Work Organization and High-paying Jobs*

Dylan Nelson — MIT Sloan
Nathan Wilmers — MIT Sloan
Letian Zhang — HBS

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Abstract

High-paying factory jobs in the 1940s were an engine of egalitarian economic growth for a generation. Are there alternate forms of work organization that deliver similar benefits for frontline workers? Work organization varies by types of complexity and their degree of employer control. Technical and tacit knowledge tasks receive higher pay for signaling or developing human capital. Higher autonomy tasks elicit efficiency wages. To test these ideas, we match administrative earnings to task descriptions from job postings. We then compare earnings for workers hired into the same occupation and firm, but under different task allocations. When jobs raise task complexity and autonomy, new hires' starting earnings increase and grow faster. However, while half the earnings boost from complex, technical tasks is due to shifting worker selection, worker selection changes less for tacit knowledge tasks and very little for adding high autonomy tasks. We also study which employers provide these jobs: frontline tacit knowledge tasks are disproportionately in larger, profitable manufacturing and retail firms; technical tasks are in newer health and business services; and higher autonomy jobs are in smaller and fast-growing firms. These results demonstrate how organization-level allocations of tasks can undergird high-paying jobs for frontline workers.

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1 Introduction

From the 1940s to the 1970s, wage compression brought a rising share of national income to the bottom half of Americans (Goldin and Margo, 1992; Piketty and Saez, 2003). Declining inequality was delivered via large manufacturing firms with high productivity growth, in which managers and engineers organized a precise division of labor among frontline workers (Piore and Sabel, 1984; Nelson, 1996; Davis and Cobb, 2010). The industrial unions that emerged in the 1940s abdicated authority over this fragmented, intensified organization of work (Montgomery, 1980), but ensured growing wages through formal consultation rights and periodic strikes (Western and Rosenfeld, 2011; Wilmers, 2019). In this Taylorist system, macro-level income equality was bought by a hierarchical division of labor (Broughton, 2014; Winant, 2021).

By the 1980s, this system had disintegrated (Kochan et al., 1994). Inequality researchers have since been rummaging through the rubble trying to identify the pillars that supported high wages at blue collar, frontline jobs. Product market niches and protectionism allow rent-sharing (Alderson and Nielsen, 2002; Dencker and Fang, 2016; Wilmers, 2018). Large, vertically integrated firms (Weil, 2014; Cobb and Lin, 2017) and insulation from aggressive financial owners (Nelson, 2023) boost pay for low-wage workers. A strong supply of skilled labor limits the college wage premium (Goldin and Katz, 2008; Liu and Grusky, 2013), and unions compress wages to the benefit of workers in the middle and bottom of the earnings distribution (Western and Rosenfeld, 2011; Farber et al., 2021). All of these macro-institutional and labor market-wide arrangements were critical supports for the old Taylorist dispensation.

Yet this research program, directed at labor market, product market and financial market contexts, stops at the factory gates and at the superstore's sliding doors. It neglects the organization of work and task allocation that were central to the old Taylorist system. In contrast, in labor economics, the decline in demand for routine work tasks has been a key explanation for rising inequality and stagnant wage growth (Acemoglu and Autor, 2011).

Moreover, organizational research has documented that beyond economy-wide changes in task demand, organization- and job-specific task allocations matter for pay (Wilmers, 2020), productivity (Ranganathan, 2023), job performance (Hackman and Oldham, 1976; Oldham and Hackman, 2010), and skill development (Anthony, 2021). This research suggests that attention to the organization of work can clarify the bases for high-paying, frontline jobs.

However, while labor economics research on wage effects of tasks has remained at the level of macro changes across occupations, the organizational literature has been confined to qualitative or industry-specific case studies and has rarely linked organization-specific task allocation directly to workers' pay (for exceptions to the latter see Osterman (2006), Wilmers (2020) and Chown (2020)). In this paper, we pull together insights from organizational studies to analyze the work organization of high-paying jobs. To do so, we first outline a theory disentangling distinct task-based strategies through which employers productively pay high wages, drawing on Perrow's classic (1967) taxonomy of work tasks. Organization-specific reallocations of work tasks can provide pay premiums by increasing worker autonomy and motivating efficiency wages; by providing unanalyzable, tacit human capital through on-the-job learning; or by activating and signaling existing human capital. We then develop a novel empirical approach to study the earnings effects of work organization by conducting the first ever merge of job descriptions to linked employer-employee earnings data. By matching new hires to the job posts they were hired under, we compare workers hired into the same employer and the same occupation, but under more complex or higher discretion task allocations. This lets us study earnings effects of changing work organization within jobs, and disentangle task-driven earnings increases that reflect true earnings premiums from those attributable to changing worker selection.

Our approach departs from two prominent theories of pay-setting. In standard labor economics models of competitive market wage-setting, workers' pay is a function of labor market-wide supply and demand for skill (Goldin and Katz, 2008). In these models, the specific tasks performed by a given worker, and therefore any single firm's organization of work, matters less than the overall demand for that worker's human capital (Autor and Dorn, 2013). In this theory, tasks matter, but only as the labor market-wide composition of tasks is modified by technological change, and therefore affects aggregate labor demand. Workers gain human capital through formal education and the value of that human capital is then determined through a competitive labor market.

An alternative perspective on pay-setting emphasizes that labor markets are imperfectly competitive, so bargaining power and labor market institutions distort the wage levels that would be expected from a competitive market. Research in this tradition studies effects of worker collective action via labor unions (Western and Rosenfeld, 2011); product market power that allows dominant firms to glean extra profits to share with workers (Wilmers, 2018); and fairness norms that compress pay within firms (Cobb, 2016). But work tasks are typically seen as epiphenomenal to the process of bargaining over rents: relational power, rather than objective task performance, determines the wage bargain struck (Tomaskovic-Devey and Avent-Holt, 2019; Rosenfeld, 2021).

Our task-driven organizational theory of wage setting specifies reasons that organizations might increase wages even absent workers' institutional power and over and above labor market-wide supply and demand dynamics. The productive completion of complex and high-autonomy tasks actually requires compensating workers for new and newly signaled human capital and for their effective exercise of discretion. Our approach thus reveals ways that work organization might actually support higher pay, rather than simply raise employee satisfaction (Hackman and Oldham, 1976) or cut costs (Braverman, 1974).

Indeed, we conduct this study in a period, 2014 to 2022, of rapid pay increases across the earnings distribution; a sustained period of egalitarian growth unprecedented since the 1970s (Aeppli and Wilmers, 2022). Unlike research on work organization undertaken during earlier decades, marked by rising inequality, the labor market context we study is one of low unemployment and strong demand for non-college workers. This external pressure could commit workers to more costly but higher productivity work organization. The findings in our study thus offer relevant lessons for employers newly grappling with high wages for frontline workers: what changes in job design can support higher pay?

2 Work Organization as a Determinant of Job Quality

A large literature on determinants of high-paying jobs canvasses union effects, rent-sharing, and fairness concerns. This work neglects the actual work tasks done in a job. Yet insofar as inequality research has studied wage effects of work tasks, it has largely focused on how shifting tasks composition of work affects labor market-wide demand for skill (Autor and Handel, 2013; Liu and Grusky, 2013). These market-wide demand effects cannot explain why some jobs pay particularly high wages, over and above what would be expected from the credentials and prior work experience of a given worker.

Moreover, organizational research has long emphasized that work organization is not strictly determined by immutable, labor market-wide technological imperatives. Work organization is a function of managerial strategies of control (Noble, 1978); decentralized job crafting and assembling (Wrzesniewski and Dutton, 2001; Cohen, 2013); shifting borders of professional jurisdictions (Chown, 2020); interdependencies in job structure (Hasan et al., 2015); managerial value commitments (Ton, 2014) and policy decisions (Fernandez, 2001; Autor et al., 2002); mental models of occupations (Barley, 1996); and the gendering of tasks (Chan and Anteby, 2016; Feldberg, 2022). Indeed, the allocation of tasks varies across and within organizations for reasons ranging from broad commitments to high-performance work systems (Handel and Levine, 2004) or structured management practices (Bender et al., 2018), to negotiated, dynamic allocations of tasks that vary even within similar jobs in the same organization (Anthony, 2021). These considerations mean that even conditional on some frontier production technology, firms can vary substantially in the details of their work organization (Beckman and Burton, 2008).

Abstracting away from the details of work organization, in favor of broad generalizations about demand for tasks and occupations—weak earnings growth for production workers;

strong growth for professionals—could therefore miss a key basis of pay differences across workers. Consistent with this, a growing research literature spanning management, economics and sociology has studied how the organization of work affects pay and inequality. Implementing high performance work practices, like teamwork and job rotation, is associated with increased pay (Osterman, 2006). Structured management practices, like lean techniques and production target accountability, are associated with higher productivity and higher pay (Bender et al., 2018; Bloom et al., 2021). Multi-tasking and devolution of management functions increases pay for skilled workers (Caroli and Van Reenen, 2001). Jobs that allow some protected turf are associated with higher pay, relative to those that are redundant with co-workers' jobs (Wilmers, 2020). Together, these findings suggest that some types of work organization could provide higher paying jobs for frontline workers.

Yet, notwithstanding this cross-disciplinary evidence, the possible link between work organization and higher-paying jobs faces two challenges. First, even when task changes appear to raise pay, it may do so particularly for higher-skilled workers (Caroli and Van Reenen, 2001). In one careful case study, job enlargement and intensification actually lowered pay for jobs at the bottom of a plant's pay distribution (Fernandez, 2001). Moreover, upgraded jobs can change the types of workers selecting into a given job, such that previously accessible jobs are filled by more-credentialed workers (Bender et al., 2018; Modestino et al., 2019). Perhaps a retail store assigns some inventory and merchandising responsibilities to its entry-level clerk position and accordingly raises its starting wage. That job might now attract and employ workers who have some retail experience or have completed their high school degree, rather than workers with less than a high school degree. In this case, redesigning the job to be more complex or higher autonomy could raise pay for a given *job*, but actually exacerbate inequality by reducing opportunities for the lowest-wage *workers*.

Second, even aside from shifting worker selection, the specific mechanisms linking task changes and higher pay are unclear. Some studies attribute the positive wage effects of alternative work organization to on-the-job learning and skill development (Lynch, 1992).

Others point broadly to increased bargaining power associated with higher worker discretion (Osterman, 2006; Wilmers, 2020). Without mapping the specific task types through which work organization affects wages, it is difficult to determine which kinds of job redesigns will be relevant and effective across multiple occupations and industries. Indeed, the extant research has typically started from assessing some specific management trend—like high-involvement work, lean production or job enlargement—rather than systematically outlining why different organizations of work can shape pay. What features of work tasks motivate employers to deviate from wages set in market-wide supply and demand?

In the following, we first address this second, theoretical question about work organization effects on pay. We then address the first, empirical selection issue, by comparing our novel research design to prior projects studying work task effects on pay.

3 Exceptions, Analyzability and Pay Premiums

We start from Perrow (1967)'s taxonomy of work organization, in which a task is either routine or nonroutine (few or many exceptions) and problems that emerge in task performance are either analyzable or unanalyzable. This parsimonious framework captures four quadrants of task types arrayed by two dimensions, which we depict in Figure 1. First, analyzability determines the extent to which a task's problems are amenable to systematic codification. When a task is not analyzable, its effective performance is less teachable and less supervisable. This means tasks must be learned through doing (craft: few exceptions, unanalyzable) or, when there are too many exceptions for know-how, an employer must rely on well-incentivized problem solving on the part of a worker (nonroutine: many exceptions, unanalyzable). Second, the number of exceptions determines the extent of routinization of a task. When tasks are analyzable but involve many exceptions, they lend themselves to formal training and education (engineering: many exceptions, analyzable). When tasks involve few exceptions and are analyzable, work performance is routine and well-ensured by clear work procedures and supervisory direction (routine: few exceptions, analyzable).

[Figure 1 about here.]

While these quadrants define ideal types, and harbor fuzzy borders, they usefully cross the key dimension in labor economics research on tasks, human capital and wages—task complexity and routinization (Autor and Dorn, 2013)—with the sociological emphasis on forms of control and power in the workplace—discretion and supervision (Braverman, 1974; Edwards, 1979). These dimensions of human capital activation and autonomy provide distinct possibilities for wage premiums for frontline workers. As employers change tasks they shift jobs along these dimensions, and their incentives to provide wage premiums intensify or weaken. We elaborate these possibilities for each task type in the following, and summarize our framework in Table 1.

[Table 1 about here.]

The Taylorist system was built around routine tasks and analyzable problems. These tasks are relatively simple and easily supervised. Jobs composed of routine tasks typically pay market rates and can withstand high turnover (Kuhn and Yu, 2021). Insofar as wage premiums exist for these tasks, they are a compensating differential for intensified work. As Taylor put it, "all you will have to do is to turn out a fair day's work and you can earn better wages than you have been earning" (quoted in Braverman (1974, p. 95)). Insofar as employers pay high wages for routine tasks, it is as part of a wage-effort bargain (Behrend, 1957), in which the disamenity of intensified work receives recompense. As noted above, the labor market institutions of the post-War period—industrial unions, formal collective bargaining, fixed pay scales, grievance procedures (Baron et al., 1986; Massenkoff and Wilmers, 2023; Jacoby, 2004)—governed wage-effort bargains without challenging managerial control over the direction of work (Lichtenstein, 1997). Absent these institutional supports, routine tasks provide little basis for wage premiums.

On the other extreme are tasks with many exceptions, in which problems are essentially unanalyzable. These tasks involve high autonomy and discretion. They present an opportunity in which an employer, who cannot directly supervise task performance against some predetermined standard, will offer an efficiency wage to incentivize effective task performance (Krueger and Summers, 1988). Here the classic ambiguities of organic firms (Burns and Stalker, 1961) and normative management (Barley and Kunda, 1992) emerge as employers cannot use standard monitoring, direct control or bureaucratic rules. Instead, employers pay workers above the level implied by their outside option in hopes of directing worker discretion toward effective task completion. While prior organization-level evidence on autonomy and pay premiums is thin, more decentralized firms pay more (Caroli and Van Reenen, 2001) and the dynamic nature of flexible specialization has been predicted to support better jobs (Piore and Sabel, 1984).

In routine, easily supervised tasks, there are few exceptions and problems are analyzable. In high autonomy tasks, there are many exceptions and tasks are unanalyzable. Beyond these matched categories, the Perrow framework sheds light on two different types of complex tasks in which human capital is critical. One off-diagonal is engineering tasks: nonroutine tasks that are amenable to analysis, as in technical responsibilities. Because formal knowledge is critical for these tasks, they are typically learned off the job through formal schooling. These tasks command a wage premium largely because they typically require formal training (Mincer, 1974) and performing them can signal skill to other employers (Galperin et al., 2020). But shifting toward these technical tasks, in which off-the-job training is typically necessary, is likely to require selecting for higher human capital workers. Note that if these engineering style tasks deliver higher earnings only via selection, then they would provide no pay premium.

The final category is tasks with relatively few exceptions that are unanalyzable. Perrow characterizes these as craft tasks. These tasks are complex, but typically not professional/technical (they cannot be analyzed) and therefore are usually learned through on-the-job work experience (Polanyi, 2009; Beane, 2019). These tasks support higher wages because workers learn know-how by performing them. For example, Crozier's cigarette factory maintenance workers derive positional power from the knowledge they have learned in repairing production machinery (Crozier, 1963). In this case, employers pay higher wages than expected from worker prior experience and education because workers are actually learning something on the job that could be in demand by other employers (Acemoglu and Pischke, 1998). These complex tasks, learned on the job, are more likely than complex tasks learned off-the-job to provide a wage premium without shifting worker selection.

This task-based schema organizes a series of mechanisms through which a given firm's work organization can undergird job-specific deviations from competitive market pay. Note that these are all different from both the standard competitive labor markets account—in which time-invariant worker skill is the key driver of wages—and the standard organizational account—in which relational power and bargaining over surplus determine pay. Tasks matter, as in the labor economics view, but do so through organizational channels of signaling existing human capital, on-the-job-learning and efficiency wages. Likewise, organizations interrupt competitive markets, as in the organizational view, but they do so in a more determinate way, governed by specific organizations of work, as compared to pure relational conflict theories of pay-setting (Tilly, 1998; Hultin and Szulkin, 1999).

Consistent with the job design literature cited above, organizations and indeed specific jobs can shift across these poles of work organization and task types. For example, when programming tasks on lathes are shifted from engineers to machinists, machinists take on more complex, technical tasks (Kelley, 1990). Similarly, when banks give tellers some responsibility for loan assessments, this adds a new, relatively technical task to the clerical tasks that they otherwise perform (Fitzgerald, 2006). Firms can also add tasks that are complex but non-technical and typically learned on the job. For example, adding care coordination to medical administrative assistants' work (Kottek et al., 2021) or giving maintenance and process improvement responsibilities to production line workers (Helper and Kuan, 2017; Adler et al., 1999). All of these examples of job enlargement (Herzberg, 1968) are the mirror opposite of deskilling and can boost pay through learning and signaling effects.

In other cases, firms can shift frontline jobs toward increasing autonomy and discretion, as in reducing the use of scripts in call centers (Rafaeli et al., 2008) or the use of the Andon cord in the Toyota Production System (Womack et al., 1991). These responsibilities involve dealing with exceptions in a setting in which problems are not fully analyzable. The resulting autonomy and discretion allotted to frontline workers gives employers a reason to pay efficiency wages. For example, Walmart has recently conducted an ambitious program of cross-training and devolution of decision making to retail associates (Wartzman, 2022). At the same time, the company has substantially increased hourly pay, even relative to other retailers also facing tight labor markets. For employers, these shifts toward frontline autonomy can bring higher productivity, but at the cost of increased pay.

4 Studying Work Organization

This theory of task effects on pay premiums, while including mechanisms consistent with substantial case study work, has not been previously tested. Indeed, the research infrastructure used to study tasks makes large-scale tests of any organizational theory of work tasks challenging. Before turning to the details of our research design, we discuss why the three main approaches to studying tasks—occupational task coding, establishment-level surveys, and firm- or industry-specific administrative data—are inadequate to testing the theory proposed above.

The standard approach to studying tasks uses Dictionary of Occupation Titles or O*NET data to categorize occupations according to their task content (Autor and Handel, 2013; Liu and Grusky, 2013). This work cannot measure the substantial task heterogeneity within occupations emphasized in recent work (Martin-Caughey, 2021). It therefore misses the key margin of employer decision making about job design: a health center would rarely choose whether to hire a doctor or a community health worker (CHA), but the center could have substantial leeway in determining which tasks should define the CHA's responsibilities (Kellogg, 2014). Likewise, a manufacturing facility will necessarily employ both engineers and assemblers, but might choose exactly how much engagement assemblers, rather than engineers, have in solving unexpected production problems (Bechky, 2003). The most actionable test of our theory would track how work organization varies within occupations.

The standard way of capturing this within-occupation variation has been to field surveys of managers, asking about establishment-specific work organization. Researchers have used this method to study work organization systems like high-involvement work practices (Osterman, 2006), joint-decision making (Black and Lynch, 2001), structured management practices (Bloom and Van Reenen, 2007), job design and enhancement initiatives (Carpini et al., 2017), and cross-functional and team-based work organization (Kalev, 2009). These surveys have been revealing, but they assume that work organization is constant within establishments (or varies only within broad categories of jobs, like white collar vs. blue collar). This prevents them from capturing details of task composition, which are inherently job- and occupation-specific. This approach also struggles to adjust for worker sorting: a cross-sectional establishment-level survey cannot rigorously distinguish apparent pay effects of work organization from changing worker selection.

Progress has recently been made on these issues by organizational scholars using firm- and industry-level administrative task data (Fernandez, 2001; Wilmers, 2020; Chown, 2020; Ranganathan, 2023). These projects can control for shifting worker selection and study within-organization variation in tasks. However, the task effects identified in these studies are essentially case studies of an industry or firm, so it is unclear whether the mechanisms identified generalize across occupations and industries. Moreover, workers cannot typically be tracked as they move across employers or in and out of a relevant industry. This means that studying selection (rather than simply controlling for it) is challenging.

In contrast to these approaches, we implement a novel research design that allows us to observe detailed information about task allocation, from job postings, and worker earnings, from labor market-wide administrative records. This design brings the granularity missing from occupation-level analyses; addresses the selection and job specificity problems in establishment surveys; and provides generalizability across occupations and industries that is impossible in case studies. To match new hires to the job posts under which they were hired, we merge a panel of job descriptions to linked employer-employee earnings data. This links high-quality, worker-side selection data to a specific task set that the worker is hired to do. It also allows a tight comparison between workers hired into the same firm and the same occupation, but with different task allocations.

In the following, we describe the substantial data infrastructure work necessary to develop this research design. But, first we note the key drawback of our approach: we do not have direct measures of workers' tasks, but only what is advertised by employers. If employers omit important responsibilities or distort the content of work, that introduces noise into our measure of tasks. We discuss this issue further below, but we see this as a limitation in our project that is counterbalanced by gains in granularity, tight comparisons, selection controls and generalizability.

5 Data and Measures

We study how work organization affects pay by comparing the earnings of similar workers in similar positions with different task allocations. We link U.S. administrative earnings records of workers starting jobs to the tasks appearing in online job posts at the same time and in the same employer, occupation and commuting zone. We then measure the task complexity and autonomy of each Burning Glass (BG) job post through an original survey asking respondents to rate job tasks common in their occupational group. We follow individual worker earnings outcomes by merging these survey-coded BG data with longitudinally-linked worker and employer data from the U.S. Census Longitudinal Household-Employer Dynamics (LEHD) data and the American Community Survey (ACS). This is the first study to our knowledge to link the rich task information from job posts to administrative earnings data. For a wide variety of employers, workers, and occupations, we track how changes in tasks within employer-by-occupation jobs affect workers' earnings.

5.1 Burning Glass Job Posts

The BG job post data draws from over 40,000 online job boards and company websites, removes duplicate posts, and parses the resulting data into a standardized database (Deming and Kahn, 2018; Hershbein and Kahn, 2018; Wilmers and Zhang, 2022). Although online job posts do not capture all job openings, a recent validation by Acemoglu et al. (2022) finds that the BG data closely align with trends observed in overall U.S. job vacancies. Earlier work suggests that 60-70% of U.S. private sector job openings in recent years have been posted online (Carnevale et al., 2014), although this rate has likely increased over the intervening decade.

To identify the tasks described by each job post, we use BG's task coding of posting text describing job tasks and responsibilities. BG identifies 16,128 unique task requirements ranging from technical activities such as *coding in Python* to physical work such as *lifting heavy objects* to generic cognitive requirements like *analytical thinking*. This BG task typology is used consistently throughout our 2010-2021 study period.

To identify these task requirements in job post texts, BG uses natural language processing classifiers comprising thousands of rules and keywords, including numerous neighborhood and negation rules. These rules allow the algorithm to distinguish homonyms such as, for example, 'kitchen chef' and the 'chef' software tool. More dramatically, the algorithm recodes similar concepts appearing across the job post texts into 16,128 tasks, confirmed by hand review. This approach attempts to consolidate effective synonyms, for example, such as 'customer engagement' and 'customer relations' into the same BG task. The distribution of appearances of unique tasks has a substantial positive skew, i.e., a long right tail.

After necessary exclusions, we obtain a sample of 168 million job posts each associated with 8 tasks on average, totaling 1.3 billion appearances of the 16,128 BG tasks. The largest sample exclusion relates to employer name, which appears in 73% of the 246 million posts with skill information (for a discussion of this, see Hershbein and Kahn, 2018). After the missing employer exclusions, 95% of BG posts include occupation and county information

(168/178 million). Occupation is coded from job titles and county is coded from the address of the employing workplace. Our resulting job post sample covers 99% of SOC occupations across all U.S. commuting zones over 12 years. Although the BG data over-sample more technical positions (Acemoglu et al., 2022), we discuss below how we weight our regressions to adjust our sample to U.S. national occupational and demographic proportions.

5.2 Survey to Classify Jobs Tasks on Dimensions of Work Organization

The BG processed job posts thus include rich information about work tasks. However, the thousands of discrete tasks appearing in the postings do not naturally correspond to the general conceptual framework discussed above and which we seek to test. As such, we field two novel surveys asking respondents to categorize job tasks into types of tasks. We then take the average of each job's task scores to measure the complexity and autonomy of that job, the main predictors needed to assess our theory of work organization and pay. We post our survey results and all code for this project at [TK post-review]. Appendix A provides technical details on the surveys.

In our surveys, respondents categorize a subset of BG task requirements on one of 2 dimensions characterized by 3 response options each. In one version of the survey, respondents are asked to map task complexity. With this question, we sought to translate the first Perrow dimension, delivering types of task complexity, into something interpretable by coders: engineering tasks are typically teachable and learned off the job; craft tasks are learned through experience and tacit knowledge. Specifically, respondents report whether a task is (i) complex and likely learned only off the job, (ii) complex and possible to be learned on the job, or (iii) non-complex or simple. Appendix A provides the full question text used to prompt these concepts.

In the second version of the survey, we map the degree of control or autonomy implied by a task, the second task dimension that we highlight above. Here we started with a simple contrast between high autonomy or discretion tasks and tasks that tend to be easily supervised and tightly controlled. However, looking through task lists, and examining respondent feedback in an initial pilot, we noted another mode of constraint, via relationships and interactions with others. This idea corresponds to customer (Sherman, 2011) and concertive (Barker, 1993) control studied widely in the sociology of work. For our final categorization, we asked respondents whether a task incorporates (i) high discretion, (ii) constraint through interaction with coworkers, clients, or customers, or (iii) high rules and/or supervision. This allows us to distinguish high discretion jobs from those that are constrained either by rules and tight supervision or by customer or coworker interaction.

In total, we collect 120,000 task-by-dimension response values: 30 responses for each of 2,000 tasks on both dimensions. We focus on only the most common 2,000 tasks that make up 93% of all task appearances in the BG the data. Appendix A provides a technical overview and an example of the complexity and autonomy surveys. Beyond standard attention check and participant screening, we assign respondents only to tasks that were most frequently in their main occupation; reduce noise by we collect 30 responses per task and per dimension; and train respondents interactively, providing instructions and feedback for categorizing tasks along the complexity and autonomy dimensions.

To transform the resulting task-level survey codings into job-post-level task allocation scores, we take the average of the task-level scores for each job post. Consider a post associated with tasks A through E. If 90% (27/30) of respondents chose discretion for task A, i.e. rather than bound by relationships or easily supervisable, and B, C, D, and E were analogously given 0%, 10%, 10%, and 40% discretion scores, respectively. Our discretion measure for that job would be $(90+0+10+10+40)/(5) = .33$. In this way, the 'discretion', 'relationships', and 'supervisable' scores for any job post will sum to 1 by design, as will that post's 'off-the-job', 'on-the-job', and 'simple' scores. Therefore, we use 4 variables as predictors: discretion and social constraint, which are compared to the supervisable reference category, and off-the-job complexity and on-the-job complexity, which are compared to the simple task reference category. Using the results of our survey covering the top 2,000 BG tasks, we code 93% of task appearances and achieve measures for 99% of jobs, giving us complexity and autonomy measures for 150 million BG job posts.

As an example of our task coding, we compare two line cook posts from 2014 and 2021, respectively, both posted in Eastern Pennsylvania by Sodexo, a large food service company. The 2014 post contains the tasks *cooking*, *food preparation*, *meal preparation*, and *physical abilities*, while the 2021 post has all four of these tasks as well as *building effective relationships*, *work area maintenance*, and *Hazard Analysis Critical Control Point (HACCP)*. These three tasks signal a rise in relationships and complex tasks. In terms of our scores, the job changes from 0.27 to 0.38 on-the-job complexity, 0.08 to 0.23 off-the-job complexity, and 0.1 to 0.21 bound by relationships. This example highlights how our scores are based on averages across skills in the job post, meaning that the pure addition of some skills can reduce scores as the prior listed skills theoretically receive less emphasis. This example also highlights how we allow tasks to have intermediate values on our work allocation dimensions: *food preparation* is 0.37 on-the-job, 0.05 off-the-job, 0.19 discretion, and 0.04 relationships, and therefore .58 simple and .77 easily supervisable, signaling different answers among respondents for this task.

To further illustrate the task codings, Figure 2 plots a random sample of BG tasks based on their average scores on the complexity and autonomy dimensions. In the upper panel, representing complexity, the origin contains simple, low-complexity tasks; the y-axis, labelled On-the-job, represents greater complexity tasks that can be learned on the job; and the x-axis, labelled Off-the-job, represents greater complexity tasks that respondents believed could not be learned on the job. Complex off-the-job tasks appear to mostly be associated with higher education. For example, 'maintaining supplies' has a relatively low on-the-job complexity score and a minimal off-the-job complexity score, 'freight flow' analysis is complex but can be learned on the job, and all respondents agreed that 'geometric dimensioning,' an engineering design system, could only be learned off the job. In general, we see most tasks sitting between on-the-job and off-the-job complexity. The overall wedge structure of the results plotted on these axes suggests a general linear tendency from simple tasks, through on-the-job complexity, and to off-the-job complexity: it was rare that survey respondents were split between whether a task was simple or complex off-the-job.

[Figure 2 about here.]

The bottom panel of Figure 2 plots BG tasks based on their average scores on the autonomy dimension. The origin represents rule-bound or easily supervised tasks that are likely constrained by detailed rules; the y-axis, labeled Relational, represents tasks in which worker autonomy is constrained by interaction with other people inside or outside the organization; and the x-axis, labelled Discretion, reflects tasks that require high autonomy and independent decision-making. For example, 'x-ray' and the 'Omniture' business software score as easily supervised tasks; 'client base retention' and 'customer referrals' are highly relational; 'creative problem solving' is maxed out on discretion; and tasks like 'clinical development' and 'performance analysis' rank relatively low on relational but intermediate on discretion. In general, most tasks sit in between the origin and full discretion, representing relatively fewer relational constraint survey responses.

5.3 LEHD-ACS: Bringing Occupation to Administrative Earnings Data

To complement the survey-coded BG task measures, we draw earnings and employment information from the confidential U.S. Census Longitudinal Employer-Household Dynamics (LEHD) data. The Census Bureau maintains the LEHD by integrating quarterly reports from employers to U.S. state unemployment insurance administrations (Graham et al., 2022). These data are combined with Internal Revenue Service and Social Security Administration records. The resulting longitudinally-linked data describe the quarterly earnings for 97% of U.S. private sector workers from 1996Q1 through 2022Q1. By 'earnings,' we refer to compensation broadly, rather than base pay rates narrowly construed: the LEHD covers salary, cash, commissions, bonuses, tips, awards, severance, vacation pay, standby pay, reimbursements, back pay, sick pay, and payments made under a deferred compensation, with some minor variation across states. We accessed these data through the Census Research Data Center program that grants non-Census researchers FBI Special Sworn Status to use confidential U.S. administrative data.

In Appendix B we describe a series of standard restrictions to the data: academic researchers receive a random sample of half of US states; we drop earnings quarters at less than half time minimum wage; and we require jobs to span at least 3 quarters, so that the second quarter is guaranteed to be a full quarter of earnings.

Note that unlike firm-specific data, the LEHD follow individual workers, even as they change employers over time. We use the LEHD data to construct two worker earnings outcome measures, plus four work history control variables. Our main outcome, *second quarter earnings*, requires no additional processing and is the first full employment quarter for a worker at a given employer-occupation job. To test effect persistence, we also predict worker earnings 4 years after job start. We measure this as the average of observed logged earnings 16 and 17 quarters after start. Observing this long-term outcome mechanically excludes workers who start after the fourth quarter of 2017, plus some workers who are not in the labor force in our covered states at the measurement time, though we compare the main and long-term samples in our Results section. We also use the LEHD earnings data to construct four work history human capital control variables, discussed in the Models section below.

Occupation is a critical aspect of our merging strategy, allowing comparisons of task allocation and pay within jobs. As LEHD contains no occupation information, we merge in the American Community Survey (ACS), a Census Bureau survey of over 5 million U.S. residents per year. The ACS also provides demographic measures including age, gender, race, and education. We link to the LEHD through a Census crosswalk based on Social Security Numbers.[1]

The ACS match cuts our LEHD job start sample from hundreds of millions to 6,624,000

[1] We also integrate responses from the 2008-2020 Current Population Survey (CPS). We recode all CPS variables to match their ACS equivalents. The CPS sample size is less than 2% of the ACS sample.

job starts.[2] Only LEHD job starters sampled by the ACS around their job start are included in our analysis. Specifically, we keep only the LEHD job starts for which we have an ACS response within an 8-quarter window before or after the job start quarter.

5.4 Merging the BG to the LEHD-ACS Data

We merge the BG and LEHD-ACS data on occupation, commuting zone, employer, and time. Our project is the first attempt to integrate these large datasets. For occupation, we recode the ACS "OCC" occupation code to 5-digit SOC used in the BG data. We do this using a crosswalk available through the Integrated Public Use Microdata Series website, which we standardize across the ACS survey years (Ruggles et al., 2020).

We also merge on commuting zone, coded from the county of the BG job post workplace county and the ACS reported workplace county. In rare instances in which the ACS workplace county is missing, we use the residence county of each worker in the year after their job start year as recorded in the worker-year-level LEHD Individual Characteristics Title 26 File, derived from Social Security Administration records (Vilhuber et al., 2014). Commuting zones were defined by the Census based on residence-to-work commuting flows from the 2000 Decennial Census.

Third, we require a match on employer name. Our goal is to maximize the power of combining the BG and LEHD data to study changes in task requirements within job positions over time. A general challenge here is that BG employer names are oriented towards employees seeking jobs, whereas the Census names are oriented towards administrative records. The BG data may be more disaggregated than the Census data because business units may post jobs under their own name with no reference to the controlling firm. Perhaps surprisingly, though, it is the administrative, rather than BG, data that introduce significant challenges. The Census Bureau business data include both a primary and secondary names for each *workplace*, where one of the names is often a workplace designation such as the city

[2] All sample size figures are rounded in this document according to Census rules where applicable.

in which the workplace operates, but both names may contain the business name as it would be conceived in the BG data. Overcoming these challenges to merge at the employer level requires 3 key steps described in Appendix C: (A) preparation, (B) fuzzy matching, and (C) cleaning the employer-occupation-(commuting zone) matched units. We process both databases similarly except where there are specific naming issues on one side.

Time is our fourth and final merge feature. A post 2 years after a job start, for example, would not seem to be a good measure of the tasks at that job, and averaging tasks over time would undermine our ability to compare within job positions over time. Our first step is to aggregate posts to the employer-(commuting zone)-occupation-quarter level by averaging the four work organizations scores across posts within these units. Most of our LEHD-ACS-BG matches use job posts in the quarter immediately before the job start quarter. If this does not exist, however, we allow for a match up to 3 quarters before job start or in the same quarter as the job start (we specifically prefer -1, -2, 0, -3 ,-4, where -1 means the post was in the quarter before the job start). We present robustness results preferring posts in the same quarter as the job start. To be clear, incorporating time in this way allows for multiple workers to be hired under the same job post so long as the start in the same occupation, employer, and commuting zone within the acceptable time frame. Conversely, each a job starts are associated with the average post scores in the matching employer-(commuting zone)-occupation-quarter unit.

5.5 Final Sample and Subsample Features

Even with strict employer name match requirements discussed in Appendix C, we match 57% (86/150 million) of BG job posts on employer name. This moves us from our original sample of 6.6 million ACS-matched LEHD job starts to 3.8 million name-matched job starts, coincidentally also 57%. Many smaller firms simply do not have an ACS measure in our study period,. After requiring work history measures and dropping all employer-occupation fixed effect singletons per Census results disclosure rules, discussed below, we have 611,000 employer-occupation-(commuting zone)-time-matched job starts in our main analytic sample. This sample comprises 570,000 unique workers and 23,500 employers.

Though we are committed to the generality of our complexity and autonomy mechanisms across labor markets, our project is motivated by the question of how to generate wage-growth for the workers left behind by changes in advanced economies over the last 20 years. To test for heterogeneity across broad occupation groups, and determine whether the pattern we see here holds for workers entering non-managerial/professional jobs, we additionally split our main sample into groups based on SOC code: managerial/professional (SOC groups 11-29) and non-managerial/professional (SOC groups 31-53). Though we also provide all results for the managerial/professional sample in Appendix Table A.3, we focus in our Results section on the main sample and the non-managerial/professional sample, comprising 293,000 job starts by 277,000 workers at 15,000 employers.

To maintain a nationally representative sample, we use ACS characteristics to re-weight the resulting merged data to have similar observable characteristics to the national ACS sample. We describe these weights in detail in Appendix D. We compare descriptive statistics between our matched data and the ACS survey data, discussed further in Appendix E and Table A.1. Although our unweighted sample is disproportionately higher-educated and white collar, the reweighted sample is similar to national figures on these and other demographic attributes.

6 Regression Models

We use fixed effect regression models to study the relationship between work organization - job complexity and autonomy - and pay. Our goal is to make tight comparisons within employer-by-occupation job positions, controlling for worker human capital and local labor market conditions. The full form of these models is:

$$y_{i,j} = \mathbf{u_j'}\beta + \alpha_{SOC5\wedge Employer} + \alpha_{Start_Q \wedge CZ \wedge SOC2} + \mathbf{x_{i,j}'}\lambda + \epsilon_{i,j} \qquad (1)$$

where $y_{i,j}$ measures second quarter earnings for worker i at job j. Our main predictors of interest, $\mathbf{u'_j}$, represent our 4 work organization scores: on-the-job complexity versus off-the-job complexity, which are compared with the reference category of simple, and discretion versus bound by relationships, which are compared with the reference category of easily supervisable. Below we discuss how $\alpha_{SOC5 \wedge Employer}$ represents fixed effects for worker job positions; $\alpha_{Start_Q \wedge CZ \wedge SOC2}$ captures local labor market conditions; and $\mathbf{x'_{i,j}}$ measures a series of human capital controls. The residual term $\epsilon_{i,j}$ reflects pay variance not explained by these predictors. We weight our regressions using our ACS-derived job-start-level factor and we cluster standard errors at the firm level to match the level at which broad work organization strategy may be assigned by managers.

The second factor in equation 1, $\alpha_{SOC5 \wedge Employer}$, represents fixed effects for worker job positions. Including this term restricts our comparison to be within the same occupation at the same employer. These comparisons may be made over time or across workplaces in the same employer. This term controls for unmeasured, time-invariant features of job positions such as a legacy of unionization, an organizational ethos dedicated to greater investment in workers, or a specific human resource culture determining job post text, which may create a spurious correlation between our measures of work organization and pay. These 'fully interacted' fixed effects provide a stronger test than including separate employer and occupation fixed effects.

The third factor in equation 1, $\alpha_{Start_Q \wedge CZ \wedge SOC2}$, captures local labor market conditions. This term restricts us to making comparisons within time-(commuting zone)-(industrial group) units. We here want to control away, for example, labor market tightening simultaneously raising complexity and/or autonomy as well as pay. Including these controls is also important because our core estimates are in part based on geographic variation in task allocation and pay within the same employer: we do not want unmeasured spatiotemporal factors driving our estimates of the relationship of our work organization measures and pay.

$\mathbf{x'_{i,j}}$ measures human capital controls from the ACS and LEHD. Workers with higher education and experience will tend to earn more and likely perform more complex and autonomous tasks. Other demographic factors may also bias manager perception of each worker's ability to complete more complex and autonomous tasks as well as the appropriate level of pay, so we want to make comparisons within demographic groups. Our ACS-based demographic features include race as White, Black, Hispanic non-Black, and Asian and other; binary gender; education as high school or less, some college or an associate's degree, a four-year-college degree, or a graduate degree; and age in years plus age squared to incorporate non-linearity.

Workers who signal higher productivity through higher pre-job-start pay will, all things equal, likely earn more on average and have more complex or autonomous jobs. For this reason, $\mathbf{x}'_{\mathbf{i,j}}$ also includes 4 work history variables that we create from the LEHD earnings records, a key strength of our approach using longitudinally-linked data. We record all earnings for each worker in the 24-quarter time window prior to their job start quarter.[3] To capture key work history human capital factors, we record (i) mean earnings, categorized into 10 deciles for non-linearity, reflecting prior signaled productivity and activated bargaining power; (ii) the slope on those earnings over observed quarters, categorized into ten deciles for non-linearity, reflecting prior earnings growth; (iii) a binary measure of whether the worker joined the labor market in the last 4 years, reflecting labor force choices beyond age and education; and, (iv) a binary measure of whether the worker history data include a zero-earnings quarter immediately preceding the job start quarter, possibly reflecting education, entrepreneurship, or unemployment prior to starting their new job. We exclude from our sample the small proportion of workers who do not have at least 1 quarter of work history, meaning that we systematically workers' first jobs.

We run 3 additional analyses described in the Results section: (1) a selection analysis of how increasing job complexity and autonomy impacts the human capital composition of workers taking those jobs; (2) an analysis of the long-term worker earnings outcomes of

[3]This series is never left-censored because the LEHD data start before 2004.

allocating more complex and autonomous tasks; and (3) a description of the characteristics of employers that tend to offer more complex and/or autonomous frontline jobs. A series of appendix sections and tables include additional descriptions and tests raised by our main analyses.

7 Main Results

7.1 Earnings Effects of Changing Work Organization

How does work organization affect earnings? Table 2, Panel A, Model 1 shows an unsaturated model, in which we predict starting earnings for all workers in our sample using task characteristics and calendar quarter fixed effects. Complex off-the-job and on-the-job and high discretion tasks are all strongly associated with higher pay. Indeed, these task categories alone have an R^2 of 0.20, or around two thirds of a standard Mincer human capital regression. Model 2 shows that the predictive power of work tasks is not simply a function of occupation: strong effects persist even after controlling for detailed, 5-digit SOC occupations (we group some SOC occupations when they are represented by only 1 group in the 500 category Census ACS OCC occupation codes). Notwithstanding the noise involved in translating work tasks into online job postings, and survey coding those tasks, variation in earnings strongly tracks work tasks. We provide a fuller set of descriptive models in Appendix F that show how controls like industry, education and firm size relate to the effects of our novel task measures.

[Table 2 about here.]

However, differences in tasks across jobs could be largely a function of working at different types of firms, spanning different industries, production technologies and market contexts. A retail clerk at Dollar General may have different tasks than one at Trader Joe's, but there are many other contributors—business strategy, market segment, ownership structure—besides tasks that could drive wage differences between those jobs. Model 3 adds occupation by firm fixed effects, which attenuates task earnings effects by between one half and two thirds. Substantial cross-firm variation in occupations' task composition indeed track earnings. But even looking at task changes within a given firm by occupation, complex and high discretion tasks strongly predict earnings.

This within-job variation could be also a function of differences between skill demand or regulation between local labor markets, that track both earnings and task composition. For example, a Dollar General in Cambridge, MA will pay more due to labor market tightness and minimum wage rules than one in Hurtsboro, AL. To adjust for these differences in local labor market dynamics, Model 4 adds calendar quarter by commuting zone by broad occupation fixed effects.

Model 4 shows that adjusting for time-varying, occupation-specific local labor market conditions, and comparing within the same firm by occupation job, more complex and higher autonomy tasks continue to pay more. A large shift—when a job goes from no complex, off-the-job tasks to one that is entirely complex—is associated with an 11% earnings increase (from exponentiating the logged earnings model). An analogous shift toward one-the-job complex tasks brings a similar earnings increase. The effect of a shift toward high discretion or autonomy tasks delivers a slightly smaller 7.5% earnings increase. Shifting work organization thus delivers higher paying jobs.

However, two caveats are in order. First, the magnitudes estimated above consider transformative changes in work organization, of a type that is unrealistic for most employers. A more modest 20 percentage point change in task dimensions—equivalent to changing a single task listed in a 5-task job posting—brings a 2% earnings increase for complex tasks and a 1.5% increase for high autonomy tasks. These realistic effects are smaller, but still meaningful real earnings increases. For comparison, from the 1970s through the early 1990s, real earnings were entirely stagnant for blue collar workers (Massenkoff and Wilmers, 2023). Task reallocation offers a rare source of quantitatively meaningful earnings growth.

Second, these estimates do not consider shifting worker selection. While they summarize within-job earnings differences associated with different task allocations, changes in task allocation are likely to elicit different kinds of worker applicants and hires. Model 5 in Table 2, Panel A, adds a detailed set of controls for human capital, described above: prior earnings, prior earnings trajectory, education, age, gender, race and a pre-hire non-employment spell. Adjusting for these attributes allows us to distinguish between earnings changes due to hiring different workers from true pay premiums. Model 5 shows that the importance of these controls, and therefore of shifting worker selection, varies across task types. A full half of the apparent earnings effect of shifting toward more complex, off-the-job tasks is accounted for by adjustments for worker characteristics. In contrast other task changes are less affected by selection: shifting toward complex on-the-job tasks is attenuated by around one third by selection, while shifting toward high discretion tasks attenuates by less than 10%. These patterns in effects and proportion explained by human capital, persist when, rather than preferring the quarter prior to the job start quarter, we prefer BG information from the same quarter at job start (see Table A.4).

In Table 3 we study exactly how worker selection changes with these task shifts, by running similar within-job task change models and swapping out quarterly earnings for worker characteristics as dependent variables. When a job shifts toward more complex, technical tasks, it draws workers who are slightly more likely to have a college degree (3 percentage point higher college share) and who have substantially higher pay at their prior job (12.6%). It also draws hires who are 3 percentage points less likely to be female and 2.5 percentage points less likely to be Black or Hispanic: workers otherwise disadvantaged on the labor market are less likely to get jobs that newly involve more technical tasks. Shifts toward complex on-the-job tasks bring similar but smaller changes in prior earnings and racial composition. On-the-job tasks more strongly select against women—the female share of hires drops by 6 percentage points—perhaps due to women's exclusion from prior jobs that provide tacit knowledge and on-the-job learning. Across all task types, we see little change in selection of workers by prior non-employment, age or earnings trajectory.

[Table 3 about here.]

Shifts toward increased discretion, in contrast, show much smaller changes in selection, consistent with the earnings results in Table 2. More discretion brings little change in a job's gender or race composition and a small increase in college-educated hires. Most strikingly, there is no increase in prior earnings for hires for new hires into higher discretion jobs: this type of work organization shifts jobs toward giving new workers a larger bump relative to their prior employment than those same jobs gave to other workers before the increase in autonomy.

These results demonstrate that work organization that shifts jobs toward more complex and higher discretion tasks delivers higher earnings. It also shows that these effects persist after rich controls for worker characteristics. This means that these task changes bring organization-specific earnings effects, which cannot be accounted for by labor market-wide supply and demand for generic skills. Indeed, even adding complex, off-the-job tasks, which has the largest share explained by selection, still brings a 5% earnings increase over and above the selection change. This increase is consistent with workers either learning or newly applying and signaling existing skills.

An outstanding question is whether these benefits really accrue in frontline jobs, or whether they essentially only matter for white collar managerial and professional workers. Panel B of Table 2 shows the same models in Panel A, but restricting to the subset of non-managerial and professional jobs in our sample. Overall, panel B shows that improved task allocation also raises earnings for frontline jobs. The main difference is that complex, off-the-job tasks deliver a consistently smaller earnings premium across models for frontline jobs compared to the full sample. Figure 3 compares the magnitude of within-job effects visually and confirms that the main difference across samples lies in the weaker role of complex, off-the-job tasks for non-managerial/professional jobs. Even in frontline jobs, improvements in work organization—particularly those involving increasing autonomy or adding more complex but tacit knowledge tasks—delivers earnings premiums.

[Figure 3 about here.]

7.2 Long-term Effects of Changing Work Organization

While complexity and autonomy may raise initial pay, do they also predict long-term worker trajectories? To analyze this question, we construct a long-term earnings measure based on workers' average earnings four years after their job start. As mentioned above, this limits our analysis to a subset of 363,000 job starts that begin early enough in our panel that we can observe the job starter's earnings 4 years later. Table A.1 shows that this long-term sample is very similar to the overall sample in terms of demographic and job attributes. Note that we mitigate selection and attrition concerns by including all workers who remain employed 4 years out, regardless of whether they have persisted in the same job they were initially sampled under.

Models 1 and 2 in Table 4 correspond to within-job task change earnings models analogous to those in Table 2 Models 4 and 5, predicting earnings 4 years after the job start, rather than starting earnings. For a same-sample comparison, we also re-estimate the short-term earnings results from Table 2 for this smaller sample.

[Table 4 about here.]

The results in Table 4 show that the immediate earnings benefits of a job shifting toward more complex tasks or higher discretion persist over the long-term. Workers hired into a more complex version of the same employer by occupation job are on average still making 6 (off-the-job) or 8 percent (on-the-job) higher earnings 4 years after hire than are very similar workers—with the same prior pay, demographics and education level—who were hired into a less complex version of the same job (Table 4, Model 2). The results are even larger for jobs that shift toward higher discretion: a similar worker hired into a higher discretion version of the same job makes 10% more 4 years out.

Indeed, after controlling for selection and worker characteristics, the point estimates for long-term effects are actually larger in the long run than the short-term benefits estimated in

Models 5 and 6. These results suggest that the earnings premiums associated with shifts in work organization actually push workers onto an alternative earnings trajectory that delivers growing benefits beyond the starting pay bump.

7.3 Which Employers Offer Higher Complexity and Autonomy Jobs?

To adjust for unobserved heterogeneity in jobs, our earnings models focus on tight comparisons among workers in the same employer by occupation positions. This shows that as jobs shift toward higher autonomy or more complex tasks, worker pay premiums increase. However, the estimated earnings benefits raise the question of which employers actually pursue higher-paying work organization in non-managerial/professional jobs. What types of firms provide these jobs? To provide some initial evidence on this question, we conclude with a descriptive analysis of the types of firms that disproportionately create more complex and higher autonomy frontline jobs.

To do this, we aggregate the data to the firm level. For this analysis, we match firms rather than specific job starts. So, we require firms to match by firm name and to have at least one occupation posted in the same commuting zone as an ACS worker start. We do not impose any time constraint on this match, which increases our sample of firms from 23,500 to 59,000. For the resulting matched firms, we then use all non-managerial/professional job posts, not restricting to those that match to a specific ACS worker start, in order to more accurately characterize the general task organization of the firms' frontline jobs. We then compare firms on administrative financial attributes drawn from the Longitudinal Business Database (LBD): employment, total annual pay, annual revenue, and profit (estimated as revenue minus labor costs). We also compare by firm age, firm type, industry and several LEHD-derived variables (earnings, earnings inequality). Table 5 presents the sample means for key variables in this sample, as well as the difference in average value between firms that are above versus below the median of each dimension of work organization.

[Table 5 about here.]

Higher off-the-job complexity employers are large with growing revenue but shrinking employment. This suggests rising productivity for these firms, which is already relatively high (measured roughly as revenue/worker). These firms also pay relatively high earnings overall, not just for frontline jobs, which would be consistent with the positive selection of workers into these more complex tasks discussed above. Employers with high off-the-job complexity for frontline workers are concentrated in the business services and health industries as C-corporations and non-profits. These industries have high technical requirements, echoing the examples of off-the-job complex tasks noted above. These employers are also relatively newer, consistent with the technical tasks that define off-the-job complexity.

Employers with high on-the-job complexity for frontline workers tend to be even larger and have both declining employment and declining revenue growth. Unlike off-the-job complexity employers, these firms are disproportionately older and in mature industries like manufacturing and retail. The tacit and craft knowledge involved in those industries provides opportunity for relatively complex task allocation to frontline workers. These firms (disproportionately C-corps) have high average pay and the largest revenue and profits per employee but face shrinking revenue and employment.

Finally, employers with higher discretion jobs are substantially younger and around one third smaller than average. These employers have higher pay and productivity, but are less profitable than average. They are also expanding both employment and revenue and increasing productivity over time. These young, fast-growing firms have a relatively higher labor share of total revenue, with above average payroll and below average revenue. They are also in labor-intensive industries like education and social services and business services. These firms, which create jobs with the autonomy tasks least marred by selection changes, pay well and are expanding.

Many of these differences across firms are attributable to industry differences. However, in Table A.5, we residualize by 2-digit industry, and overall find similar patterns. Note also that across these earnings-increasing forms of work organization, only C-corporations and non-profits are overrepresented. Frontline jobs in S-corps and partnerships are disproportionately composed of simple and easily supervised or rule-bound tasks.

This descriptive analysis characterizes the employer attributes associated with more complex and high autonomy assigned tasks to frontline workers. It shows that a broad range of firms experiment with this approach. But it also reveals key differences between firms that focus on one or another type of work organization. Higher autonomy tasks assigned to frontline workers are disproportionately present at fast growing, young firms.

8 Discussion

We motivate this study by asking whether there are alternative forms of work organization, beyond a Taylorist division of labor, that might support higher pay in frontline jobs. This inquiry builds on a long tradition of research that has grappled with providing managerial and organizational solutions to macro-level problems of pay stagnation, job quality, and alienation. Inspiring proposals such as high-involvement work systems (Osterman, 2006), training and learning organizations (Lynch, 1992), enabling bureaucracy (Adler and Borys, 1996), flexible specialization Piore and Sabel (1984), good jobs strategy (Ton, 2014), and job enrichment Herzberg (1968), have been difficult to study and test because of limited data tracking tasks across otherwise similar jobs for similar workers.

In this study, we focus on a common denominator of many of these proposals by studying work organization and the allocation of tasks specifically. Work organization can affect earnings either through human capital learning and signaling channels or through efficiency wage setting. We use Perrow's taxonomy of tasks to outline distinct types of tasks that could deliver pay premiums: high autonomy and discretion (many exceptions, unanalyzable), craft (few exceptions, unanalyzable), and engineering (many exceptions, analyzable). We then develop a new data infrastructure to test the earnings effects of tasks allocated along these lines, using tight comparisons among similar workers in the same jobs, but who are hired under different task allocations.

We find that work organization robustly predicts earnings. Even comparing within the same occupation at the same employer, and adjusting for local labor market dynamics, shifting toward more complex and higher autonomy tasks increases starting earnings. For complex tasks typically learned off the job through formal education, half of this earnings increase is attributable to shifting selection of workers into more complex jobs. But for tasks learned on-the-job, typically involving work experience and tacit knowledge, selection changes less, and for high-discretion or autonomy tasks, very little of the task-based earnings benefit is due to shifting worker selection. We also find that these benefits persist for non-managerial/professional, frontline workers and that the earnings advantages of more complex and higher autonomy tasks persist, and even increase, over at least four years following a job start. Improved work organization, and especially increased discretion, can deliver robust earnings benefits that are not explained by local labor market dynamics or shifting human capital. These effects are not temporary.

Finally, we make a preliminary attempt to characterize the employers who offer different types of tasks. We show systematic differences between employers harboring different types of frontline job task allocations. Employers with higher off-the-job complexity for frontline workers are larger, have growing productivity and are disproportionately in health and business services. On-the-job complexity firms are larger, older and in mature industries like retail and manufacturing. Finally, high discretion firms are younger, fast growing and have a high labor share of revenue. While these are only descriptive comparisons, they contextualize the tight, within-job comparisons we make in our earnings models.

That we cannot directly measure performed tasks is the key limitation of this study. Job posts are projections made by hiring managers. From a simple perspective, employers should seek to accurately define jobs to maximize candidate fit. However, the organizational literature suggests that hiring managers, due to generosity, apathy, or status pursuit, may incorporate more complex or autonomous tasks into job posts than are actually performed as a justification for or alongside higher pay, positively biasing our estimates even in the absence of changes in work organization. The extent of this bias, especially within employer by occupation job positions, is not known. If, on the other hand, employers tend to project higher discretion tasks than what actually is performed, and workers are paid the same regardless of this over-projection, we will underestimate our effects. While our within-position approach is not subject to hiring particularities at the employer, industry, or occupation level, we may still see differences across hiring managers within employers that bias our results. A related problem, present even with ideal task measures, is that the work organization factors that we measure may, after controls, predict pay because of incompletely controlled factors associated with tasks such as charisma, but separate from work organization. Studies randomizing task allocation in real jobs could address this concern, though that will forsake the breadth we obtain.

Unfortunately, there is currently no economy-wide, workplace-level data on actual work tasks that could be used as an alternative to job postings. The findings in our analysis, alongside other recent research on work tasks, therefore, gives warrant for further investment in task-related data collection. The Bureau of Labor Statistics American Time Use Survey, for example, uses a time diary instrument to ask detailed questions about respondents' leisure activities. However, it includes only a single category summarizing 'work'. Integrating direct questions about work tasks to a time use survey of that type could usefully address the problems involved in using job postings to measure tasks (Autor and Handel, 2013).

Notwithstanding this limitation, our study makes several contributions. First, we show that, across many firms and occupations, organization-level task allocation decisions affect earnings. This challenges both canonical labor economics models—which abstract away from organizational variation—and bargaining power approaches—which emphasize that positional and institutional power, rather than task performance determine pay. Within-labor market and within-occupation, we show substantial variation in task allocations that account for variation in earnings. This suggests that even absent macro-level changes in technological development or frontline workers' institutional power, meso-level organizational changes have scope to increase workers' pay.

Second, we show how insights from organizational case studies can be scaled up to study many occupations and firms across the economy. Discussions of task-driven learning and signaling and of workplace control are common in organizational ethnographies and case studies of tasks and work organization in specific occupations and workplaces. But these rich insights are rarely tested in representative, economy-wide data. Moreover, we provide a systematic framework in which to organize these insights as a function of task exceptions and analyzability. These contributions motivate further research on what drives variation in how employers organize work.

Indeed, finally, for practitioners, we identify broad paths through which work organization can support higher-paying frontline jobs. While few employers seek to increase labor costs holding all else equal, many are newly focused on retention and recruitment for frontline workers. Insofar as a tight labor market forces employers to raise pay for these workers, then determining how to adjust work organization to boost productivity and support these wages becomes crucial. While our results are not an artifact of labor market tightening—we adjust for time-varying and occupation-specific local labor market dynamics—we place our results in the broad context of rising pay at the bottom during the period we study. Employers that shift toward more complex or higher discretion frontline work organization are able to increase pay.

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Table 1: Task Bases of Pay Premiums

Category	Dimensions	Source of premium	Example	Measure
Routine	Few exceptions, analyzable	Wage-effort bargain	Taylorist manufacturing (Braverman, 1974; Lichtenstein, 1997)	Easily supervised/rule-bound
Nonroutine	Many exceptions, unanalyzable	Efficiency wages	Job enlargement and devolution at Walmart (Wartzman, 2022)	High autonomy/discretion
Craft	Few exceptions, unanalyzable	Tacit human capital	Maintenance work to production workers (Crozier, 1963; Helper and Kuan, 2017)	Complex task, typically learned on-the-job
Engineering	Many exceptions, analyzable	Formal human capital	Programming to machinists (Kelley, 1990)	Complex task, typically learned off-the-job

Note: This table summarizes the broad task categories we derive from Perrow (1967)'s framework. It also lists the connections we draw between these task categories and potential bases for pay premiums as jobs shift toward each task category. Finally, we anticipate the empirical analysis and note how we operationalize the categories with codes for specific tasks.

Table 2: How Work Organization Affects Starting Pay

Sample	A. Main				
Model	(1)	(2)	(3)	(4)	(5)
Complexity, Off	1.56***	0.450***	0.123***	0.104***	0.054***
	(0.034)	(0.025)	(0.015)	(0.013)	(0.011)
Complexity, On	1.65***	0.344***	0.120***	0.091***	0.064***
	(0.043)	(0.033)	(0.016)	(0.015)	(0.012)
Discretion	0.750***	0.145***	0.088***	0.072***	0.066***
	(0.056)	(0.033)	(0.019)	(0.016)	(0.013)
Relationships	-0.350***	-0.105***	0.016	0.011	0.010
	(0.043)	(0.025)	(0.014)	(0.013)	(0.011)
Start Quarter	×	×	×		
SOC5		×			
SOC5∧Company			×	×	×
StartQ∧CZ∧SOC2				×	×
Human Capital					×
R^2	0.20	0.47	0.67	0.73	0.82
Within R^2	0.18	0.01	0.00	0.00	0.33

Sample	B. Not Managerial/Professional				
Model	(6)	(7)	(8)	(9)	(10)
Complexity, Off	0.933***	0.490***	0.087***	0.084***	0.035*
	(0.043)	(0.037)	(0.020)	(0.019)	(0.015)
Complexity, On	1.25***	0.303***	0.126***	0.090***	0.065***
	(0.045)	(0.039)	(0.018)	(0.016)	(0.014)
Discretion	0.404***	0.102*	0.086***	0.065***	0.062***
	(0.069)	(0.043)	(0.023)	(0.019)	(0.016)
Relationships	-0.561***	-0.151***	0.014	0.004	0.011
	(0.046)	(0.031)	(0.016)	(0.015)	(0.013)
Start Quarter	×	×	×		
SOC5		×			
SOC5∧Company			×	×	×
StartQ∧CZ∧SOC2				×	×
Human Capital					×
R^2	0.17	0.38	0.63	0.71	0.79
Within R^2	0.14	0.02	0.00	0.00	0.27

Significance: * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). The dependent variable is second quarter pay throughout. The main sample (Panel A) comprises 611,000 job starts by 570,000 workers at 23,500 employers and the non-managerial/professional sample (Panel B) comprises 293,000 job starts by 277,000 workers at 15,000 employers. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table 3: Studying How Work Reorganization Changes Worker Selection

DV	4-Year College	Female	Black or Hispanic	Age
Model	(1)	(2)	(3)	(4)
Complexity, Off	0.031***	-0.032***	-0.026**	0.109
	(0.007)	(0.010)	(0.010)	(0.345)
Complexity, On	-0.002	-0.056***	-0.014	0.039
	(0.008)	(0.013)	(0.012)	(0.454)
Discretion	0.033***	-0.008	0.000	-0.810
	(0.010)	(0.012)	(0.013)	(0.437)
Relationships	0.012	0.007	-0.009	0.044
	(0.008)	(0.010)	(0.010)	(0.331)
SOC5∧Company	×	×	×	×
StartQ∧CZ∧SOC2	×	×	×	×
R^2	0.62	0.53	0.44	0.47

DV	WH Pay	WH Slope	WH Gap
Model	(5)	(6)	(7)
Complexity, Off	0.119***	0.000	-0.013
	(0.020)	(0.005)	(0.010)
Complexity, On	0.085***	-0.006	0.007
	(0.022)	(0.006)	(0.012)
Discretion	-0.005	-0.001	-0.005
	(0.025)	(0.005)	(0.013)
Relationships	0.000	0.000	0.000
	(0.019)	(0.004)	(0.009)
SOC5∧Company	×	×	×
StartQ∧CZ∧SOC2	×	×	×
R^2	0.57	0.33	0.37

Significance: * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). The dependent variables, listed across the top, are produced using the human capital controls used in our main models, with 'WH' standing for work history. We use the same sample as in our main models, comprising 611,000 job starts by 570,000 workers at 23,500 employers. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table 4: How Work Organization Affects Long-Term Pay

DV	4-Year-Later Earnings		Quarter 2 Earnings	
Model	(1)	(2)	(5)	(6)
Complexity, Off	0.116***	0.061**	0.091***	0.045**
	(0.025)	(0.022)	(0.017)	(0.014)
Complexity, On	0.107***	0.080**	0.071***	0.048**
	(0.031)	(0.028)	(0.020)	(0.016)
Discretion	0.107**	0.094**	0.058**	0.051**
	(0.033)	(0.030)	(0.0208)	(0.017)
Relationships	0.049*	0.047*	0.014	0.009
	(0.025)	(0.023)	(0.017)	(0.014)
SOC5∧Company	×	×	×	×
StartQ∧CZ∧SOC2	×	×	×	×
Human Capital		×		×
R^2	0.57	0.63	0.74	0.83

Significance: * p < .05; ** p < .01; *** p < .001 (two-tailed tests). For dependent variables, *4-Year-Later Earnings* reflects the average earnings 16 and 17 quarters after job start and *Quarter 2 Earnings* is the same dependent variable as in the main results. The sample used in this table comprises 363,000 job starts by 343,000 workers at 15,500 employers. This table draws on using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table 5: Characteristics of Firms Offering Complex and Autonomous Front-Line Tasks

Variable	Sample Mean	Off	On	Discretion	Relationships
Employment	1150	283	483	-397	199
Logged Employment	4.99	0.32	0.24	-0.04	-0.04
Logged Payroll	8.79	0.54	0.33	0.05	-0.10
Logged Revenue	9.39	0.36	0.46	-0.07	-0.20
Employment Growth	0.046	-0.004	-0.016	0.022	0.005
Revenue Growth	0.265	0.015	-0.021	0.037	0.027
Revenue/Employee	162.0	14.7	37.1	-2.1	-25.0
Profits/Employee	106.0	0.6	34.5	-10.8	-22.1
Pay/Employee	55.9	14.1	2.6	8.7	-2.9
Mean lnQ2 Pay	8.96	0.18	0.08	0.08	-0.04
Variance lnQ2 Pay	0.23	0.02	0.00	0.01	0.01
Founded Pre-1980	0.29	-0.02	0.03	-0.05	-0.03
Founded 1980-1999	0.31	0.00	0.00	0.00	-0.02
Founded Post-2000	0.40	0.02	-0.03	0.05	0.04
C Corporation	0.29	0.05	0.06	0.01	-0.03
S-Corp/Partnership	0.56	-0.13	0.00	-0.03	0.00
Non-Profit	0.15	0.08	-0.06	0.01	0.03
Manufacturing+	0.26	-0.06	0.09	-0.06	-0.09
Retail+	0.15	-0.08	0.04	-0.02	0.00
Business Services+	0.28	0.10	-0.01	0.10	-0.01
Health+	0.21	0.07	-0.09	-0.08	0.10
Education+	0.11	-0.03	-0.03	0.05	0.00

The sample used in this table comprises 59,000 employers. Columns labels with our work organization scores reflect the difference in average value between firms that are above versus below the median of the focal work organization dimension. ‘Off’ and ‘On’ refer to our off-the-job complexity and on-the-job complexity measures, respectively. The predictors were created using Burning Glass data. Mean second quarter pay and variance in second quarter pay, were created using the U.S. Census Longitudinal Employer-Dynamics Database; and all other variables were created using the Census Longitudinal Business Database.

Figure 1: Task Bases of Pay Premiums

Unanalyzable problems

Few Exceptions			Many Exceptions
	Craft Complex, tacit knowledge Know-how and learning	Nonroutine High autonomy tasks Efficiency wages	
	Routine Easily supervised/rule-bound Wage-effort bargain	Engineering Complex, explicit knowledge Signaling skill	

Analyzable problems

Figure 2: Diagram of Survey Results on Location of BG Tasks on Complexity and Autonomy Work Organization Dimensions

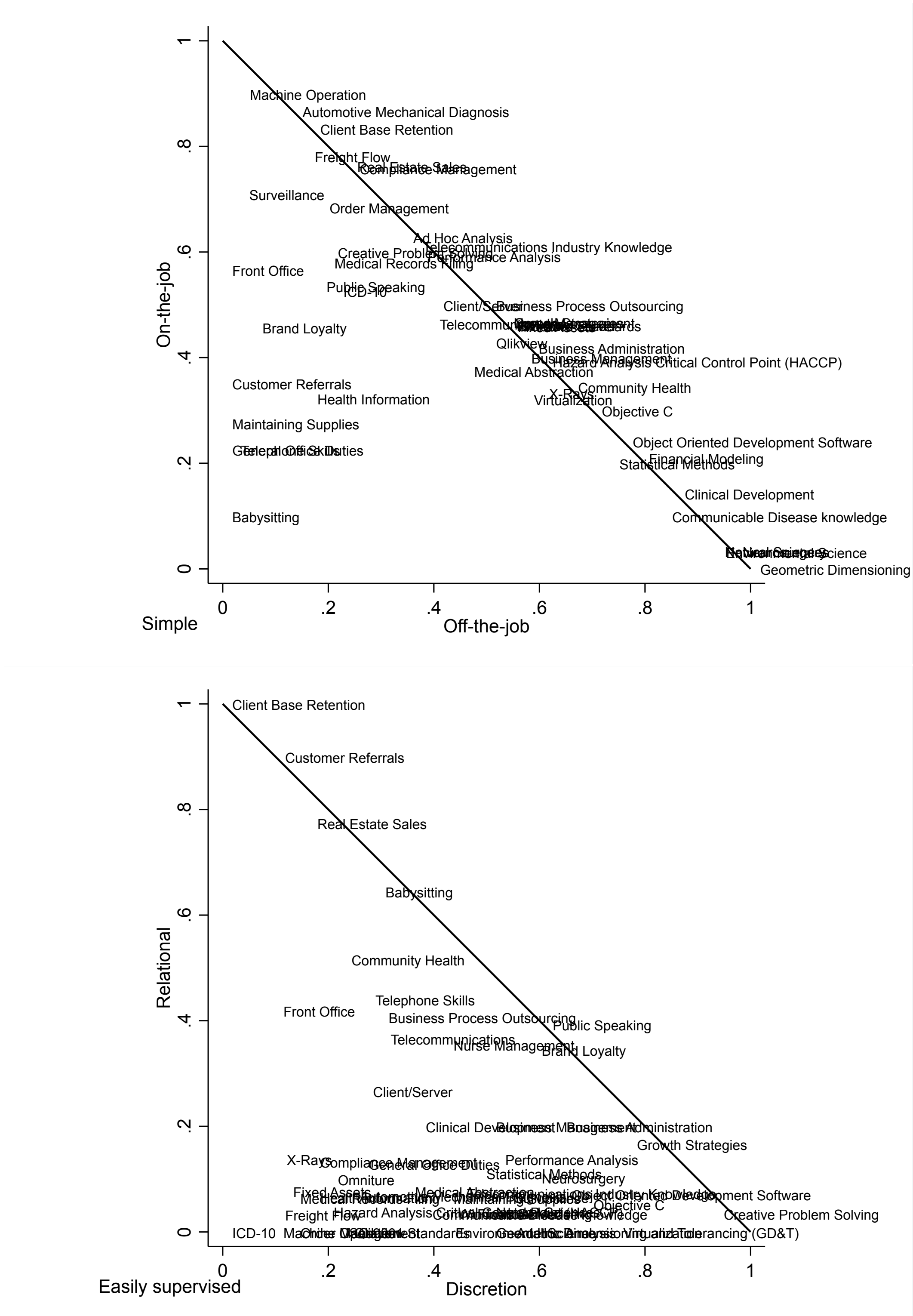
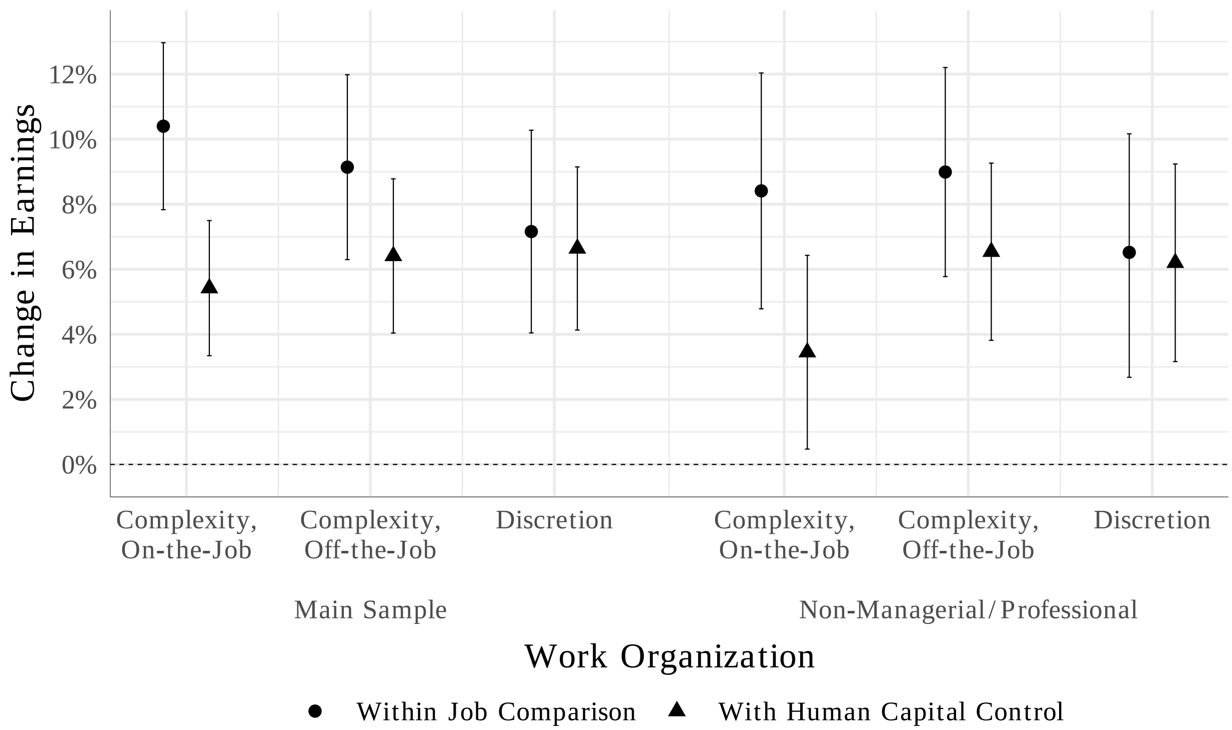


Figure 3: How Work Organization Affects Pay, Graphed by Dimension, Sample, and Model



A Survey Methodology

We conducted a series of online surveys to categorize tasks by the degree of autonomy and types of complexity involved. These surveys were completed from October 14th through October 21st, 2021, using Qualtrics forms distributed via the Prolific platform.

We fielded two surveys of a similar format: one focused on task autonomy, and the other gauging task complexity. Participants self-selected into these surveys, based on their professions, and were asked to rate a series of tasks associated with their occupational fields (we fielded multiple versions of each of the surveys to obtain full coverage of the tasks for that occupational field). In every survey type, tasks were randomly and evenly assigned across participants, with an average of 30 responses per task. Figures A.1 and A.2 give examples of the instructions given to workers with the concept definitions presented to them. We occasionally allowed people to participate in adjacent fields if the volume of available respondents in that field was low.

[Figure 4 about here.]

[Figure 5 about here.]

We also placed several restrictions on the participant pool, consistent across all surveys. We required respondents be aged 30 to 95, in order to prevent a possible skew after a viral TikTok video caused Prolific to temporarily have a disproportion of young adults in the participant pool. We also required all participants to reside in the United States and have a non-Prolific full-time job. Our average wage goal was $15 per hour, and respondents' average time ranged between 4 to 11 minutes, depending on the survey.

The surveys followed a specific and consistent flow across occupational fields. Participants were asked to pick their occupational fields (roughly corresponding to 2-digit Naics codes) and industries from multiple choices (these options were selected from the Bureau of Labor Statistics' NAICS codes), and identify their ages and genders. Age options were provided as exact numbers ranging from 20-80 years old; even though our Prolific pre-screening excluded people below 30 years, we included ages 20-29 as an option to validate whether our participants were actually 30 years or older. Gender included three options: male, female, or other. Analyzing the age and gender of the respondents also allowed us to ensure our data was not affected by the viral TikTok video.

The next section of the survey was a training section with interactive feedback, given after the participant submitted their answers. Figures A.1 and A.2 reproduce the prompt provided in this section. The training included instructions for categorizing six tasks, which were consistent across all surveys within each dimension. Specifically, in the discretion survey, participants were asked to choose whether a task required high discretion, high rules or supervision, or significant interactions with coworkers and clients. For the complexity surveys, participants were asked whether the specific task was simple, complex and learned on the job, or complex and learned off the job. In every case, the respondents also had the option 'I don't know.'

The main body of the survey showed each participant 50 different tasks based on the prevalence of those tasks across occupational fields. Tasks were randomly and evenly distributed across each respondent for consistency. Additionally, to ensure each participant was engaged with the survey and actively thinking about each task, we separated the tasks into 10 per page, for a total of 5 pages, and included an attention check question. At the end of the survey, we also included a feedback box for respondents to leave any comments on the tasks and/or the survey.

To construct the questions and instructions, we utilized the top 2,000 most frequently referenced tasks from Burning Glass, and sorted them based on the most frequently associated SOC code. We then constructed a crosswalk between SOC codes and Prolific occupation categories, and sorted the tasks based on the Prolific categories. This meant that some occupations received more tasks than others, and to ensure each task was evenly represented, we (1) rounded each survey to the next multiple of 50 tasks by using the top most common BG tasks (for example, if a occupation category had 84 tasks, we extracted 16 additional ones from the top most common tasks in Burning Glass, without repeating any values from the original occupation's tasks, to create a list of 100 tasks), and (2) matched the proportion of respondents requested to the number of tasks in each occupation's survey. This created even coverage across the surveys, ensuring that each participant saw 50 tasks, and that we received 30 responses per task. The number of participants per occupation category was determined based on the frequency of the occupation category, based on the Burning Glass frequency list.

B LEHD Data Restrictions

We identify the worker, employer, and start quarter for hundreds of millions of jobs from the first quarter of 2010Q1 through the first quarter of 2022. For privacy protection purposes, our LEHD coverage is limited to a random subset of U.S. states, as are all other external researcher Census projects. We can observe LEHD data for 24 U.S. states representing 50% of U.S. employment across the 5 main U.S. labor market regions: AZ, CO, DE, HI, IL, KS, MN, MD, NB, NV, NM, NY, ND, OH, OK, PA, TN, TX, UT, VA, WA, DC, WI, and WY. We essentially follow an established LEHD workflow (Dunne et al., 2004) to identify job starts in the LEHD: we (i) export the LEHD Employment History Files quarterly earnings data from 2004Q1-2022Q1, using years prior to 2010 only for the work history controls discussed below; and, (ii) merge the state-level employer IDs in the earnings data to the main Census employer IDs using the Employer Characteristics Title 26 crosswalk, because the Census IDs are associated with employer names used in our BG-LEHD merge. We then (iii) group about 1% of employers based on shared federal Employer Identification Number (EINs) that are not associated with any larger entity in one year but are in another, to trace smaller employers moving between the Census's core classification of one- versus multiple-workplace employers over time; (iv) adjust to 2021 dollars using the Bureau of Labor Statistics Consumer Price Index; and, (v) keep only the first job start per worker-employer that meets these criteria. This allows workers to have multiple job starts in our study so long as they are with different employers.

We follow prior literature using the LEHD to trace substantial employment relationships by excluding small and transitory payments, and to reduce the impact of large changes in hours worked on our results (Haltiwanger and Spletzer, 2021). We require, in 2021-adjusted dollars, (vi) $7,552 in earnings, or the equivalent of a full-year working 20 hours per week at the federal minimum wage, over the length of the job or the first 2 years of the job, whichever comes first; (vii) $1885 in second quarter earnings, or the equivalent of 20 hours per week at the federal minimum wage; (vii) third quarter non-zero earnings, reducing the influence of partial quarter job spells, i.e. weeks worked, on our main dependent variable of second quarter earnings. This filtering of short employment relationships is common in work using the LEHD and other administrative earnings data (Dunne et al., 2004). We also exclude a sliver of jobs where our identified second quarter earnings are not the majority of observed earnings for the worker in that quarter. We log all earnings outcomes throughout.

C Employer Name Merge

Faced with the challenge of many false employer name matches due to the presence of common words in the Census employer name data, as mentioned above, we iteratively developed a three-step strategy to incorporate employer into our merge novel of the BG and LEHD data: preparation, fuzzy matching, and processing the employer-occupation-CZ units.

For the (A) preparation step, we clean all names through standard actions such as uppercasing, removing punctuation, and removing tags such as 'Incorporated.' Sometimes these tags are useful for identifying the true company name, for example 'headquarters', but often they are not, instead capturing something like the city name that the workplace exists in or the name of the managing partner in a small firm. We apply a host of Census-data-specific cleaning strategies based on an iterative review of the cleaned data. For example, we develop an algorithm to clean out common words that are used to mark specific workplaces within employers. We then create pseudo-Census-employer-IDs for franchise companies that otherwise have distinct IDs in the Census data, effectively matching many of these entities by hand. One reason we want to do this is that we then categorize each employer in the BG and LEHD data based on whether its posts or starts were largely in only one state or across multiple commuting zones (CZs), to use as a merge criterion. While the vast majority of employers post and hire in a single CZ, the majority of workers are in multi-CZ employers.

For the (B) fuzzy matching step, we first set aside all employer names with fewer than 6 characters, which are only allowed to exact match. We then loop through all CZs as well as an indicator for multi-CZ, looping again through all sets of first 3 characters that appear within each CZ on both the BG and LEHD side. We create a name similarity score based on cosine distance for all names, after extensive testing of whether this assumption excludes a meaningful number of businesses that otherwise could be matched. We then only keep name matches that are either exact matches or that match with greater than 66% similarity and obtain 1 of 11 criteria that we defined through hand review with false match rate testing. For example, 1 criterion is that both names share the first two words and the third work on either or both side is a common ending word. This work creates a list of all employer names that match across the two databases, but this is not always a 1-1 list and may include chaining, for example when name A links to name B that links to name C, etc.

For the (C) processing the employer-occupation-CZ units step, we employer-occupation-CZ units based on clusters on the bimodal network of all sets of Census and BG employer names that fulfil our occupation and CZ criteria. We do this rather than creating employer-level units to avoid the issue of common names producing large clusters of associated names, i.e. ‘American Distributors’ linking to ‘American Distribution’ and to ‘American Tire Distributors’ when there entities in fact share no occupational or geographic overlap between the job post and start data. Units without occupational or geographic overlap would be deleted anyway in our merge, and, in our initial tests, led to false job to post linking due to large clusters of names. We then conduct hand testing of matched entities. On the one hand, we remove some BG-LEHD links that appear to be false clusters even when they pass our 12 match criteria. On the other hand, we have a research assistant hand match the 1,000 largest unmatched BG entities.

D Regression weighting according to U.S. national proportions from the ACS

If there is substantial heterogeneity in the effect of work organization on pay across demographic and occupational groups, our BG-LEHD-ACS sample may offer biased measures of the average effects of task allocation on pay for the U.S. working population. There are many reasons why our sample may diverge from U.S. occupational and demographic proportions: (i) BG's online nature may over-represent analytic rather than manual workers and more versus less technically-savvy workers and employers who regularly use the internet; (ii) employers may use job posts more for higher-level occupations, though this may be counterbalanced by more workers starting per frontline job post than, for example, per middle manager post; (iii) our focus on job starts may over-represent the types of workers who change jobs more frequently; (iv) our requirement of having observable working history may bias away from younger workers coming out of high school or college; (v) our requirement of an ACS predominant occupation response might weight towards the kinds of workers who are less likely to be unemployed; (vi) our 24 LEHD states may be different from the national sample, most notably in the absence of California and Florida; (vii) our goal of comparing within employer-occupation positions, as discussed above, may lead to an over-representation of large employers and the kinds of workers who are more likely to work for them; and, (viii) unpredictable aspects of the correlation in how the Census Bureau and BG record employer names for different types of employers may leads to the over-representation of some worker types in unexpected ways.

To address this concern, we follow the prior literature (Lemieux, 2006) to create regression weights to re-balance our sample to U.S. national demographic and occupational proportions. We specifically measure the inverse likelihood that each job start appeared in our analytic sample based on the worker's demographic and occupational characteristics. We fit a logistic regression model on the full sample of all weighted ACS responses:

$$in_i = sex_i + age_i + age_i^2 + race_i + edu_i + occ_i \ ,$$

where in_i captures whether worker i appears in our merged BG-LEHD data; sex_i is binary ; age_i is age in years at time of job start; $race_i$ captures self-identification in the White, Black, Hispanic, or Asian/other races; edu_i captures high school or less, some college or an associates degree, a four-year college degree, or a graduate degree; and occ_i captures the 2-digit SOC group. We take the fitted value for each worker in our sample based on this logistic regression model, which represents our model's prediction of the odds that that worker would appear in our analytic data. We use the inverse of this fitted value, divided by the number of starts observed for that worker in our analytic data, as a regression weight to effectively up-weight workers who were under-represented in our analytic sample, and vice versa.

Through this weighting technique, we give less weight in our estimates of average treatment effects to workers in demographic and occupational groups that are more likely to be in our BG-LEHD data than what would be expected based on U.S. national demographic and occupational proportions. We assess the effect of this weighting on our sample proportions in Appendix E.

E Descriptive Statistics

Panel A of Table A.1 compares our weighted sample and unweighted sample, to the U.S. workforce proportions, on the variables we used to weight our regressions: race, sex, age, education, and the 22 2-digit SOC groups, though occupations are aggregated in this table into 4 groups. We discuss this weighting in Appendix D above. If weighting succeeds in bringing our sample closer to U.S. national proportions, our weighted sample (labelled

Weighted) should be more similar to the U.S. workforce (labelled ACS), than our unweighted sample (labelled Unweighted) would be to the U.S. workforce (again, labelled ACS).

ACS weighting improves on the large occupational imbalance in our unweighted sample, which, in the presence of effect heterogeneity across occupational and demographic groups, would bias our estimates of the average treatment effect of our work organization dimensions on pay. Comparing Columns 3 and 4, we observe that our unweighted sample substantially over-represents higher-educated compared with lower-educated workers, and managerial/professional compared with production and services occupations. Our unweighted sample also less dramatically over-weights female, younger, and White workers, compared to U.S. national proportions. Comparing Columns 3 and 5, we observe that weighting closes the large gap on high school or less and production and services workers. Weighting also eliminates the sex imbalance, reverses the age imbalance, and slightly increases the bias towards White workers.

The descriptive statistics in Panel B of Table A.1 of means of our other analytic variables before and after ACS weighting, are easily reconciled with increasing the weight on production and service jobs compared with managerial and professional jobs. Weighting reduces average second quarter pay, off-the-job complexity, and the slope but not average of pre-job-start earnings. Our main outcome measure of average second quarter pay is 13% lower for the weighted relative to our unweighted sample. That both of these measures - 9.2 and 9.075 - are lower than the national full-time worker average of 9.4, calculated from annual pay in ACS, suggests the inclusion in our sample of some part-time work and/or partial earnings quarters. Off-the-job complexity declines by 17%, mostly made up by the Simple reference dimension. That weighting reduces work history earnings slope from 7.3 to 5.8%, but not work history average earnings, suggests that the up-weighted workers may be in more stable trajectories than the down-weighted workers, consistent with the rise in average worker age.

We finally discuss a few implications for the study of the levels for the weighted sample analytic variables in Panel B of Table A.1. Reflecting measures on a scale from 0 representing no responses for any of the tasks to 1 representing all the responses on all the tasks, complexity and autonomy scores ranging from .41 to .27 imply that (a) respondents rated many of the common tasks in the U.S. worker force as complex and/or non easily supervisable, and (b) that there is substantial variation across jobs in the number of tasks rated highly on our complexity and autonomy work organization dimensions.

Our workers come from an array of industries, though somewhat less in Manufacturing and Construction than the other four macro-sectors (weighting shifts towards fewer retail and more manufacturing and social services workers).[4] While drawing 57% of job starts at employers with 500 or more employees may seem high, this is indeed close to the U.S. workforce proportion of 53% according to the 2017 Economic Census.

The proximity of the mean work history earnings to new job second quarter pay suggests that many workers in our sample are not receiving substantial pay boosts from their moves, which may be reflected in declining work organization task dimension scores. This, in part arising from our exclusion of all 'first time job starters' based on the requirement of having past earnings in order to control for prior signals of productivity, may also reflect a sample composed of relatively older and less currently-upwardly-mobile workers than are often imagined in studies of new jobs. We estimate that 14% of workers in our sample joined the labor force in the last 4 years, and 20% are coming out of at least 1 non-earning quarter despite possessing work history, suggesting education or unemployment.

[4] We gather Naics industry and employer size from the Longitudinal Business Database (Chow et al., 2021). To report descriptive statistics, we also 5 employer industrial divisions from the highest-employing 2-digit NAICS within each employer from 2010 through 2020: manufacturing, goods, utilities, and waste (11, 21, 22, 23, 31, 32, 33, 56); retail, wholesale, and transportation (42, 44, 45, 48, 49); business services, information, and finance (51, 52, 53, 54, 55); health (62); and social services, education, and art (61, 71, 72, 81, 92).

F Augmenting Earnings Models with Work Organization Measures

We run a series descriptive models to study how controls like industry, education and firm size relate to the uncontrolled effects of our novel task measures. These results demonstrate that our work organization measures pick up variation both explained by and beyond occupation and other major factors hypothesized in the existing literature to induce a correlation in these variables. This would not be the case if differences in job posts within organizations were human resource jargon not reflecting factors that predict pay, if our long analytic chain produced noise that attenuated our effects away, there similarly is no measurable effect of task reallocation on these measures after selecting an occupation, or human capital selection was driving the measured effects.

The full form of these descriptive models is:

$$y_{i,j} = \mathbf{u_j'}\beta + \alpha_{Start_Q} + \alpha_{Occ} + \alpha_{Naics} + \alpha_{Size} + \mathbf{x_{i,j}'}\lambda + \epsilon_{i,j} \tag{2}$$

where $y_{i,j}$ measures second quarter earnings for worker i at job j. We include the α_{Start_Q} start quarter fixed effect to remove the overall time trend. The parameters of interest β's, human capital controls $\mathbf{x_{i,j}'}\lambda$, and residual term $\epsilon_{i,j}$ are the same as above. We weight these regressions by our ACS-derived job-start-level factor and cluster standard errors at the firm level.

Even if a baseline correlation reveals higher pay for jobs with higher-level tasks on our key dimensions, the most obvious concern is that merely reflects a tendency to allocate more or less complex or autonomous tasks to different occupations. We address this concern by controlling for the SOC 5-digit occupations.

Firm industry (α_{Naics}) and size (α_{Size}) may also induce a correlation in our measures of task allocation and pay. Younger and more technological industries may be linked to patterns of worker technical savvy or social skill, whereas industries that emerged in the more distant past, may sit at a local equilibrium where certain management styles persist even if a reorganization of tasks and pay could theoretically improve net profitability.[5] But even controlling for industry, larger employers may pay workers more and offer more complex or autonomous jobs because of greater profit-sharing with workers or greater likelihood of organization bargaining (Cobb and Lin, 2017). These industry and firm size fixed effects afford comparison of the task-pay relationship for workers at employers of similar size and in similar product markets.

We would expect initial associations of our work organization measures and pay to disappear under the following 4 conditions: (a) subtle changes in task content within the categories defined by our controls reflect the noise of human resource jargon; (b) noise emerging from our long analytic chain - machine-learning tasks, task dimension survey, aggregating to jobs, fuzzy employer matching - attenuates any signal in the task data not reflected by characteristics such as occupation; (c) the only way to induce substantial changes in pay is to reassign workers across occupations rather than changing the tasks assigned to similarly positioned workers; or, (d) any initial positive correlations are explained by stronger workers selecting into more complex or autonomous jobs, or the kinds of companies offering more complex or autonomous jobs.

Table A.2 reports the results of our descriptive regressions, suggesting that these four conditions do not obtain. In Panel A, estimated using our main sample, we compare results from a simple model including our task scores (Model 1) with a model including those task scores as well as human capital 'Mincer' controls bolstered with work history measures (Model 2). In our baseline, off-the-job and on-the-job complexity are associated with more than a 150% rise in pay, discretion is associated with a roughly 75% rise in earnings, and ships is associated with a 35% decline in earnings. Alone, these complexity and autonomy factors explain a substantial amount of the variance in logged earnings in the U.S. workforce, with an R^2 measure of 18%. Adding our human capital control set of education, demographics,

[5] We measure the highest employing 5-digit NAICS industry for each employer in our sample over the 2010-2020 period based on the Census LBD data. We break firm size into 4 categories: 1-499, 500-14,999, 15,000-50,000, and 50,000+ employees.

and work history, increases the explanatory power of our model to 63%. We see that our human capital measures explain about 30% of the baseline off-the-job complexity correlation, 35% of the on-the-job complexity correlation, 90% of the naive discretion effect, and only a small percentage of the ships association.

The results from Models 3 and 4 of Table A.2, which substitute occupation plus employer factors for the human capital controls in Model 2, suggest that occupation explains more of the baseline complexity task allocation effect, whereas human capital explains more of the baseline discretion effect. After controlling for occupation, moving off-the-job complexity from 0 to 1 increases pay by roughly 45%, with 34% and 15% for on-the-job complexity and discretion, respectively. Adding, in Model 4, 5-digit NAICS industry and employer size category alongside occupation, cuts our Model 2 human capital complexity estimates in half while still maintaining a relatively higher discretion estimate of an 11% rise in pay.

A comparison of the baseline Model 1 with the full form Model 6 of Table A.2 suggests that although our complexity and autonomy measures are highly correlated with human capital, occupation, and employer industry and size, the effects of our main predictors on pay are still substantively and statistically significant. Here, moving from none to total off-the-job complexity, on-the-job complexity, and discretion, is associated with a 13%, 16%, and 7% increase in pay. We therefore find evidence that the four conditions described above - BG tasks provide no signal, our analytic chain obscures that signal, only occupation changes have effects, and the effect is all selection - do not appear to obtain here. The results in Panel B of Table A.2, based on the non-managerial/professional jobs, confirms the relationships estimated with the main sample.

[Table 6 about here.]

[Table 7 about here.]

[Table 8 about here.]

[Table 9 about here.]

[Table 10 about here.]

Table A.1: Descriptive Statistics From the Main Sample Before and After ACS Weighting

Category	Variable	ACS	Unweighted	Weighted	Long-term
	Panel A - ACS Weighting Variables				
Sex	Female	0.485	0.540	0.486	0.491
Age	Mean	40.74	37.23	43.05	42.26
Race	White	0.627	0.671	0.698	0.702
Race	Black	0.116	0.123	0.094	0.093
Race	Hispanic	0.169	0.119	0.137	0.135
Race	Asian (and other)	0.087	0.087	0.071	0.070
Education	High School or Less	0.401	0.200	0.364	0.347
Education	Some College or Associates	0.257	0.321	0.328	0.334
Education	Four-Year College	0.217	0.302	0.190	0.197
Education	Graduate School	0.124	0.177	0.119	0.122
Occupation	Managerial and Professional	0.379	0.520	0.342	0.356
Occupation	Sales and Administrative	0.232	0.252	0.242	0.255
Occupation	Production	0.214	0.104	0.224	0.210
Occupation	Services	0.175	0.125	0.192	0.179
	Panel B - Key Analytic Variables				
Outcome	Mean Second Quarter Pay		9.200	9.075	9.377
Predictor	Mean Off-the-job Complexity Score		0.309	0.256	0.265
Predictor	Mean On-the-job Complexity Score		0.409	0.419	0.420
Predictor	Mean Discretion Score		0.363	0.360	0.360
Predictor	Mean Relationships Score		0.280	0.259	0.262
Sector	Manufacturing+		0.079	0.089	0.070
Sector	Retail+		0.281	0.221	0.222
Sector	Business Services+		0.239	0.230	0.247
Sector	Health+		0.270	0.261	0.274
Sector	Social Services+		0.132	0.199	0.188
Employer Size	1-499		0.207	0.228	0.221
Employer Size	500-14999		0.260	0.208	0.218
Employer Size	15000-50000		0.250	0.174	0.182
Employer Size	50000+		0.152	0.192	0.191
Work History	Mean Earnings		9.039	9.044	9.079
Work History	Mean Slope		0.073	0.058	0.057
Work History	Joined Labor Market Last 4 Years		0.156	0.140	0.126
Work History	Zero Earnings Quarter Before Start		0.191	0.197	0.182

This table describes ACS national demographic proportions, our sample before and after applying ACS weighting, and our long-term sample after applying ACS weighting. All figures in this table are in proportions of the sample, except for mean pay and work history pay in logged quarterly earnings, slope in percentage points, age in years, and the four predictor scores on their respective 0 to 1 scales. ACS values are derived from public estimates over the 2010-2020 period. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table A.2: How Work Organization Affects Starting Pay, Descriptive Results

Sample	A. Main					
Model	(1)	(2)	(3)	(4)	(5)	(6)
Complexity, Off	1.56***	0.472***	0.450***	0.254***	0.243***	0.125***
	(0.034)	(0.018)	(0.025)	(0.016)	(0.018)	(0.012)
Complexity, On	1.65***	0.576***	0.344***	0.253***	0.193***	0.155***
	(0.043)	(0.028)	(0.033)	(0.019)	(0.027)	(0.015)
Discretion	0.750***	0.069*	0.145***	0.107***	0.071**	0.065***
	(0.056)	(0.029)	(0.033)	(0.021)	(0.025)	(0.016)
Relationships	-0.350***	-0.330***	-0.105***	-0.022	-0.088***	-0.025*
	(0.043)	(0.025)	(0.025)	(0.016)	(0.019)	(0.012)
Start Quarter	×	×	×	×	×	×
SOC5			×	×	×	×
Naics5 + Size				×		×
Human Capital		×			×	×
R^2	0.20	0.63	0.47	0.54	0.67	0.70
Within R^2	0.18	0.62	0.01	0.00	0.40	0.36

Sample	B. Not Managerial/Professional					
Model	(7)	(8)	(9)	(10)	(11)	(12)
Complexity, Off	0.933***	0.459***	0.490***	0.210***	0.276***	0.105***
	(0.047)	(0.028)	(0.037)	(0.022)	(0.028)	(0.017)
Complexity, On	1.25***	0.469***	0.303***	0.237***	0.166***	0.144***
	(0.045)	(0.031)	(0.039)	(0.021)	(0.030)	(0.016)
Discretion	0.404***	0.090*	0.102*	0.105***	0.055	0.070***
	(0.069)	(0.040)	(0.043)	(0.026)	(0.032)	(0.019)
Relationships	-0.561***	-0.344***	-0.151***	-0.023	-0.111***	-0.017
	(0.046)	(0.029)	(0.031)	(0.019)	(0.024)	(0.014)
Start Quarter	×	×	×	×	×	×
SOC5			×	×	×	×
Naics5 + Size				×		×
Human Capital		×			×	×
R^2	0.17	0.55	0.38	0.49	0.60	0.65
Within R^2	0.14	0.53	0.02	0.01	0.36	0.32

Significance: * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). The dependent variable is second quarter pay throughout. The main sample (Panel A) comprises 611,000 job starts by 570,000 workers at 23,500 employers and the non-managerial/professional sample (Panel B) comprises 293,000 job starts by 277,000 workers at 15,000 employers. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table A.3: How Work Organization Affects Starting Pay, Managerial/Professional Sample

Model	(1)	(2)	(3)	(4)	(5)
Complexity, Off	0.850***	0.483***	0.175***	0.140***	0.076***
	(0.048)	(0.029)	(0.024)	(0.0223)	(0.017)
Complexity, On	2.09***	0.533***	0.147***	0.123***	0.077**
	(0.082)	(0.044)	(0.037)	(0.035)	(0.026)
Discretion	0.258***	0.263***	0.108**	0.098**	0.083***
	(0.056)	(0.040)	(0.033)	(0.031)	(0.023)
Relationships	-0.607***	0.034	0.027	0.032	0.008
	(0.048)	(0.030)	(0.027)	(0.025)	(0.019)
Start Quarter	×	×	×		
SOC5		×			
SOC5∧Company			×	×	×
StartQ∧CZ∧SOC2				×	×
Human Capital					×
R^2	0.07	0.32	0.55	0.62	0.77
Within R^2	0.05	0.01	0.00	0.00	0.39

Significance: * p < .05; ** p < .01; *** p < .001 (two-tailed tests). The dependent variable is second quarter pay throughout. This managerial/professional sample comprises 317,000 job starts by 294,000 workers at 14,500 employers. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table A.4: How Work Organization Affects Starting Pay, Results Preferring Same Quarter of Job Post and Job Start

Sample	Main Sample		Man./Prof.		Not Man./Prof.	
Model	(1)	(2)	(3)	(4)	(5)	(6)
Complexity, Off	0.102***	0.053***	0.137***	0.074***	0.080***	0.031*
	(0.013)	(0.011)	(0.022)	(0.017)	(0.019)	(0.016)
Complexity, On	0.099***	0.070***	0.120***	0.068**	0.101***	0.075***
	(0.014)	(0.012)	(0.035)	(0.025)	(0.016)	(0.014)
Discretion	0.067***	0.058***	0.075*	0.058*	0.069***	0.060***
	(0.016)	(0.013)	(0.031)	(0.023)	(0.018)	(0.016)
Relationships	0.018	0.015	0.051*	0.031	0.008	0.011
	(0.013)	(0.011)	(0.025)	(0.019)	(0.015)	(0.013)
SOC5∧Compan×	×	×	×	×	×	×
StartQ∧CZ∧SOC2	×	×	×	×	×	×
Human Capital		×		×		×
R^2	0.73	0.82	0.62	0.77	0.71	0.79
Within R^2	0.00	0.33	0.00	0.39	0.00	0.27

Significance: * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed tests). The dependent variable is second quarter pay throughout. This ‘main’ sample comprises 611,000 job starts by 570,000 workers at 23,500 employers. Rather than preferring job posts in in the quarter prior to job start, as in our main results, we here prefer job posts in the same quarter as the job start. We created this table using data from Burning Glass and the U.S. Census Longitudinal Employer-Dynamics Data linked to the American Community Survey.

Table A.5: Characteristics of Firms Offering Complex and Autonomous Front-Line Tasks, Residualized by 2-Digit NAICS Industry

Variable	Sample Means	On	Off	Discretion	Relationships
Employment	1150	461	454	-423	194
Logged Employment	5.0	0.2	0.4	0.0	0.0
Logged Payroll	8.8	0.2	0.6	0.0	0.0
Logged Revenue	9.4	0.3	0.5	0.0	0.0
Employment Growth	0.05	-0.02	0.00	0.02	0.00
Revenue Growth	0.27	-0.01	0.01	0.02	0.02
Revenue/Employee	162.0	11.9	36.9	7.1	-10.5
Profits/Employee	106.0	10.8	25.9	0.9	-9.8
Pay/Employee	55.9	1.2	10.9	6.1	-0.7
Mean ln Q2 Pay	8.96	0.05	0.15	0.05	0.00
Variance lnQ2 Pay	0.23	0.01	0.01	0.00	0.01
Founded Pre-1980	0.29	0.03	-0.01	-0.03	-0.01
Founded 1980-1999	0.31	-0.01	0.01	0.00	-0.01
Founded Post-2000	0.40	-0.02	0.00	0.03	0.02
C Corporation	0.29	0.04	0.05	-0.01	0.00
S-Corp/Partnership	0.56	-0.02	-0.10	-0.02	0.01
Non-Profit	0.15	-0.02	0.05	0.01	-0.01

The sample used in this table comprises 59,000 employers. Columns labels with our work organization scores reflect the difference in average value between firms that are above versus below the median of the focal work organization dimension. 'Off' and 'On' refer to our off-the-job complexity and on-the-job complexity measures, respectively. The predictors were created using Burning Glass data. We residualize all values by 2-digit Naics, but otherwise create the table in the same way as our firm-level results in the main text. Mean second quarter pay and variance in second quarter pay, were created using the U.S. Census Longitudinal Employer-Dynamics Database; and all other variables were created using the Census Longitudinal Business Database.

Figure A.1: Complexity Survey Instructions

We are studying how people interpret job requirements that are commonly included in online job postings. Your participation will help us give advice about how to write job postings that can attract successful applicants.

To do this, we will show you a series of job requirements that employers list on job postings. Imagine someone you know was applying to a position between 2010 to 2018, and one of the following job requirements was listed. Choose the category that you think best fits that requirement:

(1) **Simple**: by simple, we mean the requirement implies a relatively straightforward task that most people could do without advanced education or substantial prior work experience. For example, "line cook" or "cash handling" would both be simple requirements.

(2) **Complex, learned on the job**: this category means the job requirement has a **more complex skill set**, but can typically be **learned on the job**. For example, "electrical wiring" and "office management" both typically require some practice and on-the-job learning before an employee is proficient at them.

(3) **Complex, learned off the job**: job requirements in this group also require a **more complex skill set**, and must be learned **outside of the job**, such as in college. An example of this would be "computer programming" or "physical therapy", because these require a minimum of a bachelor's degree to be performed.

Before we move to the main survey, here is an example of the multiple choice questions you will see in the survey. We use this to gauge your understanding of the coding scheme.

	Simple	Complex, learned **on** the job	Complex, learned **off** the job	I don't know
Plumbing	○	○	○	○
Operating a cash register	○	○	○	○
Real estate sales	○	○	○	○
Teaching	○	○	○	○
Package delivery	○	○	○	○
Mental health therapy	○	○	○	○

Figure A.2: Autonomy Survey Instructions

We are studying how people interpret job requirements that are commonly included in online job postings. Your insight will help us give advice about how to write job postings that can attract successful applicants.

To do this, we will show you a series of job requirements that employers list on job postings. Imagine someone you know was applying to a position between 2010 to 2018, and one of the following job requirements was listed. Choose the category that you think best fits that requirement:

(1) **High discretion**: By "high discretion", we mean that an employee has a high level of autonomy to decide what to do in their job. For example, "materials selection" or "strategic planning" imply high discretion, while "pipe systems" and "co-pay collection" do not. High discretion jobs tend to place responsibility on an employee and require decision-making. These jobs are difficult to supervise and assess, so managers need to trust employees to do their best and use their best judgment.

(2) **Rule-bound or closely supervised**: By "rule-bound or closely supervised," we mean the job requirement implies either a high degree of supervision by a manager or supervisor or following detailed rules that limit employee decision-making. Unlike high discretion, this implies strong limits on employee autonomy and decision making. For example, jobs involving installation of "conveyor systems" or "reordering supplies" suggest high supervision and little decision making.

(3) **Significant relationships with coworkers and clients**: In this category, we would like you to identify job requirements that centrally involve building important relationships with coworkers and clients. For example, "human resource management" and "financial advisor" require significant relationships with coworkers and clients to perform, while "electrical design" and "preparing reports" tend to be more autonomous. In jobs with significant relationships, employee autonomy and decision-making is constrained, not by rules, but by the need to navigate relationships with others.

Before we move to the main survey, here is an example of the multiple choice questions you will see in the survey. We use this to gauge your understanding of the coding scheme.

	High discretion	Rule-bound or closely supervised	Significant relationships with coworkers and clients	I don't know
Teaching	○	○	○	○
Mental health therapy	○	○	○	○
Shelf restocking	○	○	○	○
Blog posts	○	○	○	○
Package delivery	○	○	○	○
Real estate sales	○	○	○	○