ETLA Working Papers | 111 14.12.2023

Jobs, Workers, and Firms

DISSECTING THE LABOUR MARKET EFFECTS OF FINLAND'S COVID-19 SUBSIDY PROGRAM

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Suggested citation:
Hirvonen, Johannes, Kässi, Otto & Ropponen, Olli (14.12.2023). "Jobs, Workers, and Firms: Dissecting the Labour Market Effects of Finland's COVID-19 Subsidy Program".
ETLA Working Papers No 111.
http://pub.etla.fi/ETLA-Working-Papers-111.pdf

Abstract

This paper examines the labour market impacts of Finland's initial COVID-19 subsidy program, designed to mitigate the economic fallout of the pandemic. Utilising a novel and comprehensive dataset and a judge-leniency instrumental variables design, we analyse the effects of these subsidies at both the firm and worker levels.

Our findings reveal nuanced effects: the program increased the wage sum in the treated firms and decreased the risk of unemployment. On the other hand, the subsidies reduced labour productivity in treated firms, potentially hindering creative destruction. At the worker level, subsidised employees fared better in subsequent years than their non-subsidised counterparts, with slight increases in annual salaries and a higher likelihood of being employed. However, these workers were more likely to be employed in lower-productivity firms.

This paper contributes to our understanding of the implications of fiscal interventions during crises and provides critical insights for shaping future economic policies in similar contexts.

Tiivistelmä

Business Finlandin koronahäiriörahoitus-ohjelman vaikutukset työpaikkoihin, työntekijöihin ja yrityksiin

Tarkastelemme Business Finlandin koronahäiriörahoitusohjelman työmarkkinavaikutuksia. Hyödynnämme tutkimuksessa suomalaista yritys–työntekijä-aineistoa sekä ns. tuomari-instrumenttimuuttujatekniikkaa ("judge instrumental variables") ja analysoimme tukien vaikutuksia yrityksiin ja toisaalta yritysten työntekijöihin. Tulostemme perusteella tukiohjelma lisäsi palkkasummaa tuetuissa yrityksissä sekä pienensi työntekijöiden työttömyysriskiä. Tukien vaikutus työn tuottavuuteen tuetuissa yrityksissä oli negatiivinen. Työntekijätasolla tarkasteltuna tuki nosti tuetuissa yrityksissä työskennelleiden työntekijöiden vuosipalkkoja ja vähensi työttömyysriskiä. Kuitenkin tuetuissa yrityksissä työskennelleet työntekijät olivat todennäköisemmin työllistettyinä alhaisemman tuottavuuden yrityksissä vielä pandemian jälkeenkin. Tämä tutkimus edistää ymmärrystämme tukitoimien vaikutuksista kriisien aikana ja auttaa suunnittelemaan kustannustehokkaita tukitoimia tulevien kriisien varalle.

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Acknowledgements: This research was funded by Business Finland as a part of the *Company Resilience in the Era of Globalization (CRIEG)* research project. Petja Karppinen provided valuable research assistance. We are grateful to Business Finland for providing us with the data used in this paper and to Annu Kotiranta, Kari Komulainen, Jari Hyvärinen, and Olli Sinerma from Business Finland for insights into the subsidy program. Niku Määttänen and Jyrki Ali-Yrkkö have given helpful comments for previous versions of the paper. Pekka Vanhala's help with the income register data has been invaluable. We thank the seminar participants at Euroframe 2022 for the helpful discussion. All errors are our own.

Kiitokset: Tämä tutkimus on toteutettu osana Business Finlandin rahoittamaa *Company Resilience in the Era of Globalization (CRIEG)* -hanketta. Kiitämme Business Finlandia aineistosta sekä Annu Kotirantaa, Kari Komulaista, Jari Hyväristä ja Olli Sinermaa Business Finlandista avusta aineiston tulkinnassa. Kiitämme myös Niku Määttästä ja Jyrki Ali-Yrkköä hyödyllisistä kommenteista, Petja Karppista erinomaisesta tutkimusavustajan työstä, Pekka Vanhalaa avusta tulorekisteriaineiston kanssa sekä EuroFrame 2022 -konferenssin osallistujia hyvistä kysymyksistä ja kommenteista.

Keywords: Business Finland subsidy program, Crisis subsidies, COVID-19, Fiscal policy, Productivity, Unemployment

Asiasanat: Koronahäiriörahoitus, Yritystuet, Kriisirahoitus, COVID-19, Tuottavuus, Työttömyys

JEL: H25, H32, E24

1 Introduction

One of the reasons why recessions are so costly is that they do not discriminate. In addition to acting as a mechanism for creative destruction, recessions can destroy valuable organisational capital and result in unnecessary unemployment, consequently leading to a long-term reduction in productivity (Caballero and Hammour, 1996). The public sector can protect jobs in the private sector by providing favourable loans or subsidies to firms. However, excessive subsidies to firms can tilt the scales in the opposite direction, resulting in too many unproductive jobs surviving and, consequently, too little creative destruction.

This issue is particularly relevant in the economic shock due to COVID-19, which led to massive fiscal interventions for firms, unprecedented in scope and scale (Bighelli et al., 2022; Cirera et al., 2021). Indeed, the relatively modest GDP contractions and low numbers of bankruptcies in many industrialised countries have been attributed to strong fiscal support from their governments (Auerbach et al., 2022; Andersen et al., 2022). The downside of this generous support to firms is that the support may hinder job-to-job mobility, which would benefit the economy as a whole.

This paper concentrates on this trade-off between protecting jobs and fostering creative destruction. We study a Finnish COVID-19 subsidy program on two dimensions. First, we show that the subsidies effectively reduced unemployment and increased the wage sums at firms. At the same time, we demonstrate that the subsidies did not increase sales and led to a decrease in labour productivity in the subsidised firms. Worker-level analysis further reveals that the subsidies reduced job-to-job transitions to more productive jobs. As a result, labour productivity at the level likely of the aggregate economy decreased due to the subsidy program.

To our knowledge, the most studied pandemic subsidy program is the Paycheck Protection Program (PPP). Research employing a range of econometric methods (Autor et al., 2022a; Bartik et al., 2021; Chetty et al., 2020; Dalton, 2021; Doniger and Kay, 2022; Faulkender et al., 2020; Granja et al., 2022; Hubbard and Strain, 2020; Joaquim and Netto, 2021; Kurmann et al., 2021; Li and Strahan, 2020) generally agrees that the PPP succeeded in preserving jobs, though its efficiency is debatable. Estimates suggest that the cost of saving a job through this program ranged mostly from \$150,000 to \$360,000. However, evaluating the program's efficiency solely based on the cost per job saved is insufficient. This metric overlooks subsidies' impact on labour allocation across firms and occupations. If subsidies result in workers remaining in less productive roles, this leads to a long-term reduction in overall productivity growth in the economy. Understanding the effectiveness, efficiency, and broader macro effects of crisis subsidies is critical for designing effective policies for future crises.

Our empirical context is a subsidy program targeted at Finnish firms by the Finnish innovation funding agency Business Finland at the start of the pandemic. To overcome the concern that the allocation of subsidies to firms might be endogenous, we exploit the fact that the applications were randomly allocated to decision-makers. While there were specific standards for evaluating the applications, there was substantial variation in interpreting these criteria across the decision-makers. We demonstrate that the decision-maker stringency is highly predictive of the funding decision but is uncorrelated with firm background characteristics. Consequently, we can leverage the fact that the probability of funding for identical applications randomly varied based on the decision-maker handling the application in an instrumental variable model.[1]

Our findings indicate that the subsidy program effectively safeguarded employment. The causal estimate suggests that each euro of subsidy led to a roughly five-euro increase in the total wage sum. Additionally, by the end of 2022, the subsidy had shortened the average unemployment duration of affected employees by 3.8 months. While these results point to the program's success in curbing unemployment, we also observed a decrease in labour productivity within subsidised firms. Furthermore, workers in these firms were less likely to transition to more productive firms. This suggests that the overall macroeconomic impact of the subsidy program may have been negative.

The existing causal evidence on the effectiveness of COVID-19-related stimulus programs, apart from the Paycheck Protection Program (PPP), is limited. A notable exception is the work by Cui et al. (2022), who examined the impact of a payroll tax cut in China. However, multiple studies provide causal insights into subsidy programs from past crises. For example, De Mel et al. (2012) used experimental methods to assess the response to a natural disaster in Sri Lanka, while Bruhn (2020) applied quasi-experimental techniques to examine the reaction to the Great Financial Crisis in Mexico. However, none of these studies explicitly address the unemployment risk or the job-to-job transition

[1] Our empirical approach is adapted from the judge-IV literature (e.g., Aizer and Doyle Jr, 2015; Bhuller et al., 2020; Cheng et al., 2021; Dahl et al., 2014; Dobbie and Song, 2015; Dobbie et al., 2018; Huttunen et al., 2022; Norris et al., 2021). To our knowledge, we are one of the first to apply the method outside the legal and criminal justice context.

effects associated with the subsidy programs.

The rest of this paper is structured as follows. In Section 2, we discuss the details of the subsidy program and contrast it to the PPP. Section 3 provides descriptive statistics and details the construction of the dataset. Section 4 reviews how we apply the judge-IV design in our context and argues that the decision-maker leniency instrumental variables are both valid and relevant in our context. Section 5 provides firm- and worker-level results and discusses them, and the final Section 6 concludes.

2 Institutional Context

This section first reviews the Business Finland COVID-19 subsidy scheme, its size and eligibility criteria, and contrasts it to its better-documented U.S. counterpart, the PPP.

2.1 Background of the Subsidy Program

Governments worldwide have implemented various programs to counteract the adverse effects of the COVID-19 pandemic. The most studied of these programs is the Paycheck Protection Program (PPP), introduced in the United States in March 2020.

The program provided low-interest forgivable loans to small firms facing financial distress. The loans were forgiven if the firms spent more than a pre-determined fraction of these loans on payroll and other fixed expenses such as rents and equipment, maintained average full-time employment at pre-crisis levels, and did not excessively cut worker wages. According to the program rules, it was meant for companies that had suffered from the pandemic, but this criterion was not quantified in any way.[2]

The general finding from the literature is that it protected workers from layoffs but at a high cost. In particular, the price tag of one job saved by the PPP adds up to 150,00-360,000 dollars. While the program could have been better targeted, it was extremely timely. The program was launched in late March, and over 90% of eligible firms were granted loans by the end of June.

This paper concentrates on Business Finland's "Business Development in Disruptive Circumstances" program, which shares many of the characteristics of the PPP. Like the PPP, it was a few-strings-attached program targeted at SME and midcap companies which employ under 500 people. The motivation for the program was to provide

[2]For details on the conditions related to PPP, see Autor et al. (2022b).

emergency liquidity for the Finnish corporate sector quickly before the proper legislative framework for supporting companies was in place.

The Finnish innovation funding agency, Business Finland, was responsible for administering the program. Its mandate permitted the provision of subsidies for research and development (R&D) and business development, but not specifically for crisis support. As a result, the rules of the funding scheme stipulated that applicants must convincingly propose a plan aimed at developing "new businesses, new supply chains, or new ways of organising work during and after the pandemic." In practice, any qualifying firm that submitted a project plan focused on developing something new *specifically for the firm* was considered eligible.

The funding scheme was in operation between March 20, 2020, and the end of June 2020. After the end of June 2020, a legal framework was in place for direct cost support for companies, and the business development support scheme was phased out (Koski et al., 2022). Thus, our analysis focuses on the first batch of COVID support provided to SME and midcap companies in Finland.

The program consisted of two separate grants: the pre-analysis grant ("esiselvitysrahoitus") with a cap of €10,000 and the development grant ("kehitysrahoitus") with a cap of €100,000. Neither of the programs paid advances. Instead, the application included a preliminary budget, and Business Finland retroactively reimbursed the expenses up to the budget. Only costs related to payroll and purchase of external services were reimbursed. The program also required that the self-financed portion of the project equalled at least 25% of the subsidy amount. For instance, to qualify for a €100,000 grant, a firm needed to propose a project with a budget of at least €125,000.

The total budget was roughly 1 billion EUR, accounting for around 45% of the total pandemic-related firm subsidies given to the private sector during the pandemic and around 0.4% of the Finnish GDP in 2020. While substantial, the BF program still pales compared to the PPP, with a total budget of $ 800 billion (3.7 % of the U.S. GDP in 2020). The difference in magnitudes highlights the different goals of the subsidy programs. The PPP was one of the primary vehicles for the U.S. government to support firms through the pandemic. In contrast, the BF support scheme was an early support scheme until better-targeted subsidies had passed the legislative process.

Despite the differences in magnitudes, the PPP shared many characteristics with the

Business Finland grants. Both programs offered subsidies for payroll and related expenses with relatively lax criteria. Both programs targeted small and medium-sized companies and explicitly aimed to protect jobs.

During the period in question, a total of 25,921 applications were submitted by 23,322 firms. About two-thirds of these applications received funding. This rejection rate marks a significant departure from pre-pandemic conditions, where the acceptance rate for applications was nearly 100%. In non-crisis times, the rapporteurs act as project managers on the Business Finland side, assist firms in preparing their applications and filter ineligible firms before they even enter the application process.

In the following subsection, we discuss the application process and how we leverage the random allocation of applications to decision-makers as a source of identifying variation.

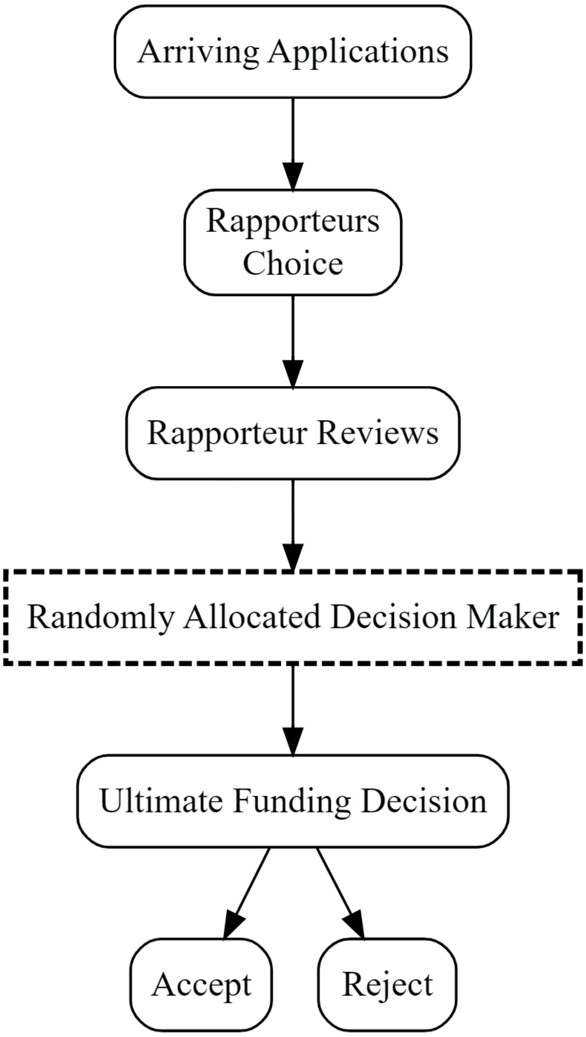
2.2 Random Allocation of Applications to Decision-Makers

The firms submitted applications electronically to Business Finland's online portal. The applications were first processed by rapporteurs, who read the application, asked for possible additional information from the applicants, and, after reviewing the application, recommended either acceptance or rejection. The rapporteurs' decisions needed to be signed off by a decision-maker. The role of the decision-makers was to authorise the rapporteurs' decisions and ensure no obvious errors occurred. The decision-makers had three options: recommend acceptance or rejection or return the application to the rapporteur for re-evaluation. The pools of decision-makers and rapporteurs were separate.

Regularly, each decision-maker specialises in a particular industry or geographic region. However, this did not happen during the pandemic. Instead, the applications were allocated to decision-makers randomly.[3] The process from application to funding decision is outlined in Fig 1.

[3] According to Business Finland's official instructions, also the rapporteurs were supposed to process applications on a first-come-first-served basis, which would have resulted in the random allocation of applications to rapporteurs in addition to random assignment to project managers. After discussing with rapporteurs, we learned that some rapporteurs cherry-picked more straightforward applications or applications from familiar industries. This resulted in a non-random allocation of applications. The allocation of applications processed by rapporteurs to project managers, on the other hand, was confirmed to be random.

Figure 1. The Application Process From Arrival of Application to Ultimate Decision.



Note: The figure visually represents the application process. The point where randomisation takes place is represented in a dashed box.

Importantly, for our identification strategy, there was a substantial amount of subjectivity in acceptance criteria, which resulted in significant differences in the results of identical applications handled by different project managers. The main reasons for rejection were that the applicant was a firm in distress,[4] or that the firm had already won substantial funding pre-pandemic,[5] or a new firm without evidence of past profitable business. Additionally, many applications were rejected for more subjective reasons. In

[4]EU state aid regulations prohibited giving any support to companies in financial distress in March of 2020 Business Finland (2023).

[5]According to EU competition regulations, the maximum sum of aid to a company is 200,000 euros over the current and two previous fiscal years. Thus, if a company had received public funding over this so-called *de minimis* limit, it was not eligible for the pandemic support Business Finland (2023b).

these cases, the reason for rejection was that the application did not show enough novelty or did not highlight the objectives of the new project in enough detail. Moreover, the acceptance decision was binary. Therefore, it is impossible for a lenient decision-maker to systematically fund larger projects than a non-lenient judge. Thus, we can also use decision-maker leniency as an instrument for the size of funding.

Using a combination of text matching and manual classification, we have classified all rejections based on the primary reason for rejection. The rejection reasons are summarised in Table 1.

Table 1. Distribution of Application Rejection Reasons

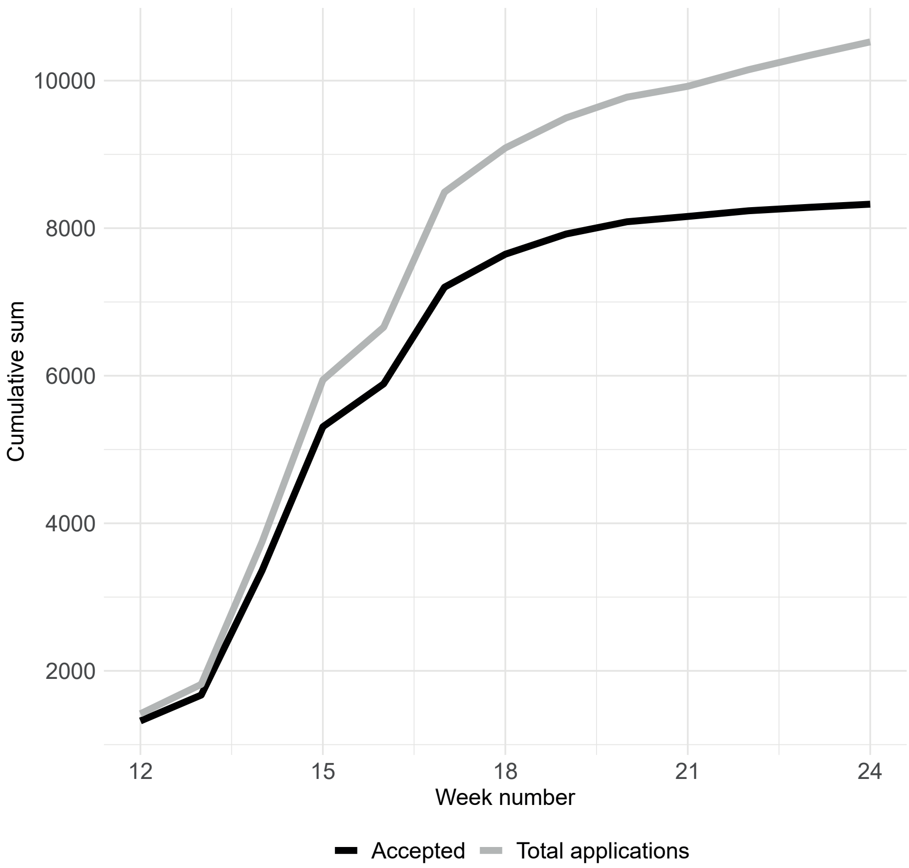
Decision	Share	Note
Accepted	67.02%	
Another funder	1.90%	
Firm not eligible	4.83%	
Funding application too big	1.46%	
Insufficient project plan	0.65%	
Intended use not eligible	9.90%	
No novel business	3.09%	
Over deminimis limit	1.16%	
Firm in difficulty	8.50%	Excluded from analysis
More than one application	1.50%	Excluded from analysis

Note: Distribution of rejection reasons. Rejection reasons are manually classified based on decision text.

For identification purposes, we impose several data restrictions. Firstly, we exclude smaller pre-analysis grants due to their relative insignificance compared to the applicants' business scales. We also omit applications rejected for multiple submissions from the same firm or for the firm being in financial difficulty, as these were already automatically deemed ineligible by rapporteurs. The reasoning behind this restriction is that we want to build our sample in a way that excludes applications from firms that have submitted applications that objectively result in rejection. Instead, we limit our data to the applications that were either accepted or rejected for subjective reasons.[6]

[6] We include projects rejected for exceeding the *de minimis* limit, as our data indicates some funded companies received support despite surpassing this threshold.

Figure 2. Cumulative Sum of Applications and Rejections During the Subsidy Program.



Note: This figure presents the cumulative sum of applications and rejections arriving in Business Finland in our restricted sample.

The exact criteria for acceptance underwent several revisions and amendments during the application period. As a result, the acceptance criteria got tighter while the program was in progress. Figure 2 illustrates this, revealing a higher proportion of rejections towards the end of the application period than at the beginning. Initially, over 90% of applications were accepted, but this rate dropped to around 25% by the program's conclusion. Although part of this shift can be attributed to variations in the applications themselves, conversations with Business Finland underscore that the acceptance criteria were significantly tightened during the spring of 2020. In our empirical analysis, we include week dummies and a linear time trend to capture the various changes in acceptance criteria.[7]

[7]When doing this, we make the reasonable assumption that all decision-makers were affected by changes in the acceptance criteria in the same way.

3 Data

3.1 Data Sources and Construction of the Analysis Sample

Our primary analysis sample consists of development grant applications submitted to Business Finland during the pandemic.

The application-level subsidy data contains information on the applying firm and the application itself, such as a firm identifier and the application date. We also know the size of the grant the firm applied for and whether the application was accepted or rejected. Additionally, we have access to the decision text for all approved and rejected grants. Importantly, for our empirical design, we also have data on the officials handling the application and making the decision, allowing us to construct an instrument based on the leniency of the decision-maker.

The firm identifier enables us to link the subsidy application data to various other administrative data from Statistics Finland. Firm-level balance sheet data contains, for example, the revenues, employee counts, wage sums, and debt levels of each firm operating in Finland annually. Combining these, we create an annual panel that contains the pre-pandemic information from firm balance sheets combined with information on our main outcomes of interest, sales, wage sum, personnel and labour productivity[8] throughout the end of 2021. We use these data in our firm-level analysis.

In the second part of the paper, we estimate the effects of the subsidies at the worker level. To do this, we utilise the income register data from the Finnish Tax Administration. We first identify all workers employed at the sample firms on the last day of 2019, at the beginning of the COVID-19 pandemic, and approximately three months before any subsidies were granted and paid. The worker-level data tracks monthly salaries earned from all employers, which enables us to identify spells without monthly salaries as unemployment spells.[9] The data thus allows us to examine how subsidies at the firm level affect workers at the winning firm and the broader labour market in which the firms operate.

We start the sample construction from the subsidy application data. There are 26,522

[8]We define labour productivity at the firm level as value-added per full-time employee.

[9]We note that the definition that a month with zero income does not necessarily mean a worker is unemployed according to the standard International Labour Organization (2013) definition. The workers with zero income could either be unemployed, furloughed with zero hours, or outside the workforce for other reasons. For brevity, we refer to these workers as unemployed.

subsidy applications in total. Of those, 14,713 are for the development subsidy. After filtering out duplicates and those originating from firms in difficulty, we are left with 12,696 applications. After combining application data with firm-level balance sheet data and filtering out observations with missing values, we are left with 10,804 observations. This is our analysis sample.

In total, we identify 233,049 workers employed in the applicant firms on the last day of 2019. Again, we create an annual panel of the firm averages of yearly earnings, months without employment, and months with a new employer for each worker linked to any of the firms in our analysis sample at the start of the pandemic.

In summary, our analysis sample integrates application-level subsidy data, firm-level balance sheet data, and worker data aggregated at the firm level. These comprehensive data allow us to investigate the impact of pandemic-related firm subsidies on different facets of the Finnish economy. Furthermore, such extensive data facilitate a thorough evaluation of the subsidy program's effectiveness and broader implications on the labour market.

3.2 Descriptive Statistics

The key summary statistics of the baseline firm-level analysis sample are reported in Table 2. All variables were measured in 2019. We report these statistics separately for the firms with an accepted application (treatment firms) and rejected firms (control).

The table demonstrates that most applicant firms are indeed small and medium-sized enterprises (SMEs). In most reported aspects, firms with accepted applications are larger than those whose applications were not rejected. Specifically, accepted firms possess greater equity and debt, have higher turnover, employ more workers, and exhibit both increased profitability and superior labour productivity. The rejected firms display a higher return on assets. Many of the differences are statistically non-significant, given the large standard errors. Nonetheless, Table 2 clearly demonstrates systematic differences in rejected and accepted firms even after the subset of firms that were facing difficulty at the time of application were filtered out.

In our analysis of the impact of subsidies on labour productivity, we employ labour productivity percentiles as the dependent variable. This approach is taken to minimise the noisiness of the dependent variable. To provide a comprehensive view, we also include

Table 2. Basic Summary Statistics of the Analysis Sample

	Rejected		Accepted	
	Mean (std. error.)	Median	Mean (std. error)	Median
Equity (1000 eur)	665.38	129.88	902.28	219.89
	(36.62)		(23.77)	
Debt (1000 eur)	580.69	72.38	820.57	129.42
	(35.63)		(22.4)	
Sales (1000 eur)	2730.63	906.35	3472.81	1322.17
	(112.25)		(66.95)	
Wage sum (1000 eur)	550.06	244.72	728.32	350.71
	(18.81)		(11.38)	
FTE personnel	14	6.92	17.63	8.87
	(0.44)		(0.26)	
Net profit (1000 eur)	55.82	14.75	58.86	19.31
	(5.65)		(3.78)	
Return on assets (%)	4.31	6.22	1.37	6.24
	(3.77)		(1.83)	
Labor productivity (1000 eur)	64.65	53.49	67.26	56.62
	(1.32)		(0.71)	
Labour productivity (percentile)	48.7	49	51.6	53
	(25.7)		(25.3)	
High-contact industry (0/1)	0.16		0.14	
	(0.01)		(0)	
N	2478		8326	

Note: The values presented in the table are based on the financial statements recorded at the end of 2019. Variable "High-contact industry" is a dummy variable, which gets value one if the firm's industry is one of the following: wholesale and retail; transportation and storage; accommodation and food services; arts, entertainment and recreation; and other services. Standard errors in parentheses. See text for details on data restrictions.

descriptive statistics of these labour productivity percentiles in Table 2. Notably, the observed differences between rejected and accepted firms remain qualitatively consistent whether we examine these differences in terms of Euros or percentile rankings.

4 Empirical Strategy

4.1 Decision-Maker Leniency Design

To motivate our empirical strategy, consider the following example for estimating the effects of winning a COVID-19 subsidy on an outcome Y_{it}, such as sales:

$$Y_{it} = \beta_0 + \beta_1 Accepted_i + \beta_2 \mathbf{X}_{it} + \varepsilon_{it}, \tag{1}$$

where i indexes firms and t years. $\mathbf{X}_{it}$ represents a vector of firm-level control variables measured in 2019, and ε_{it} is the error term. With ordinary least squares (OLS) estimation, the endogeneity problem arises due to the non-random assignment of subsidy decisions, leading to a biased estimate of β_1.

For instance, if firms with better post-pandemic prospects — even in the absence of the subsidy — are more likely to win one, the treatment effect would be overestimated. It is likely that the broad application and firm data might still only partially capture some characteristics affecting the subsidy official's decision, rendering simple OLS estimation biased.

To address this issue, we construct an instrument based on the leniency of the officials handling the applications. As outlined in Section 2, the assignment of applications to officials was random. Officials would process applications in the order they were received, and after deciding on one, they would move to the next. Decision-makers were not allowed to select from a pool of pending applications or to abandon an application once assigned, except in exceptional cases such as illness.

Furthermore, the decision-making process granted officials considerable subjective authority in determining acceptance. This led to notable variations in the likelihood of acceptance across different decision-makers. As a result, for each subsidy application, the official handling it—and consequently, the probability of its acceptance (conditional on the quality of the application)—was effectively randomised.

Following the recent literature on judge-leniency designs (Dobbie and Song, 2015; Aizer and Doyle Jr, 2015; Cheng et al., 2021; Dobbie et al., 2018; Norris et al., 2021; Bhuller et al., 2020; Dahl et al., 2014; Huttunen et al., 2022), we start with a residualised leave-out mean of all other subsidy applications the official has handled. Despite the seemingly random application assignment mechanism, potential factors could lead to selection problems.

First, officials worked in shifts around each day of the week (also during weekends) alongside their other duties. If more experienced decision-makers were busier and, consequently, had to work during the weekends, applications sent on a Thursday or Friday might be more likely to be handled by more experienced decision-makers.[10] Additionally, acceptance rates were higher during the first days of the subsidy program and went down as the program rules were amended in the first months of the pandemic. To eliminate these threats to research design validity, we first residualised the application decisions by regressing the acceptance decision on day-of-the-week and week dummies.

The residuals of this regression can be interpreted as the leniency of each official unexplained by the systematic factors described above, plus idiosyncratic shocks. The mean of the residuals from all other applications the official has handled (future and past), excluding the application at hand, is calculated for each application. Formally:

$$z_{ij} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{k\in\mathcal{J}_j} (\text{Accepted}^*_k) - \text{Accepted}^*_{ij}\right), \tag{2}$$

where $\mathcal{J}_j$ is the set of subsidy applications assigned to official j with $|\mathcal{J}_j| = n_j$, and Accepted^*_{ij} the residual acceptance decision of decision-maker j for application (or firm) i. In our main results, the predicted decision-maker (judge) leniency measure z_{it} is used as an instrument for winning a subsidy.

4.2 Instrument Relevance and Validity

4.2.1 Instrument Relevance

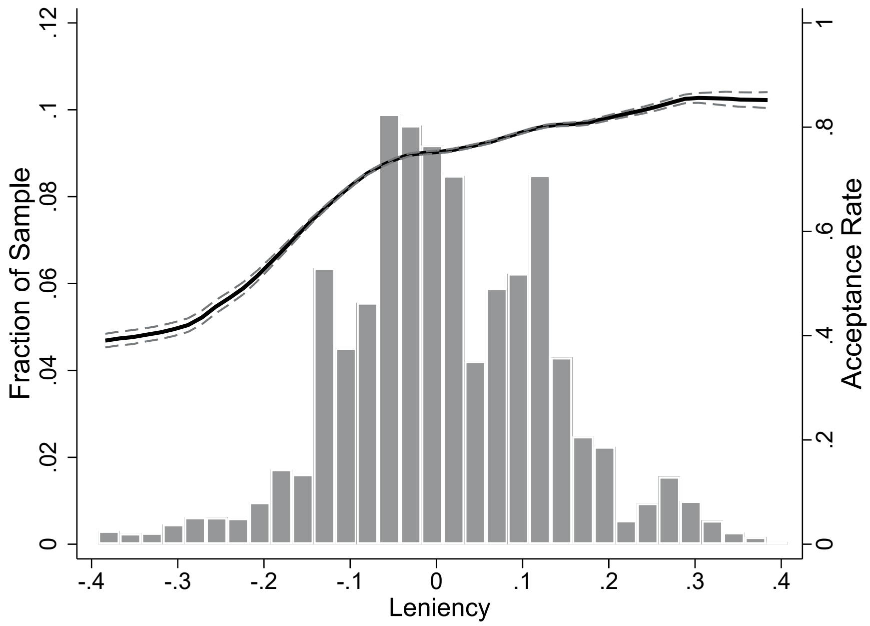
We begin examining the relevance of our instrument by plotting its distribution together with the estimated nonlinear first stage (Figure 3). The distribution of the predicted

[10] On average, it took two days after receipt for an application to be decided on.

official leniency measure, z_{it}, demonstrates the variation in leniency across officials.[11] Examining the relationship between the instrument and the probability of winning a subsidy is vital.

The first stage analysis reveals a strong positive correlation between an official's predicted leniency and the probability of an application's success. As depicted in Figure 3, transitioning from the least lenient to the most lenient decision-maker is associated with an approximate 40 percentage point increase in acceptance probability, rising from approximately 40% to 80%. Table 3 reports the estimation results and corresponding F-statistics for the first-stage regression, where we regress the Accepted dummy on the instrument. All columns indicate that the instrument is highly predictive of acceptance, even after controlling for firm-level observables.

Figure 3. First Stage Graph of Probability of Acceptance on Decision Maker Leniency.



Note: The histogram shows the density of the decision maker leniency along the left y-axis. The probability of acceptance is plotted on the right y-axis against the leave-out mean leniency of the assigned decision maker shown along the x-axis. The solid line shows a local linear regression of acceptance on decision-maker leniency. Dashed lines show 95% confidence intervals.

[11] Our data consists of 29 decision-makers, and the average number of funding decisions per decision-maker was 507.

Table 3. Test for Instrument Relevance

	(1)	(2)	(3)
Leniency	0.55***	0.46***	0.46***
	(0.05)	(0.05)	(0.05)
Week dummies	No	Yes	Yes
Day of week dummies	No	Yes	Yes
Firm level controls	No	No	Yes
F-statistic	119.31***	100.33***	97.6***

Note: Column (1) presents the estimation results of an OLS regression of the binary accepted variable on the estimated leniency calculated using Equation (2). Column (2) includes week and day-of-week dummies. Column (3) additionally includes the following control variables: equity, debt, sales, wage sum, FTE personnel, net profit, return on assets, labour productivity, and face-to-face industry. All control variables are measured at the end of 2019. The continuous variables are centred, and the reference level of the discrete variables is the sample mode. The significance levels are indicated by: . for $p < 0.10$, * for $p < 0.05$, and ** for $p < 0.01$, and *** for $p < 0.001$.

4.2.2 Instrument Validity

In addition to being predictive of acceptance, the instrument should only affect the outcome through its impact on the treatment variable and not other channels. This assumption is not directly testable. Nonetheless, Table 4 shows that decision-maker leniency is uncorrelated with firm-level observable characteristics prior to the pandemic. The first column of Table 4 reports the estimates of a regression model, where we have regressed the decision-maker leniency on variables calculated from applicant firms' financial statements in addition to week and day-of-week dummies. The coefficients on the regressors are statistically indistinguishable from zero, and the F-statistic for the joint significance of the regressors is far from significant. This suggests that firm pre-pandemic characteristics have no predictive power on the instrument, and lends support to the validity of the instrument.

In contrast, according to the second column of Table 4, the same set of regressors are considerably better predictors of acceptance, with several of the regression coefficients significantly different from zero.

The estimates in Table 4 support our assumption that the allocation of applications to decision-makers was genuinely random. Specifically, these results confirm that a firm's financial status prior to the pandemic did not influence which decision-maker was assigned to handle its application. This randomness is crucial for the validity of our instrument approach, ensuring that the assignment of decision-makers is not systematically related to the characteristics of the firms applying for the subsidy.

Table 5 displays the average subsidy size within our analysis sample. A comparison between the OLS and IV estimates shows they are nearly identical. This finding supports our hypothesis that the subsidy size does not vary with decision-maker leniency. In other words, it does not seem to be the case that a more lenient decision-maker would allocate larger subsidies than a stricter one.

5 Results

5.1 Firm-Level Results

We start by reporting the firm-level results in Table 6. All the results that follow in this section are from 2SLS regressions, where the treatment variable (acceptance dummy or granted subsidies in euros) is instrumented by the leniency measure introduced in Section 4. In addition, we control for the pre-pandemic firm characteristics reported in Tables 2 and 4. To limit the effect of outliers, we also top and bottom code all outcomes at 1% and 99% levels.

Starting from Panel A, according to the IV estimates, we find a positive but statistically non-significant effect of the subsidy on sales (measured at the end of financial years 2020 and 2021).

Panel B presents IV and OLS estimates on the subsidy's impact on the annual wage sum. According to the IV estimates, receiving the subsidy increases the wage sum by 398,000 EUR in 2020 and 354,000 EUR in 2021. These figures are notably high, especially considering the wage subsidy was capped at 100,000 EUR.

There are three potential explanations for this. Firstly, the estimates are associated with considerable standard errors, which implies that a considerably smaller 'true' effect could also give rise to our estimates. Secondly, it is also plausible that a short-term wage subsidy could yield long-term benefits by reducing layoff risks and enabling firms to maintain their original workforce. Lastly, considering the subsidy included a 25% self-financed portion, it is plausible to observe effects exceeding the subsidy amount. We discuss the possibility that the subsidy allowed firms to retain their workforce in greater

Table 4. Predictive Power of Covariates on the Instrument and Treatment Variables

	Leniency	Accepted
Intercept	-1.406	0.995***
	(0.960)	(0.046)
Equity (1000,000 eur)	0.024	0.002
	(0.024)	(0.001)
Debt (1000,000 eur)	-0.019	-0.001
	(0.014)	(0.001)
Sales (1000,000 eur)	-0.002	0.000
	(0.010)	(0.001)
Wage sum (1000,000 eur)	0.135	0.015*
	(0.141)	(0.007)
FTE personnel	-0.003	-0.000
	(0.006)	(0.000)
Net profit (1000,000 eur)	-0.045	-0.007
	(0.088)	(0.004)
Return on assets (%)	-0.000	0.000
	(0.0004)	(0.000)
Labor productivity (1000,000 eur)	-0.267	0.023
	(0.460)	(0.022)
High-contact industry (0/1)	-0.340	-0.037***
	(0.217)	(0.011)
Sample size	10804	10804
F-stat	0.99	4.29***

Note: The values presented in the table are based on the financial statements recorded at the end of 2019. Variable "Face-to-face industry" is a dummy variable, which gets value one if firm's industry is one of the following: wholesale and retail; transportation and storage; accommodation and food services; arts, entertainment and recreation; and other services. In addition to the variables reported, both columns include application week dummies and decision day-of-week dummy variables. Significance levels indicated by The significance levels are indicated by: . for $p < 0.10$, * for $p < 0.05$, and ** for $p < 0.01$, and *** for $p < 0.001$.

Table 5. Average Subsidy Size.

	(1)	(2)	(3)	(4)	(5)	(6)
	IV: Subsidy size (eur)			OLS: Subsidy size (eur)		
Intercept	-3521.84	-2690.54	110.86	0	-3265.16*	-2190.57
	(4273.94)	(6358.86)	(6340.59)	(506.46)	(1392.65)	(1433.44)
Accepted (0/1)	82749.81***	78339.05***	75883.12***	78179.79***	78947.88***	78334.83***
	(5535.79)	(6604.46)	(6609.31)	(576.92)	(634.83)	(626.50)
Week dummies	No	Yes	No	Yes	No	Yes
Day of week dummies	No	Yes	No	Yes	No	Yes
Firm level controls	No	No	Yes	No	Yes	Yes
Sample size	10804	10804	10804	10804	10804	10804

Note: This table presents the size of the subsidy. In Columns (1)-(3), the *Accepted* dummy is instrumented using the estimated decision maker leniency. Columns (4)-(6) present conventional OLS estimates. Columns (2) and (4) includes week and day-of-week dummies. Columns (3) and (6) also include the following control variables: equity, debt, sales, wage sum, FTE personnel, net profit, return on assets, labour productivity, and high-contact industry. All control variables are measured at the end of 2019. The continuous variables are centred, and the reference level of the discrete variables is the sample mode. The significance levels are indicated by: . for $p < 0.10$, * for $p < 0.05$, ** for $p < 0.01$, and *** for $p < 0.001$.

detail later when we study worker-level effects.

Panel C presents the impact of the subsidy on the full-time equivalent (FTE) personnel. Interestingly, neither the Instrumental Variables (IV) nor the Ordinary Least Squares (OLS) estimates show a positive effect of the subsidy on personnel. This outcome is somewhat at odds with the findings in Panel B, where the subsidy appeared to increase wages. However, it is important to note that FTE personnel, as reported in firms' financial statements, represents a year-end snapshot. This measure does not account for interim changes such as furloughs during the pandemic or layoffs followed by new hires within the same fiscal year.

In Panel D, we examine the effect of the development subsidy on labour productivity, defined as value added per FTE personnel. Given the extremely high variance in labour productivity, we have transformed this measure into percentile ranks within the labour productivity distribution of all Finnish firms. Additionally, we have excluded firms with fewer than five employees. The IV estimates suggest that the subsidy led to a decrease in labour productivity among subsidised firms. A plausible explanation is that while the subsidy did not contribute to an increase in turnover, it did lead to higher costs. This combination of factors likely resulted in the observed reduction in productivity.

To summarise, the IV estimates imply that the subsidy had a marginal positive effect on the wage sum and a negative effect on labour productivity. Moreover, the negative productivity effect was substantially more prominent in 2021 compared to 2020, suggesting that the adverse productivity effects lasted longer than a year.

Three important points about Table 6 are worth highlighting. Firstly, it is evident that the Instrumental Variables (IV) and Ordinary Least Squares (OLS) estimates differ in three out of the four sets of estimates, with sales as the dependent variable being the only exception. The OLS estimates encompass the entire sample, whereas the IV estimates specifically gauge the subsidy's impact on firms marginally affected by the subsidy. This suggests that the OLS intercept term represents an average across both 'never-takers' (firms that would not receive subsidies under any circumstances) and 'compliers' (firms whose applications are rejected when handled by a strict decision-maker). Similarly, the OLS *Accepted* dummy is an average over 'always-takers' (firms that would always receive subsidies) and compliers. Therefore, even when OLS and IV estimates are similar in magnitude, the causal interpretation of OLS estimates remains questionable. A comparison between the IV intercepts and means of the dependent variables in 2019 in the rejected subsample (reported in Table 2) reveals that they are very similar. This implies that receiving a subsidy is associated with an increase in the wage sum and a decrease in labour productivity. This contrasts the alternative scenario where being rejected for a subsidy would decrease the wage sum.

Furthermore, the fact that IV estimates are solely based on compliers offers an alternative explanation for the notably large IV estimates observed in the impact of the subsidy on the wage sum. If complier firms are more inclined to utilise the subsidy in a manner that increases their wage sum, the IV estimate captures this.

Finally, we highlight that relying on annual snapshots from firm financial statements can hide within-year changes, particularly in FTE employment. To address this limitation, we turn to studying monthly worker-level data from the income registry next.

Table 6. Firm-Level Regression Results

	(1) IV: 2020	(2) IV: 2021	(3) OLS: 2020	(4) OLS: 2021
Panel A: Sales (1000 eur)				
Intercept	2818***	3301***	2880***	3259***
	(814)	(937)	(165)	(191)
Accepted (0/1)	669	599	590***	651***
	(1024)	(1177)	(97)	(112)
Sample size	10685	10527	10685	10527
Panel B: Wage Sum (1000 eur)				
Intercept	368*	469**	571***	621***
	(143)	(158)	(29)	(32)
Accepted (0/1)	398*	354.	138***	161***
	(181)	(199)	(17)	(19)
Sample size	10685	10527	10685	10527
Panel C: FTE				
Intercept	22**	22*	18***	18***
	(8.2)	(9.3)	(1.7)	(1.9)
Accepted (0/1)	-6.3	-5.3	-0.5	0.28
	(10)	(12)	(0.97)	(1.1)
Sample size	10685	10527	10685	10527
Panel D: Labour Productivity Percentile Rank				
Intercept	55***	54***	52***	44***
	(5.4)	(5.1)	(1)	(1.1)
Accepted (0/1)	-3.2	-13*	0.92	0.19
	(6.6)	(6.4)	(0.63)	(0.67)
Sample size	7718	7155	7718	7155

Note: This table presents the IV and OLS regression results of the effect of winning a subsidy on firm-level outcomes. In addition to the variables reported, all regression models include acceptance week and day-of-week dummies and the following control variables: equity, debt, sales, wage sum, FTE personnel, net profit, return on assets, labour productivity, and high-contact industry. All control variables are measured at the end of 2019. The continuous variables are centred, and the reference level of the discrete variables is the sample mode. The significance levels are indicated by: . for $p < 0.10$, * for $p < 0.05$, and ** for $p < 0.01$, and *** for $p < 0.001$.

5.2 Worker-Level Results

We next turn to discuss results from worker-level regression analyses. In Table 7, we report the effect of winning a subsidy on workers employed at the firm pre-pandemic.[12]

Panel A of Table 7 shows the effect of winning a subsidy on the annual earnings of workers. The IV results suggest that workers employed at subsidised firms pre-pandemic earned an additional 6,152 euros in 2020. However, no statistically significant differences emerged in 2021 and 2022.

In Panel B, we examine how winning a subsidy affects cumulative unemployment months. Our findings indicate that workers from subsidised firms, on average, experienced 3.8 fewer months of unemployment compared to their counterparts in non-subsidised firms between April 2020 and December 2022. We also highlight that the difference between workers at subsidised and non-subsidised firms grows over time. This confirms the finding that displacement tends to have a long-term scarring effect on workers (Jacobson et al., 1993; Eliason and Storrie, 2006; Verho, 2020; Bertheau et al., 2023; Huttunen and Pesola, 2022).

Panel C reports the cumulative number of months workers spent with a new employer. While the IV estimates are negative, they are not statistically significant due to large standard errors.

Finally, in Panel D, we study whether the subsidy prevented workers from moving to more productive employers. We use a regression where the dependent variable is the labour productivity of the workers' employer.[13] The results indicate that workers associated with a subsidised employer pre-pandemic are employed at less productive firms up to three years after the pandemic in 2023.

The estimates in Tables 6 and 7 allow us to provide a back-of-the-envelope calculation for the cost of saving one month of employment. According to Table 7 Panel C column (1), the subsidy increased employment months by 1.1 in 2020. In addition, if we take into account that the average firm among compliers has 22 FTE workers (intercept in Table 6 Panel C), we can contrast these numbers with the average subsidy among compliers

[12]Current econometric theory or results do not provide robust methods for handling cluster robust standard errors in an IV context (MacKinnon et al., 2023). We aggregate worker-level observations to (pre-pandemic employer) firm level to avoid the need for clustering at the cost of reduced statistical power.

[13]Our data set does not include firm financial statements for 2023, so we utilise observations from 2022 for this year.

€75,883 (see Table 5, Column (3)). Thus, the implied cost of saving one month of employment in 2020 can be calculated as $\frac{€75,883}{22 \times 1.1} \approx €3,134$. Thus, according to our estimates, preserving 12 months of employment cost approximately € 37,600, which is very close to the median annual earnings in Finland.

We note, however, that our estimates imply that laid-off workers have a risk of not returning to employment, as evidenced by the divergence between treatment and control workers in terms of unemployment months. This indicates that, over a longer timeframe, the cost of preserving a month of employment could be even lower, and the € 37,600 price tag could be interpreted as an upper bound.

Yet, extending the comparison period complicates the cost-benefit analysis, particularly as other COVID-19-related subsidies, often conditional on not receiving a Business Finland development subsidy, were introduced subsequently. Additionally, the 2020 estimate is most directly comparable to the PPP's reported cost of saving one job at $150,000, as cited in Autor et al. (2022c).

It is also crucial to acknowledge that our cost-benefit calculation is inherently limited to the complier population. If we make the reasonable assumption that the impact of subsidies is less pronounced among always-takers, the average cost of saving a month of employment would be higher when considering a combined population of compliers and always-takers.

6 Conclusions

This paper examines the wide-ranging impacts of a crisis program, demonstrating its effectiveness in preserving jobs and organisational capital. The program also proved to be cost-efficient: Preserving 12 months of employment cost under €40,000, a fraction of the cost associated with the Paycheck Protection Program (PPP), where the cost estimates are over $150,000.

However, the program's success in job protection led to an unintended consequence. While the wage sums in subsidised firms increased, these firms did not exhibit higher turnover compared to their non-subsidised counterparts. This suggests that while the subsidy enabled firms to retain their workforce, this did not translate into increased sales. Therefore, our findings also reveal a significant trade-off. By focusing on employment rather than sales, subsidised firms experienced reduced labour productivity, which

Table 7. Worker-Level Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Annual earnings						
	2020	2021	2022	2020	2021	2022
	IV: Average salary (eur)			OLS: Average Salary (eur)		
Intercept	32451***	40290***	40088***	34865***	37285***	40505***
	(3889)	(4700)	(4438)	(1391)	(1542)	(1624)
Accepted (0/1)	6152.	5620	4096	3660***	3664***	3666*
	(3697)	(4485)	(4227)	(383)	(424)	(447)
Sample size (firms)	10518	10518	10518	10518	10518	10518
Sample size (workers)	234395	234395	234395	234395	234395	234395
Panel B: Unemployment months						
	2020	2020-2021	2020-2022	2020	2020-2021	2020-2022
	IV: Months unemployed			OLS: Months unemployed		
Intercept	2.9***	5.8***	8.9***	2.4***	4.7***	6.9***
	(0.54)	(1.1)	(1.6)	(0.23)	(0.45)	(0.66)
Accepted (0/1)	-1.1*	-2.2*	-3.8*	-0.57***	-1.1***	-1.6***
	(0.5)	(0.98)	(1.4)	(0.048)	(0.094)	(0.14)
Sample size (firms)	10518	10518	10518	10518	10518	10518
Sample size (workers)	234395	234395	234395	234395	234395	234395
Panel C: New employer						
	2020	2020-2021	2020-2022	2020	2020-2021	2020-2022
	IV: Months with new employer			OLS: Months with new employer		
Intercept	1.6***	5.1***	10**	1.2***	4.1***	8.5***
	(0.38)	(0.96)	(1.7)	(0.16)	(0.4)	(0.71)
Accepted (0/1)	-0.46	-1.4	-2.3	-0.13***	-0.31***	-0.47***
	(0.35)	(0.89)	(1.5)	(0.034)	(0.085)	(0.15)
Sample size (firms)	10326	10326	10326	10326	10326	10326
Sample size (workers)	233342	233342	233342	233342	233342	233342
Panel D: Labour productivity (percentile)						
	2020	2021	2022	2020	2021	2022
	IV: Labour productivity (percentile rank)			OLS: Labour productivity (percentile rank)		
Intercept	49***	67***	65**	44***	48***	50***
	(7.8)	(7.8)	(7.1)	(3.3)	(3.1)	(2.9)
Accepted (0/1)	-3.6	-18*	-15*	2.1**	1.2.	-0.88
	(7.3)	(7.2)	(6.6)	(0.7)	(0.66)	(0.62)
Sample size (firms)	8782	8933	9084	8782	8933	9084
Sample size (workers)	224904	208898	200052	224904	208898	200052

Note: This table presents the IV and OLS regression results on worker-level outcomes. The units of observation are averages calculated over workers affiliated with the observation firm pre-pandemic. In Panel D, only firms with at least five workers are included in the data. In addition to the variables reported, all regression models include acceptance week and day-of-week dummies and the following control variables: equity, debt, sales, wage sum, FTE personnel, net profit, return on assets, labour productivity, and face-to-face industry dummy. All control variables are measured at the end of 2019. The continuous variables are centred, and the reference level of the discrete variables is the sample mode. The significance levels are indicated by: . for $p < 0.10$, * for $p < 0.05$, ** for $p < 0.01$, and *** for $p < 0.001$.

persisted for up to two years post-pandemic. Additionally, the subsidy's effect on job transitions meant workers were more likely to remain in less productive firms. In sum, while the program had several positive aspects, particularly in terms of cost-effectiveness and job preservation, it most likely had a net negative impact on the overall labour productivity in the economy.

It is crucial to contextualise the implementation of the subsidy within the highly uncertain environment of the early stages of a global pandemic, an event of almost unparalleled scale in recent history. While the program may not have fully succeeded in its primary objective of fostering new profitable businesses, it unarguably achieved its secondary goal of preventing layoffs. This accomplishment is significant, considering the economic turmoil of the period.

Finally, we highlight that our approach does not account for any general equilibrium effects of the subsidy program. A comparison between accepted and rejected compliers cannot fully capture what might have happened had the program not existed at all. Consequently, we cannot discount the possibility that the program played a crucial role in averting mass layoffs, which could have had severe economic consequences. Despite certain limitations, the program's contribution to employment stability during a turbulent period remains significant.

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Elinkeinoelämän tutkimuslaitos

ETLA Economic Research

ISSN-L 2323-2420
ISSN 2323-2420 (print)
ISSN 2323-2439 (pdf)

Publisher: Taloustieto Oy

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