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When Experience Travels: Digital Leadership and Workforce Adaptation*

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Abstract

Otherwise similar firms differ widely in whether they offer work from home (WFH), and much of this variation remains unexplained after accounting for industry and occupation. We ask whether leaders' prior exposure to digitally intensive organizations helps explain this variation. Danish administrative data identify each firm's 2019 CEO and board members and measure the digital-workforce intensity of other organizations at which they held qualifying executive, managerial, or board roles during 2014–2018, before the unexpected COVID-19 shock raised the value of remote coordination. Linking these predetermined histories to online vacancies through 2023, we use a difference-in-differences design in which treatment intensity is continuous and fixed before the shock; we report dynamic (event-study) estimates of the year-specific gradient relative to 2019 and pooled estimates for 2020–2021 and 2022–2023. One benchmark-equivalent year of external CEO digital experience is associated with a 1.2-percentage-point larger increase in the WFH-posting share during 2020–2021 and a 2.6-point larger increase during 2022–2023, relative to 2015–2019. The later baseline estimate equals 84 percent of the 3.1-percent pre-2020 mean and falls to 0.8 points after demanding adjustments for observed pre-pandemic firm capacity and fixed-2019 two-digit industry-by-year effects. The gradient strengthens through 2023, and board digital experience also predicts a larger later increase in WFH-posting availability. Pre-2019 gradients are flat, and a 2018 placebo does not reproduce the post-2019 path. A fixed-preperiod occupation decomposition explains little of the gradient; companion WFH measures remain positive when zero-posting years are retained, and a selected-sample timing restriction yields a similar CEO relationship. The results are consistent with, but do not establish, the transfer of organizational know-how across firms.

Keywords: work from home; managerial human capital; digital leadership; job vacancies; boards; firm adaptation.

JEL classifications: J24; J63; M12; M14; O33.

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1 Introduction

Working from home (WFH) changes more than where an employee opens a laptop. It changes how work is supervised, how teams coordinate, which workers a firm can recruit, and how employees combine work with commuting and family life. These changes can improve productivity and retention in some settings, while in others they may weaken communication or increase isolation (Bloom et al., 2015; Emanuel and Harrington, 2024; Bloom et al., 2024). They also matter for worker well-being. Evidence from Denmark links occupational digital readiness and the expansion of remote work after 2019 to changes in treated mental-health outcomes and commuting (Daly and Zhu, 2026). Whether a firm advertises WFH availability is therefore an economically consequential choice. It determines which workers have access to these benefits and costs in the first place.

The persistence of large differences across employers suggests that technical feasibility is only part of the story. Two firms may hire similar workers and operate in the same broad market, yet one advertises remote or hybrid work while the other does not (Barrero et al., 2023; Hansen et al., 2023; Adrjan et al., 2025). Offering WFH requires a series of judgments. Managers must decide which tasks can be moved away from the workplace, what communication and monitoring systems are adequate, how often workers need to meet, and whether a flexible arrangement can be sustained after the immediate pressure to adopt has passed. These are implementation decisions. They depend on what the people running the firm know about digitally mediated work and on whether they can turn that knowledge into routines that employees and applicants believe.

This paper asks whether digital leadership exposure at other organizations predicts how firms respond when remote coordination suddenly becomes more valuable. More precisely, we ask whether firms led in 2019, the last pre-pandemic year, by CEOs and boards with greater digital leadership experience at other organizations subsequently offered more opportunities to work from home. Focusing on experience outside the focal firm is central to the question. Total experience combines exposure at other organizations, whether before or concurrent with the leader's role at the focal firm, with exposure accumulated inside the focal firm. The external component crosses an organizational boundary; the internal component is inseparable from the focal firm's own technology, workers, and routines. External experience therefore provides a sharper measure of organizational exposure acquired elsewhere. Estimates using total digital experience are qualitatively very similar, as we show below.

The paper's broader contribution is to examine a pattern consistent with organizational capabilities crossing firm boundaries. New technologies and work practices do not arrive with self-executing instructions. Their use depends on whether decision makers can recognize where they fit, anticipate implementation costs, and sustain the routines that make them workable. If part of this capability is embodied in people, leaders' organizational exposure elsewhere may help practices diffuse and may contribute to different responses to a common shock. The design does not identify that transfer directly. External roles may be concurrent with the focal role and therefore capture current networks or interlocks, while even strictly prior roles may reflect

nonrandom leader–firm matching. The measure records organizational exposure rather than knowledge itself. WFH offers a useful setting because the pandemic abruptly raised the return to remote coordination. The evidence also broadens the relevant unit of leadership beyond the chief executive: experience distributed across the board predicts subsequent adaptation as well. This perspective shifts attention from whether a technology or work arrangement is feasible to whether the people making and overseeing organizational choices have encountered it before.

Although WFH is the main outcome, we treat it as one dimension of workforce adaptation. Extensions examine whether the same digital leadership experience predicts overall vacancy volume, demand for digital and AI-related workers, continued observation in annual business records, and leadership continuity. These comparisons help distinguish a change in how firms organize jobs from a general expansion in recruiting or digital labor demand.

The COVID-19 shock gives this question empirical content. Denmark introduced national restrictions in March 2020, but private employers generally retained discretion over whether and how to implement WFH. The shock raised the immediate value of remote coordination for many firms at roughly the same time without assigning the same work arrangement to every firm. Earlier work shows that recessions can accelerate changes in vacancy skill requirements (Hershbein and Kahn, 2018); work on COVID-19 instead shows that firms with more sophisticated pre-pandemic technology and greater pre-pandemic WFH feasibility were more resilient when the shock arrived (Comin et al., 2022; Bai et al., 2021). Some firms could adapt existing routines quickly; others had to determine whether remote work was compatible with their jobs, install new practices, or decide that the organizational costs were too high. If external digital experience reflects portable implementation knowledge, its relationship with WFH offers should become stronger when that knowledge suddenly becomes useful. Section 2 describes the Danish setting in more detail.

The argument does not require digitally experienced leaders to be technology specialists. A CEO who has held a qualifying role at a digitally intensive organization may have learned which jobs can be coordinated at a distance, which investments complement remote work, or where implementation tends to fail. Career experience can shape later corporate decisions because leaders carry human capital across settings (Custódio and Metzger, 2013; Bertrand and Schoar, 2003). Experience, however, need not always favor adoption. Goehring et al. (2024) find that first-time and early-career film directors were more likely than experienced directors to adopt digital cinematography. Career concerns and a greater willingness to take risks, rather than knowledge carried from earlier directing settings, explain that pattern; more experienced directors were more likely to remain with traditional film. This contrast is useful because it separates our experience-based interpretation from a general claim that experienced leaders always adopt new technology first. A board with relevant exposure may be better able to evaluate proposed changes, question unrealistic plans, and support investments whose returns are initially uncertain. The CEO and board need not matter in the same year or with the same magnitude. The CEO is closer to operations; the board advises and monitors. The data allow us to examine both roles while keeping their experience measures conceptually distinct.

The main analysis links Danish administrative records to online vacancies from 2015 through 2023. The Jobindex extract used here begins in 2010; the earlier-cohort placebo also uses vacancies from 2014. The administrative data identify the CEO and formal board in 2019 and recover their organizational histories before the pandemic. We use those histories to measure the digital intensity of organizations in which each leader held a qualifying leadership position during 2014–2018. These positions may precede or coincide with the leader's role at the focal firm; "external" means that the organization differs from the focal firm, not that the role occurred earlier. Digital intensity is the share of an organization's workers in broadly classified digital occupations, scaled by the annual seventy-fifth percentile among organizations that employ at least one digital worker. No job-posting classification enters this treatment. A year in a qualifying role at an organization exactly at that benchmark contributes one benchmark-equivalent year of digital experience. We sum digital experience during 2014–2018 and exclude the focal 2019 firm, so the measure is fixed before the shock and captures exposure outside the organization whose subsequent behavior we study.

This scaling gives the coefficient a reasonably natural interpretation. One unit does not mean that the CEO is either "digital" or "not digital." It represents one year of leadership at an organization whose digital-workforce share is at the annual benchmark. In the CEO available cohort, the standard deviation of external CEO digital experience is 0.97 units. Thus a one-unit difference is close to a one-standard-deviation difference in the observed data as well as a concrete year of digital experience at a digitally intensive organization. The measure can also accumulate gradually: two years at half the benchmark produce the same stock as one year at the benchmark.

Our main outcome is the share of a firm's observed vacancies that advertise a positive work-from-home option, following the vacancy-based measure in Daly and Zhu (2026) and the multi-label job-posting classification framework in Zhu and Hardt (2025). It is an employer-side measure of job design, not a measure of how many days incumbent workers actually spend at home. This distinction matters. The outcome records whether remote, hybrid, or another home-work option is incorporated into the terms on which the firm recruits. It therefore captures a revealed organizational decision at the point when the firm is designing and filling a job. The denominator is all observed vacancies, digital and non-digital, so the outcome is not mechanically a measure of digital hiring.

We use a difference-in-differences design in which treatment intensity is continuous and fixed before the shock. The dynamic event-study estimates trace the year-specific digital-experience gradient relative to 2019, while the pooled estimates summarize the gradients for 2020–2021 and 2022–2023 relative to the pre-pandemic period. The regressions include firm and calendar-year fixed effects. Firm fixed effects absorb time-invariant differences across firms, including their average level of WFH advertising, while year fixed effects absorb changes common to all firms. Because every firm experiences the pandemic, the year effects absorb its common component; the coefficients measure how post-2019 changes vary with predetermined experience, not the level effect of the shock itself. The design interacts a continuous predetermined exposure with a common

shock, a class analyzed by Callaway et al. (2024), who show that comparisons across exposure levels are vulnerable to selection that standard parallel trends does not rule out. Section 4 states the pandemic counterfactual needed to interpret the comparison between positive measured and zero constructed exposure as the effect of entering the shock with any experience, and the additional cross-exposure restriction needed to interpret the continuous gradient as a response to additional experience. Because the firm's 2019 treatment intensity remains fixed after leadership turnover, the estimates have an intention-to-treat-style interpretation with respect to the pre-pandemic leadership cohort.

The main result is a clear post-2019 divergence. In our main estimation sample, one benchmark-equivalent year of external CEO digital experience predicts a 1.2-percentage-point larger increase in the WFH-posting share during 2020–2021 and a 2.6-point larger increase during 2022–2023, relative to 2015–2019. Against the 3.1-percent pre-2020 mean, these estimates are equivalent to about 39 percent of that mean in the early period and 84 percent in the later period. Relative to the 2019 gradient, the annual coefficients rise from 0.8 percentage points in 2020 to 1.5 points in 2021, 2.4 points in 2022, and 2.9 points in 2023. The growing gradient after the initial emergency phase in 2020 is informative. It suggests that the result is not confined to temporary compliance during lockdowns; firms led by more digitally experienced CEOs increasingly incorporated WFH into the jobs they advertised after restrictions had eased.

The CEO-only estimates are also very similar when we restrict the analysis to firms whose full 2019 leadership team satisfies the common support requirements: 1.3 and 2.8 percentage points. When CEO and board digital experience enter together, the later coefficient is 1.7 points for the CEO and 3.9 points for the board. The timing differs. CEO digital experience is already associated with a 1.1-point increase in 2020; board digital experience is small in 2020 but rises to 3.1 points in 2021 and 4.2 points in 2023. We do not interpret that pattern as proof of distinct mechanisms. It is consistent, however, with CEOs influencing the immediate operational response and boards becoming more relevant as firms formalize and extend new work arrangements.

The timing evidence is central to the interpretation. The annual pre-2019 coefficients are small, alternate in sign, and are jointly indistinguishable from zero in the main CEO specification ($p = 0.144$), the CEO common specification ($p = 0.563$), and the joint CEO–board specification ($p = 0.788$). A separate pre-pandemic placebo yields mixed evidence: it produces negative estimates in 2018 rather than the positive, steadily increasing path that begins with COVID-19. The CEO point estimates remain negative in 2019, while the board estimate reverses sign. The placebo therefore does not reproduce the sustained positive post-2020 path. Together with the immediate 2020 break, the evidence supports the interpretation that the pandemic activated a gradient in predetermined external digital leadership experience rather than extending an earlier WFH trend.

Adjustments for observed pre-pandemic capacity substantially attenuate, but do not eliminate, the later relationship. We interact calendar year with 2019 firm size, sales, and age; pre-2020 recruiting, WFH, and digital-recruiting histories; and leader characteristics. In a fixed CEO-available sample, these adjustments reduce the 2022–2023 estimate from 2.6 to 1.7 percentage

points. Adding fixed-2019 two-digit industry-by-year effects reduces it further to 0.8 points, where it remains statistically detectable. In the joint model, the corresponding board estimate falls from 3.9 to 3.0 and then 1.7 points and remains statistically detectable, while the conditional CEO estimate falls from 1.7 to 1.1 and then 0.5 points and is no longer distinguishable from zero. Because some pre-2020 histories may already reflect choices made under the selected leader, we treat these as sensitivity adjustments rather than as the preferred control set. A decomposition using fixed 2015–2019 occupation-specific WFH propensities assigns little of the estimated gradient to changes in firms' occupational mix. Estimates from a separate raw-vacancy specification with four-digit occupation-by-year effects are smaller for the CEO available sample, but retain the later CEO relationship on common support and the board relationship. That specification also changes the unit, weighting, and sample support, so it is not a pure occupation-control comparison. The WFH relationship is also positive for outcomes that follow fixed-cohort firms through years with no observed online vacancy. In a selected subsample, a similar pattern is obtained when measured exposure counts only external role-years preceding the leader's first observed focal-role year; this exercise does not hold contemporaneous interlocks fixed. Requiring fully measured external CEO histories leaves the CEO-only coefficients close to the baseline. The later estimates remain positive when firm-years must contain at least 5, 10, or 20 advertisements and when observations are weighted by vacancy volume, although vacancy weighting reduces the later CEO-available estimate. Among the 3,515 firms that retain the same selected CEO from 2019 through 2023, the pooled estimates are 1.1 and 2.7 percentage points, almost identical to the CEO common estimates. Requiring continuous firm presence from 2017 through 2022 likewise leaves them unchanged at 1.3 and 2.8 points. Finally, the later gradient is positive across all three CEO-tenure groups, with the largest point estimate among recently appointed CEOs, although the differences across tenure groups are not statistically precise. These exercises document a relationship that persists across several alternative specifications and samples, although the demanding capacity and industry adjustments substantially reduce its magnitude. The continuity checks condition on post-2019 outcomes and therefore show stability within selected subsamples rather than ruling out turnover or attrition as contributing pathways.

The comparison with other outcomes helps interpret what changed. External CEO digital experience predicts positive differential changes in the IHS WFH-posting count and in WFH vacancies per 100 fixed 2019 FTE when the fixed cohort is followed through firm-years with no observed online vacancy. These outcomes complement the conditional-share result, although their different transformation and support prevent a direct decomposition of that result into WFH and non-WFH postings. The raw total-posting estimates are positive but imprecise, while inverse-hyperbolic-sine estimates are negative; the data therefore show no statistically detectable rise in raw average vacancy volume but do not rule out an economically meaningful increase. Nor does the same digital experience produce a broad increase in digital recruitment. By 2022–2023, the digital-posting share is 0.8 percentage points lower per benchmark-equivalent year, and both the transformed digital-posting count and the probability of any digital posting are also lower.

Continued observation in annual business records and retention of the selected 2019 CEO are slightly lower. The corresponding restrictions document stability within selected subsamples, but they cannot determine whether organizational continuity contributes to the unrestricted association. We also ask whether external digital leadership experience predicts participation in a new technology domain rather than only WFH-posting availability. In a separate 2023 cross section, external CEO digital experience is positively associated with the IHS AI-posting count and the probability of any AI posting, while no board–AI association is statistically detected in this sample. Because the AI data are cross-sectional, this extension is suggestive rather than part of the event-study evidence.

The results first contribute to research on remote and hybrid work. Task-based measures establish which jobs can be performed at home, while survey and posting data document a large and persistent post-pandemic increase in remote work (Dingel and Neiman, 2020; Barrero et al., 2023; Bick et al., 2023; Adrjan et al., 2025). Other studies estimate effects on productivity, retention, and worker outcomes (Bloom et al., 2015; Emanuel and Harrington, 2024; Bloom et al., 2024; Daly and Zhu, 2026). Yet substantial dispersion across firms remains after accounting for occupation and industry (Hansen et al., 2023). Recent evidence from return-to-office announcements finds that firm and manager characteristics, including CEO age and gender, predict policy stringency more consistently than urban economic characteristics (Flynn et al., 2025). Survey evidence for the United States likewise shows markedly more WFH at younger firms and under younger CEOs (Aksoy et al., 2026), and CEO gender and experience predicted the adoption of remote-work technology among small and medium enterprises during the pandemic (Lashitew, 2023). We complement this work by measuring firm-linked histories of prior organizational exposure in administrative data for a broad sample of private corporate firms and tracing how the exposure gradient changes within firms after an unexpected shock.

The second contribution concerns managerial human capital and its movement across firms. Managers differ in behavior and policy choices, although evidence that a stable managerial "style" travels across employers is mixed (Bertrand and Schoar, 2003; Fee et al., 2013; Kaplan and Sorensen, 2021; Bandiera et al., 2020). More specific professional experiences have persistent effects on later decisions (Dittmar and Duchin, 2016; Schoar and Zuo, 2017; Custódio and Metzger, 2013). Closely related work in international trade shows that export knowledge acquired by managers at previous firms predicts export performance and entry into markets in which those managers have experience (Mion and Opromolla, 2014; Mion et al., 2025). In Danish matched employer–employee data, Meinen et al. (2022) show that hired managers carry internationalization knowledge across employers while correcting for the self-selection of mobile managers, and Stoyanov and Zubanov (2012) document productivity gains among manufacturing firms that hire workers from more productive firms, consistent with knowledge moving with people. Not every mobility flow transfers knowledge, however: in Finnish linked data, hiring workers from other firms' R&D labs into the hiring firm's own R&D produces no detectable spillover, while former R&D workers hired into non-R&D activities raise productivity and profitability (Maliranta et al., 2009). We

study a different form of organizational exposure: holding qualifying roles at digitally intensive organizations. The analysis also includes formal boards. Rather than identifying the transfer of experience itself, our contribution is to examine when predetermined leadership exposure becomes more predictive of firm behavior. The common economy-wide COVID-19 shock raised the value of one specific kind of experience across many industries at once, rather than in a single export market.

The findings also inform research on firm adaptation to large shocks. Downturns can change the skills firms demand, and firms with stronger pre-pandemic technology or greater WFH feasibility were more resilient during COVID-19 (Hershbein and Kahn, 2018; Comin et al., 2022; Bai et al., 2021). Management training can have effects that outlast the transfer of new machinery, while career incentives can also drive early technology adoption (Giorcelli, 2019; Goehring et al., 2024). Our outcome captures a different adjustment margin: how newly advertised jobs are organized. External digital leadership experience predicts a larger post-2019 increase in the WFH-posting share, a positive differential change in the IHS WFH count, and more WFH vacancies per 100 fixed 2019 FTE; it does not predict an increase in the share, IHS count, or incidence of digital vacancies.

The remainder of the paper proceeds as follows. Section 2 describes the Danish setting, Section 3 presents the data and measurement, and Section 4 develops the empirical strategy. Section 5 reports the main results; Sections 6–8 present robustness checks, heterogeneity analyses, and extensions to other outcomes. Section 9 concludes.

2 Danish Setting and the COVID-19 Shock

Denmark combines relatively flexible employment protection with generous income insurance and active labor-market institutions, an arrangement often described as flexicurity. The firms in our sample operate within this common national framework, but private employers retain substantial authority over work organization. This combination is useful for the analysis: firms encountered the same broad policy environment when the pandemic arrived, while decisions about WFH still depended on production constraints, task feasibility, and employer choices.

National restrictions began in March 2020. Public employees outside essential functions were instructed to work from home during the first lockdown. Private employers whose activities remained open were strongly encouraged to facilitate WFH where feasible and generally had more discretion over implementation. Our sample consists of private-sector corporate firms. The pandemic therefore sharply increased the value of remote coordination without creating a uniform private-sector WFH mandate. The common timing supplies the break used in the empirical design; the variation of interest is the difference in firms' responses to that break.

Private-sector adjustment also operated through a temporary wage-compensation scheme that allowed firms facing large expected layoffs to retain affected workers on payroll. Covered workers were furloughed and did not work during the compensation period. The scheme preserved

worker–firm matches, but it did not turn job retention into active remote work. This distinction is important because continued employment or continued appearance in administrative records cannot by itself be interpreted as WFH. Our outcome instead records the terms attached to newly advertised jobs. The pooled periods separate the immediate response in 2020–2021 from WFH advertising in 2022–2023, after the principal restrictions had eased.

3 Data, Sample, and Measurement

3.1 Linked data and firm cohort

All administrative records come from Statistics Denmark and are linked at the person and organization levels using stable register identifiers. Annual business records supply full-time-equivalent employment, employee counts, turnover, NACE industry, legal form, and firm start date. The BFL employer–employee records supply person–organization employment links, pay, hours, sector, and four-digit DISCO occupation codes. Because a person may appear at more than one organization in a year, we treat these as person–organization–year records rather than imposing a unique main job. The 2019 BEF population snapshot supplies age, sex, and immigrant-background information, while the 2019 UDDA snapshot supplies highest completed education. PERSBEST provides the formal corporate-leadership records described below.

Online vacancies come from Jobindex, Denmark's largest online job board, through Højbjerre Brauer Schultz Economics. The full Jobindex corpus spans 2006–2023. The extract used for this project covers 2010–2023, and the main linked analysis panel covers 2015–2023. Earlier administrative years supply the predetermined leadership histories. Appendix Section A documents coverage, linkage, and measurement.

The unit of observation is the firm-year, but the cohort and leadership team are defined once, in 2019. This choice avoids allowing post-shock survival, hiring, or leadership changes to determine whether a firm enters the analysis. Eligible organizations are private-sector corporate firms in the covered legal forms, with at least 9.5 full-time-equivalent workers, positive recorded turnover, and an identified 2019 CEO. Once a firm satisfies the cohort and leadership-support conditions, it remains in the fixed cohort through 2023 even if it is not observed in a later annual business record. The outcome may still be missing in a particular year when no vacancy is observed, but later firm performance does not retroactively redefine the cohort.

Formal corporate leadership comes from PERSBEST, an annual Statistics Denmark register built primarily from companies' statutory reports of active executive managers and board members. Reporting these appointments is legally required. We use the 2014–2023 annual records. PERSBEST distinguishes executive management from the formal board but may list several executive managers at one firm, so it does not by itself identify a unique CEO. A sole executive-management candidate is selected directly as CEO. When several candidates are present, the highest-paid candidate is selected using linked employer–employee pay records, and exact pay ties are resolved

consistently; firms without a usable pay comparison do not satisfy the CEO requirement. Among firms in the CEO available cohort, 98.4 percent of selected CEOs are either the sole executive-management candidate or uniquely highest paid among multiple candidates. When the selected CEO is also a formal director, that person is removed from the board roster before board digital experience is calculated.

We require the people whose histories form the experience measure to be observable before the shock. A *selected CEO* is the person chosen from the 2019 executive-management candidate pool using the rule above. A *supported leader* is observed in each annual demographic snapshot from 2014 through 2018 and is at least 30 years old in 2019. The *CEO available* sample requires a supported selected CEO but does not require support for every board member. The *CEO common* sample uses the CEO-only specification among firms whose full 2019 leadership team is supported. The *joint common* sample uses those same firms, requires a nonempty formal board, and enters CEO and board digital experience together. These conditions determine sample eligibility; they do not require positive measured external digital experience. A supported CEO with no qualifying non-focal leadership role remains in the sample with zero constructed exposure.

Table 1: Pre-shock firm, vacancy, and leadership characteristics

Panel A. Analysis cohorts

	CEO available sample		Joint common sample	
Characteristic	Mean	SD	Mean	SD
Firm and vacancy characteristics				
Full-time-equivalent employment, 2019	51.117	249.714	54.012	150.738
Firm age, 2019 (years)	19.256	15.090	21.576	15.517
Total job postings, 2019	5.564	80.432	4.864	40.300
Digital job postings, 2019	1.352	10.597	1.318	8.422
Fraction with an observed vacancy, 2019	0.426	0.495	0.465	0.499
Digital-posting share, 2019	0.263	0.362	0.273	0.362
CEO characteristics				
Observed CEO tenure at the focal firm (years)	4.736	1.716	4.797	1.718
Fraction of CEOs who are women	0.092	0.289	0.073	0.260
External CEO digital-leadership experience	0.211	0.971	0.248	1.058
Board characteristics				
Non-CEO board size	3.483	2.003	3.239	1.789
External board digital-leadership experience	–	–	0.362	0.930
Firms in the 2019 cohort	16,044		8,552	

Panel B. CEO available cohort by external CEO digital experience

	Zero constructed exposure		Positive measured exposure	
Characteristic	Mean	SD	Mean	SD
Full-time-equivalent employment, 2019	36.422	104.070	98.030	473.287
Firm age, 2019 (years)	18.864	14.056	20.507	17.941
Total job postings, 2019	3.644	58.741	11.694	126.730
Digital-posting share, 2019	0.236	0.359	0.325	0.362
WFH-posting share, 2019	0.030	0.137	0.036	0.138
CEO age, 2019 (years)	50.700	9.307	51.881	8.166
Fraction of CEOs who are women	0.100	0.300	0.067	0.250
Observed CEO tenure at the focal firm (years)	4.878	1.650	4.283	1.839
Firms in the 2019 cohort	12,217		3,827	

Notes: The table reports 2019 firm-level means and standard deviations. Panel A compares the analysis cohorts. Panel B divides the CEO available cohort by positive measured exposure. Zero constructed exposure can reflect no qualifying external role, observed zero digital intensity, or an incompletely measured external history. Experience covers 2014–2018 outside the focal firm and is measured in annual seventy-fifth-percentile-equivalent units. CEO tenure covers 2014–2019 and is left-censored. Posting shares require an observed 2019 vacancy; in Panel B they use 4,800 firms with zero constructed exposure and 2,041 with positive measured exposure. Board measures are for the joint common cohort. Dashes denote unreported measures.

Table 1 describes the two 2019 cohorts before the outcome-specific vacancy restriction is applied. The CEO available cohort contains 16,044 firms and the joint common cohort 8,552. The typical firm is not large: mean employment is 51 full-time equivalents in the broader cohort and 54 in the joint common cohort. The large standard deviations show that both samples nevertheless contain a right tail of much larger employers. Firms in the joint common cohort are somewhat older, at 21.6 years on average compared with 19.3 years in the broader sample.

Panel B gives a simple treatment-group comparison within the CEO available cohort. Positive measured external CEO digital experience is observed for 23.9 percent of firms. These firms are larger, advertise more vacancies, and have a higher digital-posting share in 2019. Differences in CEO characteristics are smaller: CEOs in the positive-measured group are about 1.2 years older, are 3.3 percentage points less likely to be women, and have 0.6 fewer observed years at the focal firm. The mean WFH-posting share differs by only 0.6 percentage points, with a two-sided p-value of 0.089. The empirical design uses changes within firms, so these pre-shock level differences are absorbed by firm fixed effects.

Not every firm recruits or posts a vacancy online in every year. In 2019, 42.6 percent of CEO available firms and 46.5 percent of joint common firms have at least one observed vacancy. Among firms with a posting, the mean digital-posting share is 26.3 percent in the broader cohort and 27.3 percent in the joint common cohort. The main WFH-share regressions use 10,913 CEO available firms and 6,319 CEO common or joint common firms that contribute at least one firm-year with an observed posting over 2015–2023. Count regressions can use the full cohort because a firm-year with no observed posting is assigned a zero count rather than an undefined share.

The leadership statistics provide two useful benchmarks. Observed CEO tenure at the focal firm averages about 4.7 years, although the measure is left-censored by the available history. Women account for 9.2 percent of selected CEOs in the broader sample and 7.3 percent in the joint common sample. Mean external CEO digital experience is 0.211 benchmark-equivalent years in the broader cohort and 0.248 in the joint common cohort. Its standard deviation is 0.971 and 1.058, respectively. The one-unit effect reported in the results is therefore close to a one-standard-deviation contrast in CEO digital experience. The joint common board has 3.24 non-CEO members on average, and its mean external board digital experience is 0.362 with a standard deviation of 0.930.

3.2 External digital leadership experience

Let W_{js} be the number of workers in broad digital occupations at organization j in year s, divided by all workers observed at that organization. The classification uses four-digit DISCO codes aligned with the International Standard Classification of Occupations. It covers code 1330 and the ranges 2500–2599 and 3500–3599, corresponding to digital managers, professionals, and technicians. This is an employee-occupation measure, not a job-posting measure. Let $q_{.75,s}$ be the seventy-fifth percentile of W_{js} among organization-years with a positive digital-workforce share. Normalized

digital intensity is

$$R_{js} = \frac{W_{js}}{q_{.75,s}}. \tag{1}$$

An organization at the annual benchmark has $R_{js} = 1$. Because the benchmark population excludes organization-years with zero digital workers, the cutoff is the top-quartile threshold within the positive-digital group, not within all firms. An organization-year with an observed workforce and no digital workers has zero intensity and does not determine the percentile used for scaling. The number of private firms above the threshold need not equal exactly one quarter of positive-digital private firms because the cutoff is calculated in the broader organization population and ties can occur. We consequently use the percentile primarily to define a unit of intensity rather than to label a fixed number of firms as treated.

Between 2.1 and 2.4 percent of private firms lie at or above the annual cutoff in a given year. Among private firms with a positive digital-workforce share, the corresponding range is 26.3 to 29.5 percent. Across 2014–2018, 3.5 percent of private firms ever reach the cutoff; among firms that ever employ a digital worker, the share is 31.2 percent. A firm that reaches the cutoff does so for 2.3 years on average. Thus the benchmark describes a selective but recurring set of digitally intensive organizations rather than one quarter of the entire private-firm population. Appendix Table A1 reports the annual counts and cutoffs.

For leader i and focal firm f, let $\mathcal{J}_{is}(f)$ contain the other organizations at which the person held a qualifying leadership position in year s and for which digital-workforce intensity is measured. An external role may be held before or at the same time as the focal-firm role; only organization identity matters. Annual external experience averages intensity across simultaneous measured organizations and uses zero when none is measured,

$$e_{is}^{\text{ext}}(f) = \begin{cases} \dfrac{1}{|\mathcal{J}_{is}(f)|} \displaystyle\sum_{j \in \mathcal{J}_{is}(f)} R_{js}, & |\mathcal{J}_{is}(f)| > 0, \\ 0, & |\mathcal{J}_{is}(f)| = 0. \end{cases} \tag{2}$$

Because $\mathcal{J}_{is}(f)$ contains only measured organization-years, an empty set can mean that the leader held no qualifying external role or that all qualifying external organization-years were unmeasured. Both receive zero annual external experience under the baseline construction. An observed external organization-year with no digital workers also contributes zero. When measured and unmeasured organizations occur in the same person-year, the average uses the measured intensities. Averaging across simultaneous organizations prevents one calendar year from being counted several times merely because a leader holds several positions. The annual value can exceed one when the organizations are more digitally intensive than the benchmark. The frozen stock is

$$E_i^{\text{ext}}(f) = \sum_{s=2014}^{2018} e_{is}^{\text{ext}}(f). \tag{3}$$

One unit is therefore one year at the annual benchmark. A leader who spends all five years at the benchmark accumulates five units. CEO exposure is the selected 2019 CEO's stock. Board exposure is the equal-weighted mean across supported members of the 2019 non-CEO formal board, including members with zero constructed exposure. Equal weighting gives each formal director the same role in the board measure and prevents large boards from receiving more exposure simply because they contain more people. Appendix Section A gives further details on the denominator, qualifying roles, simultaneous positions, and zero histories.

High-intensity external exposure is itself uncommon among CEOs: 4.2 percent of CEOs in the CEO available cohort held a qualifying external leadership role at a private organization at or above the benchmark the five years prior to COVID-19. Boards aggregate digital experience across several people, so 24.5 percent of joint common boards contain at least one member with such external exposure. These descriptive indicators are not the continuous treatment used in the regressions, but they help locate a benchmark-equivalent year in the observed leadership histories.

3.3 Online vacancies and WFH-posting outcomes

The vacancy file includes posting-level classifications derived from the advertisement text. The digital-recruiting extensions use seven supplied skill categories: computer support and networking, data analysis, digital design, generative AI, AI, digital marketing, and programming or software. A posting is classified as digital when at least one of these categories is positive; the AI indicator uses the AI or generative-AI categories. These vacancy-side classifications construct the digital- and AI-vacancy outcomes and predetermined vacancy-readiness controls. They do not enter the register-based leadership-experience measure. Appendix Section A.4 describes the construction and use of these vacancy-side classifications.

Let H_{ft} be the number of observed advertisements classified as permitting work from home and A_{ft} all observed advertisements for firm f in year t. The main outcome is

$$S^H_{ft} = \frac{H_{ft}}{A_{ft}}, \qquad A_{ft} > 0. \tag{4}$$

The denominator includes all observed vacancies, whether digital or not. If a firm posts ten vacancies and two advertise a home-work option, its WFH-posting share is 0.20. The share is missing in firm-years with no observed vacancy because there is no recruiting mix to describe. The baseline share estimand is therefore conditional on observed online recruiting. Count outcomes retain those firm-years as zeros. Section 6.9 examines the posting margin directly and reports WFH outcomes for every year in which a firm in the fixed 2019 cohort is in scope, including years with no observed vacancy.

We call S^H_{ft} the WFH-posting share. It measures employer-side WFH availability in newly posted jobs, not realized WFH among incumbent employees. A positive classification may represent fully remote, hybrid, or another arrangement that permits some work at home. The outcome is useful precisely because it is observed at the point when firms decide how a vacancy will be

organized and advertised. It does not tell us whether the eventual employee takes up the option or how many days are worked remotely.

4 Empirical Strategy

The treatment intensity is fixed external digital experience. Let C_f denote the frozen experience stock of firm f's 2019 CEO. The dynamic difference-in-differences specification is

$$S_{ft}^H = \alpha_f + \lambda_t + \sum_{\tau \neq 2019} \beta_\tau^C C_f \mathbf{1}\{t = \tau\} + \varepsilon_{ft}, \tag{5}$$

where α_f and λ_t are firm and calendar-year fixed effects. The omitted year is 2019, the last year before the COVID-19 shock. Standard errors are clustered by firm because CEO digital experience is fixed at the firm level while outcomes are observed repeatedly. The specification contains no additional controls; it is our baseline. For the main outcome we also report a more saturated specification with two-digit industry-by-year fixed effects (Section 6.1). Industry is taken from the firm's 2019 NACE classification and held fixed throughout the panel, so these models compare firms within predetermined industry-by-year cells. In the joint common sample, we add analogous interactions between year indicators and mean board digital experience B_f in the same regression, so the two paths are conditional on one another. The 2019 treatment intensities remain attached to the firm after any subsequent leadership change. In that limited sense, the coefficients have an intention-to-treat-style interpretation with respect to the pre-pandemic leadership cohort.

The coefficients measure changes in the cross-firm digital-experience gradient relative to 2019. Suppose, for example, that $\beta_{2023}^C = 0.02$. Two firms that differ by one benchmark-equivalent year of CEO digital experience would then be predicted to differ by two percentage points more in their 2023 WFH-posting shares than they did in 2019. This is not a comparison of the firms' levels: time-invariant differences in their average WFH shares are absorbed by firm fixed effects.

The experience stock is a continuous predetermined exposure, and the coefficients compare firms across values of that exposure. Callaway et al. (2024) analyze designs of this type: linear two-way fixed-effects summaries can be difficult to interpret even with two periods, and comparisons across exposure levels embed selection that standard parallel trends does not rule out. Because the pandemic reaches every firm, the calendar-year effects absorb its common component, and only the cross-firm gradient is estimated: the coefficients measure how post-2019 changes vary with predetermined experience, not the level effect of the shock. The binary comparison in Section 5 estimates the differential change between firms with positive measured external CEO exposure and firms with zero constructed exposure, without imposing a linear dose-response. Zero constructed exposure need not mean the absence of actual external experience because it also includes histories for which no qualifying source intensity is measured. Interpreting the difference as the effect of any experience therefore requires both the zero-experience pandemic counterfactual stated below and accurate classification of firms into that counterfactual group. The continuous gradient

uses the full exposure variation and also requires a cross-exposure restriction. We present the binary comparison in the main results and revisit it among the alternative treatment definitions in Section 6; the continuous specification carries the remaining analysis.

The pooled difference-in-differences specification replaces the annual interactions with indicators for 2020–2021 and 2022–2023; the omitted period is 2015–2019. The two post periods distinguish the emergency and early-adjustment years from the later period, when WFH was less likely to be a short-lived response to restrictions. We report both forms because they answer different questions: the annual path provides the timing evidence, while the pooled coefficients give more precise and compact magnitude estimates.

Two counterfactual restrictions are relevant for causal interpretations of the specifications (Callaway et al., 2024). For the binary comparison, the parallel-trends condition concerns a zero-experience counterfactual in the actual pandemic environment: firms whose 2019 CEO had positive measured external digital leadership experience would, had that CEO instead entered the pandemic with none, have followed the average WFH-posting path of firms assigned zero constructed exposure. This condition rules out other pre-2020 attributes correlated with experience that independently produce a different response to COVID-19; parallel paths absent COVID-19 would not suffice. The continuous gradient requires an additional cross-exposure condition: at any common experience level, firms observed at different points of the experience distribution would have had the same average pandemic response, so that comparisons across levels reflect the response to additional experience rather than selection into experience. Dichotomizing exposure relaxes the linear dose-response restriction but does not remove the between-group counterfactual requirement. Firm fixed effects absorb differences in average WFH shares and other time-invariant firm characteristics, but they do not absorb differential pandemic responses associated with those characteristics. Both causal readings also require that firms did not select their 2019 leaders in anticipation of the unexpected pandemic. A causal reading of the conditional WFH-share specification further requires that shock-induced changes in vacancy observation do not differentially alter the firm-years in which the share is defined. Outcomes that retain fixed-cohort firms in in-scope years with no observed vacancy reduce reliance on that positive-posting restriction but target different estimands.

Several features of the analysis probe specific threats to these counterfactual conditions. The annual pre-period coefficients describe whether the gradients were already changing before the shock, and they are flat. The placebo freezes an earlier leadership cohort and asks whether an artificial 2018 break produces the same pattern. It yields mixed evidence: several placebo-period tests are marginal, with negative estimates that move opposite to the post-2020 path (Appendix Table A8). Neither exercise establishes the zero-experience pandemic counterfactual or the stronger cross-exposure condition, because an attribute correlated with experience could first express itself when the pandemic arrived. The industry-by-year specifications in Section 6.1 address industry-specific pandemic responses, one important component of that concern, but not leader–firm selection within industries. The occupation decomposition and occupation-by-year

estimates ask whether the WFH gradient is instead created by changing job mix. Outcomes that include firm-years with no observed vacancy reduce reliance on an observed-vacancy denominator, while the restricted-timing exposure asks whether external role-years observed only after the first observed focal-role year account for the pattern. The same-CEO and continuous-presence restrictions condition on post-2019 outcomes that the exposure could in principle affect, so we read them as descriptive stability checks rather than identification evidence. These exercises narrow specific alternative explanations; they do not establish the counterfactual conditions above. They also do not randomize leader–firm matching, which is why any causal reading concerns the value of entering the shock with different predetermined experience rather than the effect of appointing a particular leader.

5 Main Results

We begin with the binary comparison. It describes the differential post-2019 path between firms with positive measured and zero constructed external CEO digital experience without imposing a linear dose-response; interpreting that difference as the effect of any experience requires the zero-experience pandemic counterfactual in Section 4. The indicator equals one when the selected CEO has positive measured external digital leadership experience. Figure 1 presents the event study before the pooled estimates. The pre-2019 coefficients show no systematic divergence: their joint-test p-values are 0.569 in the CEO available sample and 0.549 in the CEO common sample. The paths separate after 2019. In the broader sample, the difference rises from 0.8 percentage points in 2020 to 1.4 points in 2021, 2.5 points in 2022, and 3.3 points in 2023; the CEO common path is slightly larger and follows the same progression.

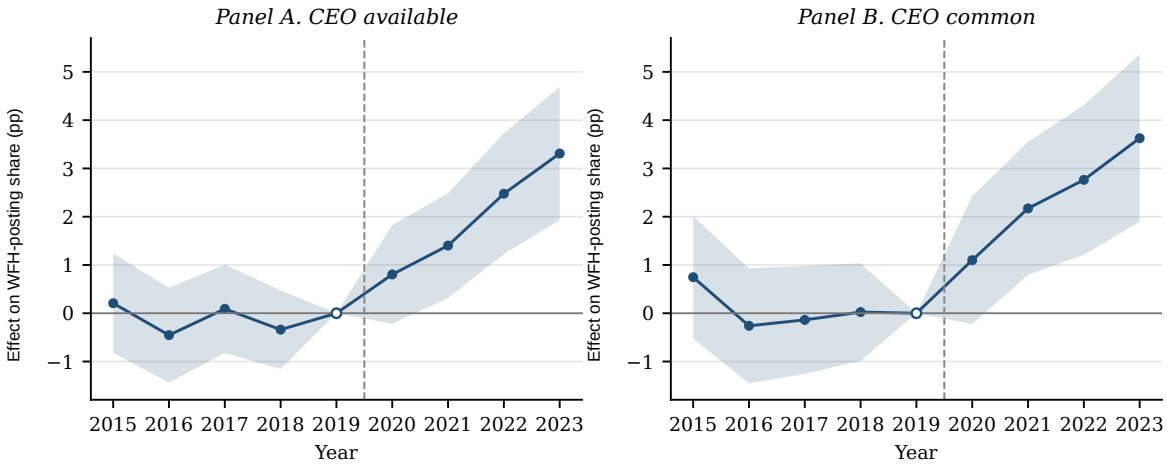

Figure 1: Positive measured external CEO digital experience and the WFH-posting share

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of an indicator for positive measured external CEO digital experience with calendar year, relative to 2019. Panel A uses the CEO available sample and Panel B the CEO common sample. The panels use a common y-axis. Shaded regions are 95 percent confidence intervals. Specifications include firm and calendar-year fixed effects and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line separates 2019 from 2020.

Table 2 summarizes the binary comparison. Firms whose selected CEO has positive measured external digital leadership experience have a WFH-posting share that rises by 1.2 percentage points more in 2020–2021 and 3.0 points more in 2022–2023 than firms assigned zero constructed exposure. The later difference is almost as large as the 3.1-percent pre-2020 mean. Because zero constructed exposure can also reflect incomplete source histories, this is not literally a comparison of actual experience with its absence. The indicator treats a small amount of measured external experience the same as several years at highly digital organizations, so the continuous specification remains our headline measure of how the response varies with the measured experience stock. Collapsing intensity to an indicator removes the linear dose-response restriction and the additional regression leverage of unusually high exposure values on the fitted slope, and a post-2019 difference remains visible under that coding. It does not show that the divergence is invariant to functional form. Firms led by extremely experienced CEOs remain in the exposed group, so the binary exercise also does not establish that those firms are collectively uninfluential.

Figure 2 reports the continuous event study for our main CEO-available estimation sample; support for every board member is not required. Every point is expressed in percentage points relative to 2019. The confidence band is a pointwise 95 percent interval. Pre-period tests are discussed in the text rather than printed in the graph.

Figure 2 shows a sharp change in the CEO-experience gradient after 2019. The four pre-period estimates range from -0.23 to 0.31 percentage points and alternate in sign. They are jointly equal to zero with $p = 0.144$. The post-period coefficients are all positive. The estimate is 0.84 percentage points in 2020 (s.e. 0.37), rises to 1.54 points in 2021 (s.e. 0.32), and reaches 2.36 and

Table 2: Positive measured external CEO digital experience and the WFH-posting share

	CEO available	CEO common
Positive measured external CEO digital experience × 2020–2021	0.012***	0.016***
	(0.004)	(0.005)
Positive measured external CEO digital experience × 2022–2023	0.030***	0.031***
	(0.005)	(0.007)
Pre-2020 outcome mean	0.031	0.030
Firms	10913	6319
Observations	59265	35096

Notes: The dependent variable is the WFH-posting share. The exposure equals one when the selected 2019 CEO has positive measured external digital experience during 2014–2018. The comparison group has zero constructed exposure, which can reflect no qualifying external role, observed zero digital intensity, or an incompletely measured external history. Coefficients are interactions with the indicated period; the omitted period is 2015–2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

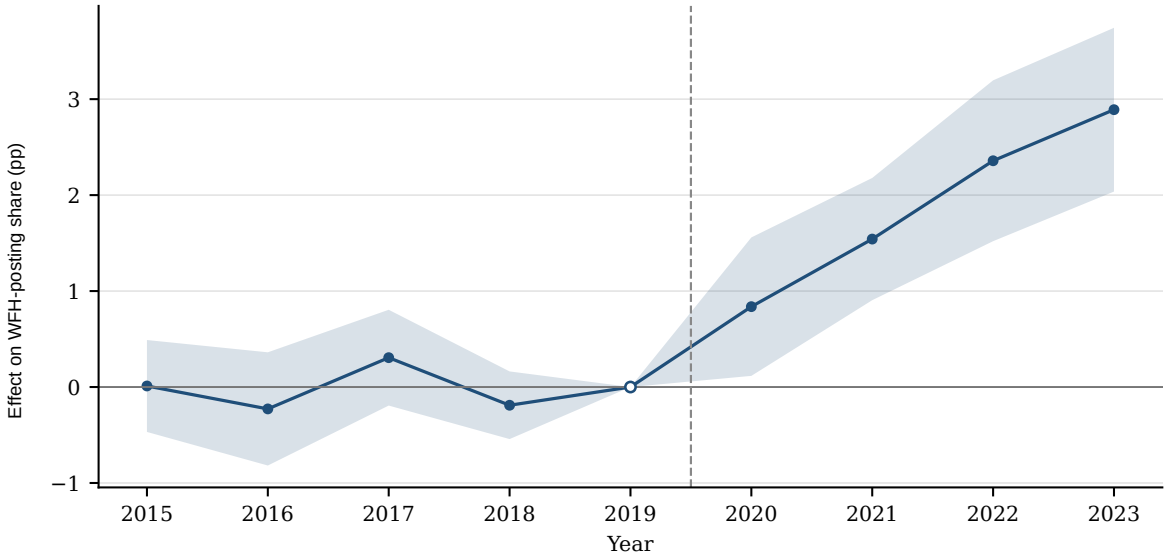

Figure 2: External CEO digital experience and the WFH-posting share

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of external CEO digital experience with calendar year, relative to 2019, in the CEO available sample. Shaded regions are 95 percent confidence intervals. The specification includes firm and calendar-year fixed effects and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line separates 2019 from 2020.

2.89 points in 2022 and 2023. The confidence interval excludes zero in every post year.

The trajectory is economically informative. If the result reflected only a temporary ability to comply with emergency restrictions, one might expect the coefficient to be largest in 2020 and then decline. The opposite occurs. The association grows as firms move from emergency arrangements to later hiring decisions. A natural reading is that leaders with external digital experience were more likely to make WFH part of the firm's continuing employment offer. The evidence does not reveal whether that persistence comes from better technology choices, confidence about monitoring, knowledge of suitable tasks, or a greater willingness to experiment.

Figure 3 then holds the sample fixed while changing the specification. Panel A shows external CEO digital experience alone in the CEO common sample. Panel B enters external CEO and board digital experience together in the joint common sample, which contains exactly the same firms. Both panels use a common y-axis, so differences across the two specifications are visually comparable.

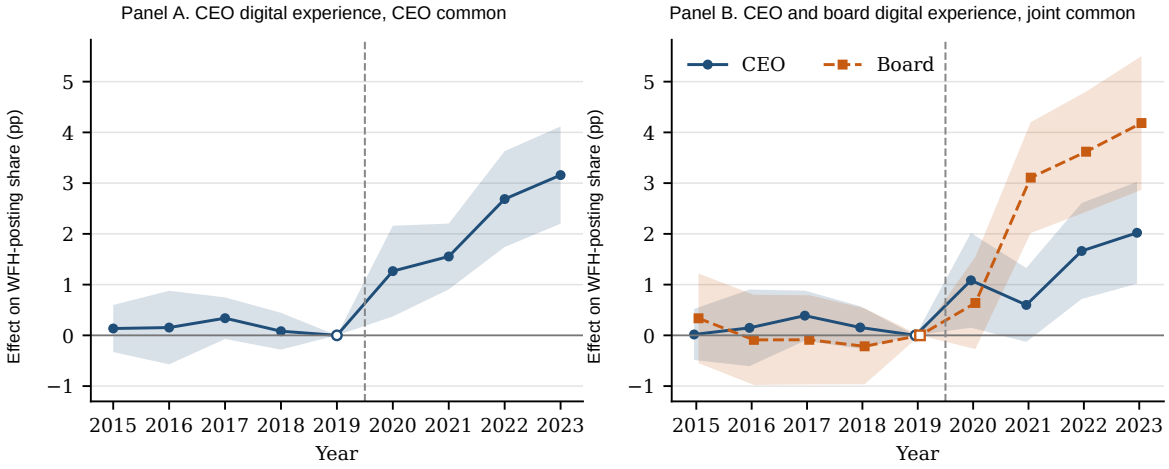

Figure 3: External CEO and board digital experience and the WFH-posting share in the common samples

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of external digital leadership experience with calendar year, relative to 2019. Panel A estimates external CEO digital experience alone in the CEO common sample. Panel B estimates external CEO and board digital experience jointly in the joint common sample, which contains the same firms. Both panels use the same y-axis. Shaded regions are 95 percent confidence intervals. Specifications include firm and calendar-year fixed effects and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line separates 2019 from 2020.

The CEO common path closely resembles Figure 2; its pre-period test has $p = 0.563$. In the joint common specification, the CEO path turns positive immediately: 1.08 percentage points in 2020, 0.60 in 2021, 1.66 in 2022, and 2.02 in 2023. The 2021 estimate is imprecise, but the 2020, 2022, and 2023 intervals exclude zero. The board path has a different shape. Its 2020 estimate is 0.64 points and does not exclude zero; it then rises to 3.11 points in 2021, 3.62 in 2022, and 4.18 in 2023. The separate pre-period tests have $p = 0.555$ for the CEO series and $p = 0.777$ for the board series, while the joint test has $p = 0.788$.

These estimates do not imply that boards are more important than CEOs. The board measure is an average across directors, its variation differs from the CEO measure, and the two roles operate through different decisions. What the joint specification shows is narrower: board digital experience contains information about post-2019 WFH-posting availability beyond the digital experience of the selected CEO. The later rise in the board coefficient is consistent with a role in evaluating or institutionalizing new practices, but the timing alone cannot identify that mechanism. Board seats also create interlock networks along which practices and technologies travel between connected firms (Li, 2021; Cabezon and Hoberg, 2026), and because our external roles may be concurrent with the focal position, the board estimates may partly reflect information flowing through directors' current networks rather than experience the directors accumulated. Our data cannot separate these channels; we read the board results as evidence that directors' external digital exposure predicts subsequent WFH advertising, whichever of the two carries it.

Table 3: External CEO and board digital experience and the WFH-posting share

	CEO available	CEO common	Joint common	
	CEO	CEO	CEO	Board
External digital experience × 2020–2021	0.012***	0.013***	0.007**	0.020***
	(0.002)	(0.003)	(0.003)	(0.004)
External digital experience × 2022–2023	0.026***	0.028***	0.017***	0.039***
	(0.003)	(0.004)	(0.004)	(0.005)
Pre-2020 outcome mean	0.031	0.030	0.030	0.030
Firms	10913	6319	6319	6319
Observations	59265	35096	35096	35096
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Notes: The dependent variable is the share of a firm's vacancies that permit work from home. The coefficient rows are interactions of continuous external CEO or board digital experience, scaled in benchmark-equivalent years, with the indicated period. The omitted period is 2015–2019, so the coefficients report changes in the experience gradient relative to that period. Columns differ only in sample and whether CEO and board digital experience enter jointly. Standard errors clustered by firm are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3 pools the annual estimates into two post periods. Column 1 uses 59,265 firm-year observations from 10,913 firms. A one-unit increase in external CEO digital experience predicts a 1.2-percentage-point increase in the WFH-posting share during 2020–2021 (s.e. 0.2 points) and a 2.6-point increase during 2022–2023 (s.e. 0.3 points). Both are statistically different from zero at the 1 percent level. Relative to the 3.1-percent pre-2020 mean, the estimates correspond to roughly 39 percent in the early period and 84 percent in the later period. Because one unit is also close to one standard deviation of CEO digital experience, these are useful magnitudes rather than effects of an extreme change in the treatment variable.

Column 2 answers a sample-composition question. It re-estimates the CEO-only specification on the 6,319 CEO common firms for which the CEO and entire formal board have common support.

The coefficients are 1.3 and 2.8 percentage points, compared with 1.2 and 2.6 in the broader sample. The CEO common pre-period mean is 3.0 percent, so the later coefficient is about 93 percent of that mean. The similarity across columns shows that the CEO result is not sensitive to restricting the sample to firms with a supported board roster. It also allows the difference between columns 2 and 3 to be interpreted as the consequence of adding board digital experience to the specification, rather than changing the set of firms.

Columns 3 and 4 enter the two leadership measures jointly. The CEO coefficient falls to 0.7 points in 2020–2021 and 1.7 points in 2022–2023. Board experience predicts 2.0 and 3.9 points. All four coefficients remain positive; the early CEO estimate is statistically different from zero at the 5 percent level, and the other three at the 1 percent level. Part of the CEO-only association is therefore shared with experience elsewhere in the leadership team, but it does not disappear. The data are most consistent with digital experience at both levels of leadership predicting the adoption of WFH, with the board gradient becoming especially large in the later period.

6 Robustness

The robustness exercises address distinct questions about the main result. Interactions between calendar year and observed pre-pandemic firm and leader characteristics ask whether the gradient is sensitive to differential pandemic responses associated with firm capacity. Industry and occupation controls ask whether it reflects differential pandemic shocks or a change in the jobs that firms advertise rather than a change in WFH terms within jobs. Alternative experience measures examine the treatment definition, its timing relative to the first observed focal-role year, and whether the source organizations advertised WFH before the pandemic. The pre-pandemic placebo asks whether the same design produces a comparable break before COVID-19. Leadership-continuity and firm-support restrictions describe whether the association remains among firms that retain their selected CEO or continue to appear in the administrative records. Requiring fully measured external histories addresses missing workforce-composition information. Finally, vacancy-support and complete-cohort exercises examine whether the share result is generated by low-volume recruiters or by selection into observed online vacancies. Each exercise changes the feature needed to isolate one concern while retaining the fixed 2019 leadership cohort.

6.1 Industry-specific pandemic shocks

Industries differed sharply in both the feasibility of WFH and the pressure to adopt it during the pandemic. If digitally experienced leaders were concentrated in industries with larger WFH shocks, the baseline year effects would not absorb that source of differential change. We therefore add two-digit industry-by-year fixed effects. Each firm remains assigned to the two-digit NACE industry recorded in 2019, even if its registered industry changes later. The specification thus compares firms within predetermined 2019-industry-by-year cells.

Table 4 shows substantial attenuation, but the later-period results remain. In the CEO common

Table 4: WFH-posting share with detailed industry-by-year fixed effects

	CEO common		Joint common	
	Baseline	Industry × year	Baseline	Industry × year
External CEO digital experience × 2020–2021	0.013***	0.005	0.007**	0.003
	(0.003)	(0.003)	(0.003)	(0.003)
External CEO digital experience × 2022–2023	0.028***	0.013***	0.017***	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)
External board digital experience × 2020–2021			0.020***	0.010**
			(0.004)	(0.004)
External board digital experience × 2022–2023			0.039***	0.021***
			(0.005)	(0.006)
Pre-2020 outcome mean	0.030	0.030	0.030	0.030
Firms	6,319	6,310	6,319	6,310
Observations	35,096	35,035	35,096	35,035
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Detailed industry × year fixed effects	No	Yes	No	Yes

Notes: The dependent variable is the WFH-posting share. Two-digit NACE industry is measured from the firm's 2019 business-register record and held fixed throughout the panel. The detailed specifications therefore compare firms within the same predetermined 2019-industry and calendar-year cell. The CEO and board measures enter jointly in columns 3 and 4. Standard errors clustered by firm are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

specification, the 2020–2021 coefficient falls from 1.3 to 0.5 percentage points and is no longer statistically different from zero. The 2022–2023 coefficient falls from 2.8 to 1.3 points and remains statistically different from zero at the 1 percent level. In the joint specification, the corresponding CEO estimates fall from 0.7 to 0.3 points and from 1.7 to 0.9 points. The board estimates fall from 2.0 to 1.0 points in the early period and from 3.9 to 2.1 points in the later period; both remain statistically different from zero. The attenuation indicates that industry-specific shocks account for part of the baseline association. The remaining later coefficients document that the CEO and board relationships are also present within predetermined 2019 industries. These within-industry estimates address industry-specific pandemic responses, one important component of the cross-exposure concern in Section 4; they do not establish the causal counterfactual or address leader–firm selection within industries.

6.2 Observed pre-pandemic firm capacity

Firms with more digitally experienced leaders differ in observable ways before the pandemic. We therefore interact calendar year with 2019 log employment, log sales, and firm age; pre-2020 vacancy activity, WFH history, digital-posting levels and trends; and CEO characteristics. The joint model also includes board size, mean board age and tenure, and board ownership. Continuous controls are accompanied by year-interacted missing indicators; continuous controls and categorical leader characteristics are interacted with year. The three comparisons reported in Appendix

Table A6 use one fixed estimation sample within each panel.

Appendix Table A6 reports the pooled results. In the CEO available sample, the 2020–2021 coefficient falls from 1.25 percentage points on the fixed sample to 0.96 points with all observed characteristics and to 0.40 points when fixed-2019 two-digit industry-by-year effects are also included. The 2022–2023 coefficient falls from 2.63 to 1.71 and then 0.78 points; the final estimate remains statistically different from zero at the 5 percent level. The CEO common estimates follow the same pattern. In the joint model, the later board coefficient falls from 3.87 to 2.96 and then 1.73 points and remains statistically different from zero at the 1 percent level. The corresponding conditional CEO coefficient falls from 1.66 to 1.12 and then 0.53 points and is no longer statistically detectable in the final specification.

These controls absorb differential pandemic responses associated with a broad set of observed starting conditions. They do not address unobserved capacity or nonrandom leader–firm matching. Moreover, the 2015–2019 WFH history, the 2015–2019 rate of years with any observed vacancy, and the 2017–2019 posting-volume and digital histories may already reflect decisions made while the selected leader was at the firm. We therefore view the sequence as a demanding sensitivity analysis rather than a preferred causal specification. The attenuation shows that the baseline relationship is sensitive to observed capacity and industry-specific shocks; the later CEO-only and board coefficients nevertheless remain positive and statistically detectable.

6.3 Occupational composition and within-occupation changes

A higher firm-level WFH-posting share could arise without a change in how otherwise comparable jobs are organized if firms led by more digitally experienced leaders shift recruitment toward occupations in which WFH was already more common. We address this possibility in two ways. First, we hold occupation-specific WFH propensities fixed at their 2015–2019 values and decompose each firm-year WFH share into the part predicted by its occupational mix and a residual net of that component. Second, we estimate vacancy-level models with firm and four-digit occupation-by-year fixed effects, which compare WFH mentions among vacancies in the same occupation and year.

The fixed-preperiod decomposition in Table 5 assigns little of the headline relationship to shifts toward occupations that were already more WFH-intensive during 2015–2019. In the CEO available sample, the composition component is -0.9 percent of the observed 2020–2021 coefficient and -1.7 percent of the 2022–2023 coefficient. Removing it changes the reported coefficients only from 0.012 to 0.012 and from 0.026 to 0.027. In the joint sample, the composition shares range from -0.4 to -2.1 percent across the CEO and board coefficients. At the reported point estimates, changes in occupational mix therefore slightly offset, rather than explain, the component valued using fixed pre-pandemic occupation-specific WFH propensities.

The direct within-occupation estimates answer a stricter question and are more mixed. In the raw-vacancy CEO available specification, the coefficients are smaller at 0.002 in 2020–2021 and 0.007 in 2022–2023 and are not statistically distinguishable from zero. On common support,

Table 5: Occupation composition and within-occupation estimates

	Observed	Fixed-pre composition share (%)	Net of composition	Within occupation × year raw-vacancy cohort
Panel A. CEO available				
External CEO digital experience × 2020–2021	0.012***	−0.9	0.012***	0.002
	(0.002)		(0.002)	(0.002)
External CEO digital experience × 2022–2023	0.026***	−1.7	0.027***	0.007
	(0.003)		(0.003)	(0.006)
Firms	10,913	10,913	10,913	11,499
Observations	59,265	59,265	59,265	797,593
Panel B. Joint common				
External CEO digital experience × 2020–2021	0.007**	−0.4	0.007**	−0.002
	(0.003)		(0.003)	(0.003)
External CEO digital experience × 2022–2023	0.017***	−1.4	0.017***	0.009**
	(0.004)		(0.004)	(0.005)
External board digital experience × 2020–2021	0.020***	−0.6	0.020***	0.012***
	(0.004)		(0.004)	(0.003)
External board digital experience × 2022–2023	0.039***	−2.1	0.039***	0.034***
	(0.005)		(0.005)	(0.005)
Firms	6,319	6,319	6,319	6,610
Observations	35,096	35,096	35,096	387,736
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Absorbed
Occupation × year fixed effects	No	No	No	Yes

Notes: Columns 1–3 decompose each firm-year WFH-posting share using occupation-specific WFH propensities fixed over 2015–2019. The propensity is leave-one-firm-out where supported; cells without other preperiod vacancies use the inclusive occupation rate and, if that is also unavailable, the overall preperiod rate. Column 2 reports the estimated composition component as a percentage of the corresponding observed coefficient; negative values mean that occupational changes slightly offset the observed relationship. No standard error is reported for this coefficient ratio. Column 3 is the WFH-posting share net of the fixed-preperiod component. Column 4 begins with raw vacancy records matched directly to the fixed 2019 leadership cohort and estimates a frequency-weighted firm–occupation–year regression that is numerically equivalent to a vacancy-level linear-probability model, with firm and four-digit occupation-by-year fixed effects. Columns 1–3 instead use the linked conditional firm-year WFH-share panel. Column 4 therefore includes 586 additional CEO-available firms and 291 additional joint-common firms; its differences from Columns 1–3 combine occupation-by-year adjustment with changes in unit, weighting, and sample support. CEO and board experience enter jointly in Panel B. Standard errors clustered by firm are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

however, the later CEO coefficient remains positive at 0.009 (s.e. 0.005) when CEO and board experience enter jointly. The corresponding board coefficients are 0.012 (s.e. 0.003) in 2020–2021 and 0.034 (s.e. 0.005) in 2022–2023. Column 4 begins with raw vacancy records matched directly to the fixed leadership cohort, whereas Columns 1–3 use the linked conditional firm-year WFH-share panel. It consequently includes 586 additional CEO-available firms and 291 additional joint-common firms. Differences from Columns 1–3 therefore combine occupation-by-year adjustment with changes in unit, weighting, and sample support; they are not the effect of occupation controls alone. The vacancy-level estimates provide within-occupation-year support for the board pattern and the later CEO pattern on common support, but the smaller CEO available estimates caution against claiming that every result is invariant to occupation and support choices. Neither exercise removes endogenous leader–firm matching or other firm-specific pandemic shocks.

6.4 Alternative definitions and timing of digital experience

The preferred measure makes two choices: it excludes experience at the focal firm and retains variation in both the duration and intensity of digital exposure. External roles, however, need not precede the leader's focal role. We vary the scope, functional form, and observed timing of exposure to learn whether the estimates depend on one narrow treatment definition. Comparing external with total experience tests the exclusion of the focal firm. The binary measures ask whether a post-2019 difference remains visible after exposure intensity is collapsed to an indicator, which removes the additional influence of unusually high treatment values on the fitted slope. A restricted-timing measure then retains only external role-years preceding the leader's first observed focal-role year.

Table 6: External and total CEO digital experience and the WFH-posting share

	External experience	Total experience
CEO digital experience × 2020–2021	0.012***	0.012***
	(0.002)	(0.002)
CEO digital experience × 2022–2023	0.026***	0.027***
	(0.003)	(0.002)
Pre-2020 outcome mean	0.031	0.031
Firms	10913	10913
Observations	59265	59265

Notes: Regressions use the CEO available sample. The dependent variable is the WFH-posting share. External CEO digital experience excludes the focal 2019 firm; total CEO digital experience includes qualifying experience at the focal firm and elsewhere. Coefficients are interactions with the indicated period. Models include firm and calendar-year fixed effects and firm-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The distinction between external and total CEO digital experience changes the interpretation more than it changes the result. In the CEO available sample, total CEO digital experience predicts a 1.2-percentage-point increase in 2020–2021 and a 2.7-point increase in 2022–2023, compared with

1.2 and 2.6 points for external CEO digital experience. Table 6 reports this direct comparison. The annual total-experience series is for the CEO common sample, not the CEO available sample. In that sample its pre-period test has $p = 0.917$, and its post-2019 path is similar to the external-experience path (Appendix Figure A1). External exposure is not mechanically a measure of short focal-firm tenure: a long-tenured focal CEO can accumulate it through simultaneous roles elsewhere, while a recently appointed CEO can have none. It can still covary with mobility, outside roles, and leader–firm matching; Section 7 examines focal-firm tenure directly. For this exercise, the relevant finding is that adding digital experience accumulated inside the focal firm barely changes the estimated WFH gradient.

Two extensive-margin exercises provide complementary descriptive comparisons. The binary external measure introduced in the main results is reported in Figure 1 and Table 2. Its pre-period p-values are 0.569 in the CEO available sample and 0.549 in the CEO common sample, followed by a growing post-2019 difference. A broader indicator records whether a leader ever held a qualifying role at an organization with any positive digital employment, without preserving either the intensity of digital employment or the distinction between focal and external experience. This definition classifies 46.9 percent of CEOs in the CEO-available WFH-share sample as exposed. It predicts a 1.8-point increase in 2020–2021 and a 5.4-point increase in 2022–2023, with a pre-period test of $p = 0.585$. Appendix Table A4 and Appendix Figure A3 report the CEO-only and joint CEO–board versions of this broader measure.

Together, the alternative definitions show that a post-2019 difference is also visible when focal-firm experience is excluded and when exposure intensity is collapsed to an indicator. The indicator specifications do not establish the linear dose-response form or rule out an important collective contribution from firms with high exposure.

External roles can overlap with the leader's focal role, so the baseline measure may combine earlier experience with exposure through contemporaneous interlocks. We therefore reconstruct digital experience using only external role-years preceding the leader's first observed year as CEO of the focal firm. Because the leadership history begins in 2014, this restriction excludes leaders first observed in the focal role in 2014; it does not recover appointment dates. Among the 3,193 CEO-available firms with a first observed focal-role year after 2014 and measured prior source intensity, the original exposure measure predicts increases of 1.6 and 3.1 percentage points in 2020–2021 and 2022–2023. On exactly the same firms, the restricted-timing measure predicts increases of 2.1 and 3.6 points. The corresponding CEO-common estimates are also close: 1.7 and 3.2 points using the original measure, compared with 2.0 and 3.7 points using the restricted-timing measure. The annual restricted-timing specification has a pre-period test of $p = 0.508$ and is statistically different from zero from 2021 onward. Appendix Table A3 and Appendix Figure A2, Panel A, report the pooled and annual estimates. This selected-sample exercise shows that the measured CEO gradient remains when the exposure variable counts only external role-years preceding the first observed focal-role year. Because CEOs with concurrent external roles remain in the sample and concurrent exposure is not held fixed, the exercise does not

rule out correlated contemporaneous interlocks as an explanatory channel. Nor does it eliminate nonrandom leader–firm matching or establish that knowledge was transferred.

Digital-workforce intensity does not by itself establish that leaders encountered WFH at their source organizations. A second exercise therefore replaces that intensity with each source organization's pooled 2014–2018 share of vacancies mentioning WFH, aggregates the measure over the leader's external-role history, and standardizes it within the comparison sample. In the source-linked CEO-available sample, a one-standard-deviation difference in source organizations' WFH advertising predicts a 0.2-percentage-point increase in 2020–2021 (s.e. 0.2 points) and a 0.8-point increase in 2022–2023 (s.e. 0.4 points). The annual relationship reaches 1.1 points in 2023, while the pre-period test gives $p = 0.600$. The corresponding CEO-common estimates are smaller, at 0.2 and 0.4 points, and neither is statistically distinguishable from zero. Appendix Table A3 and Appendix Figure A2, Panel B, report the pooled and annual estimates. We therefore treat this as suggestive evidence of a later relationship in the broader source-linked sample, not as evidence establishing capability transfer. Leaders were not randomly matched to firms, and the source-WFH measure captures a source organization's pooled advertising propensity rather than the practice encountered in each exact role-year.

6.5 Pre-pandemic placebo

Although the main event-study coefficients are flat before 2019, we also report an earlier-cohort pre-pandemic falsification. Figure 4 selects the 2017 leadership cohort, accumulates person-level external digital experience during 2014–2016, omits 2017, and treats 2018–2019 as an artificial post period. Assignment from the 2017 leadership records makes the firm-level exposure predetermined relative to the artificial 2018 break. Because the exercise uses a shorter exposure window and different eligibility and support rules than the main design, it asks whether an earlier-cohort implementation reproduces the positive and steadily increasing post-2019 path; it is not a pure retiming of an otherwise unchanged specification.

Figure 4 does not reproduce the main result, but the placebo is not uniformly null. In the CEO available sample, the 2018 coefficient is -0.56 percentage points (s.e. 0.25) and is statistically different from zero at the 5 percent level. The reported $p = 0.068$ instead tests the joint null that the 2018 and 2019 CEO interactions are both zero. In the joint common sample, the 2018 CEO and board coefficients are -0.31 and -0.72 points. The separate joint tests of the 2018 and 2019 coefficients have p-values of 0.555 for the CEO series and 0.093 for the board series; the test of all four CEO and board interactions has $p = 0.083$. The pre-period tests cover 2014, 2015, and 2016 and have p-values of 0.778 in the CEO available specification, 0.607 for the joint-model CEO series, and 0.709 for the board series; the test of all six pre-period CEO and board interactions has $p = 0.728$. The CEO point estimates remain negative in 2019, while the board estimate reverses sign. The placebo therefore does not reproduce the positive, sustained post-2019 path, although its 2018 coefficients counsel against describing it as a clean null.

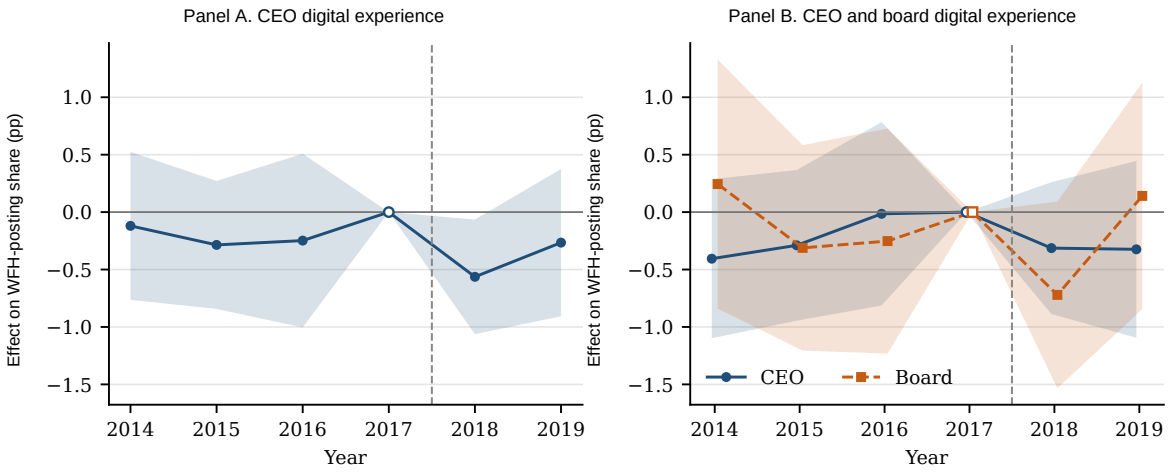

Figure 4: External digital leadership experience and the WFH-posting share in the pre-2019 placebo

Notes: The dependent variable is the WFH-posting share. Points are interactions of external CEO or board digital experience accumulated during 2014–2016 with calendar year. The 2017 leadership cohort is fixed before the artificial break; the estimation window is 2014–2019, 2017 is the omitted year, and 2018–2019 are the artificial post years. Shaded regions are 95 percent confidence intervals. Specifications include firm and calendar-year fixed effects, no additional controls, and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line marks 2017, the last pre-placebo year. Appendix Table A8 defines the joint tests.

6.6 Leadership continuity and continued firm observation

The 2019 leadership team may become a poor description of the firm if the chief executive changes after the shock. Firm closure, merger, or a loss of administrative coverage could create a related selection problem. We therefore re-estimate the CEO common specification among firms that keep the same CEO through 2023 and among firms observed continuously from 2017 through 2022.

The same-CEO restriction reduces the sample from 6,319 to 3,515 firms. Even so, the pooled estimates are 1.1 percentage points in 2020–2021 and 2.7 points in 2022–2023, compared with 1.3 and 2.8 points in the CEO common baseline. The annual paths are also similar, although the 2020 estimate is less precise in the smaller sample. Requiring continuous firm observation removes 199 firms and leaves the pooled estimates unchanged after rounding at 1.3 and 2.8 percentage points. Appendix Table A5 reports the full comparison. These restrictions condition on post-2019 outcomes that may themselves be related to experience, so they document descriptive stability rather than decomposing the contribution of turnover or attrition. The same-CEO result shows that CEO replacement is not necessary for the gradient and that the association persists among firms retaining their selected CEO. The firm-presence check shows similar stability among firms observed continuously from 2017 through 2022; because that restriction ends in 2022, its pooled 2022–2023 estimate does not require presence in 2023.

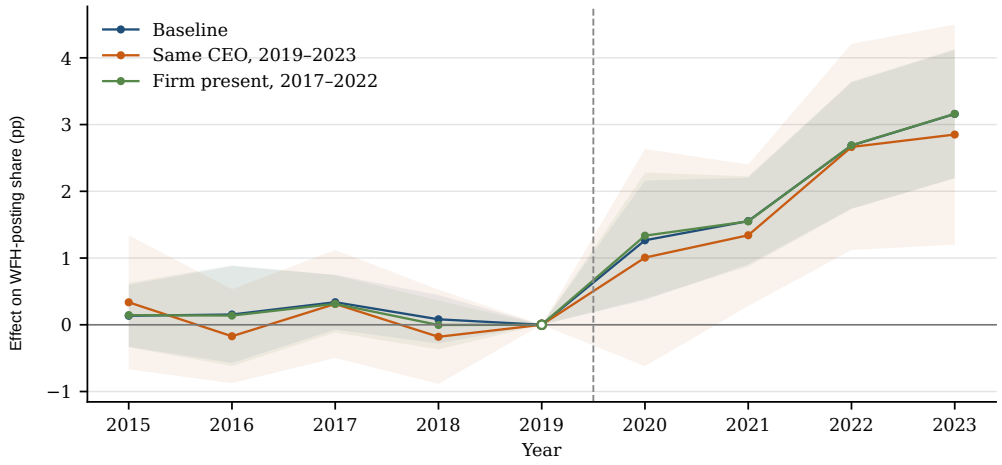

Figure 5: External CEO digital experience and the WFH-posting share under leadership and firm-observation restrictions

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of external CEO digital experience with calendar year, relative to 2019. Shaded regions are 95 percent confidence intervals. All specifications include firm and calendar-year fixed effects, no additional controls, and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line separates 2019 from 2020.

6.7 Fully measured external histories

Digital-workforce intensity cannot be measured for every qualifying external organization-year in a leader's history. To test whether these incomplete histories influence the estimates, we require every external organization-year used for the CEO measure to have observed workforce composition. We refer to these as fully measured, or "matched," histories. The joint restriction also requires complete measurement for every formal board member.

The CEO-only results are close to the baseline. In the CEO available sample, the coefficients are 1.5 and 2.9 percentage points in 2020–2021 and 2022–2023, compared with 1.2 and 2.6 points in the full sample. In the CEO common sample, they are 1.6 and 2.8 points, compared with 1.3 and 2.8 points. The joint restriction is more demanding and reduces the analysis to 1,215 firms. The CEO estimates become 0.0 and 1.2 points and are imprecise, while the board estimates remain positive and rise to 2.8 and 5.2 points. The fully measured-history exercise preserves the CEO-only result and the later board relationship; it provides less information about the CEO contribution conditional on a fully measured board because the joint sample becomes small. The master appendix reports the detailed regression table, measurement diagnostics, and annual paths. The main CEO-only relationship is therefore not produced by incomplete measurement of external organization-years, and the later board result remains strong under the more demanding joint restriction.

6.8 Vacancy support and weighting

WFH shares based on very few advertisements may be noisy, and an unweighted firm-year regression gives low- and high-volume recruiters equal influence. We therefore require at least 5, 10, or 20 advertisements in a firm-year and, in a separate specification, weight each observation by its number of advertisements.

The later CEO available coefficient remains between 2.3 and 2.6 percentage points under the minimum-posting restrictions and equals 1.7 points when vacancy weighted. In the joint specification, the later CEO coefficient ranges from 1.6 to 1.9 points, while the later board coefficient ranges from 3.7 to 4.3 points. The immediate CEO estimate becomes small under the stricter thresholds and vacancy weighting, but the early board estimate remains positive. Appendix Table A7 reports the compact comparison. The main later-period conclusion is therefore not an artifact of calculating WFH shares from a few advertisements or of giving small and large recruiters equal weight.

6.9 Selection into observed online recruiting

The main WFH-posting share is defined only in firm-years with at least one observed vacancy. If leadership experience also changes whether a firm appears in the online-vacancy data, the firms and years contributing a defined share could change differentially after 2019. Vacancy-count regressions and minimum-posting restrictions do not by themselves resolve this concern. We therefore reconstruct the fixed 2019 cohort for every year from 2015 through 2023, beginning each firm's observation period no earlier than its legal or first-observed start year. We first estimate the probability of any observed vacancy and the WFH share conditional on a vacancy. We then retain every in-scope firm-year using two unconditional outcomes: an indicator for any observed WFH vacancy and the number of WFH vacancies per 100 2019 FTE. Firm-years with no observed vacancy receive zero for both unconditional outcomes.

Table 7 shows that the positive WFH relationship is not generated by firms with more experienced CEOs becoming more likely to enter the observed-vacancy sample. In the CEO available cohort, a benchmark-equivalent year of external CEO digital experience is associated with a 0.7-percentage-point lower probability of any observed vacancy in 2020–2021 and a 1.4-point lower probability in 2022–2023. Nevertheless, the probability of observing at least one WFH vacancy is 1.2 and 1.8 percentage points higher, respectively, while WFH vacancies per 100 2019 FTE are higher by 0.098 and 0.267. The conditional-share coefficients, 1.2 and 2.6 percentage points, reproduce the baseline pattern within the reconstructed cohort.

The joint model gives the same contrast for board experience. A benchmark-equivalent year of external board digital experience is associated with a 1.1-percentage-point lower probability of any observed vacancy in 2020–2021 and a 1.9-point lower probability in 2022–2023, but with a 1.4- and 1.6-point higher probability of any WFH vacancy and 0.189 and 0.425 additional WFH vacancies per 100 2019 FTE. The joint CEO coefficients remain positive for the conditional WFH

Table 7: Selection into observed online recruiting and unconditional WFH outcomes

	Any observed vacancy	WFH share | vacancy	Any observed WFH vacancy	WFH vacancies per 100 2019 FTE
Panel A. CEO available				
External CEO digital experience × 2020–2021	−0.007**	0.012***	0.012***	0.098***
	(0.003)	(0.002)	(0.002)	(0.030)
External CEO digital experience × 2022–2023	−0.014***	0.026***	0.018***	0.267***
	(0.003)	(0.003)	(0.003)	(0.058)
Pre-2020 outcome mean	0.435	0.031	0.042	0.266
Firms	16,044	10,913	16,044	16,044
Observations	141,580	59,265	141,580	141,580
Panel B. Joint common				
External CEO digital experience × 2020–2021	−0.001	0.007**	0.010***	0.024
	(0.004)	(0.003)	(0.003)	(0.038)
External CEO digital experience × 2022–2023	−0.006	0.017***	0.015***	0.123*
	(0.005)	(0.004)	(0.004)	(0.070)
External board digital experience × 2020–2021	−0.011***	0.020***	0.014***	0.189***
	(0.004)	(0.004)	(0.004)	(0.060)
External board digital experience × 2022–2023	−0.019***	0.039***	0.016***	0.425***
	(0.005)	(0.005)	(0.005)	(0.109)
Pre-2020 outcome mean	0.474	0.030	0.045	0.255
Firms	8,552	6,319	8,552	8,552
Observations	76,012	35,096	76,012	76,012
Firm fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes

Notes: Panel A uses the CEO available cohort. Panel B enters external CEO and board digital experience jointly in the joint common cohort. Coefficients are interactions of continuous external digital experience, measured in benchmark-equivalent years, with the indicated period. Any observed vacancy and any observed WFH vacancy are linear-probability outcomes. The conditional WFH share is defined only for firm-years with a positive observed vacancy count. The two unconditional WFH outcomes retain every in-scope firm-year in the reconstructed cohort and assign zero when no vacancy is observed. WFH vacancies per 100 FTE use employment fixed at its 2019 value. All specifications include firm and calendar-year fixed effects; standard errors clustered by firm are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

share and for the probability of any WFH vacancy; the CEO per-FTE estimates are smaller and less precise in this sample.

The event studies require one qualification. For the any-vacancy outcome, the CEO pre-period test has $p = 0.055$ in the CEO available cohort; in the joint model the CEO-specific test has $p = 0.048$ and the combined CEO–board test has $p = 0.071$. We therefore do not give the negative posting-margin coefficients a causal interpretation. The pre-period evidence is cleaner for the WFH outcomes defined over the full cohort: the any-WFH-vacancy tests have $p = 0.149$ in the CEO available model and $p = 0.127$ for the combined joint test, while the corresponding WFH-vacancies-per-FTE tests have $p = 0.696$ and $p = 0.755$. These tests do not establish parallel trends, but they support the narrower conclusion that the positive WFH relationship remains when zero-posting firm-years are retained. The exercise concerns observed online vacancies and cannot rule out differential use of offline recruiting, changes in platform coverage or firm matching, or differences in reposting behavior.

Across the robustness exercises, the later CEO-only and board gradients remain positive after observed-capacity and detailed-industry adjustments, under fully measured CEO histories, with alternative vacancy-support requirements, in descriptive leadership-continuity restrictions, and under alternative experience definitions. Their magnitudes are substantially smaller in the most demanding capacity specification, and the conditional CEO coefficient in the joint model is no longer statistically detectable there. The fixed-preperiod decomposition assigns little of the firm-level gradient to shifts toward occupations that were already more WFH-intensive before the pandemic. WFH measures that follow fixed-cohort firms through in-scope years with no observed vacancy move in the same direction even though the probability of any observed vacancy does not. The immediate CEO response is more sensitive to capacity, industry and occupation controls and high-volume vacancy restrictions, whereas the 2022–2023 CEO-only relationship and the board contribution in the joint specification are more persistent. The checks narrow several alternatives but do not solve leader–firm selection.

7 Heterogeneity

External experience may matter more when a CEO is relatively new to the focal firm and less tied to established organizational practices. Longer-tenured CEOs may instead have greater authority or more firm-specific knowledge with which to implement change. Because these mechanisms point in opposite directions, we examine how the WFH gradient varies with observed CEO tenure rather than imposing a directional prediction.

Table 8 reports the implied estimates for CEOs with 0–1, 2–4, and at least 5 observed years at the focal firm. The 2020–2021 coefficients are 2.6, 1.5, and 0.8 percentage points, respectively. The corresponding 2022–2023 coefficients are 3.8, 2.8, and 2.2 points. The estimates decline monotonically with tenure, and every group has a positive later-period coefficient.

The pattern is suggestive, not statistically established heterogeneity. None of the differences

Table 8: WFH-posting share by observed CEO tenure

	0–1 years	2–4 years	5 or more years
External CEO digital experience × 2020–2021	0.026**	0.015***	0.008***
	(0.011)	(0.004)	(0.003)
External CEO digital experience × 2022–2023	0.038***	0.028***	0.022***
	(0.015)	(0.005)	(0.004)
Pre-2020 outcome mean		0.031	
Firms		10,913	
Observations		59,265	

Notes: The dependent variable is the WFH-posting share. The table reports the implied period-specific association for each tenure group from a single regression that interacts external CEO digital experience with period and observed tenure. Tenure is measured over the available 2014–2019 history and is therefore left-censored. The omitted tenure group in the interaction model is 0–1 years. None of the differences between the longer-tenure groups and the 0–1-year group is statistically different from zero at the 10 percent level. The model includes firm and calendar-year fixed effects and firm-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

relative to the 0–1-year group is statistically different from zero at the 10 percent level. Adding interactions with CEO age leaves the tenure differentials broadly similar. The supported conclusion is that the later WFH gradient is present across the tenure distribution, with point estimates that are largest among recently appointed CEOs.

8 Extensions to Other Outcomes

The main analysis documents a post-2019 change in the gradient of the share of vacancies offering WFH, but that share alone does not show which broader firm behavior changed. The extensions ask whether the result reflects a rise in the number of WFH vacancies, a general change in recruiting, a shift toward digital or AI-related labor, or differences in firm and leadership continuity. The outcomes are chosen to distinguish a change in job organization from broader expansion or contraction. The WFH, digital-hiring, and total-posting measures remain panel outcomes; the AI measures are available only for 2023; continued observation in annual business records and CEO retention describe whether the focal firm and its selected leader remain present.

8.1 WFH-posting counts and digital recruiting

A higher WFH-posting share could arise because firms advertise more WFH vacancies or because they advertise fewer vacancies without a WFH option. We therefore examine the inverse-hyperbolic-sine (IHS) count of WFH postings and both the raw and IHS total-posting counts. We also consider the digital-posting share, IHS digital-posting count, and an indicator for any digital posting. Digital hiring is a useful comparison because firms may respond to a shock by changing how newly posted jobs are organized or by changing the types of workers they seek. Evidence that large shocks can

alter posted skill requirements (Hershbein and Kahn, 2018) and that pre-pandemic technology sophistication predicts resilience during COVID-19 (Comin et al., 2022) motivates the comparison; neither study establishes that COVID-19 generally increased digital hiring.

Table 9: External CEO digital experience and WFH, digital-hiring, and total-posting outcomes

	External CEO digital-experience coefficient				
Dependent variable	× 2020–2021	× 2022–2023	Pre-2020 mean	Firms	Observations
Asinh WFH-posting count	0.028***	0.052***	0.059	16044	141580
	(0.005)	(0.008)			
Digital-posting share	−0.002	−0.008***	0.256	10913	59265
	(0.003)	(0.003)			
Asinh digital-posting count	−0.007	−0.023***	0.327	16044	141580
	(0.006)	(0.008)			
Any digital posting	−0.007**	−0.014***	0.195	16044	141580
	(0.003)	(0.003)			
Total posting count	0.286	0.483	4.960	16044	141580
	(0.344)	(0.506)			
Asinh total posting count	−0.015**	−0.029***	0.846	16044	141580
	(0.006)	(0.009)			

Notes: Each row is a separate regression in the CEO available sample. The dependent variable is listed in the first column, and the two coefficient columns report interactions of continuous external CEO digital experience with the indicated period. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Share regressions require an observed vacancy; count outcomes retain zero-posting firm-years. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 9 reports separate regressions for each outcome in the CEO available sample, using the same continuous external CEO digital-experience measure and fixed-effects structure. Count and indicator outcomes retain zero-posting firm-years, so those rows use all 16,044 cohort firms rather than only firm-years with a defined vacancy share.

The first row shows a positive differential change in the transformed WFH-posting count. A benchmark-equivalent year of external CEO digital experience predicts an increase of 0.028 IHS units in 2020–2021 (s.e. 0.005) and 0.052 units in 2022–2023 (s.e. 0.008). Both estimates are statistically different from zero at the 1 percent level. Because the IHS transformation is nonlinear, these coefficients do not by themselves establish a positive change in arithmetic WFH counts. Table 7 provides complementary evidence on a count scale normalized by fixed 2019 employment: in the CEO available cohort, a benchmark-equivalent year predicts 0.098 more WFH vacancies per 100 2019 FTE in 2020–2021 and 0.267 more in 2022–2023. The transformed-count and per-FTE outcomes retain every in-scope firm-year in the reconstructed cohort, whereas the main WFH-share regression is defined only for firm-years with positive posting activity. These results document a positive WFH-recruiting gradient over the reconstructed cohort, but do not establish that the conditional-share change is entirely a numerator effect or rule out contraction in non-WFH postings on its support. The persistence of positive share estimates under minimum-posting thresholds and vacancy weighting in Appendix Table A7 provides complementary evidence that the result is not driven by a small number of low-volume firm-years.

The next three rows show a different pattern for digital recruiting. The digital-posting share changes by -0.2 percentage points in 2020–2021 (s.e. 0.3) and -0.8 points in 2022–2023 (s.e. 0.3).

The early estimate is indistinguishable from zero; the later estimate is statistically different from zero at the 1 percent level and equals about 3.1 percent of the 25.6-percent pre-period mean. The transformed digital-posting count is also close to zero in the early period (-0.007, s.e. 0.006) and lower in the later period (-0.023, s.e. 0.008). Finally, the probability of any digital posting falls by 0.7 percentage points in 2020–2021 and 1.4 points in 2022–2023. Relative to its 19.5-percent pre-period mean, the later estimate is about 7 percent.

The contrast is useful. The firms that move most strongly toward WFH are not simultaneously expanding every form of digital recruitment. If anything, their measured digital-vacancy outcomes are slightly lower by the later period. The raw total-posting coefficients are 0.286 in 2020–2021 and 0.483 in 2022–2023, with standard errors of 0.344 and 0.506. Neither is statistically distinguishable from zero, but their confidence intervals include economically meaningful positive changes. The inverse-hyperbolic-sine estimates are negative and statistically significant, describing a different, distribution-sensitive margin that gives relatively more weight to changes among firms with few postings. The supported conclusion is therefore that there is no statistically detectable rise in raw average total-posting volume; the estimates do not rule out a positive change in that mean.

The recruiting results point more strongly to a change in job design than to a general shift toward digital occupations. In the complete reconstructed cohort, firms with more externally experienced CEOs have a higher probability of any WFH vacancy and more WFH vacancies per 100 fixed 2019 FTE; the IHS WFH-count coefficients are also positive. By contrast, the digital-posting share does not rise on the conditional positive-posting sample, while the IHS digital-posting count and the probability of any digital posting do not rise over the complete cohort. The latter two outcomes avoid the conditional-share sample and therefore complement rather than decompose the share result. The imprecise raw total-count estimates prevent us from ruling out a broad increase in average posting volume. Section 6.3 examines substitution across occupations directly; the outcomes here ask the separate question of whether WFH growth accompanies a broader expansion in recruiting or digital labor demand.

8.2 Digital workforce composition

The pooled digital-hiring estimates could conceal changes in timing, and the contrast between external and total experience may differ for workforce composition. Appendix Figure A10 therefore compares the annual digital-posting-share paths associated with the two experience measures. The digital-posting share is the fraction of observed vacancies classified as requiring digital skills. Panel A uses the external-experience specification from Table 9; its pre-period coefficients are jointly equal to zero with $p = 0.405$. Panel B uses total CEO digital experience, which adds qualifying experience at the focal firm. That series has a negative 2018 coefficient and a joint pretrend test of $p = 0.004$. The total-experience series therefore lacks the comparatively flat pre-period pattern of the WFH results. Neither annual path indicates a post-2019 increase in the share of vacancies classified as digital.

The annual workforce-composition evidence reinforces the interpretation of the main result. The post-2019 WFH divergence is not part of a general increase in digital vacancy demand; it is specific to how firms organize and advertise the jobs they are trying to fill.

8.3 AI-related vacancies

Artificial intelligence provides a separate test of whether pre-pandemic digital leadership experience is associated with a new technology-related recruiting margin. We use two 2023 outcomes: the IHS count of AI-related postings and an indicator for any AI-related posting. The extension keeps the frozen 2019 leadership cohort and external digital experience accumulated during 2014–2018. A positive association would show that the measure predicts participation in another technology domain; it would not reveal whether the channel is implementation knowledge, leader matching, or another correlated attribute. Because the outcomes are observed only in 2023, this exercise cannot reproduce the firm fixed-effects event-study design.

Table 10: External digital leadership experience, IHS AI-posting count, and any AI posting in 2023

Sample and exposure	IHS AI-posting count	Any AI posting	Firms
CEO available: external CEO digital experience	0.016***	0.007***	15231
	(0.005)	(0.002)	
CEO common: external CEO digital experience	0.012**	0.006*	8146
	(0.005)	(0.003)	
Joint common: external CEO digital experience	0.012**	0.006*	8146
	(0.006)	(0.003)	
Joint common: external board digital experience	−0.002	0.001	8146
	(0.004)	(0.003)	
2023 outcome mean (CEO available)	0.021	0.016	
2023 outcome mean (CEO common/joint common)	0.020	0.016	

Notes: The dependent variables are IHS AI-posting count and an indicator for any AI posting in 2023. Each column is a cross-sectional regression using the same frozen 2019 leadership cohort and external digital-experience measures accumulated during 2014–2018. Models include 2019 two-digit-industry fixed effects. The CEO available regressions contain 15,231 rather than 16,044 firms because 813 firms lack the required 2019 industry classification. Count and indicator outcomes retain zero-posting firms. Standard errors appear in parentheses and are clustered by firm. Because the outcomes are observed only in 2023, the table reports 2023 sample means rather than pre-period means. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 10 turns to AI-related vacancies in 2023. These outcomes are available only as a cross section, so the estimates should not be read as an extension of the firm fixed-effects event study. The CEO available AI regressions contain 15,231 rather than 16,044 firms because 813 firms lack the 2019 two-digit industry classification required by the cross-sectional industry fixed effects. In the CEO available sample, a benchmark-equivalent year of external CEO digital experience predicts 0.016 additional IHS units of AI-posting count (s.e. 0.005) and a 0.7-percentage-point increase in the probability of any AI posting (s.e. 0.2). The latter is large relative to the 1.6-percent sample mean: it is about 44 percent of that mean.

The external CEO digital-experience coefficients remain positive in the CEO common sample.

The count coefficient is 0.012 with a standard error of 0.005 in the CEO-only specification and 0.012 with a standard error of 0.006 when board digital experience is added. The corresponding any-AI estimates are 0.6 percentage points in both specifications, with $p < 0.10$. External board digital experience predicts -0.002 IHS units of AI-posting count and a 0.1-point increase in incidence; neither estimate is statistically distinguishable from zero. No board–AI association is statistically detected in this sample, but Table 10 does not test whether the CEO and board coefficients differ.

The AI estimates are cross-sectional associations and cannot absorb time-invariant unobserved firm differences. They therefore provide suggestive evidence that external CEO digital experience predicts participation in AI recruiting in 2023, not a causal estimate of AI adoption. Their value is interpretive: the positive CEO association relates the predetermined experience measure to a second technology domain, while the board estimates are inconclusive. This extension broadens the relevance of CEO digital experience without changing the main paper's within-firm WFH conclusion.

8.4 Firm survival proxy and leadership continuity

The final extension asks whether the same leadership experience predicts which firms and CEOs continue to be observed. This is relevant both as an outcome and as a possible source of selection in the vacancy results. Annual firm presence equals one when the focal firm appears in that year's business records. It is only a survival proxy: it does not distinguish legal closure from a merger or another loss of coverage. CEO retention equals one when the selected 2019 CEO remains in the role.

Table 11: External CEO digital experience, continued firm observation, and CEO retention

	External CEO digital-experience coefficient	
Dependent variable	× 2020–2021	× 2022–2023
Continued observation (survival proxy)	−0.002**	−0.006***
	(0.001)	(0.002)
Selected 2019 CEO retained	−0.018***	−0.036***
	(0.003)	(0.004)
Pre-2020 outcome mean	1.000	1.000
Firms	16044	16044
Observations	80220	80220

Notes: Each outcome row is a separate regression in the CEO available sample. The coefficient columns report interactions of continuous external CEO digital experience with the indicated period. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Continued observation records whether the firm appears in annual business records and does not distinguish legal closure from mergers or other losses of coverage. The CEO-retention outcome records whether the selected 2019 CEO remains in the role. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11 follows the fixed 2019 cohort beyond vacancies. External CEO digital experience

predicts a -0.2-percentage-point lower probability of administrative presence in 2020–2021 and a -0.6-point lower probability in 2022–2023. These differences are small, and their sign does not support the specific interpretation that the WFH relationship arises because more digitally experienced CEOs keep more firms in the observed data. The estimates do not rule out other forms of selection associated with continued observation.

External CEO digital experience also predicts a -1.8-point lower retention probability in 2020–2021 and a -3.6-point lower probability in 2022–2023. Digitally experienced CEOs may have more outside opportunities, or their firms may reorganize leadership more often; the regression does not distinguish those possibilities. Read together with the same-CEO robustness exercise, the result is informative. Digitally experienced CEOs are somewhat less likely to remain, yet the WFH gradient is nearly unchanged when the sample is restricted to firms that keep the same CEO. The association therefore does not require CEO replacement and persists among firms retaining the selected CEO. Because retention is a post-2019 outcome, the restriction cannot determine how much turnover contributes to the unrestricted association.

The AI results remain associational, the firm-presence measure is only a proxy, and the raw total-posting estimates are too imprecise to rule out an economically meaningful increase in average vacancy volume. Subject to those qualifications, the extensions sharpen the central finding: the post-2019 divergence is also visible in the transformed WFH count and in WFH vacancies per 100 fixed 2019 FTE when firm-years with no observed online vacancy are retained, is not accompanied by a general increase in digital hiring, and remains in the selected continuity subsamples. These outcomes complement the conditional-share result without decomposing it, while the continuity exercises show descriptive stability without identifying the contribution of retention or presence. The evidence therefore points to a change in how firms organize and advertise work rather than a uniform expansion in recruiting or digital labor demand.

9 Conclusion

When COVID-19 forced firms to reconsider where work could be done, they did not respond in the same way. Danish firms whose 2019 CEOs had held qualifying roles at organizations with more digitally intensive workforces subsequently increased the share of vacancies offering a work-from-home option by more. One benchmark-equivalent year of external CEO digital experience predicts a 2.6-percentage-point larger increase in the WFH-posting share during 2022–2023 than during 2015–2019, equal to 84 percent of the pre-2020 mean. Relative to the 2019 gradient, the annual difference grows from 0.8 percentage points in 2020 to 2.9 points in 2023. Board digital experience also predicts a larger later increase in WFH availability.

The result suggests that the spread of WFH depended on more than whether a job could technically be performed from home. Someone still had to decide which roles were suitable, how remote employees would be coordinated, and whether the arrangement should continue after the emergency had passed. Prior experience in a digital organization may help leaders make those

decisions. The later CEO-only and board relationships remain positive, although substantially attenuated, after observed pre-pandemic characteristics and detailed industry-by-year effects are added. The pattern also appears across the CEO-tenure distribution, is stable in the descriptive same-CEO and continuous-observation samples, and is not reproduced by the pre-pandemic placebo. A decomposition using fixed pre-pandemic occupation-specific WFH propensities assigns little of the relationship to shifts toward occupations that were already more WFH-intensive. The association is also positive for WFH measures that follow the fixed cohort through years with no observed online vacancy. In a selected subsample, a similar CEO relationship is obtained when measured experience counts only external role-years preceding the first observed focal-role year, although this exercise does not hold contemporaneous interlocks fixed. The accompanying outcomes point to a change in advertised job organization rather than a general increase in digital hiring.

The analysis measures exposure to digitally intensive organizations rather than the knowledge itself, and leaders were not randomly assigned to firms. We therefore interpret organizational know-how as a channel consistent with the evidence rather than as a directly observed mechanism. What the design establishes more directly is that a fixed pre-pandemic leadership characteristic became strongly predictive of within-firm WFH change exactly when remote coordination acquired value. The later CEO-only and board pattern remains after observed-capacity and detailed-industry adjustments, among firms with stable leadership, and under alternative measurement and vacancy-support choices, but the attenuation under the most demanding controls warrants caution about how much of the baseline gradient is specific to leadership experience.

The later-period and board results are the most stable parts of the evidence. For firms, the findings make managerial and advisory capacity a plausible part of the decision to offer flexible work. Investments in broadband and equipment may not be sufficient when firms lack experience implementing new work practices. Training, peer learning, or temporary access to relevant expertise may help firms make better-informed choices. This is not an argument that every job should be remote. It is an argument that access to workable forms of WFH may depend partly on whether the people leading a firm have seen similar organizational changes succeed elsewhere.

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A Data Sources and Construction

A.1 Administrative and vacancy sources

The administrative records come from Statistics Denmark and are linked at the person and organization levels using stable register identifiers. The annual business register provides full-time-equivalent employment, employee counts, turnover, NACE industry, legal form, firm start date, and continued annual observation. The BFL employer–employee files provide person–organization employment links, pay, hours, sector, and four-digit DISCO occupations. A person can be recorded at more than one organization in the same year; the construction therefore retains one record for each person–organization–year rather than selecting a unique main job. The 2019 BEF population snapshot provides birth date, sex, and immigrant-background information. The 2019 UDDA snapshot provides highest completed education. Annual population records from 2014 through 2018 establish the person-support requirement used for the frozen leadership cohort.

Formal corporate leadership comes from PERSBEST, an annual Statistics Denmark register built primarily from companies' statutory reports of active executive managers and board members. Reporting these appointments is legally required. We use PERSBEST records from 2014 through 2023. PERSBEST distinguishes formal board appointments from executive management; the employer–employee histories additionally identify managers through their occupations.

Online vacancies come from Jobindex, Denmark's largest online job board, through Højbjerre Brauer Schultz Economics. The full corpus spans 2006–2023. The extract used for this project covers 2010–2023 and supplies posting dates, firm identifiers, occupation codes, WFH classifications, and skill classifications used for the digital- and AI-recruiting outcomes. The main linked vacancy panel covers 2015–2023, and the earlier-cohort placebo uses 2014–2019. Leadership experience in the main analysis is accumulated during 2014–2018 and fixed before the pandemic.

A.2 Firm cohort and panel support

The firm cohort is fixed in 2019. The analysis includes covered private corporate firms with at least 9.5 full-time-equivalent workers, positive turnover, and an identified CEO. The selected CEO must be observed throughout 2014–2018 and be at least age 30 in 2019. The joint common sample applies the same support and age requirements to every non-CEO board member. Once a firm enters the cohort, it remains in the 2015–2023 panel even if it later disappears from annual business records. Job-posting shares are defined only in years with at least one observed vacancy, whereas posting-count outcomes retain firm-years with no postings. Continued business-record observation and positive posting activity are therefore separate concepts.

A.3 CEO and board rosters

PERSBEST may list several members of executive management at one firm and does not separately identify a unique CEO. If the executive-management group contains one person, that person is

selected. If it contains several people, the highest-paid executive is selected using linked employer–employee pay records; a stable ordering resolves exact pay ties. Firms without a usable pay comparison are excluded. Other executives remain part of their own leadership histories but are not treated as board members.

The board roster contains formal board members other than the selected CEO. The CEO available sample requires a supported selected CEO but does not require complete board support. The CEO common sample restricts CEO-only estimates to firms that also meet the joint common requirements. The joint common sample requires a supported selected CEO and complete support for every non-CEO board member. Sample eligibility is distinct from treatment: a supported leader with zero constructed exposure remains in the relevant sample.

A.4 Vacancy skill classification

The vacancy extract supplies 17 binary skill categories derived from the full advertisement text. For each category, the extract contains a binary indicator together with a supporting passage from the text.

The digital-vacancy indicator equals one when an advertisement is positive in at least one of seven categories: computer support and networking, data analysis, digital design, generative AI, AI, digital marketing, or programming and software. The AI indicator combines the AI and generative-AI categories. These text classifications construct the digital- and AI-vacancy outcomes and predetermined vacancy-readiness controls. The digital leadership measure instead uses employees' observed occupations in the BFL records and does not use classified vacancy text.

A.5 Employer-side work-from-home availability

An advertisement is classified as offering WFH when it permits a remote, hybrid, or other home-work arrangement. Let H_{ft} denote these advertisements and A_{ft} all observed advertisements for firm f in year t. The main outcome is H_{ft}/A_{ft} for $A_{ft} > 0$. The denominator therefore includes every observed vacancy, including vacancies outside digital occupations. A firm-year with no observed posting has an undefined share but a zero WFH-posting count.

Following Daly and Zhu (2026) and the job-posting classification framework in Zhu and Hardt (2025), the measure captures employer-side WFH availability in newly posted jobs. It does not measure realized days at home. The outcome is consequently interpreted as a change in advertised job design and recruiting terms.

A.6 External digital leadership experience

Workers are classified into broad digital occupations using four-digit DISCO codes aligned with the International Standard Classification of Occupations. The definition covers code 1330 and the ranges 2500–2599 and 3500–3599: digital managers, professionals, and technicians. Multiple employment records for the same person and organization in a year are collapsed before the workforce counts

are formed, so each person–organization–year contributes once to that organization's denominator. If duplicate records disagree on occupation, the person is classified as digital when any record has a broad-digital DISCO code. A person employed at several organizations can contribute to each organization's workforce in that year. For each organization-year, the digital-workforce share is the fraction of these worker records in the broad digital group; it is not weighted by hours or full-time-equivalent employment. The annual benchmark is the seventy-fifth percentile of that share among organizations with a positive digital workforce. Organizations with an observed workforce but no digital workers receive zero intensity and do not enter the benchmark calculation. The construction uses employees' occupations rather than the digital classification of newly posted vacancies.

Organization-year digital intensity is the organization's broad digital-workforce share divided by that annual benchmark. One unit therefore corresponds to one year at the seventy-fifth percentile among organizations with a positive digital workforce, not the top quarter of all firms. The benchmark is recalculated each year in the broader organization population, and ties may occur at the cutoff. The share of private firms at or above the benchmark can therefore differ from one quarter of private firms with a positive digital workforce.

Appendix Table A1 applies the annual benchmark to the private-firm population. It reports both the exact number of private firms at or above the cutoff and their shares among all private firms and among private firms with a positive digital workforce.

Appendix Table A1: Private firms at or above the annual digital-intensity benchmark

Year	Private firms	Benchmark firms	Percent of all	Percent of positive	Mean digital share
2014	91020	1914	2.1	27.0	63.6
2015	95452	2312	2.4	29.5	65.0
2016	99997	2421	2.4	29.4	67.1
2017	105209	2304	2.2	26.3	73.1
2018	109678	2398	2.2	26.4	73.5

Notes: Benchmark firms have a digital-workforce share at or above the annual seventy-fifth-percentile cutoff calculated by the experience module among organizations with a positive digital-workforce share. The table then applies that cutoff to the private-firm population. Percent of positive uses private firms with a positive digital-workforce share as the denominator. Digital share is shown in percent.

Qualifying histories include service as a CEO, executive manager, other manager, or formal board member. BFL managerial records are identified from DISCO major group 1, excluding records whose sector code identifies them as public. External experience excludes the focal 2019 organization and can be accumulated before or concurrently with the focal-firm role. If a person holds qualifying positions at several external organizations in the same year, the measured normalized digital intensities are averaged before experience is summed across years. Missing organization-year intensities are omitted from this within-year average when another measured intensity is available. Averaging prevents concurrent appointments from being counted additively, but it does not cap annual experience at one: the annual value can exceed one when the average

digital intensity of those organizations exceeds the annual benchmark. These annual amounts are summed from 2014 through 2018. CEO experience is the selected CEO's frozen stock. Board experience is the equal-weighted mean across supported non-CEO board members, including members whose external experience is zero.

A zero can arise because a supported leader held no qualifying external role, because the relevant external organization had an observed workforce with no digital employees, or because none of the qualifying external organization-years has measured digital intensity. The binary comparison therefore contrasts positive measured exposure with zero constructed exposure; the latter need not imply the absence of actual external experience. Among selected CEOs, 32.1 percent held no qualifying external role, 14.8 percent had an observed external workforce with no digital employees, and 29.3 percent had an incomplete external history and no positive measured intensity. Positive measured intensity is observed for 23.9 percent, including 12.9 percent who also have at least one incompletely measured organization-year. The fully measured-history exercise instead retains only leaders for whom every qualifying external organization-year has observed workforce composition.

B Supplementary Results

B.1 Annual coefficients

Appendix Table A2 reports the annual estimates underlying the main WFH-posting-share evidence for the CEO available sample, the aligned CEO common regression, and the joint common CEO–board regression. The CEO common column distinguishes a sample-composition change from the change induced by adding board digital experience. The pre-trend tests have $p = 0.144$ for the CEO available model, $p = 0.563$ and, in the joint model, $p = 0.555$ for CEO experience, $p = 0.777$ for board experience, and $p = 0.788$ for their joint test. These diagnostics are consistent with a relatively flat pre-period but do not establish parallel trends.

Appendix Table A2: Annual interactions of external digital leadership experience with year: WFH-posting share

	CEO available	CEO common	Joint common	
Year	CEO digital experience	CEO digital experience	CEO digital experience	Board digital experience
2015	0.000	0.001	0.000	0.003
	(0.002)	(0.002)	(0.003)	(0.005)
2016	−0.002	0.002	0.001	−0.001
	(0.003)	(0.004)	(0.004)	(0.005)
2017	0.003	0.003	0.004	−0.001
	(0.003)	(0.002)	(0.003)	(0.004)
2018	−0.002	0.001	0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.004)
2019	0.000	0.000	0.000	0.000
	–	–	–	–

	CEO available	CEO common	Joint common	
Year	CEO digital experience	CEO digital experience	CEO digital experience	Board digital experience
2020	0.008**	0.013***	0.011**	0.006
	(0.004)	(0.005)	(0.005)	(0.005)
2021	0.015***	0.016***	0.006	0.031***
	(0.003)	(0.003)	(0.004)	(0.006)
2022	0.024***	0.027***	0.017***	0.036***
	(0.004)	(0.005)	(0.005)	(0.006)
2023	0.029***	0.032***	0.020***	0.042***
	(0.004)	(0.005)	(0.005)	(0.007)
Pre-trend p-value	0.144	0.563	0.555	0.777
Pre-2020 outcome mean	0.031	0.030	0.030	0.030
Firms	10913	6319	6319	6319
Observations	59265	35096	35096	35096

Notes: The dependent variable is the WFH-posting share. Each coefficient is the interaction of continuous external CEO or board digital experience with the listed year. The omitted year is 2019. Standard errors appear in parentheses directly beneath decimal-aligned coefficients. All models include firm and calendar-year fixed effects and no additional controls. The joint common columns come from a single regression containing both digital-experience measures. Significance stars use two-sided p-values.

B.2 External versus total experience

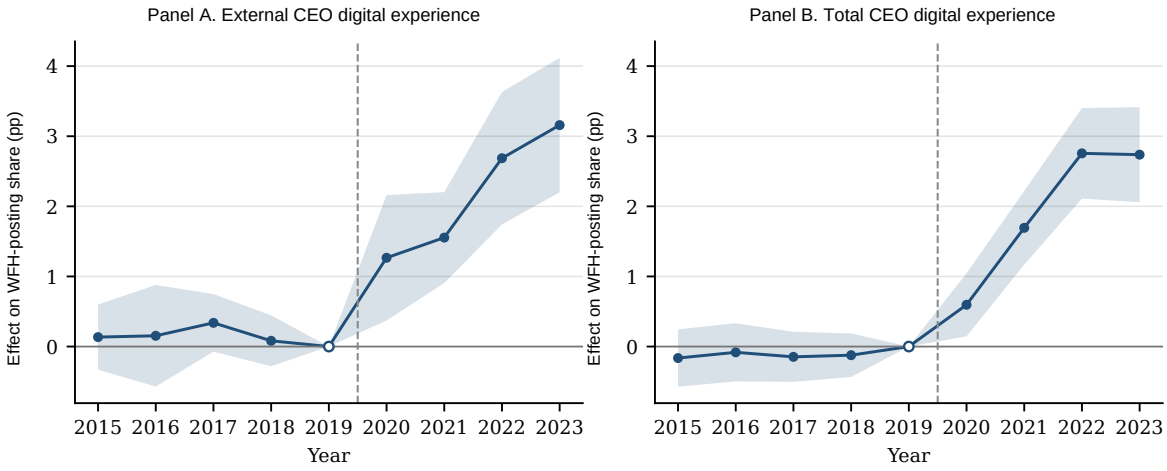
The main paper compares the pooled CEO available estimates using external and total CEO digital experience. Appendix Figure A1 compares their annual paths in the CEO common sample, which contains 6,319 firms and 35,096 observations. The total-experience pre-trend test has $p = 0.917$; the matching external-experience test has $p = 0.563$.

B.3 Exposure timing and source-organization WFH advertising

The external-experience measure excludes the focal firm but permits external roles held concurrently with the focal role. The first panel below imposes stricter observed timing by retaining only external role-years before the CEO's first observed focal-role year. Because the leadership records begin in 2014, leaders first observed in the focal role that year are excluded; the exercise does not recover appointment dates. The second panel uses a different source-organization characteristic: its pooled pre-pandemic propensity to advertise WFH. These exercises ask, respectively, whether external role-years observed only after the first observed focal-role year account for the result and whether the source organizations differed in their prior advertising of the same work arrangement.

B.4 Binary experience measures

The first binary measure equals one when the selected CEO has positive measured external digital leadership experience. It preserves the distinction between external and focal-firm experience but discards intensity. Its event study and pooled difference-in-differences estimates are reported

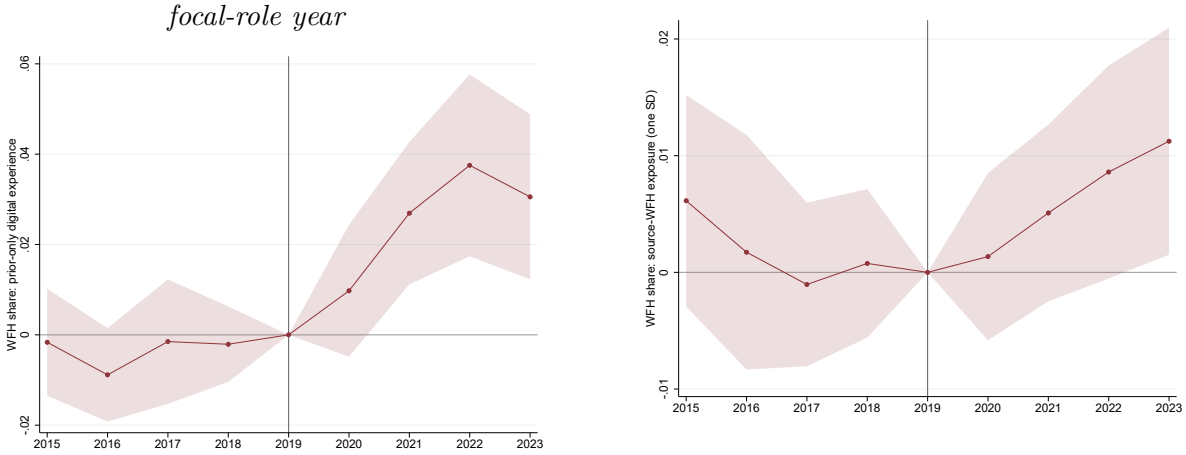


Appendix Figure A1: External and total CEO digital experience and the WFH-posting share in the CEO common sample

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of CEO digital experience with calendar year, relative to 2019. Panel A uses external CEO digital experience and Panel B total CEO digital experience. Both panels use the CEO common sample and a common y-axis. Shaded regions are 95 percent confidence intervals. Models include firm and calendar-year fixed effects and firm-clustered standard errors.

Panel A. Experience before the first observed focal-role year

Panel B. Source-organization WFH advertising



Appendix Figure A2: Alternative measures of external CEO organizational exposure

Notes: The dependent variable is the focal firm's WFH-posting share. Panel A measures external digital leadership experience using only role-years before the CEO's first observed focal-role year and uses the same benchmark-equivalent role-year unit as the main digital-experience measure. CEOs first observed in the focal role in 2014 are excluded because earlier focal-role years are unavailable; the exercise does not recover appointment dates. Panel B replaces source-organization digital-workforce intensity with the source organization's pooled 2014–2018 WFH-posting share and reports coefficients for a one-standard-deviation difference in that exposure. Points are year-specific exposure interactions relative to 2019; shaded regions are 95 percent confidence intervals. Specifications include firm and calendar-year fixed effects, no additional controls, and firm-clustered standard errors. The panels use measure-specific samples and scales. Source-WFH exposure is observed only for source organizations appearing in the vacancy data; partially observed source histories are retained when at least one source is observed, and the measure may incorporate source advertisements posted after the leader's role at that organization.

Appendix Table A3: Alternative timing and content of external CEO experience

	CEO available		CEO common	
Panel A. External digital experience before the first observed focal-role year	Original	Restricted timing	Original	Restricted timing
CEO exposure × 2020–2021	0.016***	0.021***	0.017***	0.020***
	(0.004)	(0.006)	(0.005)	(0.007)
CEO exposure × 2022–2023	0.031***	0.036***	0.032***	0.037***
	(0.006)	(0.008)	(0.007)	(0.009)
Pre-2020 outcome mean	0.037	0.037	0.035	0.035
Firms	3,193	3,193	1,743	1,743
Firm-years	17,552	17,552	9,903	9,903
Alternative-measure event-study pre-period p-value	–	0.508	–	–
Panel B. Source organizations' pre-pandemic WFH advertising	Original	Source WFH	Original	Source WFH
CEO exposure × 2020–2021	0.013***	0.002	0.014***	0.002
	(0.003)	(0.002)	(0.003)	(0.003)
CEO exposure × 2022–2023	0.026***	0.008**	0.029***	0.004
	(0.004)	(0.004)	(0.005)	(0.004)
Pre-2020 outcome mean	0.031	0.031	0.031	0.031
Firms	7,701	7,701	4,447	4,447
Firm-years	42,765	42,765	25,256	25,256
Alternative-measure event-study pre-period p-value	–	0.600	–	–

Notes: The dependent variable is the focal firm's WFH-posting share. Panel A expresses both measures in benchmark-equivalent role-years. The restricted-timing measure retains only external role-years before the CEO's first observed focal-role year. Leaders first observed in the focal role in 2014 are excluded because earlier focal-role years are unavailable; the exercise does not recover appointment dates. Panel B replaces source-organization digital-workforce intensity with the source organization's pooled 2014–2018 WFH-posting share. The original and source-WFH measures are standardized separately within each comparison sample, so their coefficients represent one-standard-deviation differences. Within each panel and sample, the original and alternative measures are estimated on identical firms. Leaders with no external role remain in Panel B; leaders with external roles must have WFH information for at least one source organization, and partially observed source histories may remain. All specifications include firm and calendar-year fixed effects, no additional controls, and firm-clustered standard errors. The reported pre-period p-values jointly test the 2015–2018 coefficients in the CEO-available annual event study for the panel's alternative measure. The pooled CEO-common comparisons are reported without corresponding annual event-study tests. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

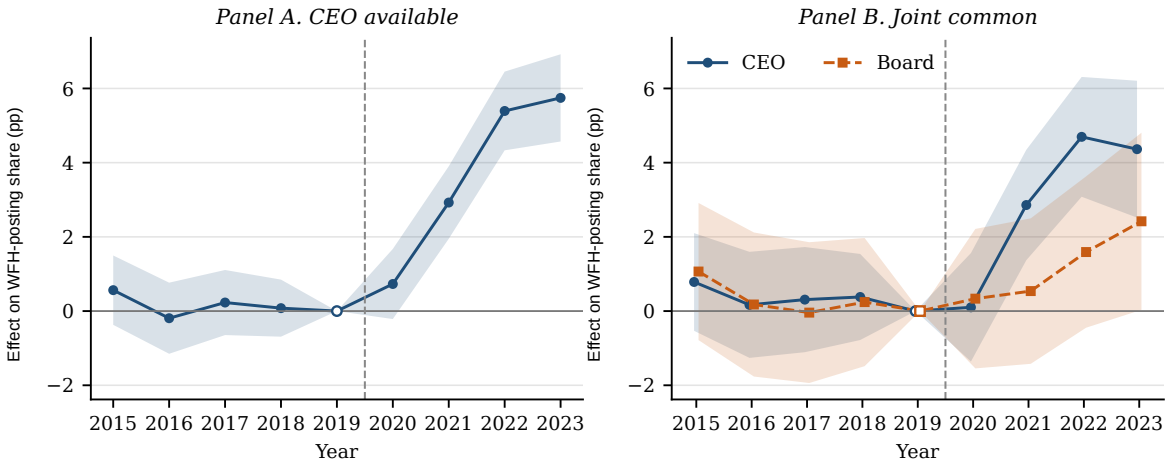
together in the main paper.

The broader binary measure records any qualifying leadership history at an organization with any positive digital employment; the board indicator equals one when any formal board member has such a history. The CEO available pre-trend test has $p = 0.585$. In the joint common model the CEO, board, and joint pre-trend tests have $p = 0.795$, $p = 0.670$, and $p = 0.642$.

Appendix Table A4: Any digital-workforce experience and the WFH-posting share

	CEO available	CEO common	Joint: CEO	Joint: board
Any digital-workforce experience × 2020–2021	0.018***	0.014***	0.013**	0.002
	(0.004)	(0.005)	(0.005)	(0.007)
Any digital-workforce experience × 2022–2023	0.054***	0.049***	0.042***	0.017**
	(0.004)	(0.005)	(0.006)	(0.008)
Exposed leaders (percent)	46.9	54.0	54.0	75.2
Pre-2020 outcome mean	0.031	0.030	0.030	0.030
Firms	10913	6319	6319	6319
Observations	59265	35096	35096	35096

Notes: The dependent variable is the WFH-posting share. The CEO indicator records any qualifying leadership history at an organization with any positive digital employment; the board indicator records whether any formal board member has such a history. Coefficients are interactions with the indicated period. Models include firm and calendar-year fixed effects and firm-clustered standard errors. The joint common columns come from one regression. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.



Appendix Figure A3: Any digital-workforce experience and the WFH-posting share

Notes: The dependent variable is the WFH-posting share. Points are annual interactions of the CEO or board indicator for any digital-workforce experience with calendar year, relative to 2019. Panel A shows the CEO available model. Panel B enters the CEO and board indicators jointly in the joint common sample. Both panels use the same y-axis. Shaded regions are 95 percent confidence intervals. Models include firm and calendar-year fixed effects and firm-clustered standard errors.

B.5 Sample restrictions

Appendix Table A5 reports the pooled estimates behind the robustness figure in the paper. Retaining the same CEO from 2019 through 2023 and requiring the firm to be present from 2017 through 2022 yield estimates close to the CEO common baseline. These are descriptive restrictions on post-2019 outcomes rather than a decomposition of turnover or attrition. The firm-presence condition ends in 2022, so its 2022–2023 pooled coefficient includes 2023 outcomes without requiring the firm to appear in the 2023 administrative records. The corresponding event-study pre-trend tests have $p = 0.563$, $p = 0.311$, and $p = 0.531$, respectively; none indicates a statistically detectable pre-period departure, although the tests have limited power.

Appendix Table A5: External CEO digital experience and the WFH-posting share under sample restrictions

	Baseline	Same CEO 2019–2023	Firm present 2017–2022
External CEO digital experience × 2020–2021	0.013***	0.011***	0.013***
	(0.003)	(0.004)	(0.003)
External CEO digital experience × 2022–2023	0.028***	0.027***	0.028***
	(0.004)	(0.006)	(0.004)
Pre-2020 outcome mean	0.030	0.029	0.030
Firms	6319	3515	6120
Observations	35096	19227	34306

Notes: The dependent variable is the WFH-posting share. All models use the CEO common sample, continuous external CEO digital experience, firm and calendar-year fixed effects, no additional controls, and firm-clustered standard errors. Coefficients are interactions with the indicated period. The firm-presence restriction requires observation in 2017–2022; its 2022–2023 coefficient includes 2023 outcomes without requiring administrative presence in 2023. Standard errors appear in parentheses directly beneath decimal-aligned coefficients.

B.6 Observed pre-pandemic firm capacity

Appendix Table A6 reports the compact covariate sequence discussed in the main paper's robustness section. The baseline, observed-characteristics, and observed-characteristics-plus-industry specifications use the same estimation sample within each panel. The full sequence, which adds firm characteristics, recruiting and digital history, WFH history, leader characteristics, and fixed-2019 industry in stages, is reported in the master appendix.

B.7 Vacancy support and weighting

The main WFH share gives each firm-year equal weight, even when firms differ greatly in the number of observed advertisements. Appendix Table A7 tests whether low-volume observations account for the result by progressively requiring more advertisements and by weighting firm-years by their vacancy counts. The later CEO and board estimates remain positive throughout. The

Appendix Table A6: WFH-posting share with observed-capacity adjustments

	(1) Fixed-sample baseline	(2) Observed characteristics × year	(3) Observed characteristics and NACE2 × year
Panel A. CEO available sample			
CEO experience × 2020–2021	0.0125***	0.0096***	0.0040
	(0.0023)	(0.0025)	(0.0027)
CEO experience × 2022–2023	0.0263***	0.0171***	0.0078**
	(0.0033)	(0.0033)	(0.0033)
Panel B. CEO common sample			
CEO experience × 2020–2021	0.0129***	0.0096***	0.0030
	(0.0028)	(0.0030)	(0.0033)
CEO experience × 2022–2023	0.0274***	0.0185***	0.0086**
	(0.0039)	(0.0038)	(0.0039)
Panel C. Joint CEO–board common sample			
CEO experience × 2020–2021	0.0070**	0.0048	0.0010
	(0.0030)	(0.0032)	(0.0034)
CEO experience × 2022–2023	0.0166***	0.0112***	0.0053
	(0.0039)	(0.0038)	(0.0039)
Board experience × 2020–2021	0.0197***	0.0182***	0.0101**
	(0.0039)	(0.0043)	(0.0044)
Board experience × 2022–2023	0.0387***	0.0296***	0.0173***
	(0.0052)	(0.0056)	(0.0057)
Firm and calendar-year fixed effects	Yes	Yes	Yes
Observed characteristics × year	No	Yes	Yes
Fixed-2019 NACE2 × year	No	No	Yes

Notes: The dependent variable is the firm-year WFH-posting share. Each panel uses one fixed estimation sample across columns. Observed firm characteristics are 2019 log employment, log sales, and age; 2017–2019 posting-volume and digital histories; the 2015–2019 rate of years with any observed vacancy; and 2015–2019 WFH levels and trends. CEO controls are birth year, sex, observed focal-firm tenure, and ownership; the joint model also controls for board size, mean age, mean observed tenure, and ownership. Continuous controls are mean-filled where missing and accompanied by year-interacted missing indicators; continuous controls and categorical sex and ownership measures are interacted with calendar year. Column 3 additionally absorbs fixed-2019 two-digit NACE industry by calendar year. Standard errors are clustered by firm and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

exercise therefore supports the persistent WFH relationship while showing that the immediate CEO estimate in the joint model is more sensitive to the treatment of low-volume firm-years.

Appendix Table A7: Sensitivity of the WFH-posting share to vacancy support and weighting

	Baseline	≥ 5 postings	≥ 10 postings	≥ 20 postings	Vacancy weighted
Panel A. CEO available					
External CEO digital experience × 2020–2021	0.012***	0.009***	0.007***	0.008***	0.006**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
External CEO digital experience × 2022–2023	0.026***	0.025***	0.023***	0.026***	0.017**
	(0.003)	(0.004)	(0.005)	(0.006)	(0.008)
Firms	10,913	3,952	2,026	986	10,913
Observations	59,265	19,399	9,983	5,045	59,265
Panel B. Joint common					
External CEO digital experience × 2020–2021	0.007**	0.004*	0.002	0.000	0.001
	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
External CEO digital experience × 2022–2023	0.017***	0.018***	0.016***	0.019***	0.016***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
External board digital experience × 2020–2021	0.020***	0.012***	0.016***	0.017***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
External board digital experience × 2022–2023	0.039***	0.038***	0.039***	0.037***	0.043***
	(0.005)	(0.006)	(0.008)	(0.009)	(0.006)
Firms	6,319	2,320	1,160	565	6,319
Observations	35,096	11,496	5,795	2,927	35,096

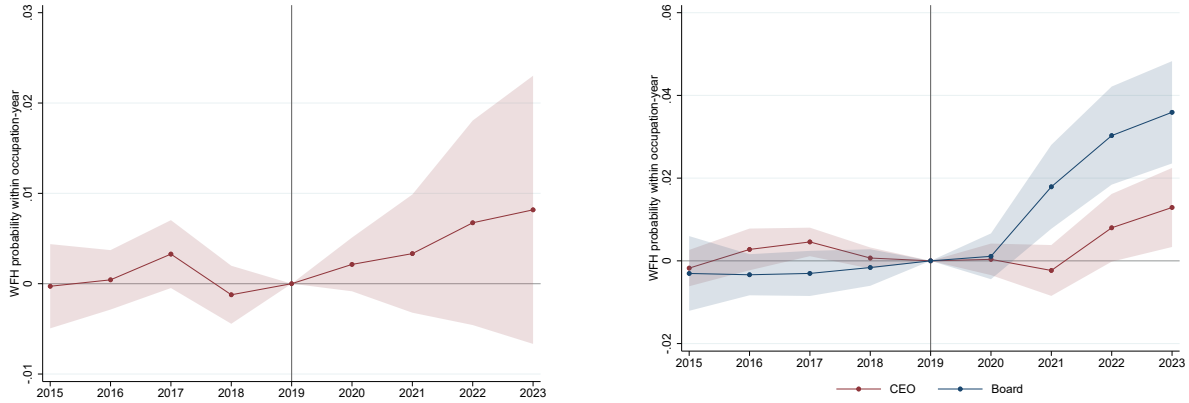
Notes: The dependent variable is the WFH-posting share. Columns 2–4 progressively exclude firm-years with few advertisements. The last column weights each firm-year by its number of advertisements, so high-volume recruiters receive more weight. The joint model includes CEO and board experience together. All models include firm and calendar-year fixed effects and firm-clustered standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

B.8 Occupation composition and within-occupation dynamics

The pooled occupation results in the main paper separate two concerns. The fixed-preperiod decomposition asks whether treatment-related changes in occupational mix, weighted by fixed preperiod WFH propensities, account for the WFH gradient. The within-occupation specifications compare WFH mentions among vacancies in the same four-digit occupation and year. The annual figures below show when the occupation-adjusted gradients emerge.

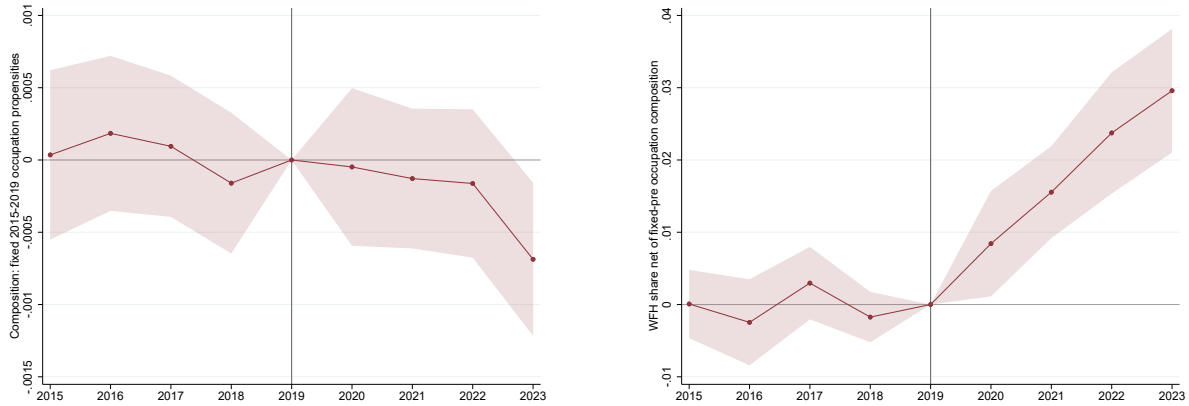
B.9 Selection into observed online vacancies

The main paper reports pooled results for the posting margin and two unconditional WFH outcomes. The event studies below retain each frozen-cohort firm from the later of 2015 and its legal or first-observed start year through 2023 and show the timing separately for any observed vacancy, any observed WFH vacancy, and WFH vacancies per 100 2019 FTE. The posting-margin paths are included to diagnose selection, not to support a causal interpretation of vacancy activity.



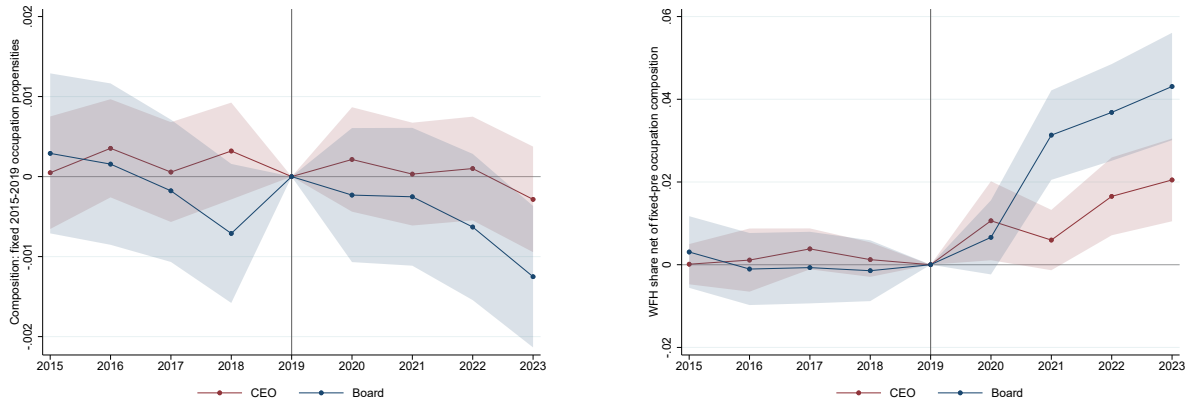
Appendix Figure A4: External digital leadership experience and WFH advertising within occupation-year cells

Notes: The dependent variable is the WFH share in a firm–occupation–year cell. Panel A reports the CEO available specification. Panel B enters CEO and board experience jointly in the common sample. Points are year-specific interactions relative to 2019. Frequency weights equal the number of vacancies in each cell, making the regression numerically equivalent to a vacancy-level linear-probability model. Models include firm and four-digit occupation-by-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals.



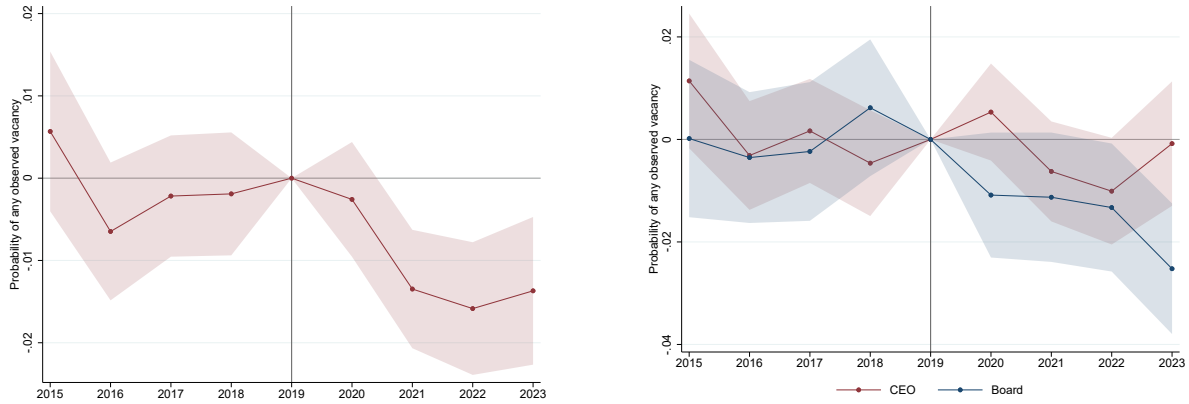
Appendix Figure A5: Fixed-preperiod occupation decomposition in the CEO available sample

Notes: Panel A reports the component of the firm-year WFH-posting share predicted by occupational shares when occupation-specific WFH propensities are fixed over 2015–2019. The propensity is leave-one-firm-out where supported, with inclusive occupation or overall preperiod fallbacks for unsupported cells. Panel B reports the observed WFH-posting share net of that component. Points are annual interactions of external CEO digital experience with calendar year, relative to 2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals.



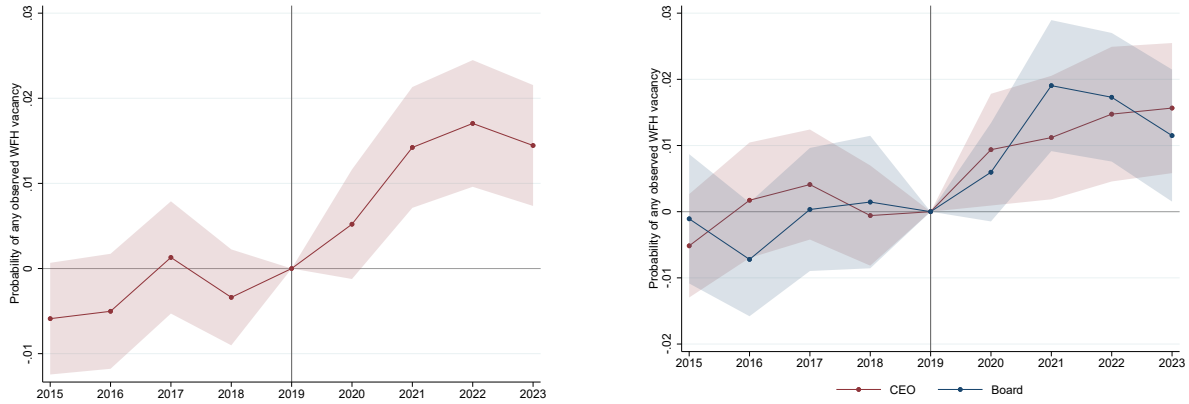
Appendix Figure A6: Fixed-preperiod occupation decomposition in the joint common sample

Notes: Panel A reports the component of the firm-year WFH-posting share predicted by occupational shares when occupation-specific WFH propensities are fixed over 2015–2019. The propensity is leave-one-firm-out where supported, with inclusive occupation or overall preperiod fallbacks for unsupported cells. Panel B reports the observed WFH-posting share net of that component. CEO and board experience enter jointly. Points are annual interactions relative to 2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals.



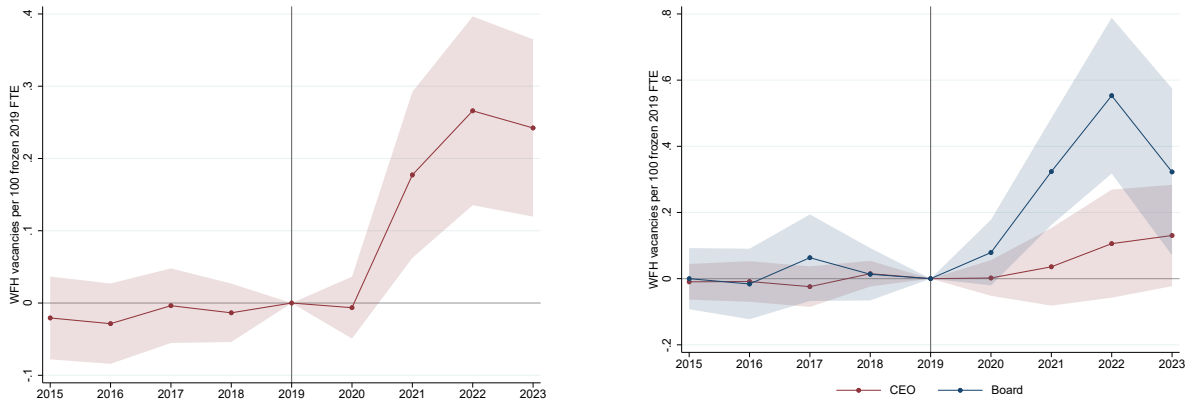
Appendix Figure A7: External digital leadership experience and any observed online vacancy

Notes: The dependent variable equals one when the firm has any observed online vacancy. Panel A reports the CEO available specification; Panel B enters CEO and board experience jointly. Every in-scope firm-year described above is retained. Points are year-specific interactions relative to 2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals. The CEO pre-period test is borderline in Panel A and rejects at the 5 percent level in Panel B, so the negative post-period coefficients are not interpreted causally.



Appendix Figure A8: External digital leadership experience and any observed WFH vacancy

Notes: The dependent variable equals one when the firm has at least one observed WFH vacancy. Firm-years with no observed vacancy receive zero. Panel A reports the CEO available specification; Panel B enters CEO and board experience jointly. Points are year-specific interactions relative to 2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals.

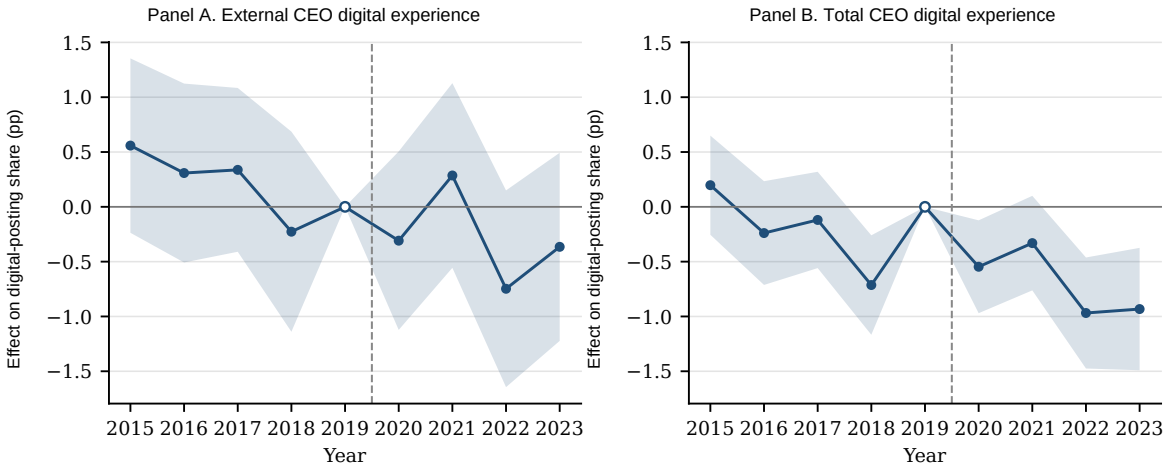


Appendix Figure A9: External digital leadership experience and WFH vacancies per 100 2019 FTE

Notes: The dependent variable is the number of observed WFH vacancies per 100 2019 FTE. Firm-years with no observed vacancy receive zero. Panel A reports the CEO available specification; Panel B enters CEO and board experience jointly. Points are year-specific interactions relative to 2019. Models include firm and calendar-year fixed effects and firm-clustered standard errors. Shaded regions are 95 percent confidence intervals.

B.10 Digital-posting-share dynamics

The pooled digital-hiring estimates in the main paper do not show whether the timing differs between external and total CEO experience. The figure below reports the corresponding annual paths. It is placed in the appendix because the main paper's pooled-outcomes table already reports the digital-posting outcomes.



Appendix Figure A10: CEO digital leadership experience and the digital-posting share

Notes: The dependent variable is the digital-posting share. Points are annual interactions of CEO digital experience with calendar year, relative to 2019, in the CEO available sample. Panel A uses external CEO digital experience and Panel B total CEO digital experience. Both panels use a common y-axis. Shaded regions are 95 percent confidence intervals. Specifications include firm and calendar-year fixed effects and firm-clustered standard errors. The hollow marker denotes the omitted year; the dashed vertical line separates 2019 from 2020.

B.11 Placebo details

The main paper reports the placebo paths, and Appendix Table A8 gives the joint tests. The placebo selects the CEO and non-CEO board from the 2017 leadership records. Person-level external digital experience is accumulated during 2014–2016 and excludes the focal firm. Assignment to firms uses the 2017 leadership roster, so the complete firm-level exposure is predetermined relative to the artificial 2018 break, not necessarily relative to the 2017 reference year. The measure otherwise uses the main specification's qualifying leadership roles, DISCO digital-occupation classification, annual seventy-fifth-percentile scaling, and averaging across simultaneous roles.

Firm eligibility is determined from the 2017 business record. The placebo retains corporate legal forms 60, 80, and 81 with at least 9.5 full-time-equivalent workers and excludes NACE divisions 84–88. Sales are not used to define this earlier cohort. The CEO available sample requires an identified 2017 CEO; the joint common sample also requires at least one non-CEO 2017 board member. Unlike the main 2019 cohort, the placebo does not require positive sales, complete demographic histories, or leaders who are at least age 30.

The outcome window is 2014–2019. The underlying vacancy file begins in 2010 even though the main estimation panel begins in 2015. The 2017 coefficient is omitted, and 2018–2019 are the artificial post years. For each single series, the pre-period test covers the three interactions for 2014, 2015, and 2016, while the artificial-post test covers the two interactions for 2018 and 2019. The combined CEO–board row tests all six pre-period interactions or all four artificial-post interactions. The CEO available 2018 coefficient is individually different from zero at the 5 percent level; its reported artificial-post p-value of 0.068 is the broader two-coefficient test. The placebo is therefore mixed rather than uniformly null. The CEO point estimates remain negative in 2019, while the board estimate reverses sign; neither path reproduces the positive and increasing post-2019 pattern.

Appendix Table A8: External CEO and board digital experience and the WFH-posting share: pre-2019 timing placebo

Sample/model	2014–2016 joint p	2018–2019 joint p	Firms
CEO available: CEO	0.778	0.068	9516
Joint common: CEO	0.607	0.555	7209
Joint common: board	0.709	0.093	7209
Joint common: CEO and board	0.728	0.083	7209

Notes: The dependent variable is the WFH-posting share. The placebo fixes the 2017 leadership cohort and accumulates external digital experience during 2014–2016. The estimation window is 2014–2019, with 2017 omitted and 2018–2019 treated as the artificial post period. For the first three rows, the first p-value tests the relevant series' 2014, 2015, and 2016 interactions jointly; the second tests its 2018 and 2019 interactions jointly. In the final row, the corresponding tests cover all six pre-period CEO and board interactions and all four artificial-post interactions. The CEO available 2018 coefficient is individually different from zero at the 5 percent level; 0.068 is the broader joint p-value for 2018 and 2019. Appendix Section B.11 gives the cohort and support rules. Firm-year observations and the pre-period outcome mean are not reported in the retained output.