Traps and Springboards: The Mobility Effects of Crisis Subsidies*

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Abstract

Emergency firm subsidies aim to preserve employment but may also reshape worker reallocation. We provide micro-level causal evidence using Finland's largest COVID-19 subsidy program, exploiting quasi-random assignment of applications to case officers of differing stringency. The same subsidy increased upward mobility for workers in high-productivity firms but reduced it for workers in low-productivity firms. At low-productivity firms, subsidies increased survival and retained workers who would otherwise have moved to more productive employers. At high-productivity firms, subsidies did not alter the frequency of separations but improved their quality. Whether crisis subsidies act as traps or springboards depends on recipient firm productivity.

Keywords: Subsidies, Productivity, Reallocation, COVID-19
JEL: H25, H32, E24, J62

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1 Introduction

Government subsidies during economic crises are intended to stabilize employment and preserve productive capacity. Yet, such interventions risk sustaining low-productivity firms and impeding the reallocation of labor toward more productive uses (Caballero and Hammour, 1996), a channel linked to aggregate productivity growth (Foster et al., 2001; Haltiwanger et al., 2025). Micro-level causal evidence on how firm subsidies affect the direction of worker flows across the productivity distribution remains limited.

We provide such evidence, exploiting a natural experiment embedded in Finland's largest COVID-19 emergency subsidy program, which granted up to EUR 100K to small and medium-sized firms. Applications were quasi-randomly assigned to case officers who differed in their approval stringency under subjective acceptance rules. Because this variation strongly predicts funding but is unrelated to firms' pre-existing characteristics, we use it as an instrument for subsidy receipt, following the judge-IV approach (Chyn et al., 2025).[1] We link the subsidy data to administrative employer–employee records and track incumbent workers' monthly earnings and employers for up to five years, measuring firm productivity as value added per worker.

On average, subsidies had limited detectable effects on firm performance. Grants increased survival and employment among low-productivity firms but had no detectable effect on revenues or labor productivity for either group. The main results concern worker reallocation, where the average masks sharp heterogeneity by pre-pandemic firm productivity. For workers in high-productivity firms, subsidies increased upward mobility: they were more likely to transition to higher-productivity employers and subsequently spent more time and earned more income at such firms. For workers in low-productivity firms, we find the opposite: subsidies reduced the probability of moving to a more productive employer and lowered both time spent and income earned at higher-productivity firms.

The mechanisms differ by firm type. Among low-productivity firms, the subsidy operated through retention: it kept marginal firms alive and prevented the separations that would otherwise have moved workers to more productive employers. This effect was strongest in rural labor markets, where high-productivity alternatives are scarce and the displacement the subsidy prevented may have been one of few realistic paths to a more productive employer. At high-productivity firms, the channel was not retention—the subsidy had no detectable effect on firm survival, and workers separated at similar rates regardless of treatment. Yet conditional on separating, they transitioned to more productive employers. The subsidy appears to have preserved the conditions under which productive separations occur—a compositional effect on the quality of transitions rather than their frequency. These patterns correspond to two competing reallocation forces identified in the macro literature: delayed cleansing among low-productivity firms (Caballero and Hammour, 1996) and a partial offset of the sullying effect among high-productivity firms (Barlevy, 2002).

Our results contribute to a growing literature on crisis subsidies and labor reallocation. The Paycheck Protection Program remains the most studied pandemic scheme, with evidence generally indicating that it preserved jobs at a cost of $150,000–$360,000

[1] For applications of judge-leniency designs in other contexts, see, 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); Sampat and Williams (2019).

per job saved.[2] This work largely focuses on job preservation rather than how subsidies redirect worker flows across the productivity distribution. Closest to our paper are Barrot et al. (2024), who find that French loan guarantees dampened reallocation toward more productive firms, and Giupponi and Landais (2023), who show that Italian short-time work subsidies generated long-run reallocation costs, both studying the financial crisis. Short-time work schemes subsidize firms to retain workers and mechanically preserve specific worker-firm matches; the Finnish program instead provided general liquidity with no direct employment conditionality, yet we find reallocation costs operating through firm survival rather than mandated retention.

We show that these costs are concentrated among workers in low-productivity firms, consistent with concerns that loosely targeted crisis funding sustains "zombie firms" (Banerjee and Hofmann, 2018), trapping workers in them. Crucially, the same subsidies directed at high-productivity firms acted as springboards, increasing upward mobility—and because firm productivity is observable at the time of allocation, these reallocation costs are not inevitable. Whether crisis subsidies trap or propel depends on the productivity of the firm that receives them.

2 Institutional Context

2.1 Background of the Subsidy Program

This paper studies Business Finland's "Business Development in Disruptive Circumstances" program, a few-strings-attached subsidy targeted at SMEs and midcap companies employing between 6 and 250 people. The motivation for the program was to provide emergency liquidity for the Finnish corporate sector before the proper legislative framework for business support 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 development plan. In practice, any qualifying firm that submitted a project plan focused on describing a new development activity specific to that firm was considered eligible. The program therefore combined emergency liquidity with minimal innovation-style screening, and its effects would plausibly vary across firms with different productivity and growth prospects.

The funding scheme was in operation between March and June 2020, after which a legal framework for direct cost support was in place and the program was phased out (Koski et al., 2022). It consisted of two separate grants: a pre-analysis grant capped at EUR 10K and a development grant capped at EUR 100K. Neither paid advances; Business Finland retroactively reimbursed expenses—limited to payroll and external services—up to a budget specified in the application, with a 25% self-financing requirement. The total budget was roughly EUR 1 billion (0.4% of Finnish GDP in 2020), accounting for around 45% of all pandemic-related firm subsidies. A total of

[2] See, among others, Autor et al. (2022a); Bartik et al. (2021); Chetty et al. (2020); Dalton (2021); Doniger and Kay (2023); Faulkender et al. (2020); Granja et al. (2022); Hubbard and Strain (2020); Joaquim and Netto (2021); Kurmann et al. (2021); Li and Strahan (2020). Enami and Ghosh (2024) find that PPP loans boosted firm exports, and Agarwal et al. (2024) show that the program reduced mortgage delinquencies. For a broader assessment of U.S. business support during COVID-19, see Chodorow-Reich et al. (2022).

25,921 applications were submitted by 23,322 firms, about two-thirds of which received funding—a sharp departure from pre-pandemic conditions, when the acceptance rate was nearly 100%.

The program shares key characteristics with the U.S. Paycheck Protection Program (PPP): both offered subsidies for payroll with relatively lax criteria, targeted small and medium-sized companies, and aimed at protecting jobs. A key difference is that the PPP conditioned loan forgiveness on maintaining payroll, whereas the Business Finland program formally tied support to forward-looking development projects.[3]

2.2 Quasi-Random Allocation of Applications to Case Officers

Firms submitted applications through Business Finland's online portal. Each application was assigned to a case officer who reviewed it and recommended acceptance or rejection. These recommendations were then signed off by a separate authorizing officer; in practice, recommendations were almost never overturned. We thus construct our instrumental variable based on case officer leniency.[4]

Two institutional features generate the variation we exploit. First, there was substantial subjectivity in the acceptance criteria. Beyond objective grounds for rejection—financial distress, exceeding prior public funding limits, or insufficient operating history[5]—many applications were rejected for subjective reasons, such as insufficient novelty or inadequate detail in the project plan. An audit by the National Audit Office found that even the objective criteria were applied inconsistently across officers (National Audit Office of Finland, 2021), in part reflecting the pace of processing: a bonus scheme required officers to complete at least 50 decisions per week, leaving roughly 42 minutes per application. Second, the allocation of applications to case officers was approximately random. Under normal circumstances, officers specialize by industry, but during the pandemic, applications were allocated on a first-come, first-served basis. The acceptance decision was also binary—officers could not vary funding amounts—so leniency shifts only the extensive margin of subsidy receipt.

Two features of this allocation require refinement in constructing the instrument. Some officers may have selectively chosen applications from familiar industries,[6] and acceptance standards tightened substantially over the program's duration (from above 90% to roughly 25%). We address both concerns by residualizing acceptance decisions on industry and application-week fixed effects when constructing the leniency measure. Our main identifying assumption is therefore that, conditional on industry and application week, the assignment of applications to case officers is as good as random. This allows officers to have handled different mixes of industries, as long as within a given industry and week, which specific application landed on which officer's

[3]For details on the PPP conditions, see Autor et al. (2022b). The programs also differed in scale: the PPP's budget of $800 billion (3.7% of U.S. GDP) dwarfed the Finnish program.

[4]During this period, case officers worked remotely and had minimal interaction with one another, so decisions were mostly made independently. Any coordination across officers would only reduce variation in leniency and weaken the instrument's relevance without violating exogeneity.

[5]EU state-aid regulations prohibited support to firms in financial distress, and EU competition rules capped *de minimis* aid at EUR 200K over three fiscal years (Business Finland, 2023a,b).

[6]Interviews with case officers revealed that some, in order to manage workload pressures, occasionally selected applications from industries they were familiar with.

desk was effectively arbitrary. We restrict the sample to case officers who handled at least 30 applications.[7]

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. Development grants were the larger subsidy type (EUR 100K) and thus more likely to have meaningful effects. We keep only the first application per firm, retain only applications that received an acceptance or rejection decision, and restrict the sample to firms with between 6 and 250 full-time equivalent employees in 2019—the program's target.[8]

The application-level subsidy data contain a firm identifier, the application date, the grant amount applied for, and the acceptance decision. We also observe the case officer handling each application, allowing us to construct our leniency instrument.

The firm identifier links the subsidy data to administrative records from Statistics Finland. Firm-level balance sheet data—covering value added, revenues, employment, wage sums, and debt—provide pre-pandemic firm characteristics measured in 2019 and a yearly panel through 2023. We supplement these with the Finnish Tax Administration's Income Register, which records earnings paid by each employer in each month through May 2025. We define a firm as active in a given month if it has any income register records; a firm is classified as having exited once it no longer appears in the register.[9] For exited firm-years, we set employment, revenue, and value added to zero.

We define labor productivity at the firm level as value added per full-time employee.[10] To reduce noise, our main specifications use labor productivity percentile ranks computed over the universe of firms with balance sheet data, not just the analysis sample.

We identify incumbent workers using Statistics Finland's FOLK employment register as all individuals employed at sample firms at the end of 2019, before the onset of COVID-19 in Finland and roughly three months before any subsidies were granted. We further restrict to workers who remain employed at the sample firm one month before the application date, ensuring that we study workers attached to the firm at the time of treatment. We track each incumbent's monthly earnings across all employers using the Income Register, identifying the main employer as the highest-paying firm

[7] Results are robust to this threshold; 94% of case officers handled at least 30 cases (median 129, mean 147).

[8] Despite the explicit eligibility restriction, some applications outside this range received funding. We exclude them because they introduce noise to the leniency estimate and are likely unrepresentative of reallocation dynamics.

[9] Formal exits are extremely rare in our data period. The government introduced a temporary moratorium on bankruptcies, and public authorities did not file for bankruptcies in cases of unpaid liabilities. Dissolving a company in Finland also entails nontrivial administrative costs. Our payroll-based definition captures economically meaningful shutdowns even when formal legal exits do not occur.

[10] Since our sample spans all sectors, estimating TFP is unattractive; firms in services likely have vastly different production functions from those in manufacturing, for example.

in each month. For brevity, we refer to months with no income register records as unemployment months.[11]

To measure worker reallocation, we compare the labor productivity of each worker's current employer to that of their baseline firm. A "higher-productivity firm" is any employer whose 2019 labor productivity percentile rank exceeds that of the worker's baseline firm. We use fixed 2019 percentiles rather than year-specific percentiles, which are post-treatment variables. Appendix Table A5 shows that results are qualitatively similar with year-specific percentiles. We describe the construction of worker-level outcome variables in detail in Section 5.2.

For the IV estimation, worker-level outcomes are aggregated to the firm level by averaging across incumbents. The unit of observation in all regressions is therefore the firm, while the interpretation pertains to the average incumbent worker.[12]

3.2 Descriptive Statistics

Appendix Table A1 reports baseline summary statistics. Sample firms are broadly similar to other Finnish firms of comparable size, employing 24.85 full-time workers and generating EUR 4.88M in revenue in 2019, with typical labor productivity and labor share. Comparing firms above and below the in-sample median labor productivity—the key heterogeneity dimension in our analysis—high-productivity firms have substantially higher revenue and value added but similar employment, confirming that the productivity split captures meaningful differences in efficiency rather than workforce size. Estimated pre-treatment characteristics of compliers show that they are smaller and more leveraged than other sample firms, but their labor productivity and labor share are close to the sample averages, suggesting that the complier subpopulation is not drawn from an unusual part of the productivity distribution.

4 Empirical Strategy

4.1 Case Officer Leniency Design

We measure case officer leniency using the residualized leave-one-out mean approval rate. Let i index applications and j index case officers. We residualize each acceptance decision on industry and application-week fixed effects (see Section 2):

$$Accepted^*_{ij} = Accepted_{ij} - \pi \mathbf{W}_i, \tag{1}$$

where $\mathbf{W}_i$ denotes week and industry dummies. The leniency instrument is then the leave-one-out mean of these residuals across all other applications handled by the same case officer:

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

[11] A zero-earnings month may not meet the standard International Labour Organization (2013) definition of unemployment; it may reflect furlough with zero hours or non-participation.

[12] Since the firm is the unit of treatment assignment, we aggregate worker-level outcomes to the firm level, giving each firm equal weight regardless of workforce size.

where $\mathcal{J}_j$ is the set of subsidy applications assigned to case officer j with $|\mathcal{J}_j| = n_j$. This leave-one-out mean isolates officer-specific approval propensity: firms assigned to more lenient officers face a higher probability of acceptance solely because of the officer's underlying tendency, generating exogenous variation in subsidy receipt. The 6,775 applications used to construct the leniency measure were handled by 46 case officers, with an average caseload of 147 applications; 94% of officers handled at least 30 applications and contribute to identification.

4.2 Validity of the Empirical Design

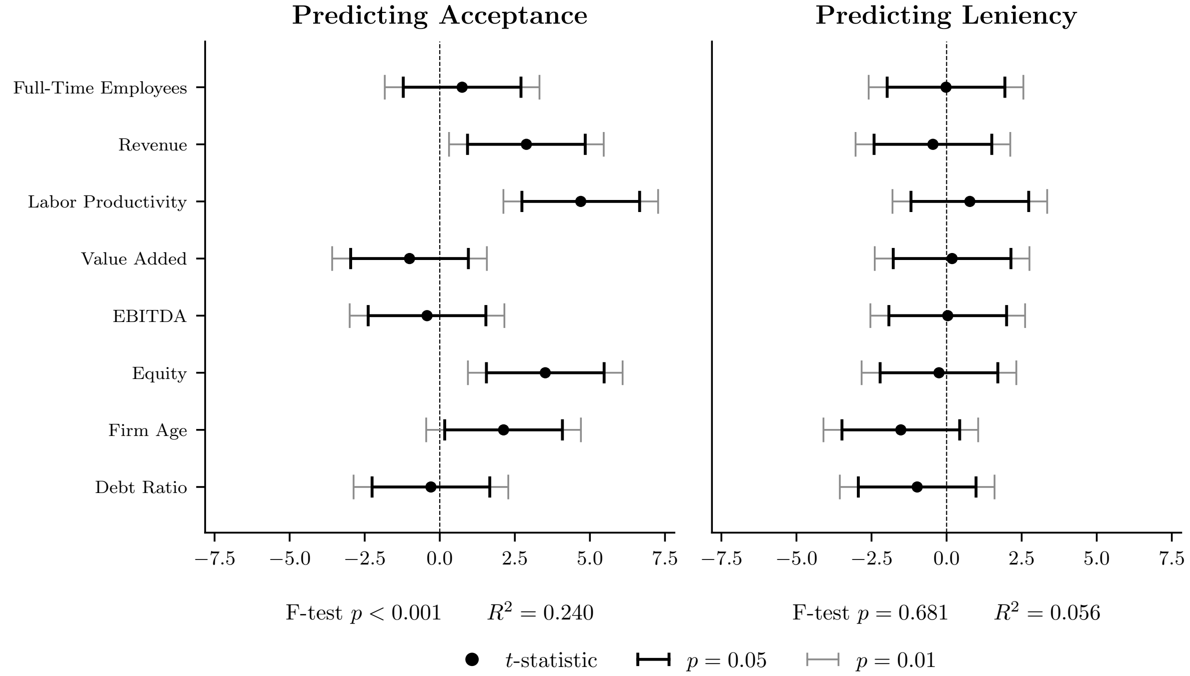
Following Chyn et al. (2025), identification in examiner designs rests on conditional random assignment, instrument relevance, and the exclusion restriction. To interpret the estimates as local average treatment effects (LATE), we also require monotonicity. We discuss each in turn.

Conditional Random Assignment. Figure 1 provides empirical support for conditional randomness of the instrument.[13] While baseline firm characteristics strongly predict acceptance—labor productivity, equity, and revenue are individually significant, and all covariates are jointly highly significant ($p < 0.001$)—the same characteristics have no detectable relationship with the leniency instrument. No covariate is individually significant at conventional levels, and the joint F-test fails to reject orthogonality ($p = 0.681$).[14]

[13] Appendix Table A2 reports the underlying coefficient estimates and standard errors.

[14] A natural complementary check would be to test whether officer leniency predicts worker mobility outcomes in a pre-treatment period. This is not feasible in our setting: the income register, our source for monthly worker-level employment and earnings, began in January 2020—only two months before the subsidy program launched—leaving insufficient pre-treatment data to construct meaningful mobility outcomes.

Figure 1. Test of Conditional Random Assignment

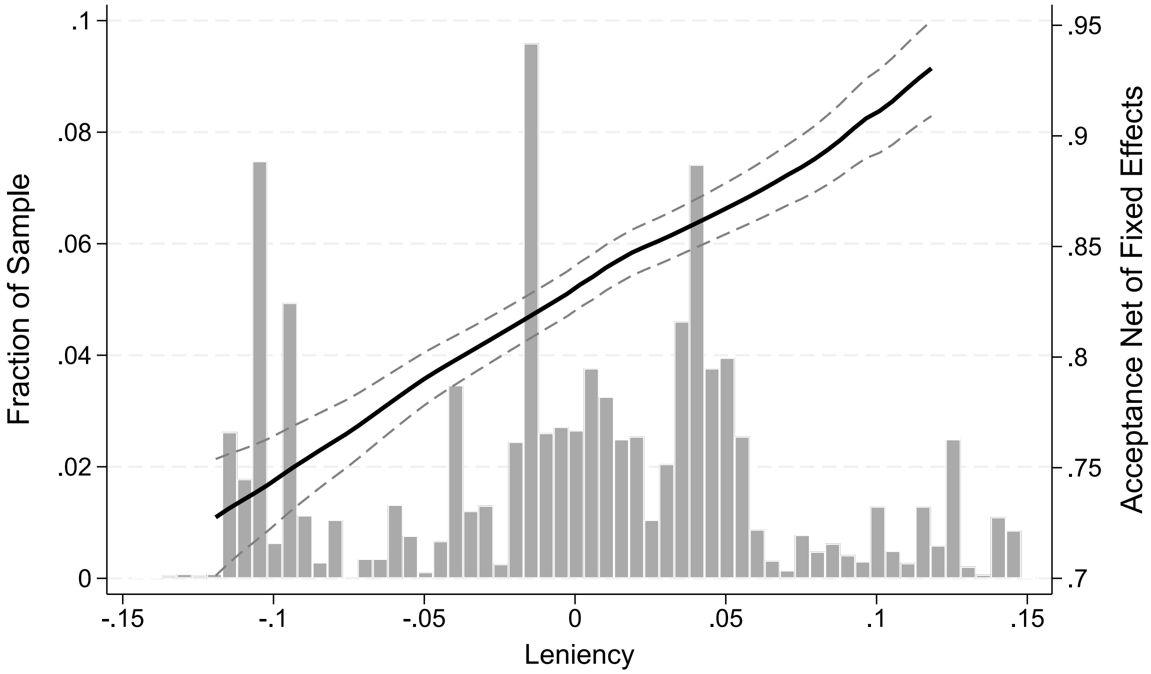


Notes: Each panel plots the t-statistic (coefficient divided by standard error) from a regression on 2019 firm characteristics, with application-week and industry fixed effects. The left panel uses the binary acceptance indicator as the dependent variable; the right panel uses the leniency instrument. The inner interval marks the 5% critical values (± 1.96); the outer interval marks the 1% critical values (± 2.576). The coefficient is statistically significant at a given level if zero falls outside the corresponding interval. The F-test evaluates the joint significance of the covariates, excluding the fixed effects. The R^2 reports the share of variation in the dependent variable explained by the covariates after partialling out application-week and industry fixed effects.

Instrument Relevance. Figure 2 plots the distribution of z_{ij} together with a local linear estimate of the first-stage relationship between leniency and acceptance, net of application-week and industry fixed effects. Because the leave-one-out construction excludes each firm's own acceptance decision from its assigned leniency, the positive relationship in the figure is not mechanical—it reflects genuine variation in officer stringency. Moving from the least to the most lenient case officer is associated with an approximately 25 percentage point increase in acceptance probability (from 70% to 95%). Appendix Table A3 reports the corresponding first-stage regressions; in our preferred specification, the coefficient of leniency on subsidies paid (EUR K) is 76.81 ($p < 0.001$).[15]

[15] The interacted specification in Equation 3 requires the instrument to predict treatment in both subgroups. Estimated separately by productivity group, the first-stage coefficient of leniency on acceptance is similar in magnitude and highly significant for both high- and low-productivity firms.

Figure 2. Leniency Distribution and the First Stage



Notes: The histogram shows the density of case officer leniency along the left y-axis. The probability of acceptance net of industry and week fixed effects is plotted on the right y-axis against the leave-one-out mean leniency of the assigned case officer shown along the x-axis. Because the leave-one-out mean excludes each firm's own acceptance decision, the plotted relationship reflects genuine variation in officer stringency. The solid line shows a local linear regression of acceptance on case officer leniency. Dashed lines show 95% confidence intervals.

Exclusion Restriction. The exclusion restriction requires that officer assignment affect firm and worker outcomes only through subsidy receipt. While inherently untestable, two institutional features support this assumption. First, case officers' role was limited to evaluating applications; they did not provide advisory services or post-award oversight that could independently affect firm performance (National Audit Office of Finland, 2021). Second, the acceptance decision was binary—officers could not vary the funding amount conditional on approval—so leniency shifts only the extensive margin of subsidy receipt. To ensure consistency with the exclusion restriction, we include the fixed effects $\mathbf{W}_i$ used in the residualization step (Equation 1) in all regressions.

Monotonicity. To interpret the IV estimates as LATE, a monotonicity assumption is needed. We rely on the weaker condition of *average monotonicity* (Frandsen et al., 2023), which requires only that the average leniency of officers who would approve a given application exceeds that of officers who would not. This allows occasional disagreements across officers as long as less lenient officers do not systematically rank certain applications higher than more lenient officers. Figure 2 shows that acceptance probability is monotonically increasing in leniency, a necessary but not sufficient condition.

Under these conditions, our estimates identify the LATE for complier firms—those whose subsidy receipt is determined by officer leniency. Compliers are smaller and more leveraged than the sample average but, crucially, similar in labor productivity (Table A1), the dimension along which the springboard and trap effects are defined.

Effects for always-takers (the majority of funded firms) cannot be recovered from our design.

5 Effects of Subsidies on Firms and Workers

5.1 Firm-Level Results

We first examine whether the subsidy affected employment, revenue, labor productivity, and survival at the firm level. We estimate the following linear IV regression:

$$Y_i = \alpha + \beta_1 Subsidy_i + \beta_2\, Subsidy_i \times LowProd_i + \gamma\, LowProd_i + \lambda_w + \delta_s + \varepsilon_i, \qquad (3)$$

where Y_i is an outcome for firm i, $Subsidy_i$ is the subsidy amount received, in units of EUR 100K (so that $Subsidy_i = 1$ corresponds to the maximum grant), and $LowProd_i$ is an indicator for firms whose in-sample labor productivity was below the median in 2019.[16] $Subsidy_i$ and $Subsidy_i \times LowProd_i$ are instrumented with case officer leniency and its interaction with $LowProd_i$. λ_w and δ_s are application-week and industry dummies, respectively. Standard errors are heteroskedasticity-robust; since both treatment assignment and outcomes are at the firm level, no clustering is required. The coefficient β_1 captures the effect for high-productivity firms; the effect for low-productivity firms is $\beta_1 + \beta_2$.

The results are presented in Table 1. The subsidy had no statistically significant effect on firm revenue or labor productivity for either group. For labor productivity, the point estimates are negligible—well under one percentile rank—with standard errors an order of magnitude larger. For low-productivity firms, however, the subsidy significantly increased headcount by roughly 14 workers from a control mean of 18.2, and raised the wage bill by a proportionally smaller amount, suggesting that the marginal employment was not full-time. The subsidy also strongly supported survival among low-productivity firms: the estimates imply a 29 percentage point increase from a control mean of 0.75, nearly closing the gap to full survival.[17] The large magnitudes are consistent with the financial fragility of complier firms, whose average equity (EUR 108K, Table A1) is close to the maximum grant size—the subsidy approximately doubled their equity buffer during a period of acute liquidity stress.

We find no statistically significant effects on any outcome for high-productivity firms. The point estimates are noisy, however, so we cannot rule out economically meaningful effects in either direction. The subsidies appear to have achieved their stabilization goal only for low-productivity firms—those unlikely to be the primary target of the program.

[16] Results are virtually identical when using a binary indicator for subsidy receipt, since the acceptance decision was binary and the grant amount was capped. Results are also qualitatively similar when using the economy-wide median productivity threshold.

[17] The implied treated complier mean slightly exceeds one, reflecting imprecision in both the estimated effect and control complier means.

Table 1. Effects of Subsidies on Firm-Level Outcomes

	(1) Monthly Workers	(2) Monthly Wage Bill	(3) Yearly Revenue	(4) Labor Prod. Percentile (2023)	(5) Survived Until 2025
Subsidy	-2.067	-24.63	746.9	-0.00590	-0.155
	(6.645)	(25.54)	(1013.3)	(7.022)	(0.134)
Subsidy × Low Prod.	15.84*	63.34*	-23.31	0.447	0.441**
	(7.856)	(28.88)	(1181.0)	(8.478)	(0.161)
High Prod. Control Mean	*12.50*	*49.28*	*2469.1*	*64.47*	*0.813*
Low Prod. Control Mean	*18.18*	*56.14*	*2342.8*	*52.89*	*0.751*
N	6,056	6,056	5,754	5,922	6,374
KP F-stat	27.02	27.62	31.06	23.84	24.57

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: 2SLS estimates from Equation 3. All specifications include application-week and industry fixed effects and a below-median labor productivity indicator. Columns 2 and 3 are in EUR K. *Subsidy* gives the effect of subsidy receipt for high-productivity firms (above the in-sample median of value added per full-time employee in 2019); adding the coefficient on the interaction term gives the effect for low-productivity firms. Outcome timing varies by data source: Columns 1–2 are averaged over the full post-application follow-up period through mid-2025; Column 3 is averaged over 2021–2023, the available financial statement years; Column 4 is the economy-wide labor productivity percentile rank as of 2023 (available only for active firms); Column 5 is an indicator for any payroll activity in 2025. Sample sizes vary across columns because revenue and labor productivity require financial statement data unavailable for all firms. Firm-level outcomes are winsorized at the 95th percentile. *High Prod. Control Mean* and *Low Prod. Control Mean* report complier control means for above- and below-median productivity firms, estimated following Abadie (2003). The last row reports the Kleibergen-Paap F-statistic for the strength of the constructed leniency instrument; standard critical values may not directly apply in examiner designs (Chyn et al., 2025), but the magnitudes suggest the instruments are not weak.

5.2 Worker-Level Results

We now turn to worker-level outcomes, examining employment attachment and reallocation across the productivity distribution. All worker-level outcomes are aggregated to the firm level by averaging across incumbents before estimation, as described in Section 3.

Employment. For each incumbent worker, we count the total months during the follow-up period spent in each of three states: at the baseline firm (B), at other firms (O), and out of employment (U). We index firms by i and workers employed at firm i at the end of 2019 as $n \in \mathcal{I}_i$, with $|\mathcal{I}_i|$ denoting the number of incumbents. The firm-level averages are:

$$\begin{aligned} \bar{M}_i^B &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} M_{ni}^B, \\ \bar{M}_i^O &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} M_{ni}^O, \\ \bar{M}_i^U &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} M_{ni}^U. \end{aligned} \tag{4}$$

Worker Reallocation. To assess whether the subsidy affected upward mobility, we follow Barrot et al. (2024). For each incumbent, we measure three outcomes: cumulative income earned at firms more productive than the worker's baseline employer, months spent at such firms, and whether the worker was ever employed by one. We denote the 2019 labor productivity of firm i as p_i and the 2019 labor productivity of worker n's employer in month t as p_{nt}. Formally:

$$\begin{aligned} Y^{\uparrow}_{ni} &= \sum_t \text{Earnings}_{nt} \, \mathbf{1}\{p_{nt} > p_i\}, \\ M^{\uparrow}_{ni} &= \sum_t \mathbf{1}\{p_{nt} > p_i\}, \\ E^{\uparrow}_{ni} &= \mathbf{1}\{M^{\uparrow}_{ni} > 0\}. \end{aligned} \tag{5}$$

We aggregate to the firm level as before:

$$\begin{aligned} \bar{Y}^{\uparrow}_{i} &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} Y^{\uparrow}_{ni}, \\ \bar{M}^{\uparrow}_{i} &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} M^{\uparrow}_{ni}, \\ \bar{E}^{\uparrow}_{i} &= \frac{1}{|\mathcal{I}_i|} \sum_{n \in \mathcal{I}_i} E^{\uparrow}_{ni}. \end{aligned} \tag{6}$$

We construct symmetric downward-mobility analogues and also compute all measures separately for workers in urban and rural areas—a proxy for labor market thickness that is easily observable by program administrators.[18] We estimate the same specification (Equation 3) for all worker-level outcomes; estimates represent the effects on the recipient firm's average worker.

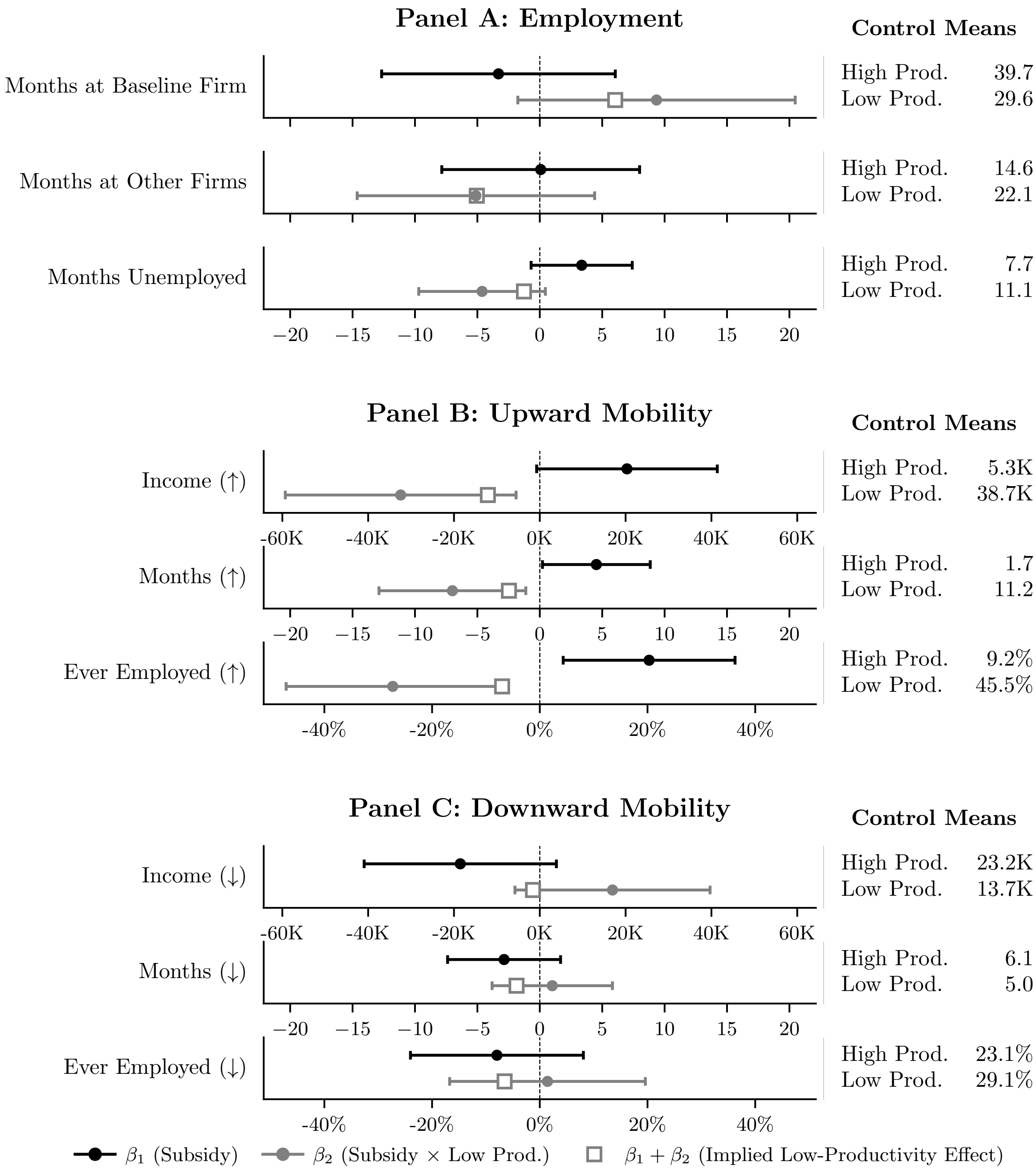
Figure 3 presents the full-sample results.[19] Panel A shows employment outcomes. Workers in subsidized high-productivity firms saw no statistically significant changes in time spent at the baseline firm, at other firms, or out of employment. For workers in low-productivity firms, the implied total effects (hollow squares) suggest more time at the baseline firm and less time unemployed or at other employers, though the individual coefficients are imprecisely estimated.

The evidence on upward mobility (Panel B) is stronger. Incumbents in high-productivity firms spent 4.5 more months employed at higher-productivity firms (from a complier control mean of 1.7 months), were 20 percentage points more likely ever to be employed by one (from a base of 9.2%), and earned EUR 20K more in cumulative income at such firms, though this last estimate is only marginally significant. The implied effects for low-productivity firms are reversed: the subsidy decreased the probability of upward mobility by about 7 percentage points, consistent with effects on months at higher-productivity firms (−2.5, from a baseline of 11.2) and income earned there (−EUR 12K, from a baseline of EUR 38.7K).

[18] The urban–rural classification is Statistics Finland's grid-level typology, which combines population, labor force, commuting, building, road network, and land use data to classify areas at a finer level than municipal boundaries. We define the urban–rural split based on workers' location rather than the firm's, since some firms have multiple establishments in different areas.

[19] Appendix Table A4 collects all estimates from the worker-level analyses.

Figure 3. Effects of Subsidies on Worker Employment and Mobility



Notes: 2SLS estimates from Equation 3. All specifications include application-week and industry fixed effects and a below-median labor productivity indicator. Filled circles show β_1 (effect for high-productivity firms, black) and β_2 (interaction with low-productivity indicator, gray). Hollow squares show $\beta_1 + \beta_2$, the implied total effect for low-productivity firms. Intervals denote 95% confidence intervals. Where panels report the same type of outcome (e.g., income, months, or probability), the x-axis scale is shared across panels to facilitate comparison. High-productivity firms are those above the in-sample median of value added per full-time employee in 2019. Panel A reports months at the baseline firm, at any other firm, and out of employment. Panel B reports upward-mobility outcomes (↑): cumulative income earned (EUR K), months employed, and the probability of ever being employed at a firm whose baseline labor productivity exceeds that of the worker's pre-pandemic employer. Panel C reports the symmetric downward-mobility outcomes (↓). All outcomes are aggregated over the full post-application follow-up period through May 2025. Complier control means are reported in the right margin, estimated following Abadie (2003).

The relative sizes of the months and extensive-margin effects are informative about timing. For low-productivity firms, each upward transition the subsidy prevented corresponds to roughly 36 fewer months (2.5/0.07) at a higher-productivity firm—substantially more than the 25 months (11.2/0.455) accumulated by the average untreated mover—implying that the blocked moves would have happened early, consistent with reallocation in which firm exit pushes workers to better employers shortly after the crisis. For high-productivity firms, each additional upward transition corresponds to roughly 22 months, pointing to moves that took place later in the follow-up, during the recovery period.

Panel C examines downward mobility. The point estimates, though imprecise, suggest that high-productivity incumbents were less likely to move down the productivity ladder. For low-productivity incumbents, the interaction terms partially offset the treatment coefficients, suggesting limited changes in downward mobility—the subsidies primarily affected upward moves rather than shielding workers from downward ones.

6 Interpreting the Mechanisms

The subsidies preserved employment and survival among low-productivity firms but produced no detectable firm-level performance effects for either group. The main effects instead operate through worker reallocation, where the same subsidy produced opposite outcomes depending on firm productivity. The results point to two distinct mechanisms.

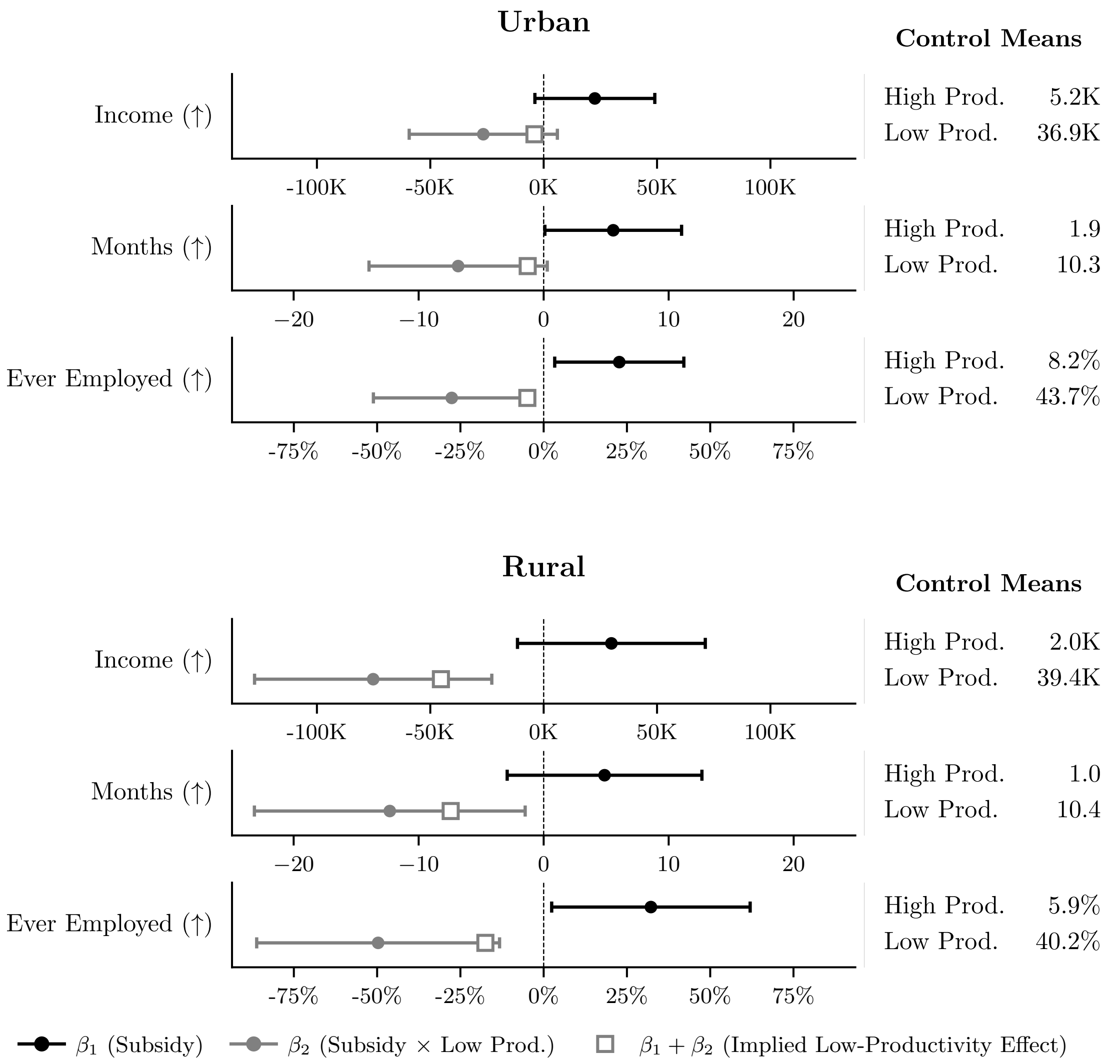
For workers in low-productivity firms, the evidence points to retention. The subsidy kept marginal firms alive (Table 1), and their workers appeared to spend more time at the baseline firm and less time at other employers (Figure 3, Panel A). Because these workers started near the bottom of the productivity distribution, the separations the subsidy prevented were disproportionately upward moves. The downward-mobility results reinforce this interpretation: the subsidy mainly prevented separations that would otherwise have led to more productive employers, rather than changing the destinations of workers who left anyway.

Under the retention mechanism, the trap effect should be strongest where outside options are scarce. Figure 4 tests this by splitting the upward-mobility results by labor market type. The springboard effect for high-productivity firms is present in both urban and rural areas. The trap effect for low-productivity firms, however, is substantially amplified in rural labor markets. In rural areas, fewer high-productivity employers operate nearby, so the displacement that the subsidy prevented may have been one of few realistic paths to a more productive employer. In urban labor markets, retained workers can more readily find alternative routes to higher-productivity firms, attenuating the trap effect.[20]

For workers in high-productivity firms, retention is an unlikely explanation. The subsidy had no detectable effect on firm survival or employment, and workers saw no meaningful change in time spent at the baseline firm, at other employers, or out

[20]Appendix Table A5 probes robustness using stricter upward-mobility thresholds (requiring gains of at least 5, 10, or 20 percentile points). The general pattern persists across all thresholds, suggesting that even larger leaps up the productivity distribution were negatively affected for rural workers in low-productivity firms. Results are also similar when using year-specific rather than baseline productivity percentiles for the current employer, indicating that post-pandemic resorting of firms is not driving our findings.

Figure 4. Upward Mobility by Labor Market Type



Notes: 2SLS estimates from Equation 3, restricting to upward-mobility outcomes (Panel B of Figure 3) and splitting the sample by the location of the firm's workers. All specifications include application-week and industry fixed effects and a below-median labor productivity indicator. Filled circles show β_1 (effect for high-productivity firms, black) and β_2 (interaction with low-productivity indicator, gray). Hollow squares show $\beta_1 + \beta_2$, the implied total effect for low-productivity firms. Intervals denote 95% confidence intervals. The x-axis scale is shared across panels for each outcome to facilitate comparison. Complier control means are reported in the right margin, estimated following Abadie (2003).

of work (Figure 3, Panel A). This leaves little room for channels that operate through extended tenure at the subsidized employer, such as on-the-job skill accumulation or network formation. Yet the subsidy sharply increased upward mobility.

The evidence is consistent with a compositional effect: the subsidy changed the conditions under which separations happened, not how often or when they occurred. A firm that is never at risk of closure can still freeze hiring, delay payments, or signal restructuring—narrowing the outside offers available to departing workers. Liquidity support may have prevented these signals, so that workers who separated did so from firms that appeared stable rather than stressed. The geographic pattern reinforces this interpretation: unlike the trap effect, which is concentrated in rural labor markets where outside options are scarce, the springboard effect is present in both urban and rural areas (Figure 4), suggesting the mechanism operates through the firm's condition at separation rather than the local availability of high-productivity employers. Though we cannot directly observe the signaling channel, this interpretation—that the subsidy preserved the quality of separations rather than their frequency—fits the full pattern of results.

These patterns connect to two competing reallocation forces in the macro literature. The retention effects among low-productivity firms are a concrete instance of delayed cleansing (Caballero and Hammour, 1996): subsidies prevented firm exit and blocked the upward worker flows that displacement would otherwise have generated. The compositional effects among high-productivity firms correspond to a partial offset of the sullying effect (Barlevy, 2002): recessions disrupt job-ladder dynamics (Postel-Vinay and Robin, 2002), and subsidies can preserve the conditions under which productive separations occur. Consistent with evidence that cleansing and sullying operate at different points in the firm productivity distribution (Haltiwanger et al., 2025), our estimates suggest that crisis subsidies can interact with either force depending on the productivity of the recipient firm.

7 Conclusion

This paper shows that a COVID-19 crisis subsidy had sharply heterogeneous effects on worker reallocation. Subsidies directed at low-productivity firms reduced upward mobility: they kept marginal firms alive and trapped workers who would otherwise have moved to more productive employers, with the largest effects in rural labor markets where alternatives are scarce. Subsidies directed at high-productivity firms did not detectably alter workers' attachment to the baseline firm, but redirected their transitions toward more productive employers—a springboard effect.

Did the program meet its goals? As a development grant, we find no positive results: subsidies had no detectable effect on revenue or labor productivity at either type of firm. As crisis relief, the picture is mixed. The subsidy supported survival and employment among low-productivity firms, but at the cost of reduced upward mobility, and the absence of any detectable post-period productivity recovery suggests that many of the preserved matches may not have been worth keeping. The high-productivity result fits neither frame—those firms saw no detectable firm-level benefit, yet their workers transitioned to more productive employers at higher rates.

The trade-off between short-run employment preservation and medium-run reallocation efficiency is not fixed: it depends on the productive capacity of the recipient firm. Our design identifies effects among compliers and does not capture general equilibrium spillovers to non-subsidized firms. But the broad pattern carries a direct implication for crisis subsidy design. On the question of whether to insure workers or jobs during recessions (Giupponi et al., 2022), our results suggest the answer may depend on firm productivity and worker location—characteristics program administrators can readily observe at the time of allocation.

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Appendix

Table A1. Summary Statistics

	External Validity		Productivity		Compliers
	All	Sample	Low Prod.	High Prod.	Average
Revenue (EUR M)	7.00	4.88	3.29	6.48	3.26
Full-Time Employees	23.04	24.85	24.43	25.27	16.57
Wage Sum (EUR K)	948.46	1012.47	828.42	1196.40	577.50
Value Added (EUR K)	1618.24	1466.21	913.10	2018.97	814.30
Labor Prod. (EUR K)	62.84	58.81	35.99	81.62	56.81
Labor Share (%)	31.78	30.89	34.50	27.39	30.64
EBITDA (EUR K)	468.61	240.76	-88.76	570.07	112.14
Equity (EUR K)	4600.64	1204.77	765.90	1643.36	107.99
Debt Ratio (%)	186.94	150.94	228.30	73.58	266.63
N	24,456	6,374	3,186	3,188	-

Notes: All variables are measured from 2019 financial statements. "Full-Time Employees" refers to full-time equivalent employees, including part-time labor. In Columns 1–4, each number gives the mean of the variable among the firms specified in the column. The first two columns compare sample firms to other firms in the economy with 6–250 employees. The next two columns compare sample firms with below-median labor productivity in the sample ("Low Prod.") and above-median labor productivity in the sample ("High Prod."). The fifth column shows estimated average complier characteristics, computed following Abadie (2003) and Chyn et al. (2025).

Table A2. Test of Random Assignment

	(1) Accepted	(2) Case Officer Leniency
Full-Time Employees	0.000335	-0.00000164
	(0.000450)	(0.0000866)
Revenue (EUR K)	0.00000137**	-5.69e-08
	(0.000000475)	(0.000000125)
Labor Productivity (EUR K)	0.000709***	0.0000209
	(0.000151)	(0.0000270)
Value Added (EUR K)	-0.00000810	0.000000304
	(0.00000804)	(0.00000166)
EBITDA (EUR K)	-0.00000369	6.70e-08
	(0.00000870)	(0.00000175)
Equity (EUR K)	0.00000397***	-5.53e-08
	(0.00000113)	(0.000000218)
Firm Age	0.000684*	-0.0000939
	(0.000322)	(0.0000616)
Debt Ratio (%)	-0.0000642	-0.0000191
	(0.000218)	(0.0000195)
N	6,390	6,390
R^2	0.240	0.0555
Joint F-test p-value	< 0.001	0.681

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Column 1 regresses the binary treatment indicator on 2019 firm characteristics. Column 2 regresses the leniency instrument on the same covariates. Both regressions include application-week and industry fixed effects. The F-test evaluates the joint significance of the covariates, excluding the fixed effects. Figure 1 presents these results graphically.

Table A3. First Stage: Leniency and Subsidies

	(1)	(2)	(3)
Panel A: Accepted			
Leniency	0.860***	0.795***	0.801***
	(0.0730)	(0.0665)	(0.0663)
Panel B: Granted Subsidies (EUR K)			
Leniency	84.69***	76.81***	77.50***
	(7.189)	(6.632)	(6.584)
N	6,410	6,410	6,390

Heteroskedasticity-robust standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Panel A uses an indicator for whether the firm is granted a subsidy as the dependent variable. Panel B uses total subsidies paid (EUR K) as the dependent variable. Column 1 includes no fixed effects or controls. Column 2 adds application-week and industry fixed effects. Column 3 further adds baseline firm controls measured in 2019 (employment, revenue, labor productivity, value added, EBITDA, equity, firm age, and the debt–equity ratio).

Table A4. Effects of Subsidies on Worker Employment and Mobility

	Full Sample			Urban			Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Employment									
	Months at Baseline Firm	Months at Other Firms	Months Unemployed	Months at Baseline Firm	Months at Other Firms	Months Unemployed	Months at Baseline Firm	Months at Other Firms	Months Unemployed
Subsidy	−3.311	0.072	3.356	−2.105	−2.244	4.638	−6.192	4.918	0.775
	(4.776)	(4.043)	(2.067)	(5.654)	(4.880)	(2.540)	(8.049)	(7.107)	(4.076)
Subsidy × Low Prod.	9.352	−5.119	−4.624	7.978	−2.273	-6.762^{*}	11.67	−11.60	1.425
	(5.669)	(4.853)	(2.588)	(6.605)	(5.731)	(3.125)	(9.515)	(8.444)	(5.068)
High Prod. Control Mean	*39.7*	*14.6*	*7.7*	*40.1*	*15.7*	*6.1*	*40.6*	*12.9*	*8.5*
Low Prod. Control Mean	*29.6*	*22.1*	*11.1*	*29.4*	*21.7*	*11.8*	*33.1*	*19.9*	*8.7*
Panel B: Upward Mobility									
	Income (↑)	Months (↑)	Ever Employed (↑)	Income (↑)	Months (↑)	Ever Employed (↑)	Income (↑)	Months (↑)	Ever Employed (↑)
Subsidy	20.32	4.531^{*}	0.203^{*}	22.57	5.565^{*}	0.227^{*}	29.87	4.871	0.322^{*}
	(10.74)	(2.203)	(0.0814)	(13.49)	(2.790)	(0.0989)	(21.13)	(3.974)	(0.152)
Subsidy × Low Prod.	-32.42^{*}	-7.002^{*}	-0.273^{**}	−26.63	−6.842	-0.276^{*}	-75.19^{**}	-12.31^{*}	-0.497^{**}
	(13.72)	(3.001)	(0.101)	(16.67)	(3.638)	(0.120)	(26.69)	(5.526)	(0.186)
High Prod. Control Mean	*5.3*	*1.7*	*0.092*	*5.2*	*1.9*	*0.082*	*2.0*	*1.0*	*0.059*
Low Prod. Control Mean	*38.7*	*11.2*	*0.455*	*36.9*	*10.3*	*0.437*	*39.4*	*10.4*	*0.402*
Panel C: Downward Mobility									
	Income (↓)	Months (↓)	Ever Employed (↓)	Income (↓)	Months (↓)	Ever Employed (↓)	Income (↓)	Months (↓)	Ever Employed (↓)
Subsidy	−18.53	−2.861	−0.0796	-28.36^{*}	−4.634	−0.120	−12.05	−3.646	−0.171
	(11.43)	(2.309)	(0.0819)	(14.26)	(3.034)	(0.102)	(21.45)	(4.866)	(0.157)
Subsidy × Low Prod.	16.95	0.999	0.0143	27.41	2.902	0.0619	16.22	2.922	0.170
	(11.60)	(2.460)	(0.0927)	(14.40)	(3.175)	(0.114)	(21.92)	(5.085)	(0.176)
High Prod. Control Mean	*23.2*	*6.1*	*0.231*	*25.6*	*6.5*	*0.234*	*15.7*	*4.6*	*0.224*
Low Prod. Control Mean	*13.7*	*5.0*	*0.291*	*13.6*	*5.1*	*0.301*	*16.5*	*5.0*	*0.249*
N	6,321	6,321	6,321	6,016	6,016	6,016	4,298	4,298	4,298

Heteroskedasticity-robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: 2SLS estimates from Equation 3. All specifications include application-week and industry fixed effects and a below-median labor productivity indicator. *Subsidy* gives the effect of subsidy receipt for high-productivity firms (above the in-sample median of value added per full-time employee in 2019); adding the coefficient on the interaction term gives the effect for low-productivity firms. All outcomes are aggregated over the full post-application follow-up period through May 2025. Panel A reports months at the baseline firm, at any other firm, and out of employment. Panel B reports upward-mobility outcomes: cumulative income earned (EUR K), months employed, and the probability of ever being employed at a firm whose baseline labor productivity exceeds that of the worker's pre-pandemic employer (↑). Panel C reports the symmetric downward-mobility outcomes (↓). All coefficients are in the units of the corresponding dependent variable. The urban–rural split in Columns 4–9 is based on the location of the firm's workers. *High Prod. Control Mean* and *Low Prod. Control Mean* report complier control means estimated following Abadie (2003). Figures 3 and 4 present these results graphically.

Table A5. Upward Mobility: Alternative Threshold Definitions

	Urban			Rural		
	(1)	(2)	(3)	(4)	(5)	(6)
	Income (↑)	Months (↑)	Ever Emp. (↑)	Income (↑)	Months (↑)	Ever Emp. (↑)
Panel A: 5 Percentile Point Threshold						
Subsidy	16.61	4.045	0.157	22.53	4.565	0.300^{*}
	(12.32)	(2.502)	(0.0898)	(17.73)	(3.513)	(0.138)
Subsidy × Low Prod.	−20.58	−5.376	−0.192	-63.63^{**}	-11.33^{*}	-0.452^{**}
	(15.62)	(3.351)	(0.112)	(23.84)	(5.072)	(0.175)
Panel B: 10 Percentile Point Threshold						
Subsidy	4.548	1.105	0.0787	2.348	0.227	0.101
	(9.700)	(1.921)	(0.0723)	(13.95)	(2.748)	(0.108)
Subsidy × Low Prod.	−5.246	−1.353	−0.0765	−38.55	−5.881	−0.248
	(13.69)	(2.910)	(0.0986)	(20.91)	(4.457)	(0.152)
Panel C: 20 Percentile Point Threshold						
Subsidy	−4.313	−1.133	−0.0271	−2.512	−1.395	−0.0236
	(6.503)	(1.242)	(0.0502)	(9.173)	(1.899)	(0.0650)
Subsidy × Low Prod.	3.537	1.722	0.0517	-44.26^{*}	-8.082^{*}	−0.140
	(11.12)	(2.301)	(0.0808)	(17.54)	(3.713)	(0.119)
Panel D: Higher Productivity Based on Current Productivity						
Subsidy	−21.32	−1.537	0.142	30.76	6.845	0.267
	(18.84)	(2.517)	(0.117)	(19.15)	(3.871)	(0.176)
Subsidy × Low Prod.	14.31	−1.412	−0.233	-57.14^{*}	-10.25^{*}	-0.526^{*}
	(20.62)	(3.180)	(0.137)	(22.34)	(4.822)	(0.209)
N	6,016	6,016	6,016	4,298	4,298	4,298

Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: 2SLS estimates from Equation 3. All specifications include application-week and industry fixed effects and a below-median labor productivity indicator. Panels A–C define upward mobility as employment at any firm whose pre-pandemic labor productivity percentile exceeds that of the worker's 2019 employer by at least 5, 10, and 20 percentiles, respectively. Panel D uses an alternative definition: an employer is classified as higher productivity if its labor productivity percentile in the year of employment exceeds that of the worker's baseline employer in 2019.