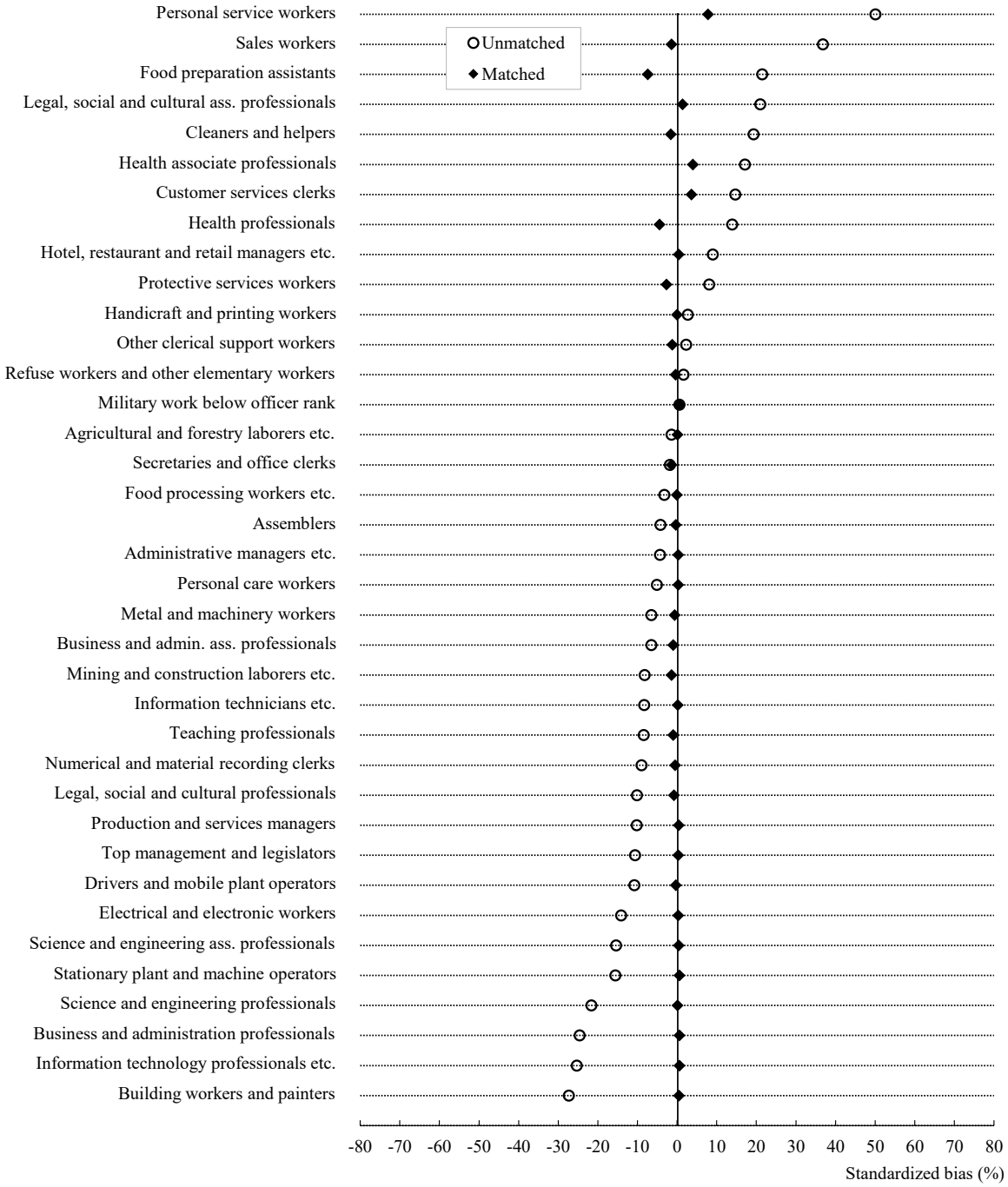
Figure 6: Standardized Biases Across Selected Covariates Before and After Matching



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 – Net Costs to the Government Per Saved Job Match

By the end of August 2020, around 273,000 wage earners had been furloughed under the Danish wage compensation scheme for employees, cf. table 5 in appendix A. Assuming that the scheme's effect on exits from employment was the same across different compensation periods, this implies that the scheme saved $0.041 \times 273,271 = 11,165$ job matches during the first wave of the pandemic. Given a total expenditure on the scheme of DKK 12.2 billion and an average compensation period of 3.8 months, this translates into an average cost per saved job match of around $\frac{\text{DKK 12.2 billion}}{\text{11,165 exits} \times \text{3.8 months}} = \text{DKK 287,086}$ per month.

In order to compute the net costs to the government per saved job match, I also account for the fact that a reduction in job losses decreases spending on unemployment insurance. Table 15 shows a back-of-the-envelope calculation for the monthly net extra costs to the Danish government from saving a job match under the Danish wage compensation scheme relative to paying unemployment insurance. I evaluate unemployment insurance at the mean level of earnings for furloughed workers, assuming that they are full-time insured, and use the Danish Ministry of Finance's estimate for monthly activation costs (Finansministeriet, 2021).[25] Under these conditions, I find that the average cost per saved job match was DKK 267,703 per month net of unemployment insurance and activation costs.

$$\text{Net costs before taxes} = \underbrace{287,086}_{\substack{\text{Wage compensation} \\ \text{per saved job match}}} - \underbrace{(19,083 + 300)}_{\substack{\text{UI and activation} \\ \text{costs}}} = \text{DKK 267,703}$$

The scheme also has implications for tax revenue to the government. On the one hand, it has ensured a higher income for workers who would have otherwise lost their jobs and taken up unemployment benefits, leading to an increase in tax revenue. On the other hand, the scheme is estimated to have reduced the labor income of furloughed workers by 1.6 percent, leading to a fall in tax revenue from workers who would have been preserved anyway. To assess the relative importance of the two channels, I assume an income tax rate

25. Unemployment insurance covers up to 90 percent of the previous salary but cannot exceed the maximum rate of DKK 19,083 (DKK 12,722) per month for full-time (part-time) insured members in 2020. Thus, when evaluated at the mean level of earnings for furloughed workers before the pandemic, i.e., at DKK 29,131 per month, and assuming that they are full-time insured and not recent graduates, unemployment insurance amounts to DKK 19,083 per month.

of 37.7 percent out of gross income and a mechanical change in tax revenue ("tilbageløb") of 23 percent out of after-tax income, in line with similar calculations by the Danish Ministry of Finance.[26] Overall, the calculation suggests that the wage compensation scheme has resulted in a net reduction in tax revenue to the Danish government per saved job match of DKK 724 per month compared to paying unemployment insurance.

$$\text{Net taxes} = (\underbrace{0.041 \times 10,005}_{\substack{\text{Higher income for}\\ \text{preserved workers}\\ =0.041\times(29,574\\ \times(1-0.016)-19,083)}} - \underbrace{(1-0.041)\times 485}_{\substack{\text{Lower income for}\\ \text{other workers}\\ =(1-0.041)\\ \times 29,574\times 0.016}}) \times (\underbrace{0.377}_{\substack{\text{Income}\\ \text{tax rate}}} + \underbrace{(1-0.377)\times 0.23}_{\substack{\text{Mechanical change}\\ \text{in tax revenue}}}) \times \underbrace{\frac{1}{0.041}}_{\substack{\text{Scaled to}\\ \text{"per saved}\\ \text{job matches"}}} = \text{DKK -724}$$

In summary, a back-of-the-envelope calculation suggests that the net extra cost to the Danish government of saving a job match under the Danish wage compensation scheme was around DKK 268,426 per month after taxes relative to paying unemployment insurance and activation costs.

$$\text{Net costs after taxes} = \underbrace{267,703}_{\substack{\text{Net costs}\\ \text{before taxes}}} + \underbrace{724}_{\substack{\text{Net reduction}\\ \text{in tax revenue}\\ =524+199}} = \text{DKK 268,426}$$

It should be noted that there are a number of factors that this calculation does not take into account. First, the calculation assumes that all furloughed workers are full-time insured, while in reality, some workers are non-insured and others only part-time insured, in which case unemployment benefits are lower. Second, it evaluates unemployment insurance at the mean level of earnings and thus does not account for differences in replacement rates for furloughed workers with salaries above or below the maximum rate. Neither does it account for lower replacement rates for recent graduates. Lastly, the calculation assumes that unemployment is the only alternative for furloughed workers who would have been laid off in the absence of the scheme, while in reality, they could have found alternative employment or chosen to leave the labor force. These factors are important to consider when interpreting the results of this calculation.

26. When the Danish Ministry of Finance was asked to assess the impact on public finances of putting employees on wage compensation instead of unemployment benefits, they assumed an income tax rate of 37.7 percent and a mechanical change in tax revenue ("tilbageløb") of 23 percent (Finansministeriet, 2021). For income taxes, the mechanical change in revenue refers to the fact that changes in disposable income impact private consumption, which in turn affects revenue from VAT taxes and duties. It is estimated to be 23 percent by the Danish Ministry of Taxation (Skatteministeriet, 2019).

Table 15: Net Costs to the Government Per Saved Job Match

Assumptions	
1. Earnings per month in comp. period	29,574
2. Wage compensation per month	11,730
3. Unemployment insurance per month	19,083
4. Activation programmes	300
5. Estimated fall in exit probability	0.041
6. Estimated fall in earnings	0.016
7. Income tax rate	0.377
8. Rate of mechanical change in tax revenue ("tilbageløb")	0.230
Net monthly costs before taxes	**267,703**
1a. Increased spending on wage compensation	287,086
1b. Reduced spending on UI and activation	19,383
Net change in income taxes	**-524**
2a. More tax revenue from additional wage earners	10,966
2b. Less tax revenue from unemployment insurance	7,194
2c. Less tax revenue due to fall in earnings	4,296
Net mechanical change in tax revenue ("tilbageløb")	**-199**
3a. More tax revenue from additional wage earners	4,168
3b. Less tax revenue from unemployment insurance	2,734
3c. Less tax revenue due to fall in earnings	1,633
Net monthly cost after taxes	**268,426**

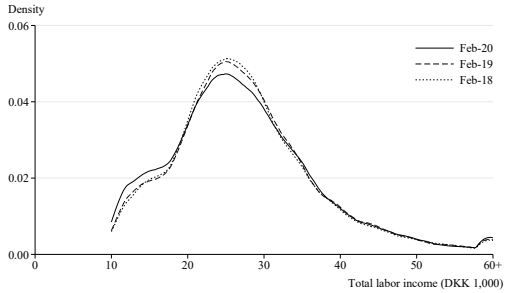
Note: The table shows a back-of-the-envelope calculation for the monthly net extra costs to the Danish government from saving a job match under the Danish wage compensation scheme relative to paying unemployment insurance. This calculation accounts for both the estimated fall in the exit probability and in labor income in response to the scheme, and assumes that workers who would otherwise have been laid off are full-time insured against unemployment. Earnings refer to estimated earnings in the absence of the scheme.

Source: Own calculations based on administrative register data from Statistics Denmark, Retsinformation, and FIU218 by the Danish Ministry of Finance.

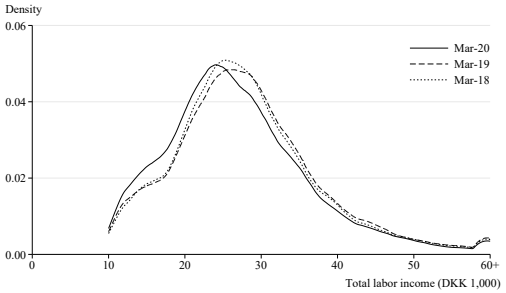
Appendix H – Labor Income Distributions for Furloughed Workers

Figure 8: Labor Income Distributions for Furloughed Workers

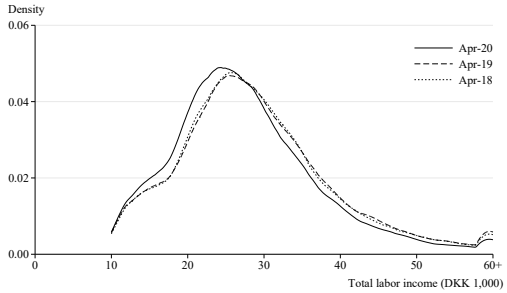
(a) February



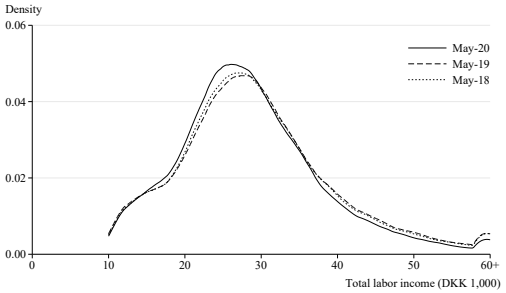
(b) March



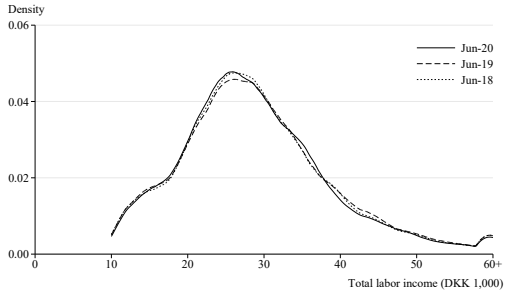
(c) April



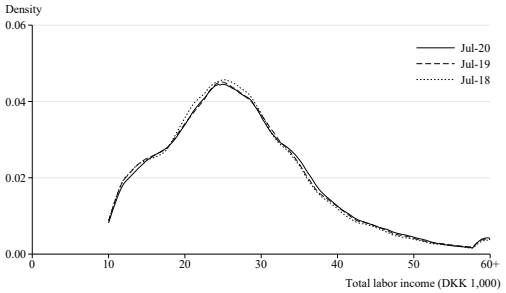
(d) May



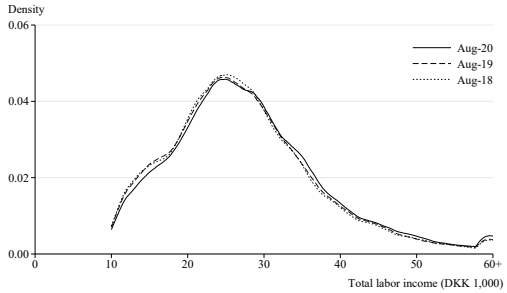
(e) June



(f) July



(g) August



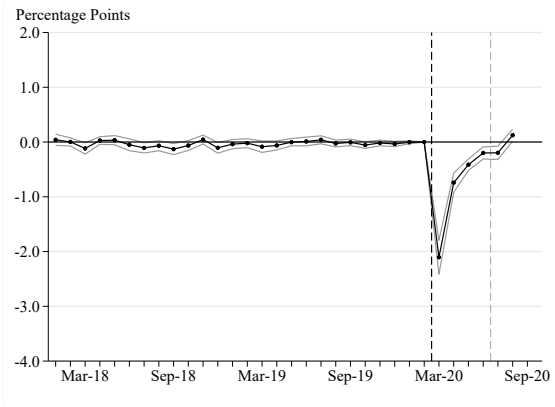
Note: The figures show monthly kernel density estimates of labor income distributions for workers furloughed from March 2020 to June 2020 in the baseline sample for the years 2018-2020. The kernel used is an Epanechnikov, and the bandwidth is 1.0. Panel (8a) shows the distributions for the pre-treatment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

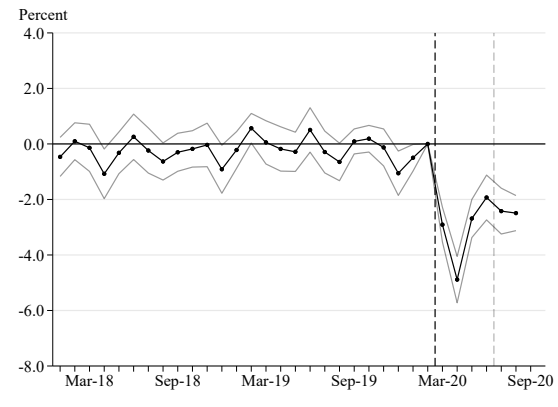
Appendix I – Estimation Results for All Furloughed Workers

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

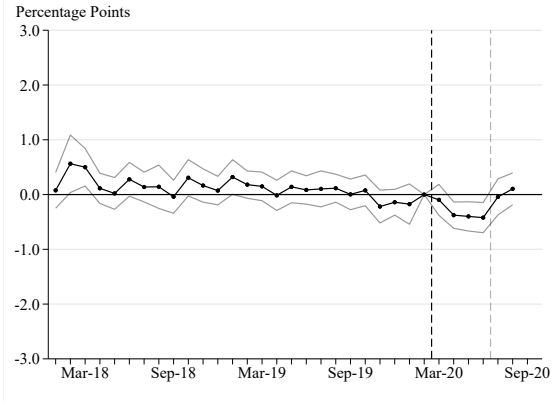
(a) Employment-exit Probability



(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.