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Local Labor Market Tightness and Job Quality: Evidence from Job Changers*

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Abstract

Using novel data from the Survey of Household Economics and Decisionmaking, we examine how labor market tightness affects workers' job quality. We estimate that a 10 percent increase in job vacancies not only increases the probability of changing jobs, it yields an 11–18 percent increase in the (unconditional) probability of switching to a better job overall, and one with greater pay and benefits, interest in the work, and advancement opportunities. Because tight labor markets improve both worker pay and job amenities in roughly the same proportion, their benefits to workers are underestimated when based on pay alone.

Key words: job quality, labor market tightness, SHED, JOLTS, Lightcast, local shocks

JEL: J23, J28, J32, J62

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1 Introduction

In canonical models of labor search and job matching, tight labor markets benefit workers through a faster arrival rate of job opportunities, higher wages, or both. Indeed, in the early recovery from the COVID pandemic in 2021 and 2022, hire rates surged[1] and worker wages, especially at the lower end of the distribution, experienced dramatic gains (Autor, Dube and McGrew, 2023). Yet stories in the popular media depicted this phenomenon, dubbed the “Great Resignation”, as primarily being about a search for better jobs in terms of more predictable schedules (Smith, 2021), more autonomy (Greene, 2025), and greater satisfaction with work tasks (Levanon, 2021), among other non-wage amenities. This focus was not just in the popular press; growing research evidence also finds that workers place significant value on job and employer characteristics beyond their pay (Maestas et al., 2023; Gallup et al., 2025). Little formal evidence exists, however, on how tight labor markets influence these other job amenities besides pay.

Using novel data from the Survey of Household Economics and Decisionmaking (SHED) and multiple measures of local labor market tightness, this paper quantifies how changes in labor market conditions affect self-reported workplace amenities. Drawing on considerable geographic and temporal variation in labor market conditions between 2020 and 2024, we find that a 10 percent increase in state-level vacancies per capita increases the likelihood that a prime-age adult changes jobs by 7 percent and changes to a job that they consider better overall by 11 percent. The same increase in labor market tightness also leads to increases in individuals changing to a job they find more interesting of 12 percent, one with better work-life balance of 11 percent, and one with better opportunities for advancement of 14 percent. These effects suggest that utility measures capturing only pay likely miss substantial benefits of labor market tightness for workers’ well-being.

Roughly three-fifths of these improvements are accounted for by a greater likelihood of changing jobs, with the remaining two-fifths from increased amenity improvement conditional on changing jobs. Moreover, back-of-the-envelope estimates of the contributions of each amenity to overall job quality improvements from tighter labor markets suggest that increased interest in the

[1] See https://fred.stlouisfed.org/graph/?g=1TDy7.

work accounts for roughly 24 percent of the improvement, pay and benefits for 23 percent, better work-life balance at 12 percent, and opportunities for advancement at 10 percent. These findings indicate that employers may improve non-pecuniary amenities to attract and retain workers during tight labor markets.

Several exercises support the robustness of these findings. First, we validate that our methodology yields estimates in line with those from previous literature for more common job dynamics outcomes. After increases in labor market tightness, greater shares of prime-age individuals quit jobs, apply for new jobs, start new jobs, and ask for and get raises. Second, we show similar effects across different data sources. Our baseline effects at the state level rely on the Job Openings and Labor Turnover Survey (JOLTS), but we obtain similar results when using job postings from Lightcast, either at the state or Core-Based Statistical Area (CBSA) level. Finally, a shift-share approach isolating changes in labor market tightness due to plausibly exogenous changes in labor demand yields broadly similar results, with slightly higher magnitudes. Using the shift-share instrument, a 10 percent increase in labor market tightness leads to a 18 percent increase in the likelihood of changing to a better job overall.

A growing literature has emphasized workers' valuation of non-pay characteristics, aiming to capture preferences beyond the traditional interpretation of compensating differentials. Estimating the value of these amenities is empirically difficult, as pay and positive job characteristics tend to be positively correlated in observational data suggesting other underlying factors influence both (Bonhomme and Jolivet, 2009; Lavetti, 2023; Mas, 2025).[2] Recent advances have come through estimation of revealed preferences, either latently by workers switching firms (Sorkin, 2018; Lamadon, Mogstad and Setzler, 2022; Lavetti and Schmutte, 2025; Humlum, Rasmussen and Rose, 2025) or through eliciting willingness to pay for amenities through experiments. The latter genre has been able to quantify the value of certain specific amenities. For example, willingness to pay for flexible and remote work ranges from 7–25 percent of earnings, depending on the

[2]Fringe benefits, opportunities for job growth, safe work environments, autonomy, and positive supervisor and co-worker relationships are job characteristics that workers have been shown to value (Royalty, 2008; Eriksson and Kristensen, 2014; Dube, Naidu and Reich, 2022; Lagos, 2025).

precise amenity and population (Mas and Pallais, 2017; Barrero, Bloom and Davis, 2021; Cullen, Pakzad-Hurson and Perez-Truglia, 2025; Maestas et al., 2023).[3] Our paper adds to this literature by estimating realized changes in specific amenities, and in some cases their valuation relative to pay, due to cyclical changes in the labor market and not just cross-sectional comparisons.

Our study is also relevant for understanding the labor market in the years after the pandemic. Autor, Dube and McGrew (2023) document a compression in the wage distribution, with pandemic recovery wage growth narrowing the college wage premium. However, college-educated workers since 2020 became substantially more likely to work from home (Board of Governors of the Federal Reserve System, 2020; Barrero, Bloom and Davis, 2023), a valuable non-wage amenity. While we show that the tight, post-COVID labor market increased the likelihood of improvement for other amenities for both workers with a bachelor's degree and those with just a high school diploma, the increases were larger in absolute terms for the more-educated. This suggests that the reduction in wage inequality becomes more nuanced when considering broader aspects of job compensation.

Furthermore, the paper adds to our understanding of the benefits of job-to-job flows (or job mobility) and interpreting the Wage Phillips Curve, which at its broadest conceptual level relates labor demand to labor costs.[4] Non-pay characteristics—job amenities—introduce an important additional theoretical mechanism to this literature. Because labor market tightness increases workers' utility not only through real wage increases but also through other job amenities, the benefits to workers of tight labor markets are likely higher than previously thought. Moreover, the inclusion of non-wage amenities into a Phillips Curve model suggests improvements in job quality are possible without a wage-price spiral (Corradini, Lagos and Sharma, 2025), especially to the extent that improvements in non-pay amenities are less subject to erosion from inflation. Such would imply

[3] Mas and Pallais (2017) find the option to work from home is valued at 8 percent of wages and to avoid short-notice schedule changes at 20 percent. Maestas et al. (2023) estimate a willingness to pay of 9 percent to set one's work schedule and 4 percent to have telework opportunities.

[4] Hahn et al. (2017) document the importance of job-to-job flows in explaining temporal variation in wage growth, and Moscarini and Postel-Vinay (2017) argue that job-to-job flows are sufficiently correlated with wage growth that they can serve as a proxy for labor demand. Additionally, Abraham, Haltiwanger and Rendell (2020) and Heise, Pearce and Weber (2026) extend measures of labor demand in the Phillips Curve beyond unemployment rates, similar to our use of job openings.

that the social returns of tighter labor markets may also be higher than previously thought.

The paper proceeds as follows. First, we present a simple framework incorporating non-wage amenities into job quality alongside job transitions. Section 3 describes the data, and Section 4 outlines our empirical methodology. Section 5 presents the results, followed by a discussion and the conclusion.

2 Theoretical Framework

In canonical models of labor demand and supply, the wage is the sufficient equilibrating mechanism. We are interested in another component of compensation, job amenities, and how demand-driven changes in labor market tightness affect this relatively unstudied component. For those already working, increased demand for labor in general could affect the wages, job amenities, or both. It could also move people not working into employment. We provide a simple framework, rooted in job search models, to rationalize how a labor demand shock could affect these margins.

Let an individual i's utility from holding a job j be:

$$U_{ij} = w_{ij} + A_{ij},$$

where w is the wage portion of compensation and A is the (wage-price equivalent) value of amenities. Amenities can include formal benefits such as health insurance or a retirement plan (as in Summers (1989)) but also more intangible options, such as more interesting work or a better work-life balance.

If an individual does not hold a job, utility is b_i. Although this parameter is conventionally treated as the benefit value of unemployment insurance, this need not be the case here. The term b_i captures the flow utility to individual i of not being employed and could include (besides cash payments for a formally unemployed worker) the value of additional leisure time or of caring for and interacting with family members. It thus also could be considered "reservation utility."[5]

[5]The term b_i may also include various costs of working not paid, such as transportation and child care. In this sense, the option of remote work, for example, could be counted, without loss of generality, as an increase in A_i rather

If $w_{ij} + A_{ij} \geq b_i$, the individual is employed at job j and is not employed otherwise. As in Diamond-Mortenson-Pissarides (DMP)-style models, there is an arrival rate of job offers from alternative employers, and a tighter labor market can be characterized by a faster arrival rate. These offers consist of bundles of wage and amenities $\{w, A\}$.

Each of these compensation terms has an individual-specific valuation, and the cost of providing amenities (relative to wages) may differ across employers. In practice, these differences can be driven by different mixes of amenities across jobs—some workers may value certain amenities more than others, and some amenities may be cheaper for employers to provide than others.

Consider first the case of a nonemployed worker. If a new offer arrives such that $w + A \geq b$, the worker will transition into employment. Note that under this two-part compensation structure, it is possible that a tighter labor market could induce more transitions to employment while the average (observed) wage of newly hired workers barely changes if increases in amenities are sufficiently strong. More generally, if wages and amenities are positively correlated (Maestas et al., 2023), observed increases in the wages of newly hired workers will underestimate changes in their utility because of the value of amenities. Conversely, if wages and amenities are negatively correlated because of compensating differentials (Humlum, Rasmussen and Rose, 2025), changes in utility could be overestimated.

Now consider the case of an incumbent worker. Because switching jobs can have psychic or relocation costs, an incumbent worker must pay a cost $p > 0$ when switching jobs. Thus the worker would be willing to switch jobs if, for the new job j', $w_{j'} + A_{j'} \geq w_j + A_j + p$. However, the employer of an incumbent worker can also raise the compensation offer, either in anticipation that the worker will receive a higher offer or in direct response to it. Because of the wedge caused by p—and the ability to adjust the amenities channel—it is possible that workers will reject an offer where $w_{j'} > w_j$. Existing employers need only raise total compensation such that the above inequality no longer holds, and the worker will choose to remain with the original firm.

Suppose there is an increase in labor demand leading to $\{w, A\}$ offers that come more fre-

than b_i.

quently. Which groups of individuals are affected, and which shifts are observable?

First, at the margin, some fraction of nonemployed workers become employed. This is a canonical result of the labor search and matching literature (e.g., Petrongolo and Pissarides, 2001). Several papers have found support for this type of response (e.g., Shimer, 2005; Elsby, Michaels and Ratner, 2015; Hall and Kudlyak, 2020), which can be estimated using either conventional survey data (for example, the Current Population Survey) or administrative data (such as the Quarterly Workforce Indicators, derived from the Longitudinal Employer Household Dynamics).

Second, some fraction of employed workers receive a better compensation offer, but the increase over the current compensation either fails to exceed the switching cost p or the incumbent employer increases compensation—either through margin w, margin A, or both—and the worker stays with the same employer. These workers experience a gain in utility, but quantifying the change requires observing an individual's employer, wage, and job amenities at multiple periods (or their changes between periods). On average, to the extent that wages and amenities are positively correlated across employees within a firm, we would expect both dimensions to increase. For a given employer or worker, however, wage changes may be negligible to large, or even negative, depending on the change in amenities. Thus, even if job-stayers do not see appreciable increases in wages following an increase in labor demand, it does not imply that they received no benefit from a tighter labor market.

Third, another fraction of employed workers receive a compensation bundle that induces them to switch jobs. This scenario is similar to the previous one, but because the increase in the compensation bundle must exceed the threshold $p > 0$ (and any increase in the incumbent employer's possible counter-offer), the increase in utility, $(w' - w) + (A' - A)$, will on average be larger—although again the change in an individual component need not be. Fully quantifying these changes requires observing an individual's employer, wage, and job amenities, as well as *changes* in these quantities over time.[6]

[6] As noted above, although previous research has been able to focus on the first two of these dimensions, very little has explored the third. An exception is Corradini, Lagos and Sharma (2025), who show that a Brazilian union that began advocating for greater female-friendly policies was able to achieve them without employers reducing wages or profits.

Thus, it is an empirically open question how much wages and amenities change for employed workers in response to a labor demand shock, and the responses may be different between workers who stay with their employer and those who change firms. To the extent that different groups of workers have different average valuations for specific amenities, changes in labor demand may also produce heterogeneous observed shifts in job amenities. We now turn to a relatively new data source that allows an empirical investigation of some cases of these amenity changes. Specifically, we examine how labor market tightness affects general labor market outcomes, including employment transitions, pay increases for employed individuals, and amenity improvements for job changers.

3 Data

3.1 Outcomes

Our primary source to measure job amenities is the Federal Reserve Board's Survey of Household Economics and Decisionmaking (SHED). The SHED is a nationally representative sample of approximately 11,000 U.S. adults focused on household finances and well-being that is conducted in October or November, covering information for the previous 12 months.[7] The SHED includes a series of questions about employment, and beginning in 2021 it asked about employed respondents' job quality, with additional questions for those who changed their main job during the prior 12 months. Although we mainly analyze the survey as a cross-sectional snapshot, in some specifications we make use of the partial panel nature of the data, as around one-third of the sample is repeated between two consecutive years. Our analysis includes SHED data from 2021 through 2024.

All respondents are asked whether they worked for pay or profit during the last month, which is our measure of employment status.[8] All respondents also were asked retrospective questions

[7]Board of Governors of the Federal Reserve System (2024). See Appendix Table A.1 for additional detail on questions used in our analysis.

[8]Note that this reference frame of the last month differs from the one-week reference frame in the Current Population Survey (CPS), the source of official U.S. labor force statistics. Although the employment measures in the SHED are thus not fully comparable with those from other sources, the definitions are internally consistent over time and Dasgupta, Shaalan and Zabek (2025) show that employment statistics from the SHED closely match the CPS.

about whether they had applied for a new job, started a new job, voluntarily left a job, or had been laid off or lost a job during the prior 12 months. These questions allow us to relate the tightness of local labor conditions to intended and actual job mobility, a prominent source of changes in job quality (Topel and Ward, 1992; Looze, 2014). Individuals who worked in the month preceding the survey are further asked whether they in the past 12 months had asked for a raise or promotion and, separately, whether they had received a raise or promotion (regardless of whether they had asked for one). These responses allow us to examine the effects of labor market tightness on several job characteristics for most employed workers, regardless of whether they changed jobs or employers, via repeated cross-sections.

The SHED asks specific job amenity questions of job changers, respondents who were employed at the time of the survey, reported starting a new job in the prior 12 months, and reported changing their main job—-either with the same or a different employer—within the past 12 months.[9] These questions ask the respondent to compare each amenity at their current job to the same amenity at their previous job, providing a within-person comparison, albeit only for job changers. The distribution of responses, across years, to this question is shown in Figure 1.

These job changers were asked, "Are each of the following better, the same, or worse at the main job you have now than the one you had a year ago?" for six specific characteristics:

1. "Pay or benefits"
2. "Opportunities for advancement"
3. "Your interest in the work"
4. "Physical demands of the job"
5. "COVID-19 policies and exposure"
6. "Work-life balance"

These respondents were subsequently asked, "Overall, is the main job you have now better, the same, or worse than the one you had a year ago?," which we use as a summary measure of

[9]More specifically, these questions are asked of individuals who said yes to the question (D1A), "Last month, did you do any work for either pay or profit?" and selected, "No, changed jobs" in response to the question (D37), "Is your main job the same as it was a year ago?" Note currently employed individuals who were not employed one year ago are not considered job changers.

changes in job quality. For job changers, we thus have seven measures of changes in job quality.[10] In Figure 2, we show the fraction of our analytic sample, described in more detail below, noting improvement in each characteristic by year.

3.2 Additional SHED Variables

The SHED includes demographic information on the individual's age, race, ethnicity, sex, and education. In some specifications we use this information to control for differences across individuals. In others we use the demographic characteristics to test for differential effects of labor market tightness across groups.

We use an internal version of the SHED with geographic identifiers at the Census tract level to assign Core-Based Statistical Areas (CBSA metropolitan and micropolitan areas) to respondents. Many of our analyses, however, rely on state identifiers, which are available in public-use versions of the SHED. As we describe more thoroughly in Section 3.4, we use this geographic information to assign labor market tightness measures (at the state and CBSA levels) to SHED respondents in each year.

3.3 Sample Construction

We limit our baseline sample to prime-age adults (between the ages of 25 and 54) to focus on individuals with a high propensity for working. Our sample contains between 5,200 and 5,600 respondents each year (Table 1). Our baseline analysis is unweighted and clusters standard errors at the level of variation of the treatment variable because our focus is a plausibly causal effect, not descriptive statistics at the population level (Solon, Haider and Wooldridge, 2015).[11]

In terms of demographics, the unweighted data skew toward the more-educated: slightly

[10] The SHED also asks whether respondents worked remotely or in hybrid arrangements the previous week. However, unlike the amenities just described, these remote work questions are not asked in the form of within-person changes and so do not permit the same analytical design we describe below. Moreover, since we are focused on the impacts of *local* labor market tightness, an outcome of remote work possibly severs the relationship with the relevant geography—we observe the residential location of the individual, not the business location for which labor market tightness may more plausibly influence job amenities. Consequently, our setting is not as conducive to examining changes in remote work as some of the earlier studies cited.

[11] SHED respondents are drawn using a probability-based sample; if we apply inverse-probability weights, results are similar.

under half of the sample have a bachelor's degree, and a further 30 percent have at least some college. The sample is more representative by sex and race/ethnicity: 51 percent are male, 63 percent non-Hispanic White, 11 percent Black, and 16 percent Hispanic.

On average, 18 percent of the sample reported starting a new job, 17 percent asked for a raise, and 43 percent received a raise. These three measures all rose between 2021 and 2022 and then fell in both 2023 and 2024.

Focusing on our main outcomes on job quality among job changers, respondents who said they had changed main jobs were asked the subsequent job quality questions comparing their new main job to the one 12 months prior. We code responses as 0 for all respondents who were not eligible to be asked these questions; thus the means are calculated over all sample respondents.[12] Around 11.5 percent of respondents between 2021 and 2024 experienced a main job change in the previous 12 months. Just under 8 percent of individuals experienced an increase in overall job quality, implying that 69 percent (0.079/0.115) of those changing jobs saw a quality increase over this period. Other job quality measures also improved on net during our sample period, led by pay and benefits, with 7 percent of individuals experiencing a gain. Job changes (and quality improvements) were most common in 2022 and 2023 (see also Figure 2).

Due to the SHED sampling design, the same individuals can appear in the SHED in multiple years. We find 507 respondents in all four years, 1,299 in three years, 2,766 in two years, and 10,088 in only a single year (Appendix Table A.2). We describe below how we address the partial panel nature of the data.

3.4 Key Tightness Measures

Our primary measure of labor market tightness is the annual number of vacancies per thousand people measured at the state level. We take vacancies from the monthly Jobs Openings and Labor Turnover Survey (JOLTS), conducted by the Bureau of Labor Statistics, which collects information from establishments on their job openings, hires, and separations. Job openings, or vacancies,

[12] We made this choice, rather than code ineligible respondents as missing, because—as we will show—the main job change itself is endogenous to labor market tightness. Our coding scheme thus implicitly captures extensive and intensive margins.

are positions that are open on the last day of the reference month, could start within 30 days, refer to a specific opening with work available, and are being actively recruited for outside the establishment. We aggregate monthly-level job openings or vacancies to an annual level to match the fielding horizon of the SHED. Specifically, we calculate the average number of monthly vacancies in each state from November of year $t-1$ through October of year t to match SHED year t. We divide this annual vacancy count by the total state population from the U.S. Census Bureau's annual population estimates.[13] We use total population rather than the size of the labor force in the denominator because the latter measure is potentially endogenous to labor market tightness (Cajner, Coglianese and Montes, 2021).

We explore the robustness of our estimates to alternative constructions of labor market tightness. While JOLTS asks a representative sample of establishments about their job openings, we also take advantage of job vacancy data from the labor market analytics company Lightcast, which compiles the near-universe of online advertised job openings on a rolling basis. There are key differences in the two measures of job openings. First, Lightcast openings are limited to those advertised online, while JOLTS includes openings that are being actively recruited for, regardless of advertising method. Second, Lightcast data capture job openings throughout the month, while JOLTS focuses on openings available on the last business day of the month. Third, JOLTS requires an opening to have specific work available for which a candidate could start within 30 days, whereas the Lightcast data do not have this restriction. Fourth, JOLTS is meant to capture the number of openings, while it is not always clear whether a single advertised position in Lightcast represents one or several openings. (For simplicity, we generally refer to the JOLTS measure as "openings" and the Lightcast measure as "vacancies," and we name the source.)

Even with these differences, we believe the Lightcast data provide a useful supplement to JOLTS in our analyses, as they provide an independent measure of labor demand to compare to JOLTS at the state level. Additionally, they are available at a greater level of geographic detail than JOLTS—including metro areas and not just states—allowing us to examine the effects of tightness

[13] See https://www.census.gov/data/tables/time-series/demo/popest/2020s-state-total.html. The population estimates cover a calendar year, so they do not exactly match the time period of the SHED and JOLTS.

measured in more localized labor markets.

We construct annualized Lightcast openings per thousand people analogously to the JOLTS data. We take the average monthly openings within a geography (state or CBSA) over the SHED-year months and divide them by the total population of the state (or CBSA) from the Census Bureau.

During our sample period, state-level labor market tightness generally increased between 2021 and 2022 but decreased over the next two years, and this pattern holds in both JOLTS and Lightcast (Figures 3 and 4). The years with greater labor market tightness match those when job changes and job improvements as measured in the SHED were most common (Figure 2). Appendix Table A.3 shows additional information on the distribution of labor market tightness measures from JOLTS and Lightcast, including at the CBSA level.

4 Methodology

The research literature on labor market shocks and worker outcomes is extensive, and several different measures of labor market tightness have been employed. Many of these measures are based on cyclical changes in job counts (e.g., Hershbein and Stuart, 2024) or unemployment rates (e.g., Yagan, 2019), with an older literature examining longer-horizon (decadal) changes of the same (e.g., Bartik, 1991; Blanchard and Katz, 1992; Bound and Holzer, 2000). The recent availability of granular job vacancy data has allowed measures historically restricted to national timeseries in macroeconomic job search applications—such as the ratio of vacancies to unemployed (θ or V/U) (e.g., Shimer, 2005; Hall, 2005; Barnichon and Figure, 2015)—to be applied in local labor market contexts. The use of vacancies has the desirability of conceptually being closer to shifts in labor demand that are exogenous to supply, unlike the equilibrium measures of changes in job counts or the supply-centric measure of changes in unemployment rates. Indeed, Bartik (1991) and Blanchard and Katz (1992) employ shift-share instruments specifically to address concerns of endogenous labor supply response over the long horizons they analyze, while more recent papers, such as Hershbein and Stuart (2024), appeal to the shorter time horizons around recessions as primarily being labor-demand driven. The canonical labor tightness measure of V/U is predicated on the number of job opportunities per person searching for work, but its implementation is complicated by job search among both the employed and those outside of the labor force, as well as the fraction of the unemployed expecting to be recalled and thus not actively searching for work (Bloesch, Lee and Weber, 2025; Heise, Pearce and Weber, 2026). Indeed, Forsythe et al. (2022) show that these concerns affecting the denominator were especially salient during the COVID pandemic and recovery and that failure to adjust for changes in "true" job seekers led to biased measures of labor market tightness.

For all these reasons—and the empirical concern of measurement error in the unemployment rate for subnational geographies—we prefer a tightness measure based primarily on vacancies, albeit normalized to the relevant population.[14] As we have shown above, this measure varied considerably over time and across areas—and for plausibly exogenous reasons—during the post-COVID period we study. We thus adopt a difference-in-differences approach, which we describe in more detail below, as our primary estimation strategy. However, it is possible that firm decisions to post vacancies may themselves depend on changing labor supply considerations in the years following the pandemic, and thus we also employ a shift-share design as a supplementary approach.

4.1 Analytical Approach

Our primary approach exploits variation in labor market tightness over time and across geographies. Specifically, we estimate the following equation:

$$y_{igt} = \alpha + \beta Tightness_{gt} + X_{igt}\gamma + \theta_g + \varepsilon_{igt}. \tag{1}$$

In this equation, the left-hand-side variable y_{igt} denotes a given job outcome for individual i in geography g in year t. The level of geography depends on the particular source being used for labor market tightness: state for models using JOLTS data, and either state or CBSA for models using Lightcast data. The outcomes are those described in Section 3.1, including a) those asked of all

[14]Recently, another measure, the ratio of quits to layoffs—the labor leverage ratio—has been proposed (Sojourner and DeVito, 2022), but quits and layoffs are not captured for subnational geographies.

respondents (on job transitions over the last 12 months), b) those asked of the subset who reported working in the previous month (on raises and promotions), and c) those asked of the additional subset who reported main job changes in the previous 12 months (on *changes* in specific amenities). While the first two subsets of outcomes are essentially estimated on repeated cross sections, the last set, because of the within-person-change nature of the questions, implicitly includes person fixed effects.[15]

Our coefficient of interest is β, the effect of labor market tightness on job quality. Our preferred measure of labor market tightness is the natural log of job openings per capita. A vector of control variables, X_{igt}, includes a quartic in age and indicators for gender, race, and education. θ_g represents geography fixed effects, and ε_{igt} represents idiosyncratic errors that we allow to be correlated within geography. Our baseline specification does not include year fixed effects, as aggregate time variation in labor market tightness during the COVID recovery was substantial, although we examine in robustness exercises the sensitivity of our estimates to focusing on within-year variation only.

4.2 Shift-Share Instrument

While much of the variation in job openings coming out of the pandemic was related to plausibly exogenous shocks, openings may still be influenced by other endogenous processes. For instance, there were supply-side considerations related to local health impacts of the pandemic, increased governmental benefits, and lingering public health concerns in certain industries. While variation due to exogenously induced changes in workers' willingness to stay in particular jobs is relevant for identifying effects of labor market tightness, using these supply-side factors complicates the interpretation of our identifying variation as coming specifically from shocks to labor demand (and possibly limiting the relevance of the findings to future, non-pandemic contexts).

Thus, to confirm our estimates are driven by exogenous variation in labor demand, we also employ a shift-share instrumental variables approach. Specifically, we project plausibly exoge-

[15] Because approximately 26 percent of our SHED sample participates in the survey in two consecutive years, we can implement for this subset a panel analysis for the two first sets of outcomes, as well. We return to this supplementary analysis below.

nous shifts in national, industry-level employment to the state (and CBSA) level using preexisting industry employment shares (Bartik, 1991; Borusyak, Hull and Jaravel, 2022).

Specifically, we construct predicted changes in employment, z_{jt}, for state j and year t using employment counts in 94 exhaustive three-digit industries k covered by state-level data from the Quarterly Census of Employment and Wages (QCEW). That is, we calculate the employment share of industry k in a prior period, year 2019, multiply this share by the one-year change in *national* employment in industry k (leaving out the focal state j) between year $t-1$ and t, and then aggregate across industries k:

$$z_{jt} = \sum_{k=1}^{K} \underbrace{\frac{L_{jk2019}}{L_{j2019}}}_{\text{Industry share}} \underbrace{\frac{L_{-jkt} - L_{-jkt-1}}{L_{-jkt-1}}}_{\text{Shifter}}. \tag{2}$$

The validity of this approach rests on the exogeneity of changes in national, industry-level employment from 2020 to 2024 (Borusyak, Hull and Jaravel, 2025).[16] Fortunately for our identification, the COVID pandemic and aftermath provide a situation where much of the industry-level variation in labor demand came from shocks to final-goods demand and to supply chains stemming from events related to the pandemic. Lockdowns and concerns over disease exposure in public places changed consumption patterns, increasing demand for goods that can be consumed from home. As the pandemic progressed, supply chain shocks led to bottlenecks for different industries at different times (Harapko, 2025), which affected production patterns. Then, during the recovery, consumption shifted to travel and hospitality as individuals wanted to make up for travel missed during the pandemic.

Our identification is more plausible when our measures of labor market tightness are uncorrelated with other factors likely to influence changes in job quality.[17] Appendix Table A.4 presents

[16]We create an alternative shift-share instrument using long differences between 2019 and the focal year in national JOLTS openings which provides qualitatively similar results to our main shift-share analysis (Appendix Table A.24). This approach has the advantage of directly reflecting national changes in our variable of interest, but it has several shortcomings. First, it may not represent exogenous demand factors because it may capture replacement hiring (possibly in response to changes in workers' preferences about working in different industries during the pandemic). Second, it limits us to a coarser set of 15 industries. Finally, it does not allow us to calculate a measure that leaves out the focal state in the national shift calculation.

[17]More formally we require that the instrument z_{jt} be uncorrelated with the error term in our main regressions ε_{igt}.

pairwise partial correlations between each measure of tightness—JOLTS openings, Lightcast vacancies (CBSA level), and the shift-share instruments—and various demographic characteristics relevant for labor market outcomes. These correlations are reassuringly small for the JOLTS and Lightcast measures, with some minor exceptions (racial shares for JOLTS and education shares for Lightcast are between 0.1–0.2). Indeed, Brave, Scott, and Erin Crust and Stefano Eusepi and Bart Hobijn and Ayşegül Şahin (2026) find that changes in JOLTS openings between 2021 and 2023 were driven more by labor demand than supply factors, with the majority of variance in JOLTS openings also attributable to labor demand factors. The correlations with our shift-share instruments are uniformly at or below 0.03. These low correlations are consistent with the unpredictable nature of the demand shocks arising from COVID that we describe above and reinforce that our approach captures labor market tightness induced by labor demand shocks.

5 Results

5.1 Main Results

5.1.1 General labor market outcomes

We first show that labor market tightness improves general job market outcomes measured for all (prime-age) SHED respondents, regardless of employment status. In Table 2, we find that a 10 percent increase in JOLTS job openings per thousand state residents leads to a 0.5 percentage point increase in the probability of applying to a new job, a 0.7 percentage point increase in the probability of starting a new job, a 0.8 percentage point increases in the probability of quitting a job, and a 0.8 percentage point increase in the probability of changing jobs.[18] These effects are statistically and economically significant relative to the mean incidence of each outcome, with increases of 1.6, 3.9, 7.0, and 6.7 percent, respectively. As expected, higher openings also correlate with a lower probability of being laid off, with a 0.3 percentage point (4.3 percent) reduction per

Evidence that individual characteristics are uncorrelated with labor market tightness measures supports this necessary assumption.

[18]For simplicity, we report a 10 percent change as 0.1 times our estimated coefficient, but technically the effect is equal to ln(1.1), or 0.095, times the coefficient.

10 percent increase in openings. Tighter labor markets also predict a greater likelihood of asking for and receiving a pay raise. These findings suggest that our measure of labor market tightness correlate reasonably with labor market dynamics for the prime-age population. Additionally, they suggest that the effects of labor market tightness on job quality could extend to workers even if they do not change jobs, consistent with the theoretical predictions discussed above.

5.1.2 Outcomes for job changers

In Table 3, we show that tighter labor markets also improve job amenities among individuals who changed jobs in the 12 months preceding the survey.[19] In panel (a), which uses state-level JOLTS openings, we find that a 10 percent increase in labor market tightness induces a 0.9 percentage point increase in the likelihood of changing to a better job overall. This effect is large relative to the sample mean of 7.9 percent (calculated across all prime-age individuals, not just those who changed jobs and thus could report an improvement in job quality). The implied elasticity of changing jobs and experiencing an improvement in job quality with respect to per-capita job openings is 1.1 ($\hat{\beta}/[P(Y=1)] = 0.089/0.0787$).

We find similar effects from a 10 percent increase in job openings per capita on improvements in pay and benefits (0.9 percentage points) and in advancement opportunities (0.8 percentage points). Estimated improvements in interest in the work and work-life balance are slightly smaller, at 0.7 and 0.6 percentage points, respectively.[20] When we scale the percentage-point estimates by the dependent variable means, the proportional effects are similar across outcomes. A 10 percent increase in job openings corresponds to a 14 percent increase in the likelihood of an improvement in pay and benefits,[21] a 12 percent increase in interest in the work, an 11 percent increase

[19]The sample includes all individuals ages 25–54, but the binary dependent variable takes the value of 1 only for individuals who reported changing their main job in the 12 months prior to the survey AND who reported an improvement in the designated characteristic for the new job relative to the previous job; the outcome variable is 0 for all others, including those who did not change jobs or are without jobs.

[20]These effects are generally statistically indistinguishable from each other, although the effect on pay and benefits is statistically larger (at 5 percent) than the effect on work-life balance.

[21]Improvement in pay is an outcome we can also examine in the CPS. Using a sample with the same age restrictions in the SHED, and focusing on the outgoing rotation group sample of workers who are asked their wages 12 months apart, we estimate that a 10 percent increase in job openings is associated with a 0.85 percentage point—or 14 percent—increase in the likelihood of reporting a nominal increase in wages, a virtually identical estimate to what we find in the SHED.

in work-life balance, and a 14 percent increase in advancement opportunities. Tightness's effect on improvements in physical demands is smaller in both absolute and proportional terms, at 0.3 percentage points and 7 percent, respectively, but it remains statistically different from zero.

The last column of Table 3 repeats the estimated effect of greater job openings on the probability of changing main jobs from column (5) of Table 2, as changing jobs is a necessary requirement for any of the previous amenities to be coded as an improvement. The relative magnitude of tightness on the likelihood of changing jobs compared with those on amenity improvements suggests that much, but not all, of the improvements in job characteristics come from the marginal inducements to job changing from the tighter labor market, which we explore further in Section 6.1.

The relationship between labor market tightness and job amenities remains when we use a conceptually similar but completely independent measure of tightness: job postings from Lightcast. As shown in panel (b) of Table 3, this alternative measure yields the same quantitative pattern between labor market tightness and improvements in job amenities. Although the point estimates using Lightcast data are generally smaller in magnitude than those using JOLTS, the corresponding estimates between the two measures are not statistically different from each other.[22]

The Lightcast data also allow us to use CBSA-level variation in labor market tightness to examine changes in job quality. CBSAs may represent local labor markets more accurately than do states, and labor market tightness could vary across CBSAs within the same state. As shown in panel (c) of Table 3, we find that switching to a smaller geography (and controlling for CBSA fixed effects) again yields similar, albeit slightly smaller, improvements in job amenities. We estimate that a 10 percent increase in CBSA-level labor market tightness corresponds to a 0.6 percentage point increase in the likelihood of moving to a better job overall, with slightly smaller effects on the other specific amenities.[23]

[22]As shown in Appendix Table A.5, the correlation between the two labor market tightness measures at the state-year level, after controlling for state fixed effects, is 0.70.

[23]Although we don't take a strong stand on why the CBSA-based estimates in panel (c) are slightly smaller than the state-based estimates in panel (b), especially since these differences are generally not statistically different, we note that migration is a possible contributing factor. If individuals move for better jobs from weaker labor markets to stronger ones within a state, the CBSA-based estimate would be biased down relative to the state-based estimate.

5.2 Robustness to Alternative Specifications

The estimates in Table 3 use both temporal and cross-sectional variation in tightness to identify job amenity changes for job changers. Although our preferred specification includes state fixed effects and controls for individual characteristics, we test the sensitivity of our results to alternative controls that isolate different sources of variation in Appendix Tables A.6 to A.12. Column 4 of each table includes individual fixed effects, exploiting the limited panel nature of the SHED. This (double-)within-person specification with time fixed effects effectively changes the identifying variation to cross-sectional geographic differences alone accounting for possible workforce composition changes.[24] Compared to the baseline estimates, including individual-level fixed effects tend to produce point estimates that are roughly one-third larger. These estimates remain statistically significant at conventional levels, even with substantial reductions in sample size.[25]

Columns 5 and 6 add fixed effects for year and year-by-Census division, respectively. Inclusion of year fixed effects substantially attenuates the estimates—often making them statistically different from our baseline estimates and indistinguishable from zero—reflecting that most of the post-COVID variation in labor market tightness comes from national temporal variation rather than cross-sectional geographic variation. Including more-flexible year-by-division fixed effects, which implicitly compares residents across states within a Census division in the same year, also reduces the magnitude of the estimates but generally not enough to make them statistically different from the baseline estimates. Thus, our main results are robust to additional controls as long as we do not net out the important variation over time in the post-COVID labor market.

Appendix Tables A.13–A.19 report similar sensitivity results using our Lightcast CBSA-level measures of job vacancies from panel (c) of Table 3. Baseline results appear in column 3 of each table. For the CBSA-level regressions, we can restrict the sample to metro areas only, which arguably have more-complete labor markets than the micro areas that were also included

[24]The baseline specification from Table 3 is already within-person because the outcome captures amenity differences between jobs for the same individual; the person fixed effects here control for pre-existing trends, within individual, of amenity changes from switching jobs.

[25]Given the larger standard errors from the sample size reduction, we cannot reject that that estimates with individual fixed effects are the same as the baseline estimates.

in the baseline estimates. The metro area-only estimates (column 4) are nearly identical for each outcome, despite the exclusion of over 300 micropolitan areas. Inclusion of individual fixed effects in the CBSA-level regressions (column 5) once again increases point estimates by about one-third, while adding year fixed effects (column 6) once again yields economically small and statistically insignificant estimates. In contrast to the JOLTS estimates at the state level, however, estimates here remain small and insignificant when including year-by-division fixed effects (column 7). This suggests that the importance of aggregate temporal variation for our baseline estimates is even greater for the Lightcast-based measures than for the JOLTS-based measures.

Our main specification uses the *log* of job openings per capita, as amenities may plausibly respond to proportional changes in job openings. Appendix Table A.20 shows that using job openings per thousand residents in levels produces similar results to panel (a) of Table 3. A 10 percent increase in job openings per capita at the mean is associated with a 1.0 percentage point increase in the likelihood of changing to a better job ($0.0034 \times 0.1 \times 28.7 \approx 0.01$; see Table A.3).[26]

Finally, we assess whether the results could be driven by an Ashenfelter-type dip among recently laid off workers experiencing a decline in their job quality before separating. In this case, a reversion to the mean upon taking a new job coinciding with the COVID recovery would lead to spurious findings. Appendix Table A.21 replicates panel (a) of Table 3 but excludes the 7 percent of respondents who experienced a layoff. The results remain nearly identical to our baseline.[27]

5.3 Shift-Share Results

Using our shift-share instruments to isolate changes in labor demand yields results similar to the main specifications, but with somewhat larger magnitudes.[28] Panel (a) of Table 4 shows estimates for the general labor market outcomes available for our entire SHED sample. Instrumented labor

[26]We prefer the estimates using log changes for two reasons. First, they minimize the influences of outliers in our tightness measure. Second, a fixed level change in a less-tight labor market intuitively yields a larger increase in the probability of job amenity improvements than the same level change in a tighter labor market.

[27]An additional concern is that individuals may move toward tighter labor markets. Excluding from the analysis the small share of individuals who moved states leads to estimates nearly identical to our baseline.

[28]We follow Andrews, Stock and Sun (2019) and Olea and Pflueger (2013) in reporting effective first-stage F-values and critical values. Our effective first-stage F-statistics are well above critical values, ruling out weak-instrument concerns.

market tightness leads to more job applications, job starts, job changes, and raises, as well as fewer quits. The estimated coefficients are roughly double the magnitude of their counterparts using non-instrumented measures of JOLTS job openings.

Using the instrumented tightness measure also increases our estimates of tightness effects on changes in job amenities, albeit more modestly. Panel (b) of Table 4 shows that a 10 percent increase in instrumented tightness raises the likelihood of moving to a better overall job, or to one with improved pay and benefits by 1.4 percentage points. Effects on other amenities, as well as on the likelihood of changing jobs, are all positive, statistically significant, and at least 50 percent larger than the corresponding non-instrumented estimates in panel (a) of Table 3.

For both panels, the differences between the baseline and instrumented estimates are sometimes statistically significant. This suggests that the estimates based on raw (log) changes in job openings per capita may capture both demand-driven and labor-supply factors (such as population aging or job-skill mismatch) that affect labor market tightness, and thus be conservative for the true effect of labor demand on job changes and amenity improvements.[29]

5.4 Heterogeneous Effects across Worker Groups

Previous research has found that women are more likely than men to work in jobs with better non-wage amenities (Morchio and Moser, 2024) and have a greater willingness to pay for workplace flexibility (Mas and Pallais, 2017; Wiswall and Zafar, 2017). Motivated by these patterns, we also estimate the effects of labor market tightness on job amenities separately by gender. As shown in Table 5, our baseline specification using JOLTS openings suggests that tightness has similar effects for men and women, with point estimates that are quite close and statistically indistinguishable for each outcome. These results need not be inconsistent with differential valuation of job amenities for women and men; rather, they suggest that tighter labor markets post-COVID did not result in appreciably greater improvements in job amenities for women vis-à-vis men.[30]

[29] Appendix Table A.24 shows results using an alternative shift-share instrument based on national changes in two-digit industry job openings and state-level industry employment shares. The estimates are close to the non-instrumented estimates (Tables 2 and 3), which may reflect that the instrument still captures labor-supply factors, such as the need for replacement hiring.

[30] Appendix Table A.23 shows that the same inference holds for the labor market dynamics outcomes from Table 2.

During the COVID recovery, demand for in-person, service-sector workers rose sharply, suggesting that less-educated workers may have faced an especially tight labor market (Autor, Dube and McGrew, 2023). At the same time, increases in remote work were concentrated among more-educated workers (Barrero, Bloom and Davis, 2023). Motivated by these differences in labor market experiences, we explore heterogeneity by education.[31] Tables 6 and 7 replicate Tables 2 and 3, separately for two education groups: respondents with at least a bachelor's degree in panel (a), and those with a high school degree or less in panel (b).[32]

Although tighter labor markets generally increased job mobility for both education groups—particularly in the likelihood to quit, change jobs, or ask for a raise—there are also a few notable differences (Table 6). Individuals with a bachelor's degree were less likely to be laid off when labor markets were tighter (0.6 percentage points per 10 percent increase in job openings, or a 12 percent reduction), while less-educated workers saw no comparable improvement; this difference is statistically significant. More-educated respondents also were (statistically) more likely to receive a raise than less-educated respondents. A similar pattern appears in Table 7: both groups report improvements in job amenities in tighter labor markets, but the percentage point increases are larger for more-educated workers, particularly for overall job quality and pay.[33]

At first glance, these findings may appear inconsistent with the narrative that the COVID recovery was especially beneficial to lower-wage (and lower-education) workers. However, scaling the percentage point effects by the (group) dependent variable means yields a more nuanced picture. Respondents with a high school education are, in absolute terms, less likely to work, to change jobs, and to experience improvements in their job amenities when they do. Yet, in *proportional* terms, less-educated job changers experienced similar gains from increased tightness on both the likelihood of changing a job and the job having better characteristics.[34] We note that

[31] While it is possible, with Lightcast data, to construct education-specific measures of labor market tightness (job postings), the findings of Abdelfattah and Caiumi (2025) suggest that endogenous education requirements would make interpretation challenging at best.

[32] We exclude individuals with some college or an associate's degree to strengthen the educational comparison.

[33] The point estimates for interest in the work, work-life balance, and advancement opportunities are larger for the more-educated, but these differences are not statistically significant.

[34] None of the effects scaled by the dependent mean (which accounts for group differences in both job-change rates and amenity-change rates) were statistically different at the 5 percent level between education groups.

these results are not incompatible with Autor, Dube and McGrew (2023), who document stronger wage growth for less-educated workers during the recovery. Because the SHED does not measure the *magnitude* of pay (and benefits) increases, it remains plausible that less-educated workers saw larger increases in pay, even if they were not *more likely* to see wage gains. Indeed, when we use the CPS to examine wage changes (as in footnote 21), we find statistically larger wage gains for less-educated workers. Our findings thus suggest that the reduction in wage inequality found by Autor, Dube and McGrew (2023) may not fully extend to total compensation inequality when the value of job quality and amenities are included.

6 Discussion

6.1 Mechanisms and Margins

Our estimated effects of labor market tightness capture effects at both the extensive margin—the improvement in amenities that comes from increased likelihood of changing a job during tighter labor markets—and the intensive margin—the *additional* improvement in job amenities, conditional on changing a job, when labor markets are tighter.[35] Although we cannot precisely decompose these channels, we estimate our main specification on the sample of job changers—those explicitly asked about changes in job amenities—to better understand the intensive margin (Table 8). While the results are descriptive, they add useful context on how workers experienced job improvements from tighter labor markets.

Among job changers, 69 percent report an improvement in overall job quality. We estimate that a 10 percent increase in labor market tightness leads to a 3 percentage point increase in the likelihood of improved overall job quality—an increase of 4.4 percent relative to the mean. The total tightness effect is an 11.3 percent increase (column 1, panel (a) of Table 3). A back-of-the-envelope calculation suggests roughly three-fifths of the overall improvement in job quality from a tighter labor market is from increased likelihood of switching jobs, with the remaining two-fifths

[35]This follows because our dependent variables for amenities are coded "1" only for individuals employed at the time of survey *and* who reported changing jobs in the previous 12 months.

due to a greater likelihood of quality improvement conditional on changing jobs.[36]

As described in Section 2, job changers include those who changed employers and those who changed jobs within the same firm; job amenities of these groups could be affected differently. Around one-quarter of our job changers remained with the same employer. We run our baseline specification on the subsample of each *type* of job changer in Appendix Table A.22 (we can separate the types only for years 2022–2024). Although the estimates are noisy due to small sample sizes, the estimated magnitudes across subsamples are generally similar, suggesting that job changers who did not change employers also benefited from greater labor market tightness via improved job amenities.

6.2 Which Amenities Matter Most for Changes in Overall Job Quality?

Our results show that tight labor markets lead to greater rates of job changing and better job amenities for job changers. However, a better understanding of the role that tightness-driven changes in each amenity play in an individual's assessment of changes in their job quality is important both for researchers (who typically focus on wages) and policymakers seeking to create better jobs.

To quantify these relationships, we multiply the effect of labor market tightness on each amenity from our main specification (Table 3) by an estimate of how improvements in each characteristic relate to changes in overall job quality. We estimate this latter quantity by regressing an indicator for a better job overall on indicators for improvements in each individual job characteristic. Each coefficient represents the average change in likelihood that the job is better overall when that individual amenity is stated to have improved, holding other job characteristics fixed.[37]

Because these estimates use a sample of job changers, they are subject to non-random selection into job transitions and omitted variable bias from other unmeasured characteristics. Following Cullen, Pakzad-Hurson and Perez-Truglia (2025), we assess and mitigate these concerns

[36] The same inference follows from a more formal approach that sets $P(Q_{improve}) = P(\Delta_{job}) \cdot \Delta(Q_{improve}|\Delta_{job}) + \Delta P(\Delta_{job}) \cdot P(Q_{improve}|\Delta_{job})$. The left-side term is the estimate from column 1, panel (a), Table 3, the first term on the right is the mean from column 7 of the same table, the second term is from column 1 of Table 8, the third term is the estimate from column 7 of Table 3, and the last term is recovered through algebra.

[37] This exercise updates an analysis from Lim and Zabek (Forthcoming) while Cullen, Pakzad-Hurson and Perez-Truglia (2025) present a similar analysis using a different survey of job changers.

in three ways. First, we implicitly control for invariant characteristics of individuals by examining job changes. Second, we gauge the importance of omitted factors by including some as controls and checking the sensitivity of the focal coefficients. Third, Lim and Zabek (Forthcoming) examine differences in relationships between job characteristic improvements and self-assessed overall job quality changes by demographic subgroups and find limited variation across worker groups. Together, these checks suggest bias should be small and that this exercise can be informative.

The job amenity most strongly associated with overall improvements in job quality is interest in the work, followed by pay and benefits, and then work-life balance (Table A.25). Improved opportunities (e.g., advancement) have a smaller effect, and physical demands are essentially uncorrelated. The results mirror those previously reported in Lim and Zabek (Forthcoming) and are quite stable across different sets of controls, suggesting that selection and omitted variable bias are unlikely to be major concerns.

Multiplying these estimates by those in Table 3 yields an estimated net effect of a labor-demand driven amenity change on the change in overall job quality. We show these effects in Table 9, in absolute and proportional terms, as well as the contribution relative to pay and benefits. Improved pay and benefits explain roughly 20–25 percent of the overall improvement in job quality driven by labor market tightness. While this share is sizable, it still accounts for only a small fraction of labor-market-driven improvements in overall job quality. Improvements in interest in the work are of similar importance to pay and benefits in worker assessments of job quality, and this holds across all three measures of labor market tightness. While the percentage point effect of tight labor markets on interest in the work is smaller than for pay and benefits (Table 3), the stronger relationship between job interest and overall job quality (Table A.25) raises the relative importance of this amenity in how labor market tightness drives change in overall job quality.

Other characteristics play a more modest role in Table 9. Despite a strong media interest in work-life balance during the post-COVID period, we attribute less than 15 percent of improvements in job quality to better work-life balance. Changes in opportunities for advancement represent roughly 10 percent, and changes in physical demands have minimal effects.

7 Conclusion

In this paper we investigate how labor market tightness affects self-assessed changes in job dynamics and amenities between 2020 and 2024, a period when national labor market conditions fluctuated and varied substantially across years and localities.

We find that higher levels of job vacancies per capita increased the likelihood of changing jobs, and of seeing improvements in pay and benefits, increased interest in the work, better opportunities for advancement, and higher overall job quality in the new jobs. The magnitudes of amenity increases in response to tighter labor markets are meaningful: a 10 percent increase in tightness increases the probability of a job amenity improvement for all prime-age individuals (regardless of their employment) by between 7 and 20 percent, depending on amenity and specification. The implied elasticities between labor market tightness and amenity improvement are thus between 0.7 and 2.0, and centered around 1.1.About three-fifths of the amenity improvement from a tighter labor market can be accounted for by the greater likelihood of changing jobs, leaving two-fifths of the improvement conditional on changing jobs. The improvement elasticity conditional on changing jobs is similar in magnitude to a wage elasticity of tightness across all employed individuals that we computed from the CPS (among those employed 12 months apart) of about 0.4.[38] Moreover, we find suggestive evidence that the improvement in overall job quality induced by tight labor markets operates through greater interest in the work at least as much as through greater pay and formal benefits, highlighting the importance of non-pecuniary job amenities.

Our results are robust across two distinct measures of labor market tightness from different data sources, and to measuring local labor markets at the state or CBSA level. When we use a shift-share IV framework to isolate the contribution of labor demand to changes in job vacancies, the estimates are at least as large and remain statistically different from zero.

We do not find differences in the responsiveness of amenity changes to labor market tightness between men and women, despite evidence of differential preferences in amenities between

[38]This comparison isn't exact: two-fifths of 1.1 is just over 0.4, but this statistic is calculated over job changers, while the wage elasticity of 0.4 requires only that the worker be employed 12 months apart, not necessarily for different jobs or employers.

the sexes (Morchio and Moser, 2024). Interestingly, we *do* find (weakly) stronger amenity responsiveness among individuals with at least a bachelor's degree, although *proportional* responses are more similar across education groups.

Our findings are relevant for policymakers and several strands of economics research. First, existing evidence that tight labor markets benefit workers is based on increased job finding rates and wage increases rather than improvements in job quality through non-wage amenities. The strong connection that we find between tightness and non-wage measures of job quality suggests that a focus on wages or employment propensity alone understates the welfare benefits of tight labor markets. Second, a key hypothesized drawback to wage increases resulting from tight labor markets—realized in the post-COVID episode—is that some proportion of those increases may be eroded by higher inflation. This drawback need not exist for non-wage measures of job quality. Finally, our result that the tight labor market after COVID increased job amenities somewhat more for individuals with a bachelor's degree than those with less education suggests that the post-COVID wage compression documented in Autor, Dube and McGrew (2023) may be more nuanced when considering broader measures of worker utility.

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Table 1: Means of outcomes and controls

	Overall	2021	2022	2023	2024
Demographics					
Female	0.489	0.497	0.485	0.496	0.480
Black	0.113	0.102	0.117	0.113	0.120
White (non-Hispanic)	0.627	0.651	0.632	0.622	0.603
Hispanic	0.162	0.151	0.157	0.166	0.175
Asian	0.053	0.053	0.051	0.054	0.055
High school or less	0.210	0.204	0.203	0.213	0.221
Some college, no BA	0.296	0.289	0.296	0.304	0.295
BA or more	0.494	0.507	0.500	0.483	0.484
Age	39.4	39.0	39.5	39.3	39.6
Main outcomes					
Changed main jobs in the past 12 months?	0.115	0.099	0.133	0.122	0.105
Improved job overall	0.079	0.068	0.100	0.083	0.065
Improved pay and benefits	0.068	0.057	0.089	0.070	0.057
Improved interest in work	0.060	0.051	0.076	0.064	0.049
Improved work-life balance	0.050	0.042	0.061	0.056	0.040
Improved opportunities	0.056	0.048	0.073	0.061	0.044
Improved physical demands	0.037	0.031	0.044	0.039	0.034
Supplementary outcomes					
Employed	0.776	0.792	0.790	0.768	0.757
Started a New Job	0.180	0.176	0.196	0.183	0.166
Quit	0.115	0.116	0.130	0.116	0.098
Laid Off	0.073	0.078	0.061	0.074	0.078
Asked for a raise	0.168	0.142	0.186	0.175	0.170
Received a raise	0.432	0.404	0.455	0.443	0.426
Observations	21,545	5,415	5,303	5,265	5,562

Note: This table provides (unweighted) variable means from the analysis sample of people ages 25–54, overall and by year. For the block "Main Outcomes," respondents were asked whether they had changed *main* jobs only if they were employed both in the reference month of the survey and reported they had changed *any* jobs within the prior 12 months; they were asked the subsequent job quality questions only if they reported changing their main job. We code responses as 0 for all respondents who were not eligible to be asked these questions; thus, the means are calculated over all sample respondents. (The probability of reporting overall job improvement, conditional on changing main jobs, is $0.079/0.115 = 0.687$.)
Source: Authors' calculations from the 2021–2024 waves of the Survey of Household Economics and Decisionmaking.

Table 2: Effects of labor tightness on labor market dynamics for the prime-age working population

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Asked raise
Logged per-capita openings	0.047	0.071	0.081	-0.031	0.077	0.124	0.096
	(0.015)	(0.022)	(0.018)	(0.012)	(0.018)	(0.046)	(0.035)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.296	0.180	0.115	0.073	0.115	0.432	0.168
Individual controls	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X
Increase in CPI						X	X

Note: This table provides estimates of the natural logarithm of JOLTS job openings per capita, at the state-year level, on individual labor dynamic behavior of prime-age SHED respondents. The outcomes are indicators for a specific behavior over the 12 months prior to the SHED survey: "Applied" is having applied for a job; "Started" is having started any new job; "Quit" is voluntarily having left a job; "Laid-off" is having been laid-off from a job; "Changed" is having starting a new job, being currently employed, and having a different job 12 months prior; "Got raise" is having received a raise or a promotion; and "Ask raise" is having asked for a raise or a promotion. All regressions include state fixed effects (including Washington, D.C.) and individual controls: a quartic in age, indicators for having at least a bachelor's degree or no more than a a high school diploma (some college is the omitted), an indicator for gender, and indicators for race/ethnicity (Black, Asian, and Hispanic). The "Increase in CPI" indicates a control for 12-month (November to November) change in the national CPI-U. The sample includes all adults ages 25–54. Standard errors are clustered by the first state in which we observe a person.

Table 3: Effects of labor market tightness on changes in job characteristics of workers

(a) JOLTS: State level	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.089	0.092	0.071	0.055	0.078	0.025	0.077
	(0.017)	(0.012)	(0.015)	(0.013)	(0.012)	(0.011)	(0.018)
Dependent variable mean	0.079	0.068	0.060	0.050	0.056	0.037	0.115
(b) Lightcast: State level	**(1) Overall**	**(2) Pay**	**(3) Interest**	**(4) Work-Life**	**(5) Opportunities**	**(6) Physical**	**(7) Change**
Logged per-capita vacancies	0.074	0.067	0.059	0.043	0.064	0.026	0.053
	(0.014)	(0.012)	(0.013)	(0.012)	(0.014)	(0.011)	(0.017)
Dependent variable mean	0.079	0.068	0.060	0.050	0.056	0.037	0.115
(c) Lightcast: CBSA level	**(1) Overall**	**(2) Pay**	**(3) Interest**	**(4) Work-Life**	**(5) Opportunities**	**(6) Physical**	**(7) Change**
Logged per-capita vacancies	0.057	0.053	0.041	0.038	0.050	0.020	0.040
	(0.012)	(0.012)	(0.011)	(0.010)	(0.012)	(0.008)	(0.014)
Dependent variable mean	0.080	0.069	0.061	0.050	0.057	0.037	0.116

Note: This table provides estimates of how labor market tightness affects self-reported improvements in specific job amenities. The sample includes all adults ages 25–54. Binary dependent variables take the value 1 only for individuals who reported changing their job in the 12 months prior to the survey AND who reported the designated characteristic improved for the new job relative to the previous one; the outcome variable is 0 for all others, including those who did not change jobs or are without jobs. Panel (a) uses as the tightness measure the natural log of the number of JOLTS job openings per thousand state residents that year; panel (b) is analogous but uses Lightcast job postings rather than JOLTS openings; and panel (c) uses Lightcast postings but with CBSA (metro) geography rather than state. The job amenity outcomes across columns are: “Overall” (overall job quality); “Pay” (pay and benefits); “Interest” (interest in the work); “Work-life” (work-life balance); “Opportunities” (opportunities for advancement); “Physical” (physical demands), and “Change” (whether the individual reported changing jobs in the past 12 months). All regressions include fixed effects for geography (state or CBSA) and individual-level controls: an age quartic, indicators for at least a bachelor’s degree or no more than a high school diploma (some college omitted), an indicator for gender, and indicators for being Black, Asian, and Hispanic. Panels (a) and (b) have 51 geographies (including Washington, DC) and 21,545 observations. Panel (c) has 730 CBSAs and 20,333 observations (individuals living outside a CBSA are omitted). All regressions are unweighted, and standard errors are clustered by the first state (or CBSA, for panel (c)) in which we observe a person.

Table 4: Shift-share estimates of the effects of labor market tightness

(a) Labor market dynamics

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.111	0.105	0.094	-0.073	0.136	0.274	0.228
	(0.025)	(0.029)	(0.026)	(0.016)	(0.024)	(0.048)	(0.033)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Effective first-stage F	154.3	154.3	154.3	154.3	154.3	160.3	160.3
F critical value	37.42	37.42	37.42	37.42	37.42	37.42	37.42
Dependent variable mean	0.296	0.180	0.115	0.073	0.115	0.432	0.168
Increase in CPI						X	X

(b) Changes in job characteristics of workers

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.142	0.139	0.108	0.093	0.115	0.054	0.136
	(0.021)	(0.017)	(0.020)	(0.015)	(0.019)	(0.015)	(0.024)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Effective first-stage F	154.3	154.3	154.3	154.3	154.3	154.3	154.3
F critical value	37.42	37.42	37.42	37.42	37.42	37.42	37.42
Dependent variable mean	0.079	0.068	0.060	0.050	0.056	0.037	0.115

Note: This table revisits the results of panel (a) of Table 2 and of Table 3 using state industry employment shares from 2019 in the QCEW (at the three-digit level) and national leave-own-state-out industry employment growth to project annual percentage differences in total state employment as an instrument for the log change in JOLTS job openings per capita. The effective first-stage F statistic and the F critical value follow Olea and Pflueger (2013), with the critical value for a 5 percent 2SLS Nagar bias of 5 percent. See note to Tables 2 and 3 for more details.

Table 5: Effects of JOLTS labor market tightness on changes in job characteristics of workers: by sex

(a) Men

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.083	0.096	0.083	0.057	0.079	0.028	0.079
	(0.018)	(0.014)	(0.014)	(0.016)	(0.015)	(0.012)	(0.024)
Observations	11,007	11,007	11,007	11,007	11,007	11,007	11,007
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.077	0.067	0.060	0.047	0.058	0.036	0.112
Estimate / DV mean	1.082	1.439	1.389	1.216	1.370	0.766	0.703

(b) Women

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.096	0.086	0.059	0.052	0.076	0.022	0.075
	(0.023)	(0.018)	(0.022)	(0.015)	(0.019)	(0.016)	(0.021)
Observations	10,538	10,538	10,538	10,538	10,538	10,538	10,538
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.080	0.069	0.059	0.052	0.055	0.037	0.117
Estimate / DV mean	1.203	1.250	0.993	0.999	1.391	0.607	0.641

Note: This table provides estimates of how JOLTS-based labor market tightness measures affect self-reported improvements in specific job amenities, separately for men and women. See note to Table 3.

Table 6: Effects of JOLTS labor market tightness on labor market dynamics: by education

(a) BA or more

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.054	0.100	0.095	-0.063	0.105	0.194	0.090
	(0.029)	(0.034)	(0.025)	(0.013)	(0.029)	(0.047)	(0.047)
Observations	10,634	10,634	10,634	10,634	10,634	10,634	10,634
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.321	0.194	0.125	0.053	0.136	0.524	0.199
Coefficient over dependent mean	0.167	0.514	0.759	-1.192	0.770	0.370	0.454
Increase in CPI						X	X

(b) High school or less

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.046	0.028	0.053	0.008	0.060	0.010	0.058
	(0.050)	(0.040)	(0.028)	(0.026)	(0.031)	(0.078)	(0.043)
Observations	4,532	4,532	4,532	4,532	4,532	4,532	4,532
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.234	0.136	0.082	0.092	0.069	0.263	0.110
Coefficient over dependent mean	0.195	0.203	0.650	0.0823	0.872	0.0371	0.531
Increase in CPI						X	X

Note: This table provides estimates of how JOLTS-based labor market tightness measures affect individual labor dynamic behavior of prime-age SHED respondents, separately by education: for individuals with a bachelor's or higher degree in panel (a), and for individuals with a high school degree or less in panel (b). Specifications in columns (5) and (6) also include a control for changes in the consumer price index. See note to Table 2.

Table 7: Effects of JOLTS labor market tightness on changes in job characteristics of workers: by education

(a) BA or more

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.109	0.117	0.081	0.057	0.089	0.026	0.105
	(0.024)	(0.020)	(0.023)	(0.021)	(0.022)	(0.018)	(0.029)
Observations	10,634	10,634	10,634	10,634	10,634	10,634	10,634
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.096	0.085	0.071	0.058	0.069	0.036	0.136
Estimate / DV mean	1.131	1.379	1.137	0.981	1.285	0.719	0.770

(b) High school or less

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.056	0.064	0.050	0.051	0.058	0.024	0.060
	(0.025)	(0.019)	(0.020)	(0.022)	(0.017)	(0.021)	(0.031)
Observations	4,532	4,532	4,532	4,532	4,532	4,532	4,532
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.046	0.039	0.038	0.029	0.031	0.028	0.069
Estimate / DV mean	1.213	1.641	1.325	1.746	1.863	0.875	0.872

Note: This table provides estimates of how JOLTS-based labor market tightness measures affect self-reported improvements in specific job amenities, separately by education: for individuals with a bachelor's or higher degree in panel (a), and for individuals with only a high school degree or less in panel (b). See note to Table 3.

Table 8: Effects of labor market tightness (JOLTS, state level) on changes in job characteristics for job changers

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical
Logged per-capita openings	0.302	0.370	0.262	0.187	0.333	0.011
	(0.072)	(0.054)	(0.068)	(0.064)	(0.054)	(0.069)
Observations	2,466	2,466	2,466	2,466	2,466	2,466
Number of states	50	50	50	50	50	50
Dependent variable mean	0.687	0.596	0.521	0.433	0.491	0.321
Individual controls	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X

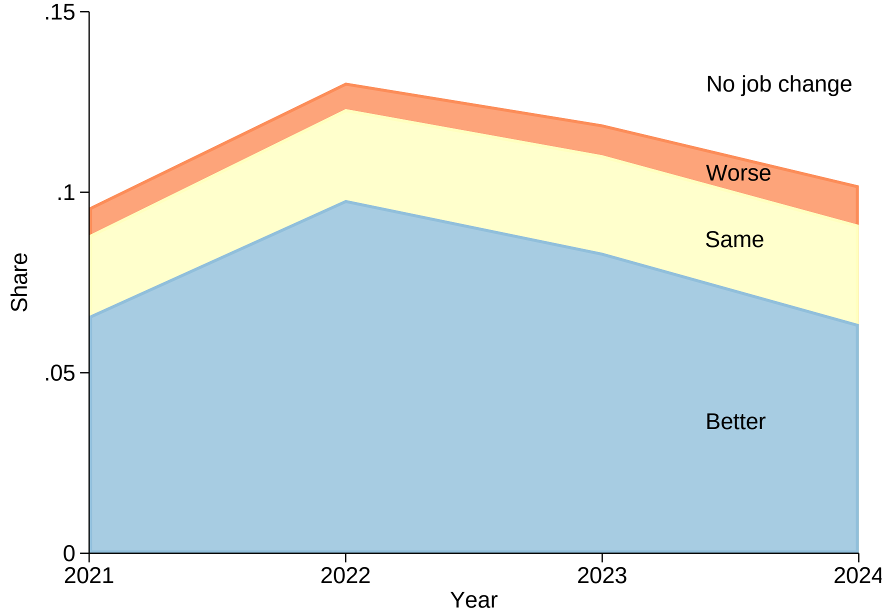
Note: This table provides estimates of how labor market tightness measures affect self-reported improvements in specific job amenities, *among the subsample reporting having changed jobs in the preceding 12 months and being employed at the time of the survey*. The tightness measure is the natural log of the number of JOLTS job openings per thousand state residents that year. See note to Table 3.

Table 9: Importance of labor-market-tightness-driven improvements in job amenities on overall job quality

	JOLTS: State			Lightcast: State			Lightcast: CBSA		
	Effect	Share	Ratio	Effect	Share	Ratio	Effect	Share	Ratio
Pay and benefits	0.020	0.228	1.000	0.015	0.199	1.000	0.012	0.206	1.000
	(0.003)	(0.028)		(0.003)	(0.037)		(0.003)	(0.040)	
Interest in the work	0.022	0.243	1.068	0.018	0.244	1.229	0.013	0.219	1.066
	(0.005)	(0.027)	(0.168)	(0.004)	(0.031)	(0.252)	(0.004)	(0.039)	(0.211)
Work-life balance	0.011	0.123	0.540	0.009	0.117	0.588	0.008	0.135	0.655
	(0.003)	(0.021)	(0.114)	(0.003)	(0.025)	(0.168)	(0.003)	(0.026)	(0.175)
Opportunities	0.009	0.101	0.442	0.007	0.100	0.501	0.006	0.101	0.493
	(0.002)	(0.023)	(0.100)	(0.002)	(0.029)	(0.122)	(0.002)	(0.029)	(0.119)
Physical demands	0.001	0.007	0.029	0.001	0.008	0.041	0.000	0.008	0.041
	(0.001)	(0.007)	(0.031)	(0.001)	(0.009)	(0.047)	(0.000)	(0.008)	(0.043)
Overall	0.089	1.000	4.392	0.074	1.000	5.027	0.057	1.000	4.859
	(0.017)		(0.535)	(0.014)		(0.934)	(0.014)		(0.944)

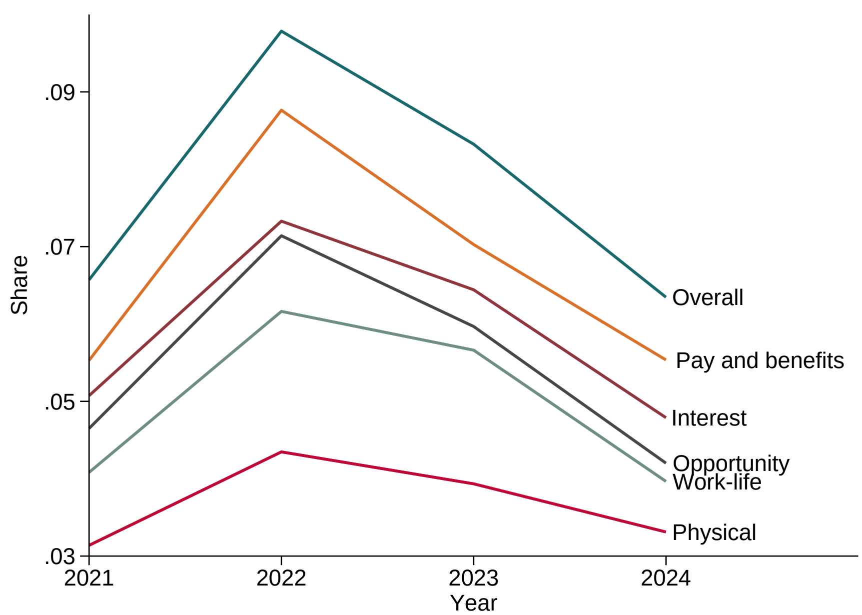
Note: This table scales the coefficient estimates in Table 3 on the effects of labor market tightness on specific job amenities by the coefficients in Table A.25 relating the importance of each amenity to overall job satisfaction. These scaled results show the relative importance of how labor-market-tightness-driven amenity improvements affect the labor-market-tightness improvement in overall job quality. The “Effect” columns capture the product of the two coefficient estimates. The “Share” columns divide the “Effect” column estimate by the estimate of labor market tightness on overall job quality from Table 3. The “Ratio” columns normalize the “Share” estimates for each amenity to a numeraire of the pay and benefits amenity. Standard errors in parentheses are computed using the delta method (via -nlcom- in Stata) and are clustered by the first state in which a person is observed. See also the notes to Tables 3 and A.25.

Figure 1: Changes in overall job quality by year



Note: This figure presents the shares of overall SHED respondents, ages 25–54, who reported the specified changes in their job quality between their job last year to their job this year. Respondents are placed in four categories: no main job change or changed main job and new job was worse, the same, or better. Results are unweighted.

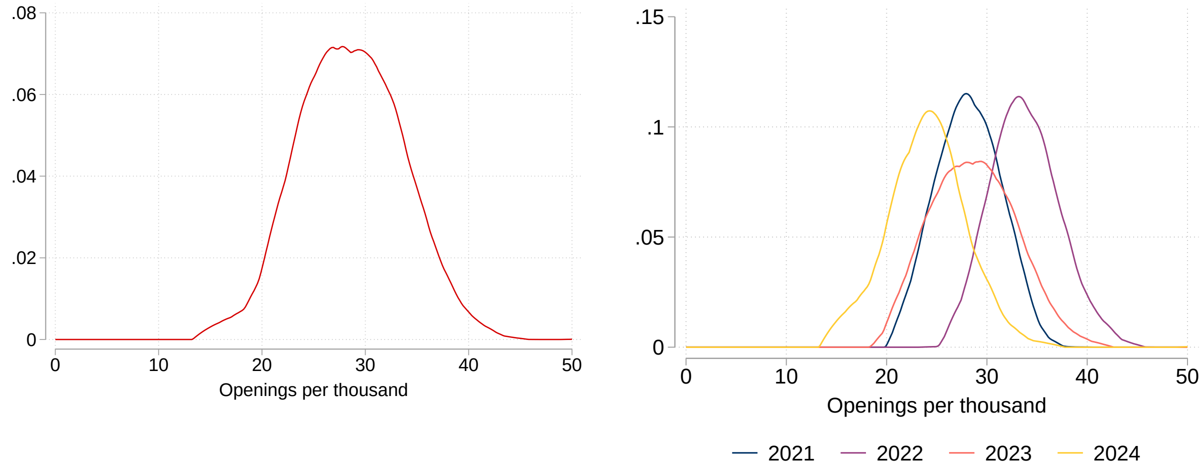
Figure 2: Share with better job characteristics by year



Note: This figure presents the shares of overall SHED respondents, ages 25–54, who changed jobs and reported improvements in the specified characteristic of their job this year relative to their previous job. Results are unweighted.

Figure 3: Kernel densities of openings per thousand

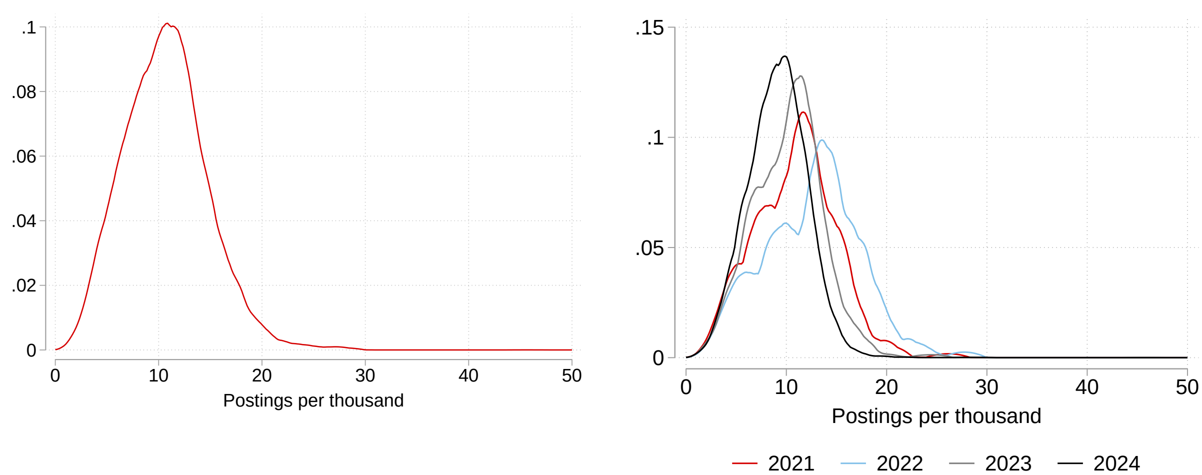
(a) Overall sample (b) By year



Note: This figure presents kernel densities of JOLTS job openings per thousand residents at the state-year level experienced by the baseline sample of SHED respondents. The openings are computed using JOLTS monthly counts from November of the previous year to October of the survey year, and the population is the count from the Census Bureau in the survey. Panel (a) presents results averaged across 2021–2024, while Panel (b) presents results separately by year. Results are unweighted. We use an Epanechnikov kernel with a bandwidth of 2.

Figure 4: Kernel densities of postings per thousand

(a) Overall sample (b) By year



Note: This figure presents kernel densities of Lightcast job postings per thousand residents, at the state-year level, experienced by the baseline sample of SHED respondents. The postings are computed using Lightcast monthly counts from November of the previous year to October of the survey year and the population is the count from the Census Bureau in the survey year. Panel (a) presents results averaged across 2021–2024, while Panel (b) presents results separately by year. Results are unweighted. We use an Epanechnikov kernel with a bandwidth of 1.

A SHED Details

A.1 Sample of Job Changers

The questions asked to define individuals who changed jobs were modified slightly after 2021. (See also Table A.1.) Starting in 2022, job changers are defined as someone who is currently employed but also has started a new job over the past year. Individuals who started a new job are then asked if their current main job is different from the one they had last year. Main-job changers are defined as those who say that their main job is different, either because their role is different or because their employer is different.[39] Although the skip patterns changed slightly between 2021 and 2022, our main outcomes of interest were asked of the same conceptual group: currently employed individuals who changed main jobs during the previous 12 months.

[39]Specifically job changers are defined as individuals who said yes to the question (D1A) ”Last month, did you do any work for either pay or profit?” and responded that they “started a new job” in response to the question (D44) “Think about any job in the past 12 months, not just your main job. In the past 12 months, have you: ...” and further said that they either had a “Different main job—new employer” or “Different main job—same employer” in response to question (D37A) “You indicated that you started a new job in the past 12 months. Is your main job (where you earn the most money) the same as it was a year ago?”

Table A.1: Information on SHED Variables

			2021 Differences	
Outcome	**Question**	**Universe**	**Question**	**Universe**
Worked	D1A	All respondents		
Choose main job tasks	D28_a	D3A=0 (employee)	N/A	
Choose how to complete tasks	D28_b	D3A=0 (employee)	N/A	
Part-time (main job)	D48, D3B	D1A=1 (working)	D3B	D1A=1 (working)
Schedule variability (main job)	D30	D3A=0 (employee)		
Remote work last week	D34A	D1A=1 (working)		
Changed main job in last year	D37A	D1A=1, D44_d=1 (worked, new job last 12 mon.)	D37	D1A=1 (working)
Job amenity changes	D38	D37A$\leq$2 (diff. main job)	D38	D37=1 (diff. main job)
Overall job change	D39	D37A$\leq$2 (diff. main job)	D39	D37=1 (diff. main job)
Asked raise/promotion past year	D44_a	D1A=1 (working)		
Rec. raise/promotion past year	D44_b	D1A=1 (working)		
Applied for new job past year	D44_c	All respondents		
Started new job past year	D44_d	All respondents		
Voluntarily left job past year	D44_e	All respondents		
Laid off or lost a job past year	D44_f	All respondents		
Demographic controls				
Female	Ipsos			
Race/Ethnicity	Ipsos			
Education	ED0	All respondents		
Age	Ipsos			
County of Residence	Ipsos			

Note: This table lists the variables from the SHED survey used in our analysis. Waves 2022–2024 use identical questions while wave 2021 uses slightly different questions than those presented here. The SHED sampling frame is drawn from a larger IPSOS panel, with demographic information taken from that panel. Race and ethnicity questions are combined into five mutually exclusive categories: Hispanic and (non-Hispanic) White, Black, Asian, or another race.

Table A.2: Sample sizes

(a) Sample sizes in each year

		Years			
	Total	2021	2022	2023	2024
Observations	21,545	5,415	5,303	5,265	5,562
State by years	204	51	51	51	51

(b) Numbers of years included

		Number of years			
	Total	1	2	3	4
Respondents	14,660	10,088	2,766	1,299	507
States	51	0	0	0	51

Note: Panel (a) shows the number of observations and states represented in the the analysis sample of people ages 25–54, both overall and by year. Panel (b) shows the total number of respondents and states represented in the the analysis sample, as well as the number of years in which each each is included.

Table A.3: Summary statistics for measures of state labor market tightness

	Mean	Standard deviation	10th percentile	90th percentile	2021 mean	2022 mean	2023 mean	2024 mean
JOLTS (State)								
Logged job openings per thousand	3.34	0.17	3.13	3.55	3.34	3.52	3.35	3.17
Job openings per thousand	28.7	5.1	22.8	34.9	28.3	33.9	28.8	24.1
N	21,545	21,545	21,545	21,545	5,415	5,303	5,265	5,562
Shift-shares (State)								
Openings since 2019 (p.p.)	1.3	0.7	0.3	2.3	1.4	2.3	1.2	0.3
Annual employment change (p.p.)	2.4	1.8	0.4	5.3	0.4	5.3	2.6	1.3
N	21,545	21,545	21,545	21,545	5,415	5,303	5,265	5,562
Lightcast (State)								
Logged state-level online posts per thousand	2.33	0.22	2.10	2.60	2.33	2.50	2.29	2.19
Online posts per thousand	10.5	2.6	8.1	13.5	10.5	12.4	10.1	9.0
N	21,545	21,545	21,545	21,545	5,415	5,303	5,265	5,562
Lightcast (CBSA)								
Logged CBSA-level online posts per thousand	2.29	0.41	1.71	2.77	2.31	2.45	2.26	2.16
Online posts per thousand	10.7	4.0	5.5	16.0	10.9	12.6	10.2	9.1
N	20,427	20,427	20,427	20,427	5,151	5,054	4,999	5,223

Note: This table gives means, standard deviations, 10th percentile, 90th percentile, and means in individual years of the various measures of labor market tightness. Statistics apply to the analysis sample of people ages 25–54 in all years and for the specified year. Statistics are unweighted. This differs somewhat from the population weighted average of these measures and the sample sizes vary for the Lightcast measures at the CBSA level because people living outside of CBSAs are excluded from these measures.

Table A.4: Correlations between key demographic factors and labor market tightness and shift-share instruments

	JOLTS	Lightcast	IV Openings	IV Employment
Has a BA or more	0.03	0.17	0.01	0.00
High school or less	-0.02	-0.13	-0.02	-0.01
Age	-0.01	-0.02	-0.00	0.01
Woman	0.01	-0.02	0.00	-0.01
White	0.15	0.01	0.03	-0.01
Hispanic	-0.15	-0.05	-0.02	0.00
Black	0.02	0.01	-0.00	0.01

Note: This table presents pairwise partial correlations from the analysis sample between the labor market tightness measure listed in the column with the demographic variable listed in the row. All labor market tightness measures are at the state level except for the Lightcast measure, which is at the CBSA level.

Table A.5: Correlations between labor market tightness and shift-share instruments (state-year level)

(a) Unadjusted variables	Logged openings per thousand	Shift share: Openings since 2019 (p.p.)	Shift share: Annual employment change (p.p.)	Logged online posts per thousand
Logged openings per thousand	1.00			
Shift share: Openings since 2019 (p.p.)	0.70	1.00		
Shift share: Annual employment change (p.p.)	0.54	0.72	1.00	
Logged online posts per thousand	0.28	0.25	0.19	1.00

(b) Residualized by state fixed effects	Logged openings per thousand	Shift share: Openings since 2019 (p.p.)	Shift share: Annual employment change (p.p.)	Logged online posts per thousand
Logged openings per thousand	1.00			
Shift share: Openings since 2019 (p.p.)	0.91	1.00		
Shift share: Annual employment change (p.p.)	0.72	0.72	1.00	
Logged online posts per thousand	0.70	0.71	0.55	1.00

Note: This table shows correlations between JOLTS log openings per capita, the two instrumental variables, and Lightcast log online postings per capita. Panel A provides raw correlations (at the state-year level) and Panel B shows correlations after residualizing state fixed effects.

Table A.6: Effects of JOLTS labor market tightness on changes in overall job quality: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.074	0.064	0.089	0.139	0.031	0.071
	(0.013)	(0.014)	(0.017)	(0.033)	(0.029)	(0.038)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.079	0.079	0.079	0.078	0.079	0.079
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on overall job quality, as captured in column 1 of panel (a) of Table 3. Column 3 repeats that baseline specification. Column 1 omits both state fixed effects and individual-level controls, while column 2 adds back in the individual controls but excludes the state fixed effects. Column 4 adds to the baseline specification individual fixed effects. Column 5 adds year fixed effects to the base specification, and column 6 adds year-by-Census-division fixed effects. Column 7 adds both individual and year fixed effects, which, because of the short, limited panel, effectively removes remaining variation from the key regressor. See also the note to Table 3.

Table A.7: Effects of JOLTS labor market tightness on changes in pay and benefits: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.072	0.065	0.092	0.127	0.073	0.094
	(0.012)	(0.013)	(0.012)	(0.025)	(0.024)	(0.032)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.068	0.068	0.068	0.067	0.068	0.068
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on changes in pay and benefits, as captured in column 2 of panel (a) of Table 3. See note to Table A.6.

Table A.8: Effects of JOLTS labor market tightness on changes in interest in the work: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.057	0.052	0.071	0.099	0.021	0.058
	(0.013)	(0.014)	(0.015)	(0.026)	(0.027)	(0.039)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.060	0.060	0.060	0.058	0.060	0.060
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on changes in interest in the job's work, as captured in column 3 of panel (a) of Table 3. See note to Table A.6.

Table A.9: Effects of JOLTS labor market tightness on changes in work-life balance: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.042	0.038	0.055	0.091	0.016	0.044
	(0.009)	(0.010)	(0.013)	(0.028)	(0.021)	(0.026)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.050	0.050	0.050	0.049	0.050	0.050
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on changes in the job's work-life balance, as captured in column 4 of panel (a) of Table 3. See note to Table A.6.

Table A.10: Effects of JOLTS labor market tightness on changes in advancement opportunities: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.060	0.054	0.078	0.101	0.031	0.051
	(0.011)	(0.012)	(0.012)	(0.025)	(0.025)	(0.035)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.056	0.056	0.056	0.058	0.056	0.056
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on changes in the job's advancement opportunities, as captured in column 5 of panel (a) of Table 3. See note to Table A.6.

Table A.11: Effects of JOLTS labor market tightness on changes in physical demands: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.021	0.019	0.025	0.047	-0.007	0.000
	(0.007)	(0.007)	(0.011)	(0.021)	(0.018)	(0.029)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.037	0.037	0.037	0.036	0.037	0.037
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on changes in the job's physical demands, as captured in column 6 of panel (a) of Table 3. See note to Table A.6.

Table A.12: Effects of JOLTS labor market tightness on changing jobs: Alternative controls

	(1) None	(2) Indiv X	(3) State FE	(4) Indiv FE	(5) Year FE	(6) Yr by Div FE
Logged per-capita openings	0.070	0.058	0.077	0.150	0.046	0.064
	(0.014)	(0.015)	(0.018)	(0.040)	(0.029)	(0.045)
Observations	21,545	21,545	21,545	11,457	21,545	21,545
Number of states	51	51	51	51	51	51
Dependent variable mean	0.115	0.115	0.115	0.112	0.115	0.115
Individual controls		X	X	X	X	X
State fixed effects			X	X	X	X
Individual fixed effects				X		
Year fixed effects					X	
Year by division fixed effects						X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of state-level JOLTS openings per thousand residents) on the likelihood of changing jobs, as captured in column 7 of panel (a) of Table 3. See note to Table A.6.

Table A.13: Effects of Lightcast CBSA labor market tightness on changes in overall job quality: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.031	0.023	0.057	0.058	0.088	0.003	0.004
	(0.005)	(0.005)	(0.012)	(0.013)	(0.023)	(0.016)	(0.019)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.080	0.080	0.080	0.081	0.079	0.080	0.080
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on overall job quality, as captured in column 1 of panel (c) of Table 3. Column 3 repeats that baseline specification. Column 1 omits both CBSA fixed effects and individual-level controls, while column 2 adds back in the individual controls but excludes the CBSA fixed effects. Column 4 restricts the sample to metro CBSA only (omitting micro areas). Column 5 adds to the baseline specification individual fixed effects. Column 6 adds year fixed effects to the base specification, and column 7 adds year-by-Census-division fixed effects. See also the note to Table 3.

Table A.14: Effects of Lightcast CBSA labor market tightness on changes in pay and benefits: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.032	0.025	0.053	0.055	0.077	-0.000	-0.001
	(0.004)	(0.004)	(0.012)	(0.013)	(0.020)	(0.016)	(0.017)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.069	0.069	0.069	0.071	0.068	0.069	0.069
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on changes in pay and benefits, as captured in column 2 of panel c) of Table 3. See notes to Table A.13.

Table A.15: Effects of Lightcast CBSA labor market tightness on changes in interest in the work: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.025	0.020	0.041	0.046	0.062	-0.007	-0.007
	(0.004)	(0.004)	(0.011)	(0.012)	(0.018)	(0.016)	(0.017)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.061	0.061	0.061	0.061	0.059	0.061	0.061
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on changes in interest in the job's work, as captured in column 3 of panel (c) of Table 3. See note to Table A.13.

Table A.16: Effects of Lightcast CBSA labor market tightness on changes in work-life balance: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.015	0.011	0.038	0.038	0.053	0.006	0.010
	(0.003)	(0.003)	(0.010)	(0.011)	(0.020)	(0.013)	(0.014)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.050	0.050	0.050	0.050	0.049	0.050	0.050
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on changes in the job's work-life balance, as captured in column 4 of panel (c) of Table 3. See note to Table A.13.

Table A.17: Effects of Lightcst CBSA labor market tightness on changes in advancement opportunities: Alternative control

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.029	0.023	0.050	0.051	0.075	0.009	0.008
	(0.004)	(0.004)	(0.012)	(0.012)	(0.020)	(0.015)	(0.016)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.057	0.057	0.057	0.058	0.059	0.057	0.057
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on changes in the job's advancement opportunities, as captured in column 5 of panel (c) of Table 3. See note to Table A.13.

Table A.18: Effects of Lightcast CBSA labor market tightness on changes in physical demands: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.010	0.010	0.020	0.021	0.026	0.007	0.009
	(0.003)	(0.003)	(0.008)	(0.009)	(0.017)	(0.012)	(0.013)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.037	0.037	0.037	0.037	0.035	0.037	0.037
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on changes in the job's physical demands, as captured in column 6 of panel (c) of Table 3. See note to Table A.13.

Table A.19: Effects of Lightcast CBSA labor market tightness on changing jobs: Alternative controls

	(1) None	(2) Indiv X	(3) CBSA FE	(4) Metro only	(5) Indiv FE	(6) Year FE	(7) Yr by Div FE
Logged per-capita vacancies	0.037	0.027	0.040	0.041	0.077	-0.010	-0.009
	(0.006)	(0.006)	(0.014)	(0.014)	(0.025)	(0.020)	(0.022)
Observations	20,427	20,427	20,333	18,700	10,899	20,333	20,333
Number of CBSAs	815	815	730	407	596	730	730
Dependent variable mean	0.116	0.116	0.116	0.117	0.113	0.116	0.116
Individual controls		X	X	X	X	X	X
CBSA fixed effects			X	X	X	X	X
Metro areas only				X			
Individual fixed effects					X		
Year fixed effects						X	
Year by division fixed effects							X

Note: This table presents alternative estimates, with varying controls, for the effects of labor market tightness (as captured by the log of CBSA-level Lightcast openings per thousand residents) on the likelihood of changing jobs, as captured in column 7 of panel (c) of Table 3. See note to Table A.13.

Table A.20: Effects of JOLTS labor market tightness on job amenities: Estimation in levels

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Openings per capita	3.394	3.493	2.686	2.038	2.921	0.922	2.943
	(0.552)	(0.404)	(0.503)	(0.420)	(0.423)	(0.386)	(0.603)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.079	0.068	0.060	0.050	0.056	0.037	0.115
State fixed effects	X	X	X	X	X	X	X
Individual controls	X	X	X	X	X	X	X

Note: This table presents estimates of how labor market tightness, as measured by the number of state-level JOLTS job openings over the preceding 12 months divided by the annual state population (in thousands), on changes in self-reported job amenities. See note to Table A.13. Unlike the results in panel (a) there, the labor market tightness measure here is not logged.

Table A.21: Effects of JOLTS labor market tightness on job amenities: Sample of non-layoffs

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical
Logged per-capita openings	0.087	0.089	0.073	0.057	0.078	0.026
	(0.017)	(0.014)	(0.016)	(0.014)	(0.011)	(0.012)
Observations	19,978	19,978	19,978	19,978	19,978	19,978
Number of states	51	51	51	51	51	51
Dependent variable mean	0.071	0.062	0.054	0.043	0.051	0.031
Individual controls	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X

Note: This table presents the same baseline specification as in panel (a) of Table 3, but for a sample excluding individuals we can identify as having been laid off in the year preceding the survey.

Table A.22: Effects of JOLTS labor market tightness on job amenities: by selected job changers

	(1)	(2)	(3)	(4)	(5)	(6)
(a) Changing employers	Overall	Pay	Interest	Work-Life	Opportunities	Physical
Logged per-capita openings	0.345	0.374	0.229	0.260	0.360	0.037
	(0.070)	(0.075)	(0.069)	(0.076)	(0.076)	(0.089)
Observations	1,485	1,485	1,485	1,485	1,485	1,485
Number of states	49	49	49	49	49	49
Dependent variable mean	0.713	0.601	0.535	0.485	0.516	0.356

	(1)	(2)	(3)	(4)	(5)	(6)
(b) Same employer	Overall	Pay	Interest	Work-Life	Opportunities	Physical
Logged per-capita openings	0.153	0.237	0.359	-0.032	0.254	0.015
	(0.145)	(0.139)	(0.161)	(0.148)	(0.110)	(0.128)
Observations	437	437	437	437	437	437
Number of states	43	43	43	43	43	43
Dependent variable mean	0.609	0.600	0.483	0.277	0.426	0.222

	(1)	(2)	(3)	(4)	(5)	(6)
(c) All changers, ’22-24	Overall	Pay	Interest	Work-Life	Opportunities	Physical
Logged per-capita openings	0.319	0.354	0.268	0.210	0.341	0.042
	(0.079)	(0.057)	(0.069)	(0.068)	(0.056)	(0.070)
Observations	1,927	1,927	1,927	1,927	1,927	1,927
Number of states	49	49	49	49	49	49
Dependent variable mean	0.689	0.601	0.522	0.437	0.494	0.324

Note: This table presents the same baseline specification as in Table 8, but on further subsamples: in panel (a), restricted to the share of people who changed employers; in panel (b), restricted to those who changed jobs while staying with the same employer; and in panel (c), restricted to all job changers in the years (2022–2024) in which we can distinguish between moves within and between employers.

Table A.23: Effects of labor tightness on labor market dynamics: by sex

(a) Men

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.048	0.065	0.081	-0.033	0.079	0.120	0.103
	(0.028)	(0.030)	(0.026)	(0.019)	(0.024)	(0.043)	(0.045)
Observations	11,007	11,007	11,007	11,007	11,007	11,007	11,007
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.287	0.167	0.104	0.074	0.112	0.479	0.190
Increase in CPI						X	X

(b) Women

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.045	0.076	0.081	-0.030	0.075	0.135	0.090
	(0.029)	(0.028)	(0.021)	(0.013)	(0.021)	(0.077)	(0.037)
Observations	10,538	10,538	10,538	10,538	10,538	10,538	10,538
Number of states	51	51	51	51	51	51	51
Dependent variable mean	0.305	0.193	0.126	0.071	0.117	0.382	0.146
Increase in CPI						X	X

Note: This table presents the same baseline specification as in Table 2, but separately for men and women. See note to Table 2.

Table A.24: Alternative shift-share estimates of the effects of labor market tightness

(a) Labor market dynamics

	(1) Applied	(2) Started	(3) Quit	(4) Laid-off	(5) Changed	(6) Got raise	(7) Ask raise
Logged per-capita openings	0.033	0.081	0.090	-0.044	0.071	0.216	0.153
	(0.019)	(0.026)	(0.018)	(0.014)	(0.019)	(0.045)	(0.031)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Effective first-stage F	138.7	138.7	138.7	138.7	138.7	142.2	142.2
F critical value	37.42	37.42	37.42	37.42	37.42	37.42	37.42
Dependent variable mean	0.296	0.180	0.115	0.073	0.115	0.432	0.168
Increase in CPI						X	X

(b) Changes in job characteristics of workers

	(1) Overall	(2) Pay	(3) Interest	(4) Work-Life	(5) Opportunities	(6) Physical	(7) Change
Logged per-capita openings	0.091	0.087	0.073	0.058	0.079	0.027	0.071
	(0.016)	(0.013)	(0.015)	(0.013)	(0.015)	(0.012)	(0.019)
Observations	21,545	21,545	21,545	21,545	21,545	21,545	21,545
Effective first-stage F	138.7	138.7	138.7	138.7	138.7	138.7	138.7
F critical value	37.42	37.42	37.42	37.42	37.42	37.42	37.42
Dependent variable mean	0.079	0.068	0.060	0.050	0.056	0.037	0.115

Note: This table revisits the results of panel (a) of Table 2 and 3 using 2019 state industry employment shares from the QCEW to project long differences between 2019 and the survey year in the JOLTS openings rate. The effective first-stage F-statistic and the F critical value follow Olea and Pflueger (2013), with the critical value for a 5 percent 2SLS Nagar bias of 5 percent. See note to Tables 2, 3, and 4 for more details.

Table A.25: Relationships between job amenity improvements and overall job quality among job changers

	(1) No controls	(2) State FE	(3) Baseline controls	(4) Openings	(5) Year FE	(6) State by year	(7) Layoff
Improved pay and benefits	0.228	0.227	0.223	0.221	0.222	0.218	0.210
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Improved interest in work	0.304	0.307	0.305	0.305	0.304	0.301	0.297
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)
Improved work-life balance	0.203	0.203	0.202	0.201	0.200	0.199	0.198
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Improved opportunities	0.114	0.115	0.116	0.115	0.115	0.120	0.117
	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.018)
Improved physical demands	0.019	0.018	0.023	0.024	0.023	0.025	0.028
	(0.023)	(0.024)	(0.025)	(0.025)	(0.024)	(0.025)	(0.025)
Observations	2,450	2,450	2,450	2,450	2,450	2,450	2,450
State fixed effects		X	X	X	X		
Individual controls			X	X	X	X	X
Log job openings				X	X	X	X
Year fixed effects					X		
State by year fixed effects						X	X
Laid-off indicator							X

Note: This table provides estimates from a linear probability model relating improvement in overall job quality to improvements in specific amenities. Controls are included as indicated in the bottom rows. The individual controls are the same as in table 3. Log job openings capture the natural log of the number of JOLTS job openings per thousand state residents that year. Standard errors clustered at the state level are in parentheses.