

Online Appendix  
Why Firms Lay Off Workers Instead of Cutting Wages: Evidence  
from Linked Survey-Administrative Data  
By Antoine Bertheau, Marianna Kudlyak, Birthe Larsen and Morten Bennedsen

 A/S  
8200 Aarhus N  
Att.: Den administrerende direktør

**Hvordan kommer dit firma styrket ud af krisen?**

Rambøll gennemfører på vegne af Københavns Universitet en spørgeskemaundersøgelse, der skal belyse, hvordan virksomheder kan komme styrket ud af Covid19-krisen. Vi spørger om hvad du/I har gjort for at komme igennem krisen og hvilke overvejelser du gør om tiden efter Covid19.

Projektet gennemføres under ledelsen af Niels Bohr Professor Morten Bennedsen, Økonomisk Institut, og er støttet af blandt andet Industriens Fond og det Samfundsvidenskabelige Forskningsråd.

Hvis du ønsker det, vil du efter undersøgelsens afslutning modtage en anonymiseret benchmarkingsrapport, hvor du kan se dine besvarelser op mod fordelingen af andre besvarelser. Vi overholder naturligvis alle databeskyttelsesreglerne.

Det tager ca. 20 minutter at udfylde spørgeskemaet. Undervejs kan du lukke skemaet og senere genoptage besvarelsen via linket, som du har modtaget her. Husk derfor at gemme denne invitation, til du har afsluttet din besvarelse.

**Sådan gør du**

Spørgeskemaet besvares elektronisk via internettet. Du kan svare på alle computere, tablets (f.eks. iPad m.m.) og smartphones. Du får adgang til dit personlige spørgeskema ved at klikke på nedenstående link:  
<https://surveys.ramboll.com/answer?key=ZNEVCQ9MSJ1Y>

Vi vil bede dig besvare spørgeskemaet senest **den 27. juni 2021**.

**Du er sikret fortrolighed**

Dine svar behandles fortroligt af Rambøll og vil kun fremgå i anonymiseret form. Du kan få mere information om behandling af personoplysninger i forbindelse med undersøgelsen på forsiden at spørgeskemaet.

**Kontakt**

Hvis du har yderligere spørgsmål, er du velkommen til at kontakte Rambøll på e-mail: [skemasupport@ramboll.com](mailto:skemasupport@ramboll.com) eller tlf. 6915 8076 på hverdage i tidsrummet kl. 8.00-16.00.

På forhånd tak for din deltagelse!

Med venlig hilsen  
Rambøll og  
Københavns Universitet

Note: The figure shows the survey invitation letter that firms received in an email. The letter is designed to provide information to recruit as many respondents as possible and minimize selection bias. It contains the following information: the deadline for completion of the survey; that the survey could be completed on any device, including tablets and smart phones; that all information provided was anonymous and the survey complied with all data protection regulations; and it explained the reward system for respondents. The letter was purposely vague about the actual research topic, it used simple language, and it displayed the logo of the University of Copenhagen. See an English translation of the letter below.

Att: The Administrative Director

On behalf of the University of Copenhagen, Rambøll is carrying out a survey to study how companies can emerge stronger from the COVID19 crisis. We ask what you and others have done to get through the crisis and what thoughts you have about the time after COVID19. The project is carried out under the leadership of Niels Bohr Professor Morten Bennedsen, Department of Economics, University of Copenhagen, and is supported by, among others, Industriens Fond and the Social Science Research Council. If you participate in the survey, we will offer you an anonymized benchmarked report that shows your responses against the distribution of the other responses. We naturally comply with all data protection regulations. It takes approximately 20 minutes to complete the questionnaire. You can close the form and resume it later by again clicking on the link below. Therefore, please remember to save this invitation until you have completed the survey.

Here's how you do it. The questionnaire is answered electronically via the Internet. You can complete the questionnaire on any computer, tablet (e.g. iPad, etc.) or smartphone. To access your personal questionnaire, click on the link below: [LINK](#)

We ask that you complete the questionnaire no later than 27 June 2021. You are guaranteed confidentiality

Your answers are treated confidentially by Rambøll and will only appear in an anonymized form. You can find more information about the treatment of personal data in connection with the survey on the front page of the questionnaire. Contact

If you have further questions, please feel free to contact Rambøll by e-mail: [skemasupport@ramboll.com](mailto:skemasupport@ramboll.com) or tel. 6915 8076 on weekdays between 8.00-16.00. Thank you in advance for your participation

Yours sincerely

Rambøll and University of Copenhagen

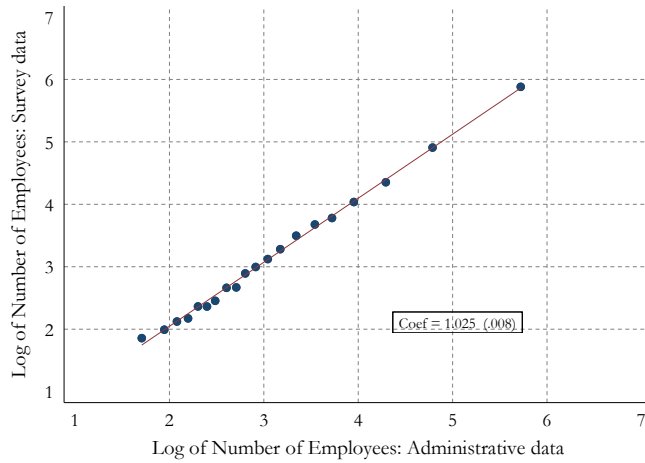


Figure A.2: Comparing Survey and Administrative Data: Number of Employees

Note: The figure compares the responses to the survey question “How many employees were in the company on May 1, 2021?” with the information from the administrative data in the BFL.

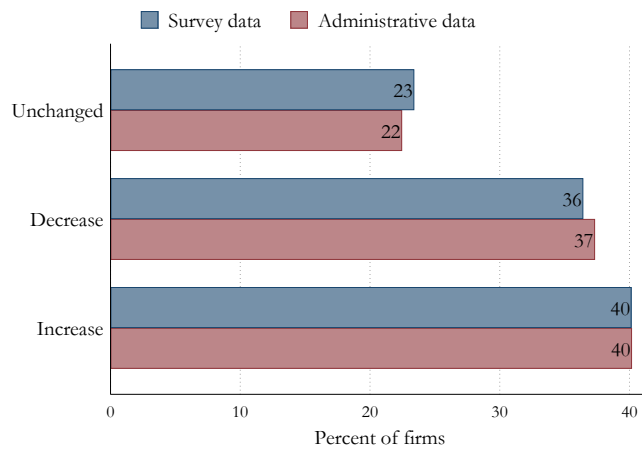


Figure A.3: Comparing Survey and Administrative Data: Revenue Change

Note: This figure compares the responses to the survey question “How much did revenue change in 2020 compared to 2019?” with the administrative data from FIRM. The category “Unchanged” is defined as a revenue growth rate between -5% and +5%. The percentage calculation excludes 27 missing responses for this question (out of 3013 firms).

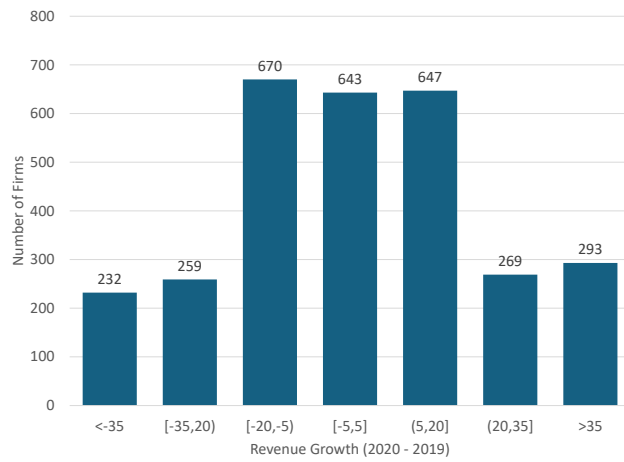


Figure A.4: Distribution of Firms by Revenue Growth between 2019 and 2020

Note: The x-axis is the firm revenue growth between 2019 and 2020 in the administrative data (FIRM).

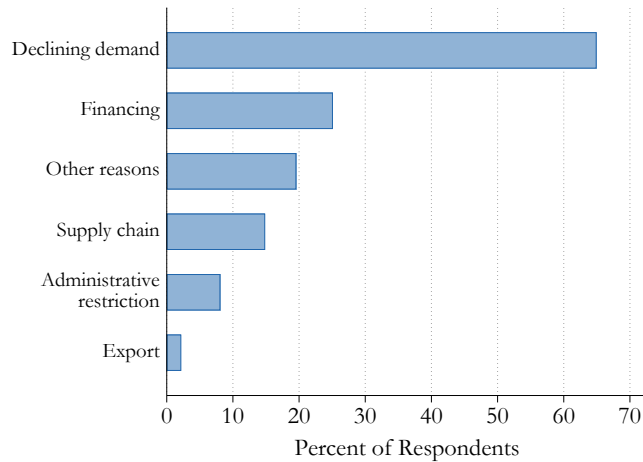


Figure A.5: Reasons for Declining Revenue in 2020

Note: The figure reports responses to the question, “The revenue decreased because...” The question was put to firms that declared having a reduction in revenue in 2020 as compared to 2019.

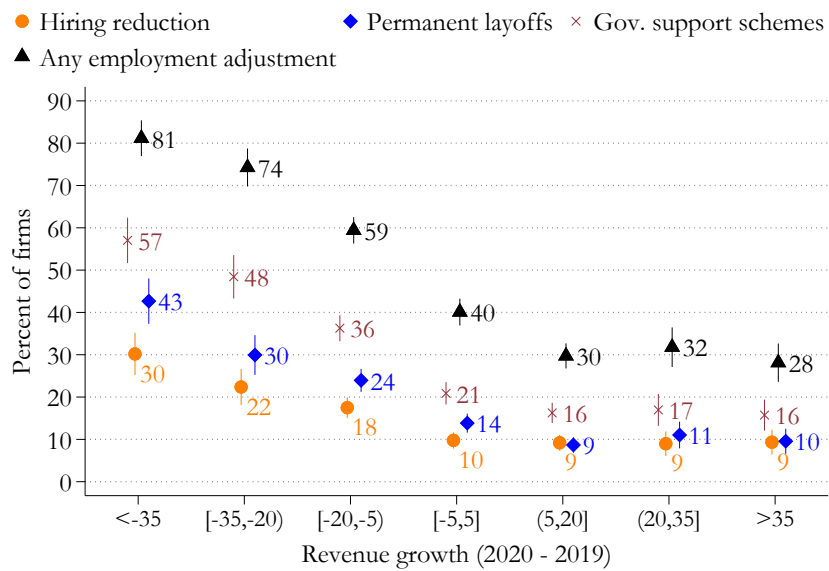
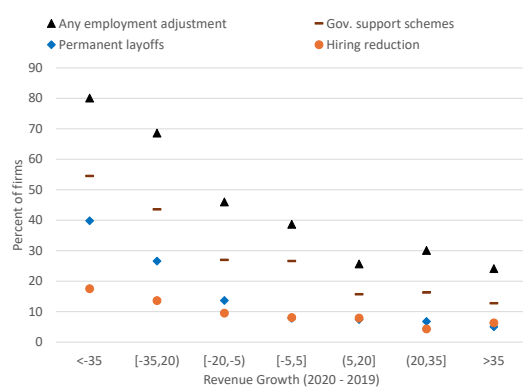
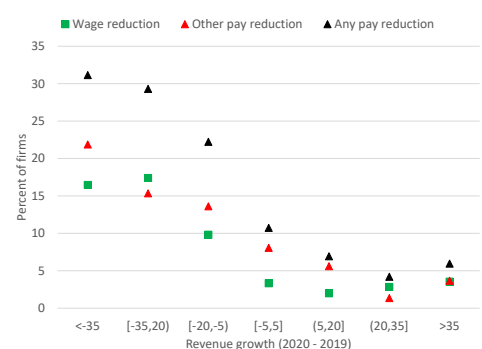


Figure A.6: Employment Adjustment by Revenue Growth: Including Government Support Schemes

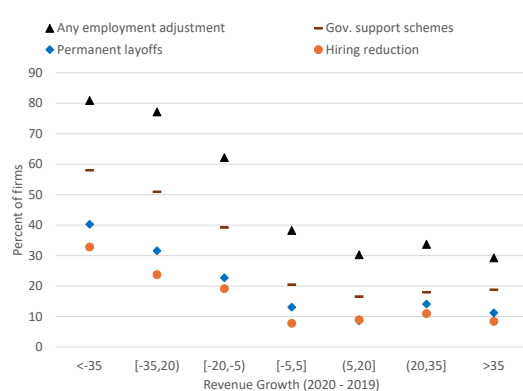
Note: The figure reproduces Panel (a) of Figure 1 with government support scheme take-up (× markers) added as a separate series and included in the “any employment adjustment” indicator. Figure 1, Panel (a), excludes government support from both.



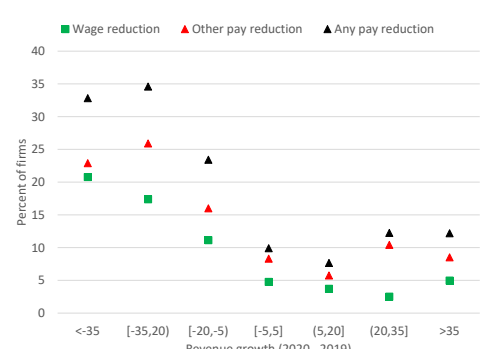
(a) Micro firms: Number of workers



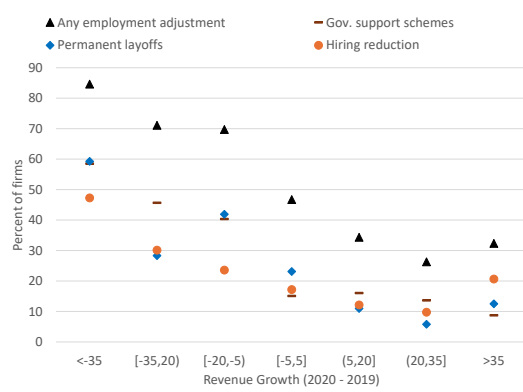
(b) Micro firms: Worker pay



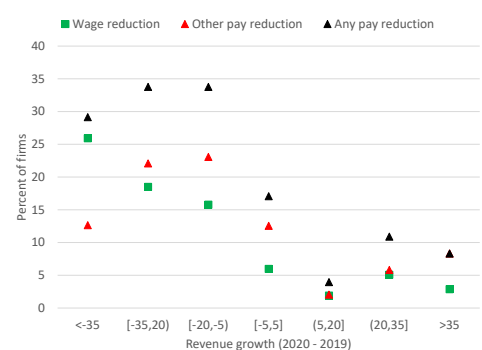
(c) Small firms: Number of workers



(d) Small firms: Worker pay



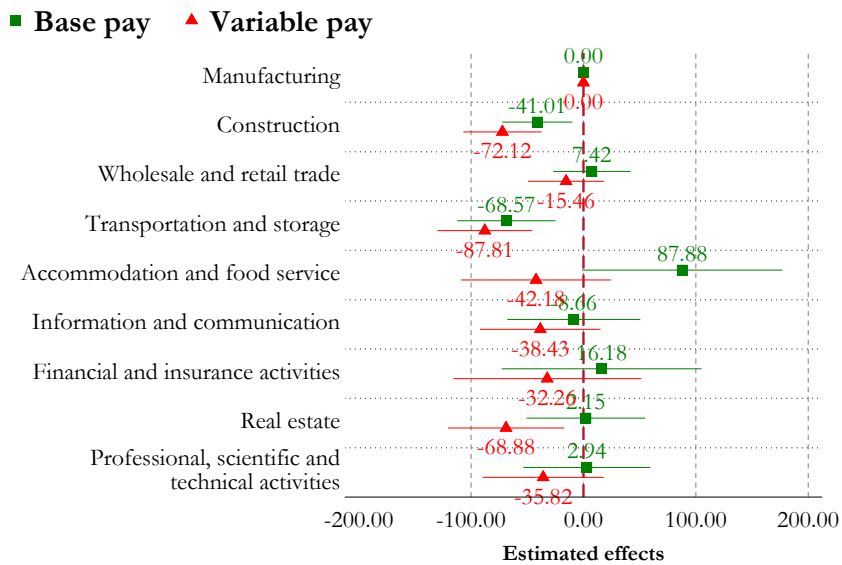
(e) Med/Large firms: Number of workers



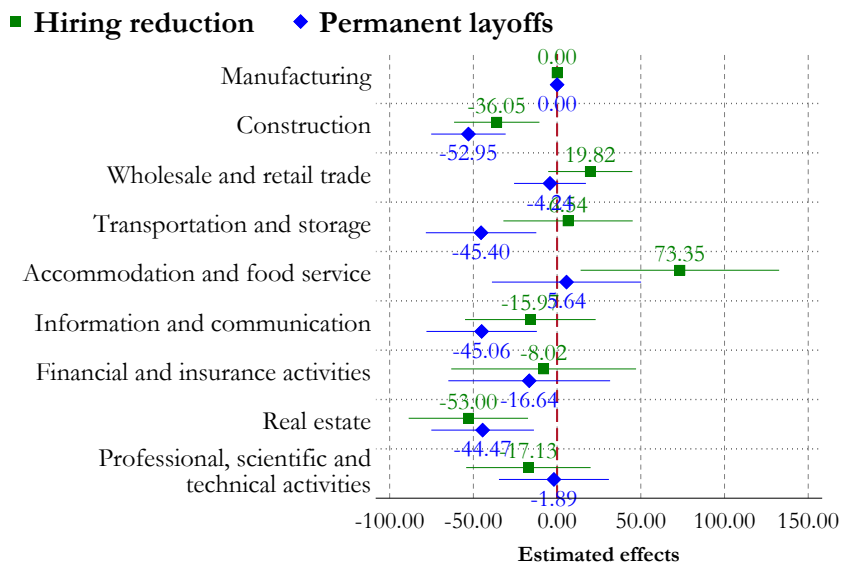
(f) Med/Large firms: Worker pay

Figure A.7: Labor Cost Adjustment Approaches in 2020 by Revenue Growth, by Firm Size

Note: Panels (a)–(f) show the percentage of firms answering “yes” to each labor adjustment method, split by firm size. As in Figure A.6, “any employment adjustment” includes government support schemes. The x-axis is 2019-2020 revenue growth (FIRM). Size categories: micro: <10 employees; small: 10–49; medium-large:  $\geq 50$ .



(a) Worker pay



(b) Number of employees

Figure A.8: Adjustments to Worker Pay and the Number of Workers: Industry-Specific Effects

Note: The figure shows the industry fixed effects coefficients from firm-level OLS regressions where the outcome variable takes the value one if the respondent answers “yes” to questions about the corresponding labor adjustment method. Additional controls are the 2019-2020 firm revenue growth rate, the 2019-2020 job growth rate, productivity, average wages, number of employees, routine task index, percentage of unionized workers, debt ratio, average educational attainment of employees, percentage of female workers, average worker age, average job tenure, geographic fixed effects, firm-specific labor market tightness, an indicator of the base pay set at the industry level, and an indicator for the presence of an employee representative. All covariates are from the administrative datasets, and measured in 2019 unless stated otherwise (except for an indicator for worker representative, which is obtained from our survey).

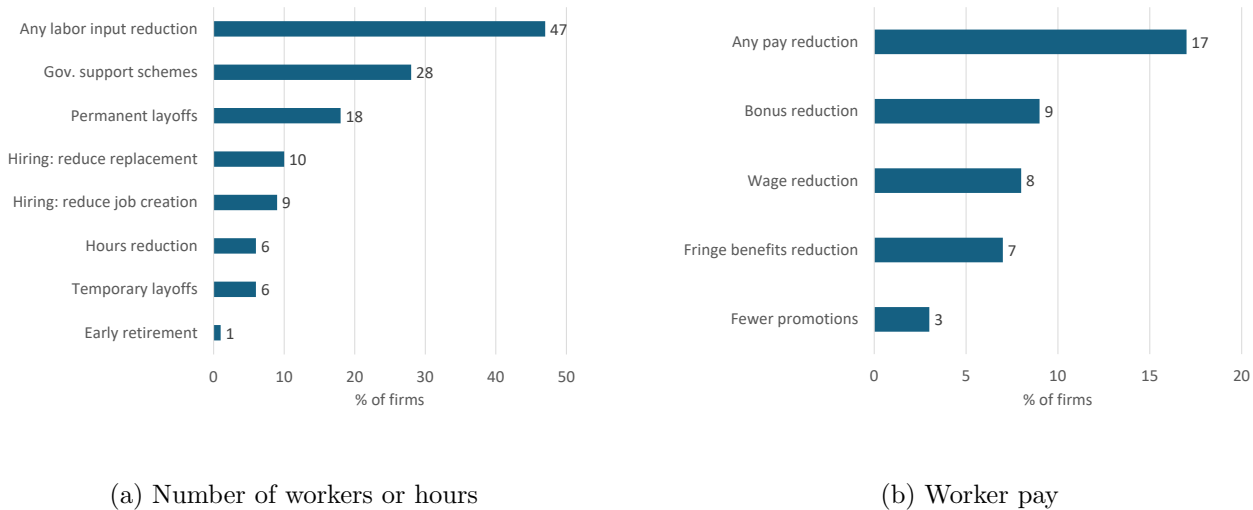


Figure A.9: Labor Cost Adjustment Approaches in 2020, all firms

Note: Panels (a) and (b) show the percentage of firms that answered “yes” to questions about the corresponding labor adjustment method; the categories are not mutually exclusive. Temporary layoffs are defined in the questionnaire as layoffs with expected reemployment.

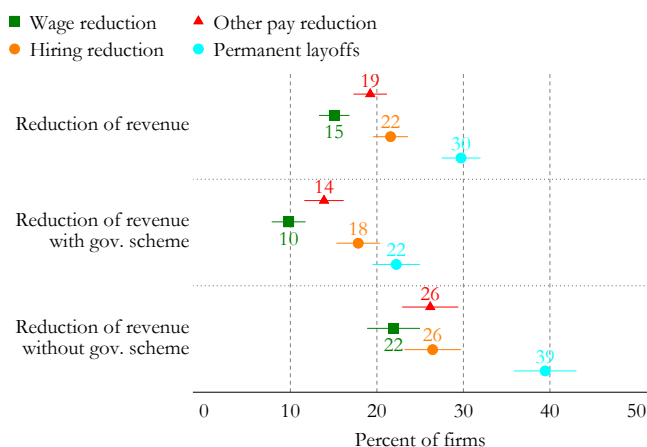


Figure A.10: Labor Adjustment Approaches Conditional on the Reported Use of the Government-Sponsored Furloughs, Firms with Revenue Reduction

Note: The figure shows the share of firms reporting various labor adjustment approaches in 2020 conditional on the firm reporting the use or not of the government-sponsored furloughs.

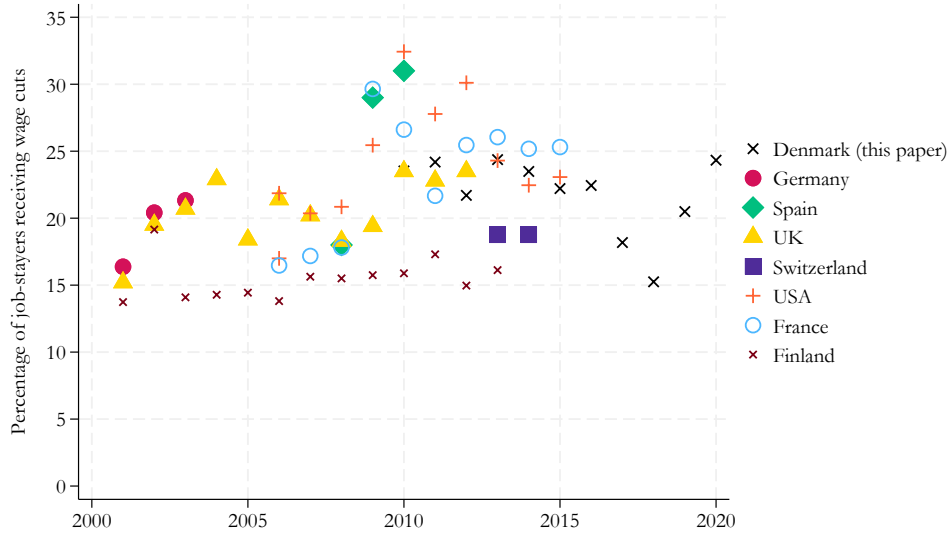


Figure A.11: The Incidence of Hourly Nominal Wage Cuts in Administrative Data, This Paper and the Related Literature

Note: The figure plots estimates of the incidence of nominal hourly wage cuts from administrative data sources. We plot one observation per country per year. Sources plotted: Denmark: this paper, LONN data, 2010-2020. Germany: Ehrlich and Montes (2024). Finland: Vainiomäki (2020). France: Gautier et al. (2019). Switzerland: Funk and Kaufmann (2022). UK: Elsby et al. (2016) for 2001-2006, Schaefer and Singleton (2022) for 2007-2018. USA: Jardim et al. (2019). Overlapping observations not plotted: UK: Elsby et al. (2016) report the percentage of negative log nominal wage changes as 20.2, 18.3, 19.4, 23.5, 22.8, 23.5 in 2007-2012, respectively; Fongoni et al. (2023) report 25.7% of job stayers experiencing year-to-year cuts in gross wages in 2004. USA: Kurmann and McEntarfer (2024) report 20% and 24% of job stayers experiencing an earnings cut in 2006 and 2010, respectively.

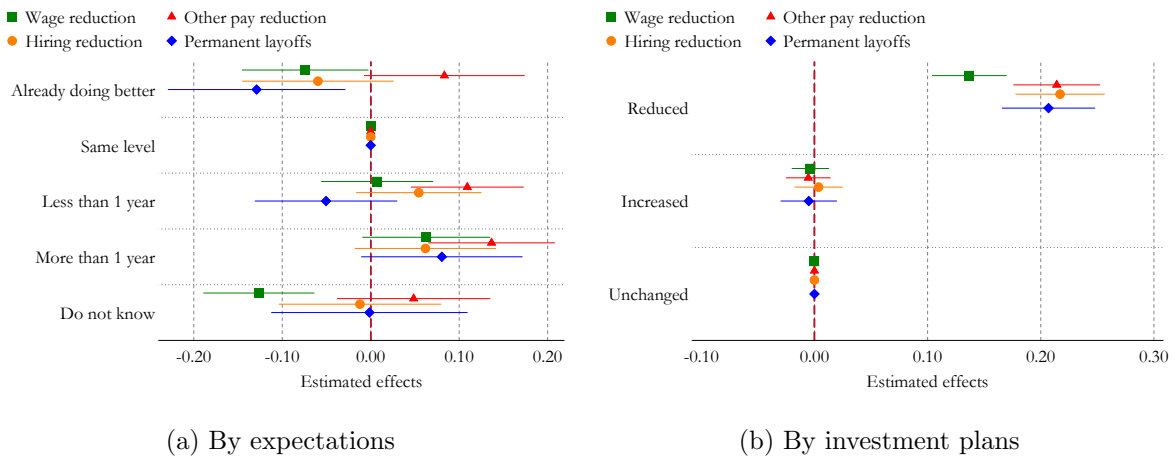


Figure A.12: Labor Adjustment Approaches, by Expected Duration of the Revenue Reduction and by the Firm’s Investment Plan

Note: Panel (a) reports coefficients from the regressions of using a specific labor adjustment method (e.g., wage reduction, etc.) on the dummies that capture firms’ expectations. The question is, “How long do you expect it will take before the revenue is back to its 2019 pre-crisis level?” The question is conditional on the firm reporting a decrease in revenue in 2020. Panel (b) reports coefficients from the regressions of using a specific labor adjustment method on the dummies that capture a firm’s investment plan for the following year. Both specifications control for industry and local labor market fixed effects. Joint  $F$ -tests reject the null that all non-base category coefficients equal zero for wage reductions ( $p = 0.014$ ), bonus reductions ( $p = 0.018$ ), and permanent layoffs ( $p = 0.001$ ) after excluding “Do not know” responses ( $N = 767$ ); hiring reductions are marginally significant ( $p = 0.054$ ). Results are similar in the full sample including “Do not know” ( $N = 845$ ).

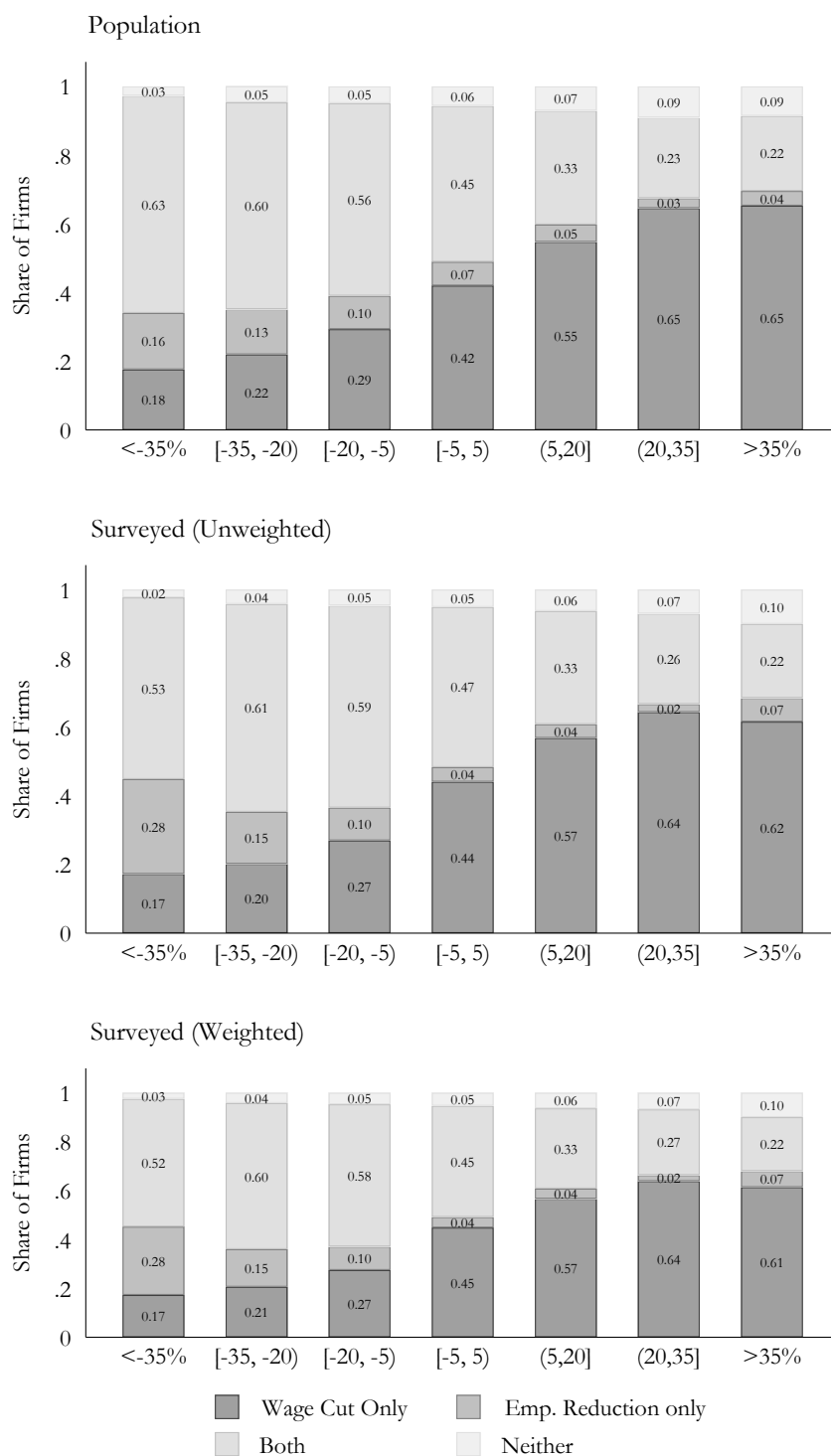
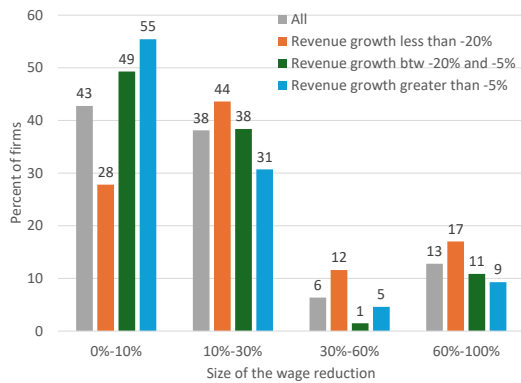
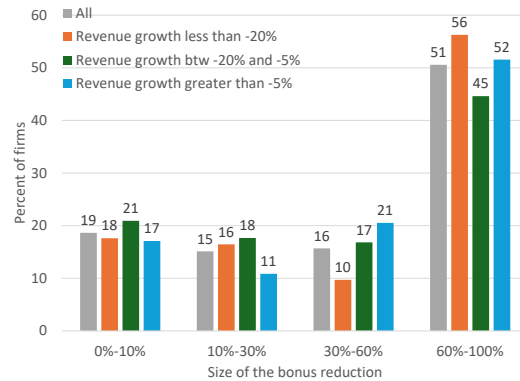


Figure A.13: Administrative Analogue to Figure 2: Joint Incidence of Pay Cuts and Employment Reductions by Revenue Growth

Note: Each bar shows the share of firms in one of four mutually exclusive categories—Wage Cut Only, Employment Reduction Only, Both, and Neither—within a 2019-2020 revenue-growth bin. A firm has an administrative pay cut if  $\geq 5\%$  of job stayers experienced a base-wage decline  $> 2\%$  (log); a firm has an employment reduction if headcount declined between 2019 and 2020. Left panel: full administrative population ( $N \approx 28,000$ ). Center: surveyed firms, unweighted. Right: surveyed firms, entropy-weighted.



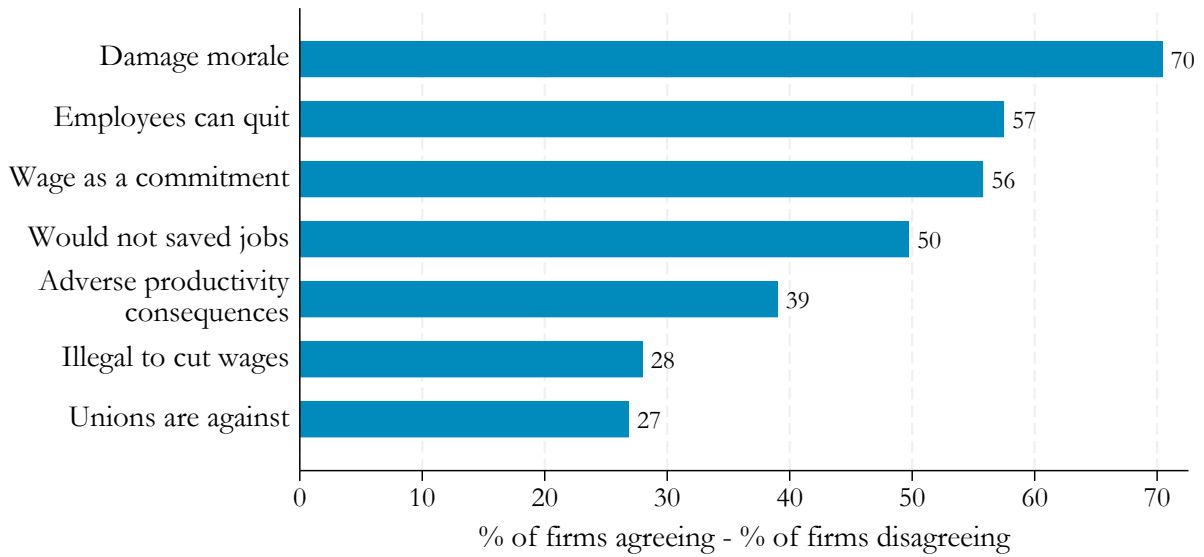
(a) Wage reductions



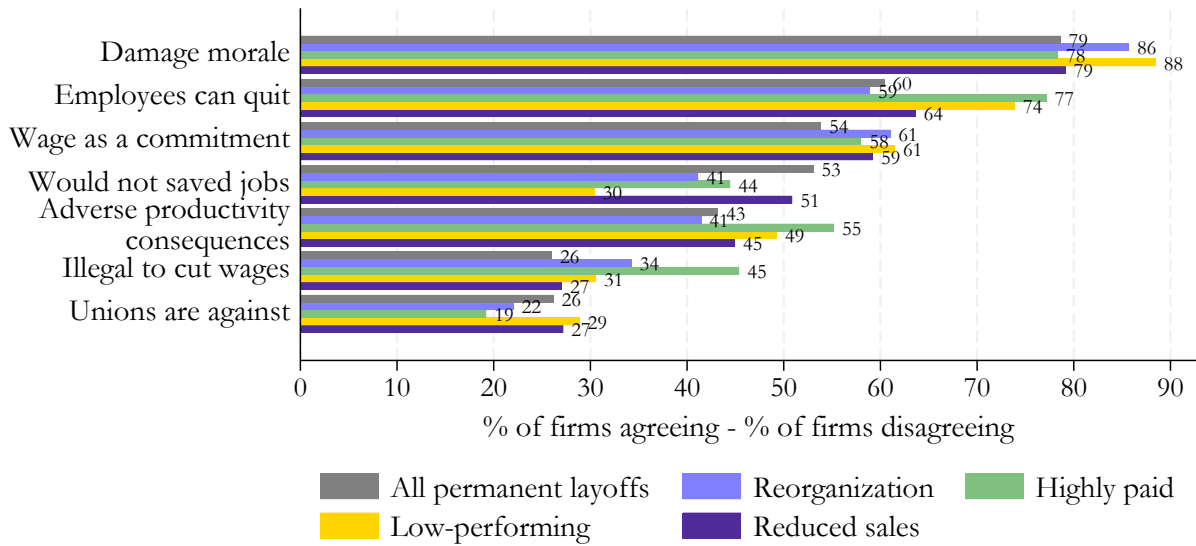
(b) Fewer/lower bonuses

Figure A.14: Distribution of Firms by the Reported Size of Pay Cut, Conditional on Implementing Pay Cuts

Note: The results in the figure are conditional on firms that answer “yes” to questions about using wage reductions, panel (a), and bonus reductions, panel (b). The average size of the reported wage reduction is 26%, with the firms with greater revenue reduction reporting larger wage reductions: 33% in the sample of firms with revenue growth less than -20%, 23% in the sample of firms with revenue growth between -20 and -5%, 21% in the sample of firms with revenue growth above -5%. Most firms implemented wage reductions under 30% (eighty percent of firms); however, thirteen percent of firms reported wage reductions of 60% or larger. In contrast, the average size of the bonus reduction is larger (61.5%), and more than half of the firms implemented bonus reductions above 60%.



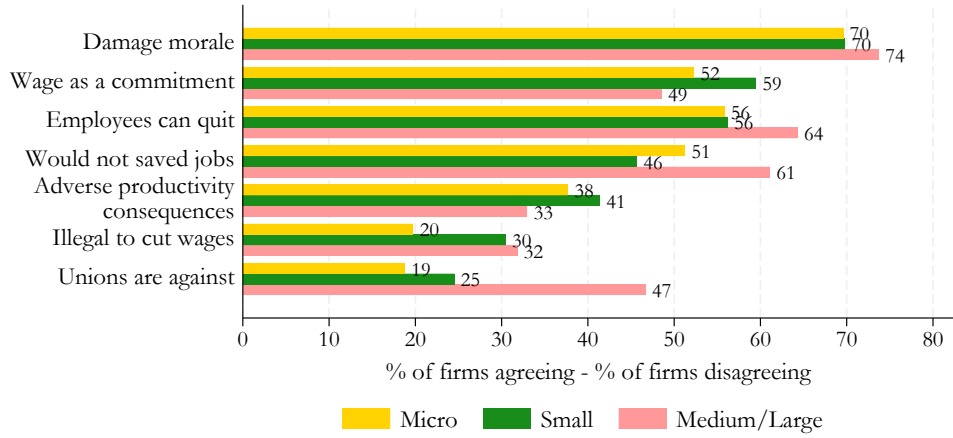
(a) Full sample



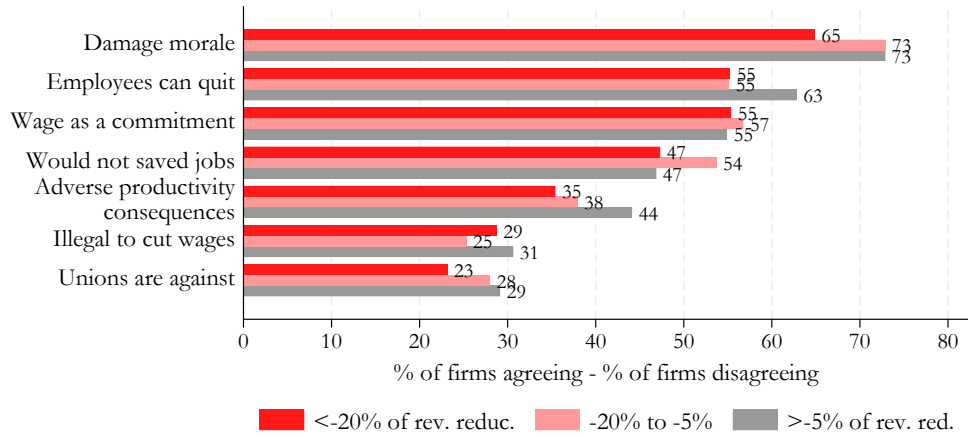
(b) By reason for permanent layoff

Figure A.15: Reasons for Not Lowering Base Pay

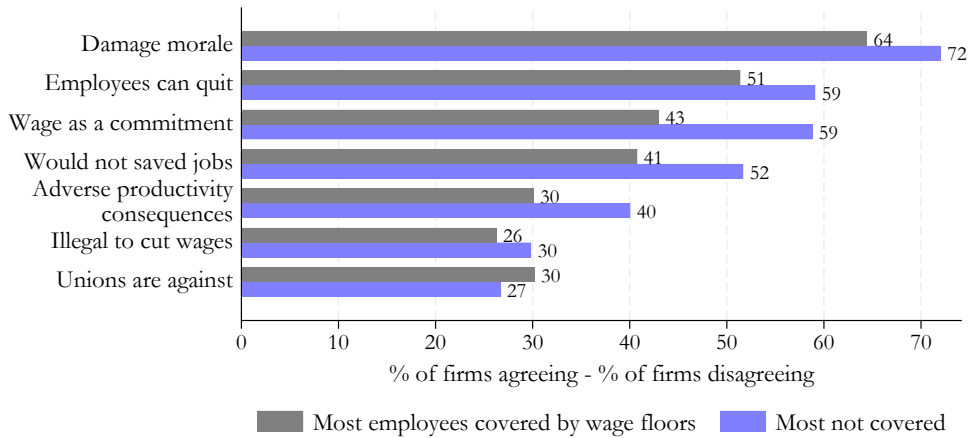
Note: The figure reports responses to the question: “What are the main reasons for not lowering the contractual base pay?” The question is conditional on revenue reduction and not doing wage reductions.



(c) By firm size



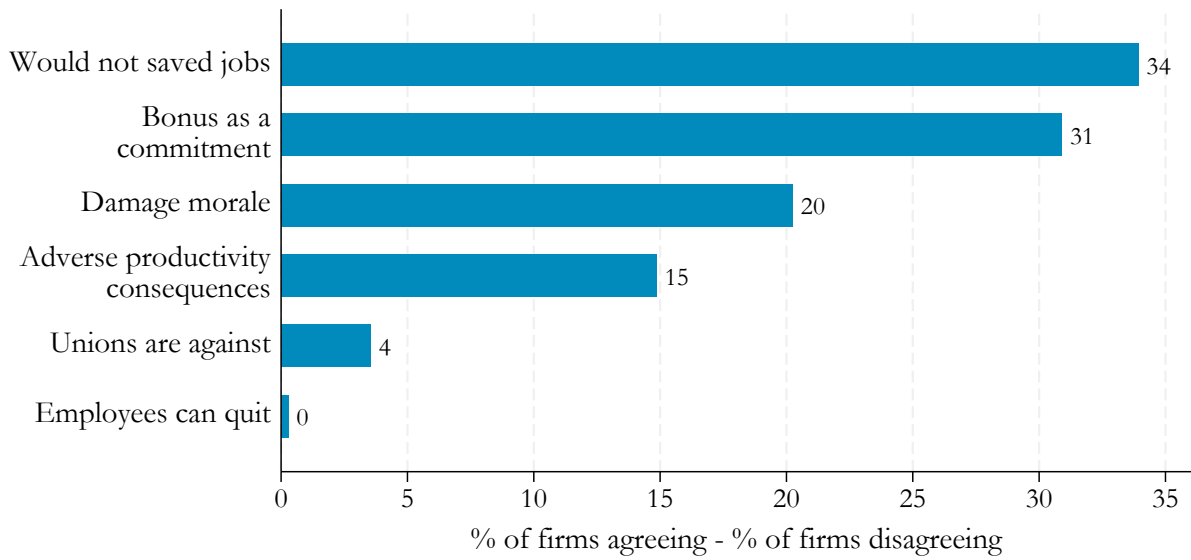
(d) By revenue growth



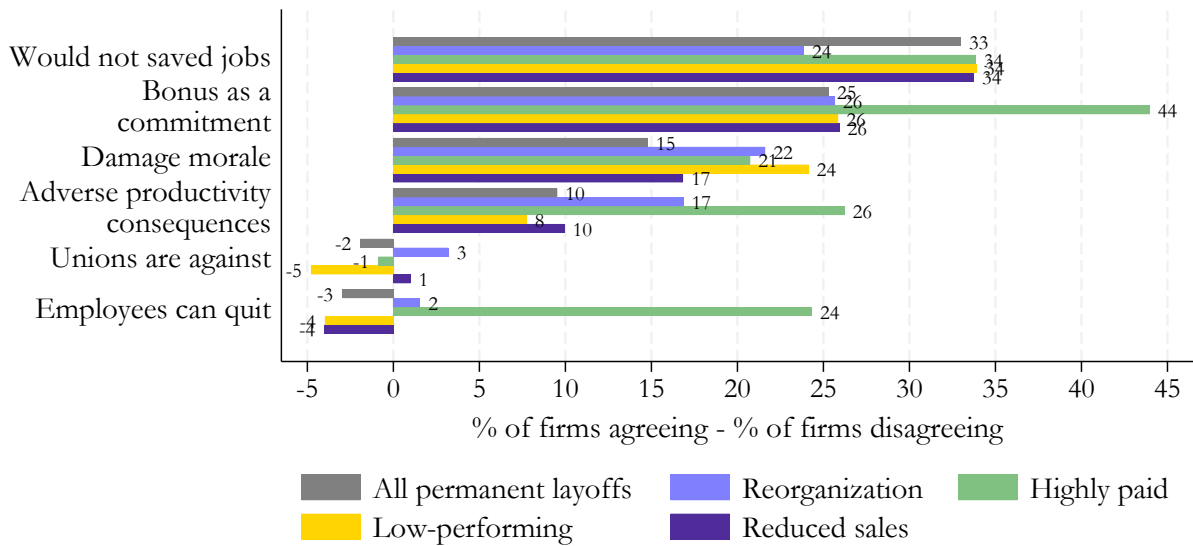
(e) By wage-setting

Figure A.15: Reasons for Not Lowering Base Pay (continued)

Note: The figure reports responses to the question: “What are the main reasons for not lowering the contractual base pay?” The question is conditional on revenue reduction and not doing wage reductions.



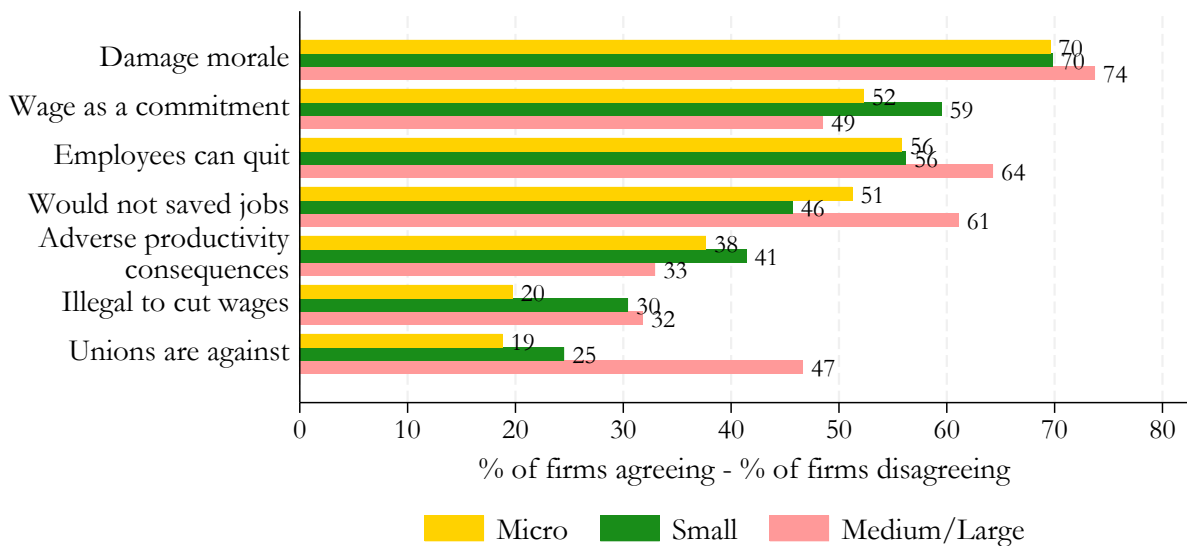
(a) Full sample



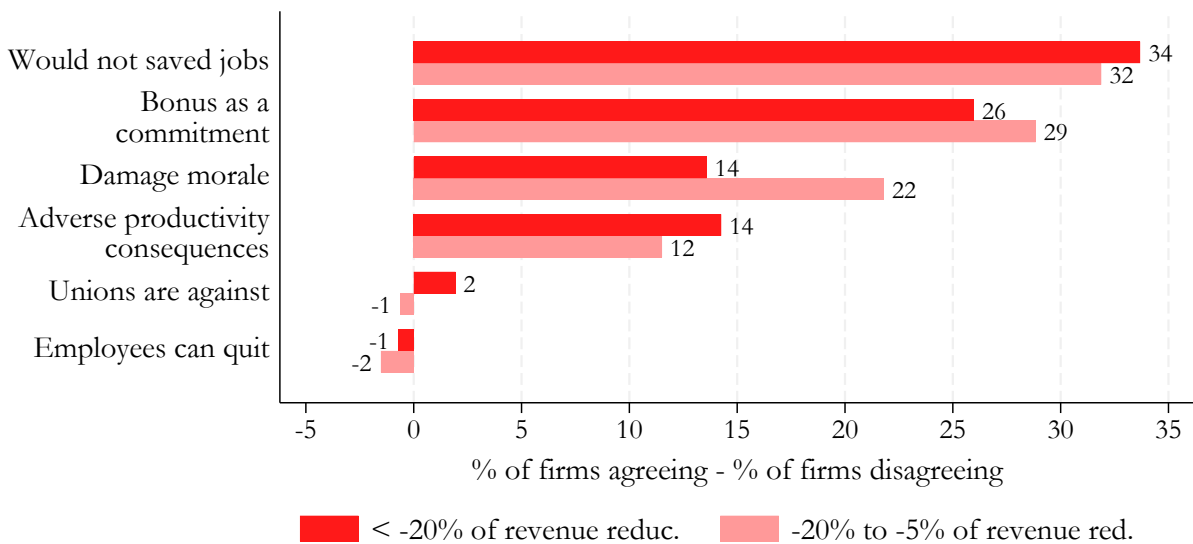
(b) By reason for permanent layoff

Figure A.16: Reasons for Not Lowering Bonus Pay

Note: The figure reports responses to the question: “What are the main reasons for not lowering non-contractual supplements and/or bonuses?” The question is conditional on revenue reduction and not doing “fewer/lower bonus”.



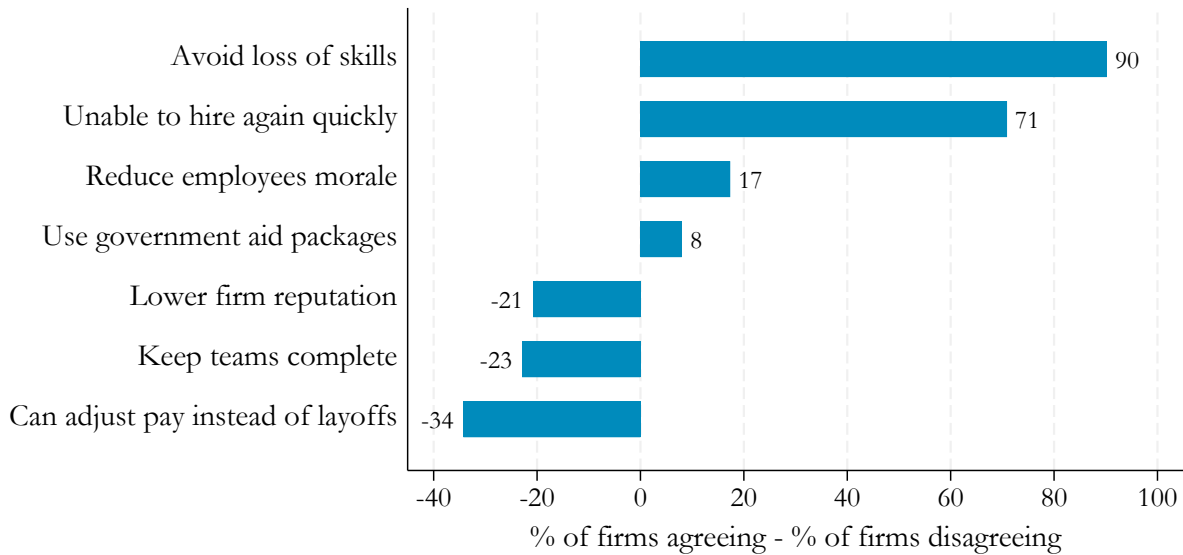
(c) By firm size



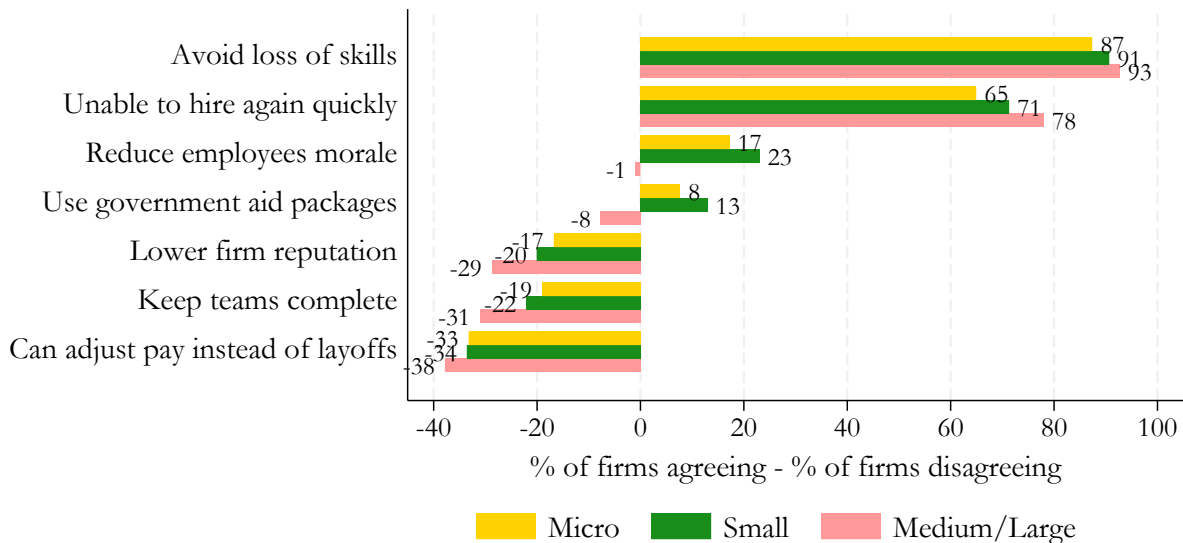
(d) By revenue growth

Figure A.16: Reasons for Not Lowering Bonus Pay (continued)

Note: The figure reports responses to the question: “What are the main reasons for not lowering non-contractual supplements and/or bonuses?” The question is conditional on revenue reduction and not doing “fewer/lower bonus”.



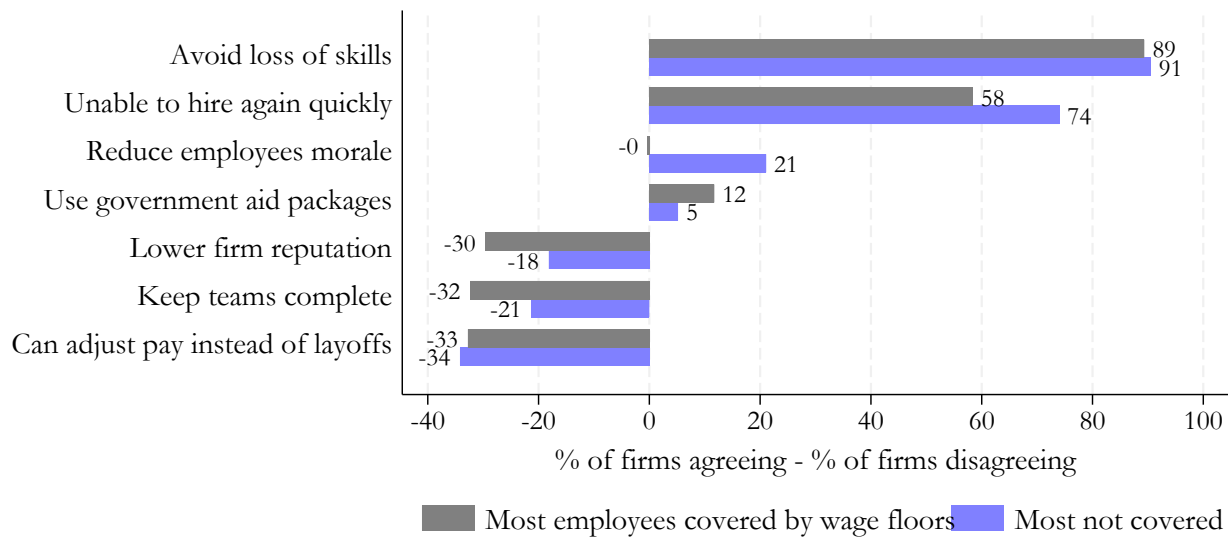
(a) Full sample



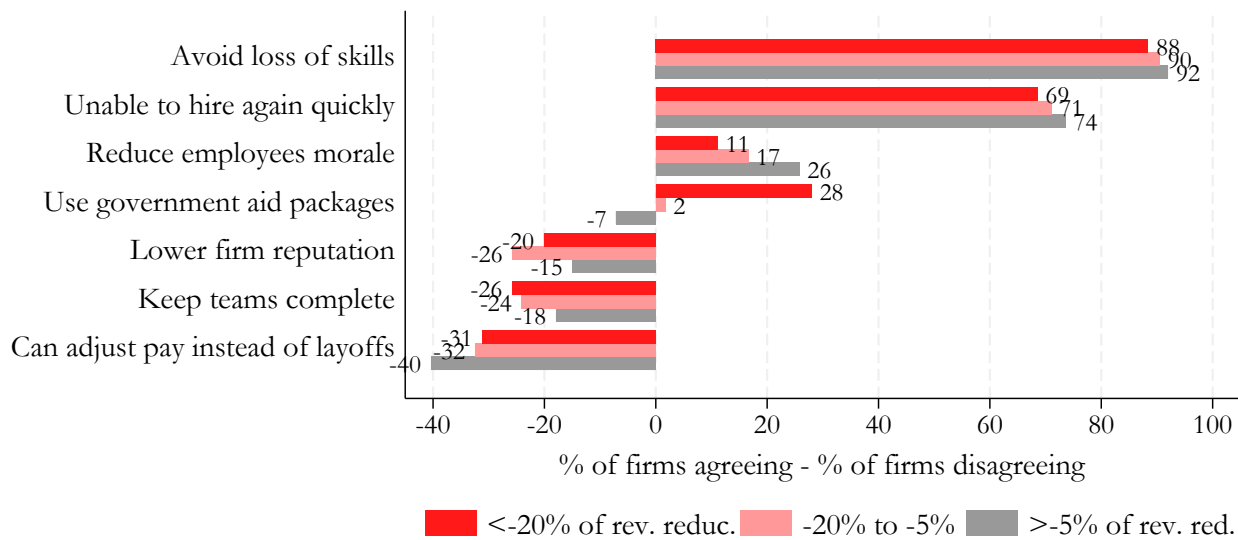
(b) By firm size

Figure A.17: Reasons for Retaining Employees despite Reduced Revenue

Note: The figure reports responses to the question, “What were the main reasons for retaining employees despite a reduction in sales and other cost pressures? Even if you have laid off some employees, consider why you have not laid off more.” The question is conditional on reporting a reduction in revenue in 2020. The exact statements that the respondent could choose from are: We want to keep current employees to avoid loss of skills and knowledge; We may not be able to find and hire again quickly when needed during recovery; The employees work in teams, and we cannot lay off some of them; Layoffs will be detrimental to morale among the remaining employees; We can use government aid packages; Instead of layoffs, we can reduce pay; Layoffs will be detrimental to the firm’s reputation.



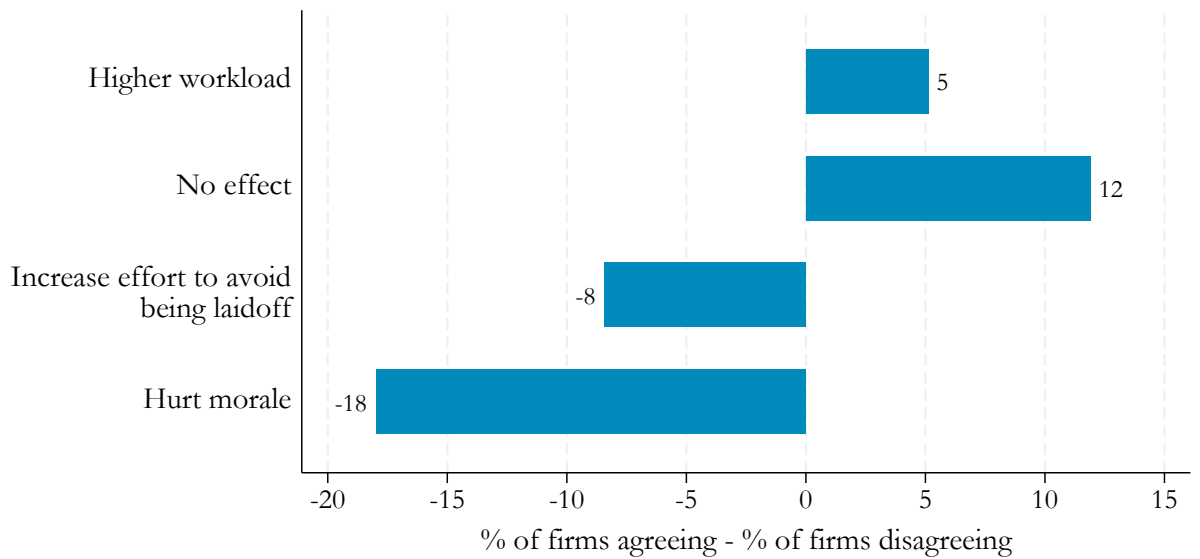
(c) By wage-setting



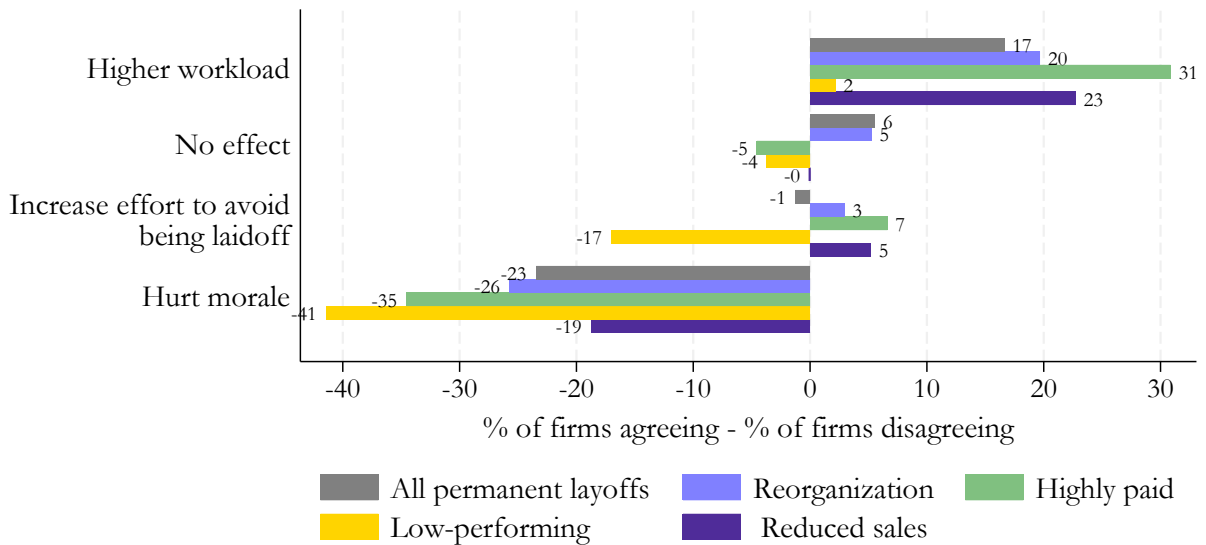
(d) By revenue growth

Figure A.17: Reasons for Retaining Employees despite Reduced Revenue (continued)

Note: The figure reports responses to the question, “What were the main reasons for retaining employees despite a reduction in sales and other cost pressures? Even if you have laid off some employees, consider why you have not laid off more.” The question is conditional on reporting a reduction in revenue in 2020. The exact statements that the respondent could choose from are: We want to keep current employees to avoid loss of skills and knowledge; We may not be able to find and hire again quickly when needed during recovery; The employees work in teams, and we cannot lay off some of them; Layoffs will be detrimental to morale among the remaining employees; We can use government aid packages; Instead of layoffs, we can reduce pay; Layoffs will be detrimental to the firm’s reputation.



(a) Full sample



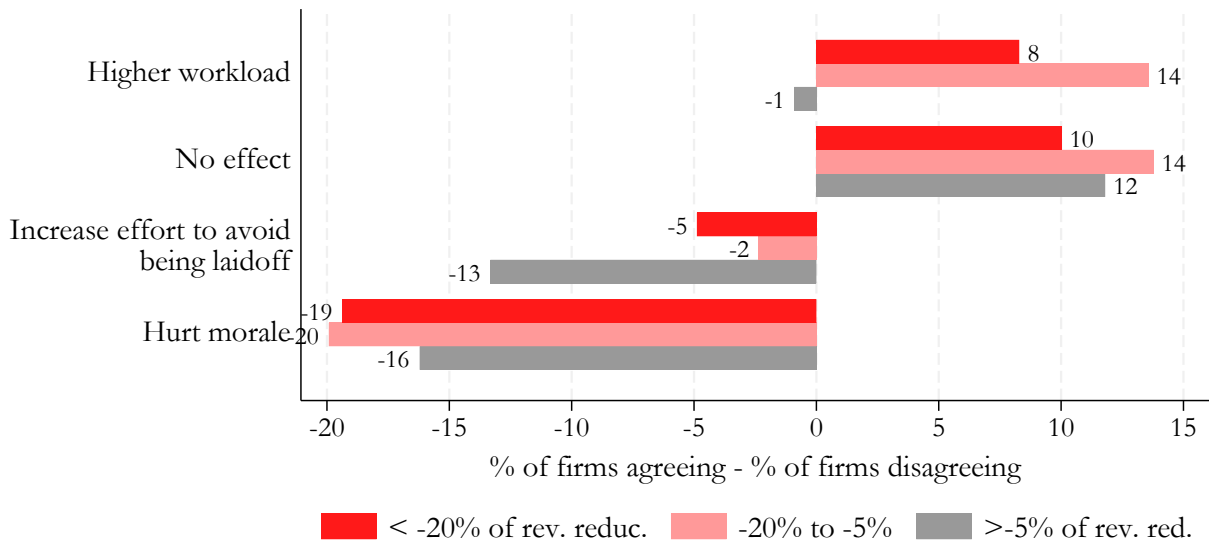
(b) By reason for permanent layoff

Figure A.18: The Perceived Consequences of Layoffs on the Remaining Employees

Note: The figure shows responses to the question, “How have layoffs affected the remaining employees?” The question was put to firms that reported having laid off employees in 2020. The statements are: Employees have a higher workload as there are fewer; Employees work harder to avoid being laid off; Layoffs hurt morale and work ethics among the remaining employees. There is no effect on the remaining employees.



(c) By firm size



(d) By revenue growth

Figure A.18: The Perceived Consequences of Layoffs on the Remaining Employees (continued)

Note: The figure shows responses to the question, “How have layoffs affected the remaining employees?” The question was put to firms that reported having laid off employees in 2020. The statements are: Employees have a higher workload as there are fewer; Employees work harder to avoid being laid off; Layoffs hurt morale and work ethics among the remaining employees. There is no effect on the remaining employees.

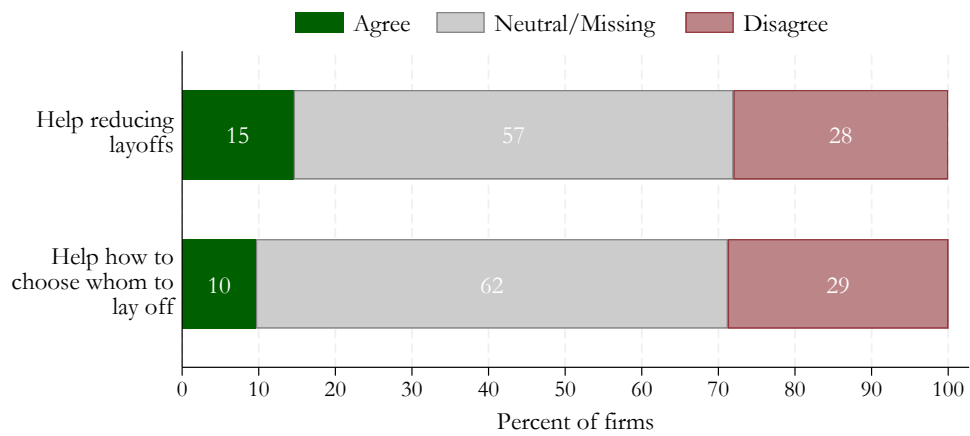
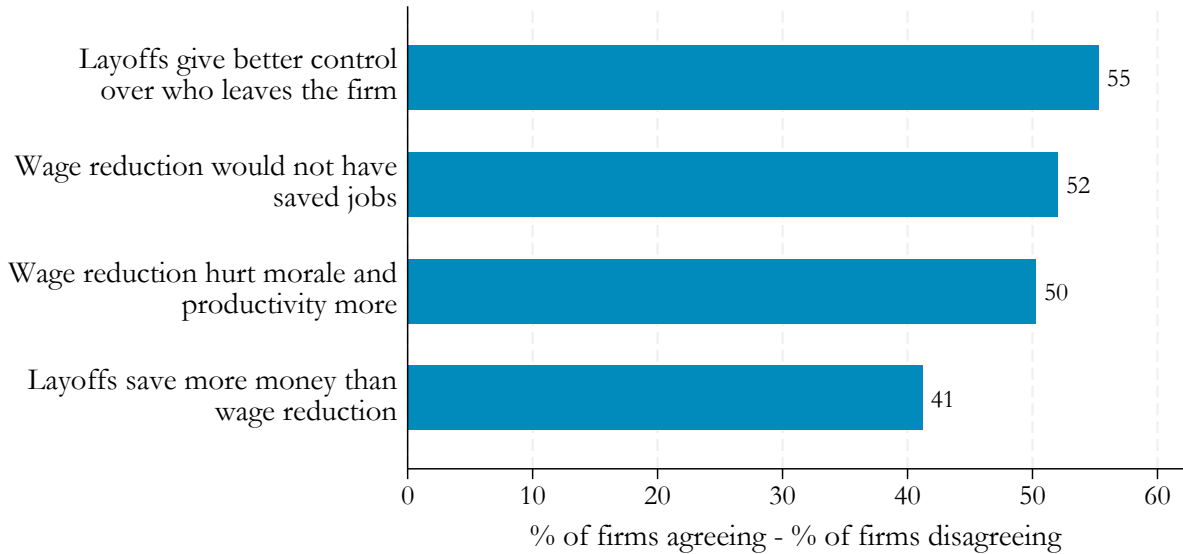
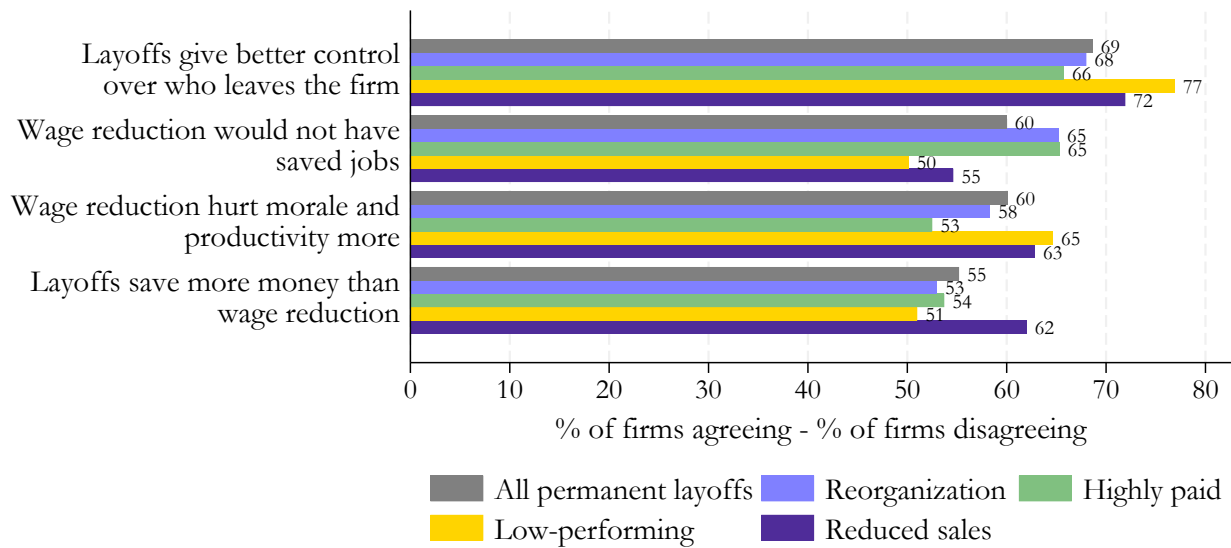


Figure A.19: The Role of Worker Representative in Layoff Decisions

Note: The figure reports responses to the question, “What is your position on the following statements? Union representatives help reduce the number of layoffs by finding alternative solutions to reduce wage costs (reorganization, wage reduction, etc.). Union representatives help implement layoffs by identifying low/high-performing employees or setting criteria for who can be laid off.”



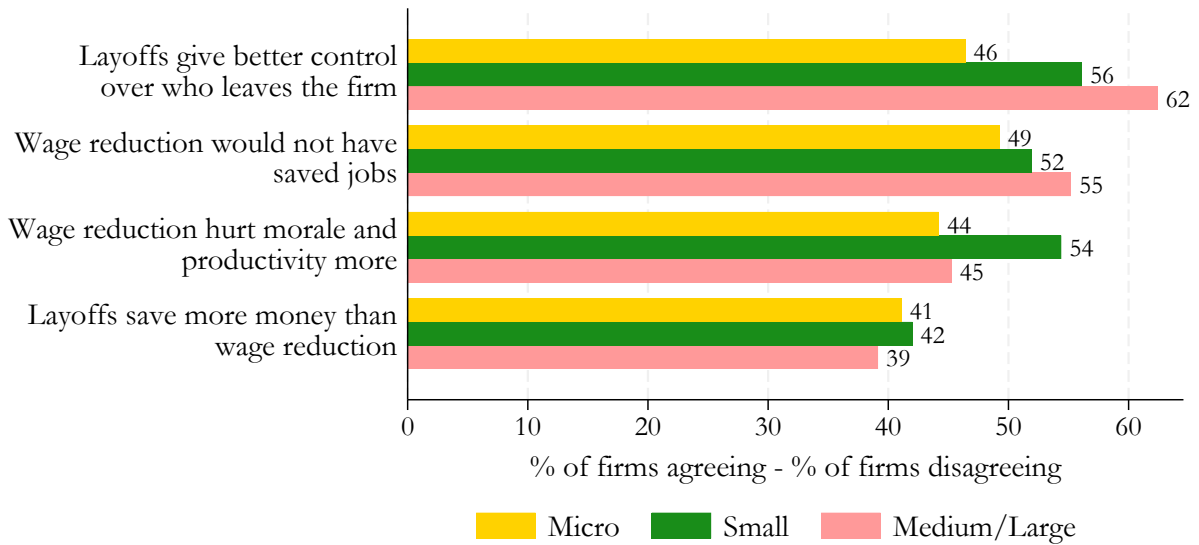
(a) Full sample



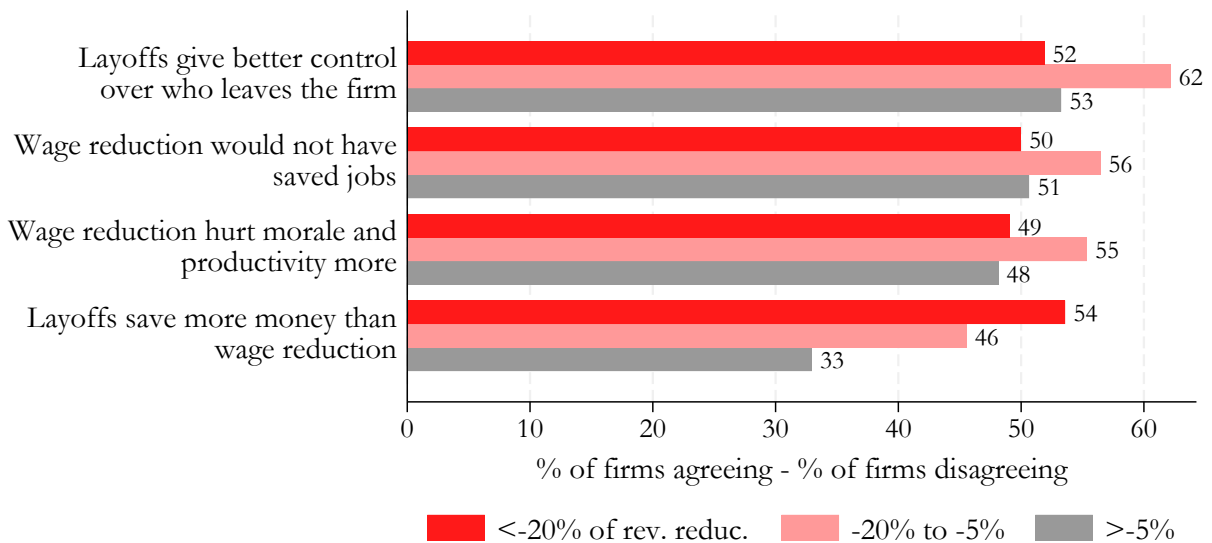
(b) By reason for permanent layoff

Figure A.20: Reasons for Layoffs instead of Pay Cuts

Note: The figure reports responses to the question: “Why didn’t you lower pay instead of laying off employees?” The question was put to firms that reported having laid off employees in 2020, which pertains to 1,129 firms. The statements are: Pay cuts would not have saved jobs; Pay cuts would hurt morale and productivity more than layoffs; Layoffs give better control over who leaves the firm; Layoffs save more money than pay cuts do.



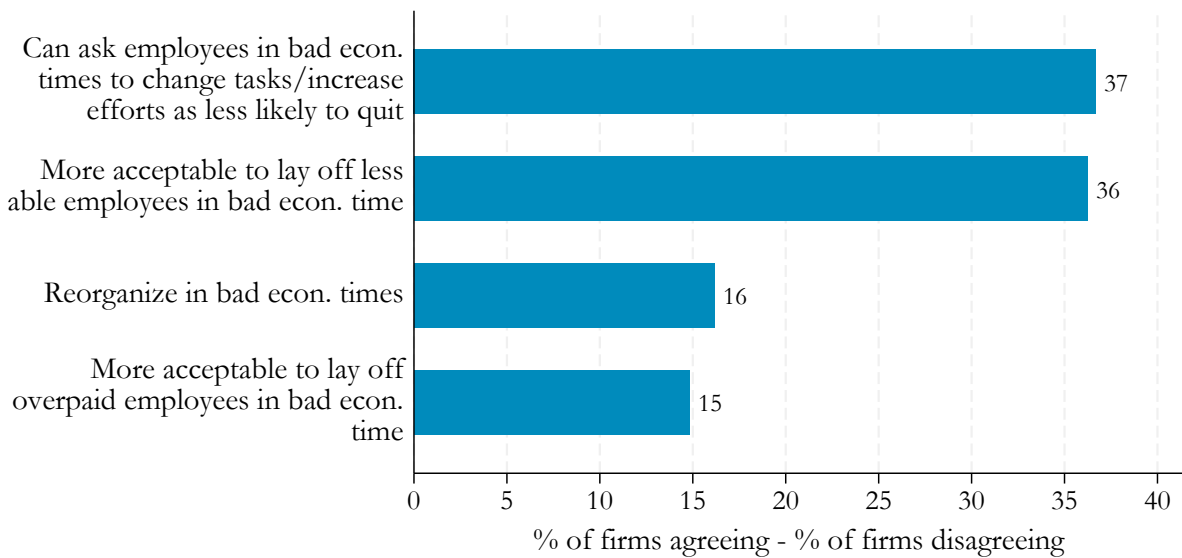
(c) By firm size



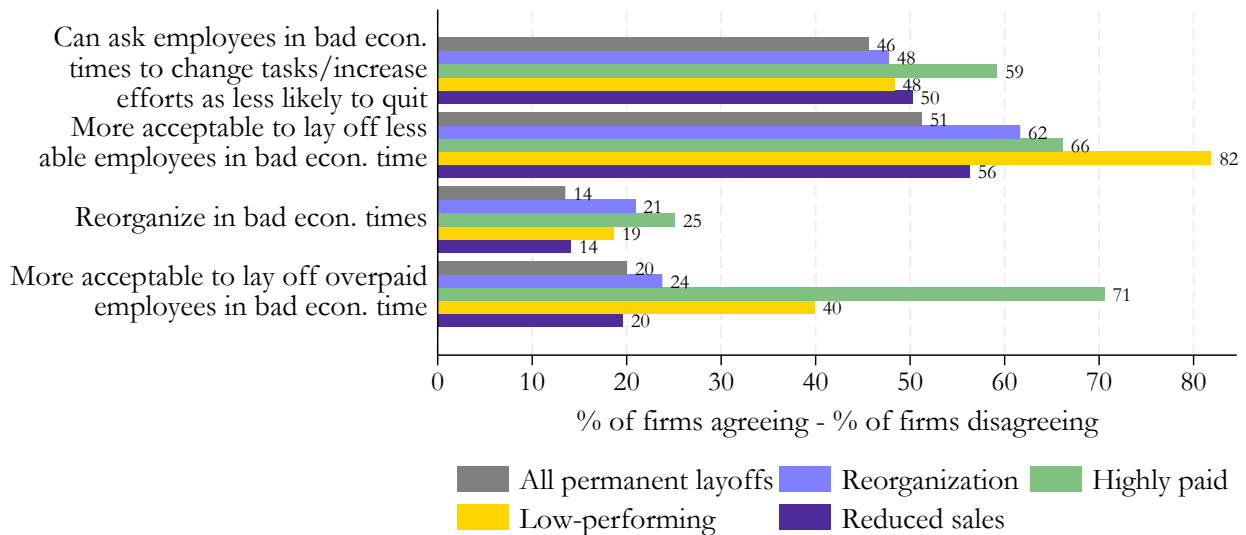
(d) By revenue growth

Figure A.20: Reasons for Layoffs instead of Pay Cuts (continued)

Note: The figure reports responses to the question: “Why didn’t you lower pay instead of laying off employees?” The question was put to firms that reported having laid off employees in 2020, which pertains to 1,129 firms. The statements are: Pay cuts would not have saved jobs; Pay cuts would hurt morale and productivity more than layoffs; Layoffs give better control over who leaves the firm; Layoffs save more money than pay cuts do.



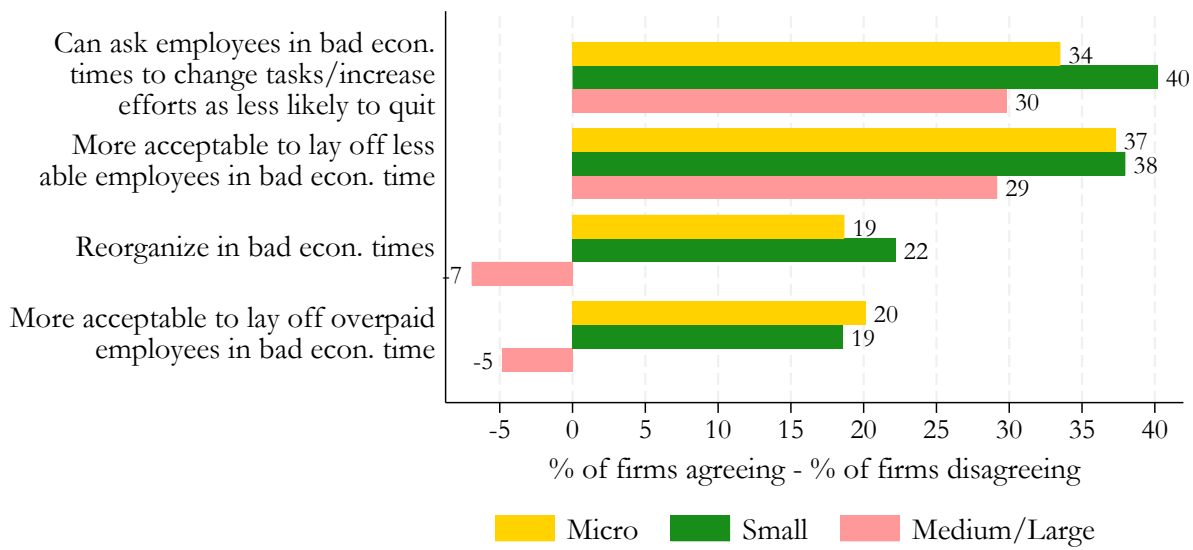
(a) Full sample



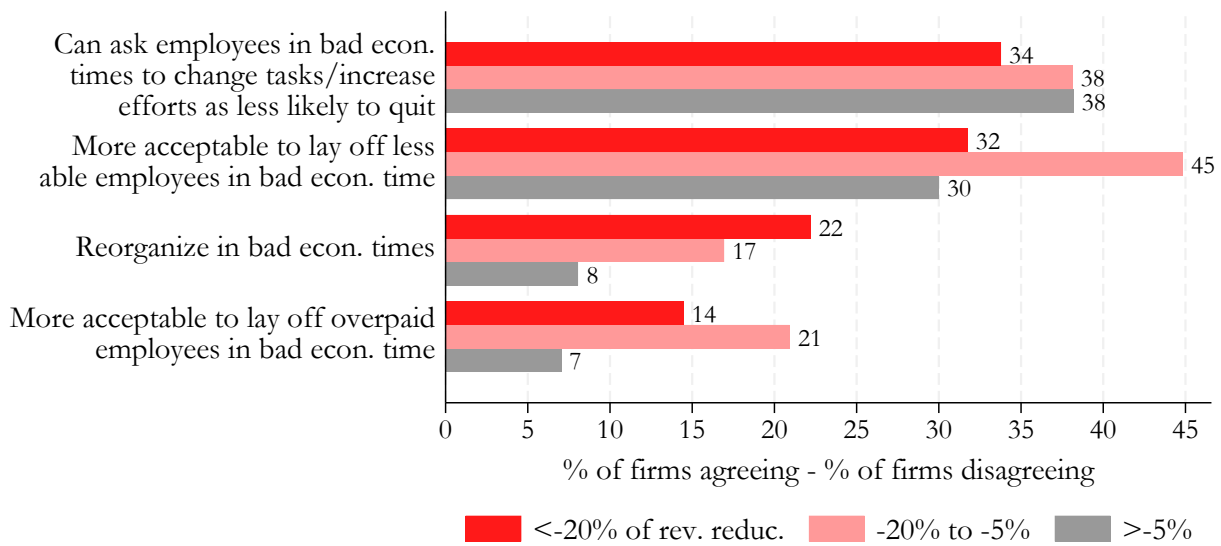
(b) By reason for permanent layoff

Figure A.21: Is Crisis an Opportune Time for Layoffs?

Note: The figure shows responses to the question “Do you agree with the following statements? Note: Even if you have laid off some employees, consider why you have not laid off more.” The question is asked of respondents who reported a reduction in revenue in 2020.



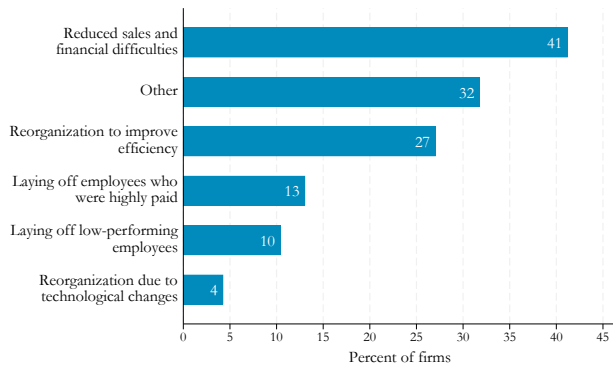
(c) By firm size



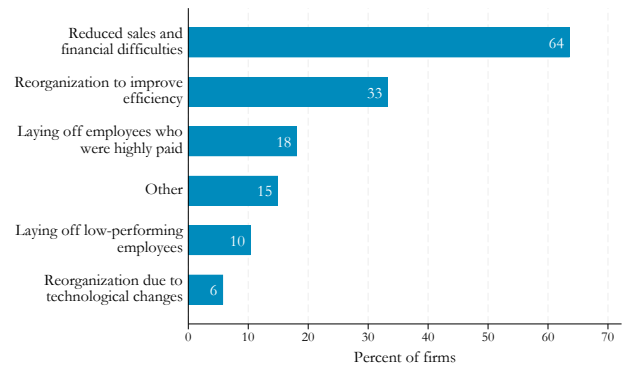
(d) By revenue growth

Figure A.21: Is Crisis an Opportune Time for Layoffs? (continued)

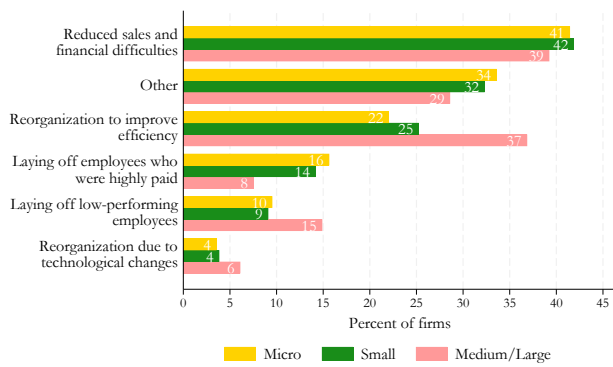
Note: The figure shows responses to the question “Do you agree with the following statements? Note: Even if you have laid off some employees, consider why you have not laid off more.” The question is asked of respondents who reported a reduction in revenue in 2020.



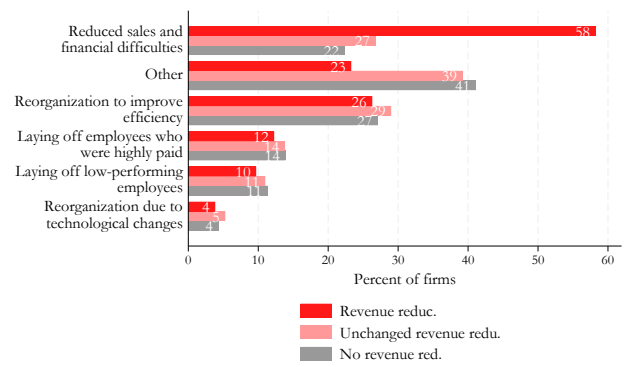
(a) All layoffs



(b) Permanent layoffs



(c) By firm size

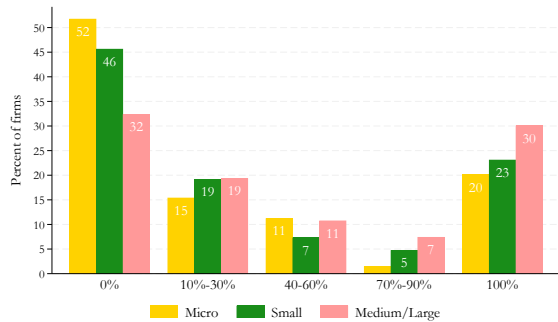


(d) By revenue growth

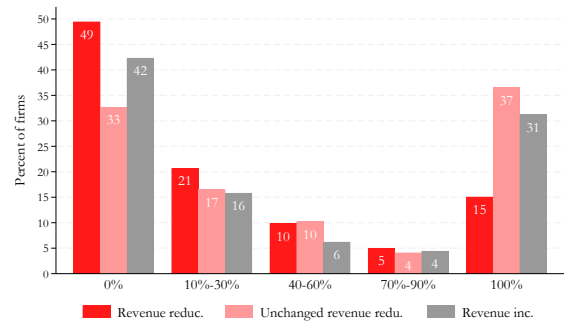
Figure A.22: Reasons for Layoffs

Note: The figure shows responses to the question, “What were the main reasons for the company’s layoffs in 2020? Check as many as apply.” Statements (abbreviated in the figure) correspond to: (1) Our company did not experience layoffs in 2020 (not shown in the figure); (2) Reduced sales and financial difficulties; (3) Reorganization due to technological changes; (4) Reorganization to improve efficiency (eliminate unnecessary labor); (5) Laying off employees who were highly paid relative to their productivity; (6) Laying off low-performing employees; (7) Other.<sup>a</sup>

<sup>a</sup>The most frequent open-ended “Other” responses, such as “get rid of poor performing employees” and “reduce staff,” overlap substantially with the existing coded categories. Because the open-ended responses were tabulated separately, they cannot be linked at the firm level to the coded responses, and we therefore do not reclassify them.



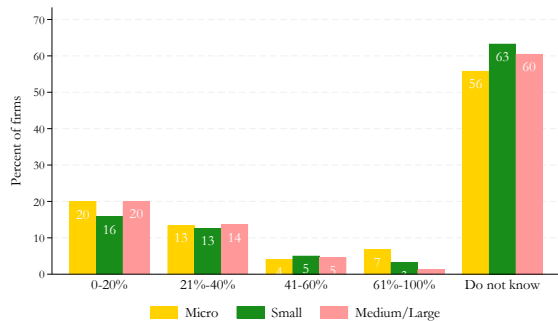
(a) By firm size



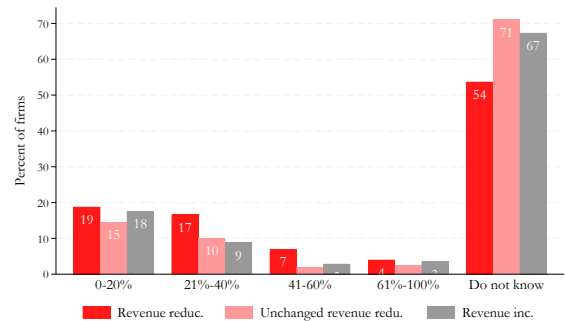
(b) By revenue growth

Figure A.23: Share of Layoffs that Would Have Occurred Even without the Pandemic Recession

Note: The figure reports responses to the question, “How many of these layoffs would have taken place in 2020 or over the next two years if there had not been a pandemic?”. The question was put to firms that reported having laid off employees in 2020.



(a) By firm size



(b) By revenue growth

Figure A.24: What Reduction in the Total Wage Costs Could Have Prevented Layoffs?

Note: The figure reports responses to “What reduction in the total wage cost (base pay and bonuses) could have prevented layoffs?”. The question is put to respondents that laid off workers in 2020. The options are: 0-20 percent; 21-40 percent; 41-60 percent; 61-80 percent; more than 80 percent; Do not know.

Table A.1: Response Rate in the Survey and in the Target Population

	Number of observations
<i>Response rate after sample restriction from administrative datasets:</i>	
Response rate to the survey	14.19% (4164/29349)
1) Refuse to participate	17.32% (721/4164)
2) Missing responses to some questions	14.87% (619/4164)
3) All questions answered	67.82% (2824/4164)
Participation rate adjusted for non-response (1)	11.73% (3443/29349)
<i>Sample Restriction:</i>	
1) Little human resources policies knowledge	9.58% (330/3443)
2) Missing answers to key questions	5.26% (181/3443)
3) Incoherent answers to key questions	1.60% (55/3443)
Restriction 1) 2) and 3)	11.18% (385/3443)
Number of observations after sample restriction 1), 2), and 3) is 3013.	

Note: 29,349 firms represent the population of private and public limited firms (ApS and A/S) that were invited by email by Ramboll to participate in the survey, and also they had at least five employees in 2019. Firms in the agricultural and mining sectors are not in Ramboll’s sample frame. The row “Little human resources policies knowledge” refers to the number of respondents that chose the answer “I know only a little about pay and employment conditions.” or “Do not know” to the question, “In the following questions, we ask about pay and employment practices. How close are you to such decisions?” We deleted the responses from such respondents from our analysis. The row “Missing answers” refers to the number of respondents who do not answer at least 10 questions out of the first 34 questions in our survey. The row “Incoherent answers” refers to the number of respondents with contradictory responses.

Table A.2: Data Sources and Variables

Name	Description	Dataset name	Variable used in dataset
<i>Firm demographics in 2019:</i>			
Age	Number of years in business in 2019	FIRM	JUR_FRA_DATO
Firm size in FTE	Number of employees in full-time equivalent (FTE) in 2019, calculated as annual total hours / 1924	BFL	AJO_LOENTTIMER
Firm size	Number of employees in 2019	FIRM	GF_ANSATTE
Industry	10 Industry categories	FIRM	GF_GR019_DB07
Wage floors	=1 if 50% of employees are subject to the <i>normallønssystemet</i> wage-setting (i.e. wages are mainly set at the industry level)	—	—
<i>Firm financial characteristics in 2019:</i>			
Revenue growth	Revenue growth from 2019 to 2020	FIRM	GF_OMS
Productivity	Value added over firm size	FIRM	GF_VTV
Labor costs	Compensation including social security costs	FIRM	GF_LGAGMV
Average wages	Labor costs / Firm size (FTE)		
<i>Workforce characteristics in 2019:</i>			
Female	Percent of female in 2019	IDAP	KON
Unionization	Percent of unionized workers	IND	FAGFKD
Education	Average educational attainment	UDDA	HFAUDD
Age	Average age	IDAP	ALDERNOV
<i>Analysis of earnings and hours worked at the individual-level:</i>			
Hours	Annual total hours paid	BFL	AJO_LOENTTIMER
Earnings	Annual total earnings	BFL	AJO_BREDT_LOENBELOEB
Hourly wage	Earnings / hours	BFL	
Base pay	Annual base pay	LONN	BASIS_STAND
Total pay	Annual total pay	LONN	FORTJ_STAND
<i>Aggregate conditions:</i>			
Tightness	Firm-specific labor market tightness	—	—
Location	11 regions (NUTS3)	FIRM	JUR_BEL_REGION_KODE

Table A.3: Sample Description of the Linked Survey-Administrative Data

	(1)	(2)	(3)
	Population	Sample	Weighted sample
<b>Firm characteristics</b>			
Number of employees	39.15	47.29	39.16
Age	16.59	19.13	18.80
Revenue growth in 2020 (%)	3.02	3.31	3.50
Value added per worker ('000 EUR)	85.76	92.16	91.79
Labor costs per worker ('000 EUR)	64.68	68.86	68.79
In the manufacturing sector (%)	13.84	17.13	13.84
In the services sector (%)	60.67	60.07	60.67
In other sectors (%)	25.49	22.80	25.49
Wage floors (%)	15.27	16.98	16.94
<b>Employee characteristics</b>			
Female (%)	28.68	29.01	28.85
Age	40.09	42.09	41.97
Unionized workers (%)	53.49	58.50	58.16
<b>Labor market characteristics</b>			
Tightness (vacancy/unemployment)	0.11	0.11	0.11
Observations	29349	3013	3013

*Note:* Column (1) reports population means for Danish private firms with  $\geq 5$  employees. Column (2) reports the unweighted respondent sample, and column (3) reports the entropy-balanced sample (Section 2.4). All characteristics are from administrative sources.

Table A.4: Sample Description: Subsample of Firms that Report Revenue Reduction

	(1)	(2)	(3)
	Population	Sample	Weighted Sample
<b>Firm characteristics</b>			
Number of employees (FTE)	40.90	48.66	41.16
Age	17.18	19.31	18.89
Revenue growth in 2020 (%)	-21.50	-12.67	-12.59
Value added per worker ('000 EUR)	85.27	87.98	87.60
Labor costs per worker ('000 EUR)	64.29	66.57	66.52
In the manufacturing sector (%)	14.83	19.05	15.52
In the services sector (%)	63.51	64.26	65.64
In other sectors (%)	21.66	16.69	18.84
Wage floors (%)	14.90	17.15	17.18
<b>Employee characteristics</b>			
Female (%)	30.47	31.73	31.68
Age	40.12	41.80	41.71
Furloughed workers in 2020 (%)	23.89	29.92	29.72
Unionized workers (%)	52.21	57.18	56.69
<b>Labor market characteristics</b>			
Tightness (vacancy/unemployment)	0.11	0.10	0.10
Observations	14680	845	845

Note: The table compares the mean of firm characteristics from the sample to the corresponding population of firms. Column 1 reports means from the population, i.e., firms with at least 5 full-time employees. Column 2 reports means from the raw sample and Column 3 from the sample weighted by entropy balancing as described in Section 2.

Table A.5: Firm Characteristics Associated with Reasons for Not Lowering Base Pay

	Commitment to employees (1)	Productivity (shirking) (2)	Quit concern (3)	Morale concern (4)	Union is against (5)	Pay cut would not saved jobs (6)
Productivity	1.53 (2.50)	-0.70 (2.40)	-4.59** (2.25)	-0.54 (2.11)	6.63** (2.70)	0.13 (2.61)
Average wages	1.07 (2.48)	-0.14 (2.36)	4.82* (2.53)	5.26* (2.97)	-9.17*** (2.50)	0.35 (2.74)
Revenue growth rate in 2020 (%)	2.22 (2.29)	1.63 (2.33)	4.05* (2.13)	2.12 (1.85)	-0.47 (2.26)	1.19 (2.30)
Routine task index	1.35 (1.49)	3.41** (1.56)	2.16 (1.42)	0.61 (1.20)	6.61*** (1.50)	2.50* (1.47)
Unionization (%)	-1.75 (2.00)	-0.01 (2.05)	0.30 (1.95)	0.24 (1.83)	1.69 (2.05)	2.25 (1.95)
Worker representative	-4.91 (4.30)	8.74* (4.68)	-2.59 (4.23)	1.60 (3.52)	10.88** (4.67)	-3.17 (4.31)
Tightness	-0.15 (2.05)	-0.65 (2.25)	5.15** (2.15)	0.14 (2.05)	4.47* (2.41)	-0.27 (2.10)
Number of employees	-0.24 (2.62)	-4.10 (2.94)	0.67 (2.70)	3.54 (2.34)	3.50 (3.39)	0.43 (2.57)
Firm age	-1.11 (2.03)	-3.40* (2.05)	-2.47 (1.92)	0.09 (1.66)	2.37 (2.02)	-0.19 (1.95)
Debt ratio	1.95 (2.90)	3.71 (2.63)	3.13 (2.29)	-0.93 (2.25)	-2.17 (2.90)	-1.76 (2.70)
<i>N</i>	580	575	581	581	573	579
Mean Dep. Var.	3.62	3.43	3.63	3.78	3.31	3.56
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports marginal effects of the probability of agreeing or strongly agreeing from ordered probit models where covariates are evaluated at their means. Additional controls are a dummy for the family firm and the job of the respondent. Asterisks report statistical significance at the 1, 5 and 10% (\*\*\*, \*\*, \* respectively). Standard errors are reported in parentheses. The question is, “What are the main reasons for not lowering the contractual base pay? Please state your position on the following statement.” The company sees the base pay as a commitment to its employees; Pay cuts can damage productivity because employees do not work as hard; Pay reduction would lead employees to quits; Pay reduction damages morale and is demotivating for employees in general; Trade unions/employee representatives are against pay cuts; Pay reductions would not save jobs.

Table A.6: Firm Characteristics Associated with Reasons to Retain Employees Despite Reduced Revenue

Statement:	Lose skills (1)	Unable to re-hire (2)	Team concern (3)	Morale concern (4)	Use gov. aid package (5)	Can reduce pay instead (6)	Reputation concern (7)
Productivity	-0.63 (1.02)	-2.14 (1.89)	2.53 (1.65)	0.47 (2.26)	-4.36 (2.67)	-2.05 (1.72)	1.41 (1.62)
Average wages	1.32 (1.50)	1.55 (1.73)	-1.38 (1.70)	1.51 (2.52)	-6.58** (3.01)	1.98 (1.62)	-1.25 (1.69)
Revenue growth rate in 2020 (%)	1.72* (0.89)	0.94 (1.83)	1.38 (1.46)	2.83 (2.12)	-4.21* (2.26)	1.33 (1.21)	0.07 (1.11)
Routine task index	-1.19** (0.55)	0.67 (1.09)	-0.73 (0.87)	-4.16*** (1.30)	-1.07 (1.30)	-1.40 (0.88)	-0.60 (0.88)
Unionization (%)	-0.15 (0.85)	-0.26 (1.52)	-0.09 (1.25)	-2.93 (1.80)	-3.88** (1.94)	0.25 (1.18)	1.07 (1.20)
Worker representative	0.74 (1.60)	4.66 (3.26)	0.28 (2.61)	0.05 (3.91)	-6.40 (4.06)	1.29 (2.58)	4.56* (2.56)
Tightness	-0.56 (0.95)	1.74 (1.62)	-0.32 (1.27)	-1.16 (2.03)	-2.53 (2.17)	-2.26* (1.29)	-1.95 (1.33)
Number of employees	0.69 (1.02)	0.34 (1.82)	-1.81 (1.47)	-3.39 (2.27)	-2.93 (2.36)	-0.98 (1.70)	-1.07 (1.47)
Firm age	2.11** (0.89)	3.80** (1.56)	-0.62 (1.23)	1.08 (1.81)	2.26 (1.79)	-0.34 (1.25)	-0.87 (1.18)
Debt ratio	-1.86** (0.85)	-2.59* (1.40)	-0.82 (1.34)	-2.45 (1.72)	5.35** (2.13)	1.09 (1.48)	-1.49 (1.37)
<i>N</i>	752	738	725	724	736	727	731
Mean Dep. Var.	3.93	3.75	2.74	3.18	3.07	2.64	2.78
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table reports marginal effects of the probability of agreeing (or strongly agreeing) from ordered probit models where covariates are evaluated at their means. Figure 5 in the main text reports the histogram of this question. Additional controls are a dummy for the family firm and the job of the respondent. Asterisks report statistical significance at the 1, 5 and 10% (\*\*\*, \*\*, \* respectively). Standard errors are reported in parentheses. The survey question is “What were the main reasons for retaining employees despite a reduction in sales and other cost pressures? Even if you have laid off some employees, consider why you have not laid off more.” The question was asked of those firms that reported a reduction in revenue in 2020. Possible statements are: We want to keep current employees to avoid loss of skills and knowledge (1); We may be unable to find and hire again quickly when needed during the recovery (2); The employees work in teams and we cannot lay off some of them (3); Layoffs will be detrimental to morale among the remaining employees (4); We can use government aid packages (5); Instead of layoffs, we can reduce pay (6); Layoffs will be detrimental for the firm’s reputation (7).

Table A.7: Firm Characteristics Associated with Perceptions of the Effects of Layoffs on the Remaining Employees

Question: How have layoffs affected the remaining employees of the firm? Statement:	Higher workload (as fewer employees) (1)	Greater effort (to avoid layoffs) (2)	Hurt morale (3)	No effect (4)
Productivity	-0.16 (1.79)	-1.66 (1.39)	-0.53 (1.55)	3.23 (2.05)
Average wages	-1.09 (2.02)	1.61 (1.47)	0.10 (1.73)	0.62 (2.21)
Revenue growth rate in 2020 (%)	-0.47 (1.47)	-1.44 (1.13)	1.31 (1.19)	-0.23 (1.48)
Routine task index	-0.45 (0.99)	0.35 (0.80)	0.18 (0.83)	1.62 (1.09)
Unionization (%)	-0.27 (1.51)	0.88 (1.20)	-0.78 (1.26)	-2.29 (1.56)
Worker representative	1.89 (3.01)	3.65 (2.35)	2.99 (2.43)	-6.72** (3.20)
Tightness	-2.08 (1.47)	-2.41** (1.16)	1.03 (1.10)	-1.71 (1.54)
Number of employees	-1.91 (1.63)	-1.45 (1.14)	-0.56 (1.37)	1.25 (1.70)
Firm age	2.11 (1.31)	0.04 (1.07)	-0.13 (1.10)	-0.55 (1.42)
Debt ratio	1.93 (1.52)	0.69 (1.02)	-0.57 (1.31)	-2.48* (1.48)
<i>N</i>	977	972	971	984
Mean Dep. Var.	3.06	2.9	2.8	3.12
Additional controls	Yes	Yes	Yes	Yes

Note: The table reports marginal effects of the probability of agreeing (or strongly agreeing) from ordered probit models where covariates are evaluated at their means. Figure 6 in the main text reports the histogram of this question. Additional controls are a dummy for the family firm and the job of the respondent. Asterisks report statistical significance at the 1, 5 and 10% (\*\*\*, \*\*, \* respectively). Standard errors are reported in parentheses. The question is, “How have layoffs affected the remaining employees of the firm? Please state your opinion on the following statement.” The question was asked for firms that reported having laid off workers in 2020 (with or without a reduction in revenue). The statements are: Employees have higher workload as there are fewer (1); Employees work harder to avoid being laid off (2); Layoffs hurt morale and work ethics among the remaining employees (3); There is no effect on the remaining employees (4).

Table A.8: Firm Characteristics Associated with Attitudes towards Crisis as an Opportune Time for Layoffs

	Reorganize	More acceptable to lay off		Easier to change tasks
		less able workers	overpaid worker	as workers less likely to quit
	(1)	(2)	(3)	(4)
Productivity	-2.25 (2.41)	-0.70 (2.28)	-4.29* (2.33)	-0.66 (2.29)
Average wages	0.47 (2.56)	-0.18 (2.43)	4.39** (2.20)	2.07 (2.27)
Revenue growth rate in 2020 (%)	-2.63 (1.93)	-0.74 (1.73)	-1.41 (1.66)	2.27 (1.98)
Routine task index	-2.03 (1.27)	-1.79 (1.40)	-0.46 (1.31)	1.53 (1.41)
Unionization (%)	0.00 (1.85)	3.54* (1.85)	1.29 (1.73)	0.66 (1.86)
Worker representative	-0.87 (3.88)	1.32 (4.03)	3.80 (3.76)	-1.99 (3.98)
Tightness	2.00 (2.20)	4.86** (2.23)	0.16 (1.90)	-1.05 (2.08)
Number of employees	-5.32** (2.44)	-3.31 (2.04)	-5.81*** (2.20)	-1.45 (2.36)
Firm age	-0.29 (1.92)	2.46 (1.83)	-1.29 (1.69)	0.27 (1.79)
Debt ratio	1.51 (2.63)	-1.13 (2.09)	2.27 (1.94)	-1.25 (2.29)
<i>N</i>	733	736	730	732
Mean Dep. Var.	3.16	3.38	3.13	3.37
Additional controls	Yes	Yes	Yes	Yes

Note: The table reports marginal effects of the probability of agreeing (or strongly agreeing) from ordered probit models where covariates are evaluated at their means. Additional controls are a dummy for the family firm and the job of the respondent. Asterisks report statistical significance at the 1, 5 and 10% (\*\*\*, \*\*, \* respectively). Standard errors are reported in parentheses. The question is: “Do you agree with the following statements? Note: Even if you have laid off some employees, consider why you have not laid off more.” The statements are: (1) Management has less focus on efficiency and cost reductions during good times and therefore the firm reorganizes during bad economic conditions; (2) It is more acceptable to lay off the less able employees during bad economic conditions; (3) It is more acceptable to lay off employees who are highly paid relative to their productivity during bad economic conditions; (4) It is easier to ask employees to change their tasks / increase their work effort in bad times, as employees are less likely to quit during bad economic conditions.

Table A.9: Firm Characteristics Associated with Reasons for Layoffs Instead of Pay Cuts

Question: Why didn't you lower pay instead of laying off employees? Statement:	Pay reduction would		Layoffs	
	not have saved jobs	hurt morale and productivity more than layoffs	give better control over who leaves	save more money than pay cuts
	(1)	(2)	(3)	(4)
Productivity	-2.69 (2.01)	0.23 (2.00)	3.59* (2.18)	-0.71 (2.26)
Average wages	2.15 (2.04)	2.98 (2.14)	-1.61 (2.05)	-2.06 (2.27)
Revenue growth rate in 2020 (%)	-0.29 (1.45)	2.40 (1.65)	1.87 (1.55)	-1.36 (1.54)
Routine task index	-0.19 (1.04)	1.61 (1.12)	1.11 (1.04)	3.04*** (1.13)
Unionization (%)	0.07 (1.55)	1.78 (1.67)	3.52** (1.61)	1.40 (1.78)
Worker representative	2.15 (3.25)	1.82 (3.48)	2.14 (3.24)	-5.40 (3.49)
Tightness	-2.21 (1.44)	0.03 (1.56)	-3.06** (1.47)	-1.57 (1.76)
Number of employees	0.45 (1.88)	-0.19 (2.06)	0.86 (2.04)	-1.97 (1.94)
Firm age	1.36 (1.53)	0.97 (1.57)	3.31** (1.60)	0.07 (1.56)
Debt ratio	2.01 (1.92)	1.68 (2.26)	4.37** (1.96)	3.53* (1.95)
<i>N</i>	943	933	932	938
Mean Dep. Var.	3.63	3.62	3.68	3.51
Additional controls	Yes	Yes	Yes	Yes

Note: The table reports marginal effects of the probability of agreeing (or strongly agreeing) from ordered probit models where covariates are evaluated at their means. Figure 9 presents the histogram of this question. Additional controls are a dummy for the family firm and the job of the respondent. Asterisks report statistical significance at the 1, 5 and 10% (\*\*\*, \*\*, \* respectively). Standard errors are reported in parentheses. The survey question is “Why didn't you lower pay instead of laying off employees?” The statements are: Pay reduction would not have saved jobs; Pay reduction would hurt morale and productivity more than layoffs; Layoffs give better control over who leaves the company; Layoffs save more money than pay cuts do.

Table A.10: How Many Layoffs Would Have Occurred Even without the Pandemic Recession

	(1)	(2)	(3)
Productivity	1.09 (2.01)	0.11 (2.02)	-0.94 (1.83)
Average wages	1.63 (1.88)	1.19 (1.89)	0.11 (1.58)
Revenue growth rate in 2020 (%)	4.82*** (1.51)	5.14*** (1.52)	2.54** (1.24)
Routine task index	-1.11 (1.05)	-1.64 (1.04)	-0.68 (0.93)
Unionization (%)	3.12** (1.51)	2.31 (1.57)	2.39* (1.38)
Worker representative	-0.55 (2.95)	-3.18 (3.17)	-1.06 (2.94)
Tightness	-1.60 (1.78)	-2.17 (1.86)	-2.30 (1.82)
Layoff: Reduced Sales			-25.57*** (3.62)
Layoff: Technological change			11.65* (6.63)
Layoff: Improve efficiency			13.11*** (3.40)
Layoff: Highly paid			2.54 (4.14)
Layoff: Low-performing			15.14*** (4.70)
Layoff: Other			-15.09*** (4.37)
<i>N</i>	771	771	771
Mean Dep. Var.	30.92	30.92	30.92
Adj.R2	0.025	0.034	0.207
Additional controls	No	Yes	Yes

Note: The dependent variable is the reported share of the layoffs that would have occurred in 2020 or the next 2 years if not for the pandemic. The mean of the dependent variable is 34%.

Table A.11: The Probability of “Do Not Know” Answer to the Question about the Reduction in the Total Wage Costs Could Have Prevented Layoffs

	(1)	(2)	(3)
Productivity	0.06** (0.02)	0.05** (0.02)	0.04* (0.02)
Average wages	-0.02 (0.02)	-0.02 (0.02)	-0.03 (0.02)
Revenue growth rate in 2020 (%)	0.01 (0.02)	0.02 (0.02)	0.00 (0.02)
Routine task index	0.00 (0.01)	-0.00 (0.01)	0.00 (0.01)
Unionization (%)	0.03 (0.02)	0.02 (0.02)	0.02 (0.02)
Worker representative	-0.15*** (0.04)	-0.15*** (0.04)	-0.14*** (0.04)
Tightness	-0.01 (0.02)	-0.02 (0.02)	-0.02 (0.02)
Layoff: Reduced Sales			-0.19*** (0.05)
Layoff: Technological change			0.08 (0.09)
Layoff: Improve efficiency			-0.01 (0.04)
Layoff: Highly paid			-0.05 (0.05)
Layoff: Low-performing			0.10* (0.06)
Layoff: Other			0.09 (0.06)
<i>N</i>	751	748	748
Mean Dep. Var.	.54	.54	.54
Adj.R2	0.017	0.023	0.072
Additional controls	No	Yes	Yes

Note: The dependent variable is the indicator for responding “Do not know” to the question about a hypothetical reduction in the total pay cost that could have prevented layoffs. Additional controls are included. The mean of the dependent variable is 54 percent. Sample of firms that implemented any layoffs.

Table A.12: What Reduction in the Total Pay Cost Could Have Prevented Layoffs?

	(1)	(2)	(3)
Productivity	-0.04 (0.03)	-0.04 (0.04)	-0.03 (0.03)
Average wages	-0.01 (0.04)	-0.01 (0.04)	-0.00 (0.04)
Revenue growth rate in 2020 (%)	-0.03 (0.02)	-0.03 (0.02)	-0.01 (0.02)
Routine task index	0.02 (0.02)	0.03 (0.02)	0.02 (0.02)
Unionization (%)	-0.02 (0.03)	-0.01 (0.03)	-0.01 (0.03)
Worker representative	-0.06 (0.05)	-0.03 (0.06)	-0.04 (0.05)
Tightness	-0.06** (0.03)	-0.06** (0.03)	-0.05* (0.03)
Layoff: Reduced Sales			0.31*** (0.05)
Layoff: Improve efficiency			0.00 (0.06)
Layoff: Highly paid			0.05 (0.07)
Layoff: Low-performing			0.01 (0.08)
<i>N</i>	382	382	382
Mean Dep. Var.	.54	.54	.54
Adj.R2	0.022	0.025	0.104
Additional controls	No	Yes	Yes

Note: The dependent variable is the indicator for the hypothetical reduction in the total pay cost of 21-100%, conditional on the firms that reported a reduction. Additional controls are included.

# A The Survey Questionnaire

This section reports the first 35 questions of the questionnaire. While some phrases can seem uncommon in English, they are perfectly understandable in Danish. Key phrases and Danish words are reported in parenthesis in Danish for Danish speakers.

## A.0.1 Background Questions

1. What is your role in the company?

- Owner manager
  - Director without ownership
  - Board member without ownership
  - Owner without being a board member
  - Other
- 

2. Does a person or family have 50% or more of the ownership?

- Yes
- No
- Do not know

3. Do you consider the company to be a family business?

- Yes
- No
- Do not know

4. How many employees were there in the company on May 1, 2021? Note: Include all employees, including full-time, part-time, furloughed and employees on apprenticeship and parental leave. Give your best estimate.

- \_\_\_\_\_

5. How much did revenue (*omsætningen*) change in 2020 compared to 2019? Note: If you do not know the exact change, give your best estimate.

- Decreased by 100 percent
- Decreased (indicate the percentage): \_\_\_\_\_
- Unchanged
- Increased (indicate the percentage): \_\_\_\_\_
- Increased by 100 percent or more

6. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)”.  
The revenue decreased because...

- Declining demand for goods and services
- The administrative challenges due to COVID made it difficult to work
- Challenges of buying supplies for the company
- Challenges of obtaining external funding

- Challenges of buying and selling across borders
  - Other reasons
7. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)”.  
How long do you expect it to take before revenue reaches its 2019 precrisis level?
- Our revenue has already passed the precrisis level
  - We are at the same level as before the crisis
  - Less than 3 months from today
  - 3-6 months from today
  - 6-12 months from today
  - 12-24 months from today
  - Other/Do not know
8. Compared to 2019, investments in 2021 will be ...
- Reduced (indicate percentage reduction): \_\_\_\_\_
  - Unchanged
  - Increased (indicate percentage increase): \_\_\_\_\_
9. In the last 12 months we have been negotiating with the bank about bank credit.
- Yes, and the negotiations have been normal
  - Yes, and the negotiations have been more accommodating than normal
  - Yes, and the negotiations have been more restrictive than normal
  - No, because we do not use bank credits
  - No, we did not need further bank credits
  - No, due to 'other'.
10. Is the company primarily a subcontractor (*underleverandør*) to other companies?
- Yes, for 90 percent or more of the revenue
  - Yes, for 50 percent to 89 percent of the revenue
  - Yes, for 25 percent to 49 percent of the revenue
  - Yes, for 10 percent to 24 percent of the revenue
  - Yes, for less than 10 percent of the revenue
  - No
  - Do not know
11. In the following questions, we ask about wage (*løn*)<sup>52</sup> and employment practices (*ansættelsespraksis*) in the firm. How close are you to such decisions?
- I am responsible for wage and employment conditions
  - I am not responsible, but I know about wage and employment conditions
  - I only know a little about wage and employment conditions

---

<sup>52</sup>In Danish, the word *løn* is usually translated as salary, pay or wages. The definition in the dictionary ordnet.dk is “payment that an employee receives for working”.

## A.0.2 The Adjustment of Worker Pay and the Number of Employees in 2020

12. Did your company use the following practices in 2020? Check as many as apply.

- Wage reduction (*lønnedgang*)
- Fewer/lower bonuses
- Fewer/lower fringe benefits
- Fewer promotions
- None of the above

13. If the answer to the Question 12 is different from “None of the above”. Respondents were asked to indicate the percentage of and the percentage of employees affected for each practice that they indicated to have used:

- The percentage of the [wage reduction;fewer/lower bonuses;fewer/lower fringe benefits;fewer promotions]: \_\_\_\_\_
- The percentage of the employees affected by [wage reduction;fewer/lower bonuses;fewer/lower fringe benefits;fewer promotions]: \_\_\_\_\_

14. Did your company use the following practices in 2020? Check as many as apply.

- Freezing or reducing new hires - for existing jobs
- Freezing or reducing new hires - for new jobs
- Permanent layoffs
- Temporary layoffs (expects reemployment)
- Furloughs with support from the government COVID-19 aid packages
- Negotiated separations via pensions or early retirement plans (*efterløn*)
- Reduction in hours without the use of government aid packages
- None of the above

15. If the answer to the Question 14 is different from “None of the above”. Indicate to what extent (in number of affected employees) the following practices were used:

- Freezing or reducing new hires - for existing jobs: \_\_\_\_\_
- Freezing or reducing new hires - for new jobs: \_\_\_\_\_
- Permanent layoffs: \_\_\_\_\_
- Temporary layoffs (expects reemployment): \_\_\_\_\_
- Furloughs with support from the government COVID-19 aid packages: \_\_\_\_\_
- Negotiated separations via pensions or early retirement plans: \_\_\_\_\_
- Reduction in hours without the use of government aid packages: \_\_\_\_\_

### A.0.3 Perceptions, Attitudes and Reasoning Regarding Layoffs

16. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)”. What were the main reasons for retaining employees despite a reduction in sales and other cost pressures? Note: Even if you have laid off some employees, consider why you have not laid off more. Please express your opinion on the following statements. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

Meget enig	Enig	Hverken eller	Uenig	Meget uenig
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

- We want to keep current employees to avoid loss of skills and knowledge
  - We may not be able to find and hire workers again quickly when needed during the recovery
  - Employees work in teams and we cannot lay off some of them
  - Layoffs will be detrimental to morale among the remaining employees
  - We can use government aid packages
  - Instead of layoffs, we can reduce pay
  - Laying off will be detrimental for the company’s reputation
17. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)”. Do you agree with the following? Note: Even if you have laid off some employees, consider why you have not laid off more. Please express your opinion on the following statements. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

- Management has less focus on efficiency and cost reductions during good times and therefore the company reorganizes itself during bad times (*dårlige tider*)
- It is more acceptable to lay off the less-good employees (*mindre gode*) during bad times
- It is more acceptable to lay off employees who are highly paid relative to their productivity during bad times
- It is easier to ask employees to change their tasks or to increase their work effort in bad times, as employees are less likely to quit

18. What were the main reasons for the firm’s layoffs in 2020? Check as many as apply.

- Our company did not experience layoffs in 2020
- Reduced sales and financial difficulties
- Reorganization due to technological changes
- Reorganization to improve efficiency (eliminate unnecessary labor)
- Laying off employees who were highly paid relative to their productivity
- Laying off low-performing employees (for example, employees with outdated skills and knowledge)

- Other. Please provide details
- 

19. If the answer to Question 18 is not “Our company did not experience layoffs in 2020”- How have layoffs affected the remaining employees of the company? Please state your opinion on the following statement. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

- Employees have a higher workload as there are fewer
- Employees work harder and make a greater effort to avoid being laid off
- Layoffs hurt morale and work ethics among the remaining employees
- There is no effect on the remaining employees

20. If the answer to Question 18 is not “Our company did not experience layoffs in 2020”. How many of these layoffs would have taken place in 2020 or over the next two years if there had not been a pandemic? If you are uncertain, give your best estimate. Please choose between 0% (no one would have been laid off) and 100% (everyone would have been laid off). See the screenshot below.

- 0%, ingen ville være blevet fyret
- 10%
- 20%
- 30%
- 40%
- 50%
- 60%
- 70%
- 80%
- 90%
- 100%, alle ville være blevet fyret

#### A.0.4 Institutional Setting

21. Are you using / have used in 2020 you used at least one of the government’s aid packages mentioned below? The government’s aid packages include: Furlough scheme, work distribution scheme, compensation for fixed costs.

- Yes
- No
- Do not know

22. If the answer to Question 21 is “Yes”. Do you think the company would have survived the pandemic crisis without government aid packages? Even if you are not sure, provide your best estimate.

- No, we would not have survived
- Yes, we would have survived. Enter the probability in percent (1 = least likely, 100 = we would have survived with certainty).

23. Which of the following forms of employee representation currently exist in the firm? List as many as apply.

- Trade union representative for the entire company without professional divisions, TR (*Tillisrepræsentant*)
- Trade union representatives divided into professional groups and with an overall joint shop steward
- Employee representative at the board-level (*Medarbejderrepræsentanter*)
- Cooperation Committee, SU (*Samarbejdsudvalg*)
- None of the above

24. If the answer to Question 23 is “Trade union representative [...], TR”. For the year 2020, what is your position on the following statements? Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

- Union representative (s) help reduce the number of redundancies by finding alternative solutions to reduce wage costs (reorganization, wage reduction, etc.)
- Union representative (s) help implement firing strategies by identifying low / high performing employees or setting criteria for who can be fired.

25. If the answer to Question 23 is “Trade union representative [...], TR”. Other, please write

26. If the answer to Question 23 is “Employee representative at the board-level”. For the year 2020, what is your position on the following statements? Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

- Employees on the board help to reduce the number of redundancies by finding alternative solutions to reduce wage costs (reorganization, wage reduction, etc.)
- Employees on the board help to implement layoff strategies by identifying low / high performance or setting criteria for who can be laid off
- Has no employees on the board
- Other, please write.

27. Open-ended question put to all respondents. In general, what are the main considerations that come to mind when thinking about reducing the number of employees? Please use the text box below and write as much as you like. Your opinion and thoughts are very important to us. There is no right or wrong answer. If you do not want to share your views, then skip this question.

### **A.0.5 Perceptions, Attitudes and Reasoning Regarding Adjustment of Worker Pay**

28. Do you think that this company offers lower or higher salaries than competing companies in your industry? Competing companies are other employers that hire people with the same skills in your region. If you are not sure, please come up with an estimate. Respondents have five options (much lower, lower, about the same, higher, much higher).

29. If the answer to Question 28 is “higher” or “much higher”. Why do you offer higher salaries than others in your industry? Please state your position on the following statement.

- We want to compensate for negative aspects of the job (job insecurity, working conditions, etc.)

- We want to attract the best candidates
  - We want to hire quickly
  - We want to ensure stable employees who do not change jobs often (avoid employees switching to competitors)
  - We want to increase employee morale
  - We want to reduce the need to control and monitor employees
  - We want to share the high earnings we generate with the employees.
30. If the answer to Question 28 is “lower” or “much lower”. Why do you offer lower salaries than others in your industry? Please state your position on the following statement.
- We cannot pay higher wages (low demand for our products / service or high level of competition)
  - We do not need to pay high wages as there are few competing employers
  - We do not have to pay too high wages as we can offer a lot of valuable facilities that compensate for higher wages (job security, work environment etc)
  - We need to keep wages low in order to invest the earnings we generate in other strategic priorities (e.g. research and development, marketing).
31. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)” AND the answer to Question 12 is not “wage reduction”. What are the main reasons for not lowering the contractual base pay (*basisløn*)? Please state your position on the following statements. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).
- It would be illegal or almost impossible to change the base pay and contractual supplements
  - The company thinks of the base pay as a commitment to its employees
  - Pay reduction can damage productivity because employees do not work as hard
  - Pay reduction would lead employees to quit
  - Pay reduction damages morale and is demotivating for employees in general
  - Unions/employee representatives are against pay reductions
  - Pay reduction would not save jobs
32. If the answer to Question 5 is “Decreased by 100 percent” OR “Decreased (indicate the percentage)” AND the answer to Question 12 is not “Fewer/lower bonuses”. What are the main reasons for not lowering noncontractual supplements and / or bonuses? Please state your position on the following statement. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).
- The company thinks of bonuses as a commitment to its employees
  - Bonus reduction can damage productivity because employees do not work as hard
  - Bonus reduction would lead employees to quit
  - Bonus reduction damages morale and is demotivating for employees in general
  - Unions / employee representatives are against bonus reductions
  - Bonus reduction would not save jobs
33. If the answer to Question 18 is not “Our company did not experience layoffs in 2020”. What reduction in the total salary cost (salary plus supplements and bonuses) could have prevented layoffs? The options are listed in the screenshot.

- 0-20%
- 21-40%
- 41-60%
- 61-80%
- >80%
- Ved ikke

34. If the answer to Question 18 is not “Our company did not experience layoffs in 2020”. Why not lowering wage instead of laying off employees? Please express your opinions on the following statements. Respondents have five options (strongly agree, agree, neutral, disagree, and strongly disagree).

- Wage reduction would not have saved jobs
- Wage reduction would hurt morale and productivity more than layoffs
- Layoffs give better control over who leaves the company
- Layoffs save more money than wage reduction

35. Open-ended question put to all respondents. Do you think that the company’s wage policy will help your business strategy in bad times? Please use the text box below and write as much as you like. Your opinion and thoughts are very important to us! There is no right and wrong answer. If you do not want to share your views, then skip this.

## B Unemployment Insurance and Firm Separation Incentives in Denmark

This appendix provides additional background on Denmark’s unemployment insurance (UI) system and its implications for firms’ incentives when comparing layoffs to alternative cost-saving margins.

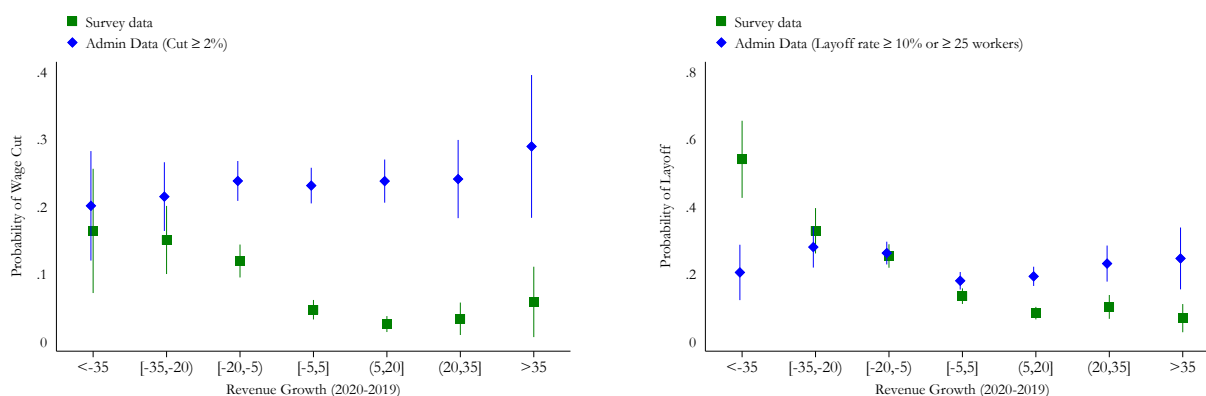
*Eligibility and the role of separation type.* UI coverage in Denmark is voluntary and organized through unemployment insurance funds (A-kasser). Eligibility for UI benefits requires that the worker satisfies membership and prior-income (or employment-history) requirements. Eligibility also depends on the nature of the separation: voluntary quits typically trigger a waiting/quarantine period before benefit receipt, whereas layoffs (redundancies) generally allow benefit receipt without such a waiting period. These rules imply that separation type matters for workers’ short-run benefit access and therefore can affect workers’ preferences over a layoff versus a quit.

*Financing and employer-side incentives.* A key distinction from the U.S. system is that Denmark does not finance UI through experience-rated employer payroll taxes that increase with the firm’s layoff history. Instead, UI benefits are largely financed through general taxation and worker-paid contributions, and employer UI taxes do not rise with the firm’s subsequent insured unemployment duration. There is, however, a limited employer-paid component known as “G-days,” under which the employer pays unemployment benefits for the first days of unemployment for eligible workers. Importantly, this employer-paid component is (i) small in scope, and (ii) targeted at entry into unemployment rather than the duration of the unemployment spell. As a result, Denmark does not generate U.S.-style dynamic layoff-tax incentives that mechanically encourage firms to avoid separations in favor of wage cuts.

*Implications for interpreting layoffs vs. pay cuts.* Because employer-side UI financing incentives are limited relative to experience-rated systems, our empirical results on firms’ preference for layoffs over wage cuts are unlikely to be driven by strategic avoidance of UI taxes. Instead, the institutional setting suggests that layoffs in Denmark primarily reflect employment protection rules, collective bargaining institutions, and firm-level labor-cost and productivity considerations, consistent with the survey evidence on selection and reorganization motives.

## C What the Survey Measures and What the Registers Measure

The survey and administrative registers provide related but conceptually distinct measures of labor-cost adjustment. Even for margins where comparison is possible—permanent layoffs and base-wage changes among job stayers—the two sources capture different objects: the survey records whether the firm deliberately used a given adjustment tool as a firm-level policy, while the registers record realized outcomes—worker separations and wage changes—shaped by compositional turnover, idiosyncratic renegotiations, and mechanical adjustments alongside any deliberate policy. For margins such as deliberate hiring reductions, policy coverage, motives, and constraints, the survey is the only source. We document both dimensions below.



(a) Probability of Wage Cuts by Revenue Growth Bin.

(b) Probability of Layoffs by Revenue Growth Bin.

*Notes:* Estimated with standard set of controls. Wage data from LONN.

Figure C.1: Survey vs Administrative Data: Wage Cuts and Layoffs by Revenue Growth Bin

Figure C.1 compares survey and administrative indicators of wage cuts (panel a) and permanent layoffs (panel b) across revenue-growth bins in the linked sample. The two measures co-move: firms with larger revenue declines are more likely to show adjustment in both sources. But the levels diverge substantially for wage cuts, where the administrative measure flags a much higher share of firms than the survey. This gap is expected: the administrative indicator captures all firms in which some job stayers experienced base-wage declines—regardless of whether the firm implemented a deliberate pay-cut policy—while the survey captures only firms that report deliberately reducing base wages as a firm-level decision.

Table C.1 quantifies this divergence. The administrative data classify 51.5% of firms as having wage cuts (based on stayer base-wage changes), while only 18.1% report a deliberate base-wage reduction in the survey—a nearly three-to-one ratio. Overall agreement is 55.3%. The asymmetry is informative: when firms report cutting wages in the survey, administrative data confirm that stayer wages did decline for 68.7% of those firms—deliberate pay-cut policies are real and show up in the registers. But when the administrative data flag a wage

Table C.1: Survey vs Administrative Wage Cut: Confusion Matrix

Survey wage cut ( $S_i^W$ )	Administrative wage cut ( $A_i^W$ )		
	No (0)	Yes (1)	Total
<i>No (0)</i>	1,286.87 52.33% 88.31%	1,172.46 47.67% 75.85%	2,459.33 100.00% 81.90%
<i>Yes (1)</i>	170.34 31.33% 11.69%	373.32 68.67% 24.15%	543.66 100.00% 18.10%
<i>Total</i>	1,457.22 48.53% 100.00%	1,545.78 51.47% 100.00%	3,003 100.00% 100.00%
<i>Agreement and classification rates</i>			
Overall agreement (diagonal share)	55.3%		
<i>Survey as reference:</i>			
Sensitivity: $\Pr(A^W=1 \mid S^W=1)$	68.7%		
Specificity: $\Pr(A^W=0 \mid S^W=0)$	52.3%		
<i>Administrative data as reference:</i>			
Sensitivity: $\Pr(S^W=1 \mid A^W=1)$	24.2%		
Specificity: $\Pr(S^W=0 \mid A^W=0)$	88.3%		
$N$	3,003		
Total entropy-balancing weight	29,245.86		

Notes: Cell entries: weighted frequencies (top), row % (middle), column % (bottom).

decline, only 24.2% of those firms report a deliberate wage cut. The vast majority of administrative “wage cuts” are not the result of deliberate firm policy—they reflect compositional changes, idiosyncratic renegotiations, or mechanical adjustments that were never intended as firm-level cost management.

Administrative data alone would therefore vastly overstate the incidence of deliberate wage cutting—recording as “wage cuts” a large number of incidental wage declines that were never intended as firm-level adjustment policies. The survey captures a distinct and policy-relevant object: the firm’s deliberate decision to reduce wages as a cost-management tool. This is the object the paper’s analysis is built around—firms’ choices, motives, and constraints—and it cannot be recovered from realized wage-change distributions alone.

**Policy coverage.** The distinction between survey and administrative measures extends beyond the extensive margin to pay-adjustment coverage. Figure 3 is not intended to replicate an administrative wage-change distribution. Instead, it measures policy coverage: conditional on reporting a base-wage reduction (panel (a)) or a bonus reduction (panel (b)), the firm reports the share of employees covered by the pay-cut policy at the time it was implemented (Survey Question 13). This object is central for our interpretation because it speaks to whether pay cuts are used as a broad, firm-level adjustment instrument (a multilateral policy) rather than a narrowly targeted tool.

Administrative wage records are indispensable for measuring realized pay changes, and Appendix J uses registers to quantify the incidence of base-wage cuts among job stayers. However, administrative wage changes alone do not, on their own, identify the intended scope of a firm-wide pay policy—and they do not yield a simple, comparable measure of the breadth of bonus/supplement adjustments across firms—without additional assumptions. The survey and the registers therefore speak to related but distinct concepts:

1. *Policy coverage vs. realized pay changes.* A broad pay policy need not translate one-for-one into observed base-wage cuts in the registers. Firms may implement freezes (foregone raises), adjust the timing of wage updates, or shift pay components that are not captured in the base-wage field. Conversely, observed base-wage declines can reflect idiosyncratic renegotiations that were not part of a firm-wide policy.
2. *Workforce definition and constant-composition measurement.* Survey Q13 refers to the workforce the respondent has in mind when the policy is implemented (which may include workers who later separate, workers with changing hours, or groups not observed as job stayers). Administrative “breadth” is most naturally measured among job stayers to hold composition fixed. Even with accurate reporting, the policy-coverage concept and the stayer-based concept need not coincide mechanically.
3. *Component coverage.* Figure 3 distinguishes (i) reductions in the contractual base wage (including fixed contractual supplements) from (ii) reductions in discretionary/non-contractual supplements and bonuses. Registers provide a standardized base-wage measure for job stayers, but bonus and irregular-pay adjustments are harder to summarize as a single firm-level “coverage” statistic that is comparable across firms and pay systems.

In sum, the survey and registers are complementary. The registers provide objective measures of realized wage changes and worker flows; the survey identifies which of those changes reflect deliberate firm policy, how broadly that policy was applied, and the motives and constraints that shaped the decision.

## D Survey-Administrative Agreement by Respondent Knowledge

The paper restricts the analysis sample to respondents who report being close to pay and employment decisions at the firm (Section 2.1). This section validates that screen by comparing survey-administrative agreement between knowledgeable respondents ( $N = 3,003$ ) and the small group of low-knowledge respondents ( $N = 106$ ) who are excluded from the main analysis.

Table D.1 reports within-group agreement rates. Knowledgeable respondents agree with administrative records at a rate of 54.9% for layoffs and 9.8% for wage reductions, compared with 52.8% and 7.6% for low-knowledge respondents. Both differences go in the expected direction, low-knowledge respondents are less accurate, though the magnitudes are modest.

Table D.2 tests this more formally. We regress a survey-administrative mismatch indicator on the low-knowledge dummy, controlling for revenue-growth bins, firm size, and industry fixed effects. Column (1) reports the layoff mismatch; columns (2)-(4) report wage-cut mismatch at three thresholds for classifying an administrative wage reduction (share of stayers with a non-increase  $\geq 5\%$ ,  $10\%$ , or  $20\%$ ). The low-knowledge coefficient is positive in all specifications (0.2-1.0 percentage points) but small and statistically insignificant, consistent with limited power from only 106 low-knowledge respondents. Revenue-growth bins, by contrast, are strongly associated with wage-cut mismatch: firms in negative-revenue bins have significantly lower mismatch rates, reflecting the mechanical increase in the base rate of wage adjustments under distress.

Overall, these results provide directional support for the conjecture that low-knowledge respondents are less accurate, while confirming that the knowledge screen excludes a small group whose removal does not materially affect the paper’s conclusions.

Table D.1: Survey-Administrative Agreement Rates by Respondent Knowledge

	(1)	(2)
	Knowledgeable	Low Knowledge
Agreement rate—Layoff	0.549	0.528
Agreement rate—Wage cut	0.098	0.076
$N$	3,003	106

Notes: Agreement is defined as the share of firms for which the survey indicator and the administrative indicator coincide. Knowledgeable respondents are those who report being close to pay and employment decisions; low-knowledge respondents answered “I only know a little” or “Do not know.” The  $2 \times 2$  confusion matrices cannot be reported for the low-knowledge group due to minimum cell-size requirements.

Table D.2: Mismatch Regressions: Effect of Low Respondent Knowledge

	(1)	(2)	(3)	(4)
	Mismatch layoff	Mismatch wage cut (5%)	Mismatch wage cut (10%)	Mismatch wage cut (20%)
Low Knowledge	0.007 (0.054)	0.002 (0.029)	0.010 (0.029)	0.003 (0.032)
< -35%	0.051 (0.062)	-0.149*** (0.041)	-0.168*** (0.046)	-0.165*** (0.048)
[-35, -20)	0.076* (0.043)	-0.116*** (0.029)	-0.121*** (0.031)	-0.123*** (0.033)
[-20, -5)	0.005 (0.029)	-0.073*** (0.017)	-0.076*** (0.018)	-0.081*** (0.020)
[-5, 5]	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
(5, 20]	0.013 (0.029)	0.018 (0.013)	0.028** (0.014)	0.024 (0.016)
(20, 35]	0.074* (0.043)	0.005 (0.020)	0.019 (0.023)	0.021 (0.025)
> 35%	0.036 (0.065)	-0.033 (0.035)	-0.020 (0.040)	-0.029 (0.044)
Observations	2,837	2,837	2,837	2,837
R-squared	0.062	0.082	0.073	0.066
Firm Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Notes: Each column reports OLS estimates of a mismatch indicator (= 1 if survey and administrative indicators disagree) on a low-knowledge dummy and controls. Columns (2)-(4) vary the threshold for classifying an administrative wage reduction (share of stayers with a non-increase  $\geq$  5%, 10%, or 20%). Revenue-growth bin  $[-5, 5]$  is the omitted category. Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## E Administrative Employment and Earnings Changes in Respondent vs. Non-Respondent Firms

To assess external validity, we compare realized 2019-2020 changes in employment, labor costs, and earnings per worker between survey respondents and non-respondents within the survey sampling frame (private and public limited companies with at least 5 employees in 2019): 3,012 respondents and 26,283 non-respondents (Appendix Table E.2).

We define three firm-level outcomes:  $\Delta \log L_i$  (log employment change),  $\Delta \log LC_i$  (log labor-cost change), and  $\Delta \log(LC/L)_i$  (log earnings-per-worker change), all measured between 2019 and 2020. Using the same revenue-growth bins as in the paper, we estimate:

$$y_i = \alpha + \sum_{b \neq b_0} \gamma_b \mathbf{1}\{\text{RevBin}_i = b\} + \beta \text{Resp}_i + \sum_{b \neq b_0} \delta_b (\text{Resp}_i \times \mathbf{1}\{\text{RevBin}_i = b\}) + X_i' \kappa + \mu_{s(i)} + \varepsilon_i, \quad (\text{E.1})$$

where  $\text{Resp}_i = 1$  for survey respondents,  $b_0 = [-5, 5]$  is the omitted revenue bin,  $\mu_{s(i)}$  are industry fixed effects, and  $X_i$  includes log employment and firm age in 2019. We estimate (E.1) both with entropy-balancing weights applied to respondents (odd columns) and unweighted (even columns).

Appendix Table E.1 reports the results. The respondent main effect in the baseline bin is near zero and insignificant for labor costs and earnings per worker. In the moderate revenue-change bins ( $[-20\%, +20\%]$ ), which contain the majority of firms, the respondent  $\times$  bin interactions are economically small and statistically insignificant across all three outcomes. The joint  $F$ -test fails to reject equality for earnings per worker. Employment and labor costs reject, but entirely because respondents in the severely distressed tail ( $< -35\%$ ) contract *less* than non-respondents, a direction that is conservative for our thesis. In the core of the revenue-shock distribution where the paper’s main analysis operates, respondent and non-respondent firms are indistinguishable on all three margins. If anything, the full population of non-respondent firms displays a larger wedge between pay and employment adjustment than our sample—reinforcing the non-substitutability result. Results are nearly identical with and without entropy-balancing weights. Results are also robust to using the broader administrative population of all firms with at least 5 employees (54,857 firms, without the company-type restriction); in that specification, the  $> 35\%$  revenue-growth interaction for labor costs is large and significant ( $-0.243$ ) but attenuates to  $-0.041$  under the correct sampling frame, confirming it was an artifact of including firms outside the survey eligibility criteria.

Table E.1: Employment and Earnings Adjustment: Survey Respondents vs. Non-Respondents

	Employment		Labour Cost		Earnings/Worker	
	(1) Weighted	(2) Unw.	(3) Weighted	(4) Unw.	(5) Weighted	(6) Unw.
Respondent	0.008 (0.008)	0.009 (0.008)	0.008 (0.009)	0.008 (0.009)	-0.001 (0.010)	-0.003 (0.010)
<-35%	-0.389*** (0.014)	-0.397*** (0.014)	-0.420*** (0.015)	-0.427*** (0.015)	0.046*** (0.014)	0.048*** (0.014)
[-35, -20)	-0.146*** (0.009)	-0.151*** (0.008)	-0.154*** (0.008)	-0.159*** (0.007)	-0.002 (0.009)	-0.003 (0.009)
[-20, -5)	-0.055*** (0.005)	-0.057*** (0.005)	-0.069*** (0.005)	-0.072*** (0.005)	-0.013** (0.006)	-0.013** (0.006)
[-5, 5)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
(5, 20]	0.047*** (0.005)	0.047*** (0.005)	0.045*** (0.005)	0.046*** (0.005)	-0.001 (0.006)	-0.001 (0.006)
(20, 35]	0.090*** (0.006)	0.091*** (0.006)	0.107*** (0.006)	0.108*** (0.006)	0.018** (0.008)	0.017** (0.007)
>35%	0.163*** (0.008)	0.163*** (0.008)	0.252*** (0.010)	0.253*** (0.010)	0.089*** (0.010)	0.089*** (0.010)
<-35% × Respondent	0.150*** (0.032)	0.157*** (0.031)	0.149*** (0.036)	0.150*** (0.036)	-0.076** (0.037)	-0.077** (0.037)
[-35, -20) × Respondent	0.033* (0.019)	0.031* (0.019)	0.043** (0.018)	0.043** (0.019)	0.002 (0.022)	0.006 (0.022)
[-20, -5) × Respondent	0.003 (0.012)	0.004 (0.012)	0.004 (0.012)	0.004 (0.012)	0.000 (0.014)	0.000 (0.014)
(5, 20] × Respondent	-0.012 (0.011)	-0.012 (0.011)	-0.010 (0.011)	-0.012 (0.011)	0.002 (0.014)	0.001 (0.013)
(20, 35] × Respondent	0.014 (0.016)	0.014 (0.015)	-0.007 (0.017)	-0.009 (0.016)	-0.022 (0.019)	-0.022 (0.019)
>35% × Respondent	-0.023 (0.022)	-0.024 (0.022)	-0.041* (0.022)	-0.041* (0.023)	-0.017 (0.026)	-0.016 (0.026)
Log Employment (2019)	-0.015*** (0.002)	-0.017*** (0.002)	-0.000 (0.002)	0.003 (0.002)	0.013*** (0.003)	0.018*** (0.002)
Firm age (2019)	-0.000 (0.000)	-0.000 (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Observations	28,939	28,939	29,213	29,213	28,913	28,913
R-squared	0.184	0.183	0.197	0.195	0.025	0.020
Joint $F$ -stat	5.880	5.880	5.300	5.340	1.130	1.070
Joint $p$ -value	$2.1 \times 10^{-5}$	$3.9 \times 10^{-6}$	$1.6 \times 10^{-5}$	$1.8 \times 10^{-5}$	0.376	0.339

Notes: Each column reports OLS estimates of equation (E.1). Dependent variables are log changes between 2019 and 2020 in employment (columns 1-2), total labor cost (columns 3-4), and earnings per worker (columns 5-6). Odd columns apply entropy-balancing weights to respondents (non-respondents receive weight 1); even columns are unweighted. Revenue bins use administrative revenue growth; omitted bin:  $[-5, 5)$ . All regressions include industry fixed effects. The joint  $F$ -test tests  $\delta_b = 0$  for all respondent  $\times$  bin interactions. Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table E.2: Number of Firms by Revenue-Growth Bin and Survey Response Status

Revenue Growth Bin	Non-Respondent	Respondent	Total
<-35%	2,712	232	2,944
[-35, -20)	2,613	259	2,872
[-20, -5)	5,412	670	6,082
[-5, 5)	5,144	642	5,786
(5, 20]	5,165	647	5,812
(20, 35]	2,351	269	2,620
>35%	2,886	293	3,179
Total	26,283	3,012	29,295

## F Pre-Shock Administrative Characteristics of Surveyed and Non-Surveyed Firms

To assess whether non-response introduces systematic selection, we compare pre-shock (2018) firm characteristics and industry composition between the full administrative population ( $N = 27,619$ ) and the survey sample ( $N = 2,893$ ). Table F.1 reports means and standardized differences for eight firm-level variables. Differences are small for most characteristics: employment (0.03 SD), assets ( $<0.01$  SD), capital per worker ( $<0.01$  SD), and operating return on assets (0.03 SD). The largest gaps are for firm age (0.18-0.21 SD) and the share of high-skilled employees (0.17-0.18 SD); entropy weighting, which targets employment and industry composition, does not alter these two substantively. Table F.2 compares the industry distribution. The Pearson  $\chi^2$  test rejects equality ( $\chi^2(10) = 140.47$ ,  $p < 0.001$ ), as expected given the large population; entropy-weighted sample counts track the population counts closely across all sectors.

Table F.1: Pre-Shock Firm Characteristics: Population vs. Survey Sample (2018)

Variable	Unweighted sample			Entropy-weighted sample		
	Population	Sample	Std. Diff.	Population	Sample	Std. Diff.
Employment	40.40	48.23	0.03	40.40	38.10	-0.01
Firm age	15.82	18.68	0.21	15.82	18.30	0.18
Assets	104.69	106.97	0.00	104.69	87.20	-0.01
Average salary	224.30	236.78	0.04	224.30	237.00	0.04
Capital per worker	3.29	2.67	0.00	3.29	2.80	0.00
Share high-skilled emp.	15.01	18.65	0.17	15.01	18.70	0.18
OROA	0.10	0.10	0.03	0.10	0.10	0.03
Value added per worker	0.76	0.82	0.06	0.76	0.82	0.05
Observations	27,619	2,893				

Note: Pre-shock characteristics measured in 2018 administrative data. Standardized differences are (sample mean - population mean) / population SD. Entropy-balancing weights target firm-size and industry-composition moments in the population.

Table F.2: Industry Distribution: Population vs. Survey Sample

Code	Industry	Population	Sample	Sample (weighted)
B	Mining and quarrying	50	8	8.98
C	Manufacturing	3,799	547	453.69
D	Electricity, gas, steam and air conditioning	58	8	8.65
E	Water supply; sewerage, waste management	123	22	24.23
F	Construction	4,744	419	468.29
G	Wholesale and retail trade	6,864	834	847.38
H	Transportation and storage	1,338	161	159.58
I	Accommodation and food service	2,057	116	117.51
J	Information and communication	1,835	253	275.58
M	Professional, scientific and technical	2,288	339	343.32
N	Administrative and support services	1,570	186	185.77
Total		24,726	2,893	2,893.00
Pearson $\chi^2(10) = 140.47, p < 0.001$				

Note: Sample counts are unweighted; weighted column applies entropy-balancing weights targeting employment and industry composition. The  $\chi^2$  test compares the unweighted sample distribution to the population.

## G Administrative Wage and Employment Adjustment in Pre-Survey Exit Firms

A concern with any firm survey is that establishments that exit before the survey is fielded cannot respond. In our setting, because invitations were sent to firms in the May 2021 Bismode export, firms that shut down before May 2021 are outside our sampling frame. This appendix uses administrative registers to examine how such “exit firms” adjusted labor costs in 2020 prior to shutdown and whether their adjustment patterns differ systematically from those of continuing firms.

Under a simple substitution view in which wage cuts are a primary alternative to layoffs, one might expect that among firms facing the most severe distress and approaching shutdown, wage reductions would be used more aggressively. Under our interpretation, wage cuts are not a generally available substitute margin: the binding constraints—morale, retention, internal pay structures—apply regardless of survival status, so even among distressed firms, base-wage adjustment should be similar to that of continuing firms facing the same shock.

**Exit indicator, sample, and outcomes.** We start from all firms with at least 5 employees in 2019 (the target population) and positive employment in 2020 so that 2020 outcomes are observed. We define

$$\text{ExitByMay2021}_i \equiv \mathbf{1}\{\text{firm active in 2019-2020 but not active by May 2021}\},$$

operationalized using the business-register active-status flag. We measure two firm-level outcomes in 2020: (i) the administrative layoff rate  $A_i^{LR}$  (permanent separations to unemployment divided by 2019 employment); and (ii) the share of job stayers with a base-wage non-increase,  $A_i^{NI} \equiv \frac{1}{N_i^S} \sum_{j \in S_i} \mathbf{1}\{\Delta w_{ij} \leq 0.02\}$ , where stayers are workers employed at firm  $i$  in both 2019 and 2020, and  $\Delta w_{ij}$  is the percent change in the contractual hourly base wage from LONN records. The wage-outcome sample is smaller because it requires LONN coverage.

**Specification.** We compare exit and continuing firms within the same revenue-growth bin by estimating, for each outcome  $y_i \in \{A_i^{NI}, A_i^{LR}\}$ :

$$y_i = \alpha + \sum_{b \neq b_0} \gamma_b \mathbf{1}\{\text{RevBin}_i = b\} + \beta \text{ExitByMay2021}_i + \sum_{b \neq b_0} \delta_b (\text{ExitByMay2021}_i \times \mathbf{1}\{\text{RevBin}_i = b\}) + X_i' \kappa + \mu_{s(i)} + \varepsilon_i, \quad (\text{G.1})$$

where  $b_0 = [-5, 5]\%$ , using the same seven revenue-growth bins as Figure 1;  $X_i$  are firm controls; and  $\mu_{s(i)}$  are industry fixed effects. The interaction coefficients  $\delta_b$  test whether exit firms adjust differently within the same shock bin.

**Results.** Exit firms are substantially more likely to be located in the severe revenue-decline bins, consistent with shutdown occurring under extreme distress. Conditional on revenue bins, the exit  $\times$  shock interactions are insignificant for wage non-increases across all negative-revenue bins (Table G.1). For layoffs, the sole significant interaction in the negative bins is  $< -35\%$  ( $-0.092$ ,  $p < 0.01$ ), indicating that exit firms in the most extreme bin lay off *less*

than continuing firms—a direction that is conservative for our thesis. Exit firms in the most distressed bins do not adjust stayer wages differently from continuing firms facing the same shock.

Table G.2 reports the within-bin contrasts directly. The exit effect on wage non-increases is insignificant in every negative-revenue bin:  $-13.8$  percentage points for  $< -35\%$  (CI:  $[-61.1, 33.4]$ ),  $-12.3$  for  $[-35, -20)\%$  (CI:  $[-32.4, 7.9]$ ), and  $+21.7$  for  $[-20, -5)\%$  (CI:  $[-10.2, 53.7]$ ). For layoff rates, exit firms in the  $< -35\%$  bin have a significantly *lower* rate ( $-7.0$  pp,  $p < 0.001$ ), consistent with near-shutdown firms proceeding directly to closure rather than engaging in pre-shutdown layoffs.

Even among the most distressed firms (those that actually shut down), wage adjustment for stayers is no more aggressive than for continuing firms facing the same shock. If the canonical substitution view were correct, near-shutdown firms should be cutting wages aggressively to try to survive. They do not. The binding constraints on pay adjustment apply regardless of survival status. These results indicate that excluding pre-May-2021 exit firms from the survey sampling frame does not bias the paper’s conclusions about the limited scope for pay cuts to substitute for layoffs in practice.

Table G.1: Firm Exit and Revenue Growth: Non-increase and Layoff Outcomes

	(1) Non-increase	(2) Layoff
Exited by May 2021 = 1	0.114 (0.099)	0.023 (0.028)
< -35%	0.028*** (0.011)	0.021*** (0.003)
[-35, -20)	0.021** (0.009)	0.020*** (0.002)
[-20, -5)	0.017** (0.007)	0.009*** (0.002)
[-5, 5)	0.000 (.)	0.000 (.)
(5, 20]	-0.020*** (0.007)	-0.002 (0.002)
(20, 35]	-0.057*** (0.009)	0.001 (0.002)
> 35%	-0.043*** (0.010)	0.010*** (0.002)
Exited = 1 × < -35%	-0.252 (0.261)	-0.092*** (0.030)
Exited = 1 × [-35, -20)	-0.237* (0.143)	-0.055 (0.037)
Exited = 1 × [-20, -5)	0.103 (0.191)	0.005 (0.054)
Exited = 1 × [-5, 5)	0.000 (.)	0.000 (.)
Exited = 1 × (5, 20]	0.120 (0.100)	-0.005 (0.035)
Exited = 1 × (20, 35]	-0.245** (0.100)	-0.078*** (0.028)
Exited = 1 × > 35%	-0.345** (0.135)	-0.028 (0.034)
Observations	12,079	29,066
$R^2$	0.032	0.036
Firm controls	Yes	Yes
Industry FE	Yes	Yes

Standard errors in parentheses.

Wage data from LONN (hence smaller sample size than layoff outcome).

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table G.2: Contrasts: Effect of Firm Exit by Revenue Bin

	(1) Non-increase	(2) Layoff
<i>Exited by May 2021 = 1 vs. 0:</i>		
< -35%	-0.138 [-0.611, 0.334]	-0.070*** [-0.094, -0.046]
[-35, -20)	-0.123 [-0.324, 0.079]	-0.032 [-0.081, 0.016]
[-20, -5)	0.217 [-0.102, 0.537]	0.027 [-0.064, 0.119]
[-5, 5)	0.114 [-0.081, 0.309]	0.023 [-0.032, 0.077]
(5, 20]	0.234*** [0.212, 0.256]	0.018 [-0.023, 0.059]
(20, 35]	-0.131*** [-0.150, -0.112]	-0.055*** [-0.060, -0.051]
> 35%	-0.231* [-0.411, -0.051]	-0.005 [-0.043, 0.033]
<i>N</i>	12,079	29,066

95% confidence intervals in brackets.

Wage data from LONN (hence smaller sample size than layoff outcome).

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

## H Administrative Benchmarking of Wage and Employment Dynamics: Pre-Pandemic vs. Pandemic

This section benchmarks 2020 administrative outcomes against the pre-pandemic period and compares surveyed firms to comparable non-surveyed firms in the sampling frame. The exercise addresses two external-validity concerns raised by the Associate Editor: (i) whether wage and employment dynamics in 2020 are qualitatively different from the pre-period, and (ii) whether surveyed firms behave similarly to the broader population.

**Group definitions.** We partition firms into two groups: *surveyed* (survey respondents,  $N = 3,012$ ) and *non-surveyed* (non-respondents meeting the same survey eligibility criteria—at least 5 employees in 2019 with valid identifiers—in the administrative sampling frame,  $N = 26,283$ ). For the surveyed group we report both unweighted statistics and statistics using the entropy-balancing weights that target population moments for firm size and sector shares (see Section 2.4).

### H.1 Wage-change distributions

We construct year-over-year log changes in nominal *base hourly pay* and *total hourly pay* among job stayers (workers employed at the same establishment in consecutive years), using the same administrative wage sources and sample restrictions as Appendix Figure J.1. We compare two periods: 2018-2019 (pre-pandemic) and 2019-2020 (pandemic).

Table H.1 reports the mean, 25th percentile, median, and 75th percentile of the wage-change distribution, separately for non-surveyed firms, surveyed firms (unweighted), and surveyed firms (entropy-weighted). Figures H.1 and H.2 show the corresponding kernel density estimates.

Three patterns emerge. First, the 2019-2020 wage-change distribution shifts modestly relative to 2018-2019 for both pay components—the median base-wage change rises from 2.1 to 2.6 log points for surveyed (weighted) firms, and the left tail widens slightly (p25 moves from  $-1.9$  to  $-2.8$ )—but the overall shape is preserved. Second, nominal wage cuts are present in both periods: the 25th percentile is negative in 2018-2019 as well, confirming that downward wage flexibility is not unique to the pandemic. Third, surveyed and non-surveyed firms’ distributions closely overlap, especially after entropy weighting, indicating that sample selection does not drive the patterns.

### H.2 Employment dynamics

We construct firm-level employment flows from administrative worker records for 2016-2020. For each firm  $i$  in year  $t$ , we define:

$$\text{hire\_rate}_{it} = \frac{H_{it}}{E_{i,t-1}}, \quad \text{sep\_rate}_{it} = \frac{S_{it}}{E_{i,t-1}}, \quad \text{net\_g}_{it} = \frac{E_{it} - E_{i,t-1}}{E_{i,t-1}},$$

where  $H_{it}$  denotes hires,  $S_{it}$  denotes separations, and  $E_{it}$  denotes employment. We also report Davis-Haltiwanger-Schuh (DHS) rates, which use the average of  $E_{it}$  and  $E_{i,t-1}$  as the denominator to accommodate firm entry and exit symmetrically.

Table H.2 reports the mean, 25th percentile, median, and 75th percentile of each flow measure by year and group. Figure H.3 plots the time series.

Table H.1: Wage Change Dynamics: Pre-Pandemic vs. Pandemic, Surveyed vs. Non-Surveyed Firms

Period	Non-Surveyed				Surveyed (unweighted)				Surveyed (weighted)			
	Mean	p25	p50	p75	Mean	p25	p50	p75	Mean	p25	p50	p75
<i>Panel A: Base Wage (<math>\Delta \ln w^{base} \times 100</math>)</i>												
2018-2019	3.504	-1.419	2.357	8.116	4.019	-1.922	2.231	8.691	3.625	-1.922	2.109	8.222
2019-2020	4.419	-2.230	2.707	9.878	4.144	-2.821	2.602	9.809	4.266	-2.821	2.602	9.809
<i>Panel B: Total Wage (<math>\Delta \ln w^{tot} \times 100</math>)</i>												
2018-2019	5.579	-1.527	3.954	12.140	6.132	-1.118	4.172	12.107	5.727	-1.118	3.940	11.630
2019-2020	6.686	-1.611	4.619	13.752	6.738	-1.295	4.742	13.564	6.806	-1.295	4.742	13.564

*Notes:* Year-over-year log wage changes ( $\times 100$ ) among job stayers (workers employed at the same establishment in consecutive years). “Non-surveyed” refers to firms in the administrative population meeting survey eligibility criteria (at least 5 employees in 2019) but not in the survey sample. “Surveyed (weighted)” applies the entropy-balancing weights targeting population moments for firm size and sector shares.

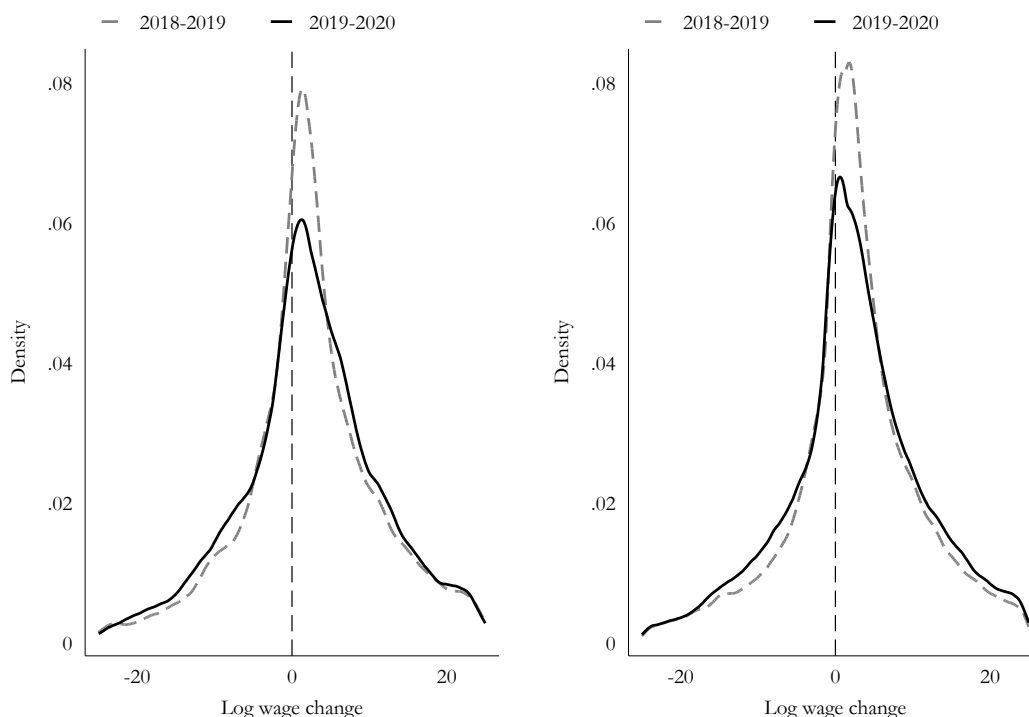


Figure H.1: Kernel Density of Base Wage Changes: Surveyed Firms vs. Full Sample

Note: Kernel density estimates of year-over-year log base wage changes ( $\times 100$ ) among job stayers. Left panel: non-surveyed firms (full population). Right panel: surveyed firms. Gray dashed line: 2018-2019. Black solid line: 2019-2020. Vertical dashed line marks zero.

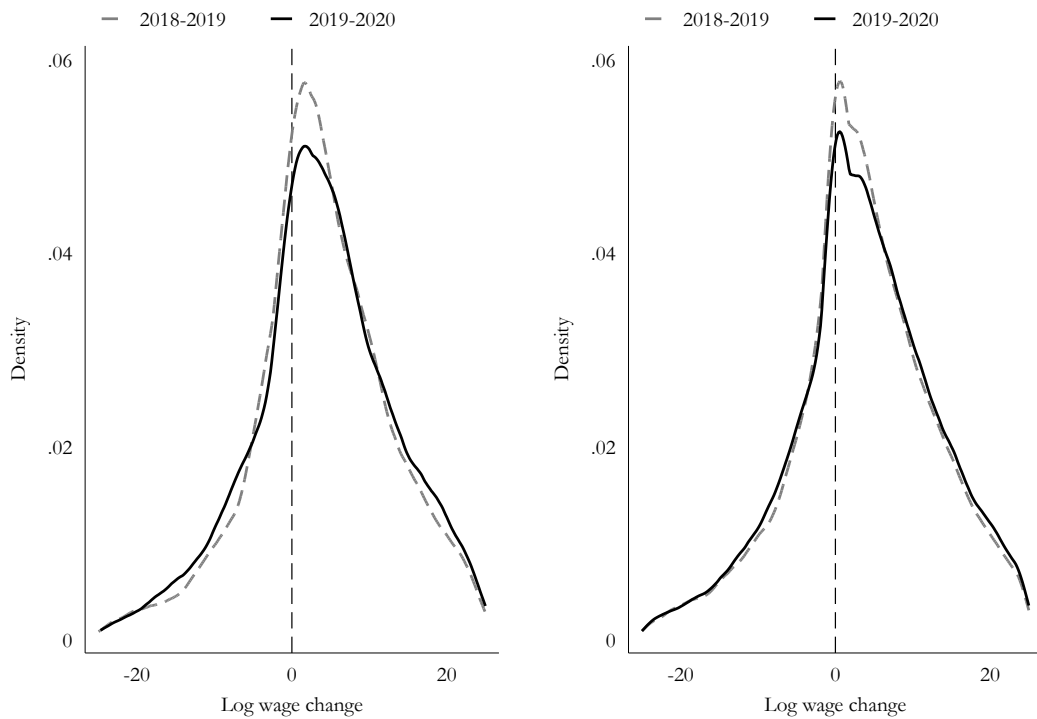


Figure H.2: Kernel Density of Total Wage Changes: Surveyed Firms vs. Full Sample

Note: Kernel density estimates of year-over-year log total wage changes ( $\times 100$ ) among job stayers. Left panel: non-surveyed firms (full population). Right panel: surveyed firms. Gray dashed line: 2018-2019. Black solid line: 2019-2020. Vertical dashed line marks zero.

Surveyed and non-surveyed firms exhibit parallel trends throughout 2016-2019 on all flow measures. The 2020 adjustment is moderate: hiring rates decline (from 0.41 to 0.36 for surveyed weighted firms), while separation rates are stable or slightly declining (from 0.35 to 0.33). Net employment growth turns mildly negative ( $-0.011$ ) but remains far from a collapse. The stability of separation rates in 2020 is consistent with government furlough schemes preserving existing employment matches. Importantly, the parallel trends between surveyed and non-surveyed firms indicate that cross-sectional survey findings cannot be attributed to pre-existing differences between the two groups.

Table H.2: Employment Dynamics: Surveyed vs. Non-Surveyed Firms, 2016-2020

Year	Non-Surveyed				Surveyed (unweighted)				Surveyed (weighted)			
	Mean	p25	p50	p75	Mean	p25	p50	p75	Mean	p25	p50	p75
<i>Panel A: Hiring Rate</i>												
2016	0.4666	0.2500	0.3990	0.5837	0.4295	0.2286	0.3579	0.5211	0.4314	0.2286	0.3621	0.5211
2017	0.4764	0.2595	0.4030	0.5959	0.4308	0.2356	0.3636	0.5387	0.4328	0.2356	0.3684	0.5387
2018	0.4804	0.2640	0.4034	0.5979	0.4230	0.2500	0.3636	0.5384	0.4253	0.2500	0.3663	0.5384
2019	0.4641	0.2500	0.4024	0.5801	0.4081	0.2222	0.3504	0.5000	0.4108	0.2237	0.3504	0.5000
2020	0.4022	0.2210	0.3843	0.5152	0.3533	0.1989	0.3062	0.4550	0.3565	0.1989	0.3062	0.4550
<i>Panel B: Hiring Rate (DHS)</i>												
2016	0.4216	0.2222	0.3502	0.5530	0.3822	0.2344	0.3524	0.4997	0.3844	0.2344	0.3548	0.4997
2017	0.4286	0.2296	0.3708	0.5612	0.3900	0.2393	0.3589	0.5079	0.3920	0.2393	0.3618	0.5079
2018	0.4340	0.2288	0.3718	0.5698	0.3924	0.2500	0.3616	0.5098	0.3943	0.2500	0.3616	0.5098
2019	0.4217	0.2222	0.3737	0.5565	0.3774	0.2290	0.3517	0.4866	0.3795	0.2290	0.3517	0.4866
2020	0.3828	0.2222	0.3735	0.5127	0.3388	0.2053	0.3112	0.4496	0.3414	0.2053	0.3158	0.4496
<i>Panel C: Separation Rate</i>												
2016	0.3988	0.2222	0.3500	0.5000	0.3611	0.2000	0.3112	0.4545	0.3629	0.2000	0.3143	0.4566
2017	0.4030	0.2290	0.3608	0.5113	0.3608	0.2067	0.3152	0.4614	0.3628	0.2067	0.3182	0.4614
2018	0.4034	0.2222	0.3531	0.5142	0.3532	0.2000	0.3175	0.4598	0.3551	0.2000	0.3175	0.4598
2019	0.4024	0.2222	0.3458	0.5133	0.3458	0.2000	0.3038	0.4437	0.3480	0.2000	0.3077	0.4437
2020	0.3843	0.2211	0.3317	0.5000	0.3317	0.1990	0.2941	0.4357	0.3340	0.1990	0.2981	0.4381
<i>Panel D: Separation Rate (DHS)</i>												
2016	0.3672	0.2222	0.3301	0.4879	0.3300	0.1997	0.3068	0.4409	0.3322	0.1997	0.3068	0.4409
2017	0.3708	0.2296	0.3344	0.4978	0.3344	0.2097	0.3077	0.4421	0.3363	0.2097	0.3111	0.4421
2018	0.3718	0.2048	0.3323	0.4984	0.3324	0.2048	0.3077	0.4427	0.3340	0.2048	0.3125	0.4427
2019	0.3737	0.2222	0.3257	0.5000	0.3257	0.2000	0.2997	0.4314	0.3274	0.2000	0.2997	0.4315
2020	0.3735	0.2222	0.3238	0.4997	0.3238	0.2000	0.2988	0.4286	0.3257	0.2000	0.2988	0.4286
<i>Panel E: Net Employment Growth</i>												
2016	0.0544	-0.0692	0.0000	0.1538	0.0506	-0.0597	0.0303	0.1467	0.0510	-0.0597	0.0303	0.1505
2017	0.0510	-0.0749	0.0000	0.1538	0.0541	-0.0640	0.0361	0.1538	0.0542	-0.0640	0.0361	0.1538
2018	0.0522	-0.0718	0.0000	0.1538	0.0490	-0.0594	0.0379	0.1432	0.0489	-0.0594	0.0379	0.1432
2019	0.0335	-0.0955	0.0000	0.1408	0.0314	-0.0793	0.0000	0.1322	0.0319	-0.0793	0.0000	0.1322
2020	-0.0188	-0.1346	0.0000	0.1053	-0.0122	-0.1127	0.0000	0.0952	-0.0111	-0.1127	0.0000	0.0952
<i>Panel F: Net Employment Growth (DHS)</i>												
2016	0.0918	-0.0669	0.0000	0.1667	0.0916	-0.0579	0.0308	0.1583	0.0918	-0.0579	0.0308	0.1628
2017	0.0922	-0.0722	0.0000	0.1667	0.0880	-0.0620	0.0368	0.1667	0.0882	-0.0620	0.0368	0.1667
2018	0.0915	-0.0693	0.0000	0.1667	0.0738	-0.0577	0.0387	0.1542	0.0742	-0.0577	0.0387	0.1542
2019	0.0724	-0.0912	0.0000	0.1515	0.0600	-0.0763	0.0000	0.1416	0.0611	-0.0763	0.0000	0.1416
2020	0.0127	-0.1262	0.0000	0.1111	0.0101	-0.1067	0.0000	0.0999	0.0118	-0.1067	0.0000	0.0999

*Notes:* Firm-level employment flows from administrative worker records. Hiring rate =  $H_{it}/E_{i,t-1}$ , separation rate =  $S_{it}/E_{i,t-1}$ , net employment growth =  $(E_{it} - E_{i,t-1})/E_{i,t-1}$ . DHS rates use the average of  $E_{it}$  and  $E_{i,t-1}$  as the denominator. “Non-surveyed” refers to firms in the administrative population meeting survey eligibility criteria (at least 5 employees in 2019) but not in the survey sample. “Surveyed (weighted)” applies entropy-balancing weights targeting population moments for firm size and sector shares.

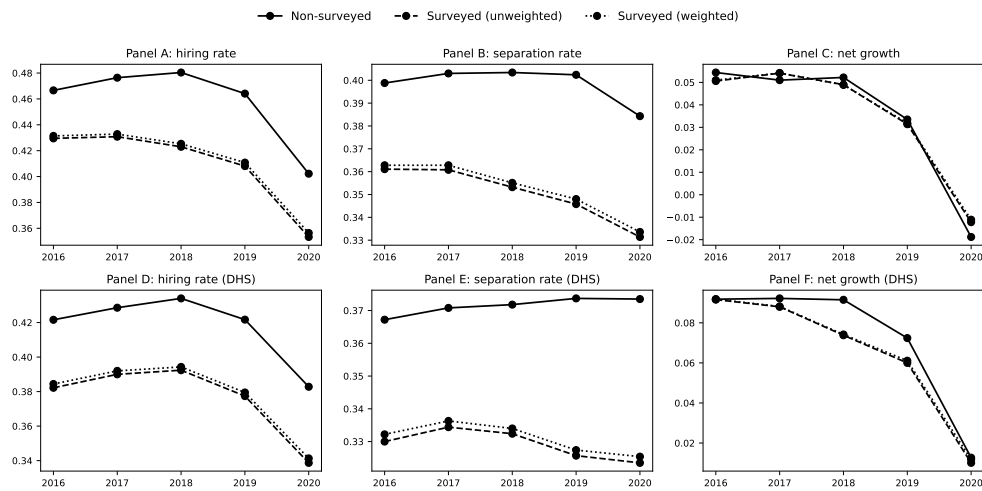


Figure H.3: Employment Dynamics: Surveyed vs. Non-Surveyed Firms, 2016-2020

Note: Firm-level hiring rates, separation rates, and net employment growth, 2016-2020. Each panel plots means for non-surveyed firms (full population meeting eligibility criteria), surveyed firms (unweighted), and surveyed firms (entropy-weighted). DHS rates use the Davis-Haltiwanger-Schuh denominator. The three groups exhibit parallel trends throughout 2016-2019; the 2020 adjustment is moderate and similar across groups.

# I Unions and Adjustment

Unionization shifts the chosen adjustment margin in the direction one expects: more unionized firms are less likely to cut wages and more likely to do permanent layoffs (Section I.1). However, in Denmark, “unionization” is not what primarily explains the stated barriers to base-pay cuts in the Figure 4 constraint battery once you condition on firm/industry characteristics (Section I.2). But unionization does show up in the “why layoffs rather than pay cuts” logic: more unionized firms are more likely to endorse the idea that layoffs give better control over who leaves (Section I.3).

## I.1 Unionization rates and the use of wage reductions vs. permanent layoffs

This section documents how firms’ use of base-wage reductions and permanent layoffs varies with unionization intensity. We estimate firm-level linear probability models where the dependent variable is an indicator (multiplied by 100) for whether the firm reports using the corresponding adjustment margin in 2020. The key regressor is the firm’s unionization rate (standardized). All specifications control for revenue growth, industry fixed effects, local labor-market tightness, and the standard firm controls used elsewhere in the paper; we report heteroskedasticity-robust standard errors.

Table I.1 reports the full specification. More unionized firms are less likely to report base-wage reductions and more likely to implement permanent layoffs, controlling for revenue shocks and other covariates. Table I.2 shows that the unionization coefficients are stable across alternative control sets.

## I.2 Unionization, contracting, and the “Illegal/Almost Impossible” base-pay constraint

This section addresses external validity concerns related to Denmark’s high unionization and interpret two survey facts from the “reasons for not lowering base pay” battery (Figure 4): (i) union presence is not the dominant stated barrier to base-pay cuts, and (ii) many firms report that base-pay cuts are “illegal/almost impossible.”

A key nuance is that the “reasons for not lowering base pay” battery (Figure 4 object) is conditional on revenue reduction and on not doing base wage reductions. The purpose of the empirical exercises below is therefore to assess whether the “illegal/almost impossible” response is primarily a union-contract proxy or reflects broader contractual/renegotiation constraints that also operate outside the presence of a union. The analysis uses the subsample for which this battery is defined: firms reporting a revenue reduction in 2020 and reporting no base-wage reduction. Results use the entropy-balancing weights.

*“Illegal/almost impossible” versus union opposition.* Online Appendix Table I.3 cross-tabulates agreement with the “illegal/almost impossible” item and agreement with the “unions are against” item. Agreement with “illegal/almost impossible” is strongly associated with agreement that unions oppose pay reductions. However, the relationship is not one-for-one: a sizable share of firms that agree that base-pay cuts are illegal/almost impossible do not agree that unions are against pay reductions. This pattern is consistent

Table I.1: Unionization and the Use of Wage Reductions vs. Permanent Layoffs (full specification)

	Base wage reduction (1)	Permanent layoffs (2)
Productivity	-3.47*** (0.63)	-4.60*** (0.98)
Average wages	2.41*** (0.83)	2.48** (1.07)
Revenue growth rate in 2020 (%)	-3.60*** (0.80)	-6.15*** (1.05)
Routine task index	-1.51*** (0.41)	-1.58*** (0.55)
Unionization (std.)	-1.56*** (0.60)	1.81** (0.79)
Worker representative	2.71** (1.21)	-0.05 (1.64)
Tightness	-1.81*** (0.37)	-2.08*** (0.62)
Observations	2,771	2,771
Mean Dep. Var.	8.30	18.77
Adj. $R^2$	0.036	0.047
Additional controls	Yes	Yes

Notes: Dependent variables are indicators (multiplied by 100) for whether the firm reports using the corresponding adjustment margin in 2020 (see Figure 1). “Unionization” is the standardized share of unionized employees in the firm. All specifications include the standard firm controls and industry fixed effects used elsewhere in the paper, and controls for revenue growth and local labor-market tightness. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table I.2: Unionization Coefficient Robustness across Control Sets

	Base wage reduction	Permanent layoffs
No controls	-1.47***	2.09***
Basic controls	-1.53***	2.25***
More controls	-1.56***	1.81**

Notes: Each row reports the coefficient on the standardized unionization measure from a separate firm-level regression. “No controls” includes only unionization. “Basic controls” adds productivity, average wages, revenue growth, the routine task index, worker representative, and local labor-market tightness. “More controls” adds the extended set of controls used elsewhere (e.g., respondent role, wage-setting knowledge, wage-floor exposure, firm size, age, and leverage). Significance stars as in Table I.1.

with the “illegal/almost impossible” item capturing a broader contractual and renegotiation constraint beyond union opposition alone.

*Union opposition versus wage-setting institutions.* Online Appendix Table I.4 relates the union-opposition response to a proxy for wage-setting institutions (at least 50% covered by wage floors). The shares are similar across wage-floor coverage groups, suggesting that this wage-floor proxy does not, by itself, explain cross-firm variation in the union-opposition response within Denmark.

*Regression evidence.* Online Appendix Table I.5 reports linear probability models for both constraint items, controlling for revenue-shock severity, industry fixed effects, and the baseline firm controls used elsewhere in the paper. A key practical issue is that “union presence” defined as any unionized worker is close to universal in this subsample, limiting its informativeness as a stand-alone indicator. We therefore interpret the results using the richer measures available in the memo specifications (worker representative and, where included, the unionization share). The main pattern is that union-related measures load much more strongly on the “unions are against” response than on the “illegal/almost impossible” response, consistent with the view that the “illegal/almost impossible” item reflects broader contractual/renegotiation constraints and not only on-site union opposition.<sup>53</sup>

Summarizing, within the revenue-decline/no-base-cut sample, the perceived “illegal/almost impossible” constraint is strongly correlated with citing union opposition, but it is not reducible to union presence or wage-floor coverage—consistent with a broader contractual/renegotiation constraint on base-pay cuts in Denmark.

Table I.3: “Illegal/almost impossible” vs. “Unions are against” (weighted cross-tab)

Agree: illegal/almost impossible	Agree: unions are against pay reductions		Total
	0	1	
0	79.17	20.83	100.00
1	39.66	60.34	100.00
Total	60.01	39.99	100.00

Notes: Row percentages. Sample: Figure 4 base-pay battery (revenue reduction and no base-wage reduction).

### I.3 Unionization controls across main survey batteries

This section examines whether our main results look different in firms with a union presence. In Denmark, union coverage and workplace representation are widespread, so a simple 0/1 “union presence” split is not very informative in many of the subsamples used for our core survey batteries. To provide a more informative within-country check, Appendix Tables A.5-A.12 include controls for the firm unionization rate and workplace representation (and, where relevant, wage-setting proxies), alongside the baseline firm controls, revenue-shock controls, and industry fixed effects. Below we summarize what these union-related controls do across the main batteries.

<sup>53</sup>Specifications using the share of unionized employees in place of the union-presence indicator yield similarly small and statistically weak relationships.

Table I.4: Union-Opposition Response vs. Wage-Floor Coverage (weighted cross-tab)

Agree: unions are against	At least 50% covered by wage floors		Total
	0	1	
0	84.91	15.09	100.00
1	82.75	17.25	100.00
Total	84.05	15.95	100.00

Notes: Row percentages. Sample: Figure 4 base-pay battery (revenue reduction and no base-wage reduction).

- *Table A.5 (reasons not to use base pay cuts)*. Including the firm unionization rate does not materially change the patterns in the reasons-for-not-cutting-pay battery. The unionization rate itself is not a robust predictor of selecting the union-related reason. Instead, workplace representation is the clearer correlate of stating that employee representatives oppose pay reductions. This supports the interpretation that the relevant friction is not a simple union/non-union divide within Denmark, but rather how pay-setting and representation structures shape the feasibility of renegotiation.
- *Table A.6 (reasons to retain workers despite reduced revenue)*. The headline retention motives (avoiding loss of valuable skills; fear of slow rehiring; team/cooperation concerns) do not become “union-driven” once unionization controls are included. Union-related variables are not systematic predictors of these motives, and the primary patterns remain tied to the shock and firm characteristics rather than unionization intensity. One exception is that unionization is negatively associated with citing government aid packages as the retention reason.
- *Table A.7 (perceived effects of layoffs on remaining employees)*. Controlling for unionization does not change the qualitative patterns in perceived post-layoff impacts on stayers. The unionization rate is not a robust predictor of reporting that layoffs hurt morale or raise workload/effort. Workplace representation is associated with a lower probability of answering that layoffs had “no effect,” consistent with more active internal processes around communicating and managing layoffs.
- *Table A.8 (crisis as an opportune time for layoffs)*. Unionization intensity is not systematically related to viewing the crisis as an opportunity for reorganization. The only notable association is a positive relationship between unionization and agreement that layoffs are more acceptable because the firm can select which workers leave; other items are not robustly related to unionization.
- *Table A.9 (why layoffs rather than pay cuts)*. Unionization is not strongly related to morale-based objections to pay cuts in this battery. Where it matters, unionization is positively associated with the statement that layoffs offer better control over who leaves the company, aligning more with selection/reorganization logic than with a pure “unions block wage cuts” mechanism.

Table I.5: Union Institutions and Stated Constraints on Base-Pay Reductions

	DV: illegal/almost impossible		DV: unions are against	
	(1)	(2)	(3)	(4)
Union presence in firm	-0.075 (0.174)		0.221** (0.129)	
Worker representative in firm		0.109** (0.053)		0.191*** (0.051)
At least 50% covered by wage floors	-0.034 (0.062)	-0.018 (0.062)	-0.027 (0.062)	-0.027 (0.065)
Severe shock (<-35%)	-0.188 (0.159)	-0.186 (0.159)	-0.111 (0.152)	-0.119 (0.152)
Moderate shock ([-35%,-5%])	-0.046 (0.071)	-0.050 (0.071)	0.006 (0.069)	0.000 (0.069)
Observations	512	512	507	507
R-squared	0.081	0.089	0.154	0.160
Firm controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Notes: Linear probability models estimated on the Figure 4 base-pay battery sample (revenue reduction and no base-wage reduction). Dependent variables are indicators for agreeing/strongly agreeing with the corresponding Figure 4 statement. All specifications include the standard firm controls and industry fixed effects used elsewhere in the paper, and the revenue-shock controls shown. Robust standard errors in parentheses.

- *Table A.10 (how many layoffs would have happened anyway)*. Higher unionization is positively associated with reporting that a larger share of layoffs would have occurred even without the pandemic shock, and this relationship remains when adding additional controls. This indicates that unionization does not mechanically imply that layoffs are avoided; if anything, unionization is correlated with a larger perceived structural component of layoffs in this question.
- *Table A.11 (probability of answering “Do not know” on the pay-cut-vs-layoff counterfactual)*. The unionization rate does not predict “Do not know” responses. Workplace representation is associated with fewer “Do not know” answers, consistent with more structured internal discussion or clearer views about the feasibility of alternative margins of adjustment.
- *Table A.12 (size of the pay-cost reduction that could have prevented layoffs)*. Unionization intensity is not a robust predictor of whether firms report that large pay-cost reductions would have been needed. In contrast, labor market tightness is associated with reporting smaller required pay-cost reductions, consistent with the idea that alternative adjustment margins and constraints vary with market conditions.

**Takeaway.** Across Appendix Tables A.5 -A.12, adding unionization controls does not overturn the paper’s main qualitative patterns. Within Denmark, unionization intensity rarely

predicts the stated feasibility/cost obstacles to base-pay cuts in a robust way. When unionization does matter, it is more tightly connected to selection-oriented layoff rationales (control over who leaves) and to the perceived “layoffs anyway” component than to a simple mechanism in which unions mechanically prevent wage reductions. These results help address concerns that our findings are driven by Denmark’s high unionization rates: the central patterns are not specific to a small non-union corner of the sample, and they persist when conditioning on within-country unionization measures.

## J Evidence on Pay Cuts from Administrative Data

Given an abundant macroeconomics literature that relies on wage rigidity, our findings that pay cuts are not rare are perhaps surprising. However, we find that the finding of our survey that pay cuts are not rare is also supported in the administrative data.

We examine the incidence and extent of pay cuts from two additional datasets: (1) LONN—a mandatory annual employer survey on earnings components for all firms with at least 10 employees, which separately identifies the contractual base wage from other pay components; and (2) the BFL (*Detaljeret lønmodtagerdata fra e-Indkomst*)—monthly administrative records of worker earnings and hours, which allows calculating an average wage from total earnings and hours worked. Focusing on full-time salaried job stayers (e.g., workers who remained in the same establishment within the same job function (using a 6-digit occupation code) for 24 consecutive months, worked at least 1,820 hours per year, was not absent from work for more than a month, was classified as salaried and not hourly worker), we find that nominal cuts to the contractual wage as well as to the total pay are not rare. In LONN, during 2019-2020, 18.3% of the job stayers workers received base wage cuts and 25.9% received cuts to total hourly pay; during 2018-2019—13.3% and 20.2%, respectively (Figure J.1). In BFL, during 2019-2020, 23.2% of the job stayers experienced a reduction in total nominal hourly pay (Figure J.2).<sup>54</sup> The conclusions are similar from the linked survey-BFL data.

The finding that the greater the revenue reduction, the more likely is the firm to implement wage reduction also holds in the administrative data. Specifically, we link the LONN data to the firm’s financial information (the FIRM dataset), and group the firms into the revenue growth bins as above. Then, for every job-stayer in the private sector in the administrative wage data, we construct a dummy variable equal to one if the worker experiences a negative hourly wage change of more than 5% between year  $t - 1$  to  $t$ . Figure K.1 shows the relation between the percent of job stayers receiving a wage reduction and the firm’s 2019-2020 revenue growth (by bin). The probability of receiving a wage reduction increases as firm’s revenue declines.

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<sup>54</sup>To validate the responses from our survey, we link our firm-level survey data with the LONN data using firm-level identifiers and compare the firms’ responses to pay cuts in the survey with the wage cuts reported in LONN for those workers who are employed by the companies in the survey. As expected, in the LONN data, the share of workers who received base hourly wage cuts is higher in the firms that reported wage reductions in the survey. Specifically, for each linked firm, we use worker-level LONN data and calculate the average nominal base wage growth between 2019 and 2020 across all the workers in the firm. Figure J.3 Panel (a) plots the distribution of the average base wage growth between 2019 and 2020 in LONN for two sub-samples of firms: the sample of the firms that in our survey reported wage reductions in 2020 (red line), and the sample of the firms that in our survey reported no wage reductions in 2020 (blue line). The figure shows that the former distribution has a higher mass on observations with the negative growth. Figure J.3 Panel (b) shows the distribution of the firm-level share of workers affected by base wage cut in LONN data for two sub-samples of firms — those that reported wage cuts in our survey and those that report no pay cuts in our survey. To construct the figure, we use the LONN data and for each firm we calculate the share of workers whose nominal hourly base wage between 2019 and 2020 declined by more than 5%. We then plot the distribution of these shares separately for firms that in the survey reported a wage cut (red area) and those that report no cut (blue area). The figure shows that for firms that reported no wage cuts, there is a larger mass of the distribution close to zero than for firms that reported some wage cuts.

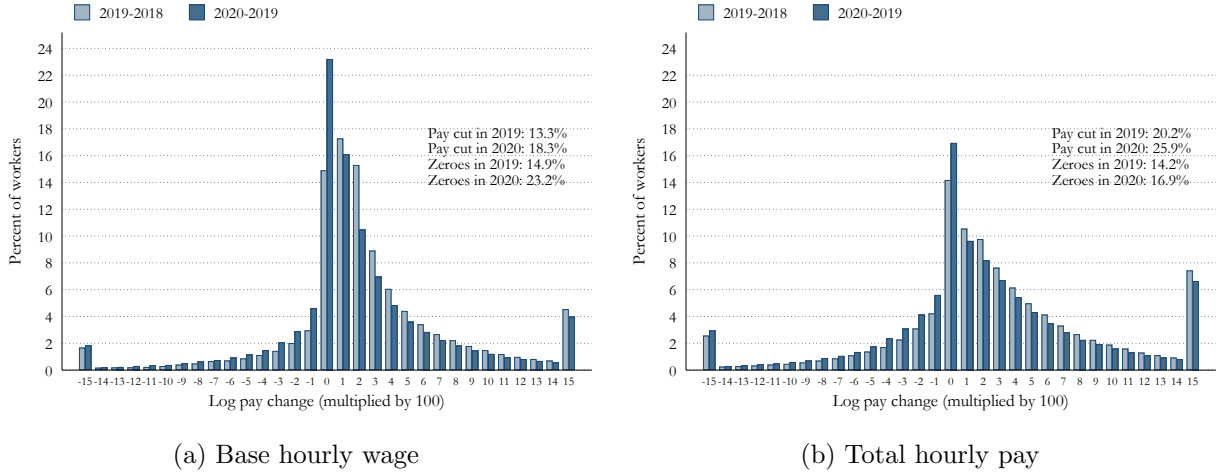


Figure J.1: The Distribution of the Annual Growth of Nominal Hourly Pay

Note: The figure shows the annual nominal changes in logarithmic pay using data from the mandatory firm survey, LONN. The pay is the base hourly wage in Panel (a) and total hourly pay in Panel (b). See text for definition of the base wage in LONN. The sample is restricted to salaried workers who remain in the same establishment and within the same job function (6-digit occupation codes) from year  $t - 1$  to year  $t$ . The log pay change is computed as the log differences between year  $t$  minus year  $t - 1$ , multiplied by 100. Each pay change value  $x$  includes log changes in the interval  $x - 0.5$  and  $x + 0.5$ .

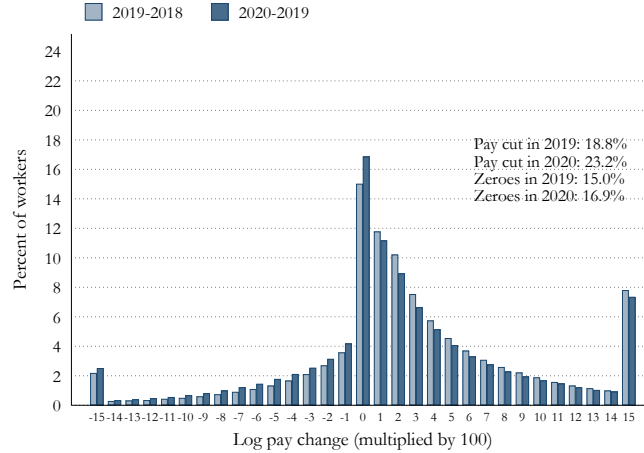
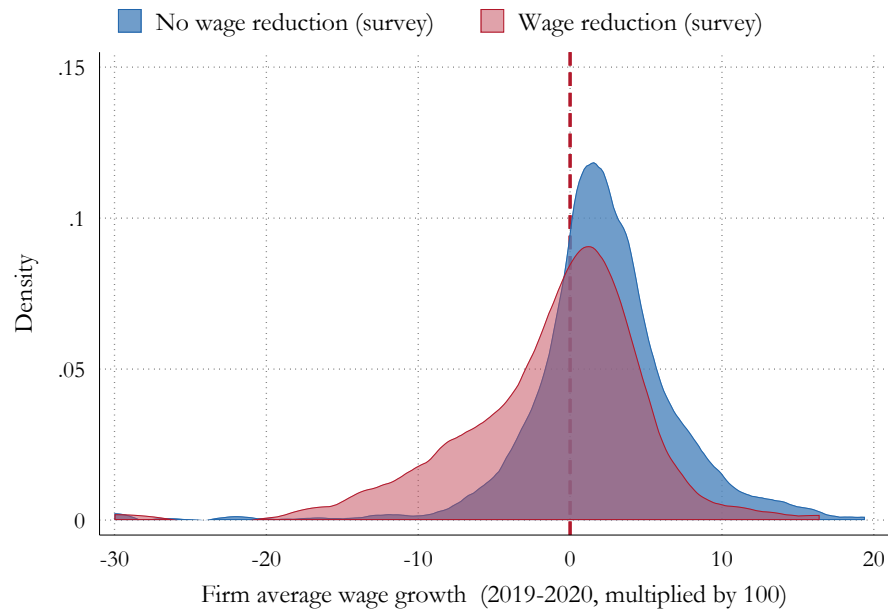
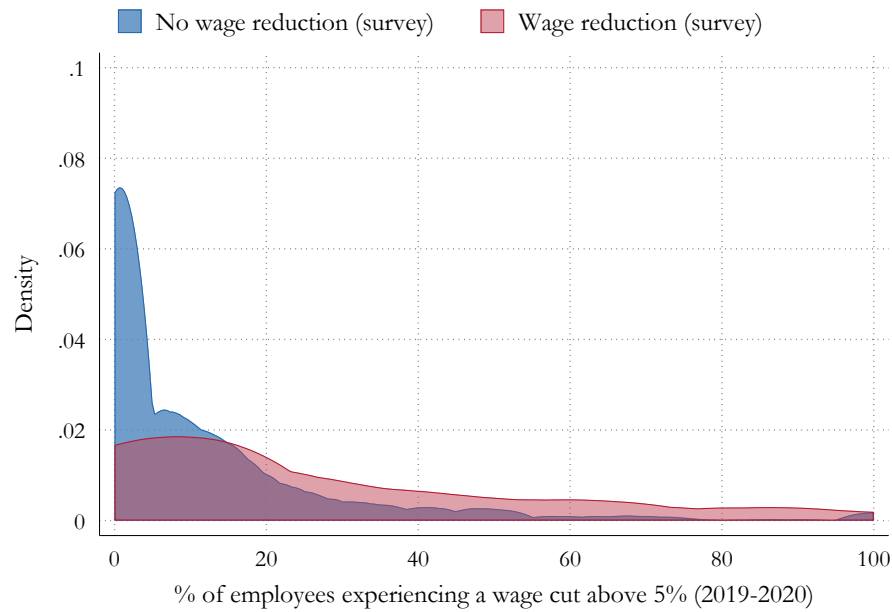


Figure J.2: Growth of Nominal Total Hourly Pay in the Administrative Data, Year-over-Year

The figure shows the annual growth in total nominal hourly pay in the administrative data from BFL. The log pay change is computed as the log differences between year  $t$  minus year  $t - 1$ , multiplied by 100. Each pay change value  $x$  includes log changes in the interval  $x - 0.5$  and  $x + 0.5$ . The sample consists of the salaried employees continuously employed for 24 months in private sector firms, at the same establishment, and in the sample occupation.



(a) Distribution of the average firm base wage growth



(b) Distribution of the firm's share of workers affected by base wage cut

Figure J.3: Nominal Wage Cuts Reported in Our Survey and in the Mandatory Survey of Firms

Note: Panel (a) shows distributions of the average firm nominal base wage growth 2019-2020 from the LONN data linked to our survey data, by the reported wage reductions at the firm level in our survey. Panel (b) shows the distribution of the firm-level share of workers affected by base wage cut in the LONN data, linked to our survey data.

## K Administrative Evidence on Selection in Layoffs and Pay Adjustments within Firms

This appendix uses the linked employer-employee administrative registers to study which workers were laid off and which experienced pay adjustments during 2020. The goal is to complement the survey evidence that layoffs provide employers with finer control over who leaves the firm (Figure 9) by documenting within-firm selection patterns.

For the layoff-selection analysis, we start from the universe of workers employed at a surveyed firm in 2019. We define  $\text{Layoff}_{ij} = 1$  if worker  $j$  employed at firm  $i$  in 2019 separates from the firm during 2020 in a way consistent with an employer-initiated layoff under our baseline administrative definition. For pay-adjustment outcomes we restrict to stayers, defined as workers who remain employed at the same firm in both 2019 and 2020. We use linked administrative registers (BFL for employment spells, LONN for wage components and bonuses, and IDAP/IND for the layoff indicator).

We study three sets of outcomes:

1. *Administrative layoffs.* The layoff indicator  $\text{Layoff}_{ij}$  is defined in the preceding paragraph.
2. *Base-wage adjustments among stayers.* Using contractual hourly base wages (including fixed contractual supplements), we compute the percent change in the base wage between 2019 and 2020 and define indicators for (i) a base-wage cut and (ii) a base-wage non-increase (freeze or cut). See Appendix N for the precise construction.
3. *Variable-pay cuts among stayers.* Using annual irregular pay components (bonuses), we define  $\text{VarCut}_{ij} = 1$  if a worker with positive bonus pay in 2019 experiences a decline in bonus pay in 2020.

**Proxies for worker value / performance.** The administrative registers do not include direct performance ratings. We therefore construct two pre-pandemic proxies for worker value that vary within firm and occupation: (i) a bonus indicator  $\text{Bonus}_{ij,2019}$  equal to one if the worker receives any bonus/irregular pay in 2019; and (ii) a within-firm  $\times$  occupation relative wage  $\tilde{w}_{ij,2019}$ , defined as the worker’s 2019 base wage relative to the mean wage of coworkers in the same firm and 2-digit DISCO occupation; this relative wage may be capturing worker value/skill (or rents attached to seniority/roles). For the variable-pay analysis we additionally use the bonus share in 2019, defined as bonus pay divided by base pay in 2019 (winsorized at the 99th percentile).

**Empirical specification.** We estimate worker-level linear probability models of the form

$$y_{ij} = \alpha + \beta_1 \text{Bonus}_{ij,2019} + \beta_2 \tilde{w}_{ij,2019} + X'_{ij} \Gamma + \phi_{i \times o(j)} + \varepsilon_{ij}, \quad (\text{K.1})$$

where  $y_{ij}$  is either the layoff indicator (pre-period worker sample) or one of the pay-adjustment indicators (stayer sample),  $o(j)$  denotes worker  $j$ ’s 2-digit DISCO occupation,  $\phi_{i \times o(j)}$  are firm  $\times$  occupation fixed effects, and  $X_{ij}$  includes standard worker controls (gender, age, tenure, experience, full-time status, salaried status, and immigrant status). Standard errors are clustered at the firm level.

The firm  $\times$  occupation fixed effects imply that identification comes from comparisons of workers within the same firm and occupation.

**Main results.** Layoffs are strongly selective within firm $\times$ occupation cells: workers with higher relative pre-pandemic wages and longer tenure are significantly less likely to be laid off (Appendix Table K.1). The qualitative layoff-selection patterns are robust to various specifications: (i) the results are similar if instead of the bonus dummy we use the bonus share in 2019, defined as bonus pay divided by base pay in 2019; (ii) the qualitative selection patterns are not confined to a single shock category and remain largely unchanged if we add interactions of bonus with firm revenue-shock severity; (iii) baseline layoff-selection results are not driven by cells with no layoffs (e.g., hold when the sample is restricted to firm $\times$ occupation cells with within-cell variation in layoffs).

In contrast to layoffs, contractual base-wage cuts among stayers do not appear to be selected (Appendix Table K.1). Base-wage non-increases and bonus cuts show more selectivity. Bonus recipients are less likely to experience a base-wage non-increase (Appendix Table K.2), and workers with higher pre-pandemic bonus shares are more likely to experience a variable-pay cut (Appendix Table K.3).

The selection pattern for pay adjustments is twofold: (i) variable-pay cuts concentrate among workers with more variable pay to begin with (mechanically intuitive), and (ii) wage non-increases relate to the bonus proxy in a way consistent with valued workers still receiving raises, even though layoffs on average are not selective on the bonus proxy.

**Connecting administrative selection to the survey “control” motive.** To link the administrative evidence more directly to Figure 9, we also estimate specifications that interact worker value proxies with the firm-level indicator that the firm agrees that layoffs provide better control over who leaves the company. Consistent with the survey interpretation, bonus recipients are significantly less likely to be laid off in firms that cite this control motive (Appendix Table K.4), providing a direct admin-data counterpart to Figure 9.

**Robustness: pre-shock wage growth.** As an additional proxy for worker value, we also consider pre-shock base-wage growth (2018-2019) within firm $\times$ occupation cells. The main qualitative conclusions are unchanged; pre-shock wage growth is particularly informative for base-wage non-increases in the restricted-sample specifications.

Table K.1: Selection into Layoffs and Base-Wage Cuts within Firm×Occupation Cells

	All pre-crisis workers			Stayers		
	Layoff			Base Wage Cut		
	(1)	(2)	(3)	(4)	(5)	(6)
Bonus Dummy	-0.0067 (0.01)	0.0005 (0.00)	0.0019 (0.00)	-0.0013 (0.01)	0.0183 (0.01)	0.0091 (0.02)
Relative Wage (2019)	-0.0023** (0.00)	-0.0023*** (0.00)	-0.0029*** (0.00)	0.0037 (0.00)	0.0032 (0.00)	0.0041* (0.00)
Male	-0.0084** (0.00)	-0.0075*** (0.00)	-0.0039 (0.00)	0.0221*** (0.01)	0.0185*** (0.01)	0.0054 (0.01)
Employee Age	-0.0033** (0.00)	-0.0032*** (0.00)	-0.0021* (0.00)	-0.0027 (0.00)	-0.0017 (0.00)	-0.0002 (0.00)
Employee Age (Sq.)	0.0000*** (0.00)	0.0000*** (0.00)	0.0000** (0.00)	0.0000 (0.00)	0.0000 (0.00)	0.0000 (0.00)
Tenure (Years)	-0.0019*** (0.00)	-0.0022*** (0.00)	-0.0022*** (0.00)	-0.0008 (0.00)	-0.0002 (0.00)	-0.0005 (0.00)
Full Time	-0.0119* (0.01)	-0.0115* (0.01)	-0.0140* (0.01)	-0.0079 (0.01)	0.0134 (0.01)	0.0166 (0.01)
Salaried	-0.0278*** (0.00)	-0.0149*** (0.00)	-0.0191*** (0.00)	0.0097 (0.07)	0.0347 (0.07)	-0.0240 (0.04)
Observations	20,585	20,515	18,853	19,927	19,849	18,225
R-squared	0.0076	0.1983	0.2935	0.0018	0.1286	0.2237
Firm FE	No	Yes	Yes	No	Yes	Yes
Firm × Occupation FE	No	No	Yes	No	No	Yes

Note: Worker-level linear probability models. “Relative wage” is measured within firm×occupation in 2019. Firm×occupation fixed effects use 2-digit DISCO occupations. Standard errors clustered at the firm level.

Table K.2: Selection into Base-Wage Non-Increases among Stayers

	Stayers		
	Non-Increase		
	(4)	(5)	(6)
Bonus Dummy	-0.0720*** (0.02)	-0.0338** (0.01)	-0.0398*** (0.01)
Relative Wage (2019)	0.0037 (0.01)	0.0033 (0.01)	0.0021 (0.01)
Male	-0.0012 (0.01)	0.0022 (0.01)	-0.0047 (0.01)
Employee Age	0.0092** (0.00)	0.0100** (0.00)	0.0122*** (0.00)
Employee Age (Sq.)	-0.0000 (0.00)	-0.0000 (0.00)	-0.0001 (0.00)
Tenure (Years)	0.0002 (0.00)	0.0004 (0.00)	-0.0003 (0.00)
Full Time	0.0127 (0.02)	0.0257 (0.02)	0.0362* (0.02)
Salaried	-0.1032 (0.08)	-0.0195 (0.07)	-0.1043* (0.06)
Observations	19,927	19,849	18,225
R-squared	0.0250	0.2195	0.3042
Firm FE	No	Yes	Yes
Firm $\times$ Occupation FE	No	No	Yes

Note: Dependent variable is an indicator for a base-wage non-increase (freeze or cut) between 2019 and 2020. Worker-level linear probability models with firm $\times$ occupation fixed effects (2-digit DISCO) and standard errors clustered at the firm level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table K.3: Selection into Variable-Pay Cuts among Stayers

	Stayers		
	Var. Pay Cut		
	(4)	(5)	(6)
Share of Bonus (% Base Pay)	0.0050* (0.00)	0.0096*** (0.00)	0.0120*** (0.00)
Relative Wage (2019)	-0.0124 (0.01)	-0.0180** (0.01)	-0.0236** (0.01)
Male	-0.0344 (0.02)	-0.0378*** (0.01)	-0.0293** (0.01)
Employee Age	-0.0006 (0.01)	-0.0039 (0.01)	0.0047 (0.01)
Employee Age (Sq.)	0.0000 (0.00)	0.0000 (0.00)	-0.0000 (0.00)
Tenure (Years)	0.0067*** (0.00)	0.0023*** (0.00)	0.0026*** (0.00)
Full Time	0.0747** (0.03)	0.0141 (0.03)	0.0111 (0.03)
Salaried	0.0017 (0.09)	-0.0177 (0.08)	0.0443 (0.07)
Observations	8,909	8,794	8,056
R-squared	0.0147	0.3534	0.4587
Firm FE	No	Yes	Yes
Firm × Occupation FE	No	No	Yes

Note: Dependent variable is an indicator for a reduction in bonus/irregular pay between 2019 and 2020 among workers with positive bonus pay in 2019. Standard errors clustered at the firm level. Worker-level linear probability models with firm×occupation fixed effects (2-digit DISCO) and standard errors clustered at the firm level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table K.4: Heterogeneity by the Survey Motive “Layoffs Give Better Control over Who Leaves”

	Control		
	(1) No FE	(2) Firm FE	(3) Firm-Occ FE
<i>Treatment Variables</i>			
Bonus Dummy	−0.006 (0.01)	0.008 (0.01)	0.007 (0.01)
<i>Interaction: Control Reason</i>			
Control Reason × Bonus	−0.031 (0.03)	−0.015* (0.01)	−0.016** (0.01)
<i>Employee Characteristics</i>			
Relative Wage (2019)	−0.004** (0.00)	−0.005*** (0.00)	−0.005*** (0.00)
Male	−0.014*** (0.01)	−0.011** (0.00)	−0.005 (0.00)
Employee Age	−0.005** (0.00)	−0.003 (0.00)	−0.003 (0.00)
Employee Age (Sq.)	0.000*** (0.00)	0.000** (0.00)	0.000** (0.00)
Tenure (Years)	−0.003*** (0.00)	−0.004*** (0.00)	−0.004*** (0.00)
Full Time	−0.026** (0.01)	−0.025** (0.01)	−0.025** (0.01)
Salaried	−0.041*** (0.01)	−0.018*** (0.01)	−0.020*** (0.01)
Observations	11,221	11,203	11,179
R-squared	0.015	0.201	0.237

Note: The dependent variable is the layoff indicator. The key regressor is the interaction between the firm-level survey indicator that the firm agrees that layoffs provide better control over who leaves the company from the survey battery described in Figure 9 and the worker’s 2019 bonus indicator. Worker-level linear probability models with firm×occupation fixed effects (2-digit DISCO) and standard errors clustered at the firm level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

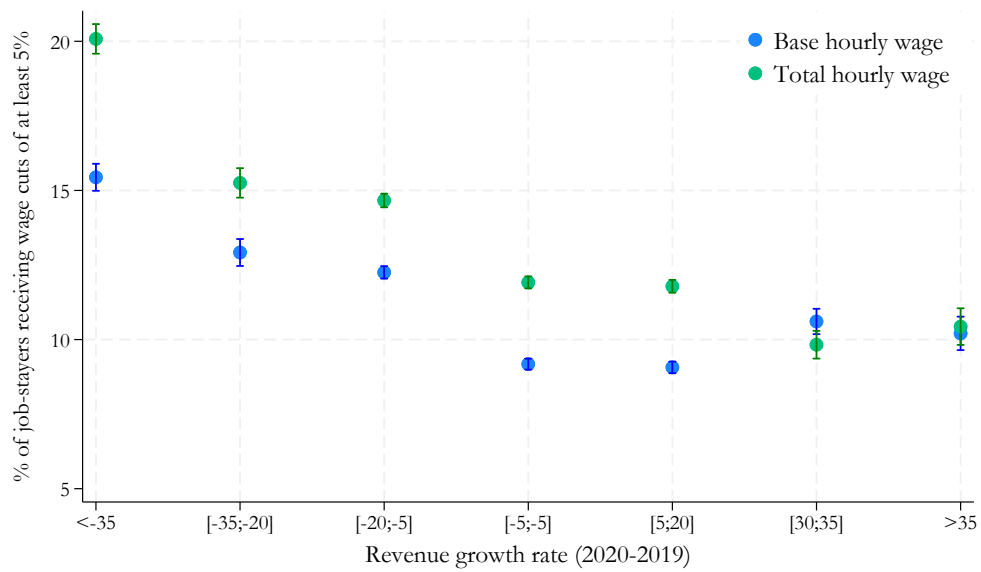


Figure K.1: Percentage of Workers with Wage Reduction, by Firm Revenue Growth

Note: The figure shows the estimated probability of a job-stayer receiving a 5% nominal wage reduction conditional on the firm's 2019-2020 revenue growth, using LONN wage data. See text for details.

## L Are Worker Retention Motives Pandemic-Specific? Additional Evidence

Our survey was fielded after the pandemic recession and this might raise the concern that the headline retention motives in Figure 5 (avoid losing valuable skills; concerns about rehiring) may be unusually salient in a lockdown environment with an anticipated sharp rebound. We assess whether these motives are concentrated in pandemic-specific settings by examining heterogeneity by (i) lockdown-exposed industries and (ii) firms' expectations about shock duration.

In all exercises in this section, we use the same survey subsample for which Figure 5 is defined: firms reporting a revenue reduction in 2020. We use the entropy-balancing weights from Section 2.4. Throughout, missing survey responses are coded as missing and excluded from the relevant regressions.

For each firm  $i$ , we define agreement indicators (Agree or Strongly Agree) for the two leading motives: (i) retaining workers to avoid loss of skills and knowledge, and (ii) concerns about rehiring quickly during recovery. All specifications condition on revenue-shock severity and include the baseline firm controls used elsewhere (e.g., size and other standard covariates) as well as industry fixed effects. Standard errors are heteroskedasticity-robust.

**Lockdown exposure.** Online Appendix Table L.1 relates agreement with the two leading retention motives to a lockdown-exposure indicator, controlling for shock severity and firm characteristics. The estimated differences by lockdown exposure are modest: agreement with the skill-retention motive is slightly higher in lockdown-exposed industries, while the rehiring-difficulty motive is not statistically different.

**Expected duration of the revenue decline.** Online Appendix Table L.2 examines whether these motives are concentrated among firms expecting a short-lived shock (the rapid-rebound logic). Relative to firms expecting a revenue decline lasting under one year, expected longer duration is not associated with lower agreement for either motive. This pattern is inconsistent with an interpretation in which the headline motives are driven primarily by expectations of a rapid post-lockdown rebound.

**Pre-shock firm characteristics.** Online Appendix Table L.3 reports an additional check relating retention motives to pre-shock (2019) proxies for skill intensity and local labor market tightness. These pre-shock proxies are not strong predictors of agreement once we condition on shock severity and observables, consistent with retention motives being broadly shared across firms in the revenue-decline sample.

Table L.1: Retention Motives and Lockdown Exposure (Figure 5 sample)

	Agree: Skill retention motive		Agree: Rehiring-difficulty motive	
	Coef.	SE	Coef.	SE
Lockdown-exposed industry	0.071*	(0.041)	-0.134	(0.167)
Moderate shock ([-35%, -5%])	-0.006	(0.025)	-0.040	(0.041)
Severe shock (<-35%)	-0.028	(0.047)	-0.080	(0.078)
Observations		605		605
$R^2$		0.036		0.091
Firm controls		Yes		Yes
Industry FE		Yes		Yes

Notes: Dependent variables are indicators for Agree/Strongly agree with the corresponding Figure 5 retention-motive statements. Sample restricted to firms reporting a revenue reduction. Lockdown-exposed industries are defined as in the internal memo (NACE 55, 56, 79, 51). Specifications use entropy-balancing weights and robust standard errors. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table L.2: Retention Motives and Expected Duration of the Revenue Decline (Figure 5 sample)

	Agree: Skill retention motive		Agree: Rehiring-difficulty motive	
	Coef.	SE	Coef.	SE
Expected duration (base: Under 1 year)				
More than 1 year	0.007	(0.023)	-0.027	(0.043)
Do not know	-0.089**	(0.044)	-0.036	(0.056)
Moderate shock ([-35%, -5%])	-0.007	(0.029)	-0.038	(0.050)
Severe shock (<-35%)	0.050	(0.055)	-0.051	(0.083)
Observations		439		439
$R^2$		0.074		0.090
Firm controls		Yes		Yes
Industry FE		Yes		Yes

Notes: Expected duration categories come from the survey question on how long the adverse conditions were expected to last. Base category is “Under 1 year”. Sample restricted to firms reporting a revenue reduction. Specifications use entropy-balancing weights and robust standard errors. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table L.3: Retention Motives and Pre-Shock Firm Characteristics (2019)

	Agree: Skill retention motive		Agree: Rehiring-difficulty motive	
	Coef.	SE	Coef.	SE
Share of high-skilled workers, 2019	0.009	(0.011)	0.000	(0.021)
Labor market tightness, 2019	-0.006	(0.016)	-0.007	(0.020)
Moderate shock ([-35%, -5%])	-0.008	(0.026)	-0.039	(0.041)
Severe shock (<-35%)	-0.027	(0.047)	-0.081	(0.078)
Observations		605		605
$R^2$		0.036		0.089
Firm controls		Yes		Yes
Industry FE		Yes		Yes

Notes: Pre-shock firm characteristics are measured in 2019 and standardized, e.g., expressed in standard deviations. Sample restricted to firms reporting a revenue reduction. Specifications use entropy-balancing weights and robust standard errors. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## M Hypothetical Pay-Cut Threshold, Realized Pay Adjustments, and the Eligibility Bound

This appendix provides variable definitions, regression specifications, and supporting tables and figures for the analysis in Section 7.2 of the hypothetical job-saving pay-cut threshold and its relationship to realized pay adjustments.

**Pay-cut intensity index.** We construct a pay-cut intensity index from the survey (Q13) that combines the depth of the cut with the share of workers affected, separately for base pay and bonus pay:

$$\text{base\_int}_i = \frac{\text{base cut}\%_i}{100} \times \frac{\text{base affected}\%_i}{100}, \quad (\text{M.1})$$

$$\text{bonus\_int}_i = \frac{\text{bonus cut}\%_i}{100} \times \frac{\text{bonus affected}\%_i}{100},$$

$$\text{pay\_int}_i = \min\{1, \text{base\_int}_i + \text{bonus\_int}_i\}, \quad (\text{M.2})$$

$$\text{any\_paycut}_i = \mathbf{1}\{\text{any base-pay or bonus-pay reduction}\}.$$

If the firm did not use a particular pay-reduction method, the corresponding intensity is set to zero. The index is a descriptive measure of realized pay-cut activity; it is not an exact payroll-share object because the survey does not separately identify the bonus share in total compensation.

**Eligibility upper bound.** To assess the share of layoff cases for which a job-saving pay-cut counterfactual is conceptually relevant, we define an eligibility indicator that requires simultaneously that layoffs are financially motivated, that the firm does not reject wage reductions as job-saving, and that it provides a quantifiable threshold:

$$\begin{aligned} \text{eligible\_upper}_i &\equiv \mathbf{1}\{\text{layoff\_firm}_i = 1\} \times \mathbf{1}\{\text{layoff\_financial}_i = 1\} \\ &\times \mathbf{1}\{\text{not\_agree\_nosave}_i = 1\} \times \mathbf{1}\{\text{savecut\_cat}_i \in \{0-20, 21-40, 41-60, 61-100\}\} \end{aligned} \quad (\text{M.3})$$

where  $\text{layoff\_financial}_i = 1$  if the firm attributes layoffs to reduced sales or financial difficulties;  $\text{not\_agree\_nosave}_i = 1$  if the firm does *not* agree or strongly agree with the statement “wage reductions would not have saved jobs” (Section 5.2; Figure 9); and the final term requires a numeric threshold rather than “Do not know.” The generous definition treats firms that neither agree nor disagree as potentially open to substitution; the strict definition requires active disagreement.

**Regression specification.** To assess the conditional relationship between the hypothetical threshold and realized pay adjustments, we estimate firm-level OLS regressions with the 0-20% threshold group as the omitted category. Let  $\mathcal{K} \equiv \{21-40, 41-60, 61-100, DK\}$

denote the set of non-reference threshold categories. We estimate:

$$\begin{aligned} \text{pay\_int}_i &= \alpha + \sum_{k \in \mathcal{K}} \beta_k \cdot \mathbf{1}\{\text{savecut\_cat}_i = k\} \\ &\quad + \Gamma(\text{shock3}_i) + \lambda \ln(\text{emp2019}_i) + \mu_{s(i)} + X_i' \kappa + \varepsilon_i, \end{aligned} \quad (\text{M.4})$$

$$\begin{aligned} \text{any\_paycut}_i &= \alpha + \sum_{k \in \mathcal{K}} \beta_k \cdot \mathbf{1}\{\text{savecut\_cat}_i = k\} \\ &\quad + \Gamma(\text{shock3}_i) + \lambda \ln(\text{emp2019}_i) + \mu_{s(i)} + X_i' \kappa + u_i, \end{aligned} \quad (\text{M.5})$$

$$\begin{aligned} \text{admin\_layoff\_rate}_i &= \alpha + \sum_{k \in \mathcal{K}} \delta_k \cdot \mathbf{1}\{\text{savecut\_cat}_i = k\} \\ &\quad + \Gamma(\text{shock3}_i) + \lambda \ln(\text{emp2019}_i) + \mu_{s(i)} + X_i' \kappa + \eta_i, \end{aligned} \quad (\text{M.6})$$

where  $\Gamma(\text{shock3}_i)$  denotes dummies for the three-category revenue shock (mild, moderate, severe),  $\mu_{s(i)}$  are industry fixed effects, and  $X_i$  includes firm controls. The coefficient  $\beta_{DK}$  measures the difference between “Do not know” and the 0-20% group conditional on shock severity and firm observables. Under the canonical substitution view, we expect  $\beta_{DK} < 0$  for pay-cut intensity (“Do not know” firms should cut less) and/or  $\delta_{DK} > 0$  for layoff rates (they should lay off more). Under our interpretation,  $\beta_{DK}$  should be near zero: both groups are at a floor of minimal pay-cut activity.

To test whether “Do not know” responses reflect state-dependent differences, we also estimate a specification interacting the DK indicator with shock severity:

$$\text{pay\_int}_i = \dots + \beta_{DK} \mathbf{1}\{DK_i\} + \delta_1 (\mathbf{1}\{DK_i\} \times \text{Moderate}_i) + \delta_2 (\mathbf{1}\{DK_i\} \times \text{Severe}_i) + \dots \quad (\text{M.7})$$

Columns 4-6 of Table M.3 restrict the sample to eligible firms (generous definition).

Table M.1: Upper Bound on the Share of Layoff Cases for Which the Pay-Cut Substitution Counterfactual Is Conceptually Relevant

	Firm-weighted	Job-weighted
<i>Eligible share among layoff firms</i>		
Generous upper bound	0.226	0.231
Strict upper bound	0.046	0.051

Note: The generous upper bound counts firms that neither agree nor disagree with the statement “wage reductions would not have saved jobs” as potentially open to substitution. The strict upper bound requires active disagreement. Both definitions require that layoffs are financially motivated, that the firm does not reject pay cuts as job-saving, and that it provides a numeric threshold (excluding “Do not know”). Sample: firms that laid off workers in 2020. Weighted by entropy-balancing weights.

**Scatter plot: hypothetical threshold vs. realized pay-cut intensity.** Figure M.1 plots the weighted mean of realized pay-cut intensity against the midpoint of each hypothetical-threshold category. The figure displays both total pay-cut intensity (`pay_int`, combining

Table M.2: Mean Realized Pay Adjustments by Hypothetical Job-Saving Pay-Cut Threshold

Hypothetical threshold	Pay-cut intensity	Base-cut intensity	Bonus-cut intensity	Any cut	Firms
<i>Panel A: All layoff firms</i>					
0-20%	0.067	0.023	0.044	0.242	185
21-40%	0.103	0.037	0.066	0.387	136
41-60%	0.292	0.087	0.226	0.598	51
61-100%	0.279	0.129	0.211	0.536	36
Do not know	0.050	0.019	0.033	0.150	638
<i>Panel B: Financially motivated layoff firms</i>					
0-20%	0.101	0.036	0.064	0.339	83
21-40%	0.117	0.043	0.076	0.420	102
41-60%	0.374	0.097	0.309	0.721	36
61-100%	0.353	0.159	0.273	0.548	28
Do not know	0.078	0.021	0.058	0.238	201

Note: Pay-cut intensity is defined in equation (M.2). “Any cut” is an indicator for any base-pay or bonus-pay reduction. Panel B restricts to firms attributing layoffs to reduced sales or financial difficulties. Weighted by entropy-balancing weights. Sample: firms that laid off workers in 2020.

base-pay and bonus reductions) and base-cut intensity separately. The decomposition shows that much of the realized pay-cut activity at higher thresholds is driven by bonus reductions rather than base-pay cuts, consistent with base wages being more resistant to downward adjustment than variable pay components.

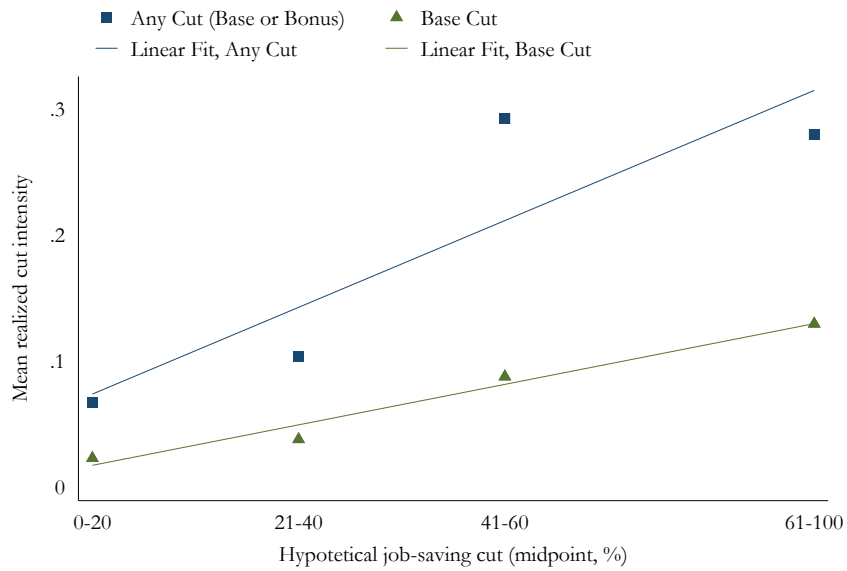


Figure M.1: Hypothetical Job-Saving Pay-Cut Threshold vs. Realized Pay-Cut Intensity

Note: Each point represents the weighted mean of realized pay-cut intensity (total and base-cut separately) for a hypothetical-threshold category (midpoints: 10, 30, 50, 80%). The line is a fitted linear relationship through the four category means. Pearson correlation (firm-level, weighted):  $r = 0.274$ ,  $p < 0.001$ . “Do not know” firms are excluded from the scatter plot but included in the regression analysis (Table M.3). Sample: firms that laid off workers in 2020.

Table M.3: Hypothetical Pay-Cut Threshold and Realized Wage and Layoff Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Cut int.	Base int.	Any cut	Cut int. (el.)	Base int. (el.)	Any cut (el.)	Layoff	Cut int.	Base int.
0-20%	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)		
21-40%	0.010 (0.029)	0.020 (0.016)	0.095 (0.062)	0.062 (0.047)	0.058* (0.032)	0.155* (0.087)	0.020 (0.016)		
41-60%	0.224*** (0.067)	0.054* (0.030)	0.389*** (0.085)	0.324*** (0.085)	0.082** (0.039)	0.493*** (0.107)	0.008 (0.017)		
61-100%	0.207*** (0.078)	0.112** (0.055)	0.262*** (0.101)	0.296*** (0.103)	0.173** (0.081)	0.321** (0.140)	0.031 (0.026)		
Do not know	-0.020 (0.024)	0.003 (0.011)	-0.073* (0.044)				0.008 (0.010)		
Mild shock	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Moderate shock	-0.040 (0.026)	-0.004 (0.013)	0.091** (0.046)	-0.057 (0.075)	0.052 (0.033)	0.175 (0.136)	-0.001 (0.011)	-0.064 (0.045)	0.006 (0.015)
Severe shock	-0.118* (0.069)	0.005 (0.049)	-0.069 (0.116)	-0.051 (0.164)	0.119 (0.108)	0.229 (0.257)	-0.037 (0.027)	-0.077 (0.096)	0.029 (0.062)
Do not know = 1								-0.078* (0.042)	-0.005 (0.007)
Do not know = 1 × Moderate								0.032 (0.047)	-0.015 (0.012)
Do not know = 1 × Severe								-0.086 (0.076)	-0.037 (0.045)
Observations	686	686	686	187	187	187	685	685	685
$R^2$	0.185	0.148	0.205	0.325	0.231	0.235	0.062	0.140	0.126
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Eligible firm sample	No	No	No	Yes	Yes	Yes	No	No	No
DK × shock interactions	No	No	No	No	No	No	No	Yes	Yes

Note: Omitted threshold: 0-20%. Columns 1-3: equations (M.4)-(M.5), all layoff firms. Columns 4-6: eligible firms (generous definition, eq. (M.3)). Column 7: eq. (M.6). Columns 8-9: eq. (M.7). Strict eligibility omitted ( $N = 37$ ). Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Entropy-balancing weights.

## N Administrative Wage Changes for Stayers in Layoff and Non-Layoff Firms

We link surveyed firms to administrative employer-employee records and restrict attention to workers who remain employed at the same firm in both the pre-period (2019) and the pandemic period (2020), which we refer to as stayers. We exclude workers who separate from the firm during the pandemic period so that outcomes capture wage adjustments for the remaining workforce rather than compositional changes due to separations.

For each stayer  $j$  in firm  $i$ , we compute the percent change in the contractual hourly base wage (including fixed contractual supplements) between 2019 and 2020,

$$\Delta w_{ij} \equiv \frac{w_{ij,2020}^{\text{base}} - w_{ij,2019}^{\text{base}}}{w_{ij,2019}^{\text{base}}}.$$

We define three mutually exclusive indicators:

$$\text{Cut}_{ij} = \mathbf{1}\{\Delta w_{ij} < -0.02\}, \text{Freeze}_{ij} = \mathbf{1}\{-0.02 \leq \Delta w_{ij} \leq 0.02\}, \text{NonInc}_{ij} = \mathbf{1}\{\Delta w_{ij} \leq 0.02\},$$

where  $\text{NonInc}_{ij}$  captures a wage cut or a wage freeze.

We define  $\text{Layoff}_i$  as an indicator that firm  $i$  conducts layoffs during the pandemic period. Our baseline layoff definition uses the linked survey-administrative overlap so that “layoff firms” correspond to firms that both report layoffs in the survey and are classified as having layoffs in the registers.<sup>55</sup> We measure firm revenue growth between 2019 and 2020 and discretize it into bins. In our baseline specification we use five bins:  $< -35\%$ ,  $[-35, -20)\%$ ,  $[-20, -5)\%$ ,  $[-5, 5]\%$ ,  $> 5\%$ . We also report a 7-bin robustness.

We estimate worker-level models of the form

$$\Pr(y_{ij} = 1) = G\left(\alpha + \sum_{b \neq b_0} \gamma_b \mathbf{1}\{\text{RevBin}_i = b\} + \sum_b \delta_b (\mathbf{1}\{\text{RevBin}_i = b\} \times \text{Layoff}_i) + X'_{ij}\beta + \mu_{s(i)}\right), \quad (\text{N.1})$$

where  $y_{ij} \in \{\text{Cut}_{ij}, \text{Freeze}_{ij}, \text{NonInc}_{ij}\}$ ,  $b_0$  denotes the reference revenue bin (neutral growth,  $[-5, 5]\%$ ),  $G(\cdot)$  is a probit link,  $X_{ij}$  includes the baseline set of worker- and firm-level controls

<sup>55</sup>To construct the data set for the analysis, we merge (via firm identifiers) our firm survey data with the firm-worker administrative data (BFL), to obtain individual-level wage information, which we then merge with individual-level IDAP register, which contains information on individual’s labor force status, and with unemployment records data IND to identify which workers were laid off. First, we identify workers with some non-negligible attachment to their employer—those who worked for their employer at least 74 hours per month (e.g., roughly 2 weeks) and with more than 3 months of work in 2019 and more than 2 in 2020). We then define stayers as workers with an active employment spell at firm  $i$  in both 2019 and 2020. In the administrative data, we classify a firm as experiencing layoffs if the firm laid off more than 2% of its workforce or at least 25 employees. We identify a layoff in the administrative data if an individual is employed at the firm in 2019 and their labor force status is listed as unemployed in IDAP in 2020 or 2021 or if the individual received unemployment insurance benefits (arblhu) or cash assistance for subsistence (KONTANTHJ) in 2020 or 2021 (to account for severance payment) as listed in IND. We have 586 firms that reported layoffs both in the survey and which we also classified as having implemented layoffs in the administrative data (out of 664 firms that reported layoffs in the survey).

used elsewhere in the paper, and  $\mu_{s(i)}$  are industry fixed effects. Standard errors are clustered at the firm level.

The interaction coefficients  $\delta_b$  are the key objects: they measure the within-revenue-bin difference in the likelihood of a wage cut/freeze (for stayers) between layoff and non-layoff firms. Because the probit is a nonlinear model, we report within-bin contrasts—differences in predicted probabilities between layoff and non-layoff firms evaluated at the same covariate values—rather than the latent-index coefficients. These contrasts are directly interpretable as percentage-point differences in the probability of each wage outcome.

Our baseline wage measure comes from the Lønstatistikken (LONN), a mandatory firm survey that separately identifies the contractual hourly base wage and fixed supplements. LONN isolates the exact object of interest for base-wage rigidity: the contractual rate set by the firm, stripped of overtime, bonuses, and other variable components. We also estimate all specifications using total hourly earnings from the administrative e-Indkomst register (BFL), which covers all firms but conflates the base-wage decision with variable pay components. The BFL contrasts are directionally consistent with the LONN results but noisier, as expected from the additional measurement error introduced by non-base-wage components.

Figure N.1 plots the worker-level distribution of base-wage changes for stayers in the full sample ( $N = 52,507$ ) and in revenue-decline firms ( $N = 16,817$ ). Wage freezes are the dominant form of downward adjustment (47% and 44%, respectively); outright cuts account for roughly a quarter of stayers (24.9% and 27.2%). Figure N.2 compares the firm-level distribution of the share of stayers with base-wage non-increases across layoff and non-layoff firms; the two distributions largely overlap, indicating that layoff firms do not systematically exhibit more stayer wage adjustment.

Table N.1 reports the number of firms and stayers by revenue-growth bin and layoff status. Table N.2 reports the baseline 5-bin contrasts. Table N.3 reports the 7-bin robustness.

As an additional robustness check, we replace the layoff dummy with layoff-intensity categories based on the share of the workforce laid off (No layoffs  $\leq 2\%$ ; Low 2-5%; Medium 5-10%; High  $> 10\%$ ) and interact intensity with revenue-shock states. Table N.4 reports the selected intensity interactions.

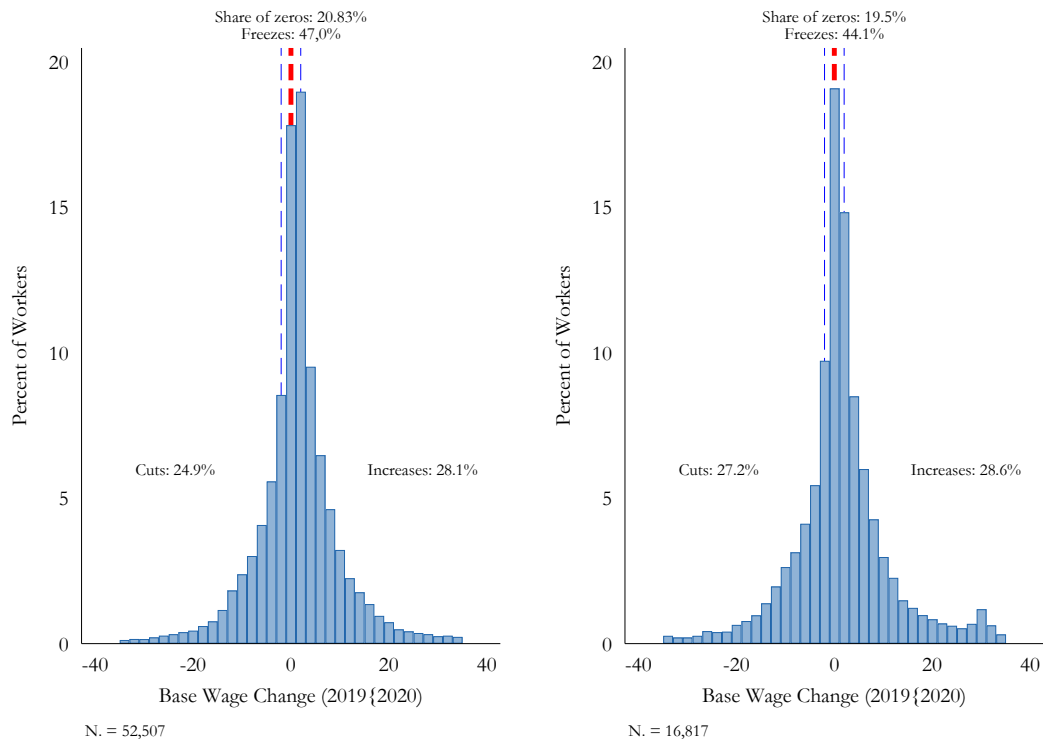


Figure N.1: Distribution of Base Wage Changes among Job Stayers

Note: LONN data, 2019-2020 base wage changes for job stayers. Left panel: full sample ( $N = 52,507$ ). Right panel: revenue-decline firms ( $N = 16,817$ ). Dashed lines mark zero. Shares of cuts ( $\Delta w < -2\%$ ), freezes ( $|\Delta w| \leq 2\%$ ), and increases ( $\Delta w > 2\%$ ) are annotated.

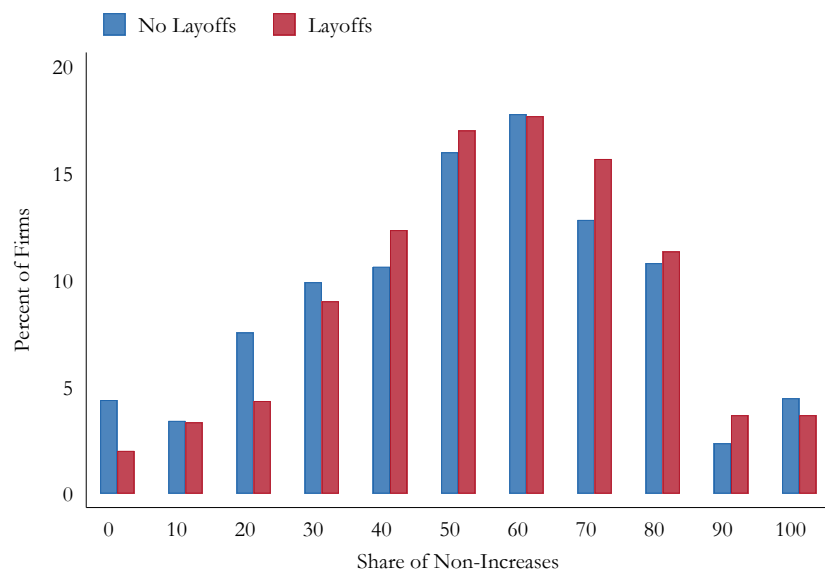


Figure N.2: Firm-Level Share of Base-Wage Non-Increases by Layoff Status

Note: LONN data. For each firm, the share of job stayers with a base-wage non-increase ( $\Delta w \leq 2\%$ ) is computed. The histogram compares firms that conducted layoffs to firms that did not. The two distributions largely overlap.

Table N.1: Sample Composition: Firms and Workers by Revenue Growth and Layoff Status

Revenue Bin	Number of Firms		Number of Workers		Avg. Firm Size		Total Firms
	No Layoff	Layoff	No Layoff	Layoff	No Layoff	Layoff	
< -35%	128	103	492	266	3.8	2.6	231
[-35, -20)%	173	84	950	348	5.5	4.1	257
[-20, -5)%	486	184	3,820	3,008	7.9	16.3	670
[-5, 5)%	547	94	6,175	2,283	11.3	24.3	641
(5, 20)%	585	62	3,637	819	6.2	13.2	647
(20, 35)%	241	28	739	119	3.1	4.3	269
> 35%	243	24	686	447	2.8	18.6	267
Missing	18	7	-	-	-	-	25
Total	2,421	586	16,499	7,290	6.8	12.4	3,007

Note: LONN data. Stayers are workers employed at the same firm in both 2019 and 2020. Layoff status uses the linked survey-administrative definition.

Table N.2: Within-Bin Contrasts: Layoff vs. No-Layoff Firms — Five-Bin Specification

Layoff Dummy	Shock bin	(1) Wage cut	(2) Wage freeze	(3) Cut or freeze	(4) Wage cut <sub>r</sub>	(5) Wage freeze <sub>r</sub>	(6) Cut or freeze <sub>r</sub>
<i>Specification A — Layoff Dummy = 1</i>							
	Bin 1	0.046** [0.012, 0.080]	-0.100*** [-0.132, -0.067]	-0.062*** [-0.097, -0.028]	0.036 [-0.000, 0.073]	-0.063*** [-0.098, -0.027]	-0.032 [-0.069, 0.006]
	Bin 2	0.114*** [0.078, 0.149]	-0.042* [-0.077, -0.007]	0.077*** [0.040, 0.114]	0.132*** [0.096, 0.168]	-0.057** [-0.092, -0.022]	0.078*** [0.040, 0.115]
	Bin 3	0.005 [-0.010, 0.019]	-0.004 [-0.019, 0.011]	0.005 [-0.011, 0.021]	0.005 [-0.011, 0.021]	0.013 [-0.005, 0.030]	0.021* [0.003, 0.038]
	Bin 4	0.054*** [0.040, 0.069]	0.011 [-0.005, 0.028]	0.067*** [0.051, 0.084]			
	Bin 5	0.011 [-0.007, 0.029]	-0.039*** [-0.057, -0.022]	-0.034*** [-0.054, -0.015]			
<i>Using Firm Equal Weight — Layoff Dummy = 1</i>							
	Bin 1	-0.024 [-0.135, 0.088]	0.020 [-0.076, 0.116]	-0.002 [-0.093, 0.089]	-0.039 [-0.149, 0.072]	0.025 [-0.072, 0.122]	-0.010 [-0.101, 0.081]
	Bin 2	0.089 [-0.010, 0.187]	-0.012 [-0.102, 0.077]	0.074 [-0.014, 0.161]	0.091 [-0.006, 0.188]	-0.017 [-0.107, 0.072]	0.073 [-0.016, 0.161]
	Bin 3	0.009 [-0.043, 0.060]	0.016 [-0.034, 0.066]	0.024 [-0.027, 0.076]	-0.001 [-0.053, 0.051]	0.028 [-0.024, 0.080]	0.026 [-0.026, 0.079]
	Bin 4	-0.001 [-0.056, 0.055]	-0.008 [-0.058, 0.042]	-0.011 [-0.073, 0.050]			
	Bin 5	0.033 [-0.025, 0.090]	-0.017 [-0.069, 0.036]	0.016 [-0.050, 0.082]			
<i>N</i>		68,663	68,663	68,663	25,971	25,971	25,971
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	
Clustered SE (firm)	Yes	Yes	Yes	Yes	Yes	Yes	
Rev. reduction sample	No	No	No	Yes	Yes	Yes	

Note: LONN data. Contrasts are differences in predicted probabilities (layoff minus no-layoff) within each revenue bin, from worker-level probit models. 95% confidence intervals in brackets. Columns (4)-(6) restrict to firms with revenue declines. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Table N.3: Within-Bin Contrasts: Layoff vs. No-Layoff Firms — Seven-Bin Specification

Layoff Dummy	Shock bin	(1) Wage cut	(2) Wage freeze	(3) Cut or freeze	(4) Wage cut <sub>r</sub>	(5) Wage freeze <sub>r</sub>	(6) Cut or freeze <sub>r</sub>
<i>Specification A — Layoff Dummy = 1</i>							
	< -35	0.040 [-0.050, 0.131]	-0.100 [-0.218, 0.017]	-0.067 [-0.145, 0.011]	0.036 [-0.046, 0.118]	-0.063 [-0.170, 0.044]	-0.032 [-0.116, 0.053]
	[-35, -20)	0.113* [0.020, 0.207]	-0.041 [-0.126, 0.044]	0.078 [-0.018, 0.173]	0.132** [0.039, 0.225]	-0.057 [-0.139, 0.024]	0.078 [-0.022, 0.177]
	[-20, -5)	-0.001 [-0.054, 0.053]	-0.004 [-0.058, 0.050]	0.000 [-0.058, 0.058]	0.005 [-0.044, 0.054]	0.013 [-0.039, 0.064]	0.021 [-0.032, 0.073]
	(5, 20]	0.053 [-0.015, 0.121]	0.012 [-0.046, 0.071]	0.066* [0.002, 0.130]			
	(20, 35]	-0.029 [-0.082, 0.024]	-0.046 [-0.100, 0.008]	-0.082** [-0.142, -0.022]			
	> 35	0.179* [0.010, 0.349]	-0.016 [-0.263, 0.231]	0.157** [0.049, 0.265]			
<i>Using Firm Equal Weight — Layoff Dummy = 1</i>							
	< -35	-0.023 [-0.135, 0.088]	0.020 [-0.076, 0.116]	-0.001 [-0.092, 0.090]	-0.039 [-0.149, 0.072]	0.025 [-0.072, 0.122]	-0.010 [-0.101, 0.081]
	[-35, -20)	0.088 [-0.010, 0.187]	-0.012 [-0.101, 0.077]	0.073 [-0.014, 0.161]	0.091 [-0.006, 0.188]	-0.017 [-0.107, 0.072]	0.073 [-0.016, 0.161]
	[-20, -5)	0.009 [-0.042, 0.060]	0.016 [-0.034, 0.066]	0.025 [-0.027, 0.076]	-0.001 [-0.053, 0.051]	0.028 [-0.024, 0.080]	0.026 [-0.026, 0.079]
	(5, 20]	0.000 [-0.056, 0.055]	-0.007 [-0.058, 0.043]	-0.010 [-0.072, 0.051]			
	(20, 35]	-0.018 [-0.077, 0.042]	0.016 [-0.050, 0.082]	-0.002 [-0.083, 0.079]			
	> 35 (bin 6)	0.030 [-0.081, 0.140]	-0.046 [-0.154, 0.062]	-0.014 [-0.159, 0.132]			
	> 35 (bin 7)	0.216* [0.021, 0.411]	-0.078 [-0.226, 0.069]	0.128 [-0.066, 0.323]			
<i>N</i>		68,449	68,449	68,449	25,971	25,971	25,971
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	
Clustered SE (firm)	Yes	Yes	Yes	Yes	Yes	Yes	
Rev. reduction sample	No	No	No	Yes	Yes	Yes	

Note: LONN data. See notes to Table N.2. The reference bin is  $[-5, 5]\%$ . \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Table N.4: Probit Coefficients — Shock Bins  $\times$  Layoff Intensity (Seven-Bin)

	(1) Wage cut	(2) Wage freeze	(3) Cut or freeze
<i>Shock growth rate bins (omitted: [-5, 5]%)</i>			
< -35	0.543*** (0.11)	-0.085 (0.14)	0.359*** (0.09)
[-35, -20)	0.187* (0.10)	-0.061 (0.08)	0.090 (0.09)
[-20, -5)	0.143* (0.08)	-0.048 (0.05)	0.052 (0.07)
(5, 20]	-0.019 (0.08)	-0.020 (0.07)	-0.032 (0.08)
(20, 35]	0.227 (0.20)	-0.273*** (0.10)	-0.061 (0.17)
> 35	0.198 (0.13)	-0.090 (0.11)	0.069 (0.11)
<i>Layoff intensity (omitted: Low)</i>			
Medium	0.141 (0.10)	0.037 (0.09)	0.149* (0.09)
High	0.159 (0.19)	-0.157* (0.08)	-0.021 (0.16)
<i>Shock <math>\times</math> layoff intensity interactions</i>			
< -35 $\times$ Medium	-0.120 (0.19)	-0.179 (0.20)	-0.225 (0.14)
< -35 $\times$ High	-0.599** (0.28)	0.098 (0.24)	-0.395* (0.21)
[-35, -20) $\times$ Medium	0.200 (0.19)	-0.149 (0.18)	0.060 (0.17)
[-35, -20) $\times$ High	-0.068 (0.25)	0.160 (0.15)	0.111 (0.21)
[-20, -5) $\times$ Medium	-0.132 (0.14)	-0.068 (0.13)	-0.155 (0.11)
[-20, -5) $\times$ High	-0.076 (0.20)	0.147 (0.11)	0.100 (0.18)
(5, 20] $\times$ Medium	0.098 (0.13)	-0.069 (0.11)	0.020 (0.12)
(5, 20] $\times$ High	-0.057 (0.22)	0.063 (0.15)	0.014 (0.19)
(20, 35] $\times$ Medium	-0.444* (0.26)	0.139 (0.15)	-0.185 (0.21)
(20, 35] $\times$ High	-0.064 (0.33)	0.519*** (0.17)	0.420 (0.34)
> 35 $\times$ Medium	0.108 (0.28)	-0.032 (0.22)	0.081 (0.20)
> 35 $\times$ High	0.145 (0.34)	-0.427** (0.20)	-0.138 (0.26)
<i>N</i>	68,449	68,449	68,449
Industry FE	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes
Clustered SE (firm)	Yes	Yes	Yes
Rev. reduction sample	No	No	No

Note: LONN data. Probit latent-index coefficients. Layoff intensity: Low  $\leq 5\%$ ; Medium 5-10%; High  $> 10\%$  of workforce. Standard errors in parentheses, clustered at the firm level. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## O Pay Adjustment by Layoff Status in Revenue-Decline Firms

This section reports the firm-level regressions underlying Section 8.2. We restrict attention to the linked survey-administrative sample and construct firm-level pay-adjustment outcomes using job stayers (workers employed at firm  $i$  in both 2019 and 2020; same stayer concept as Appendix N). For each stayer, we compute percent changes in contractual base wages, variable pay, and total pay from LONN records; we then average across stayers within each firm. We also compute firm-level incidence measures: the share of stayers with base-wage cuts ( $\Delta w < -2\%$ ) and the share with base-wage freezes ( $|\Delta w| \leq 2\%$ ).

We classify firms as “few/no layoffs” if their administrative layoff rate (permanent layoffs in 2020 divided by 2019 employment) is below 2%. Revenue shocks are measured using administrative revenue growth 2019-2020, discretized into the same bins as Figure 1.

Appendix Table O.1 reports the results for the full sample and Appendix Table O.2 restricts to revenue-decline firms (revenue growth  $< -5\%$ ). In both tables, the few/no-layoff coefficients and interaction terms are small and statistically insignificant across all five outcomes, indicating that firms with few or no layoffs do not substitute toward larger pay reductions conditional on shock severity.

To compare survey-reported magnitudes from Question 13 to administrative outcomes, we construct a survey-implied average cut intensity for each pay-cut method as the product of the reported percent reduction and the reported percent of employees affected. Appendix Table O.3 reports the mapping between this survey-implied intensity and administrative pay adjustment. In the pooled regression (Panel B), the Q13 base-wage intensity coefficient is 0.099 ( $p < 0.01$ ). The interaction with the few/no-layoff indicator is not significant ( $-0.063$ ,  $p = 0.18$ ), indicating that the survey-administrative mapping does not differ between groups.

Table O.1: Pay Adjustment by Layoff Status and Revenue Bin — Full Sample

	(1) Base Cut	(2) Var Cut	(3) Total Cut	(4) Base Cut Share	(5) Base Freeze Share
< -35%	-0.027 (0.027)	-0.031 (0.081)	-0.021 (0.027)	0.065 (0.044)	-0.025 (0.047)
[-35, -20)	0.014 (0.019)	-0.005 (0.051)	0.017 (0.019)	0.065* (0.034)	0.007 (0.031)
[-20, -5)	0.004 (0.007)	-0.021 (0.042)	-0.003 (0.007)	0.034* (0.019)	-0.017 (0.019)
[-5, 5]	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
(5, 20]	-0.005 (0.006)	-0.076** (0.035)	-0.009 (0.007)	-0.018 (0.018)	0.012 (0.019)
(20, 35]	0.001 (0.011)	0.052 (0.085)	-0.004 (0.012)	0.017 (0.029)	-0.020 (0.029)
> 35%	0.028 (0.026)	-0.103 (0.081)	0.013 (0.027)	0.064 (0.046)	-0.039 (0.043)
Few layoffs dummy	-0.005 (0.008)	0.065 (0.063)	-0.001 (0.008)	0.007 (0.022)	0.038 (0.024)
< -35% × Few layoffs = 1	0.004 (0.020)	0.166 (0.174)	0.011 (0.024)	-0.045 (0.057)	-0.013 (0.057)
[-35, -20) × Few layoffs = 1	0.121 (0.179)	0.079 (0.194)	0.125 (0.179)	-0.045 (0.052)	-0.070 (0.049)
[-20, -5) × Few layoffs = 1	0.006 (0.011)	-0.057 (0.083)	0.008 (0.011)	-0.021 (0.033)	-0.022 (0.033)
[-5, 5] × Few layoffs = 1	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
(5, 20] × Few layoffs = 1	0.026 (0.017)	-0.073 (0.074)	0.019 (0.017)	0.039 (0.032)	-0.027 (0.034)
(20, 35] × Few layoffs = 1	0.003 (0.014)	-0.250** (0.107)	-0.009 (0.014)	-0.022 (0.043)	-0.018 (0.047)
> 35% × Few layoffs = 1	-0.039* (0.020)	-0.176** (0.082)	-0.045** (0.020)	-0.080* (0.047)	0.041 (0.045)
Observations	1,483	1,482	1,483	1,483	1,483

Notes: Dependent variables are firm-level averages across job stayers (workers employed at firm  $i$  in both 2019 and 2020). Columns (1)-(3): mean percent change in base wages, variable pay, and total pay, respectively (sign-reversed so that positive values indicate cuts). Columns (4)-(5): share of stayers with base-wage cuts ( $\Delta w < -2\%$ ) and base-wage freezes ( $|\Delta w| \leq 2\%$ ). “Few layoffs” = 1 if the firm’s administrative layoff rate is below 2% of 2019 employment. Revenue bins use administrative revenue growth 2019-2020. Omitted revenue bin: [-5, 5]. All regressions include industry fixed effects, firm controls, and entropy-balancing weights. Wage data from LONN. Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table O.2: Pay Adjustment by Layoff Status and Revenue Bin — Revenue-Decline Sample

	(1) Base Cut	(2) Var Cut	(3) Total Cut	(4) Base Cut Share	(5) Base Freeze Share
< -35%	0.009 (0.047)	0.081 (0.154)	0.023 (0.048)	0.057 (0.071)	-0.028 (0.069)
[-35, -20)	0.026 (0.021)	0.029 (0.075)	0.034 (0.022)	0.043 (0.038)	0.015 (0.034)
[-20, -5)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Few layoffs dummy	-0.006 (0.015)	-0.003 (0.053)	-0.001 (0.015)	0.013 (0.027)	0.010 (0.025)
< -35% × Few layoffs = 1	0.003 (0.025)	0.258 (0.178)	0.009 (0.029)	-0.017 (0.058)	0.011 (0.058)
[-35, -20) × Few layoffs = 1	0.112 (0.177)	0.164 (0.194)	0.115 (0.177)	-0.027 (0.056)	-0.044 (0.050)
Observations	556	556	556	556	556

Notes: Sample restricted to revenue-decline firms (revenue growth < -5%). Dependent variables and controls as in the full-sample table above. Omitted revenue bin: [-20, -5). Wage data from LONN. Robust standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table O.3: Survey Q13 vs Administrative Pay Adjustment (Revenue-Decline Sample)

	(1)	(2)	(3)	(4)
	Base   Few lay. = 1	Base   Few lay. = 0	Var   Few lay. = 1	Var   Few lay. = 0
<i>Panel A: Split-sample</i>				
Q13: Avg base wage cut	0.111 (0.098)	0.070** (0.028)		
< -35%	0.099 (0.155)	-0.033 (0.030)	0.601 (0.372)	-0.038 (0.164)
[-35, -20)	0.173 (0.208)	0.013 (0.014)	0.262 (0.233)	-0.004 (0.082)
[-20, -5)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
Observations	179	369	174	361
<i>Panel B: Pooled with interaction</i>				
	Base Wage		Variable Pay	
	(1)	(2)	(1)	(2)
Q13: Avg base wage cut	0.099*** (0.038)		0.108 (0.127)	
Few layoffs = 1	0.022 (0.033)		0.073 (0.054)	
Few layoffs = 1 × Q13: Avg base cut	-0.063 (0.047)		0.144 (0.377)	
< -35%	0.011 (0.045)		0.153 (0.158)	
[-35, -20)	0.067 (0.071)		0.082 (0.099)	
[-20, -5)	0.000 (.)		0.000 (.)	
Observations	548		548	

Note: Sample restricted to revenue-decline firms (revenue growth < -5%). Dependent variable: firm-level administrative pay cut (sign-reversed mean stayer wage change). Q13 average cut intensity = (reported % reduction) × (reported % employees affected). Panel A estimates separately by layoff group. Panel B pools both groups and includes an interaction with the few/no-layoff indicator. All regressions include revenue-bin fixed effects, industry fixed effects, firm controls, and entropy-balancing weights. Omitted revenue bin: [-20, -5). Wage data from LONN. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## P Retention of Remaining Workers after Layoffs

We link surveyed firms to administrative employer-employee records and focus on workers who remain employed at the same firm in both 2019 and 2020 (job stayers). For the retention analysis, we restrict to firms in the revenue-reduction sample.

We use annual IDAN employment records to identify whether the worker is employed at the same firm in each post-2020 year, and annual IDAP records to exclude subsequent layoffs. Let  $E_{ij,t}$  denote an indicator that worker  $j$  is employed at firm  $i$  in year  $t$  and is not classified as laid off in IDAP in that year. We define indicators for whether the worker remains employed at the firm for at least  $h$  years after 2020:

$$R_{ij}^{(h)} \equiv \mathbf{1}\{E_{ij,2020} = E_{ij,2021} = \dots = E_{ij,2020+h} = 1\}, \quad h \in \{1, 2, 3\}.$$

Thus,  $R_{ij}^{(1)} = 1$  means the worker remains employed at the firm through 2021,  $R_{ij}^{(2)} = 1$  means through 2022, and  $R_{ij}^{(3)} = 1$  means through 2023.

Let  $\Delta w_{ij}$  denote each stayer's contractual hourly base-wage change between 2019 and 2020 (including fixed contractual supplements). We classify wage changes into mutually exclusive categories using a  $\pm 2\%$  band:

$$\text{Cut}_{ij} = \mathbf{1}\{\Delta w_{ij} < -0.02\}, \quad \text{Inc}_{ij} = \mathbf{1}\{\Delta w_{ij} > 0.02\},$$

with wage freezes/no-changes ( $-0.02 \leq \Delta w_{ij} \leq 0.02$ ) as the omitted category.

To quantify the relationship between wage adjustments and subsequent retention while controlling for shock severity and other observables, we estimate worker-level linear probability models of the form

$$R_{ij}^{(h)} = \alpha_h + \beta_h^{\text{cut}} \text{Cut}_{ij} + \beta_h^{\text{inc}} \text{Inc}_{ij} + \Gamma_h(\text{Shock}_i) + X'_{ij}\theta_h + X'_i\kappa_h + \mu_{s(i)} + \varepsilon_{ij}, \quad (\text{P.1})$$

where  $\Gamma_h(\text{Shock}_i)$  is a measure of the shock (e.g., revenue-decline),  $X_{ij}$  and  $X_i$  denote the baseline worker and firm controls used elsewhere in the paper, and  $\mu_{s(i)}$  are industry fixed effects. Standard errors are clustered at the firm level.

Figure P.1 and Table P.1 summarize the descriptive retention patterns. Retention rates are nearly identical across wage-change groups: three-year retention is 66.7% for workers who received a pay cut, 70.4% for those with a freeze, and 67.5% for those with an increase.

Table P.2 reports linear probability model estimates of equation (P.1) with firm fixed effects, using LONN base-wage data. The firm fixed effects absorb  $\Gamma_h$ ,  $X_i$ , and  $\mu_{s(i)}$ , so identification comes entirely from within-firm variation in wage changes and worker characteristics. Results using BFL total-earnings data are directionally consistent.

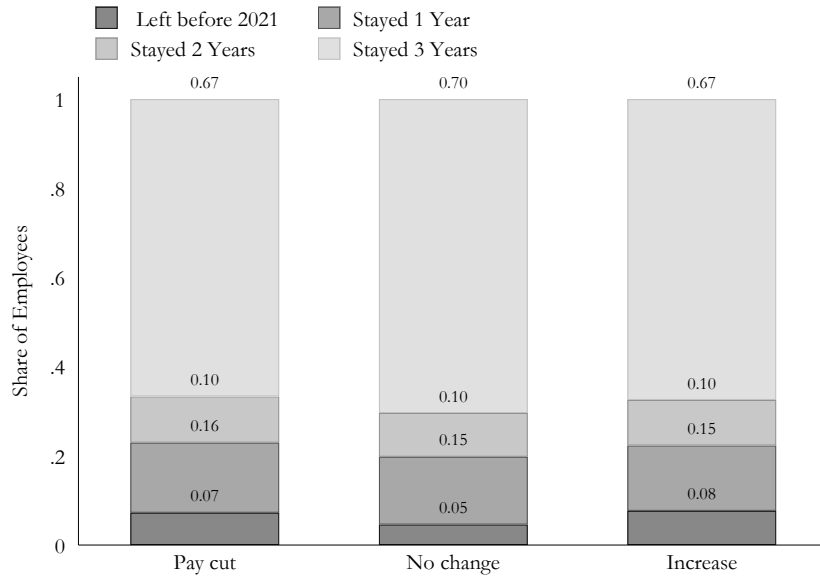


Figure P.1: Retention by Pay Adjustment (Revenue-Reduction Sample)

Note: LONN data, revenue-reduction sample ( $N = 15,417$  job stayers). Each bar shows the share of workers in each wage-change group (pay cut, no change, increase) by retention status: left before 2021, stayed 1 year (through 2021), stayed 2 years (through 2022), stayed 3 years (through 2023). Wage changes classified using a  $\pm 2\%$  band on the 2019-2020 base-wage change.

Table P.1: Employee Retention by Wage Change Category

	Cut	Freeze	Increase	Total
Left before 2021	299	244	459	1,002
Stayed 1 Year	648	807	876	2,331
Stayed 2 Years	428	521	611	1,560
Stayed 3 Years	2,751	3,737	4,036	10,524
<b>Total</b>	<b>4,126</b>	<b>5,309</b>	<b>5,982</b>	<b>15,417</b>

Note: LONN data, revenue-reduction sample. Wage changes classified using a  $\pm 2\%$  band on the 2019-2020 contractual hourly base-wage change.

Table P.2: Retention of Remaining Workers — Linear Probability Model with Firm Fixed Effects (LONN Data)

	(1) Stayed 1 year	(2) Stayed 2 years	(3) Stayed 3 years
Pay cut	−0.0007 (0.01)	0.0030 (0.01)	−0.0252 (0.02)
No change (omitted)	0.0000 (.)	0.0000 (.)	0.0000 (.)
Increase	−0.0296 (0.03)	−0.0035 (0.01)	0.0145 (0.03)
Male	−0.0021 (0.01)	−0.0033 (0.01)	0.0111 (0.01)
Employee Age	−0.0222*** (0.00)	−0.0113*** (0.00)	0.0384*** (0.00)
Employee Age <sup>2</sup>	0.0002*** (0.00)	0.0001*** (0.00)	−0.0004*** (0.00)
Tenure (Years)	−0.0037*** (0.00)	−0.0019*** (0.00)	0.0118*** (0.00)
Full Time	0.0037 (0.01)	−0.0154* (0.01)	0.0188 (0.01)
Observations	15,417	15,417	15,417
Firm FE	Yes	Yes	Yes
Rev. reduction sample	Yes	Yes	Yes

Note: LONN data, revenue-reduction sample. Linear probability model with firm fixed effects. Dependent variable: indicator for remaining at the same firm through 2021 (col. 1), 2022 (col. 2), or 2023 (col. 3). Omitted pay-change category: no change ( $\pm 2\%$  band). Standard errors in parentheses, clustered at the firm level. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

## Q Labor Market Slackness, Layoffs, and Pay Cuts

This appendix reports the full results for the test of Bewley (1999, p. 230)’s implicit-contract prediction that slack labor markets should make firms less inclined to let workers go.

### Q.1 Setup

We interact seven revenue-growth bins (as in Figure 1) with terciles of firm-specific labor-market tightness (vacancy-to-unemployment ratio, constructed as in Section 2). Tercile 1 denotes *slack* markets (low tightness) and Tercile 3 denotes *tight* markets (high tightness). We estimate linear probability models of the form:

$$y_i = \alpha + \sum_{b \neq 4} \gamma_b \mathbf{1}\{\text{RevBin}_i = b\} + \sum_{t \neq 3} \lambda_t \mathbf{1}\{\text{Tercile}_i = t\} + \sum_{b \neq 4} \sum_{t \neq 3} \delta_{bt} \mathbf{1}\{\text{RevBin}_i = b\} \times \mathbf{1}\{\text{Tercile}_i = t\} + X_i' \kappa + \mu_{s(i)} + \varepsilon_i, \quad (\text{Q.1})$$

where  $y_i$  is either an indicator for permanent layoffs or an indicator for base-wage reductions,  $X_i$  are firm controls (size, age, productivity, unionization, wage floors), and  $\mu_{s(i)}$  are industry fixed effects. The omitted revenue bin is  $[-5, 5]\%$  and the omitted tightness tercile is Tight. We estimate on two samples: the *revenue-decline sample* (firms with negative revenue growth,  $N = 1,066$ ) and the *full sample* ( $N = 2,750$ ). Heteroskedasticity-robust standard errors are used throughout. All regressions use entropy-balancing weights.

### Q.2 Results

Table Q.1 reports cell sizes by revenue bin and tightness tercile. Table Q.2 reports the full regression estimates. Table Q.3 reports predicted probabilities from the interacted specification. Table Q.4 reports Slack–Tight and Moderate–Tight differences within each revenue bin, with  $p$ -values and 95% confidence intervals.

The key finding is in the decline sample: among firms with the most severe revenue declines ( $< -35\%$ ), those in slack labor markets are 18.9 percentage points less likely to use permanent layoffs ( $p = 0.048$ ). The effect is marginally significant in the  $[-20, -5]\%$  bin ( $p = 0.086$ ). Labor-market slackness has no significant effect on base-wage reductions in any revenue bin.

Table Q.1: Cell Sizes by Revenue Bin and Tightness Tercile

Revenue Bin	Slack	Moderate	Tight	Total
$< -35\%$	76	84	63	223
$[-35, -20)$	89	79	90	258
$[-20, -5)$	189	225	235	649
$[-5, 5]$	194	232	202	628
$(5, 20]$	220	204	206	630
$(20, 35]$	91	85	84	260
$> 35\%$	93	79	89	261
Total	952	988	969	2,909

*Notes:* Tightness terciles are based on firm-specific labor-market tightness (vacancy-to-unemployment ratio). Tercile 1 = Slack (low tightness), Tercile 3 = Tight (high tightness).

Table Q.2: Labor Market Tightness and Revenue Shocks: Layoff and Wage Reduction Outcomes

	Revenue Decline Sample				Full Sample			
	(1) Layoff	(2) Layoff	(3) Wage red.	(4) Wage red.	(5) Layoff	(6) Layoff	(7) Wage red.	(8) Wage red.
< -35%	0.291*** (0.109)	0.217** (0.092)	-0.072 (0.083)	-0.032 (0.077)	0.438*** (0.079)	0.365*** (0.053)	0.082 (0.053)	0.132*** (0.040)
[-35, -20)	-0.014 (0.061)	0.053 (0.044)	-0.042 (0.045)	0.010 (0.037)	0.113** (0.055)	0.181*** (0.037)	0.049 (0.037)	0.112*** (0.029)
[-20, -5)	0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)	0.107*** (0.036)	0.119*** (0.023)	0.052** (0.023)	0.072*** (0.016)
[-5, 5]					0.000 (.)	0.000 (.)	0.000 (.)	0.000 (.)
(5, 20]					-0.054* (0.031)	-0.047** (0.020)	-0.012 (0.016)	-0.015 (0.012)
(20, 35]					-0.060 (0.042)	-0.030 (0.030)	-0.003 (0.024)	-0.004 (0.019)
> 35%					-0.082* (0.047)	-0.059 (0.045)	0.019 (0.034)	0.029 (0.033)
Slack	-0.121* (0.070)	-0.117* (0.061)	-0.033 (0.052)	0.004 (0.046)	-0.048 (0.044)	-0.031 (0.031)	0.003 (0.028)	0.008 (0.020)
Moderate	-0.033 (0.055)	-0.026 (0.047)	0.002 (0.042)	0.039 (0.037)	-0.014 (0.039)	0.001 (0.025)	-0.019 (0.022)	0.024 (0.017)
Interactions	Yes	No	Yes	No	Yes	No	Yes	No
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,066	1,066	1,066	1,066	2,750	2,750	2,750	2,750
R <sup>2</sup>	0.124	0.118	0.074	0.072	0.128	0.122	0.092	0.087

Notes: Linear probability models with entropy-balancing weights and heteroskedasticity-robust standard errors. Dependent variables: indicator for permanent layoffs (odd columns) and indicator for base-wage reductions (even columns). Omitted revenue bin: [-5, 5]%. Omitted tightness tercile: Tight. Revenue-decline sample restricts to firms with negative revenue growth. Interaction terms (RevBin × Tightness) are included in odd-numbered columns but suppressed for space; full interaction coefficients are available upon request. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table Q.3: Predicted Probabilities by Revenue Bin and Tightness Tercile

	Revenue Decline Sample		Full Sample	
	(1) Layoff	(2) Wage red.	(3) Layoff	(4) Wage red.
<i>Revenue bin: &lt; -35%</i>				
Slack	0.394*** [0.272, 0.515]	0.220*** [0.116, 0.323]	0.414*** [0.296, 0.531]	0.221*** [0.120, 0.322]
Moderate	0.421*** [0.302, 0.539]	0.236*** [0.137, 0.336]	0.427*** [0.312, 0.543]	0.246*** [0.148, 0.343]
Tight	0.582*** [0.452, 0.713]	0.182*** [0.082, 0.282]	0.550*** [0.422, 0.679]	0.168*** [0.075, 0.262]
<i>Revenue bin: [-35, -20)</i>				
Slack	0.233*** [0.129, 0.337]	0.167*** [0.079, 0.254]	0.249*** [0.154, 0.345]	0.173*** [0.090, 0.257]
Moderate	0.394*** [0.279, 0.508]	0.239*** [0.144, 0.335]	0.410*** [0.299, 0.521]	0.247*** [0.151, 0.343]
Tight	0.284*** [0.181, 0.388]	0.136*** [0.063, 0.208]	0.254*** [0.161, 0.347]	0.122*** [0.058, 0.186]
<i>Revenue bin: [-20, -5)</i>				
Slack	0.170*** [0.091, 0.249]	0.098** [0.036, 0.160]	0.192*** [0.129, 0.254]	0.112*** [0.062, 0.163]
Moderate	0.258*** [0.198, 0.317]	0.133*** [0.084, 0.183]	0.281*** [0.222, 0.341]	0.146*** [0.097, 0.194]
Tight	0.291*** [0.211, 0.371]	0.131*** [0.074, 0.189]	0.252*** [0.193, 0.311]	0.109*** [0.068, 0.149]
<i>N</i>	1,066	1,066	2,750	2,750

*Notes:* Predicted probabilities from the interacted specification in Table Q.2 (columns 1, 3, 5, 7), computed using Stata `margins`. 95% confidence intervals in brackets. Decline-sample columns focus on negative revenue-growth bins. Full-sample columns additionally include non-negative bins (available upon request). \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ .

Table Q.4: Slack–Tight and Moderate–Tight Differences in Predicted Layoff and Wage-Reduction Probabilities

Outcome	Comparison	Revenue Bin	Coef.	S.E.	<i>p</i> -value	95% CI
<i>Panel A: Permanent Layoffs — Revenue Decline Sample</i>						
Layoff	Slack vs. Tight	< -35%	-0.189	0.095	0.048	[-0.376, -0.002]
Layoff	Moderate vs. Tight	< -35%	-0.162	0.091	0.077	[-0.341, 0.018]
Layoff	Slack vs. Tight	[-35, -20)	-0.051	0.081	0.529	[-0.211, 0.108]
Layoff	Moderate vs. Tight	[-35, -20)	0.109	0.081	0.176	[-0.049, 0.268]
Layoff	Slack vs. Tight	[-20, -5)	-0.121	0.070	0.086	[-0.259, 0.017]
Layoff	Moderate vs. Tight	[-20, -5)	-0.033	0.055	0.546	[-0.141, 0.075]
<i>Panel B: Base-Wage Reductions — Revenue Decline Sample</i>						
Wage red.	Slack vs. Tight	< -35%	0.038	0.077	0.626	[-0.114, 0.189]
Wage red.	Moderate vs. Tight	< -35%	0.054	0.072	0.455	[-0.088, 0.196]
Wage red.	Slack vs. Tight	[-35, -20)	0.031	0.062	0.617	[-0.091, 0.153]
Wage red.	Moderate vs. Tight	[-35, -20)	0.104	0.062	0.094	[-0.018, 0.225]
Wage red.	Slack vs. Tight	[-20, -5)	-0.033	0.052	0.525	[-0.136, 0.069]
Wage red.	Moderate vs. Tight	[-20, -5)	0.002	0.042	0.962	[-0.080, 0.084]
<i>Panel C: Permanent Layoffs — Full Sample</i>						
Layoff	Slack vs. Tight	< -35%	-0.137	0.090	0.129	[-0.313, 0.040]
Layoff	Moderate vs. Tight	< -35%	-0.123	0.089	0.166	[-0.297, 0.051]
Layoff	Slack vs. Tight	[-35, -20)	-0.005	0.070	0.940	[-0.142, 0.131]
Layoff	Moderate vs. Tight	[-35, -20)	0.155	0.075	0.037	[ 0.009, 0.302]
Layoff	Slack vs. Tight	[-20, -5)	-0.060	0.048	0.211	[-0.155, 0.034]
Layoff	Moderate vs. Tight	[-20, -5)	0.030	0.044	0.503	[-0.057, 0.116]
<i>Panel D: Base-Wage Reductions — Full Sample</i>						
Wage red.	Slack vs. Tight	< -35%	0.053	0.071	0.457	[-0.086, 0.192]
Wage red.	Moderate vs. Tight	< -35%	0.078	0.069	0.260	[-0.057, 0.212]
Wage red.	Slack vs. Tight	[-35, -20)	0.051	0.054	0.341	[-0.055, 0.157]
Wage red.	Moderate vs. Tight	[-35, -20)	0.125	0.059	0.034	[ 0.009, 0.241]
Wage red.	Slack vs. Tight	[-20, -5)	0.003	0.035	0.927	[-0.066, 0.072]
Wage red.	Moderate vs. Tight	[-20, -5)	0.037	0.033	0.266	[-0.028, 0.101]

*Notes:* Differences in predicted probabilities from the interacted LPM in Table Q.2. “Slack vs. Tight” reports  $\hat{\Pr}(y = 1 \mid \text{Slack}) - \hat{\Pr}(y = 1 \mid \text{Tight})$  within each revenue bin. Heteroskedasticity-robust standard errors; 95% confidence intervals in brackets.

## R Do Stated Concerns About Pay Cuts Predict Pay-Cut Avoidance and Employment Stability?

This exercise studies whether firms that report worrying about the adverse consequences of pay reductions cut pay less often, and whether employment is less stable in such firms, linking survey responses about motives to realized adjustment behavior using the survey-administrative link.

### R.1 Key measurement detail: two worry measures and two subsamples (driven by survey skip logic)

The survey batteries that elicit concerns about morale, quits, and productivity are asked under different conditions. As a result, the relevant worry measure differs across subsamples:

- Layoff-firm worry measure (available for layoff firms). For firms that report layoffs, the survey asks why wages were not reduced instead of layoffs. From this battery we construct a layoff-firm worry index based on items capturing concerns about adverse consequences of wage reductions (morale, quits, and productivity-type concerns). This measure is not mechanically restricted to firms that did not cut wages, so it can be related to the extensive margin of wage cutting within the layoff-firm subsample. The estimation sample includes 1,004 firms with non-missing responses, of which 260 strongly agree and 427 agree. In the revenue-decline subsample, this corresponds to 527 firms (120 strongly agree and 240 agree). The intersection with LONN wage data reduces the estimation sample for administrative outcomes, but the layoff-rate specification retains better overlap.
- Base-pay worry measure (available for non-cutters in the revenue-decline sample). Separately, for firms reporting a revenue decline that did not report base-wage reductions, the survey asks reasons for not lowering base pay. From this battery we construct a base-pay worry index using analogous morale, quits, and productivity-type concerns. Because this battery is asked only of firms that did not report base-wage reductions, it cannot be used to explain the extensive margin “did the firm cut base pay” in that subsample; it can be used to relate concerns to administrative realized outcomes and to employment outcomes within that sample. This battery yields 648 firms with non-missing responses in the full cross-section and 573 in the revenue-decline subsample. The intersection with LONN wage data reduces the estimation sample: columns 1-2 of Table R.2 use 274 and 161 firms, respectively, while the layoff-rate specification retains 517 firms because it does not require individual wage records.

For the layoff-firm worry measure, the relevant battery contains a single item capturing morale and productivity concerns (“pay reductions hurt morale/productivity more than layoffs”), so the measure is binary:

$$W_i^{\text{layoff-q}} \in \{0, 1\},$$

with Panel A using the Strongly Agree threshold and Panel B using Agree or Strongly Agree.

For the base-pay worry measure, we construct firm-level indices as follows. A natural starting point is a binary indicator equal to one if the firm agrees or strongly agrees on any of the three items. However, most firms endorse at least one item, leaving too little variation for regression. We therefore use the row mean of item-level indicators, which takes values 0, 1/3, 2/3, or 1 depending on how many items the firm endorses:

$$W_i^{\text{base,agree}} \equiv \frac{1}{3} \left( W_i^{\text{morale,agree}} + W_i^{\text{quit,agree}} + W_i^{\text{prod,agree}} \right),$$

$$W_i^{\text{base,top}} \equiv \frac{1}{3} \left( W_i^{\text{morale,top}} + W_i^{\text{quit,top}} + W_i^{\text{prod,top}} \right),$$

where  $W_i^{m,\text{agree}} = \mathbf{1}\{\text{Agree or Strongly Agree}\}$  and  $W_i^{m,\text{top}} = \mathbf{1}\{\text{Strongly Agree}\}$  for each item  $m$ .

## R.2 Outcomes

We relate these worry measures to: (i) survey indicators for base-wage reductions and bonus reductions in 2020, (ii) survey-reported cut intensity (reported percentage reduction  $\times$  percentage of employees affected), (iii) administrative measures of realized base-wage non-increases and base-wage cuts among job stayers, and (iv) an administrative layoff-rate measure.

- Survey policy outcomes:

$$S_i^{\text{W}} \equiv \mathbf{1}\{\text{firm reports base-wage reduction}\}, \quad S_i^{\text{B}} \equiv \mathbf{1}\{\text{firm reports fewer/lower bonuses}\}.$$

We also construct cut intensity:

$$S_i^{\text{W,int}} \equiv \left( \frac{\text{cut}\%}{100} \right) \left( \frac{\text{affected}\%}{100} \right) \quad \text{and similarly for bonuses } (S_i^{\text{B,int}}).$$

- Administrative realized outcomes (firm-level; stayer-based constant composition): Using the stayer base-wage-change data, we define:

$$A_i^{\text{NI}} \equiv \frac{1}{N_i^{\text{S}}} \sum_{j \in S_i} \mathbf{1}\{\Delta w_{ij} \leq 0.02\}, \quad A_i^{\text{cut}} \equiv \frac{1}{N_i^{\text{S}}} \sum_{j \in S_i} \mathbf{1}\{\Delta w_{ij} < -0.02\}.$$

We construct employment stability outcomes (administrative) at the firm level:

$$A_i^{\text{LR}} \equiv \frac{\#\text{AdminLayoffs}_{i,2020}}{\text{Employment}_{i,2019}}.$$

## R.3 Specifications

We control for shock severity using a 3-state shock control:

$$\text{Severe}_i = \mathbf{1}\{g_i < -0.35\}, \quad \text{Moderate}_i = \mathbf{1}\{-0.35 \leq g_i < -0.05\},$$

$$\text{Mild}_i = \mathbf{1}\{g_i \geq -0.05\} \text{ (omitted base),}$$

where  $g_i$  is 2019-2020 revenue growth.

- Do worried firms cut pay less?

1. Layoff-firm worry measure. Sample: survey layoff firms with non-missing  $W_i^{\text{layoff-q}}$ . Estimate:

$$y_i = \alpha + \beta W_i^{\text{layoff-q}} + \gamma_1 \text{Moderate}_i + \gamma_2 \text{Severe}_i + X_i' \kappa + \mu_{s(i)} + \varepsilon_i, \quad (\text{R.1})$$

where  $y_i \in \{S_i^{\text{W}}, S_i^{\text{B}}, S_i^{\text{W,int}}, S_i^{\text{B,int}}, A_i^{\text{NI}}, A_i^{\text{cut}}\}$ . This directly addresses “do worried firms cut pay less often?” within a sample where the worry item is not mechanically conditioned on non-cutting.

2. Base-pay worry index (available only among firms that did not report base-pay cuts). Sample: revenue-reduction firms with  $S_i^{\text{W}} = 0$  and non-missing  $W_i^{\text{base}}$ . Because  $S_i^{\text{W}}$  is fixed by design here, we focus on administrative realized pay outcomes:

$$y_i = \alpha + \beta W_i^{\text{base}} + \gamma_1 \text{Moderate}_i + \gamma_2 \text{Severe}_i + X_i' \kappa + \mu_{s(i)} + \varepsilon_i, \quad (\text{R.2})$$

where  $y_i \in \{A_i^{\text{NI}}, A_i^{\text{cut}}\}$ . Among firms that did not cut base pay, this tests whether higher concerns predict broader realized wage freezes or cuts among stayers.

Controls  $X_i$  are standard firm covariates (size, age, productivity, unionization, etc.) and  $\mu_{s(i)}$  are industry FE.

- Is employment less stable in worried firms?

1. Layoff-firm worry measure sample: Estimate

$$A_i^{\text{LR}} = \alpha + \delta W_i^{\text{layoff-q}} + \gamma_1 \text{Moderate}_i + \gamma_2 \text{Severe}_i + X_i' \kappa + \mu_{s(i)} + \eta_i, \quad (\text{R.3})$$

Under the canonical substitution view,  $\delta > 0$ ; under our alternative view,  $\delta$  may be small.

2. Base-pay worry index sample (revenue-reduction, no reported base-pay cuts): Estimate the same regression as (R.3) but with  $W_i^{\text{base}}$  in place of  $W_i^{\text{layoff-q}}$ , on the sample where  $W_i^{\text{base}}$  is observed. This answers the “employment stability” question in the most natural “pay-cut avoiders” sample.

All specifications control for revenue-shock severity using coarse revenue-growth categories (moderate vs. severe declines relative to a mild/positive baseline), and include the standard firm covariates and industry fixed effects used elsewhere in the paper. We use the entropy-balancing weights, heteroskedasticity-robust standard errors, and linear probability models.

## R.4 Results and interpretation

Tables R.1 and R.2 report the main estimates for Constraint measures 1 and 2, respectively.

For Constraint measure 1 (Table R.1), higher worry is negatively associated with the probability of reporting base-wage reductions in both panels (col. 1:  $-0.059$ ,  $p < 0.10$  in Panel A;  $-0.082$ ,  $p < 0.05$  in Panel B). The effect strengthens under the broader agreement threshold. In Panel B, the constraint is also associated with more bonus cuts (col. 2:  $0.084$ ,  $p < 0.01$ ), lower cut intensity (col. 3:  $-0.108$ ,  $p < 0.10$ ), and a significantly lower layoff rate (col. 7:  $-0.125$ ,  $p < 0.01$ ). On the administrative side, worry does not robustly predict base-wage non-increases (col. 5) or base-wage cuts (col. 6) among job stayers in either panel.

For Constraint measure 2 (Table R.2), none of the three administrative outcomes—non-increases (col. 1), cuts (col. 2), or the layoff rate (col. 3)—is significantly associated with the constraint measure.

This combination of findings is consistent with the view that base-pay cuts are perceived as costly and blunt, and that avoiding base-pay cuts does not mechanically translate into greater use of layoffs, because firms can adjust through other margins and because layoffs serve selection and reorganization roles.

Table R.1: Constraint Measure 1: Morale Barrier and Realized Adjustment Behavior (Layoff Firms)

	Survey Outcomes				Administrative Outcomes		Administrative Layoffs
	(1) Survey Cut	(2) Survey Bonus Cut	(3) Cut Intensity	(4) Bonus Cut Int.	(5) Admin Non-Inc.	(6) Admin Cut	(7) Admin Layoff Rate
<b>Panel A: Strongly Agree</b>							
Constraint	-0.059* (0.032)	0.019 (0.036)	-0.037 (0.068)	0.051 (0.089)	0.003 (0.026)	0.029 (0.027)	-0.006 (0.044)
Moderate shock [-35%, -5%]	0.057 (0.041)	0.025 (0.044)	0.066 (0.084)	-0.383*** (0.139)	0.001 (0.034)	0.016 (0.033)	0.040 (0.054)
Severe shock (< -35%)	-0.013 (0.116)	-0.122 (0.105)	0.245 (0.208)	-0.790** (0.332)	0.013 (0.082)	-0.010 (0.092)	-0.014 (0.133)
Observations	657	657	108	110	396	396	657
$R^2$	0.106	0.081	0.371	0.330	0.049	0.081	0.127
<b>Panel B: Agree or Strongly Agree</b>							
Constraint	-0.082** (0.034)	0.084*** (0.030)	-0.108* (0.059)	-0.198* (0.100)	0.008 (0.024)	-0.015 (0.026)	-0.125*** (0.038)
Moderate shock [-35%, -5%]	0.063 (0.042)	0.019 (0.044)	0.040 (0.080)	-0.404*** (0.140)	0.000 (0.034)	0.018 (0.033)	0.047 (0.053)
Severe shock (< -35%)	-0.001 (0.116)	-0.139 (0.104)	0.237 (0.197)	-0.716** (0.337)	0.010 (0.081)	-0.003 (0.093)	0.013 (0.131)
Observations	657	657	108	110	396	396	657
$R^2$	0.111	0.091	0.399	0.359	0.049	0.079	0.141
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

*Notes:* Constraint measure 1 is a binary indicator from the “Why not lower wages instead of layoffs?” battery asked to layoff firms: = 1 if the firm agrees that “pay reductions hurt morale/productivity more.” Panel A uses Strongly Agree; Panel B uses Agree or Strongly Agree. Wage data from LONN. All columns use entropy-balancing weights and include baseline firm controls and industry fixed effects (not shown). Robust standard errors in parentheses. Columns 1-4 use survey-reported outcomes: (1) base-wage reduction indicator, (2) bonus reduction indicator, (3) base-wage cut intensity (reported % cut  $\times$  % affected), (4) bonus cut intensity. Columns 5-6 use administrative outcomes among job stayers: (5) share with base-wage change  $\leq 2\%$  (non-increase), (6) share with base-wage change  $< -2\%$  (cut). Column 7: administrative layoff rate (layoffs / 2019 employment). Revenue-shock controls classify firms by 2019-2020 administrative revenue growth. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Table R.2: Constraint Measure 2: Morale Barrier and Realized Adjustment Behavior (Revenue-Decline Firms That Did Not Cut Base Wages)

	(1) Admin Non-Inc.	(2) Admin Cut	(3) Admin Layoff Rate
<b>Panel A: Strongly Agree</b>			
Constraint	0.040 (0.042)	0.014 (0.038)	0.028 (0.067)
Severe shock (< -35%)	-0.078 (0.118)	0.043 (0.128)	0.069 (0.162)
Moderate shock [-35%, -5%]	-0.043 (0.046)	0.051 (0.046)	0.052 (0.067)
Observations	274	161	517
$R^2$	0.087	0.165	0.161
<b>Panel B: Agree or Strongly Agree</b>			
Constraint	-0.009 (0.039)	-0.060 (0.043)	-0.055 (0.059)
Severe shock (< -35%)	-0.059 (0.116)	0.046 (0.127)	0.069 (0.162)
Moderate shock [-35%, -5%]	-0.033 (0.045)	0.065 (0.048)	0.051 (0.067)
Observations	274	161	517
$R^2$	0.085	0.180	0.162
Firm Controls	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes

*Notes:* Constraint measure 2 is constructed from the “Reasons for not lowering base pay” battery, asked to revenue-decline firms that did not report base-wage reductions. The measure is the row mean of Strongly Agree indicators (Panel A) or Agree-or-Strongly-Agree indicators (Panel B) across the morale, quit, and productivity items. Wage data from LONN. All columns use entropy-balancing weights and include baseline firm controls and industry fixed effects (not shown). Robust standard errors in parentheses. Column 1: share of job stayers with base-wage change  $\leq 2\%$  (non-increase). Column 2: share of job stayers with base-wage change  $< -2\%$  (cut). Column 3: administrative layoff rate (layoffs / 2019 employment). Revenue-shock controls classify firms by 2019-2020 administrative revenue growth. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## S Wage-Cut Constraints and Employment

This appendix presents the supporting evidence for Section 8.6. We construct three firm-level indicators of stated wage-cut constraints from separate survey batteries and compare employment trajectories across constraint groups using two complementary event-study approaches: one in employment growth rates and one in employment levels.

### S.1 Construction of constraint indicators

We define three time-invariant firm-level indicators from the survey, drawn from distinct batteries and therefore based on different samples.

The first, which we label *morale-constrained* (non-cutter sample), equals one if the firm strongly agrees on at least one of four items capturing morale, productivity, quit, and employee-promise concerns from the “reasons for not lowering base pay” battery (Figure 4). This battery is asked of firms that experienced a revenue reduction but did not cut base wages. These firms recognize the wage-cut margin but judge its endogenous cost prohibitively high. The morale (non-cutter) sample comprises 648 firms (405 other, 243 morale-constrained; 37.5%).

The second, which we label *morale-constrained* (layoff sample), equals one if the firm strongly agrees that “wage cuts hurt morale and productivity more than layoffs” from the “why not lower wages instead of layoffs” battery. This battery is asked of layoff firms. This is the same measure used in Section 8.5 (Appendix R), where it is validated cross-sectionally: it predicts significantly fewer base-wage cuts ( $-8.2$  percentage points,  $p < 0.05$ ), more bonus cuts ( $+8.4$  percentage points,  $p < 0.01$ ), and fewer layoffs ( $-12.5$  percentage points,  $p < 0.01$ ). The morale (layoff) sample comprises 1,004 firms (744 other, 260 morale-constrained; 25.9%).

The third, which we label “Do not know,” equals one if the firm responds “Do not know” when asked what percent pay cut would have saved the jobs it eliminated (Figure 10). This question is asked of layoff firms. For these firms, the wage-cut-for-layoff substitution trade-off does not exist as an operational concept: they cannot formulate the counterfactual as a quantitative problem. The DK sample comprises 1,046 firms (408 other, 638 DK; 61%).

The three indicators capture economically different mechanisms. The two morale constraints reflect endogenous costs on a recognized margin: firms know they could cut wages but judge the consequences too severe. “Do not know” reflects the absence of the margin altogether. Because the batteries target different populations, the samples do not fully overlap.

### S.2 Pre-shock comparability

Table S.1 reports standardized differences in pre-shock firm characteristics between morale-constrained (non-cutter sample) and other firms. Standardized differences in employment growth (2018-2019), firm size, revenue level, and female workforce share are all below 0.20 in absolute value. The largest difference is the bachelor share ( $-0.18$

SD), indicating that morale-constrained firms employ a somewhat less educated workforce. Table S.2 reports the industry distribution. A Pearson  $\chi^2(7) = 2.93$  ( $p = 0.891$ ) cannot reject equal industry composition across groups. Covariate balance focuses on morale-constrained firms because their event-study pre-trends (discussed below) warrant closer inspection; for the morale (layoff) and “Do not know” groups, the event-study pre-trends themselves serve as the primary comparability diagnostic.

Table S.1: Pre-Shock Balance: Morale-Constrained vs. Other Firms

	Unweighted				Weighted			
	Other	Morale	SD	Std. Diff.	Other	Morale	SD	Std. Diff.
Bachelor share	0.201	0.164	0.207	-0.183	0.204	0.163	0.210	-0.193
Employment Growth (2018-2019)	0.073	0.086	0.187	0.071	0.072	0.083	0.190	0.058
Female share	0.310	0.309	0.221	-0.008	0.311	0.308	0.222	-0.014
Ln. Employment (2019)	3.665	3.664	0.988	-0.001	3.590	3.597	0.932	0.008
Ln. Revenue (2019)	17.243	17.120	1.443	-0.085	17.172	17.065	1.401	-0.076
Revenue Growth (2018-2019)	0.023	0.069	0.284	0.161	0.023	0.068	0.288	0.157

Note:  $N = 648$  firms (405 Other, 243 Morale). Standardized differences computed as (mean Morale – mean Other) / SD(Other). Weighted columns apply entropy-balancing weights. SD = standard deviation of the “Other” group.

Table S.2: Industry Distribution by Constraint Group

Industry	Unweighted			Weighted		
	Other	“Morale”	Total	Other	“Morale”	Total
Manufacturing	92	51	143	77.50	43.42	120.92
Construction	40	27	67	45.09	30.27	75.36
Wholesale and Retail	124	74	198	126.85	76.07	202.92
Transport	22	13	35	22.45	13.25	35.70
Accommodation	33	21	54	33.93	21.49	55.42
Information & Comm.	33	15	48	36.63	16.58	53.21
Liberal, Scientific	33	16	49	33.80	16.25	50.05
Admin. Services	24	20	44	24.29	20.14	44.43
Total	401	237	638	400.53	237.47	638.00

Note: Unweighted counts and entropy-balancing weighted counts by industry and constraint group. Pearson  $\chi^2(7) = 2.93$ ,  $p = 0.891$ : cannot reject equal industry distribution across constraint groups. “Morale” Firms = 1 if firm reports morale/productivity/quit concerns as a constraint on base-wage cuts. Real Estate and Other Services excluded due to Statistics Denmark disclosure restrictions; the regression sample is 648 firms (405 other, 243 morale-constrained) for the morale (non-cutter) indicator and 1,046 firms (408 other, 638 DK) for the “Do not know” indicator.

### S.3 Employment trajectory specification

We estimate two complementary event-study specifications over a firm-year panel (2016-2022), both restricted to revenue-decline firms. The shock variable classifies firms as Severe ( $g < -0.35$ ), Moderate ( $-0.35 \leq g < -0.20$ ), or Mild ( $-0.20 \leq g < -0.05$ ); firms with positive revenue growth are excluded.

**Levels.** We regress log employment on year-by-constraint interactions:

$$\ln \text{emp}_{it} = \alpha_i + \gamma_t \text{ShockSeverity}_i + \sum_{\tau \neq 2019} \phi_\tau \text{Constraint}_i \times \mathbf{1}\{t = \tau\} + \varepsilon_{it}, \quad (\text{S.1})$$

where  $\alpha_i$  are firm fixed effects and  $\gamma_t \text{ShockSeverity}_i$  are shock-severity  $\times$  year interactions. The  $\phi_\tau$  coefficients trace out the employment level gap between constrained and unconstrained firms year by year, relative to 2019. Pre-2020 coefficients test parallel trends; post-2020 coefficients capture differential dynamics.

**Growth rates.** We regress employment growth on the same year-by-constraint interactions:

$$\Delta \ln \text{emp}_{it} = \alpha_i + \gamma_t \text{ShockSeverity}_i + \sum_{\tau \neq 2019} \psi_\tau \text{Constraint}_i \times \mathbf{1}\{t = \tau\} + \varepsilon_{it}, \quad (\text{S.2})$$

where  $\psi_\tau$  measures the differential annual employment growth rate for constrained vs. other firms in year  $\tau$ , relative to 2019. Because pre-trend coefficients may be non-zero, we report `lincom` tests that compare each post-2020 coefficient to the average pre-trend coefficient (mean of 2016-2018); a significant difference indicates a post-shock break in differential growth beyond the pre-existing pattern.

Standard errors are clustered at the firm level throughout. Under the canonical view,  $\phi_\tau < 0$  for  $\tau \geq 2020$  (levels) and  $\psi_\tau$  should be significantly more negative after 2020 than before (growth rates): firms with tighter constraints on base-wage cuts should experience larger employment declines and slower recovery. Under the interpretation advanced in this paper, these coefficients should be approximately zero: tighter base-wage-cut constraints should not translate into differential employment losses because firms do not treat pay cuts as an operational substitute for layoffs.

### S.4 Results

**Levels.** Table S.3 reports the levels event-study coefficients from Equation S.1. For both morale measures, a pre-existing employment gap is evident: the 2018 coefficient is  $-0.049$  ( $p < 0.01$ ) for the non-cutter measure and  $-0.038$  for the layoff measure. Post-2020 coefficients remain in the same range, with no break at 2020: the 2022 coefficient is  $-0.059$  ( $p < 0.05$ ) for the non-cutter measure, only 1 log point beyond the pre-trend. For the layoff measure, all post-2020 coefficients are small and insignificant. For “Do not know” firms, pre-2020 coefficients are all insignificant, supporting parallel trends; the 2022 coefficient is  $-0.058$  ( $p < 0.10$ ), marginally significant. Figure S.1 plots the corresponding event-study coefficients with 95% confidence intervals.

Table S.3: Event Study in Levels: Log Employment ( $\ln E$ ) by Constraint Group (Three Measures)

Year $\times$ Constraint	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample			Balanced Panel		
	Morale	Morale-Layoff	Don't Know	Morale	Morale-Layoff	Don't Know
2016 $\times$ Constraint	-0.0530 (0.0324)	-0.0507 (0.0376)	-0.0134 (0.0343)	-0.0566* (0.0336)	-0.0633* (0.0357)	-0.0135 (0.0355)
2017 $\times$ Constraint	-0.0371 (0.0235)	-0.0384 (0.0301)	-0.0332 (0.0268)	-0.0227 (0.0233)	-0.0440* (0.0262)	-0.0163 (0.0268)
2018 $\times$ Constraint	-0.0486*** (0.0177)	-0.0376 (0.0259)	-0.0194 (0.0210)	-0.0488*** (0.0177)	-0.0484** (0.0213)	0.0025 (0.0200)
2019 $\times$ Constraint	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
2020 $\times$ Constraint	-0.0326 (0.0222)	0.0067 (0.0220)	0.0063 (0.0205)	-0.0307 (0.0229)	0.0016 (0.0230)	0.0179 (0.0213)
2021 $\times$ Constraint	-0.0283 (0.0255)	-0.0116 (0.0303)	-0.0132 (0.0260)	-0.0275 (0.0265)	-0.0245 (0.0296)	-0.0053 (0.0261)
2022 $\times$ Constraint	-0.0586** (0.0286)	-0.0255 (0.0357)	-0.0576* (0.0303)	-0.0589** (0.0295)	-0.0405 (0.0344)	-0.0577* (0.0297)
Observations	3,043	5,011	3,579	2,618	4,228	2,996
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Shock $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Firm-year panel regression with firm fixed effects. Dependent variable: log employment ( $\ln emp_{it}$ ). Baseline year: 2019 (omitted). Morale (Non-Cutter S.) = 1 if Strongly Agree on any morale/productivity/quit/promise item; Morale (Layoff S.) = 1 if firm cited morale/productivity concerns as reason for preferring layoff over pay cut; Don't Know = 1 if firm cannot quantify a job-saving pay-cut threshold. All specifications include shock-severity  $\times$  year interactions and condition on revenue decline. Columns (1)-(3): full sample; columns (4)-(6): balanced panel. Standard errors clustered at the firm level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

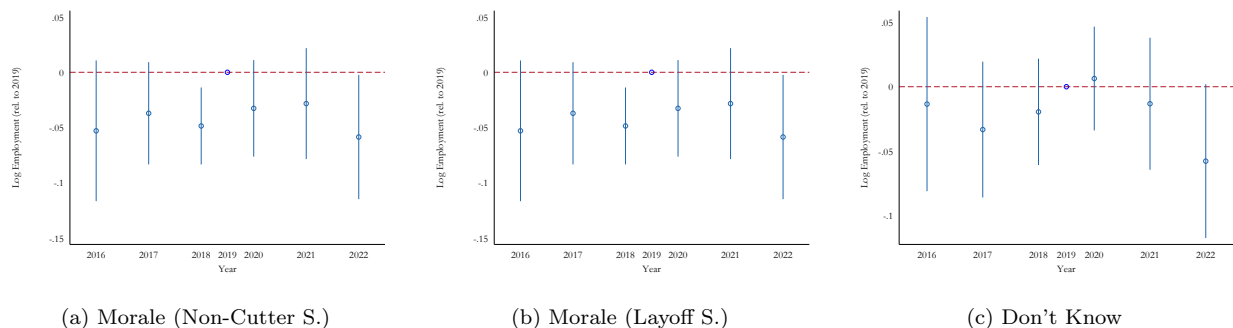


Figure S.1: Levels Event Study: Log Employment by Constraint Group

Note: Estimated from Equation S.1. Dependent variable: log employment. Baseline year: 2019 (omitted). Shaded areas denote 95% confidence intervals. Standard errors clustered at the firm level.

**Growth rates.** Table S.4 reports the growth-rate event-study coefficients from Equation S.2. For both morale measures, several pre-trend coefficients are negative, significantly so for 2016 and 2018 in the non-cutter measure and for 2018 in the layoff measure, indicating that constrained firms were already growing more slowly before 2020. Raw post-2020 coefficients are also negative: in the full sample, the 2020 coefficient for the non-cutter measure is  $-0.083$  ( $p < 0.01$ ) and the 2022 coefficient is  $-0.076$  ( $p < 0.01$ ). For the layoff measure, 2021 =  $-0.078$  ( $p < 0.05$ ) and 2022 =  $-0.054$  ( $p < 0.10$ ).

The `lincom` tests in the bottom panel compare each post-2020 coefficient to the average pre-trend (mean of 2016-2018). For both morale measures, none of the post-minus-pre-trend differences is statistically significant: the post-2020 negative coefficients do not exceed the pre-existing pattern of slower growth. For “Do not know” firms, pre-trend coefficients are all near zero and insignificant, but the 2022 coefficient is  $-0.065$  ( $p < 0.05$ ), yielding a significant `lincom` difference of  $-0.060$  ( $p < 0.01$ ) in both the full sample and balanced panel. Figure S.2 plots the growth-rate event-study coefficients.

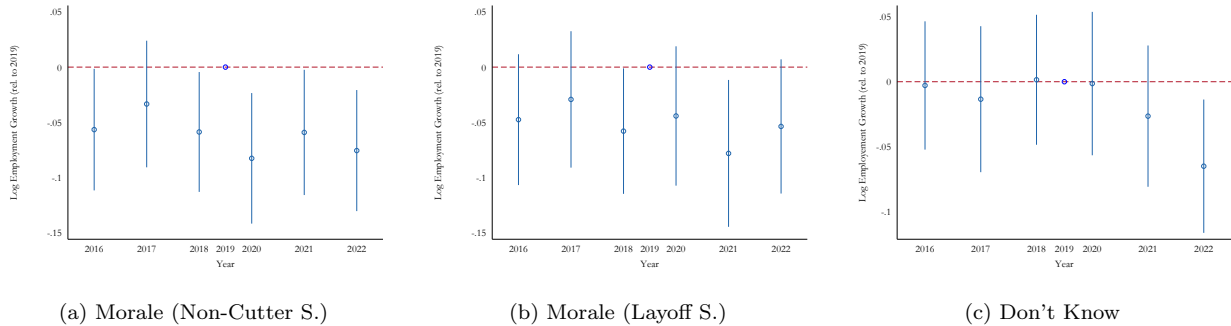


Figure S.2: Growth-Rate Event Study: Employment Growth by Constraint Group

Note: Estimated from Equation S.2. Dependent variable: employment growth ( $\Delta \ln emp_{it}$ ). Baseline year: 2019 (omitted). Shaded areas denote 95% confidence intervals. Standard errors clustered at the firm level.

Table S.4: Event Study in Growth Rates: Employment Growth ( $\Delta \ln E$ ) by Constraint Group (Three Measures)

Year $\times$ Constraint	(1)	(2)	(3)	(4)	(5)	(6)
	Full Sample			Balanced Panel		
	Morale	Morale-Layoff	Don't Know	Morale	Morale-Layoff	Don't Know
2016 $\times$ Constraint	-0.0566** (0.0280)	-0.0479 (0.0301)	-0.0029 (0.0252)	-0.0527* (0.0284)	-0.0573* (0.0294)	0.0053 (0.0241)
2017 $\times$ Constraint	-0.0334 (0.0292)	-0.0290 (0.0314)	-0.0135 (0.0286)	-0.0265 (0.0289)	-0.0224 (0.0295)	0.0022 (0.0277)
2018 $\times$ Constraint	-0.0587** (0.0276)	-0.0574** (0.0289)	0.0015 (0.0255)	-0.0754*** (0.0271)	-0.0511* (0.0288)	0.0069 (0.0254)
2019 $\times$ Constraint	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
2020 $\times$ Constraint	-0.0825*** (0.0301)	-0.0442 (0.0321)	-0.0014 (0.0281)	-0.0733** (0.0294)	-0.0481 (0.0295)	0.0064 (0.0272)
2021 $\times$ Constraint	-0.0591** (0.0288)	-0.0776** (0.0338)	-0.0265 (0.0277)	-0.0444 (0.0292)	-0.0674** (0.0317)	-0.0235 (0.0276)
2022 $\times$ Constraint	-0.0755*** (0.0279)	-0.0535* (0.0309)	-0.0651** (0.0261)	-0.0790*** (0.0269)	-0.0583** (0.0288)	-0.0555** (0.0247)
Observations	3,012	4,942	3,525	2,607	4,211	2,985
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Shock $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
<i>Post-period differential relative to pre-trend average (p-values in brackets):</i>						
2020 - avg(2016-2018)	-0.0336 [0.157]	0.0006 [0.981]	0.0043 [0.851]	-0.0223 [0.332]	-0.0045 [0.849]	0.0035 [0.874]
2021 - avg(2016-2018)	-0.0098 [0.703]	-0.0329 [0.170]	-0.0222 [0.353]	0.0068 [0.787]	-0.0238 [0.322]	-0.0288 [0.221]
2022 - avg(2016-2018)	-0.0262 [0.275]	-0.0087 [0.723]	-0.0598*** [0.009]	-0.0276 [0.243]	-0.0147 [0.539]	-0.0601*** [0.007]

Note: Firm-year panel regression with firm fixed effects. Dependent variable: employment growth ( $\Delta \ln emp_{it}$ ). Baseline year: 2019 (omitted). Each year coefficient is the differential annual growth rate for constrained vs. other firms. The bottom panel reports `lincom` tests of whether each post-2020 coefficient differs significantly from the average pre-trend coefficient (2016-2018);  $p$ -values in brackets. Morale (Non-Cutter S.) = 1 if Strongly Agree on any morale/productivity/quit/promise item; Morale (Layoff S.) = 1 if firm cited morale/productivity concerns as reason for preferring layoff over pay cut; Don't Know = 1 if firm cannot quantify a job-saving pay-cut threshold. All specifications include shock-severity  $\times$  year interactions and condition on revenue decline. Columns (1)-(3): full sample; columns (4)-(6): balanced panel. Standard errors clustered at the firm level. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

In sum, neither morale-based constraint measure produces employment effects that survive pre-trend adjustment. For the non-cutter morale measure, a pre-existing employment gap of approximately 5 log points persists throughout the sample period with no break at 2020; the growth-rate `lincom` tests confirm that post-2020 growth is not significantly slower than the pre-trend. For the layoff morale measure, the constraint that Section 8.5 validated cross-sectionally (predicting fewer pay cuts and fewer layoffs) yields no significant post-minus-pre-trend difference in any year. For “Do not know” firms, the levels specification shows clean pre-trends and a marginally significant 2022 coefficient ( $-0.058$ ,  $p < 0.10$ ); the growth-rate specification sharpens this to a significant 2022 break ( $-0.060$ ,  $p < 0.01$ ). Under the canonical mechanism, however, the employment effect of wage-cut constraints should appear at the time of the shock (2020), when firms face the acute cost-adjustment decision; no constraint measure produces a significant 2020 effect in either specification.