JOURNAL OF FINANCIAL AND QUANTITATIVE ANALYSIS Vol. 59, No. 4, June 2024, pp. 1541–1585
© The Author(s), 2023. Published by Cambridge University Press on behalf of the Michael G. Foster School of Business, University of Washington. This is an Open Access article, distributed under the terms of the Creative Commons Attribution licence (https://creativecommons.org/licenses/by/4.0), which permits unrestricted re-use, distribution and reproduction, provided the original article is properly cited.
doi:10.1017/S0022109023000522

Corporate Hiring Under COVID-19: Financial Constraints and the Nature of New Jobs

Murillo Campello
Cornell University Johnson Graduate School of Management and NBER
campello@cornell.edu (corresponding author)

Gaurav Kankanhalli
University of Pittsburgh Katz Graduate School of Business
gkankanhalli@katz.pitt.edu

Pradeep Muthukrishnan
Tulane University Freeman School of Business
pmuthukrishnan@tulane.edu

Abstract

Big data on job postings reveal multiple facets of the impact of COVID-19 on corporate hiring. Firms disproportionately cut new hiring for high-skill positions, with financially constrained firms reducing skilled hiring the most. Applying machine learning methods to job-ad texts, we find that firms have skewed their hiring toward operationally-core functions. New positions display greater flexibility regarding schedules and tasks. While job posting levels show signs of recovery starting in late-2020, changes to job descriptions and skill profiles persist through early-2022. Financial constraints amplify these changes, with constrained firms' new hires witnessing greater adjustments to job roles and employment arrangements.

I. Introduction

The onset of the COVID-19 pandemic triggered the largest economic dislocation in decades. While prior shocks to corporate activity came through channels such as the supply of financial capital, competition, and technology, the COVID-19

We thank an anonymous referee and Ran Duchin (the editor) for insightful suggestions. We are grateful to Tom Ashenmacher at LinkUp and David Gilbertson at UKG (Ultimate Kronos Group) for data access and support. We thank Warren Bailey, Utpal Bhattacharya, Charles Calomiris, Sudheer Chava, Will Cong, Gustavo Cortes, Juan Dubra, Daniel Ferrés, Julia Fonseca, John Graham, Jussi Keppo, Hyunseob Kim, Ernst Maug (WFA discussant), Justin Murfin, Marcus Painter (FMA discussant), Kosali Simon, David Sraer, Eva Steiner (FIFI discussant), Margarita Tsoutsoura, Michael Weisbach, and conference and seminar participants at the 2021 Financial Management Association Meeting, 2021 International Conference of the Financial Engineering and Banking Society, 2020 Journal of Finance and Chicago Booth Fama-Miller Center Conference on the Financial Consequences of the COVID-19 Pandemic, 2021 Fixed Income and Financial Institutions Conference, 2021 National University of Singapore Analytics for X Conference, 2020 Shanghai-Edinburgh Fintech Conference, 2021 Western Finance Association Meeting, Georgia Tech, Hong Kong University of Science and Technology, Office of the Comptroller of the Currency, Universidad de Montevideo, University of Edinburgh, and University of Hong Kong for valuable comments.

https://doi.org/10.1017/S0022109023000522

https://doi.org/10.1017/S0022109023000522

health crisis uniquely impacted firms' human capital and workplace (see Duchin and Harford (2021)). The initial rapid spread of the coronavirus led to unprecedented disruptions in work environments alongside high unemployment. Understanding the forces driving these dynamics requires assessing how corporate hiring and employment arrangements have evolved under the pandemic. It also involves assessing the role played by credit access in shaping outcomes.

Hiring is a costly, forward-looking investment in human capital and the decision to recruit workers reflects the constraints that firms face. This article analyzes these decisions throughout the COVID-19 crisis using *big data* on millions of job postings sourced from websites and hiring boards of thousands of companies across all industries and all 50 states. Our main data come from a leading labor market analytics firm (LinkUp) and contain detailed information about the employer, position sought, desired worker skills, and location of each job from Jan. 2017 to Jan. 2022; encompassing 3 years before the pandemic hit the United States and 2 years after. The database is unique in featuring the full-text description of job vacancy postings. This allows us to develop several machine learning-based metrics to assess how corporate hiring and employment arrangements have evolved under COVID-19. We complement this information with data on workers' online job-seeking efforts (Google Trends). Administrative data (Job Openings and Labor Turnover Survey, JOLTS, and Quarterly Workforce Indicators, QWI) and private-sector payroll information (from Kronos, a leading HR provider) are further utilized to verify that our job postings data are representative and predictive of realized employment levels. Using a firm-location-time triple panel, we are able to trace and contrast hiring changes within firms and across different geographical areas over time, incorporating an array of firm financial information. Our extended sample period allows us to gauge long-term hiring impacts of the pandemic and associated labor market developments such as the "great resignation" wave of 2021–2022.

It is important to lay out our priors concerning the impact of the COVID-19 pandemic on corporate hiring. COVID-19 embeds a number of shocks that inform our analysis. At a basic level, the onset of the pandemic embeds a negative "demand shock" for firms across the board; particularly those operating in areas and activities most affected by the health dimension of the crisis (see Bloom, Fletcher, and Yeh (2021), Papanikolaou and Schmidt (2022)). COVID-19 also embeds an "uncertainty shock" (Altig, Baker, Barrero, Bloom, Bunn, Chen, Davis, Leather, Meyer, Mihaylov, Mizen, Parker, Renault, Smietanka, and Thwaites (2020)). We characterize the impact of these shocks on firms' hiring decisions under a real options framework. The framework suggests that the more irreversible the decision to invest in human capital, the stronger the incentive for firms to wait before committing to hire (Bloom (2009)). Critically, this friction drives a heterogeneous effect on firm hiring decisions that is based on worker skills. Simply put, high-skill hiring involves greater fixed costs,[1] making such decisions costlier to reverse than low-skill hiring. Under this framework, firms should hire relatively fewer high-skill workers under the pandemic, disproportionately cutting on searches for positions such as managers and scientists relative to attendants and sales workers (*within-firm*

[1]These costs include training and certification costs, firing costs, and contractual rigidities such as noncompetes.

https://doi.org/10.1017/S0022109023000522

downskilling). Notably, financial constraints amplify firms' "wait-and-see" incentives in the face of uncertainty (see Gilchrist, Sim, and Zakrajšek (2014), Alfaro, Bloom, and Lin (2018)). Access to financing should thus enhance firms' ability to modify their hiring during the pandemic (see Michelacci and Quadrini (2009), Caggese, Cuñat, and Metzger (2019)).

We study the above predictions with our comprehensive data. Our baseline tests show that firms cut their job postings by 58% of the pre-pandemic weekly average from the onset of the pandemic in Week 9 of 2020 (when the first COVID-19 fatality was reported in the United States) through Week 36 of 2020 (Labor Day); hiring cuts tapered off afterward. *Within-firm* analyses reveal that cuts were more pronounced at the high end of the worker skill spectrum. In particular, after mapping each job posting's data to a worker-skill level using the O*NET-Job Skill Zone linking table (Autor, Levy, and Murnane (2003)), we find substantially larger hiring cuts in high-skill postings compared to low-skill ones. In the first 28 weeks of the pandemic, the firm ratio of high-to-low-skill job ads declined by 7 percentage points; one-fourth of the ratio for the same weekly window in the pre-pandemic years.

Characterizing our base findings, we show that the COVID-19 contagion *dynamically drives* observed outcomes as we condition our tests on the local-area spread of the coronavirus. To wit, location-time-specific estimations show that cuts in new job postings (particularly for high-skill positions) were progressively more pronounced in areas that registered higher levels of COVID cases. Our work further provides evidence that the changes in corporate hiring patterns are driven by demand-side preferences, even in the face of a tighter labor supply (see, e.g., Domash and Summers (2022)). While unable to completely separate the effects of concurrent labor demand and supply shifts, our tests suggest that measures of COVID exposure explain observed changes in hiring levels even after controlling for various dimensions of labor supply, including state-level unemployment insurance policies and lockdowns. As the pandemic posed challenges to human interaction protocols, issues such as workplace logistics became critical. Building on this dimension of the crisis, our analysis further shows that firms with a high share of jobs that can be performed remotely posted fewer job vacancy ads, particularly for high-skill roles.

Our study uniquely shows how firms adjusted the *nature of positions* they seek to fill and associated *employment arrangements* in response to the pandemic. Our database allows us to develop relevant metrics to this end by applying machine learning and natural language processing methods to job-ad texts. We do so within firm-ZIP-week triples over 5 years of data. The first metric that we develop is CORE_JOBS, through which we show that firms focused their hiring far more into positions that are "core" to their operations; jobs whose functions are more tightly aligned with their principal business lines. Second, we develop a metric of JOB_FLEXIBILITY, which is a particularly relevant dimension to study, given the abrupt transition to alternative work arrangements required by social distancing protocols. We measure the degree of flexibility embedded in a new position by computing cooccurrences of "flexibility"-related keywords (identified via a word-embedding model) in a job posting. Using this metric, our results point to a large increase in the array of schedules and tasks associated with a new job opening since the pandemic.

https://doi.org/10.1017/S0022109023000522

In an extended sampling window that tracks job posting outcomes through early 2022, we show that declines in the levels of job postings are transitory, while qualitative changes in the skill profiles and textual descriptions of job postings are long-lived.

Our article is the first to study the role of financial constraints in shaping hiring under COVID-19. We identify and report significant heterogeneity in hiring along firm financial constraints. We do so using multiple metrics of firms' ex ante access to funding, such as their size, credit ratings, access to credit lines, and liquid assets. Across all such proxies, we find that financial constraints amplified job posting cuts at the onset of the pandemic; particularly more so for high-skill positions. Small firms in our sample, for example, reduced their weekly new postings by 23% more of their pre-pandemic average than did their larger local-area counterparts. Likewise, firms without bank credit lines cut their job postings by 16% more of the 2017–2019 average than comparable firms with at least one line available.

Our tests show that changes to the nature of hiring are also magnified for financially constrained firms. Those firms altered job attributes the most since the pandemic, skewing their new hiring toward core positions and increasing the degree of flexibility required under each job role. Additional analyses seek to provide plausible causal evidence of the effect of financial constraints on hiring by exploiting pre-crisis variation in firms' debt maturity profiles (Almeida, Campello, Laranjeira, and Weisbenner (2012)) and risk of losing investment-grade status (Acharya and Steffen (2020)). These added tests identify firms' lack of access to financing as a key factor pushing them to alter various quantitative and qualitative dimensions of their hiring since the pandemic. One important insight from our results is that workers hired by financially constrained firms seem to bear a greater burden of adjustment into new employment arrangements under COVID-19.

Our findings paint a rich picture of corporate hiring responses under the pandemic across four distinct dimensions: firms reduce their new hiring, disproportionately for high-skill roles, while simultaneously increasing job flexibility and core job functions. These hiring dynamics were more pronounced for firms with high "work-from-home" adaptability and those facing greater financial constraints. Taken together, they suggest that firms' ability to retain existing high-skill workers through flexible work arrangements, combined with the option to defer irreversible hiring costs, played a central role in driving corporate pandemic hiring responses. As changes to workplace arrangements are expected to outlive the immediate effects of the pandemic, our results suggest lasting effects on the nature of new jobs.

Our study contributes to a growing body of important work on the impact of the COVID-19 pandemic on firms. A nonexhaustive list of papers includes Acharya and Steffen (2020), Ramelli and Wagner (2020), Bai, Brynjolfsson, Jin, Steffen, and Wan (2021), Bloom et al. (2021), Cejnek, Randl, and Zechner (2021), Ding, Levine, Lin, and Xie (2021), Fahlenbrach, Rageth, and Stulz (2021), Kargar, Lester, Lindsay, Liu, Weill, and Zúñiga (2021), Lewellen and Severino (2021), O'Hara and Zhou (2021), Pettenuzzo, Sabbatucci, and Timmermann (2021), and Papanikolaou and Schmidt (2022), who gauge much of that impact based on stock returns, bond yields, and firm financial policies and performance. None of those papers focus on corporate hiring. Closer to our work, Cajner, Crane, Decker, Grigsby, Hamins-Puertolas, Hurst, Kurz, and Yildirmaz (2020) look at broad metrics of employment

https://doi.org/10.1017/S0022109023000522

using firm-anonymized payroll data, while Barry, Campello, Graham, and Ma (2022) utilize anonymous CFO surveys. Our study adds to this literature and yet is different from existing papers by providing *granular*, *firm-*, and *job-level* analyses of hiring during the pandemic, characterizing the most affected companies and types of jobs.

Our work also adds to the literature on the interaction between firm financial constraints and employment. Existing work suggests that firm financial constraints negatively impact decisions regarding employment growth (Pagano and Volpin (2008), Chodorow-Reich (2014), Falato and Liang (2016), Giroud and Mueller (2017), and Benmelech, Bergman, and Seru (2021)), wages (Graham, Kim, Li, and Qui (2019)), worker skills (Baghai, Silva, Thell, and Vig (2021)), and labor supply (Brown and Matsa (2016)). Our study is unique in showing how financial constraints play a critical role in amplifying changes to hiring and the nature of work under a *health crisis*. Notably, it sheds light on the extent to which firm financial constraints influence whether the adjustment costs are borne by firms versus workers.

Exploiting big data via machine learning methods, we assess how COVID-19 has altered the nature of corporate jobs in nonstandard ways, including changes in worker skills, work schedule and location, employment flexibility, and job role orientation. Our analysis pushes forward a growing finance literature that applies AI-based techniques to textual data (examples include Hoberg, Phillips, and Prabhala (2014), Hoberg and Phillips (2016), Manela and Moreira (2017), Gentzkow, Kelly, and Taddy (2019), Bybee, Kelly, Manela, and Xiu (2020), Loughran and McDonald (2020), Bena, Erel, Wang, and Weisbach (2021), Kelly, Papanikolaou, Seru, and Taddy (2021), and Li, Liu, Mai, and Zhang (2021a)).

II. Conceptual Framework

We use a simple real-options-based conceptual framework to guide our tests of the heterogeneous impact of the COVID-19 pandemic on corporate hiring across the spectrum of worker skill levels. The pandemic combines both negative first-moment ("bad news") and positive second-moment ("uncertainty") shocks to business conditions (see, e.g., Altig et al. (2020)). We use this framework to develop an analytical model in which we explicitly solve for a representative firm's corporate hiring decisions in Appendix A. In what follows, we discuss the economic intuition underlying that model representation and what can be brought to testing.

Consider the decision of a firm to hire a new worker; that is, post a new job advertisement. Let the firm's decision problem involve investing in two "types" of human capital: high-skill hiring and low-skill hiring. These hiring decisions differ (among other margins) in the extent of fixed costs (and thus degree of irreversibility) incurred. Hiring represents a forward-looking investment in human capital by the firm, and therefore, the manager chooses whether or not to invest based on her expectations of future cash flows to the firm. In forming these expectations, the manager considers both the first and second moments of the distribution of future cash flows.

The firm incurs sunk costs when hiring a new worker. Hiring costs vary with the job skill level. Such costs may include the outlays on the recruiting process

https://doi.org/10.1017/S0022109023000522

(e.g., screening and interviewing costs) as well as initial on-boarding and training expenses. Hiring costs are partly irreversible as they cannot be recouped even if the hiring decisions are later reversed (e.g., through layoffs). We assume that the irreversible costs of hiring a new high-skill worker exceed that of recruiting a new low-skill worker. This assumption is supported by a sizeable literature in labor economics on the particular rigidities of high-skill relative to low-skill hiring (see, among others, Oi (1962), Blatter, Muehlemann, and Schenker (2012)). As we explain below, these differential costs of irreversibility imply that firms tend to cut back disproportionately more on their high-skill postings relative to low-skill postings, leading to the emergence of within-firm downskilling under the pandemic.

The firm can choose (either) to "hire now" or "hire later." If a firm "hires now" it incurs sunk costs, ex ante, whereas if it chooses to "hire later" it forgoes the initial cash flow that comes from labor input. However, the firm can observe how conditions evolve and choose to hire and commit to sunk costs if, and only if, realized conditions are sufficiently favorable to cover those costs, ex post. In other words, the *option to delay hiring* is a valuable one in the face of irreversible hiring costs.

Prediction 1. The negative first-moment component and the increase in uncertainty brought on by the positive second-moment component of the COVID-19 shock both lead to a decline in new high- and low-skill job postings.

Prediction 1 is informative in that it likely captures conditions faced by most firms at the onset of the COVID-19 pandemic. Next, we address the role played by the differential costs of irreversibility of high-skill and low-skill hiring. Notably, if the firm faces greater irreversibility costs in its high-skill hiring, it will advertise for even fewer such workers (relative to low-skill counterparts) under increased uncertainty. In other words, uncertainty reduces hiring across the board, and the effect is shaped by the degree of costs incurred by reversing their skills-based hiring decisions. This gives rise to our second empirical prediction:

Prediction 2. An increase in uncertainty brought on by the second-moment component of COVID-19 leads to disproportionately fewer high-skill job postings than low-skill job postings.

Our framework further speaks to the corporate hiring responses to emergent trends including the rise of "work-from-home" arrangements, the tightening of firm financial constraints, and the contraction of labor supply witnessed over the pandemic period. Flexible work arrangements provide firms with an option to retain existing workers, particularly high-skill workers who tend to be concentrated in nonphysical roles (Dingel and Neiman (2020)). The widespread adoption of flexible work-from-home arrangements under COVID-19 presented firms with a novel means of retaining existing workers (particularly high-skill workers). In our framework, an increased ability to retain workers on the part of the firm reduces its incentives to hire new roles (reducing new job postings according to Prediction 1). From Prediction 2 it follows that the effect is particularly pronounced for high-skill roles, which have greater associated irreversibility costs. Prediction 3 summarizes this intuition:

https://doi.org/10.1017/S0022109023000522

Prediction 3. Higher access to flexible work arrangements ("work-from-home") combined with the second-moment component of COVID-19 leads to disproportionately fewer new high-skill job postings than low-skill job postings.

The tightening of financial constraints represents a positive shock to the cost of capital of the firm. All else equal, this reduces the set of new workers that the firm can profitably hire as the irreversible costs of hiring become more binding (see also Alfaro et al. (2018)). When faced with the dual negative first-moment and positive second-moment pandemic shock, the firm is forced to undertake only the new hiring that it can afford under its reduced capital availability, and corresponding levels of high- and low-skill job postings are also reduced (following from Predictions 1 and 2). The following prediction summarizes this intuition:

Prediction 4. Under the combined first- and second-moment components of COVID-19, higher firm financial constraints lead to lower levels of new high- and low-skill job postings.

The last pandemic-related development we analyze is the notable contraction in labor supply that occurred during this period. While the framework abstracts away from the supply-side decisions of workers, one can still assess the potential implications of a labor supply contraction by viewing it as a restriction on the pool of workers a firm can hire from. This restraint on labor supply would reduce the firm's new hiring into both high- and low-skill roles, as stated in the following prediction:

Prediction 5. The aggregate contraction in labor supply under COVID-19 constrains firms' hiring abilities leading to declines in new high- and low-skill job postings.

In what follows, we take these predictions to our granular data on firms' job posting activities covering the duration of the COVID-19 pandemic.

III. Data and Main Variables

A. Job Postings Data

The core of our data is obtained from LinkUp, a leading provider of job market data and analytics. LinkUp maintains a comprehensive database of job openings sourced directly from nearly 60,000 employers since 2007. These data are continuously updated by crawling corporate websites, capturing information on, among other things, job posting creation, modification, and deletion dates. LinkUp's data gathering approach differs from that of other job postings databases that obtain their data from online job boards (e.g., Burning Glass). We note that there are several advantages to sourcing postings directly from employer websites as opposed to third-party job boards. First, firms update their own websites more regularly than they update job boards. As a result, posting modification and deletion dates are accurate, reflecting only current, relevant job openings. Second, employers pay job boards a fee to post a job for a pre-determined time window, creating staleness in

https://doi.org/10.1017/S0022109023000522

data sourced from such aggregators. Third, since a firm will post a job opening only once on its website (as opposed to multiple ads for the same job across various job boards), the problematic issue of "duplicate postings" is eliminated.

Our sampling runs from Jan. 2017 to Jan. 2022. We focus on American firms and job postings. The basic data for each posting contain information on the job title, firm identifier, and geographical tracking to the ZIP code level. LinkUp further attributes an O*NET occupation code to each posting based on a natural language processing algorithm.[2] The final data encompass some 27,000 firms, both public and private. Our analysis centers on publicly listed U.S. firms. We match these firms to their tickers and Compustat GVKEYs in order to obtain firm-level control variables from a variety of data sources that we discuss in subsequent sections. To gauge the skill level of a job posting, we map each posting's O*NET code to a Job Skill Zone (1 to 5 scale) based on the O*NET-Job Skill Zone linking table.[3] We perform several detailed checks, outlined in Appendix B, aimed at validating the comprehensiveness and representativeness of the LinkUp job postings data. In these data validation tests, we find ample support for the fact that the LinkUp data are consistent with those reported by administrative sources.

B. Other Data Sources

Our analysis uses additional data on firm fundamentals and operations, labor markets, credit conditions, as well as geography-level information. We obtain firm financial data from Compustat's Quarterly and Annual files. For information on firm employment, we use the Your-economy Time-Series (YTS) database, maintained by the Business Dynamics Research Consortium at the University of Wisconsin. The YTS database is compiled from Infogroup's historical business files and are linked longitudinally to track location, employment, and sales information at the establishment level for both public and private firms. We obtain weekly data on the number of individuals' searches for job openings by state from Google Trends from Jan. 2017 to Jan. 2022. Information on firms' credit ratings comes from Thomson Reuters Eikon. Data on outstanding credit lines are from WRDS-Reuters DealScan. We classify job postings into jobs that may be performed remotely using the O*NET occupation code-level teleworking index from Dingel and Neiman (2020). We also use the industry-level teleworking classification scheme proposed by Papanikolaou and Schmidt (2022) based on American Time Use Survey (ATUS) data. State-level unemployment and labor force figures are from the U.S. Department of Labor. We utilize statistics on daily recorded COVID-19 cases in each U.S. county from the New York Times, which we map to 3-digit ZIP codes.

[2]We manually inspect the full job ad texts for a random subset of posts to verify that the assigned O*NET occupation codes closely approximate the main function of the roles being advertised. We also verify that the location assigned by LinkUp matches with the actual geographical location identified from reading the text of the job postings.

[3]The O*NET classification of Job Skill Zones is based upon the Specific Vocational Preparation required for an occupation as per the Dictionary of Occupational Titles (see Autor et al. (2003)); available on O*NET Online.

https://doi.org/10.1017/S0022109023000522

For further data validation checks, we supplement the LinkUp database with data on employee payroll records from Kronos, a leading provider of workforce management services. Kronos works with more than 30,000 companies employing nearly 4 million workers across all 50 states. The HR company shared with us anonymized weekly data on (either in person or via remote work, or both) person-hours worked from Mar. 2019 through Dec. 2020. Kronos gathers data directly from time sheets submitted by hourly-wage employees with a typical lag of only 2–3 days. Their data allow us to assign firms to ZIP codes, industry, and size categories on a weekly basis.

C. Variable Construction and Measurement

1. Job Posting Activity and Worker Skill Levels

We collapse the job posting-level data into a firm-week-ZIP code panel consisting of over 17 million observations, representing 2,212 public firms. We compute the dependent variables in our base tests using this panel as follows: NEW_JOB_POSTINGS is the log of 1 plus the total number of new job postings *created* by a given firm in a given week in a given 3-digit ZIP code area.[4]

Our next dependent variable gauges heterogeneity in the skill profile of job postings. We compute the HIGH_TO_LOW_SKILL_RATIO as the number of job postings created in O*NET Job Zone 5 divided by the number of job postings created in O*NET Job Zone 1 for a given firm-ZIP-week triple. Through this ratio, we can measure whether hiring activity *within a firm and local labor market* is skewed toward low-skill (if the ratio is <1) or high-skill (if the ratio is >1) positions under the pandemic. A ratio < 1 for a construction firm in our sample, for example, would indicate downskilling, or increased hiring into low-skill positions such as cement masons, concrete finishers, and painting, coating, and decorating workers (Job Zone 1) relative to hiring into high-skill positions such as architectural and engineering managers, environmental engineers, and materials scientists (Job Zone 5).[5]

2. Job Description Textual Measures

We study two salient dimensions of individual job posting descriptions using textual analysis and natural language processing (NLP) tools as follows:

[4]We follow Chetty, Friedman, and Saez (2013) and Chetty, Friedman, Hendren, Stepner, and The Opportunity Insights Team (2020) in defining the boundaries of our geographical analysis. There are 899 3-digit ZIP codes in the United States and they provide for more granular mapping than commuting zones (709) or MSAs (392), yet allow for more meaningful estimations than 5-digit ZIP codes (often arbitrarily assigned to large buildings or universities). The mean (median) population of a 3-digit ZIP code is 349,490 (212,964) based on the 2010 Census. We show in Table IA.8 in the Supplementary Material that our results are robust to alternative levels of geographical aggregation.

[5]A select list of occupations in Job Zones 1 and 5 is in Table IA.1 in the Supplementary Material (see O*NET OnLine for a complete listing). Figure IA.1 in the Supplementary Material depicts the distribution of job postings across the five skill zones. A small fraction (<5%) of job postings belong to O*NET occupation codes that are not mapped to any corresponding skill zone. Our baseline results are robust to the exclusion of these job postings. As shown in Table IA.7 in the Supplementary Material, our results are robust to alternate definitions of low- and high-skill jobs.

https://doi.org/10.1017/S0022109023000522

Core Jobs. Our first measure captures how central a given job's function is to the firm's overall operations. Conceptually, we define a posted job as being "core" if its text is highly similar to the posting firm's own business description (e.g., an accountant is more core to an accounting firm than to a technology firm). As explained next, we develop a tangible measure of how core a job is by identifying the distinguishing characteristics of a firm's own business description and comparing it with the distinguishing features of the job's posted text.[6]

We first gather texts of business descriptions from each firm's recent (pre-COVID-19) 10-K filings. To extract the distinguishing characteristics from these texts, we use a TextRank algorithm (see Mihalcea and Tarau (2004)), which works as follows: The algorithm first extracts noun and verb chunks from a text and links them to other parts of speech, sentence by sentence, based on cooccurrences. It then selects the chunks with the highest vertex score based on its interconnections with other parts of the text. The top one-third of the scored noun chunks are retained and this forms the core set of the text's "keywords." We add another layer of uniqueness to this set of keywords by removing words that occur in more than 50% of the 10-Ks so that we retain only the most unique keywords for each firm's business description. In a similar fashion, we identify the most unique keywords for each job posting's description.

CORE_i, calculated as the cardinality of the intersection of the two sets of keywords (from job and firm descriptions), scaled by the cardinality of the set of firm-description keywords. Formally, given a set of keywords for firm j, denoted $\text{KW}_j^{\text{Firm}}$, and a set of keywords for job i denoted KW_i^{Job}, we define

$$\text{(1)} \qquad \text{CORE}_i = \frac{|\text{KW}_j^{\text{Firm}} \cap \text{KW}_i^{\text{Job}}|}{|\text{KW}_j^{\text{Firm}}|}.$$

Finally, we average this measure across jobs i posted by firm j in week t in ZIP z to obtain

$$\text{(2)} \qquad \text{CORE\_JOBS}_{j,t,z} = \frac{1}{N}\sum_{i \in N} \text{CORE}_i.$$

Job Flexibility. Our second job characterization metric is JOB_FLEXIBILITY, which we construct as the log of the number of occurrences of "flexibility"-related keywords scaled by the total posting length. We then average this quantity across all jobs posted by a firm in a given ZIP and week.

Rather than defining flexibility-related keywords ex ante (e.g., based on the Oxford English Dictionary), we adopt an in situ approach in which we dynamically select these keywords. This is advantageous for two reasons. First, it allows us to capture many more dimensions of flexibility in the context-specific domain of job postings than we would if we restricted ourselves to context-free dictionary synonyms of the word. Second, the nature of flexibility itself varies with time and with the nature of the job and our approach accounts for this as well.

[6]Our approach is analogous to Hoberg and Phillips's (2016) textual-similarity-based industry classification.

https://doi.org/10.1017/S0022109023000522

We obtain the list of flexibility-related keywords by applying a Word2Vec word embedding model on a random sample of job descriptions drawn from 2017 to 2020. Word2Vec is a shallow two-layer neural net that creates vector representations of words in a high-dimensional vector space (see Mikolov, Chen, Corrado, and Dean (2013) for the original method and Li, Mai, Shen, and Yan (2021b) for an early application in the finance literature). We use a skip-gram architecture that minimizes the cosine similarity between our base word ("flexibility") and words that appear in the surrounding context to predict the closest related words in the sample domain. We obtain a list of 55 keywords that have a nontrivial overlap with flexibility. The most notable among such keywords are presented in Figure 1. Reassuringly, the word embedding model picks up a number of terms that are intuitively related to job flexibility including "flexible shift," "work from home," "adaptable," "telework," and "telecommute." We also pick up a number of non-obvious keywords such as "pto" (Paid Time Off), "overtime," and "rotating" that capture employer responses to the pandemic in terms of changing their job

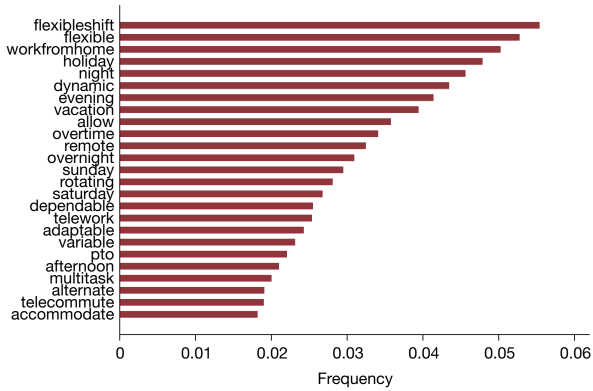
FIGURE 1

"Flexibility" Keywords

Figure 1 presents the list of "flexibility-" related keywords identified by the Word2Vec model. Graph A presents all 55 keywords with the text size of each word proportional to its relative frequency of occurrence in the data. Graph B provides an alternate representation of the 25 most commonly occurring keywords in the form of a histogram.

Graph A. Wordcloud

Graph B. Histogram



https://doi.org/10.1017/S0022109023000522

requirements. Note that our measure captures a range of nonconventional work arrangements, a salient feature of contemporary labor markets.

3. Conditioning Variables

We proxy for several forces that could potentially impact corporate hiring responses to COVID-19 by employing a number of conditioning variables.

Financial Constraints. Our main set of conditioning variables consists of standard measures of firm financial constraints. SMALL_FIRM is an indicator variable that takes the value of 1 for firms that lie below the median of total assets (measured in the last available year), and 0 otherwise. SPECULATIVE_GRADE is an indicator that takes the value of 1 for firms with an S&P issuer rating of less than BBB− (or unrated) as of 2019, and 0 otherwise. NO_CREDIT_LINES is an indicator that takes the value of 1 for firms with no active lines of credit, and 0 for firms with at least one line of credit as of the end of 2019. LOW_CASH_HOLDINGS takes the value of 1 for firms that lie below the median of the corporate cash-to-asset distribution as of Dec. 2019, and 0 otherwise.

To identify likely exogenous changes in firms' financial constraints status, we rely upon two treatment indicators. The first of these indicators, drawn from Almeida et al. (2012), is HIGH_CURRENT_LT_DEBT taking the value of 1 for firms with more than 20% of total long-term debt maturing within 1 year (as of 2019), and 0 for a matched set of firms with less than 20% of total long term debt maturing within 1 year (as of 2019). The latter group of firms is matched using a coarsened exact matching strategy (Iacus, King, and Porro (2012)), based on the firm characteristics described in Section III.C.4. The next indicator, based on Acharya and Steffen (2020), is BBB, which takes the value of 1 for firms with an S&P credit rating of BBB−, BBB, or BBB+ (as of 2019), and 0 for firms with a credit rating of A− or above (as of 2019).

Workplace Characteristics. HIGH_TELEWORKING_DN is an indicator variable that takes the value of 1 for firms that lie above the median of the distribution of the share of total firm job postings in O*NET occupation codes classified as teleworkable by Dingel and Neiman (2020), and 0 otherwise. HIGH_TELEWORKING_PS is an indicator variable that takes the value of 1 for firms in industries that lie below the median of the "work-from-home" difficulty index calculated by Papanikolaou and Schmidt (2022), and 0 otherwise.

COVID Exposure. We also construct a variable that captures the intensity of the coronavirus contagion at a local-area level; that is, in the locality where a firm seeks to hire. HIGH_EXPOSURE is an indicator that takes the value of 1 for each county-week belonging to the highest tercile of the number of confirmed COVID-19 cases per capita in the United States, and 0 for the lowest tercile.[7] We further account for

[7]We map ZIP codes to counties using the HUD-USPS ZIP Code Crosswalk. While we partition areas into terciles, our results are robust to conditioning on alternative cutoffs along the COVID-19 case distribution (see Table IA.4 in the Supplementary Material). We end our sample period for tests that differentiate the effects of the pandemic across different areas of the country by Labor Day, when virtually all U.S. counties had registered a significant high number of COVID-19 cases. Further, several

https://doi.org/10.1017/S0022109023000522

temporal variation in the exposure of local areas to successive COVID-19 waves by introducing a variable that captures the relative ranking of a given county (in terms of COVID-19 cases) across successive points in time. COVID_CASE_RANK is the rank of a county in the weekly distribution of COVID-19 cases across the United States, with the county with highest number of COVID-19 cases for the week being assigned a rank of 1.

4. Control Variables

Google Trends Job Search Interest. We account for measurable "supply intensity" in local labor markets (the intensity with which workers seek jobs) by incorporating data from Google Trends for a comprehensive set of employment-related search terms. We rely on Google's Keyword Planner tool to identify the top 100 (out of 877) most relevant search terms related to "job openings." These include "jobs," "job openings," "job openings near me," "immediate job openings," "vacancies," "careers," "any vacancies near me," "open positions," "work," and "employment opportunities," among others. We first obtain from Google the aggregate weekly time series data at the national level from Jan. 2017 to Jan. 2022. We also obtain the cross-sectional series of normalized search volumes across 50 states for each week over the same period. In order to generate a comparable measure of job search intensity across states and weeks, we denormalize the data using an algorithm adapted from Memon, Razak, and Weber (2020). The algorithm works as follows: For each week t in the time series, denote the normalized state-level search index by G_{it} for each state $i \in S$ and week $t \in \tau$. Let r denote Week 1 in the sample, and G_{Ct} denote the country-level time series. Google Trends normalization works such that $\max_{t \in \tau}(G_{Ct}) = 100$ and $\max_{i \in S}(G_{it}) = 100$ for each $t \in \tau$. Within each week the denormalized state-level trends are set to sum up to the denormalized country-level trends. We impute this denormalized value, $\widehat{G}_{it}$, for each state-week (alternately, month) pair, i, t:

$$\widehat{G}_{it} = G_{it} \times \frac{G_{Ct}}{G_{Cr}} \times \frac{\sum_{i \in S} G_{Sr}}{\sum_{i \in S} G_{St}}. \tag{3}$$

Denote each of the 100 series of denormalized search interest for the keywords as $\widehat{G}_{it}^{k} \forall k \in \{1, \cdots, 100\}$. We define our control variable, JOB_SEARCH_INTEREST$_{i,t}$, as the log of 1 plus the average of $\widehat{G}_{it}^{k}$, capturing real-time job search intensity at the state-week (alternately, month) level.

State and Firm Controls. We account for additional variables that are likely to influence firm hiring. At the state-month level, we control for the log of the total labor force, the unemployment rate, and the average unemployment benefits. At the firm-quarter level, we control for the log of total assets, profitability (net income divided by lagged assets), net financial leverage (total short- and long-term debt minus cash divided by lagged assets), Q (ratio of the market value of equity plus the

countries had begun issuing preliminary approvals for candidate COVID-19 vaccines around this time, marking an end to the initial acute phase of the pandemic in which a potential medical solution was elusive.

https://doi.org/10.1017/S0022109023000522

difference between the book value of assets and the book value of equity plus deferred taxes to the book value of assets), and investment (capital expenditures divided by lagged assets). At the firm-year level, we control for the log of total employees.

IV. Summary Statistics and Empirical Methodology

A. Summary Statistics

Table 1 reports descriptive statistics for the key variables used in our analysis. The raw data are collapsed to the firm-ZIP-week level. While most of the variables yield several million observations, some naturally yield only tens of thousands of observations given how they are computed (see Section III). The average number of new postings by a public firm in a given ZIP per week between Jan. 1, 2017 and Sept. 8, 2020 is 1.34 (or, expressed in log terms, 0.29). This reflects our baseline sampling period, chosen to capture the initial spread and pervasiveness of the COVID-19 pandemic. The average for the pre-COVID period alone was a much higher 1.43 postings (0.36 in log terms). Public firms

TABLE 1

Summary Statistics

Table 1 presents descriptive statistics for the main variables used in our baseline empirical analyses. The unit of observation is a firm-ZIP-week, where ZIP is the 3-digit ZIP-code of a job posting. The variable definitions are provided in Appendix A.3.

Variable	No. of Obs.	Mean	Std. Dev.	Median	IQR
Dependent Variables					
Full sample					
log(NEW_JOB_POSTINGS)	17,203,560	0.29	0.61	0.00	0.00
HIGH_TO_LOW_SKILL_RATIO	241,258	0.26	1.56	0.00	0.00
CORE_JOBS	7,848,065	0.20	0.12	0.22	0.17
log(JOB_FLEXIBILITY)	7,848,065	1.14	0.81	1.20	1.02
Conditioning tests sample					
log(NEW_JOB_POSTINGS)	13,464,649	0.32	0.64	0.00	0.69
HIGH_TO_LOW_SKILL_RATIO	188,824	0.27	1.70	0.00	0.00
CORE_JOBS	6,121,491	0.21	0.14	0.24	0.18
log(JOB_FLEXIBILITY)	6,121,491	1.33	0.92	1.16	1.24
COVID-19 Exposure					
HIGH_EXPOSURE	11,433,098	0.76	0.45	1.00	0.00
Workplace Characteristics					
HIGH_TELEWORKING_DN	13,464,649	0.17	0.37	0.00	0.00
HIGH_TELEWORKING_PS	13,464,649	0.50	0.50	1.00	1.00
Financial Constraints Measures					
SMALL_FIRM	13,464,649	0.17	0.13	0.00	0.00
SPECULATIVE_GRADE	13,464,649	0.52	0.50	1.00	0.00
NO_CREDIT_LINES	13,464,649	0.19	0.29	0.00	0.00
LOW_CASH_HOLDINGS	13,464,649	0.76	0.35	1.00	0.00
State Controls					
log(JOB_SEARCH_INTEREST)	17,203,560	4.14	0.15	4.15	0.18
UNEMPLOYMENT_RATE (%)	17,203,560	4.48	2.45	4.00	1.10
log(LABOR_FORCE)	17,203,560	15.26	0.92	15.26	1.36
Firm Controls					
log(SIZE)	17,203,560	9.13	1.69	9.10	3.21
NET_LEVERAGE	17,203,560	0.33	1.29	0.27	0.31
PROFITABILITY	17,203,560	0.02	0.04	0.01	0.02
Q	17,203,560	1.98	1.66	1.76	1.07
INVESTMENT	17,203,560	0.03	0.04	0.02	0.03
log(EMPLOYMENT)	17,203,560	8.19	3.76	9.18	4.12

https://doi.org/10.1017/S0022109023000522

in our sample post 2.6 high-skill jobs for every 10 low-skill jobs, reflected in the average HIGH_TO_LOW_SKILL_RATIO of 0.26. The statistics underlying our derived variable HIGH_TELEWORKING are consistent with those reported in Papanikolaou and Schmidt (2022). Finally, summary statistics for firm-level control variables suggest that the public firms in our sample are representative of the Compustat universe along dimensions such as size, leverage, profitability, *Q*, and investment (see Albuquerque, Koskinen, Yang, and Zhang (2020) and Fahlenbrach et al. (2021) for studies on COVID-19 using Compustat data).

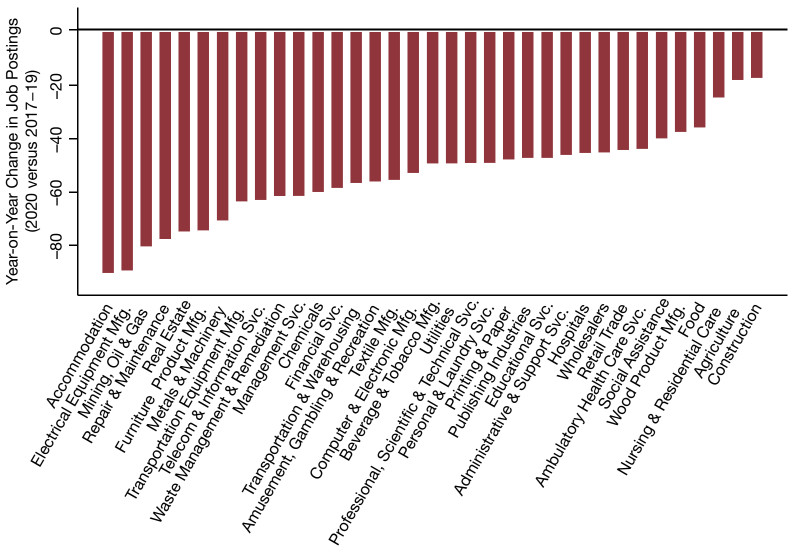
B. Industry Heterogeneity

We showcase the sectoral heterogeneity in the way COVID-19 affects the economy in Figure 2. Firms in the accommodation and electrical equipment manufacturing industries posted the greatest decline in hiring activity, nearly 90%. This is almost five times as large as that of the least affected industries.[8] Industries in the latter category include construction, agriculture, and nursing and residential care facilities, whose services and goods were deemed essential, and consequently were in high demand since the onset of the pandemic.

FIGURE 2

Industry Distribution of Post-COVID Declines in Job Postings

Figure 2 plots the industry distribution of the cumulative percentage change in the number of active job postings for the post-COVID period between Mar. 2020 and Sept. 2020 (relative to the average number of active job postings in the same period of 2017–2019). The cumulative percentage change is calculated as follows: For each day between Mar. 2020 and Sept. 2020 we calculate the percentage change in active job postings relative to the average of the same day across years 2017–2019 for all firms in a given industry. These daily percentage changes are compounded over the entire period between Mar. 2020 and Sept. 2020 to give the overall percentage decline in active job postings in each industry over the post-COVID period of Mar. 2020 to Sept. 2020 (shown on the *y*-axis).



[8]The reported changes are compounded from daily declines in active job postings over Mar. 2020 to Sept. 2020 period (relative to the same period in 2017–2019).

https://doi.org/10.1017/S0022109023000522

Figure 3 illustrates cross-industry heterogeneity in the extent of up/down-skilling following the pandemic. Firms in 21 out of 35 industries posted greater declines in high-skill job postings relative to low-skill postings (Graph A). Graph B plots the change in job posting activity for industries at either end of the down-skilling–upskilling spectrum. Expectedly, industries experiencing the most down-skilling were those in which social distancing protocols necessitated the creation of new low-skill jobs. Examples include temperature checkers in amusement parks and elevator attendants in professional services offices.[9] Those same industries experienced substantially larger reductions in high-skill postings, presumably due to the suspension of expansion activities (e.g., lack of construction of new amusement parks implies lower demand for architects, engineers, and project managers). At the other end of the spectrum, a few industries, primarily in the business-to-business sector, experienced relatively smaller declines in high-skill postings (e.g., repair and maintenance, which includes IT support services). These findings point to a substantial reallocation of hiring both *across* industries (akin to the capital reallocation discussed by Duchin and Harford (2021)) and *within* industries, across job skill levels.

The variation in both the extent of overall job posting declines and down-skilling suggests the inclusion of industry-by-time-fixed effects in our analysis as a way to alleviate concerns that our results may be driven by industry dynamics. These data patterns further illustrate the unique insights obtained from tracking new hiring, as opposed to total employment levels. Notably, hiring activity serves as a forward-looking metric of how the COVID-19 pandemic has reshaped the skill composition of firms' workforce.

C. Empirical Specifications

The timeline of our base empirical tests begins Jan. 1, 2017 and runs through Sept. 8, 2020. As a baseline, we estimate a model that relates firms' job postings with a time indicator variable that captures the onset of the COVID-19 pandemic and its spread over the Mar. 2020–Sept. 2020 period (see Figure 4). This sampling covers the period from the initial outbreak of COVID-19 through when it had spread almost uniformly across the United States. The end of this period also marks the first successful results from clinical trials (and preliminary regulatory approvals) of various vaccines and therapeutics (e.g., monoclonal antibodies), signifying a turning point in the public health response to the pandemic.

Further supporting our choice of baseline sample period, in a meta-analysis of corporate responses to the pandemic, Pagano and Zechner (2022) identify this as the period in which public firms in the United States (and Europe) experienced the most severe economic impact. In subsequent specifications, we interact that time indicator with several conditioning variables, while controlling for other drivers of firms' postings. In order to assess the persistence of the effects we observe during our main sample period (corresponding to the peak of the pandemic), we extend the sample until Jan. 2022 in additional tests. Our baseline specification takes the following form:

[9] *Bloomberg*, May 19th, 2020. "Reopening U.S. Economy Will Mean Creating All Kinds of New Jobs."

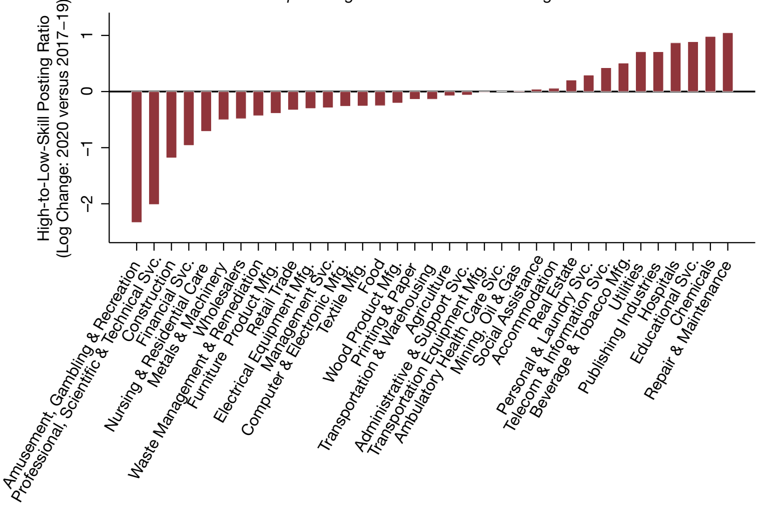
https://doi.org/10.1017/S0022109023000522

FIGURE 3

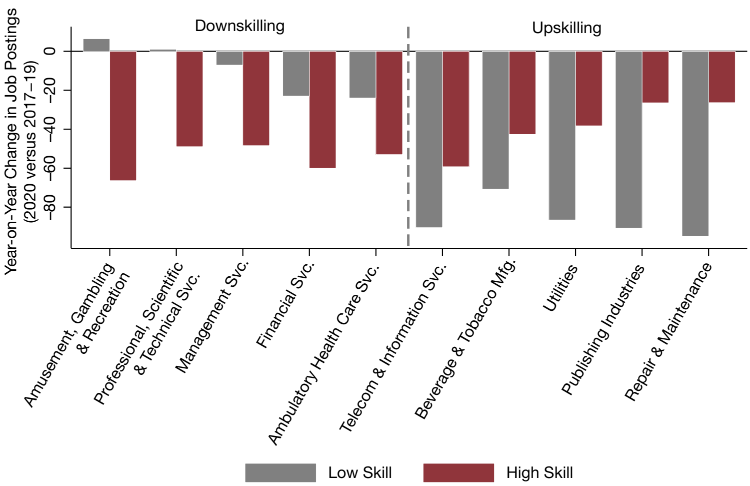
Industry Distribution of Post-COVID Declines in Job Postings by Skill Levels

Graph A of Figure 3 plots the industry distribution of the ratio of log change in number of new-high skill postings (Job Zone 5) to low-skill postings (Job Zone 1) for the post-COVID period of Mar. 2020 to Sept. 2020 (relative to the average number of active job postings in the same period of 2017–2019). We construct this metric by subtracting from i) the logarithm of the high-to-low skills new job posting ratio for each day between Mar. 2020 and Sept. 2020; ii) the average (between 2017 and 2019) of the logarithm of the same ratio for each corresponding day. We then average this difference across all days in the March–September period. The interpretation is as follows: For example, in the "Construction" industry, a logarithmic change of -1.2 implies that the high-to-low skills new job postings ratio declines to 30% ($=e^{-1.2}$) of the pre-pandemic baseline, meaning seven fewer new high-skill job postings for every 10 new low-skill job postings. Graph B plots the cumulative percentage change in the number of active job postings for the post-COVID period of Mar. 2020 to Sept. 2020 (relative to the average number of active job postings in the same period of 2017–2019) for the top 5 downskilling and top 5 upskilling industries in Graph A. The cumulative percentage change is calculated as in Figure 2.

Graph A. High-to-Low-Skills Job Postings Ratio



Graph B. Downskilling and Upskilling

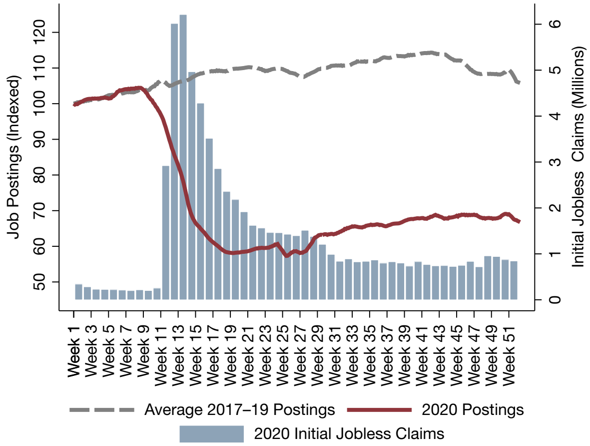


https://doi.org/10.1017/S0022109023000522

FIGURE 4

Aggregate Job Posting Dynamics

Figure 4 plots the daily 7-day rolling average of total active job postings for each day of 2017–2019 (starting from Day 7, reflecting a 6-day burn-in period used to calculate the rolling average), contrasting it to the level of total active job postings over the same period of 2020. The figure depicts the aggregate dynamics of all U.S. job postings on the left *y*-axis, along with the weekly level of initial jobless claims (in millions) on the right *y*-axis. While the job postings series are plotted at a daily level, only weeks are marked on the *x*-axis for readability and to match the data frequency of the initial jobless claims. The job postings series are indexed to 100 as of Day 7 (end of Week 1).



$$Y_{i,z,w} = \beta \text{COVID}_w + \gamma' \Gamma_{s,m-1} + \theta' \Theta_{i,q-1} + \phi_i + \zeta_z + \varepsilon_{i,z,w}, \tag{4}$$

where $Y_{i,z,w} \in$ {NEW_JOB_POSTINGS, HIGH_TO_LOW_SKILL_RATIO, CORE_JOBS, JOB_FLEXIBILITY} for firm i in ZIP code z in week w. COVID is a dichotomous variable that takes the value of 1 for each week after Feb. 29, 2020. Γ is a vector of state-month control variables described in Section III.C.4 for state s containing ZIP z and lagged month $m-1$. Θ contains the firm-quarter control variables described in Section III.C.4 for firm i in lagged quarter $q-1$. ϕ and ζ denote firm- and ZIP-fixed effects, respectively. Standard errors are dual-clustered by firm and week in all of our estimations.[10] Depending on the outcome variable, the estimate of our coefficient of interest, β, corresponds to either an extensive margin or intensive margin effect of the pandemic on firms' hiring decisions. For instance, on the extensive margin, we estimate equation (4) with equations as the dependent variable. In this case, β represents the marginal change in the level of new postings made by firms during the pandemic relative to the 2017–2019 (pre-pandemic) period. On the intensive margin, we estimate the same specification with CORE_ JOBS and JOB_FLEXIBILITY as dependent variables. Here, β represents the marginal change in these textual attributes of the jobs that firms post ads for under the pandemic, compared to those in the pre-pandemic period (for a given O*NET, ZIP, and week of year).[11]

[10]Note that our clustering scheme accounts for the fact that unadjusted standard errors could be biased downward due to our relatively large sample size (see Abadie, Athey, Imbens, and Wooldridge (2017)).

[11]Naturally, we are unable to include time-fixed effects in this specification as our coefficient of interest, β, is identified purely on the basis of time variation.

https://doi.org/10.1017/S0022109023000522

Our next specification dynamically captures the *relative* impact of the pandemic spread. To that end, we interact the COVID time indicator with a conditioning variable, HIGH_EXPOSURE, which takes the value of 1 for a given ZIP code z if it is in the highest tercile of the number of confirmed per-capita COVID-19 cases in week w. The specification is given by

$$Y_{i,j,z,w} = \beta[\text{COVID}_w \times \text{HIGH\_EXPOSURE}_{z,w}] + \gamma'\Gamma_{s,m-1} + \theta'\Theta_{i,q-1} + \phi_i + \zeta_z + \iota_j \times \kappa_m + \omega_w + \varepsilon_{i,j,z,w}, \tag{5}$$

where $Y_{i,j,z,w} \in$ {NEW_JOB_POSTINGS, HIGH_TO_LOW_SKILL_RATIO, CORE_JOBS, JOB_FLEXIBILITY} for firm i (belonging to industry j) in ZIP code z in week w. The terms Γ, Θ, ϕ, and ζ are structured as in equation (4). ι denotes industry-fixed effects, ω denotes week-fixed effects, while κ denotes month-fixed effects for month m containing week w. Equation (5) accounts for dynamic industry $\times$ month-fixed effects via the interactive term $\iota_j \times \kappa_m$.

In our final specification, we include a broader set of conditioning variables aimed at capturing additional fundamental differences across firms (all measured prior to the onset of the pandemic). The specification can be written as follows:

$$Y_{i,j,z,w} = \beta[\text{COVID}_w \times X_{i,z,2019}] + \gamma'\Gamma_{s,m-1} + \theta'\Theta_{i,q-1} + \phi_i + \iota_j \times \kappa_m + \zeta_z + \omega_w + \varepsilon_{i,j,z,w}, \tag{6}$$

where $Y_{i,z,w}$, Γ, ϕ, κ, ζ, and ω are as before. The set $X_{i,z,2019} \in$ {SMALL_FIRM, SPECULATIVE_GRADE, NO_CREDIT_LINES, LOW_CASH_HOLDINGS, HIGH_TELEWORKING_DN, HIGH_TELEWORKING_PS} contains conditioning variables, classified as of 2019 (pre-pandemic). In this specification, the coefficient of interest is β, which represents a "difference-in-differences." It captures the change in the dependent variable between, for example, small versus large firms (first difference), under the pandemic versus before the pandemic (second difference).

V. Base Results

A. Job Posting Activity

We estimate equation (4) to gauge the base impact of COVID-19 on firms' job posting activity. We also assess whether corporate hiring responses are heightened in areas with more severe exposure to COVID-19 as the pandemic spreads by estimating equation (5). The results are reported in Table 2.

Columns 1 and 2 in Panel A of Table 2 present the estimates obtained from the weekly-level regression specification in equations (4) and (5). The estimate in column 1 summarizes the negative and highly significant impact of COVID-19 on firm hiring. The economic magnitude is quite large. The coefficient of −0.011 implies that firms cut their average weekly postings in a ZIP code area by 3.1% (= 0.011/0.36) of the 2017–2019 average starting in Mar. 2020. The estimated weekly cut of 3.1% corresponds to a cumulative 58% decline in new postings over the 28 weeks following the onset of the pandemic. The magnitude of this effect

TABLE 2
The Impact of COVID-19 on Job Postings

Table 2 reports the output from equations (4) and (5). The dependent variables are NEW_JOB_POSTINGS and HIGH_TO_LOW_SKILL_RATIO. In all panels, the unit of observation is a firm-ZIP-week, where ZIP is the 3-digit ZIP-code of a job posting. Variable definitions are as provided in Appendix A.3. In each specification, state and firm controls are included as indicated. Firm-, ZIP-, industry × month-, and week-fixed effects are included as indicated. All regressions are estimated over a sample of public firms over Jan. 2017 to Sept. 2020 period, except for columns 6 and 8, which are estimated over a sample of public firms over the Jan. 2020 to Sept. 2020 period. Robust standard errors, reported in parentheses, are dual-clustered by firm and week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	NEW_JOB_POSTINGS		HIGH_TO_LOW_SKILL_RATIO	
Panel A. Baseline				
	1	2	3	4
COVID	−0.011***		−0.072***	
	(0.002)		(0.012)	
COVID × HIGH_EXPOSURE		−0.018***		−0.074***
		(0.003)		(0.005)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	No	Yes	No	Yes
Week	No	Yes	No	Yes
No. of obs.	17,203,560	11,433,098	241,258	160,609
R^2	0.459	0.477	0.257	0.255
Panel B. Supply-Side Controls				
	5	6	7	8
COVID	−0.047***		−0.050***	
	(0.011)		(0.006)	
JOB_SEARCH_INTEREST	0.091***	0.088***	0.027	0.024
	(0.021)	(0.018)	(0.017)	(0.020)
UNEMPLOYMENT_RATE	−0.006	−0.001	−0.000	−0.003
	(0.005)	(0.001)	(0.001)	(0.002)
LABOR_FORCE	−0.032	−0.013	0.001	−0.002
	(0.093)	(0.020)	(0.003)	(0.003)
UNEMPLOYMENT_BENEFITS	−0.018***	−0.036***	0.036	0.019
	(0.003)	(0.014)	(0.029)	(0.022)
COVID_CASE_RANK		−0.169***		−0.043**
		(0.011)		(0.018)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
No of obs.	17,203,560	1,829,382	241,258	32,879
R^2	0.496	0.492	0.253	0.218
Panel C. State Lockdown and Reopening Dates				
	9	10	11	12
COVID × EARLY_REOPENING	−0.012***	−0.010	−0.005**	−0.007
	(0.004)	(0.017)	(0.002)	(0.005)
COVID × HIGH_EXPOSURE		−0.024***		−0.046***
		(0.005)		(0.009)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes
No of obs.	10,393,952	1,912,544	128,365	22,426
R^2	0.188	0.214	0.588	0.680

(continued on next page)

https://doi.org/10.1017/S0022109023000522

https://doi.org/10.1017/S0022109023000522

TABLE 2 (continued)

The Impact of COVID-19 on Job Postings

Panel D. Workplace Characteristics

	13	14	15	16
COVID × HIGH_TELEWORKING_DN	−0.012***		−0.025***	
	(0.003)		(0.008)	
COVID × HIGH_TELEWORKING_PS		−0.017***		−0.033***
		(0.004)		(0.006)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes
No of obs.	13,464,649	13,464,649	188,824	188,824
R^2	0.338	0.252	0.542	0.399

increases as we condition on the local level of coronavirus contagion.[12] In particular, the estimate in column 2 implies that firms scaled back their weekly job posting activity within areas with highest levels of confirmed COVID-19 cases by 1.64 $(= -0.018/-0.011)$ times the unconditional effect, relative to those areas with fewer cases.[13] These results are consistent with the dual impact of COVID-19 as a negative first-moment and positive second-moment shock on firms' hiring decisions, as we discuss in Prediction 1 (see Section II).

In additional analyses presented in Table IA.3 in the Supplementary Material, we further address the possibility that weekly job postings measures could be noisy and might not capture the realistic time frame within which firms adjust their hiring policies. We do so by reestimating our baseline results with the dependent variables aggregated at a monthly frequency. The corresponding results are consistent with those presented in Table 2. This consistency is reassuring, and it suggests that our weekly specification accurately reflects the time horizon over which firms substantively modified their hiring in response to the onset of the pandemic. In light of the confirmatory evidence from the monthly specification, we opt to retain the weekly specification as the empirical workhorse model for the remainder of our analysis. We do so in order to fully retain the granularity and richness of the variation in our "big data" sample, which we would partly lose by collapsing the data to a lower time frequency.

Motivated by recent work on the consistency of panel regression estimates when the dependent variable is a count variable, we reestimate our baseline specifications in equations (4) and (5) using a Poisson fixed effects model (see Cohn, Liu, and Wardlaw (2022)). The results, reported in Table IA.5 in the Supplementary

[12]The uninteracted COVID and HIGH_EXPOSURE terms are subsumed by time-interacted fixed effects.

[13]Table IA.4 in the Supplementary Material shows that our results are robust to alternative cutoffs of the COVID-19 case distribution, as well as using the number of weekly cases per capita as a continuous treatment variable. We further explore alternative classification schemes of COVID-19 exposure in Section V.C.

https://doi.org/10.1017/S0022109023000522

Material, show that our inferences remain unchanged under this alternate estimator. Additionally, to verify that our results are not simply capturing inherent differences in firm-level hiring practices, we reestimate the specifications in Panel A of Table 2 under firm × ZIP interactive fixed effects. Table IA.6 in the Supplementary Material shows that our results continue to obtain under this fully saturated specification.[14]

B. Worker Skill Level

Next, we assess whether and how the *skill level* of workers that firms seek to hire has changed with the pandemic. We do so by comparing changes in new postings for high-skill positions relative to low-skill positions by the same firm in the same locality over time. In particular, we measure the relative declines in high-skill postings vis-à-vis low-skill postings by way of the variable HIGH_TO_LOW_SKILL_RATIO. The results obtained from estimating equations (4) and (5) are reported in columns 3 and 4 of Table 2.

Our reported estimates reveal that the HIGH_TO_LOW_SKILL_RATIO drops significantly as the pandemic spreads. The drop of -0.072 (column 3) in this ratio represents 24% ($=0.072/0.30$) of the 2017–2019 average HIGH_TO_LOW_SKILL_RATIO. The corresponding drop in this ratio is significantly higher in the areas most exposed to COVID-19 (column 4).[15] Once again, we find that the results continue to obtain at a monthly frequency (columns 3 and 4 of Table IA.3 in the Supplementary Material). This is particularly important to verify for tests in which HIGH_TO_LOW_SKILL_RATIO is the dependent variable, as firms may have differential hiring schedules for high and low skill positions. The consistency of our results at various aggregation levels implies that mechanical differences in hiring schedules are not responsible for the pandemic-led changes in skill-based hiring that we document.

As discussed in Prediction 2 of our conceptual framework, when firms are faced with the COVID-induced uncertainty shock, they have differential responses in terms of their high- and low-skill hiring. That firms markedly cut back more on hiring high-skill workers is consistent with these workers having costlier, less flexible employment contracts (including training and noncompete clauses). Low-skill workers, in contrast, often work under more flexible arrangements and with

[14]We omit these fixed effects so that our estimates are not directly ascribable to variation in job postings among the largest firms that hire across multiple ZIP codes.

[15]We show in Table IA.7 in the Supplementary Material that our inferences are robust to alternative definitions of high- and low-skill jobs. In Table IA.8 in the Supplementary Material, we show the robustness of our results to aggregation at the commuting zone level (as opposed to ZIP code level). Additionally, we reestimate the baseline results using analogous dependent variables constructed at the firm-week level (as opposed to firm-ZIP-week level). Table IA.9 in the Supplementary Material shows that our results continue to obtain at this alternative level of aggregation. It is, therefore, unlikely that our results are driven by firms reallocating postings across regions of the country. We note that the low observation count in this table is due to the fact that the HIGH_TO_LOW_SKILL_RATIO variable is defined only for ZIP codes where firms post job ads corresponding to both Job Zones 1 and 5. Columns 3 and 4 in Table IA.9 in the Supplementary Material verify the representativeness of the results in columns 3 and 4 of Table 2 by showing that they are not driven by specific ZIP code areas where firms hire both high- and low-skill workers.

lower rehiring costs. Our evidence on the pandemic-led firm downskilling is new and points to a reversal of the upskilling trend observed since the Financial Crisis (see Hershbein and Kahn (2018)). While the recent pandemic and the earlier crisis share a commonality, that is, the tightening of firms' financial constraints, the differential skill-based hiring responses suggest an important economic distinction between the two episodes. Notably, many firms were able to respond to the health crisis by adopting flexible work practices (e.g., teleworking), plausibly dampening demand for new (inflexible) high-skill hiring relative to the previous episode.[16] Our job ad data are informative as they enable us to highlight the downward shift in firms' skill-based hiring demand, which is distinct from changes in the skill distribution of their workforce.

C. Changes in Labor Supply and Policy Responses

The results that we discuss in the previous sections point to substantial changes to corporate hiring over the course of the pandemic. While we attribute these effects to firms' labor demand-led decisions, our estimates could be partly capturing firms' reactions to the labor supply-side responses of workers driven by the coronavirus contagion, as discussed in Prediction 5 of our conceptual framework. Notably, several studies have documented measurable tightening of labor supply during the pandemic (see, e.g., Domash and Summers (2022)). In our next set of tests, we examine whether concurrent changes in labor supply can fully account for the variation in corporate job postings that we find in Panel A of Table 2. We do so by explicitly controlling for time-varying measures of labor supply "tightness" during the pandemic. We augment our base set of state-level control variables, to include UNEMPLOYMENT_BENEFITS and COVID_CASE_RANK. Informed by contemporaneous work on the effects of unemployment benefits on workers' labor supply decisions (Holzer, Hubbard, and Strain (2021)), we include the variable UNEMPLOYMENT_BENEFITS, which is the average level of unemployment insurance claims awarded by a state in a given month. We note that other studies suggest that workers cut back on their hours worked due to concerns directly arising from COVID-19 exposure (e.g., Faberman, Mueller, and Şahin (2022)). To account for this driver of changing labor supply, we include the temporal exposure of local areas to COVID-19 contagion, as reflected in their relative ranking over time in the cross-sectional distribution of confirmed COVID-19 cases (COVID_CASE_RANK). The results are reported in Panel B of Table 2.

The estimates in columns 5 and 7 show that our baseline COVID term continues to be negative and highly significant even in the presence of the various controls for concurrent changes in labor supply. Interestingly, heightened unemployment benefits offered during the pandemic seem to negatively impact job postings levels but do not display any significant relationship with the compositional changes (along the lines of worker skill levels) in firm hiring. Columns 6 and 8 highlight the role played by COVID-19 exposure in shaping corporate hiring

[16]We discuss this channel in Prediction 3 and provide corresponding empirical evidence in Sections V.D and VI.

https://doi.org/10.1017/S0022109023000522

https://doi.org/10.1017/S0022109023000522

responses, despite controlling for the various labor supply proxies.[17] In all, we find sufficient justification for our interpretation that hiring was substantially affected by firm demand-driven factors.

The results thus far also point to the fact that the level of COVID-19 exposure of a firm's geographical area amplifies its hiring responses. We perform a series of tests to support the notion that it is the virus exposure itself, and not policy measures such as lockdowns and reopenings, that drive this amplification effect. In these tests, we consider an alternative scheme for classifying regions as being more or less exposed to the COVID-19 pandemic, exploiting the heterogeneous timing of state lockdown and reopening policies. Specifically, we construct a variable, EARLY_REOPENING, which takes the value of 1 for all ZIP code regions located in states whose initial reopening dates were in the top tercile of the national distribution. These states either never implemented a state-wide lockdown or were the earliest to reopen (had the briefest lockdowns). The same variable takes the value of 0 for states that were the latest to reopen, belonging to the bottom tercile of reopening dates. We obtain the dates of state lockdowns and reopenings from Nguyen, Gupta, Andersen, Bento, Simon, and Wing (2021). The results are reported in Panel C of Table 2.

The results show that while firms cut on postings relatively more in states that eased restrictions earlier (column 9), this effect is driven by the subset of early reopening states that were also highly exposed to the COVID-19 contagion itself (column 10); examples include Arizona, Louisiana, and Florida. Columns 11 and 12 report similar results for HIGH_TO_LOW_SKILL_RATIO, suggesting that the virus contagion also drives the observed downskilling phenomenon. Altogether, we find evidence that the observed phenomena are unlikely to be driven by states' lockdown policies.

D. Workplace Flexibility

COVID-19 has called for a number of new workplace protocols that may affect firms' hiring. Notably, firms with the ability to migrate to different working arrangements (e.g., teleworking) may respond differently to the health crisis. Accordingly, we examine the role played by this important workplace characteristic in modulating hiring responses to the 2020 pandemic following our discussion in Prediction 3 of Section II. Specifically, we consider the effect of the ability of workers to perform their jobs remotely. Panel D of Table 2 reports the results.

In columns 13 and 15, we classify firms into high- and low-teleworking categories based on the share of jobs in the pre-pandemic period that can be performed remotely (based on Dingel and Neiman (2020)). In columns 14 and 16, we use an alternate classification scheme which ranks industries based on the difficulty of "working-from-home" (Papanikolaou and Schmidt (2022)). The results indicate that high-teleworking firms (e.g., firms in the technology and professional services industries) cut on new job postings (particularly high-skill ones) by more than low-teleworking firms. These results are new with respect to

[17]Naturally, the COVID_CASE_RANK variable is not available in the pre-COVID period, resulting in a reduced observation count for columns 6 and 8 of Table 2.

https://doi.org/10.1017/S0022109023000522

studies on teleworking in the pandemic (e.g., Brynjolfsson, Horton, Ozimek, Rock, Sharma, and TuYe (2020), Bai et al. (2021)). They show that firms' ability to adapt to remote working arrangements reduces demand for new hiring in the pandemic. Our results up to this point suggest the role of at least two concurrent channels that drive firms' pandemic new hiring responses. The reduction in *new job postings*, particularly for high-skill roles, by firms with greater teleworking adaptability likely reflects their ability to retain a subset of *existing workers* under flexible work arrangements. They do so while minimizing the sunk costs of their new hiring decisions (costs that are particularly acute for high-skill roles) under heightened uncertainty. As such, the findings that we report are fully consistent with Prediction 3 of our conceptual framework.

VI. Textual Analysis of Job Postings

We now exploit text descriptions of job postings to assess how the pandemic has affected the nature of jobs and employment arrangements around positions that firms look to fill. Utilizing machine learning techniques, we construct two variables meant to capture these characteristics as described in Section III.C.2: CORE_JOBS and JOB_FLEXIBILITY. We use them as dependent variables in equations (4) and (5). The results are reported in Table 3.

Our text-based measures identify various dimensions along which jobs have changed. The first dimension that we consider is how "core" a job is to a firm. The coefficient in column 1 shows that the average CORE_JOBS measure has increased. Since Mar. 2020, jobs that firms advertise for are 2.8% ($=0.005/0.18$) more central to the firm's operations (compared to the pre-COVID baseline). Differently put, firms are disproportionately hiring for positions that involve performing functions that are more critical to their core, mission-oriented operations

TABLE 3

The Impact of COVID-19: New Job and Workplace Characteristics

Table 3 reports the output from equation (4). The dependent variables are CORE_JOBS and JOB_FLEXIBILITY. Variable definitions are as provided in Appendix A.3. All regressions are estimated over a sample of public firms over Jan. 2017 to Sept. 2020 period. In each specification, state and firm controls are included as indicated. Firm- and ZIP-fixed effects are included as indicated. Robust standard errors, reported in parentheses, are dual-clustered by firm and week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	CORE_JOBS		JOB_FLEXIBILITY	
	1	2	3	4
COVID	0.005***		0.003***	
	(0.001)		(0.001)	
COVID × HIGH_EXPOSURE		0.006***		0.005***
		(0.002)		(0.001)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	No	Yes	No	Yes
Week	No	Yes	No	Yes
No of obs.	7,848,065	5,179,722	7,848,065	5,179,722
R^2	0.212	0.334	0.219	0.440

https://doi.org/10.1017/S0022109023000522

since the pandemic and in regions most impacted by the virus contagion (see column 2). On the flip side, this finding implies that pandemic-induced hiring cuts have been particularly acute for nonessential roles at firms.

COVID-19 has forcibly led to the adoption of new and unprecedented measures regarding the work environment. We investigate whether these measures have prompted firms to revise their expectations and requirements when hiring new workers, particularly in terms of employment arrangements. Specifically, we consider the JOB_FLEXIBILITY index that is derived from job ad text descriptions. Using this measure, we find that firms have geared their job openings toward more flexible arrangements in their hiring following the pandemic. In particular, note that the average job posting text contained around 3 flexibility-related words per 100 words in the pre-COVID-19 period. The estimate in column 3 shows that following the onset of the pandemic, the average number of words related to flexibility increased by 1 ($= e^{0.003}$), to around 4 per 100 words. This points to a notable increase in job flexibility (along dimensions such as overtime, adaptability, and work performed over nights and holidays) reflecting additional expectations and needs of firms when hiring under COVID-19. The result in column 4 further suggests that firms have altered the flexibility in job postings the most for positions located in the areas that became more highly exposed to COVID-19, underscoring the fact that the pandemic has played an important role in reshaping the workplace. These results are consistent with a real options perspective wherein firms faced with higher uncertainty offset irreversible costs associated with new hiring by increasing the flexibility of employment contracts as well as job roles and requirements.

The results in Table 3 are new to the literature and make it clear that the pandemic has had a multi-faceted impact on the nature of positions firms are looking to fill, as evidenced by changes in their job posting descriptions. These changes, combined with short-run inelasticity in workers' skill sets, offer an explanation for the difficulty faced by firms in filling vacancies since the start of the pandemic, pointing to a potentially problematic labor market recovery.

VII. Persistence of Changes in Corporate Hiring

Our base results reveal a number of changes in corporate hiring demand and the nature of work that firms expect their new employees to perform. These results are obtained over a sample period spanning the height of the pandemic, from Mar. 2020 to Sept. 2020. Notably, this was prior to the availability of effective vaccines and therapeutics, and thus considerable uncertainty remained about the duration of the health crisis. In this section, we turn to examine the persistence of these changes beyond the acute phase that forms our baseline sample period. We do so in order to gauge the long-run effects on corporate hiring and the nature of new jobs, if any, that the COVID-19 pandemic has induced. Specifically, we focus on the four main variables that capture the level of new job postings (NEW_JOB_POSTINGS), the skewness in the skill levels demanded (HIGH_TO_LOW_SKILL_RATIO), and the nature of work performed (CORE_JOBS and JOB_FLEXIBILITY).

Our analysis in this section differs from the previous setup along two critical dimensions. First, we extend the sample, which previously ends in Sept. 2020, to run through Jan. 2022. This extended sample comprises several distinct stages of

https://doi.org/10.1017/S0022109023000522

the pandemic. In this analysis, we define three time-indicator variables that identify these stages. The first of these indicators, COVID_MAR_2020 takes the value of 1 for all months after Mar. 2020; and 0 for all months prior. COVID_SEP_2020 takes the value of 1 for all months after Sept. 2020; 0 for all months prior. This indicator identifies the period after the first major wave of COVID-19 cases subsided (as did drastic policy responses, such as lockdowns). The last indicator, COVID_APR_2021, takes the value of 1 for all months after Apr. 2021; 0 for all months prior. This marks the period where large-scale vaccination programs against COVID-19 were launched across the country, and concurrently, a reversal of COVID-19 protocols such as social distancing, indoor gathering, and mask mandates for fully vaccinated individuals was observed. The results of these analyses are reported in Table 4.

The results reveal contrasting patterns across the outcome variables considered. Column 1 implies that the drop in new job postings was confined to the initial crisis period (between Mar. 2020 and Sept. 2020). New job posting levels display a salient "drop-and-rebound" pattern, with the period after Sept. 2020 registering positive and significant *growth* in corporate hiring (relative to the initial period of decline). Our finding is consistent with (and provides micro-evidence for) widely-reported anecdotes and aggregate data showing a strong rebound in hiring starting in late 2020. Turning to the remaining outcomes, our results in columns 2–4 show that the skill profile of hiring as well as the qualitative aspects of new jobs under the pandemic remained altered (for more than 1 year) relative to the pre-pandemic baseline. That is, while firms restored (or exceeded) their hiring *levels*

TABLE 4

Persistence of Changes in Job Postings and Nature of Work

Table 4 reports the output from equation (4). The dependent variables are NEW_JOB_POSTINGS, HIGH_TO_LOW_SKILL_RATIO, CORE_JOBS, and JOB_FLEXIBILITY. Variable definitions are provided in Appendix A.3. In each specification, state and firm controls are included as indicated. Firm-, industry × month-, ZIP-, and week-fixed effects are included as indicated. All regressions are estimated over a sample of public firms over Jan. 2017 to Jan. 2022 period. Robust standard errors, reported in parentheses, are dual-clustered by firm and week. The dependent variables in the first two columns are based on counts of job posting at the firm-ZIP-week level, while those in the next two columns are based on textual analysis of job postings (and hence contain fewer observations, see Table 3). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Job Postings Variables		Textual Variables	
	NEW_JOB_POSTINGS	HIGH_TO_LOW_SKILL_RATIO	CORE_JOBS	JOB_FLEXIBILITY
	1	2	3	4
COVID_MAR_2020	−0.066***	−0.021***	0.009***	0.015***
	(0.009)	(0.006)	(0.003)	(0.002)
COVID_SEP_2020	0.070***	−0.012**	0.002**	0.005**
	(0.011)	(0.004)	(0.001)	(0.002)
COVID_SEP_2021	0.143***	0.010	−0.001	0.001
	(0.014)	(0.007)	(0.001)	(0.001)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
No of obs.	41,260,357	520,396	11,492,848	11,492,848
R^2	0.190	0.127	0.160	0.404

https://doi.org/10.1017/S0022109023000522

after a period of dramatic declines, their altered job requirements and skill expectations from new employees continued to persist for long after. Our results underscore the fact that the COVID-19 pandemic induced persistent changes in the nature of work and positions into which firms seek to hire. Policies targeting the post-pandemic labor market should ideally extend their goals beyond the level of job openings to include targeted support in preparing workers for the longer-term changes in skills and requirements that we uncover.

VIII. Financial Constraints and Corporate Hiring Responses

A. Financial Constraints and Job Posting Activity

Our analysis considers several characteristics that modulate firms' hiring responses. The tests in this section concern firms' access to financing and are motivated by the implications of Prediction 4 of our conceptual framework. For robustness, we consider a number of constraint proxies (defined in Section III.C.3): a firm's size, whether its debt is rated as speculative or investment grade, whether it has an outstanding credit line, and whether it holds a large cash buffer. We condition our baseline specification equation (6) on these proxies, presenting the results in Panel A of Table 5.

Across all measures of financial constraints, we find that constrained firms cut their job postings substantially more than their unconstrained counterparts since the start of the pandemic. The estimate of −0.082 in column 1 implies that over Weeks 9–36 of 2020 small firms reduced their postings by an additional 22.8% of the 2017–2019 average posting rate than large firms. Notably, small firms appear to skew their job postings away from high-skill jobs relative to large firms (see column 5). Through these tests, we provide evidence that the disproportionate impact on high- versus low-skill postings is unlikely to be driven by simple rehiring dynamics, whereby firms laid off (and subsequently rehired) low-skill workers en masse. Constrained firms jointly displayed the greatest reductions in new hiring while simultaneously registering the largest declines in the high-to-low-skill ratio, suggesting a compositional change in new hiring (as opposed to increased rehiring), consistent with the notion of within-firm downskilling. The estimate in column 3 further implies that firms without access to liquidity in the form of credit lines reduced their weekly job postings by an extra 16.4% of the 2017–2019 average as compared to firms with at least one credit line outstanding.[18] The coefficient in column 4 implies that cash savings performed a similar function, with job posting cuts made by cash-constrained firms significantly exceeding those made by cash-rich firms.[19] Unlike the Financial Crisis of 2008–2009, the COVID-19 crisis did not originate in the financial system, yet lack of access to financing has substantially hampered firms' hiring activity.

[18]The role of credit lines as a buffer during the COVID-19 crisis has been studied in a number of contemporaneous papers (e.g., Acharya and Steffen (2020), Greenwald, Krainer, and Paul (2020), Li, Strahan, and Zhang (2020), and Chodorow-Reich, Darmouni, Luck, and Plosser (2022).

[19]Sample cash-constrained (cash-rich) firms had cash reserves averaging to 2% (13%) of assets as of 2019.

https://doi.org/10.1017/S0022109023000522

TABLE 5

Firm Financial Constraints, Job Postings, and Workplace Characteristics Under COVID-19

Table 5 reports the output from equation (6). The dependent variables in Panel A are NEW_JOB_POSTINGS and HIGH_TO_LOW_SKILL_RATIO. The dependent variables in Panel B are CORE_JOBS and JOB_FLEXIBILITY. The unit of observation is a firm-ZIP-week, where ZIP is the 3-digit ZIP-code of a job posting. Variable definitions are provided in Appendix A.3. All regressions are estimated over a sample of public firms over Jan. 2017 to Sept. 2020 period. In each specification, state and firm controls are included as indicated. Firm × month-, industry × month-, firm-, ZIP-, and week-fixed effects are included as indicated. Robust standard errors, reported in parentheses, are dual-clustered by firm and week. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Job Posting Activity

	NEW_JOB_POSTINGS				HIGH_TO_LOW_SKILL_RATIO			
	1	2	3	4	5	6	7	8
COVID × SMALL_FIRM	−0.082***				−0.272***			
	(0.015)				(0.049)			
COVID × SPECULATIVE_GRADE		−0.032***				−0.128***		
		(0.006)				(0.020)		
COVID × NO_CREDIT_LINES			−0.059***				−0.025***	
			(0.014)				(0.005)	
COVID × LOW_CASH_HOLDINGS				−0.028***				−0.037**
				(0.007)				(0.005)
Controls								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects								
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No of obs.	13,464,649	13,464,649	13,464,649	13,464,649	188,824	188,824	188,824	188,824
R^2	0.317	0.244	0.159	0.214	0.111	0.118	0.153	0.223

Panel B. Workplace Characteristics

	CORE_JOBS				JOB_FLEXIBILITY			
	9	10	11	12	13	14	15	16
COVID × SMALL_FIRM	0.003***				0.003***			
	(0.001)				(0.001)			
COVID × SPECULATIVE_GRADE		0.004***				0.004*		
		(0.001)				(0.002)		
COVID × NO_CREDIT_LINES			0.006***				0.012**	
			(0.002)				(0.005)	
COVID × LOW_CASH_HOLDINGS				0.005*				0.010***
				(0.002)				(0.002)
Controls								
State	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects								
Firm	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No of obs.	6,121,491	6,121,491	6,121,491	6,121,491	6,121,491	6,121,491	6,121,491	6,121,491
R^2	0.512	0.283	0.314	0.267	0.477	0.276	0.204	0.117

B. Financial Constraints and Job Description Texts

The preceding set of results shows that access to finance impacts firms' ability to hire into new, high-skill positions. Through the tests performed in this section, we characterize several dimensions along which financial constraints shape the nature of new jobs created since the pandemic. To this end, we estimate equation (6) with the dependent variables set to either of our two job textual

https://doi.org/10.1017/S0022109023000522

measures. The independent variable of interest is the interaction between the COVID indicator with a relevant measure of financial constraints (alternatively, SMALL_FIRM, SPECULATIVE_GRADE, NO_CREDIT_LINES, and LOW_CASH_HOLDINGS). The results are reported in Panel B of Table 5.

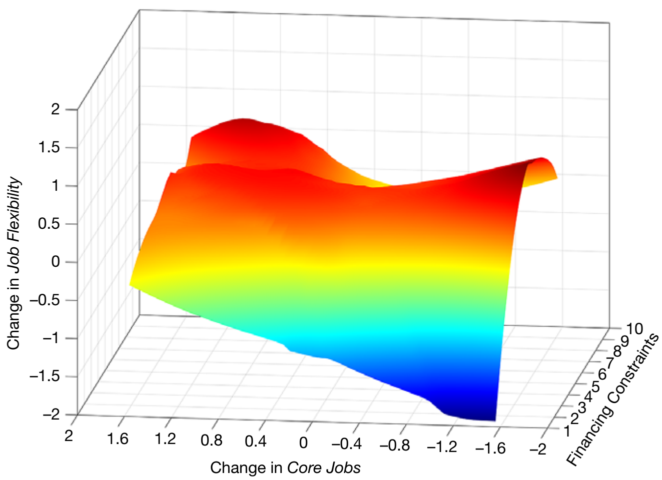
Lack of financing appears to drive firms to significantly alter the nature of positions they seek to fill starting in Mar. 2020. We find that constrained firms increasingly sought to fill in more "core" positions (columns 9–12), suggesting that their pandemic hiring was predominantly driven by their immediate business needs and unwillingness to enter into longer-term, inflexible labor contracts. Supporting this, we find that constrained firms increased the degree of employment flexibility required in the jobs they sought to hire for (columns 13–16). This includes emphasis on dimensions such as variable shifts, multitasking, and adaptability, further capturing the increased utilization of nontraditional work arrangements such as freelance, contract, and gig-based work.

It is important to show the multifaceted nature of relations between employment arrangements and firm financial constraints under COVID-19. Figure 5 plots a three-dimensional surface graph depicting changes in JOB_FLEXIBILITY and CORE_JOBS at various levels of financial constraints. The figure shows that firms facing greater financial constraints have opted for more flexible employment arrangements since the pandemic. Moreover, firms with greater increases in organizationally core hiring under the pandemic also increased the flexibility they expect from their employees. The plot is particularly informative in showing

FIGURE 5

JOB_FLEXIBILITY, CORE_JOBS, and Financial Constraints

Figure 5 depicts the cross-sectional relationship between the changes in JOB_FLEXIBILITY, CORE_JOBS, and financial constraints. Changes in JOB_FLEXIBILITY and CORE_JOBS are measured by the *Z*-scored residuals of changes in the corresponding measures since the onset of COVID-19, accounting for variables at the firm-ZIP-week level (see equation (4)). Financial constraints are measured by firm size. The axis extending to the left represents the change in CORE_JOBS, the axis extending to the right represents deciles of firm financial constraints, and the axis pointing up represents the change in JOB_FLEXIBILITY. The surface depicts the quadratic regression fit of post-pandemic changes in the incidence of flexibility-words, with associated levels of CORE_JOBS and financial constraints, with colors toward the red (blue) end of the spectrum representing increase (decrease) in the number of flexibility-related words (see Section III.C).



https://doi.org/10.1017/S0022109023000522

the interaction between all three variables. It reveals that the most pronounced hiring changes under the pandemic, reflected in increasingly core job positions with greater flexibility requirements, occurred across the most financially constrained firms.

C. Plausibly Exogenous Variation in Financial Constraints

The results across Panels A and B of Table 5 suggest that the availability and access to financing at the onset of the pandemic shaped changes in firms' hiring activities in subsequent months. In this section, we substantiate these associations by exploiting quasi-exogenous variation in firm financial constraints. We do so by drawing on two complementary identification strategies.

In the first experiment, we follow Almeida et al. (2012) and partition firms based on the fraction of their long-term debt coming due in the next 1 year, as of 2019 (pre-2020 pandemic). This identification strategy relies on the prior that firms selected their debt maturity profiles long before the COVID-19 pandemic emerged on managers' planning horizons. As such, firms with an abnormally large share of their long-term debt coming due right in 2020 faced a sudden increase in refinancing risk relative to otherwise similar firms whose debt maturity profiles were longer dated.

The second identification strategy builds on the work of Acharya and Steffen (2020), who show that firms with BBB credit ratings (as of 2019) hoarded cash at the onset of the 2020 pandemic to avoid a ratings downgrade and consequent loss of their investment-grade status. Such firms faced an exogenous shortfall in the availability of funds relative to firms in the ratings category immediately above them, which even if downgraded, would retain their investment-grade status.

1. Identification Under Corporate Debt Refinancing

In the first of our identification strategies, we set our treated group to comprise firms who faced a sudden need to refinance their existing debt at the onset of the pandemic. The corresponding control group is an otherwise comparable (matched) set of firms with only a small fraction of their long-term debt maturing in 2020. We measure the refinancing need through the variable HIGH_CURRENT_LT_DEBT, which is set to 1 for firms with more than 20% of their total debt maturing within 1 year (as of 2019). This variable analogously takes the value of 0 for a set of firms with less than 20% of their total debt maturing within 1 year, matched on multiple firm characteristics (see Section III.C.4) using a coarsened exact matching strategy (Iacus et al. (2012)). We compare the differential impact of COVID-19 on the job posting levels, within-firm downskilling, and changes in job descriptions of the above two categories of firms. The results are reported in Panel A of Table 6.[20]

Our estimates point to a causal effect of firms' financial constraints on their hiring activity. To wit, treated firms faced greater uncertainty as to whether their long-term debt would be refinanced with the onset of COVID-19. Ex ante, these firms would be reluctant to engage in costly and irreversible hiring decisions,

[20]The dependent variables in the first two columns of Table 6 are based on counts of job posting at the firm-ZIP-week level (extensive margin), while those in the next two columns are based on textual analysis of job postings (intensive margin, and hence contain fewer observations, see Table 3).

TABLE 6

Financial Constraints and Hiring: Plausibly Exogenous Variation

Table 6 reports the output from equation (6). The dependent variables are NEW_JOB_POSTINGS, HIGH_TO_LOW_SKILL_RATIO, CORE_JOBS, and JOB_FLEXIBILITY. The unit of observation is a firm-ZIP-week, where ZIP is the 3-digit ZIP-code of a job posting. Variable definitions are as provided in Appendix A.3. All regressions are estimated over a sample of public firms over Jan. 2017 to Sept. 2020 period. In each specification, state and firm controls are included as indicated. Firm-, industry × month-, ZIP-, and week-fixed effects are included as indicated. Robust standard errors, reported in parentheses, are dual-clustered by firm and week. The dependent variables in the first two columns are based on counts of job posting at the firm-ZIP-week level, while those in the next two columns are based on textual analysis of job postings (and hence contain fewer observations, see Table 3). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Job Postings Variables		Textual Variables	
	NEW_JOB_POSTINGS	HIGH_TO_LOW_SKILL_RATIO	CORE_JOBS	JOB_FLEXIBILITY
Panel A. Corporate Debt Maturity				
	1	2	3	4
COVID × HIGH_CURRENT_LT_DEBT	−0.026***	−0.005***	0.002**	0.004**
	(0.004)	(0.001)	(0.001)	(0.001)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes
No of obs.	2,692,740	20,450	147,953	147,953
R^2	0.155	0.655	0.107	0.270
Panel B. Credit Rating Downgrade				
	5	6	7	8
COVID × BBB	−0.082***	−0.096***	0.013***	0.003**
	(0.009)	(0.012)	(0.003)	(0.001)
Controls				
State	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Fixed effects				
Firm	Yes	Yes	Yes	Yes
ZIP	Yes	Yes	Yes	Yes
Industry × Month	Yes	Yes	Yes	Yes
Week	Yes	Yes	Yes	Yes
No of obs.	8,653,373	62,101	1,468,193	1,468,193
R^2	0.202	0.077	0.060	0.218

amplifying the baseline effects of the COVID-19 shock itself. We find results consistent with this prior across all columns in Panel A of Table 6. Column 1 shows that treated firms cut back on new job postings significantly more than their unconstrained counterparts. As in Table 5, the result in column 2 suggests that the treated firms also witnessed more pronounced downskilling. Firms facing a sudden increase in refinancing needs at the onset of the pandemic also disproportionately hired into positions closely aligned with their core business lines (increased CORE_JOBS, column 3). This while increasing the flexibility demanded of their workers, as highlighted by the positive and significant coefficient in column 4.

2. Identification under Impending Credit Rating Downgrades

The second of our identification strategies compares firms facing a heightened risk of being downgraded to junk status at the onset of the pandemic to firms just above them on the ratings scale. We define the treated group in this setup as firms

https://doi.org/10.1017/S0022109023000522

https://doi.org/10.1017/S0022109023000522

with an S&P credit rating of BBB ± as of 2019. The control group is the set of public firms in our sample that have an S&P credit rating of A (or higher). As before, we compare the differential hiring responses of these two categories of firms. We report the corresponding results in Panel B of Table 6.

Columns 5 and 6 show that firms facing a risk of being downgraded to junk status curtailed their job postings (particularly for high-skill positions) more severely than firms for which a downgrade would not entail the loss of an investment-grade rating. This disproportionate decline in human capital investment is consistent with such firms hoarding more cash on their balance sheets by drawing down on credit lines in early 2020, as reported by Acharya and Steffen (2020). As seen in columns 7 and 8, these firms exhibit a higher tendency to adapt their job description texts following the pandemic, making new positions increasingly core and more flexible.

The analyses in Table 6 reinforce the argument that financial constraints play a key role in shaping firms' hiring responses to the COVID-19 shock. Our results imply that new hires in constrained firms are expected to accommodate their employers' changing needs to a greater extent than their counterparts in unconstrained firms. In this sense, firms with limited financial flexibility appear to pass on their pandemic adjustment costs to their new employees. Our results suggest that financial constraints could accentuate changes in worker skills, roles, requirements, and employment arrangements and are likely to shape hiring going forward. Our findings give a reference point for assessing changes in the nature of jobs and work arrangements in a post-pandemic world, as well as the role of financial constraints in influencing these trends. They also suggest that constrained firms (and their workers) might require further targeted policy support in their path to recovery following the COVID-19 crisis.

IX. Concluding Remarks

This study provides a comprehensive account of the effects of the COVID-19 pandemic on corporate hiring based on granular, firm-level job posting activity. We report sharp declines in hiring across the board, but with meaningful heterogeneity along firm financial constraints and job skill requirements. A particularly concerning trend is that firms are disproportionately cutting back on high-skill hiring (*within-firm downskilling*). Firms that are more adaptable to remote work exhibit depressed new job posting levels, potentially reflecting their enhanced ability to retain existing workers in flexible work arrangements. Financially constrained firms witness particularly severe declines in new hiring, notably for high-skill positions. Textual analyses of job postings further suggest that the pandemic has led to major changes in job roles and requirements, particularly in financially constrained firms. The positions that firms created after the pandemic require greater flexibility and are more core to the organization.

The hiring patterns that we document speak to fundamental changes in both the nature of the work performed by employees and their workplace environment. Critically, we uncover an apparent divergence between the medium-term recovery in job posting *levels* and the relatively persistent changes to the *qualitative aspects* of job descriptions. Our analysis is particularly informative in distinguishing

https://doi.org/10.1017/S0022109023000522

between aggregate labor market developments (e.g., increased difficulty in filling low-skill roles) and within-firm hiring responses (e.g., downskilling) that may coexist under the pandemic. By emphasizing the latter, we highlight the importance of ensuring that firms have adequate access to financing in order restore and grow their human capital base.

Appendix A. Theory and Variable Definitions

A.1. Theoretical Framework

A.1.1. Setup

Consider the hiring decision of a firm operating for three periods, $t=0,1,$ and 2. The firm's decision problem involves choosing whether and when to invest in two "types" of human capital: high-skill hiring and low-skill hiring.[21] The firm faces a continuum of potential projects, n, which lies on the interval $[0,N]$. Each project requires both high-skill workers, denoted by h, and low-skill workers, denoted by l, with $l>h$.[22]

Investment Income. Let the firm's cash flows from investing in a project, n, at $t=1,2$, be $v_t^{(n)}>0$, an independently and identically distributed (IID) random variable of the form $v_t^{(n)}=v_t$. In this setting, $v_t>0$ represents the time-varying demand for output generated by the firm's projects. The projects' cash flow, v_t, is distributed as $v_t \sim P(\overline{v_t},p)$, where the mean of v_t is equal to $\overline{v_t}$, the variance is equal to $\sigma^2(p)$, and p is an index of the mean-preserving spread. Specifically, $p'>pP(\cdot,p')$ is a mean-preserving spread (MPS) of $P(\cdot,p)$ and $\int v_t dP(\cdot,p)=\overline{v_t}\forall p$.[23]

Investment Costs. In order to undertake each project n, the firm incurs a sunk cost of high-skill hiring, denoted by $F_H(\eta,h)=\eta h$, and a sunk cost of low-skill hiring, denoted by $F_L(\lambda,l)=\lambda l$, both of which increase linearly in h and l, and thus n. The parameters $\eta>0$ and $\lambda>0$ capture the degree of (partial) irreversibility of the respective investments in high- and low-skill labor, as these components (which also scale linearly) cannot be recovered if the hiring decisions are reversed. One assumption we make is that $\eta>\lambda$ (Oi (1962)). This reflects the fact that high-skill hiring involves greater fixed costs, rendering such decisions costlier to reverse than low-skill hiring. This assumption is supported by a sizeable literature in labor economics on the particular rigidities of high-skill (relative to low-skill hiring).[24] An implication of these differential costs of irreversibility is the emergence of within-firm downskilling under the pandemic, where firms disproportionately cut back on high-skill postings relative to low-skill postings.

The firm can choose whether to invest in project n, and when to invest (either at $t=0$ or $t=1$). If it invests in n at $t=0$, it incurs sunk costs $\eta h+\lambda l$ at $t=1$, and earns

[21] As explained in Section A.1.1.2, these hiring decisions differ in the extent of fixed costs – and thus degree of irreversibility – incurred by the firm.

[22] This assumption captures the typical corporate employment structure of firms in our sample. These firms post, on average, only 3 high-skill job ads for every 10 low-skill position they advertise.

[23] See also Lee and Shin (2000) for a similar modeling approach.

[24] See, for example, Blatter et al. (2012) for evidence on the positive relation between hiring costs and worker skill levels.

https://doi.org/10.1017/S0022109023000522

revenues $v_1 + v_2$. If it chooses not to invest at $t=0$, waiting instead to invest at $t=1$, it incurs the fixed costs $\eta h + \lambda l$ at $t=2$, earning only the revenue v_2.

A.1.2. Analysis and Results

In what follows, we present analysis and results corresponding to the firm's hiring decision. We also derive results on the cross-sectional implications of the role played by irreversibility costs on high-skill versus low-skill hiring. Proofs of propositions and lemmas immediately follow the model exposition.

To solve the firm's hiring problem, we must first consider its decision at $t=1$, and then iterate backward. If the firm had initiated any projects at $t=0$, it obtains the second period cash flow v_2 per project. Among projects that the firm left uninvested at $t=0$, the firm can choose to invest in any of them at $t=1$ and earn $v_2 - (\eta h + \lambda l)n$ per project. Alternatively, it can discard any uninvested projects and earn 0. The firm will optimally discard a given project, $\tilde{n}$, when its expected revenue is less than the associated costs of hiring. The firm ceases to operate at the end of $t=2$ and any project that is not undertaken at either $t=0$ or $t=1$ will have a value of 0. The firm's hiring decision at $t=1$ will, therefore, be guided by value in the second period that is generated by project $\tilde{n}$. The value function, π_2, can be characterized as

$$\text{(A-1)} \qquad 1\,\pi_2(\tilde{n}) = \begin{cases} v_2 & \text{(Early Hiring)}, \\ v_2 - \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n} \ \ \text{if}\ v_2 > \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n} & \text{(Delayed Hiring)}, \\ 0 \qquad\qquad\qquad\quad \text{if}\ v_2 \leq \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n} & \text{(No Hiring)}. \end{cases}$$

Next, we consider the firm's decision at $t=0$. The optimal total investment level at $t=0$ can be expressed in terms of n^*, the breakeven project. Note that the breakeven project n^* uniquely maps to optimal levels of high- and low-skill hiring, h^* and l^*. The firm will invest in all projects in the range $[0, n^*)$, and not invest in projects in the range $[n^*, N]$, instead waiting until $t=1$ to decide whether to undertake any of those projects. The firm's expected profit from investing in project $\tilde{n}$ at $t=0$ is $v_1 + \mathbb{E}[v_2] - \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n}$. Its expected profit from not investing in $\tilde{n}$ at $t=0$, while choosing instead to wait until $t=1$ is $\mathbb{E}\left[\max\left(v_2 - \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n}, 0\right)\right]$. The firm invests in project $\tilde{n}$ at $t=0$ if

$$\text{(A-2)} \qquad 1\ \underbrace{v_1 + \mathbb{E}[v_2]}_{\text{Expected Revenue}} \geq \underbrace{\left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n}}_{\text{Cost of Hiring}} + \underbrace{\mathbb{E}\left[\max\left(v_2 - \left(\eta\tilde{h} + \lambda\tilde{l}\right)\tilde{n}, 0\right)\right]}_{\text{Value of Waiting}}.$$

The breakeven condition for determining the optimal project level n^* at $t=0$ is

$$\text{(A-3)} \qquad 1 v_1 + \mathbb{E}[v_2] = (\eta h^* + \lambda l^*)n^* + \mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0)].$$

In Lemma 1, we prove the existence of the optimal $t=0$ project investment level, n^*.

Lemma 1. The optimal project investment level n^* (and therefore, hiring levels h^* and l^*) at $t=0$ is given by equation (A-3) for sufficiently large N.

The breakeven condition in equation (A-3) implies that the firm invests in all projects at $t=0$ up to project n^*, hiring high-skill (low-skill) workers up to h^* (l^*), for which the benefits are expected to exceed the costs. It is straightforward to derive the impact of a negative first-moment shock from the condition in equation (A-3).

https://doi.org/10.1017/S0022109023000522

A reduction in expected cash flows, v_2, reduces the left-hand side of equation (A-3) by more than it does the second term on the right-hand side. It follows that the breakeven project level n^*, as well as breakeven high-skill and low-skill hiring levels, h^* and l^*, fall in order for the expression to hold with equality. This reflects the simple intuition that when faced with expectations of declining cash flows, the firm will cut back on its forward-looking hiring decisions across roles corresponding to all skill levels. We state this result formally in Proposition 1.

Proposition 1. A negative first-moment shock leads to less hiring (both high- and low-skill hiring) at $t=0$. For $\overline{v_t}' > \overline{v_t}$, $n^*(\overline{v_t}') > n^*(\overline{v_t})$, $h^*(\overline{v_t}') > h^*(\overline{v_t})$, and $l^*(\overline{v_t}') > l^*(\overline{v_t})$. That is, $\frac{dn^*}{d\overline{v_t}} > 0$, $\frac{dh^*}{d\overline{v_t}} > 0$, and $\frac{dl^*}{d\overline{v_t}} > 0$.

We next turn to analyzing the role of a second-moment shock on the firm's hiring. The embedded optionality in the firm's hiring decision is key in generating a negative relation between uncertainty and investment. An increase in uncertainty in the distribution of v_t reduces the breakeven project level n^*, and correspondingly shrinks the set of high- and low-skill workers the firm hires in at $t=0$. We establish this result in Proposition 2.

Proposition 2. Increased uncertainty (a positive second-moment shock) leads to less hiring at $t=0$. For $p' > p$, namely when $P(\cdot,p')$ is obtained by a mean-preserving spread of $P(\cdot,p)$, $n^*(p') < n^*(p)$, $h^*(p') < h^*(p)$, and $l^*(p') < l^*(p)$. That is, $\frac{dn^*}{dp} < 0$, $\frac{dh^*}{dp} < 0$, and $\frac{dl^*}{dp} < 0$.

Taken together, Propositions 1 and 2 imply that the firm's forward-looking hiring declines in the face of a dual negative first-moment and positive second-moment shock. These propositions are informative in that they likely capture conditions faced by most firms at the onset of the COVID-19 pandemic. In turn, we address the role played by the differential costs of irreversibility of high-skill and low-skill hiring in the following proposition.

Proposition 3. A higher degree of irreversibility of high-skill hiring (relative to low-skill hiring) leads to less high-skill hiring (relative to low-skill hiring) for higher levels of uncertainty in the first period (i.e.,$\eta > \lambda \frac{dh^*}{dp} < \frac{dl^*}{dp}$.).

Proposition 3 implies that if the firm faces higher irreversibility costs in its high-skill hiring, it will recruit even fewer such workers (relative to low-skill counterparts) at $t=0$ when facing an increase in the distribution of v_t. Differently put, an increase in uncertainty reduces hiring across the board in the first period, and the effect is shaped by the degree to which reversing their skills-based hiring decisions incurs costs.

The following section lays out the mathematical proofs for the above lemma and propositions.

A.2. *Proofs*

A.2.1. Proof of Lemma 1

Proof. Let us define

$$H(n^*) = v_1 + \mathbb{E}[v_2] - (\eta h^* + \lambda l^*)n^* - \mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0)].$$

https://doi.org/10.1017/S0022109023000522

To guarantee the existence of n^* as characterized by equation (A-3), it suffices to show that $H(n^*) = 0$ for some $n^* \in [0, N]$. Since $H(\cdot)$ is a sum of continuous functions, it is itself continuous. Since $v_1 > 0$ and $v_2 > 0$, it follows that

$$H(0) = v_1 + \mathbb{E}[v_2] - \mathbb{E}[\max(v_2, 0)] = v_1 > 0.$$

Finally, for $N \to \infty$, we have that

$$\lim_{N\to\infty} H(N) < 0$$

Thus, the intermediate value theorem guarantees that there exists an $n^* \in [0, N]$ (and, therefore, h^* and l^*) such that $H(n^*) = 0$. □

A.2.2. Proof of Proposition 1

Proof. Let us define

$$H(n^*) = v_1 + \mathbb{E}[v_2] - (\eta h^* + \lambda l^*)n^* - \mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0)] = 0.$$

The proposition follows immediately by examining the first-order condition of the above, since the "delta" of a call option will always be bounded from above by 1. □

A.2.3. Proof of Proposition 2

Proof. Let us define

$$H(n^*; p) = v_1 + \mathbb{E}[v_2] - (\eta h^* + \lambda l^*)n^* - \mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0); p] = 0.$$

We know that, by the implicit function theorem,

$$\frac{dn^*}{dp} = -\frac{\partial H / \partial n^*}{\partial H / \partial p}.$$

Considering first the derivative of H with respect to n^*, it immediately follows that the numerator of the above expression is positive. What remains to be shown is that the denominator is positive. Considering the derivative of H with respect to p, we have

$$\frac{\partial H(n^*; p)}{\partial p} = -\frac{\partial}{\partial r}\mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0); r].$$

Because $P(\cdot, p')$ is a MPS of $P(\cdot, p)$, for any convex function $J(\cdot)$,

$$\begin{aligned} \mathbb{E}[J(v_2); p'] &= \int J(v_2) dP(v_2, p') \\ &\geq \int J(v_2) dP(v_2, p) \\ &= \mathbb{E}[J(v_2); p]. \end{aligned}$$

Since $\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0)$ is convex in v_2, it follows that

$$\mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0); p'] \geq \mathbb{E}[\max(v_2 - (\eta h^* + \lambda l^*)n^*, 0); p] \forall p' > p.$$

https://doi.org/10.1017/S0022109023000522

This implies

$$\frac{\partial}{\partial p}\mathbb{E}[\max(v_2-(\eta h^*+\lambda l^*)n^*,0);p]\geq 0.$$

Thus,

$$\begin{aligned}\frac{\partial H(n^*;p)}{\partial p} &= -\frac{\partial}{\partial p}\mathbb{E}[\max(v_2-(\eta h^*+\lambda l^*)n^*,0);p]\\ &\leq 0.\end{aligned}$$

Putting these conditions together, we have

$$\frac{dn^*}{dp}=-\frac{\partial H/\partial n^*}{\partial H/\partial p}<0.$$

□

A.2.4. Proof of Proposition 3

Proof. Let us define

$$H(n^*)=v_1+\mathbb{E}[v_2]-(\eta h^*+\lambda l^*)n^*-\mathbb{E}[\max(v_2-(\eta h^*+\lambda l^*)n^*,0)]=0.$$

Examining the conditions from the proof of Proposition 2, it becomes clear that, $\frac{\partial H(h^*;p)}{\partial p}$ and $\frac{\partial H(l^*;p)}{\partial p}$ differ only in that $\frac{\partial H(h^*;p)}{\partial p}$ is an increasing function of η and $\frac{\partial H(l^*;p)}{\partial p}$ is an increasing function of λ. Therefore, Proposition 3 follows immediately. □

A.3. Variable Definitions

NEW_JOB_POSTINGS: Logarithm of 1 plus the total number of job postings created in a firm-ZIP-time triple. Source: LinkUp.

HIGH_TO_LOW_SKILL_RATIO: Total number of job postings created with O*NET occupation codes corresponding to Job Zone 5 divided by the total number of job postings corresponding to Job Zone 1 in a firm-ZIP-time triple. Source: LinkUp.

CORE_JOBS: Average ordinality of the intersection of keywords present in firm 10-K business descriptions and job posting description texts, scaled by the ordinality of the set of firm-level keywords, across all new postings in a firm-ZIP-time triple (see Section III.C.2). Source: LinkUp/EDGAR.

JOB_FLEXIBILITY: Logarithm of the number of occurrences of "flexibility"-related keywords scaled by the total posting length, averaged across all job postings created in a firm-ZIP-time triple (see Section III.C.2). Source: LinkUp.

COVID-19 Exposure

COVID: Indicator variable that takes the value of 1 for each week after Feb. 29, 2020, and 0 otherwise.

HIGH_EXPOSURE: Indicator variable that takes the value of 1 for each county-week belonging to the highest tercile of the number of confirmed COVID cases per capita

https://doi.org/10.1017/S0022109023000522

and 0 for each county-week belonging to the lowest tercile of the number of confirmed COVID cases per capita. *Source: New York Times*.

COVID_CASE_RANK: Rank of county in the weekly distribution of COVID cases across the U.S., with the county with highest number of COVID cases for the week assigned a rank of 1. *Source: New York Times*.

COVID_MAR_2020: Indicator variable that takes the value of 1 for all months after Mar. 2020, and 0 for all months prior.

COVID_SEP_2020: Indicator variable that takes the value of 1 for all months after Sept. 2020, and 0 for all months prior.

COVID_APR_2021: Indicator variable that takes the value of 1 for all months after Apr. 2021, and 0 for all months prior.

EARLY_REOPENING: Indicator variable that takes the value of 1 for all ZIPs located in states whose opening dates were in the top tercile (earliest to reopen, or no state-wide shutdown policy) of the chronological distribution of state reopening dates and 0 for ZIPs located in states whose opening dates were in the bottom tercile (latest to reopen). Source: Nguyen et al. (2021).

Workplace Characteristics

HIGH_TELEWORKING_DN: Indicator variable set to 1 for firms above median share of active job postings belonging to O*NET occupation codes classified as tele-workable by Dingel and Neiman (2020) (measured over the 2017–2019 period), and 0 otherwise. Source: LinkUp/Dingel and Neiman (2020).

HIGH_TELEWORKING_PS: Indicator variable that takes the value of 1 for firms in industries above median of work-from-home difficulty index (Papanikolaou and Schmidt (2020)), and 0 otherwise. Source: Papanikolaou and Schmidt (2020).

Financial Constraints Measures

SMALL_FIRM: Indicator variable that takes the value of 1 for firms below the median of total assets (measured in the last available year), and 0 otherwise. Source: Compustat.

SPECULATIVE_GRADE: Indicator variable that takes the value of 1 for firms with a speculative grade rating, and 0 otherwise. Source: Eikon.

NO_CREDIT_LINES: Indicator variable that takes the value of 1 for firms with no outstanding lines of credit, and 0 otherwise. Source: DealScan.

LOW_CASH_HOLDINGS: Indicator variable that takes the value of 1 for firms below median of cash (scaled by lagged assets), and 0 otherwise. Source: Compustat.

HIGH_CURRENT_LT_DEBT: Indicator variable that takes the value of 1 for firms with more than 20% of total long-term debt maturing within 1 year as of 2019 and 0 for firms with less than 20% of total long-term debt maturing within 1 year as of 2019, following Almeida et al. (2012). Source: Compustat.

BBB: Indicator variable that takes the value of 1 for firms with an S&P credit rating of BBB−, BBB, or BBB+ as of 2019, and 0 for firms with a credit rating of A− or above as of 2019. Source: Eikon.

https://doi.org/10.1017/S0022109023000522

State Controls

JOB_SEARCH_INTEREST: Workers' intensity of job search at state-time level. Source: Google Trends.

UNEMPLOYMENT_RATE: Unemployed people as percentage of labor force within state-time. Source: BLS.

LABOR_FORCE: Sum of the employed and unemployed people within state-time. Source: BLS.

UNEMPLOYMENT_BENEFITS: Logarithm of the average weekly unemployment benefit paid by the state. Source: U.S. Dept. of Labor.

Firm Controls

SIZE: Logarithm of total assets. Source: Compustat.

NET_LEVERAGE: Total short- and long-term debt minus cash divided by lagged assets. Source: Compustat.

PROFITABILITY: Net income divided by lagged assets. Source: Compustat.

Q: Ratio of the market value of equity plus the difference between the book value of assets and the book value of equity plus deferred taxes to the book value of assets. Source: CRSP/Compustat.

INVESTMENT: Capital expenditures divided by lagged assets. Source: Compustat.

EMPLOYMENT: Logarithm of total employees. Source: Compustat/YTS.

Appendix B. Data Validation

It is important to demonstrate the correspondence of our job postings data with realized job creation in the economy. We do so by comparing our data with administrative data on employment. Figure B1 plots this relation across various sample characterizations. Graphs A–D point to a significant LinkUp-QWI relation within data characterized along the lines of firm size and job skills. In Table B1, we perform Granger causality tests of total new postings from LinkUp data and lead (next-quarter) job gains from the U.S. Census Bureau's Quarterly Workforce Indicators (QWI) data. Column 1 shows that increased job postings are predictive of firm job gains in the subsequent quarter in the vast majority of U.S. states. The monthly times-series correlation between the total job postings in LinkUp and the total private-sector hires in BLS's Job Openings and Labor Turnover Survey (JOLTS) over 2017–2020 is also highly significant ($\rho = 0.6^{***}$). The robust associations between the LinkUp job postings data and administrative data on job creation and realized employment levels are important given the absence of micro-level, real-time data on firm layoffs. We additionally confirm the representativeness of the LinkUp data along the lines of industry coverage. Graph A of Figure B2 reveals that the industry distribution of LinkUp job postings relative to JOLTS job vacancies is remarkably similar to the analogous comparison reported by Hershbein and Kahn (2018).

We further verify that new job postings in LinkUp lead to realized job creation. Column 2 of Table B1 establishes that increased job posting activity Granger-causes higher actual employment proxied by employee payroll records (obtained from Kronos)

https://doi.org/10.1017/S0022109023000522

FIGURE B1

Cross-Sectional Data Validation

Graph A (B) of Figure B1 plots the average state-level relation between lagged total new postings (from LinkUp data) and firm job gains from the U.S. Census Bureau's Quarterly Workforce Indicators (QWI) for the sample of smallest (largest) firms. Firm size is based on the number of employees, as reported in the QWI data. Small firms consist of firms with 0–19 employees and large firms consist of firms with over 500 employees. Graph C (D) depicts the relation within the sample of jobs in Zone 1 (5), per LinkUp, and lowest (highest) education requirement, per QWI. Data are in logs and represented in the form of 20 equal-sized bins based on the cross-sectional distribution of the depicted variables.

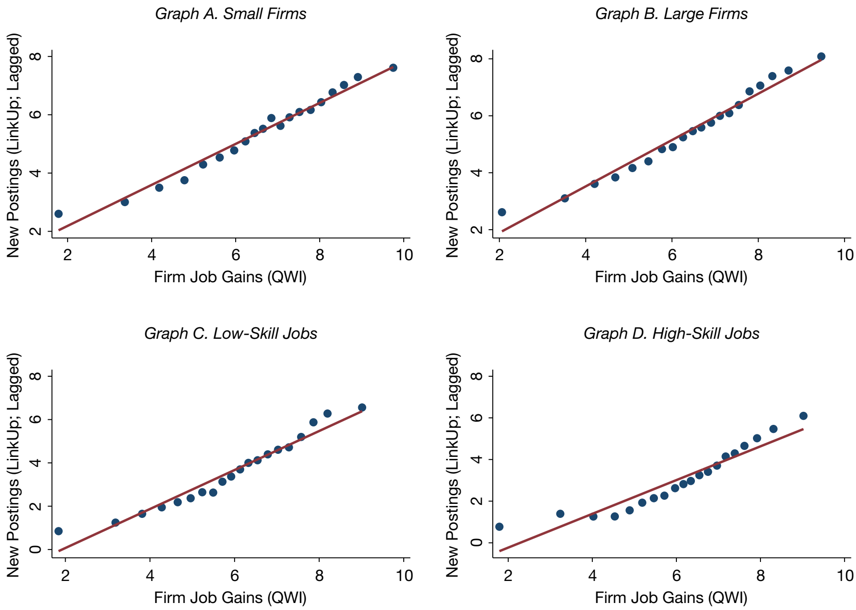


TABLE B1

Granger Causality Tests

Table B1 reports output from the joint vector auto regression (VAR(1)) estimation of pairs of variables representing job posting activity and firm job gains and losses. The estimation takes the form $\mathbf{y}_t = A_1\mathbf{y}_{t-1} + \mathbf{e}_t$ followed by a pairwise Granger causality test. The unit of observation is state-quarter. Variable definitions are as provided in Appendix A.3. The table reports the average coefficient and robust standard errors obtained from estimating the time-series for each state, the average F-statistic corresponding to the Granger causality test, and percentage of observations where the corresponding p-value is significant at the 10% level. Columns 1 and 3 are estimated over the 2017:Q1 to 2018:Q3 period. Column 2 is estimated over the 2019:Q1 to 2020:Q4 period. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	FIRM_JOB_GAINS$_t$	ΔEMPLOYEE_PAYROLLS$_t$	NEW_JOB_POSTINGS$_t$
	1	2	3
NEW_JOB_POSTINGS$_{t-1}$	1.221***	0.700***	
	(0.371)	(0.198)	
FIRM_JOB_LOSSES$_{t-1}$			0.356*
			(0.200)
Granger causality			
F	4.377	34.050	8.165
$p < 0.01$	50%	100%	73%

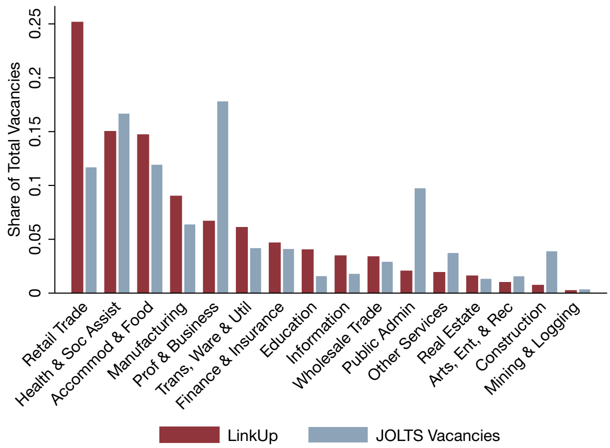
https://doi.org/10.1017/S0022109023000522

FIGURE B2

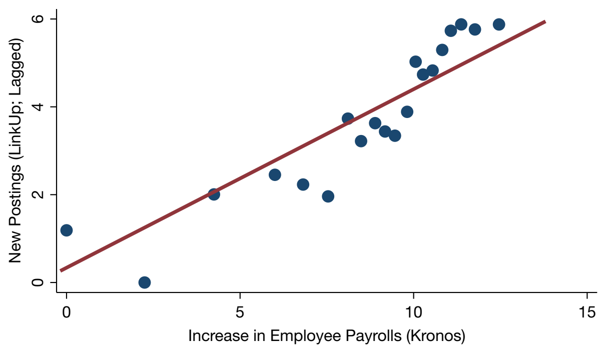
Job Vacancies, Job Postings, and Employment Levels

Figure B2 represents the cross-sectional relationship between LinkUp job postings, administrative data on vacancies, and realized employment levels. Graph A plots the share of total vacancies from the LinkUp data (in red) and JOLTS (in blue) across NAICS 2-digit industry codes averaged over the period of 2017 to 2020. Graph B plots the relation between total new postings (from LinkUp data) and the increase in employee payrolls from Kronos in the subsequent quarter at the industry-county level. Data are in logs and represented in the form of 20 equal-sized bins based on the cross-sectional distribution of the depicted variables.

Graph A. Industry Distribution of Job Vacancies



Graph B. Firm Job Postings and Employment Levels



in the subsequent quarter across all U.S. states (see also Chetty et al. (2020)).[25] We illustrate this fact by plotting the relation between LinkUp job postings and the increase in employee payrolls. Graph B of Figure B2 shows that job ads translate into future (one-quarter ahead) increases in de facto person-hours worked.

[25]The payroll records themselves are derived from employees' reporting of work start and end times, which can occur both physically (through punch cards) or virtually (through Kronos' widely-used online portal, applicable for both in-person and remote positions). Kronos data are likely to be broadly representative of employment across the skill spectrum. For instance, Kronos' payroll management systems are widely used in organizations such as research laboratories and academic institutions that employ workers at the high end of the skill spectrum, yet pay on hourly basis, requiring them to log their hours worked. Other examples include billed-by-the-hour lawyers and consultants in the private sector.

https://doi.org/10.1017/S0022109023000522

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109023000522.

References

Abadie, A.; S. Athey; G. Imbens; and J. Wooldridge. "When Should you Adjust Standard Errors for Clustering?" *Quarterly Journal of Economics*, 138 (2023), 1–35.

Acharya, V., and S. Steffen. "The Risk of Being a Fallen Angel and the Corporate Dash for Cash in the Midst of COVID." *Review of Corporate Finance Studies*, 9 (2020), 430–471.

Albuquerque, R.; Y. Koskinen; S. Yang; and C. Zhang. "Resiliency of Environmental and Social Stocks: An Analysis of the Exogenous COVID-19 Market Crash." *Review of Corporate Finance Studies*, 9 (2020), 593–621.

Alfaro, I.; N. Bloom; and X. Lin. "The Finance Uncertainty Multiplier." NBER Working Paper No. 24571 (2018).

Almeida, H.; M. Campello; B. Laranjeira; and S. Weisbenner. "Corporate Debt Maturity and the Real Effects of the 2007 Credit Crisis." *Critical Finance Review*, 1 (2012), 3–58.

Altig, D.; S. Baker; J. M. Barrero; N. Bloom; P. Bunn; S. Chen; S. Davis; J. Leather; B. Meyer; E. Mihaylov; P. Mizen; N. Parker; T. Renault; P. Smietanka; and G. Thwaites "Economic Uncertainty Before and During the COVID-19 Pandemic." *Journal of Public Economics*, 191 (2020), 104,274.

Autor, D.; F. Levy; and R. Murnane. "The Skill Content of Recent Technological Change: An Empirical Exploration." *Quarterly Journal of Economics*, 118 (2003), 1279–1333.

Baghai, R.; R. Silva; V. Thell; and V. Vig. "Talent in Distressed Firms: Investigating the Labor Costs of Financial Distress." *Journal of Finance*, 76 (2021), 2907–2961.

Bai, J.; E. Brynjolfsson; W. Jin; S. Steffen; and C. Wan. "Digital Resilience: How Work-From-Home Feasibility Affects Firm Performance." NBER Working Paper No. 28588 (2021).

Barry, J.; M. Campello; J. Graham; and Y. Ma. "Corporate Flexibility in a Time of Crisis." *Journal of Financial Economics*, 144 (2022), 780–806.

Bena, J.; I. Erel; D. Wang; and M. Weisbach. "Specialized Investments and Firms' Boundaries: Evidence from Textual Analysis of Patents." NBER Working Paper No. 29174 (2021).

Benmelech, E.; N. Bergman; and A. Seru. "Financing Labor." *Review of Finance*, 25 (2021), 1365–1393.

Blatter, M.; S. Muehlemann; and S. Schenker. "The Costs of Hiring Skilled Workers." *European Economic Review*, 56 (2012), 20–35.

Bloom, N. "The Impact of Uncertainty Shocks." *Econometrica*, 77 (2009), 623–685.

Bloom, N.; R. Fletcher; and E. Yeh. "The Impact of COVID-19 on US Firms." NBER Working Paper No. 28314 (2021).

Brown, J., and D. Matsa. "Boarding a Sinking Ship? An Investigation of Job Applications to Distressed Firms." *Journal of Finance*, 71 (2016), 507–550.

Brynjolfsson, E.; J. Horton; A. Ozimek; D. Rock; G. Sharma; and H.-Y. TuYe. "COVID-19 and Remote Work: An Early Look at US Data." NBER Working Paper No. 27344 (2020).

Bybee, L.; B. Kelly; A. Manela; and D. Xiu. "The Structure of Economic News." NBER Working Paper No. 26648 (2020).

Caggese, A.; V. Cuñat; and D. Metzger. "Firing the Wrong Workers: Financing Constraints and Labor Misallocation." *Journal of Financial Economics*, 133 (2019), 589–607.

Cajner, T.; L. D. Crane; R. Decker; J. Grigsby; A. Hamins-Puertolas; E. Hurst; C. J. Kurz; and A. Yildirmaz. "The US Labor Market During the Beginning of the Pandemic Recession." Working Paper, University of Chicago, Becker Friedman Institute for Economics (2020).

Cejnek, G.; O. Randl; and J. Zechner. "The COVID-19 Pandemic and Corporate Dividend Policy." *Journal of Financial and Quantitative Analysis*, 56 (2021), 2389–2410.

Chetty, R.; J. Friedman; N. Hendren; M. Stepner; and The Opportunity Insights Team. "How Did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data." NBER Working Paper No. 27431 (2020).

Chetty, R.; J. Friedman; and E. Saez. "Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings." *American Economic Review*, 103 (2013), 2683–2721.

Chodorow-Reich, G. "The Employment Effects of Credit Market Disruptions: Firm-Level Evidence from the 2008–9 Financial Crisis." *Quarterly Journal of Economics*, 129 (2014), 1–59.

Chodorow-Reich, G.; O. Darmouni; S. Luck; and M. Plosser. "Bank Liquidity Provision Across the Firm Size Distribution." *Journal of Financial Economics*, 144 (2022), 908–932.

Cohn, J.; Z. Liu; and M. Wardlaw. "Count (and Count-like) Data in Finance." *Journal of Financial Economics*, 146 (2022), 529–551.

Ding, W.; R. Levine; C. Lin; and W. Xie. "Corporate Immunity to the COVID-19 Pandemic." *Journal of Financial Economics*, 141 (2021), 802–830.

Dingel, J., and B. Neiman. "How Many Jobs Can Be Done at Home?" *Journal of Public Economics*, 189 (2020), 104,235.

Domash, A., and L. Summers. "How Tight are US Labor Markets?" NBER Working Paper No. 29739 (2022).

Duchin, R., and J. Harford. "The COVID-19 Crisis and the Allocation of Capital." *Journal of Financial and Quantitative Analysis*, 56 (2021), 2309–2319.

Faberman, J.; A. Mueller; and A. Sahin. "Has the Willingness to Work Fallen During the Covid Pandemic?" *Labour Economics*, 79 (2022), 102,275.

Fahlenbrach, R.; K. Rageth; and R. Stulz. "How Valuable Is Financial Flexibility when Revenue Stops? Evidence from the COVID-19 Crisis." *Review of Financial Studies*, 34 (2021), 5474–5521.

Falato, A., and N. Liang. "Do Creditor Rights Increase Employment Risk? Evidence from Loan Covenants." *Journal of Finance*, 71 (2016), 2545–2590.

Gentzkow, M.; B. Kelly; and M. Taddy. "Text as Data." *Journal of Economic Literature*, 57 (2019), 535–574.

Gilchrist, S.; J. Sim; and E. Zakrajšek. "Uncertainty, Financial Frictions, and Investment Dynamics." NBER Working Paper No. 20038 (2014).

Giroud, X., and H. Mueller. "Firm Leverage, Consumer Demand, and Employment Losses During the Great Recession." *Quarterly Journal of Economics*, 132 (2017), 271–316.

Graham, J.; H. Kim; S. Li; and J. Qiu. "Employee Costs of Corporate Bankruptcy." NBER Working Paper No. 25922 (2019).

Greenwald, D.; J. Krainer; and P. Paul. "The Credit Line Channel." Working Paper, Federal Reserve Bank of San Francisco (2020).

Hershbein, B., and L. Kahn. "Do Recessions Accelerate Routine-Biased Technological Change? Evidence from Vacancy Postings." *American Economic Review*, 108 (2018), 1737–1772.

Hoberg, G., and G. Phillips. "Text-Based Network Industries and Endogenous Product Differentiation." *Journal of Political Economy*, 124 (2016), 1423–1465.

Hoberg, G.; G. Phillips; and N. Prabhala. "Product Market Threats, Payouts, and Financial Flexibility." *Journal of Finance*, 69 (2014), 293–324.

Holzer, H.; G. Hubbard; and M. Strain. "Did Pandemic Unemployment Benefits Reduce Employment? Evidence from Early State-Level Expirations in June 2021." NBER Working Paper No. 20038 (2021).

Iacus, S.; G. King; and G. Porro. "Causal Inference Without Balance Checking: Coarsened Exact Matching." *Political Analysis*, 20 (2012), 1–24.

Kargar, M.; B. Lester; D. Lindsay; S. Liu; P.-O. Weill; and D. Zúñiga. "Corporate Bond Liquidity during the COVID-19 Crisis." *Review of Financial Studies*, 34 (2021), 5352–5401.

Kelly, B.; D. Papanikolaou; A. Seru; and M. Taddy. "Measuring Technological Innovation over the Long Run." *American Economic Review: Insights*, 3 (2021), 303–320.

Lee, J., and K. Shin. "The Role of a Variable Input in the Relationship Between Investment and Uncertainty." *American Economic Review*, 90 (2000), 667–680.

Lewellen, K., and F. Severino. "The Cyclical Growth of Public Firms and the COVID Crisis." Available at SSRN 3827999 (2021).

Li, K.; X. Liu; F. Mai; and T. Zhang. "The Role of Corporate Culture in Bad Times: Evidence from the COVID-19 Pandemic." *Journal of Financial and Quantitative Analysis*, 56 (2021a), 2545–2583.

Li, K.; F. Mai; R. Shen; and X. Yan. "Measuring Corporate Culture Using Machine Learning." *Review of Financial Studies*, 34 (2021b), 3265–3315.

Li, L.; P. Strahan; and S. Zhang. "Banks as Lenders of First Resort: Evidence from the COVID-19 Crisis." *Review of Corporate Finance Studies*, 9 (2020), 472–500.

Loughran, T., and B. McDonald. "Textual Analysis in Finance." *Annual Review of Financial Economics*, 12 (2020), 357–375.

Manela, A., and A. Moreira. "News Implied Volatility and Disaster Concerns." *Journal of Financial Economics*, 123 (2017), 137–162.

Memon, S. A.; S. Razak; and I. Weber. "Lifestyle Disease Surveillance Using Population Search Behavior: Feasibility Study." *Journal of Medical Internet Research*, 22 (2020), e13347.

Michelacci, C., and V. Quadrini. "Financial Markets and Wages." *Review of Economic Studies*, 76 (2009), 795–827.

https://doi.org/10.1017/S0022109023000522

Mihalcea, R., and P. Tarau. "TextRank: Bringing Order into Text." *Proceedings of the 2004 Conference on Empirical Methods in Natural Language Processing* (2004), 404–411.

Mikolov, T.; K. Chen; G. Corrado; and J. Dean. "Efficient Estimation of Word Representations in Vector Space." arXiv preprint arXiv:1301.3781 (2013).

Nguyen, T.; S. Gupta; M. Andersen; A. Bento; K. Simon; and C. Wing. "Impacts of State COVID-19 Reopening Policy on Human Mobility and Mixing Behavior." *Southern Economic Journal*, 88 (2021), 458–486.

O'Hara, M., and X. A. Zhou. "Anatomy of a Liquidity Crisis: Corporate Bonds in the COVID-19 Crisis." *Journal of Financial Economics*, 142 (2021), 46–68.

Oi, W. "Labor as a Quasi-fixed Factor." *Journal of Political Economy*, 70 (1962), 538–555.

Pagano, M., and P. Volpin. "Labor and Finance." *London Business School, Mimeo* (2008).

Pagano, M., and J. Zechner. "COVID-19 and Corporate Finance." *Review of Corporate Finance Studies*, 11 (2022), 849–879.

Papanikolaou, D., and L. Schmidt. "Working Remotely and the Supply-side Impact of COVID-19." *Review of Asset Pricing Studies*, 12 (2022), 53–111.

Pettenuzzo, D.; R. Sabbatucci; and A. Timmermann. "Outlasting the Pandemic: Corporate Payout and Financing Decisions During COVID-19." Available at SSRN 3846260 (2021).

Ramelli, S., and A. Wagner. "Feverish Stock Price Reactions to COVID-19." *Review of Corporate Finance Studies*, 9 (2020), 622–655.

https://doi.org/10.1017/S0022109023000522