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Skill Remoteness and Post-Layoff Labor Market Outcomes

Abstract

This paper quantifies how the local skill remoteness of a laid-off worker's last job affects subsequent wages, employment, and mobility rates. Local skill remoteness captures the degree of dissimilarity between the skill profiles of the worker's last job and all other jobs in a local labor market. I implement a measure of local skill remoteness at the occupation-city level and find that higher skill remoteness at layoff is associated with persistently lower earnings after layoff. Earnings differences between workers whose last job was above or below median skill remoteness amount to a loss of more than $10,000 over 4 years, and are mainly accounted for by lower wages upon re-employment (not lower hours worked). Workers who lost a skill-remote job also have a higher probability of changing occupation, a lower probability of being re-employed at jobs with similar skill profiles, and a higher propensity to migrate to another city after layoff. Finally, I show that jobs destroyed in recessions are more skill-remote than those lost in booms. Taking all these facts together, I conclude that the local skill remoteness of jobs is an empirically relevant factor to understand the severity and cyclicality of displaced workers' earnings losses and reallocation patterns.

JEL-Codes: E240, J240, J630.

Keywords: mismatch, job loss, worker reallocation, occupational change, migration.

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1 Introduction

This paper quantifies how the *local skill remoteness* of a laid-off worker's last job affects subsequent wages, employment, and mobility rates. Local skill remoteness measures the degree of dissimilarity between the skill profiles of a worker's job and all other jobs in a local labor market. In the presence of search frictions, such disparities may affect both individual economic outcomes and aggregate allocative efficiency. However, whether such effects are quantitatively relevant, and along which dimensions, is an open question. The literature, for instance, has shown that mismatch between vacancies and job seekers, across industries and occupations, explains only a modest portion of cyclical fluctuations in the aggregate unemployment rate (Şahin et al., 2014; Patterson et al., 2016; Herz and van Rens, 2019). Should, then, mismatch be written off as a minor phenomenon in the economics of labor markets? This paper argues that it needs not be so, as I find that the effects of mismatch extend to wages and workers' reallocation patterns.

I propose the concept of local skill remoteness as a measure of skill mismatch between jobs in a local labor market. I then leverage rich micro-data on workers' careers from the National Longitudinal Survey of Youth 1979 (NLSY79) and build on the well-established literature on the individual effects of job displacement, to show how local skill remoteness affects wages and reallocation rates, in addition to employment. I provide evidence that workers who are displaced from a job more locally skill-remote than the median (a "skill-remote" job) have persistently lower earnings than comparable workers who lost a job less locally skill-remote than the median (a "skill-central" job). Lower earnings are mainly accounted for by lower wages upon re-employment (not lower hours worked). Workers laid-off from a skill-remote job also have a lower probability to be re-employed at jobs with similar skill profiles, a factor contributing to their lower earnings. I conclude that local skill remoteness is an empirically relevant factor in determining the severity of post-displacement earnings losses and the reallocation patterns that bring them about — which in turn supports the hypothesis that mismatch affects the labor market well beyond its documented modest influence on employment rates.

Post-displacement earnings losses are more severe when layoff happens during an economic downturn (Jacobson et al., 1993; Davis and von Wachter, 2011). Local skill remoteness speaks to this margin, as well. In particular, I find that jobs destroyed in recessions are more likely to be locally skill-remote than those destroyed in booms. Data from the Current Population Survey (CPS) 1996-2017 shows that the percentage of destroyed skill-remote jobs is 60.3% in recessions and 46.6% in booms. Because losing a skill-remote job is associated with larger earnings losses, this fact contributes to accounting for the cyclicality of displaced workers' earnings losses. It also complements existing narratives which identify in occupational change and human capital losses the underlying reasons for large and cyclical post-displacement earnings losses (Huckfeldt, 2021; Jarosch, 2021). To this respect, local skill remoteness offers a way to identify vulnerable workers even before layoff occurs — thus it is a potentially useful tool to effectively target policy interventions.

My measure of local skill remoteness incorporates two potential sources of frictions to worker reallocation. First, differences in the skill content of jobs which imply imperfect substitutability between workers of different occupational backgrounds. Second, heterogeneity in the availability of jobs across different geographical locations, a consequence of spatial specialization patterns and costly geographic mobility. To capture these two margins, I follow three steps. First, I use detailed O*NET data on the skill content of occupations to characterize jobs as vectors of skills and compute skill distances between them. This first step results in a matrix of pairwise skill distances between occupations. Then, to consider how plentiful various jobs are in different locations, I calculate occupational shares for different metropolitan areas using data from the Occupational Employment Statistics (OES). Finally, for each job in a given metropolitan area, I calculate the average distance between that job and all other occupations (including itself) in the metropolitan area, using the occupational shares as weights. This yields a measure of how similar the skills utilized in one occupation are to those in the many other jobs in each local labor market. This employment-weighted average distance is what I call a job's *local skill remoteness.*

The empirical local skill remoteness measure displays substantial cross-sectional variation across occupations in the same location and for the same occupation across locations. Such variation reflects known specialization patterns across cities and highlights how these patterns create geographically distinct "skill clusters" across local labor markets. Consider, for example, the following two metro areas: Charlotte, NC and San Diego, CA. Charlotte has developed into a hub of production and transportation jobs, with American Airlines, Lowe's, Compass, and Carolina Beverage Corporation Inc. (two food manufacturers) among the local large employers. In San Diego, the largest employers are the University of California San Diego and its associated hospital system, two large Healthcare providers (Sharp Health-Care and Scripps Health) and Qualcomm (a creator of semiconductors, software, and services related to wireless technology). Estimated occupational skill distances — which I incorporate in local skill remoteness — show that the skills required in average retail and transportation jobs are more similar to those utilized in the average production job, than those in a typical healthcare or engineering position. Hence, Production occupations are relatively more skill-remote in San Diego than they are in Charlotte.

I exploit cross-sectional and time variation in the local skill remoteness of jobs to study how the local skill remoteness of a worker's job *at the time of layoff* affects the worker's recovery *after layoff.* Detailed individual labor market histories from the NLSY79 allow me to evaluate earnings losses associated with displacement from a skill-remote job. In the month of layoff, the earnings of workers whose last job was locally skill-remote are on average $491.18 lower than workers whose last job was locally skill-central. The difference is economically and statistically significant until 4 years after job loss and stable at around $200 per month. Overall, in the course of 4 years after layoff, workers who were laid-off from a locally skill-remote job earn an estimated $10,111.49 *less* than workers who were laid-off from a locally skill-central job — approximately 5 months of the median worker's pre-layoff income. On average, the earnings losses after layoff are 17.7% larger for skill-remote workers over a 4-years period, a significant difference.

Most of the long-term earnings losses associated with skill remoteness at layoff are accounted for by lower wages at re-employment, not reduced hours worked. This fact points to the potential for mismatch between the skills used in the pre-layoff job and the current one to significantly affect displaced workers' wages — while not having a substantial impact on employment rates. Skill remoteness substantially affects workers' mobility rates, as well. Skill-remote laid-off workers are more prone to both occupational and locational change, even 4 years after displacement. Skill remoteness at job loss is a good predictor of the direction of such changes; skill-remote laid-off workers are, on average, re-employed in an occupation whose skill content is further from the previous job's skill profile than skill-central workers. Migration rates across cities are also directed: workers who leave their city of residence after layoff tend to reduce their occupation's skill remoteness in the new location. The difference in local skill remoteness between the two locations is increasing in the level at origin, and more so than a random migration scenario would predict.

In conclusion, evidence from the NLSY79 highlights how workers laid-off from a skill-remote job experience more severe earnings losses than comparable skill-central workers, partially on account of more frequent and significant occupational changes. Skill-remote workers are also more likely to migrate to a new city after layoff. I interpret this evidence as suggesting that local skill remoteness, a measure of skill mismatch between jobs, affects workers' post-layoff outcomes through wage changes and reallocation patterns — even as it does not influence their employment rate. Together with the fact that jobs lost in recessions tend to be disproportionately skill-remote than those destroyed in booms, the evidence from displaced workers suggests that the effects of mismatch on the labor market may extend well beyond its small contribution to employment changes.

Contribution to the literature This paper primarily contributes to two strands of literature: the effects of mismatch on employment, wages, and worker reallocation, and the role of specific human capital in the consequences of job loss.

Studies of skill mismatch have broadly fallen into two categories: "macro" and "micro". The goal of this paper is to bridge these two strands and address the open questions at their intersection.

The "macro" literature employs structural approaches to compute mismatch as deviations from the optimal allocation in a model of choice and focuses largely on business cycle frequency. Şahin et al. (2014) is a compelling example of this approach, where the authors investigate the contribution of mismatch to changes in aggregate employment during the Great Recession. Using a similar empirical strategy and data from the United Kingdom, Patterson et al. (2016) document, instead, the effect of mismatch on labor productivity growth. Hertz and van Rens (2019) also calibrate a structural framework to a system of sectorally and geographically distinct markets and estimate a modest contribution of increases in mismatch to increases in unemployment of U.S. workers since the 1990s.

The "micro" strand of the literature utilizes empirical measures of mismatch that are predicated on models of individual behavior and speak primarily to individual outcomes. Andersson et al. (2018); Marinescu and Rathelot (2018); Manning and Petrongolo (2017) propose mismatch measures that arise from job openings not being located in the same geographic location as job seekers (geographic mismatch only).[1] On the other hand, Guvenen et al. (2020) use an empirical measure of

[1] These papers provide robust evidence that workers prefer jobs that are close to their local area of residence. I interpret their results as a compelling argument in favor of a local definition of labor

skill mismatch that emphasizes occupational skills but does not take into account differences across space (occupational mismatch only), building upon the seminal work of Gathmann and Schoenberg (2010) — the first to propose occupational skill distances as a measure of mismatch.[2]

Lastly, my work contributes to the literature on earnings losses after displacement by investigating variation in workers' local skill remoteness at the time of layoff. Existing studies have focused largely on understanding why workers laid-off during recessions experience worse labor market outcomes than those laid-off during booms (Jacobson et al., 1993; Farber et al., 1993; Davis and von Wachter, 2011; Krolikowski, 2017; Huckfeldt, 2021; Jarosch, 2021). This paper reaffirms the view that the loss of (specialized) human capital emerges as a significant factor in accounting for post-layoff losses. Specifically, I show that jobs destroyed in recessions are disproportionately locally skill-remote and that losing a skill-remote job is associated with larger earnings losses after layoff.[3]

Structure of the paper The paper is organized as follows: section 2 introduces my local skill remoteness measure and the data I use to implement it at the occupation-city level. With a measure of local skill remoteness in hand for all occupations and metropolitan areas, section 3 describes how the local skill remoteness of jobs varies in the cross-section and over the business cycle. Section 4 moves on to investigate how skill remoteness at layoff affects earnings, employment, and workers' mobility, using individual labor market histories in the NLSY79. Section 5 discusses the interpretation of my results and their robustness by comparing the NLSY79 analysis with similar empirical exercises in the CPS Displaced Workers Survey (DWS) and assessing the role of selection on unobservables. Section 6 concludes.

markets.

[2]I also build upon Gathmann and Schoenberg (2010), who use a measure of average distance similar to skill remoteness in this paper as an instrumental variable (see footnote 20 on page 31 in their paper), arguing that such a measure would be orthogonal to a worker's unobserved ability.

[3]Neffke, Otto, and Hidalgo (2018) explore a related idea: whether the local *industry* mix matters for how job-seekers trade off geographical against skill distance. This paper complements their analysis by proposing a classification of (dis)similarities between jobs based on occupational skills, not cross-industry flows, thus isolating the component of worker flows that pertains to specific human capital differences.

2 Local skill remoteness

2.1 Conceptual framework

The starting point of my analysis is modeling jobs as vectors of skills. The distance in the skill space between two vectors is then a natural way to assess the dissimilarity between the two jobs. Let $a = 1, \ldots, A$ denote skill attributes and $j = 1, \ldots, J$ index jobs. Denote by ℓ_{ja} the level of skill a demanded by job j, which can therefore be understood as vector of length A:

$$job_j = [\ell_{j1},\ \ldots,\ \ell_{jA}]$$

so that the skill distance between two jobs, j and j', is the following absolute norm:[4]

$$d_{jj'} = \frac{1}{A} \sum_{a=1}^{A} |\ell_{ja} - \ell_{j'a}| \tag{1}$$

Naturally, the distance between two jobs of the same type d_{jj} is zero (they utilize the same skills), and the distance between j and j' is the same as the distance between j' and j. Occupational distances as in (1) allow for pairwise comparisons between one job and another: the larger the distance in the skill space between two jobs, the more dissimilar are the jobs. Such pairwise distances predict occupational transitions with great accuracy, as shown in Appendix C.2. Furthermore, literature has shown that such distances can account for individual wage dynamics and occupational mobility, especially of young workers (Gathmann and Schönberg, 2010; Guvenen et al., 2020).

I build on the skill distance in (1) to provide a measure of how a specific job j relates to *all* the jobs in a location — not simply to another job j'. Therefore, loosely speaking, I offer a way to distinguish between jobs whose skill profile is similar to *many* jobs in a local labor market (skill-central jobs), in contrast to jobs whose skill profile is similar to *few* jobs in a local labor market (skill-remote jobs). To do so, I introduce the concept of local skill remoteness as follows:

$$\mathcal{R}_{jc} = \sum_{k=1}^{J} \omega_{kc} d_{jk} \tag{2}$$

where $c = 1, \ldots, C$ denotes local labor markets (cities) and I omit the time subscript for ease of notation. A job's local skill-remoteness is a weighted average of pairwise skill distances, where the weights reflect the spatial distribution of economic activity.

[4] Using the Euclidean distance in (1) yields very similar results.

In my baseline implementation, local skill remoteness explicitly takes into account a location's peculiar job mix through the use of each job's local employment shares as ω_{jc}. These are used to weigh the pairwise distances across jobs' skill profiles, thus providing a way to emphasize the skill distance between j and other jobs which are plentiful in market c (while playing down the skill distance between j and jobs that account for a smaller employment share in c). As a consequence of the design in 2, any job j will display a different level of local skill remoteness across locations, depending on what other jobs make up the local job mix. At the same time, jobs j and j' will have different $\mathcal{R}_{jc}$ even when in the same location because of the differences in each job's skill attributes, and how these relate to the skill profiles of other jobs in c.

There is another insightful way to see how local skill remoteness captures the availability of jobs which are meaningful possibilities for a worker whose last job is in occupation j.[5] Let me rewrite (2) as

$$\mathcal{R}_{jc} = \sum_{k=1}^{J} d_{jk}\omega_{kc}$$

so that a job's local skill remoteness is a weighted average of local employment shares, where the weights are the pairwise skill distances from job j to any other job j'. For a given employment share, the larger the skill distance, the higher the weight of occupation j' (a job "possibility") in j's local skill remoteness. Indeed, a large skill distance between the two occupations implies that job j' is, to some extent, "meaningless" to workers in occupation j. As a consequence, the larger the skill distance between j' and j, the more the local share of jobs in occupation j' will contribute to make j skill-remote in c.

In what follows, let median skill remoteness be denoted by $\mathcal{M}_{ct}$ and correspond to the local skill remoteness of some job j^m_{ct} in a city c at time t, such that 50% of jobs have higher local skill remoteness than $\mathcal{M}_{ct}$ and 50% of jobs have lower local skill remoteness value than $\mathcal{M}_{ct}$. I will use the term "locally skill-remote job" (or simply, "skill-remote job") for any job whose local skill remoteness is larger than $\mathcal{M}_{ct}$, and "locally skill-central" (or simply, "skill-central") for any job whose local

[5] I thank an anonymous referee for suggesting this interpretation.

skill remoteness is smaller than $\mathcal{M}_{ct}$.[6]

2.2 Empirical implementation

2.2.1 Occupational skills data from O*NET

To operationalize the notion of local skill remoteness in (2), I follow the literature in presuming that the first-order dimension of skill heterogeneity is at the occupation level, thus I identify "jobs" with "occupations" (Kambourov and Manovskii, 2008). I then use the Occupational information Network (O*NET) as my primary source for occupational skills in the U.S. labor market.[7]

O*NET describes more than 900 detailed occupations in the United States, spanning the years 2000-2017. Its core information is the mix of knowledge, skills, and abilities that occupations require. To conduct my analysis, I identify 22 aggregate occupational groups corresponding to the 2-digit SOC classification. Examples are Computer and Math occupations; Healthcare occupations; Sales occupations; Production occupations; Transportation occupations. To characterize the skill portfolio of these occupational groups, I use the *Skills* descriptor. In the O*NET questionnaire, skills are defined as "the ability to perform a task well, usually developed over time through training or experience, that can be used to do work in many jobs or in learning." Workers are asked to indicate " the level [of each skill] *needed* to perform the [worker's] current job" on a scale from 1 to 7. For example, the Skills descriptor in O*NET explicitly considers abilities and attitudes a worker may learn and ameliorate on the job, such as *Active Listening*, *Service orientation* and *Complex problem solving*.[8]

Skill data from O*NET complements other sources and offers a few advantages

[6]The average, city-level skill remoteness of jobs is computed as follows:

$$\bar{\mathcal{R}}_c = \sum_{j=1}^{J} \omega_{jc} \mathcal{R}_{jc} \tag{3}$$

[7]The literature also recognizes the role of industry-specific knowledge and skills. However, data on industry-specific skill portfolios does not exist. Consequently, although local skill remoteness only varies at the occupation-city level, all empirical specifications in this paper will include extensive industry controls (Neal, 1995, 1999; Pavan, 2011).

[8]The literature has used with success also individual qualities — such as *Physical strength* or *General intelligence* (Poletaev and Robinson, 2008) — and operations workers carry out — such as *Setting limits and tolerances* (Autor et al., 2003). A full list of occupations and related skills, in addition to several illustrative examples, is found in Appendix A.1.

of their own. Skills characterize human capital developed on-the-job more closely than other attributes, since they explicitly identify characteristics "developed [...] through training or experience". By definition, then, skills can be taught, practiced, and potentially used in many professions — while specific tasks or innate abilities cannot. Therefore, focusing on skills, rather than tasks or abilities, is particularly informative to provide insights towards promoting the career development of displaced workers.[9] At the same time, much literature highlights how skills are not perfectly transferable across jobs, thus generating a cost to occupational switches and a potential role for the last job's local skill remoteness in explaining the consequences of layoff (Neal, 1995, 1999; Kambourov and Manovskii, 2009a,b; Poletaev and Robinson, 2008). Indeed, I find that the skill distance as measured through O*NET data is a major determinant of cross-occupational flows (Appendix C.2).[10]

As an example of occupational skills familiar to most readers of this article, consider the occupation of an economist. As described by O*NET, the three skills an average economist uses the most are *Critical Thinking*: using logic and reasoning to identify the strengths and weaknesses of alternative solutions; *Mathematics*: using mathematics to solve problems; and *Reading Comprehension*: understanding written sentences and paragraphs in work related documents. Among the skills that economists do *not* use in their job, we find skills closely associated with social work, such as *Social Perceptiveness*: being aware of others' reactions and understanding why they react as they do; or skills associated with manual jobs, for example *Operation Monitoring*: watching gauges, dials, or other indicators to make sure a machine is working properly. Given the skill profile described above, the "closest" occupations to economists (in the skill space) are Operations Research Analysts, Statisticians, and Sociologists. Among the "furthest", instead, Nurse Practitioners, Registered Nurses, and Nurse Midwives, and Welding and Soldering Machine

[9] Training programs received considerable attention after the 2008-2009 Recession, since job losses were concentrated in declining sectors (Jaimovich and Siu, 2020), and the U.S. government greatly increased public spending on training programs for laid-off workers. Though historically deemed of little comfort, recent scholarship has highlighted their usefulness in attenuating the consequences of job loss (Hyman, 2018).

[10] In addition, thanks to the O*NET design, skills also offer an exhaustive description of occupations that is standardized and overall consistent both over time and across jobs: 7 standardized levels of 35 skills for each occupation, reported annually. Being primarily derived from job incumbents, O*NET data may also be less prone to classification and coding errors by "occupational experts", unlike occupational descriptions in the Dictionary of Occupational Titles (DOT). The DOT was compiled by analysts who attributed task scores based on job descriptions, resulting in a non-standardized, volatile set of tasks attributed to various jobs over time.

Operators. Appendix C offers further illustration of how, in practice, various jobs are described by multidimensional skill vectors and relate to each other through the distance between these skill vectors.

2.2.2 Cities' occupational mixes from OES

With pairwise skill distances between all occupations on hand, I consider how differences in the local job mix can be reflected in my measure of local skill remoteness. I weight pairwise skill distances (derived from O*NET skill scores) by local occupational *employment* shares.

I derive local occupational employment shares from the Occupational Employment Statistics (OES). The OES, a data product of the Bureau of Labor Statistics (BLS), is an establishment-based survey of employers that reports data on the occupational composition of more than 500 metropolitan and micropolitan urban areas in the U.S. since the early 1990s.[11] I identify metropolitan statistical areas as the appropriate spatial unit of analysis and refer to these as "cities".[12] This choice is motivated by the specialization patterns documented by economic geography literature (Moretti, 2004, 2011; Hsieh and Moretti, 2015; Davis and Dingel, 2019, 2020) and evidence that more than 80% of job applications by unemployed workers are concentrated in their CBSA of residence (Marinescu and Rathelot, 2018). I further discuss this empirical strategy, and compare it with vacancy-based weights, in Appendix B.1, where I show that data on employment shares is available for a larger cross-section and a longer time series than data on vacancy shares, and the two are very highly correlated in the smaller subset of years, occupations, and cities for which both variables are available.

3 Skill remoteness in the cross-section and over time

Local skill remoteness is a characteristic of jobs (in fact, of occupations) that depends on (a) how jobs compare in the multidimensional skill space and (b) the geographical distribution of jobs across physically distinct labor markets. In what follows, I first illustrate the extent of cross-sectional variation in the local skill remoteness

[11] According to the Office of Management and Budget (OMB) that defines these geographical units, "a metropolitan area contains a core urban area of 50,000 or more population. A micropolitan area contains an urban core of at least 10,000, but less than 50,000, population."

[12] To be precise, I use 2013 delineations for Core Based Statistical Areas, or CBSAs, excluding those with fewer than 50,000 inhabitants (micropolitan areas).

of *filled* jobs. The goal is to offer some comforting evidence on the proposed local skill remoteness measure, and reveal underlying patterns of specialization across space and occupations. To do so, I study individual employment histories from the Current Population Survey (section 3.1).[13] Then, in section 3.2, I proceed to show that a job's local skill remoteness is predictive of the likelihood that the job will be destroyed, resulting into a separation for the worker who used to be employed in it.[14]

3.1 Local skill remoteness across occupations and locations

I compute local skill remoteness using O*NET, OES, and CPS data, and consider 442 metropolitan areas (cities) and 20 occupational groups over the period 1994-2017. A city-occupation pair defines the scope of a local labor market in each year.[15] Table 1 offers an overall view of the resulting local skill remoteness measure, with selected occupations illustrating the extent of variation in the data. After standardizing local skill remoteness to have a unit variance, the measure ranges from a minimum of 1.22 (Sales workers, in Palm Coast, FL, in 2009) to a maximum of 9.02 (Math & Computer workers, in Elkhart-Goshen, IN, in 2000). Different occupations have diverse mean and variance over space: Management, Food, and Office & Admin occupations are the least skill remote in an average sense, while Computer & Math, Architecture & Engineering, and Production are the most skill remote. This observation is consistent with the intuition that the skills in Production or Architecture & Engineering are more specialized to those occupations, while the skills used in Management or Office & Admin jobs are common to many occupations, and this ranking loosely holds in all locations. Sales and Cleaning & Maintenance occupations also have the least variance across space (0.38 and 0.39), while Architecture & Engineering and Production occupations have the largest (1.10 and 1.22): this suggests that the spread across U.S. metropolitan areas' share of employment in Sales and Cleaning & Maintenance occupations is larger than in Architecture & Engineering and Production occupations. In other words, employment in the latter occupations tends to be more spatially clustered than in the former.

[13] As a cross-sectional dataset, the CPS offers a comprehensive and detailed view of employment and unemployment across detailed occupational and geographical cells.

[14] To this end, I exploit the CPS' short panel dimension and compute monthly transitions from and into non-employment, by local skill remoteness. Since the CPS is a survey of individuals, not of jobs, I attribute to each worker a skill remoteness value according to the metropolitan area and occupation of her current job (or last job if looking at the non-employed).

[15] Details are in appendix A.1.

Table 1: Local skill remoteness, summary statistics for 442 CBSAs and 21 occupational groups, in the period 1994-2017. There is substantial heterogeneity in the local skill remoteness of jobs across locations and occupations. Source: author's calculation from CPS-ONET-OES data.

(a) Across Occupations

	Mean	Std. Dev.	25th Pct	Median	75th Pct	*cjt* cells
All occs. – raw	3.71	1.16	2.92	3.51	4.13	232,050
All occs. – std	**3.20**	**1.00**	**2.52**	**3.03**	**3.56**	**232,050**

(b) Across Metropolitan Areas, Within Occupations

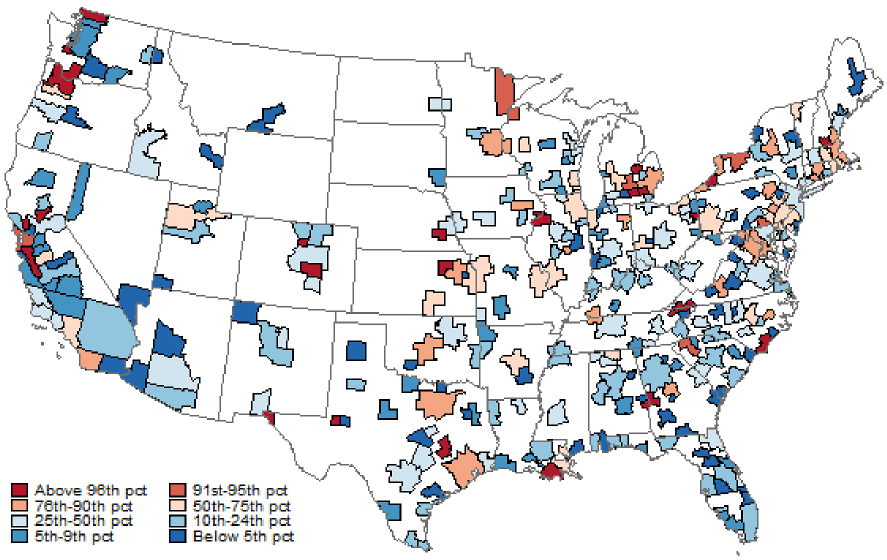
	25th Pct	Median	75th Pct
Management	3.39	3.58	5.41
cbsa	Little Rock, AR	Memphis, TN	Modesto, CA
(year)	(2015)	(2005)	(2004)
Food	4.10	4.66	6.08
	Worcester, MA	Las Vegas, NV	New York, NY
	(2017)	(2008)	(2000)
Office & Admin	4.11	5.03	6.22
	Minneapolis, MN	Stamford, CT	Providence, RI
	(2010)	(2004)	(2003)
Healthcare	4.28	5.23	6.12
	Charlotte, NC	Sarasota, FL	New York, NY
	(2005)	(2004)	(1996)
Cleaning & Maintenance	5.04	5.66	6.78
	Tucson, AZ	Huntsville, AL	Chicago, IL
	(2005)	(2015)	(2000)
Sales	4.73	5.76	6.76
	Tucson, AZ	Los Angeles, CA	San Francisco, CA
	(2014)	(2007)	(2008)
Production	7.02	8.25	8.88
	Charlotte, NC	Fresno, CA	San Diego, CA
	(2014)	(1996)	(1998)

Table 1 also illustrates how my local skill remoteness measure is consistent with stylized facts about occupational specialization patterns across U.S. cities. Figure 1 generalizes the points shown by table 1 by depicting variation in the skill remoteness of the local job mixes across all U.S. metropolitan areas. In the figure, metropolitan areas in shades of blue have a local skill mix that is less skill remote than the median. In other words, jobs in these locations are more similar to each other than in the median city. On the other hand, metropolitan areas in shades of red have a local skill mix that is more remote than the median — which implies that jobs in those cities have more distinct skill profiles than in the median city.

A few examples help in fixing ideas. Tucson, AZ is the port of entry for a large portion of U.S.-Mexico trade, accounting for over $10 billion in annual sales and more than 100,000 jobs in the state of Arizona alone (`cbp.gov`, 2017). Therefore, it is unsurprising that occupations in the SOC Sales grouping are relatively skill-central in Tucson. On the other hand, jobs in San Francisco, CA are disproportionately in Computer & Math and Engineering, occupations that use different skills than Sales occupations — so Sales occupations are relatively skill-remote in San Francisco. Or consider again the example of Charlotte and San Diego that was mentioned in the introduction. The Charlotte, NC metro area (which encompasses also the cities of Concord and Gastonia and a few counties in South Carolina) has developed into a hub of production and transportation jobs, with American Airlines, Lowe's, Compass, and Carolina Beverage Corporation Inc. (two food manufacturers) among the local large employers. Local skill remoteness reflects the local skill composition of jobs and illustrates that Production jobs are relatively skill-central in Charlotte, NC. The opposite is true in San Diego, CA where the largest employers are the University of California San Diego and its associated hospital system, two large Healthcare providers (Sharp HealthCare and Scripps Health) and Qualcomm (a creator of semiconductors, software, and services related to wireless technology).[16]

[16] It is worth underlining how the local share of Production occupations is not the *only* factor determining those occupations' local skill remoteness. After all, Lowe's is a retail trade company and American Airlines an air transportation one. The key observation is that the skills required in average retail and transportation jobs are more similar to those utilized in the average production job, than those in a typical healthcare or engineering position. Hence, Production occupations are relatively more skill-central in Charlotte, NC than they are in San Diego, CA. The table also reports different years that are connected to a specific value of local skill remoteness for any occupation-city pair. Though in theory the skill data from O*NET is updated every year for a least a subset of occupation, in practice there is a limited amount of time variation in occupational skill vectors over time. The limited time variation translates into local skill remoteness also being a rather

Figure 1: Average city-level local skill remoteness of jobs across 382 CBSAs over the period 1996-2017. There is substantial heterogeneity in the local skill remoteness of jobs across locations. Source: CPS.



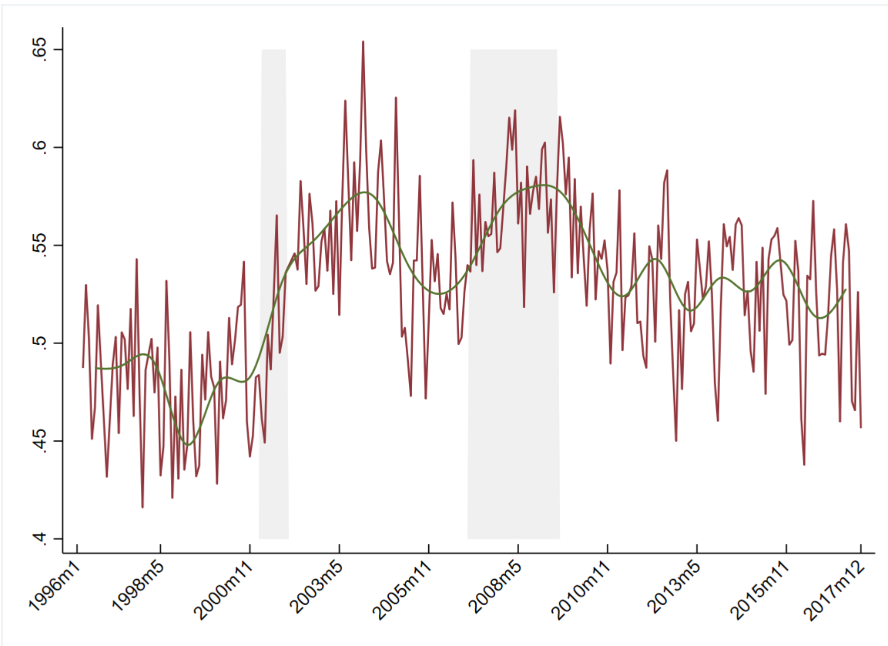
In conclusion, I find that the data displays substantial heterogeneity in local skill remoteness both within occupations and across locations, and within locations across occupations. The variation in local skill remoteness reveals how jobs relate to each other and how different cities compare in terms of their skill mix. It is this variation that I will exploit in my subsequent analysis.

3.2 Skill-remote jobs are more likely to be destroyed than skill-central ones during recessions

The previous section explored variation in the local skill remoteness of jobs, across occupations and locations. I now turn to job dynamics. First, I measure the propensity of workers to separate from their job, as it varies with the local skill remoteness of their occupation and over the business cycle. In recessions, the share of workers separating from locally skill-remote jobs increases disproportionately. In the data, however, there exist virtually no difference in the re-employment probability of workers whose last job was skill-remote or skill-central (the median unemployment duration is 12 weeks for both groups, with 1 in 4 workers in both groups having unemployment spells of at most 4 weeks). Therefore, I conclude that locally skill-

slow-evolving objects; this is partially due to the skill data, but is also also an artifact of using OES employment shares, which are constructed as 3-years moving averages.

Figure 2: Percentage of workers separated from a job more locally skill remote than the median job. This percentage increases in recessions (shaded areas). The red line is raw monthly transitions, while the green line is smoothed through an interpolated cubic spline. Source: CPS 1996-2017.



remote jobs are more likely to be destroyed than the median job during recessions. This fact suggests that the local skill remoteness of the last job is a potentially useful characteristic to shed light on the cyclicality in the consequences of job loss.[17]

The percentage of destroyed jobs that are more locally skill-remote than the median job increases during economic downturns. This fact is illustrated by figure 2, which plots this percentage over time, computed as the percentage of jobs that (i) end in separation (layoff) at month t, and (ii) are more locally skill remote than the median job at month t. One can see that, during recessionary periods, the percentage of

[17] In the CPS, one can measure employment-to-unemployment transitions at the monthly frequency. This implies that unemployment spells may go undetected if the separation occurs after the interview date in month m but a new job is found before the interview date in the subsequent month $m+1$. If workers losing a skill-remote job were more likely to experience this kind of very short unemployment spells, it would be unclear whether skill-remote jobs are more likely to be destroyed in recessions or simply more likely to lead to longer unemployment spells. However, both the raw data on unemployment duration, shown in appendix C (Table C.5), and the controlled evidence on hours worked after layoff in section 4.3, provide support for the hypothesis that differences in post-layoff re-employment rates are minimal between skill-remote and skill-central laid-off workers.

Table 2: The percentage of destroyed jobs that are more locally skill-remote than the median job is larger in recessions (60.3%) than in booms (46.6%). "Layoffs only" denotes separations occurring because of (i) plant closure or plant distress, or (ii) layoff or position abolished. CPS 1996-2017.

% of jobs destroyed that are locally skill-remote			
	Recessions	Booms	Std. Dev.
Layoffs only	55.2	49.4	2.1
All separations	60.3	46.6	3.0

jobs which are more skill-remote than the median job increases.

How large is the countercyclical increase depicted in figure 2? To assess this, I regress the percentage of lost jobs that are skill-remote on a recession indicator (based on the NBER business cycle dates). Controls include detailed occupation fixed effects. I find that when looking only at layoffs, the cyclical disparity is equal to 6 percentage points and, for all separations, it is equal to approximately 14 percentage points (respectively equivalent to about three and four times the standard deviation of the outcome variable, see table 2). I conclude that jobs destroyed in recessions are more likely to be locally skill-remote and this difference is economically meaningful.

4 Skill remoteness and post-layoff labor market outcomes

In this section, I quantify the effect of losing a locally skill-remote job on a worker's post-layoff earnings, employment, and mobility. I take an empirical approach that exploits the richness of longitudinal micro-data to estimate this effect net of unobserved individual characteristics as much as possible.[18] I further use a variety of data sources to distinguish the effect of local skill remoteness from many potential confounders, including city size and the local density of college-educated workers. Through a series of thoroughly controlled regressions, I find a robust, large, and per-

[18]The effects of the last job's local skill remoteness on a worker's labor market outcomes are better identified in the case of layoffs as opposed to any type of separation. Layoffs are separation episodes not initiated by the worker and not related to the worker's performance or conduct on the job, and typically identified in the data as plant closures or positions abolished. Therefore, in layoff cases, unobserved characteristics of the separating worker are not likely to play a significant role in the decision to terminate the employment spell.

sistent negative association between losing a locally skill-remote job and a worker's post-layoff earnings. Ex ante, it needs not be so. Losing a locally skill-remote job could have been a proxy for having experience with a unique (and, therefore, in high demand) set of skills and tasks. However, the data does not support this interpretation. On the contrary, I find that the majority of the earnings losses associated with layoff from a skill-remote job are accounted for by lower wages at re-employment — themselves associated with occupational change and larger skill distance between the new and the old job.

4.1 Displaced workers in the NLSY79

To study how the local skill remoteness affects individual labor market outcomes after layoff, I draw on detailed individual labor market histories in the National Longitudinal Survey of Youth 1979 (NLSY79).[19] The NLSY79 is a comprehensive survey of American residents who were aged between 14 and 22 in 1979. NLSY79 respondents answer questions about current and previous jobs and, from this information, a longitudinal record spanning from the date of the first interview through the most current interview date is constructed for each respondent. As a result, the NLSY79 Work History Data provides researchers with a week-by-week longitudinal work record of each NLSY79 respondent.[20] Importantly for the purposes of this paper, NLSY79 interviewers go to great lengths to follow individuals as they migrate across cities. Therefore, the data allows me to study changes in local skill remoteness that occur as individuals move across space and take different jobs in different places.

I focus on workers who lost their job involuntarily, because of (i) plant closure or plant distress, or (ii) layoff or position abolished (i.e., displacement episodes). In particular, I exclude non-employment episodes that originate in a firing or a quit,

[19] Specifically, I use the restricted-use geo-coded version of the data to identify the respondents' metropolitan area of residence.

[20] Data is available from from January 1, 1978 through December 31, 2012 (covering up to December 2014, for the last wave). Though NLSY79 data is available since 1978, I use it only since 1994 because in that year the survey underwent a major re-design to implement, among other improvements, dependent-coding techniques that make employment and occupational affiliation data significantly less prone to measurement error. Furthermore, although the data is weekly, because respondents are interviewed once a year, changes in wages and occupational status are often registered once or twice a year only, depending on how often the respondent changes job. To mitigate recall bias but still offer a detailed picture of how the effect of skill remoteness changes over time, I aggregate the panel at the monthly level.

because, in those cases, the estimated effects of skill remoteness on post-separation outcomes would likely be contaminated by selection on unobservables. A large and very fruitful literature has treated displacement events as plausibly exogenous job loss episodes, and I follow this interpretation as well.[21] I restrict the sample to individuals who have complete information on their occupation and metropolitan area (CBSA) of residence at the time of layoff, and are also observed in the sample for at least 12 consecutive months prior to and 48 months after the job loss episode. I exclude CBSA-occupation cells that have fewer than 5 individuals per year. I exclude workers who, at the time of layoff, were usually employed for fewer than 35 hours per week or 50 weeks per year, working without pay, self-employed, or given a recall notice by their employer. Of these workers, I drop those laid-off from a job that had lasted at most 1 quarter. For all others, I use information on the occupation and city of residence at layoff to attribute a value of local skill remoteness to each individual according to the last job held.

When matched with the O*NET-OES derived measure of local skill-remoteness in this way, the final sample contains 2,009 individuals, observed across 20 years, 212 metropolitan areas (CBSAs), and 22 occupational groups. The average number of individuals per metro area is 9.5 and the average number of individuals per occupation is 91. The average number of layoff episodes per worker ever laid-off in the NLSY79 is 1.5. The mean value of local skill remoteness at layoff is 3.67, with a standard deviation of 1.

Laid-off workers in my NLSY79 sample are 29 to 56 years old at the time of layoff, and used to be employed disproportionately in Production, Transportation, and Material Moving occupations (25%), Office and Administrative Support occupations (20%), and Managerial and Sales occupations (15%). Layoff rates increase in recession periods, as expected: 2001 accounts for almost 10% of all layoff episodes in the sample, while the Great Recession years (2008-2009) for over 14% — an equal distribution would result in each year accounting for 5%. As can be seen in table 3, 44% of the sample is female, 12% has attained less than a high school diploma, and 48% has at least a high school diploma or equivalent but no college degree. The median (mean) monthly earnings at layoff is around \$2,080 (\$2,540) — a little

[21] Specifically, Jacobson, Lalonde and Sullivan (1993), Davis and von Wachter (2011), Huckfeldt (2021), Krolikowski (2017), Farber (2017), and Jarosch (2021) also consider exogenous job loss episodes. In the CPS Displaced Worker Survey, displaced workers are also identified precisely as those "who lost or left jobs because their plant or company closed or moved, or their position or shift was abolished". See section 5 for empirical analysis with DWS data.

Table 3: The NLSY79 laid-off workers sample (1994-2014, metropolitan areas only)

	all	below median remoteness	above median remoteness
Observations	139,633	69,924	69,709
Individuals[a]	2,009	1,196	1,072
Layoff episodes	3,203	1,703	1,500
Layoffs per individual	1.59	1.42	1.40
Layoffs per year	160	85	75
% female	43.93	54.67	31.73
% non-white	37.98	37.58	36.67
% less than high school	11.91	12.62	13.13
% high school but no college	47.46	48.09	46.73
Age at layoff (median)	43	43	42
Tenure at layoff (median, in months)	80	77	86
Earnings at layoff (median, monthly)	2,080	1,888	2,275
Usual hours worked at layoff (median, weekly)	40	40	40
Wage at layoff (median, hourly)	12.75	11.82	14.00
% changed occupation after layoff	35.25	33.70	37.17
% changed CBSA after layoff	4.99	4.05	6.02

[a]Columns 2 and 3 do not sum to column 1 because some individuals are above median skill remoteness for one layoff episode and below for another.

higher for workers above median skill remoteness (at $2,275), who also have somewhat longer tenure on the job, than for workers below median skill remoteness (at $1,888). The median (mean) tenure on the job was of 80 (173) months. After layoff, I find that 5% of the sample has moved to a different CBSA and approximately 1 in 3 workers has changed occupation. These proportions are a bit higher for workers above median skill remoteness.

4.2 Skill-remoteness and earnings

As highlighted in much literature, displacement is followed by a significant and persistent drop in earnings. On average, laid-off workers in the NLSY79 earn only about 60% of their pre-displacement earnings 4 years after job loss, similarly to what previous studies have shown (Jacobson et al., 1993; Farber, 2011, 2015, 2017; Davis and von Wachter, 2011). In addition, I document that post-layoff earning losses are systematically correlated with local skill remoteness at layoff.

4.2.1 Empirical design

To investigate post-layoff earnings losses as a function of skill remoteness, I run a fixed-effects regression of monthly individual earnings on local skill remoteness at the time of layoff.[22] I consider a time frame between 12 months before and 48 months after layoff.

$$earnings_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it} \tag{4}$$

I denote by t_0 the month of layoff and by i the individual. The outcome variable $earnings_{it}$ is monthly earnings for worker i in month t. The explanatory variable of interest is $\texttt{above}_{it_0}$, an indicator that takes value 1 when worker's i job at the time of layoff t_0 exceeded the median skill remoteness, and 0 otherwise.

I follow the literature and estimate the effect of losing a locally skill-remote job by interacting the $\texttt{above}_{it_0}$ dummy with a series of time indicators that "turn on" at different dates since layoff. In this fashion, the coefficients trace out the post-layoff outcomes for workers as a function of their local skill remoteness at layoff, in excess of the pure effect of time (before and after layoff). Furthermore, notice that the regressor of interest, $\texttt{above}_{it_0}$ is determined by the occupation and city of residence of worker i at the time of layoff. This empirical design prevents contamination of the estimates of δ_m by occupational changes and migration episodes that happen after layoff. Standard errors are clustered at the individual level.

My preferred regression specification includes a large variety of controls, summarized by $\mathbf{X}_{it}, \mathbf{X}_{it_0}$ in (4). These include a vector of fixed effects for individual α_i, and fixed effects for the calendar date (month-year), the worker's CBSA, occupation, and industry at the time of layoff (respectively 212, 22, and 21 categories), in addition to indicators for the month of re-employment (60 categories). The latter indicator variable proxies for unobservables that drive unemployment duration and could potentially confound the estimates of the coefficients of interest, δ_m.[23] I also include in the control set indicators for the individual's age, and indicators for whether

[22] I restrict the attention to labor income, so that earnings are zero for non-employed workers.

[23] A large body of literature has analyzed the prevalence and interpretation of duration dependence, i.e. the negative relationship between the duration of a jobless spell and the worker's probability of finding new employment. Most recent studies, among which Alvarez, Borovickova, and Shimer (2019), find that negative selection in the pool of long-term unemployed accounts for most of this relationship, while "true" duration dependence has a negligible role. For this reason, I control for unemployment duration using 60 dummies for the month of re-employment.

the individual has changed occupation, industry, or metropolitan areas of residence after layoff — following the literature that finds a significant role of such changes in explaining post-layoff earnings losses (Neal, 1995, 1999; Huckfeldt, 2021).

Cognizant of the spatial economics literature and the important agglomeration effects it documents, I also include controls for city size, the local share of college-educated workers, the local unemployment rate, the local share of employment in the same occupation as the worker's last job, and a shift-share style control that proxies for the level of local labor demand. Finally, I also allow the effect of city size to vary by occupation at layoff, to encompass differential agglomeration effects by occupational groups. Including these controls nets out the effect of workers' unobserved characteristics that are correlated with both their location choices before layoff and their earnings after layoff.[24]

4.2.2 Results

After netting out the effect of the aforementioned covariates, I find no association between one's job's local skill remoteness at layoff and pre-layoff earnings. Conversely, the data displays a negative correlation between skill remoteness at layoff and post-layoff monthly earnings. Figure 3 depicts the estimated earnings losses for laid-off workers whose last job was either above (red) or below (blue) the median local skill remoteness. Earnings losses are expressed in contemporary dollars. The figure displays the familiar V-shaped pattern featured in all studies of post-layoff earnings and illustrates that: (i) the effect of skill remoteness at layoff on post-layoff earnings is negative; (ii) in contrast, a job's local skill remoteness does not display any effect on pre-layoff earnings.[25]

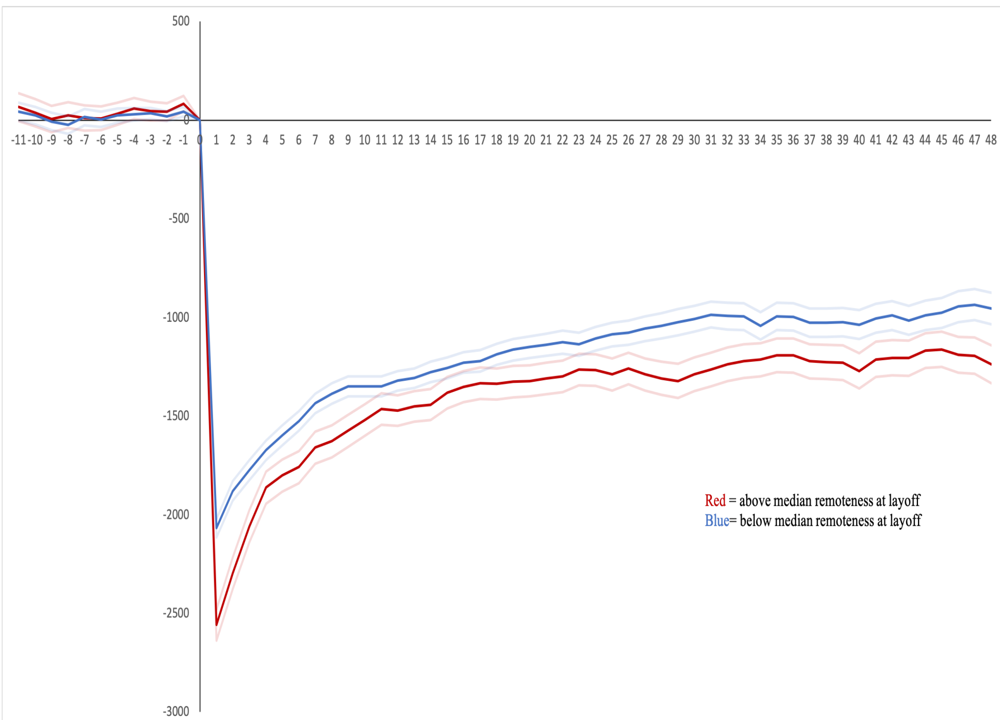
[24]Specifically, the set of geographic controls is as follows: (i) log-population, as in the 2000 Decennial Census; (ii) city-level share of college-educated workers — in the spirit of Moretti (2004; 2011); (iii) city-level share of people younger than 25 — as per the "young workers hypothesis" in Shimer (2001); (iv) city-level unemployment rate, constructed from the Local Area Unemployment Statistics at any date (following Davis and von Wachter, 2011); (v) the city-level employment share of the lost job's occupation from OES; (vi) a Bartik-style control (Bartik, 1991; Blanchard and Katz, 1992), constructed as follows:

$$\texttt{bartik}_{oct} = \sum_{c' \neq c} \omega_{oct-1} \Delta empl_{oc't}$$

where ω_{oct-1} is the share of employment in occupation o, city c, time $t-1$, Δ indicates the first difference time-series operator, and c' are all cities in the data different from c. Unless otherwise noted, all control variables are computed from the monthly Current Population Survey 1994-2017.

[25]The precise estimates are reported in column 1 of table 4, and in greater detail in tables C.7 and C.8 in Appendix C.5, which also reports the same graph and tables in percentage terms as

Figure 3: Regression (4): skill remoteness at layoff is *negatively* associated with monthly earnings after layoff. Overall, in the course of 4 years after layoff, workers who were laid-off from a locally skill-remote job earn over \$10,000 less than workers who were laid-off from a locally skill-cebntral job. Source: NLSY79, laid-off workers sample, 1994-2014.



The difference in post-layoff earnings between skill-remote and skill-central workers is substantial. In the month of layoff, the earnings of workers whose last job was locally skill-remote are on average \$491.18 lower than workers whose last job was locally skill-central (about 25% larger of the estimated baseline loss, which is \$2067.58 at t_0). The difference is still economically and statistically significant until 4 years after job loss and stable at around \$200 per month. At 6 months since layoff, workers who lost a skill-remote job earn \$224 per month less than those who lost at skill central one. At 12 (24, 36, 47) months, the difference is about \$142 (\$202, \$196, \$283), hovering between 15% and 20% of baseline estimated losses.[26] Overall,

a fraction of estimated pre-layoff average earnings for both workers who lost a skill-central or a skill-remote job(figure C.2, tables C.9), C.10.

[26] I also test for increasingly negative effect as the lost job exceeds higher levels of local skill remoteness. Appendix table C.11 report coefficients from regression (4), but using indicators for the job exceeding the 75th, 90th, and 50th percentile (the baseline). Negative effects associated

in the course of 4 years after layoff, workers who were laid-off from a locally skill-remote job earn an estimated \$10,111.49 *less* than workers who were laid-off from a locally skill-central job — approximately 5 months of the median worker's pre-layoff income. On average, the earnings losses after layoff are 17.7% larger for skill-remote workers over a 4-years period (see table C.7 and C.8 in Appendix C.5).[27]

4.3 Hours worked and wages at re-employment

Losing a skill-remote job is associated with larger earning losses than losing a skill-central one. In this section, I ask whether the robust negative association between skill remoteness at layoff and earnings after layoff is due to lower hours worked because of non-employment or under-employment, or rather lower wage rates upon re-employment. I find that lower wages account for practically the entirety of earnings losses associated with local skill remoteness. The data does not support the hypothesis of more persistent or frequent non-employment episodes for workers who lost a skill-remote job vis-a-vis those who lost a skill-central one.

To show how the extensive and intensive margins of employment affect post-layoff earnings, I articulate my empirical strategy across two different regressions and report the resulting estimates in table 4 columns 2 and 3. The first of these two columns depicts the estimated coefficients from a fixed effects regression where the outcome is hours worked at various dates after layoff, which are equal to 0 when non-employed. Column 3, on the other hand, reports the estimates from a similar fixed effects specification that studies hourly wages after layoff. The empirical design is identical to the earnings regression (4), with all controls included. Standard errors

with losing a job exceeding higher skill remoteness percentiles are larger than those associated with losing a job which is more skill-remote than the median.

[27]Everything else equal, a smaller share of local jobs in the exact same occupation as the lost job can exacerbate the negative earnings effects associated with local skill remoteness. In a regression specification in which the local skill remoteness and the employment share of the lost job's occupation are *both* included, and interacted with time dummies, I find that there is a significant negative earnings effect of losing a job in a city where the lost job's occupation is a smaller share of total employment. This effect is distinct from the earnings losses associated with skill remoteness at layoff, which are large and precisely estimated also in this augmented specification. See table C.12.

Table 4: Higher skill remoteness at layoff is associated with lower earnings after layoff (column 1). The data does not support a negative effect on hours worked, however, so that earnings losses are mostly accounted for by lower wages at re-employment (columns 2 and 3). Monetary values are in contemporary dollars; variables are at monthly frequency unless otherwise specified. Source: NLSY79, laid-off workers sample, 1994-2014.

t	**Earnings**	**Hours worked**	**Hourly Wage**
-6	6.16	-.012	-0.33
	(60.21)	(2.75)	(0.30)
-1	41.71	-0.30	0.02
	(49.02)	(1.44)	(0.18)
0	-491.18	-1.62	-1.94
	(78.34)	(2.61)	(0.40)
1	-416.53	-1.01	-1.54
	(80.53)	(2.79)	(0.41)
2	-285.51	0.95	-1.17
	(81.66)	(2.89)	(0.40)
3	-189.34	4.73	-0.62
	(81.83)	(2.94)	(0.41)
6	-223.99	-1.31	-0.82
	(81.74)	(2-92)	(0.40)
9	-171.28	3.21	-0.63
	(80.01)	(2.96)	(0.39)
12	-142.76	5.24	-0.52
	(76.25)	(2.89)	(0.37)
18	-161.70	0.50	-0.78
	(79.83)	(3.05)	(0.39)
24	-202.37	-0.06	-1.00
	(80.71)	(3.07)	(0.40)
30	-278.14	-1.91	-1.34
	(84.61)	(3.13)	(0.41)
36	-196.12	0.73	-0.88
	(87.62)	(3.3.7)	(0.43)
42	-191.55	-0.76	-0.97
	(90.01)	(3.39)	(0.43)
47	-283.67	-2.84	-1.16
	(95.59)	(3.49)	(0.44)
Cumulative earnings loss	-10,111.49	–	–
Controls	age, geographic confounders, mobility indicators		
FE	individual, time, occupation, industry, city, re-empl. month		
N	124,199	124,199	124,199
R^2 (within)	0.36	0.39	0.29

are clustered at the individual level.[28]

$$h_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it} \quad (5)$$

$$w_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it} \quad (6)$$

The results from estimating regression (5) are consistent with no meaningfully differential reduction in hours worked for workers who lost a skill-remote job with respect to those who lost a skill-central one.[29] On the contrary, workers who lost a locally skill-remote job earn persistently less per hour than workers who lost a skill-central job, and this effect is precisely estimated for at least 48 months since layoff.

As in the earnings regression, there is no association between hourly wage rates before layoff and local skill remoteness at layoff. However, the coefficients becomes negative after layoff, and is equal to -1.94 dollars per hour in the month of layoff. Therefore, losing a locally skill-remote job implies a drop of 14.4% with respect to the pre-layoff median wage of 13.5 dollars. The reduction in wages associated with skill remoteness at layoff is still significant in the four years after layoff and equals on average to -0.95 dollars per hour. For the median worker, who in this sample works 40 hours per week and 52 weeks per year, this translates in a persistent drop in total yearly wages of almost $8,000 over four years (a large portion of the cumulative $10,000 estimated earnings losses).

4.4 Career changes and migration rates

4.4.1 Occupational mobility

Evidence from laid-off workers in the NLSY79 shows that workers displaced from a locally skill-remote job tend to have lower wages at re-employment than workers who lost a skill-central job. In addition, much literature emphasizes how changing

[28] I also run a logit specification where the outcome variable is the individual probability of being employed at any date after layoff and come to the same conclusion, that is, there is no apparent association between post-layoff employment rates and local skill remoteness at layoff.

[29] It bears noting that the estimated regression does predict a significant reduction in hours worked after layoff, just not as a function of the local skill remoteness of the lost job.

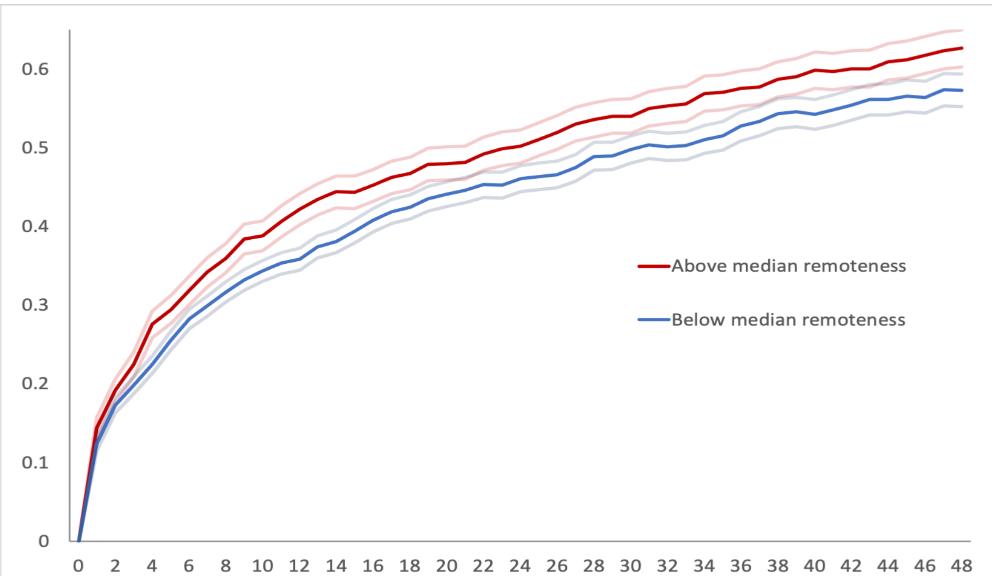
occupation affects wage levels and wage growth over the life-cycle and after displacement episodes (Kambourov and Manovskii, 2008; Gathmann and Schönberg, 2010; Huckfeldt, 2021). Motivated by these findings, I investigate two aspects of the re-employment job that may account for wage disparities between skill-remote and skill-central workers: the job's occupation, and the new job's skill distance from the old one. I find that skill-remote laid-off workers, with respect to skill-central ones, are (i) more likely to change occupation after layoff and (ii) more likely to be re-employed in a job that is "far" from the lost one in the skill space.

Skill remoteness at layoff is associated with increased and more meaningful occupational mobility. To show this, my empirical strategy consists of two regressions: the first one studies the propensity to work in a different 2-digit occupation than at time of layoff at any of the first 48 months *after layoff*. I construct first a set of 48 indicators $\texttt{change}_{it}$ for $t = 0, 1, 2, 3, \ldots, 47$. These variables take value 0 if the workers is employed in the same occupation she had before layoff at $t = 1, 2, 3, \ldots, 47$, and 1 otherwise. (Recall that $t = 0$ indicates the month when layoff occurred.) I then use a linear probability specification and follow the empirical design in equation (4) when it comes to the independent variable and covariates. The estimated coefficients are depicted in figure 4 (and, in greater detail, in tables C.15 and C.16 in the Appendix). On average, workers who lost a skill-remote job are 11% more likely to change occupation after layoff than workers who lost a skill-central job. This difference is economically and statistically significant until 48 months after layoff. At that time 63% of those laid-off from a skill-remote job are employed in a different occupation than the lost job, vis-a-vis 57% of those laid-off from a skill-central job.[30]

Occupational changes are more prevalent among workers who lost a skill-remote job than those who lost a skill-central one. But how substantial are these occupational changes? I now investigate the extent to which a change in occupational affiliation represents a significant change in the skills the worker uses in her job and whether more significant changes are more likely for skill-remote than for skill-central workers. To do so, I compute skill distances between occupations from O*NET data following equation (1). I first calculate the distance between occupation o, the oc-

[30] A few clarifications about figure 4: (i) the depicted propensity is cumulative, that is, a worker who changed occupation at month 1 and has not returned to her pre-layoff occupation at month 2 (3, 4, ...) will be counted as a "switcher" at month 1 and 2 (and 3, 4, ...) as long as she does not switch back to the pre-layoff occupation — a phenomenon that does not appear common in the data. (ii) The sample selection is such that workers are required to be in the same job pre-layoff for at least a quarter, so that is why estimates of occupational change pre-layoff are not available.

Figure 4: Higher skill remoteness at layoff is associated with a higher propensity to change occupation after layoff. Source: NLSY79, laid-off workers sample, 1994-2014.



cupation associated with the lost job, and occupation o', the occupation associated with the job at re-employment. I do so for each individual in my NLSY79 sample who has changed occupation following layoff. For ease of interpretation, I standardize the distance so that it has unit standard deviation and denote it by $d^{std}_{oo'}(i)$. Then, I run a linear regression of the standardized distance $d^{std}_{oo'}(i)$ on an extensive set of fixed effects $\mathbf{I}_{it}$ for year, metro-area, industry, occupation, sex, race, age, education, and marital status, and the local unemployment rate $u_{c(i)t}$, plus an indicator variable $\texttt{above}_{it_0}$ that takes value 1 if the worker's lost job was more skill-remote than the median one at the time of layoff.[31]

$$d^{std}_{oo't}(i) = \alpha \mathbf{I}_{it} + \beta u_{c(i)t} + \gamma \texttt{above}_{it_0} + \epsilon_{it} \tag{7}$$

The effect of skill remoteness at layoff on the skill distance at re-employment corresponds to the coefficient γ in (7). I find that it is equal to 0.242, and statistically and economically significant (standard error equal to 0.0241). I then compare conditional means from regression's (7) to unconditional ones, as depicted in table 5. The skill distance between the old and the new occupation is, for workers who lost a skill-remote job, higher by 15.5% (17%) with respect to the average job switcher

[31] Errors are clustered at the cbsa-year level.

(the average skill-central worker who changed occupation after layoff). To sum up, not only are skill-remote workers more likely to change career after layoff, but they are also more likely to do so going through substantial skill portfolio changes, "traveling" a larger distance in the skill space between the old and new job.

Table 5: Higher skill remoteness at layoff is associated with larger skill distance between the old and new job at re-employment. Source: NLSY79, laid-off workers sample, 1994-2014.

$d^{std}_{oo'}(i)$	group	value	SD/SE
unconditional mean	all workers	0.981	1
unconditional mean	occupational switchers	1.576	0.833
conditional mean from (7)	occupational switchers, skill-central	1.433	0.144
conditional mean from (7)	occupational switchers, skill-remote	1.675	0.161

Does the skill distance between the old and the new job explain some of the earnings losses documented in 4.2.2? The data suggests a positive answer, though the results are noisier (and the sample smaller) than in previous sections. To get at this conclusion I run the following empirical specification on the sample of occupational switchers. As usual, I consider a time frame between 12 months before and 48 months after layoff.

$$earnings_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}}\mathtt{exceed}_{it} + \epsilon_{it} \tag{8}$$

I denote by t_c the month in which worker i changed occupation after layoff. The outcome variable $earnings_{it}$ is monthly earnings for worker i in month t (before and after switching occupation). The explanatory variable of interest is $\mathtt{exceed}_{it}$, an indicator that takes value 1 when the distance between the job i held at t_0 (the "old" job) and the one she holds at t (the "new" job, which started at t_c) exceeds the median skill distance for job switchers.[32] I find that the coefficients δ_m are negative (higher distance between the old and new jobs is associated with lower earnings) and, in magnitude, equal to approximately a third of the coefficients on local skill remoteness estimated in Table 4. In other words, a new job exceeding median skill distance is associated with an average loss of \$60 per month (vis-a-vis an average loss of \$200 per month associated with the old job being above median skill remoteness at layoff). I conclude that skill-remote workers who change occupation after layoff on average "travel" a greater distance in the skill space at

[32] This specification is identical to 4 when it comes to controls and the independent variable, but the explanatory variable and the underlying sample are different.

re-employment. This may account for up to 30% of the overall earnings losses associated with local skill remoteness at layoff — with the caveat that coefficients on skill distance are estimated with noise and often are not statistically significant. Details are in Appendix table C.17.

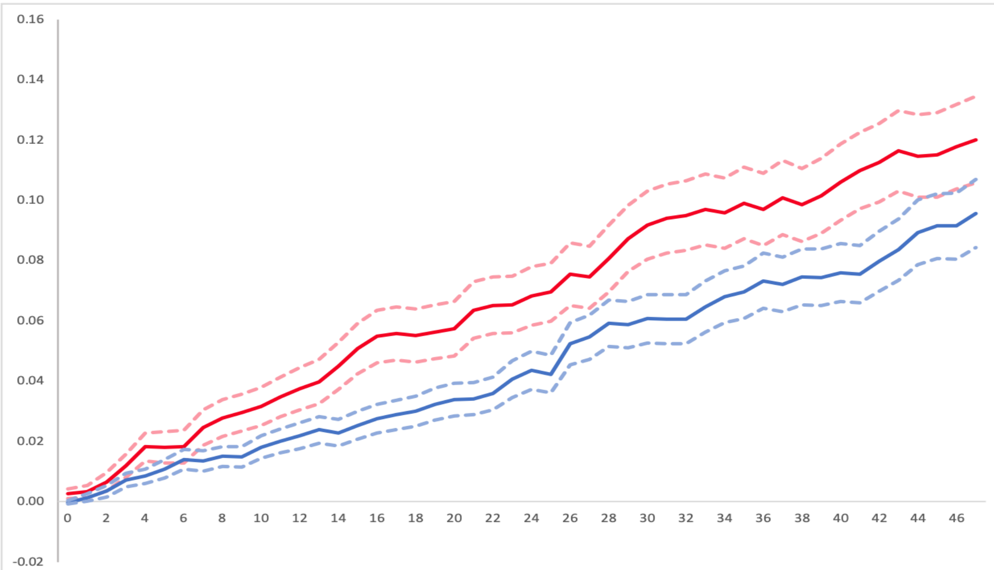
4.4.2 Migration

In addition to occupational change, migration represents an important margin of adjustment for laid-off workers. I find that 1 out of 7 workers who have undergone a displacement episode in the NLSY79 has changed their metropolitan area of residence (CBSA/CBSA) in the 4 years after layoff. This is a considerably higher share than in the general population — whose ratio is less than 1 in 20 in a 4-years period. Further, I find that workers who lost a locally skill-remote job are more likely to change their metro-area of residence after layoff than those who lost a locally skill-central job. Finally, migration leads to a decrease in workers' local skill remoteness. This drop is larger the larger was a worker's local skill remoteness at layoff. I interpret these results as evidence that migration is directed; workers recognize how the human capital accumulated in their previous job may be less skill-remote in local labor markets other than their current one, and migrate across cities accordingly.

There is a positive association between the local skill remoteness of the lost job and the propensity to migrate to a different metro-area after layoff. To show this, I construct an indicator variable that takes value 1 if the worker ever changes her metro-area of residence in the first 48 months after layoff. I then use a linear probability model to investigate the relationship between the propensity to migrate and local skill remoteness at layoff. The control set includes a rich set of fixed effects $\mathbf{I}_{it}$ for year, metro-area, industry, occupation, sex, race, age, education, marital status, and the local unemployment rate in the origin city. Errors are clustered at the metro area, year level. I find that losing a job above local median skill remoteness increases the probability of migrating to another metro area by about 40% with respect to the unconditional mean for all laid-off workers (+0.021, over a mean of 0.047).[33]

[33] This effect is quite sizable and equal to a bit less than half of the change associated with being married (-0.049) and about 6% of the change associated with a one percentage point increase in the local unemployment rate (-0.373). An alternative and slightly different empirical design regresses migration dummies at all dates after layoff on the interaction of time after layoff and a continuous measure of local skill remoteness at layoff. This returns similar results, with coefficients ranging

Figure 5: Higher skill remoteness at layoff is associated with a higher propensity to move across space after layoff. Source: NLSY79, laid-off workers sample, 1994-2014.



If post-layoff mobility over space is directed, we expect that workers on average move to cities where the skills accumulated in their previous occupation are less remote than in the current metro area. To verify this hypothesis, I compute the change in local skill remoteness that results from a move across two different metropolitan areas. Specifically, I first consider the local skill remoteness of the lost job in the city where the job was originally located. Then, I investigate the local skill remoteness of the lost job after migration — that is, in the new city.[34] I finally correlate the percentage change in the lost job's skill remoteness across the two locations with the level of skill remoteness at layoff.

On average, workers who migrate after layoff tend to reduce their occupation's skill remoteness: the average drop is equal to 13.7% for the sample of laid-off migrants. Furthermore, the post-migration decreases in local skill remoteness are larger in absolute value the higher that the value of local skill remoteness was at layoff. In other words, workers who lose a job more skill-remote than the median tend to reduce their level of local skill remoteness after migration by a *larger* percentage than those who lost a job less skill-remote than the median. To show this, I regress

from 0.012 to 0.035. Details are in Appendix Table C.19.

[34]Notice that I keep the job's occupation fixed (at $t = 0$, the month of displacement), so this difference can be computed regardless of whether the worker is re-employed in the new city.

the percentage change in the lost job's skill remoteness across the two locations on a dummy that takes value 1 if the lost job was more skill remote than the median at the time and location of layoff ($\texttt{above}_{it}$). The control set includes fixed effects $\mathbf{I}_{it}$ for year, metro-area, industry, occupation, sex, race, age, education, marital status, and the local unemployment rate in the origin city. Errors are clustered at the metro area, year level. I find that skill-remote workers who migrate to a different metropolitan area tend to lower their old job's local skill remoteness by about 35% (with an estimated standard error of 2.4%).

Of those workers who change city, approximately 1 in 2 also change occupation. For these, the total change in remoteness is the sum of two components. First, the change in local skill remoteness of the old occupation between the old and new city, the "spatial component". Second, the change in local skill remoteness between the old and the new occupation in the new city, the "occupational component". Then, the overall change in local skill remoteness can be written as follows, where "prime" denotes post-migration variables:

$$\begin{aligned} &\mathcal{R}_{oc} - \mathcal{R}_{o'c'} = \\ &= (\mathcal{R}_{oc} - \mathcal{R}_{oc'}) - (\mathcal{R}_{o'c'} - \mathcal{R}_{oc'}) \\ &= \textit{spatial} \;+\; \textit{occupational} \end{aligned}$$

On average, the spatial component $(\mathcal{R}_{oc} - \mathcal{R}_{oc'})$ accounts for 40% of the overall decline.[35]

The observed declines are significantly larger than what would be implied by a mechanical "regression to the mean" effect. I compare the changes in the data with the counterfactual changes that would occur if, instead of actual migration patterns, I considered fictitious migration propensities based only on the distribution of population across metro areas ("random" migration). The linear projection under random migration has an estimated slope of -0.010 (a drop of 1%), with a standard error of 0.003 (0.3%). Clearly, the slope under the random migration scenario is an order of magnitude smaller than in the data, therefore I conclude that the negative

[35] This, however, masks a much more varied distribution, with multiple relative peaks. Approximately 15 percent of migrants experiences a contribution from the spatial component of either 20% or 60%, and up to 12 percent of either 5% or 95% (for a full distribution, see figure C.1 in Appendix C.4).

relationship between skill remoteness at layoff and post-migration changes is not accounted for by regression to the mean.[36]

5 Robustness

5.1 Displaced workers in the CPS-DWS

An additional data source to study post-layoff labor market outcomes is the Current Population Survey's Displaced Workers Survey (CPS-DWS). A yearly supplement to the basic monthly CPS survey, the CPS-DWS identifies workers who were separated from their jobs because of layoff in the previous 36 months and asks them several questions about the lost job and, if they have one, the current job. I used information on pre-layoff occupation and city of residence, together with earnings before and after layoff, to estimate the effect of losing a locally skill-remote job on earnings after layoff. The CPS-DWS has the disadvantage of not being a "true" panel since it records pre- and post-layoff earnings at fixed intervals (at 12, 24, or 36 months, to be precise) in a single interview, rather than monthly as the NLSY79 does. Nevertheless, it has a large sample size and can provide an additional piece of evidence on the role of local skill remoteness in the consequences of layoff.

My empirical specification for the CPS-DWS is similar to equation (4), with the control set including fixed effects for state, industry, and occupation at the time of layoff, demographics such as sex, marital status, race, Hispanic ethnicity, and education (4 categories), in addition to an indicator for being displaced during a recession, and the log-population of the city of residence at the time of layoff. Standard errors are clustered at level of the city of residence at the time of layoff ($c(i)t_0$). The outcome variable is the percentage change in earnings at 12, 24, or 36 months after layoff, in terms of pre-layoff earnings. The explanatory variable of interest is

[36] To understand better the comparison between actual and counterfactual migration patterns, here I detail how I construct the fictitious random migration scenario in three steps. First, I compute the probability of moving to city c as the share of overall population that lives in c. This assumption captures the stylized fact that larger cities attract more migrants on average. Then, I calculate the distribution of post-migration skill remoteness as a weighted average of local skill remoteness across all metro areas, where the weights are equal to the fictitious propensities to migrate computed in the previous step. Finally, I compare the implied changes in remoteness under random migration with the changes in the data. Under random migration, we see a much less significant negative relationship between local skill remoteness at layoff and post-migration changes than under actual migration patterns. I interpret this result as evidence that migration is directed and that skill remoteness at layoff is an important determinant of migration patterns for laid-off workers.

the interaction between an indicator for being at 12, 24, or 36 months after layoff ($\mathbf{I}_{\{t=t_0+m\}}$) and an indicator for losing a job more locally skill-remote than the median job at the time of layoff ($\texttt{above}_{it_0}$). This specification, modeled after (Huckfeldt, 2021), is illustrated in equation (9). The resulting coefficients are reported in table 6.

$$\begin{aligned} \Delta\ earnings_{i,t-t_0} &= \mu + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48}\ \gamma_m \mathbf{I}_{\{t=t_0+m\}} \\ &\quad + \textstyle\sum_{m\in\{12,24,36\}}\ \delta_m \mathbf{I}_{\{t=t_0+m\}}\texttt{above}_{it_0} + \epsilon_{c(i)t_0} \end{aligned} \tag{9}$$

My findings are consistent with a negative association between local skill remoteness at layoff and post-layoff earnings. In particular, displaced workers whose last job was above median local skill remoteness earn between 6.9% and 8.6% less, in terms of pre-layoff earnings, than comparable workers whose last job was below median skill remoteness. The estimated losses from the NLSY analysis are comparable in magnitude, between 5 and 7% in this time horizon (see figure C.2 and tables C.9, C.10 in appendix C.5). Therefore, I see the results in table 6 as confirming the fact that losing a locally skill-remote job leads to persistently lower earnings after layoff.

Table 6: Displaced workers whose last job was above median local skill remoteness earn less after layoff than comparable workers whose last job was below median skill remoteness. Estimated losses as a percentage of pre-layoff earnings. Column (1)-(3): CPS Displaced Workers Survey 1996-2017, N=13,626. Last column: NLSY79 laid-off workers sample, 1994-2014, N=2,009.

months after layoff	skill-central	skill-remote	difference DWS	difference NLSY
12	-0.324	-0.410	0.086	0.051
	(0.062)	(0.018)	(0.065)	(0.030)
24	-0.156	-0.227	0.071	0.076
	(0.022)	(0.024)	(0.033)	(0.035)
36	-0.031	-0.100	0.069	0.074
	(0.024)	(0.029)	(0.038)	(0.037)

5.2 Selection on unobservables

There is a concern that skill-remote and skill-central workers differ in unobservable dimensions that are correlated with wage and mobility outcomes. If this were true, the results of my regressions would reflect the effect of such unobservables, and not local skill remoteness. To fix ideas, it is helpful to think of this unobservable characteristic as "ability".

I investigate this issue in three ways. I include in my baseline regression specifications individual fixed effects and an extensive set of controls for observable characteristics that could lead to selection. For example, I use fixed effects for city, industry, occupation, and the time and month of re-employment. I also control for several characteristics of displaced workers' cities and occupations that have been shown in the literature to lead to sorting over space (namely, city size — allowed to vary by occupational groups —, the share of college-educated people, the share of workers in the same occupational groups). Any unobservable dimension of relevance must be orthogonal to these individual and aggregate factors and still be able to drive selection.

Secondly, while a simple unobserved ability selection story may fit the earnings results in figure 3, the intuition for why lower ability workers might migrate and change occupation more often is less clear-cut. After all, for intrinsically less productive workers, the benefit of migration or occupational change is likely to be limited. If the skill remoteness of the lost job is a proxy for worker's quality, shouldn't we find, then, that skill-remote workers migrate and change occupation *less* often than skill central ones? Therefore, not only the suspected unobservable variable has to be orthogonal to all the observable factors included in the regression, it also has to be able to convincingly explain the wage, employment, and mobility patterns *jointly* over time. I find that such a factor is hard to identify with confidence.

Finally, I explicitly look for evidence in favor of omitted variable bias in the NLSY79 and find none. Specifically, I correlate the residuals from a Mincerian regression of wage levels and year-over-year wage growth with the value of local skill remoteness for each worker. There is no correlation between either of the two residualized wage measures and the local skill remoteness of the lost job — as one could subsume from the absence of a relationship between earnings and local skill remoteness *before* layoff (see figure 3). Even though one cannot exclude it *ex ante*, therefore, the data does not support the presence of a significant omitted variable problem that biases the association between skill remoteness at layoff and post-layoff wages and reallocation patterns.

6 Concluding remarks

By all accounts, job loss is a traumatic experience that has profound repercussions on the involved worker's ability to find meaningful employment, her earning capacity, and even her health and life satisfaction. The large, negative, and persistent consequences of layoff represent a substantial puzzle in our understanding of the labor market, and a significant challenge to policy makers tasked with improving laid-off workers' well-being. There is ample evidence of both pecuniary and non-pecuniary losses: Davis and von Wachter (2011) show that displaced workers lose between 1.4 and 2.8 years of pre-displacement earnings over a 20 years horizon. Sullivan and von Wachter (2009) also point out that "job displacement leads to a 15-20% increase in death rates during the following 20 years". All of these effects are more pronounced for workers laid-off in recessions (Davis and von Wachter, 2011). Much literature investigates why. Several convincing explanations point to job-level factors, such as occupational change, employer-specific human capital, or match quality (Huckfeldt, 2021; Jarosch, 2021; Lachowska et al., 2020). This paper offers another potential, complementary factor: the local skill remoteness of a worker's last job.

Local skill remoteness measures the degree of dissimilarity between the skill profiles of a worker's pre-displacement job and all other jobs in a local labor market. Therefore, the measure incorporates two dimensions of heterogeneity that plausibly affect worker reallocation after displacement: (1) differences in the skill content of jobs that make workers with different occupational backgrounds imperfect substitutes; and (2) heterogeneity in the availability of suitable jobs across different geographical locations, as the literature on spatial specialization patterns and costly geographic mobility maintains.

The skill remoteness measure I propose emphasizes that skill disparities between workers and local jobs are an important source of frictions to worker reallocation and, as such, affect labor market outcomes for laid-off workers along multiple dimensions. However, differences in skill profiles are not the only factors that affect worker reallocation across occupations and locations. Recent literature suggests that occupational licensing is common in the U.S., raising concerns about its effect on employment, especially for low-skilled workers (Mulholland and Young, 2016). Since licensing requirements vary across states, a combined study of licensing regulations and local skill structure differences is a promising avenue for future research.

My measure of local skill remoteness proves empirically successful in accounting for the severity and cyclicality of earnings losses for displaced workers. In particular, I provide evidence that workers who are displaced from a locally skill-remote job have persistently lower earnings and a lower probability to be re-employed at jobs with similar skill profiles, with respect to comparable workers who lost a locally skill-central job. The difference is large, about $10,000 over 4 years or 5 months of the median worker's pre-layoff earnings. I also show that jobs destroyed in recessions are 30% more likely to be locally skill-remote than those destroyed in booms. I conclude that, on average, it is precisely the jobs destroyed in downturns — i.e, the locally skill-remote ones — that lead to the most severe earnings losses for the workers who used to be employed in them.

Emphasizing the role of jobs' local skill remoteness represents a step forward in our understanding of job loss in at least two ways. First, it provides explicit evidence of the role played by the local jobs' skill mix. Since both a worker's skills and her location are concrete characteristics amenable to policy action, I believe this is progress. Skills can be practiced and taught; with appropriate training, a worker's skill portfolio can reach beyond whatever her previous job entailed — presumably improving her earnings potential. Furthermore, incentives to relocation can be evaluated further in the light of how a job's *local* skill remoteness affects workers tied-in into a specific location by other frictions (for example, financial ones).

Second, documenting the importance of the last job's local skill remoteness also redirects the spotlight on the characteristics of jobs, rather than workers. This is helpful because it can help identify at-risk individuals, before such risk materializes. As shown by previous work, structural transformation disproportionately occurs during recessions (Autor et al., 2003; Jaimovich and Siu, 2020). That jobs destroyed in recession are disproportionately skill-remote can be interpreted as another manifestation of this phenomenon. In the data, jobs are not destroyed at random: it is precisely those whose skill profile is locally "remote" that are at higher risk of dissolution. This, in turn, is associated with more negative earnings consequences for workers. Crucially, locally skill-remote jobs — and the workers employed in them — can be identified as vulnerable by observing the skills they entail and those utilized by other jobs in the same location. While it is beyond the scope of this paper to test specific policies predicated on the local skill remoteness of jobs, the evidence I provide is potentially useful to outline effective and targeted labor market policies, especially by local decision-makers.

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Appendix

A Data

A.1 Occupational Skills in O*NET

The Occupational Network (O*NET) dataset is a detailed data source that describes occupations in the United States from a varied set of different dimensions, including their skill content. The information in O*NET is collected through individual-level questionnaires that are addressed to both job incumbents and occupational experts. Information for O*NET is collected using a two-stage design: first a statistically random sample of businesses expected to employ workers in the targeted occupations is identified and, second, a random sample of workers in those occupations within those businesses is selected. For occupations where it would be difficult to sample workers, such as those that have a small number of workers or ones in which employees work in remote locations, occupation experts are identified from professional and/or trade association membership lists. In addition to the questionnaires completed by workers and occupation experts, additional ratings are provided by occupation analysts. Responses from all three sources — workers, occupation experts, and occupation analysts — are used to provide information for each occupation.

O*NET classifies occupations according to its own taxonomy: the O*NET-SOC occupational classification system. This is based on the Standard Occupational Classification (SOC) and periodically revised to keep up with an ever-changing labor market. The last revision of the O*NET-SOC taxonomy was in 2019. Regardless of whether the taxonomy is being updated, a new version of the O*NET occupational descriptors is released yearly, so to reflect changes in job requirements and characteristics. As the updating procedure does not follow a systematic schedule, the literature has been divided on whether it is appropriate to take advantage of the time variation in O*NET or not. In my main results, I remain agnostic and use the data at face value, judging that the U.S. DOL is trustworthy in both measuring and updating the content of occupations. Using the 2000 O*NET value only leaves my results unchanged.

Although there are between 720 and 955 occupations each year in the O*NET, I collapse them to 22 2-digits SOC groups to reduce measurement error when I match the O*NET with household surveys. This prevents the occurrence of occupation-city cells that are very sparsely populated, preserves an informative signal-to-noise

Table A.1: Major occupational groups (SOC 2-digits classification).

11	Management	31	Healthcare Support
13	Business and Financial	33	Protective Service
15	Computer and Mathematical	35	Food Preparation and Serving
17	Architecture and Engineering	37	Cleaning and Maintenance
19	Life, Physical, and Social Sciences	39	Personal Services
21	Social Services	41	Sales
23	Legal	43	Office and Administrative
25	Education	45	Farming and Fishing
27	Arts, Sports, and Media	47	Construction
29	Healthcare Practitioners and Technicians	49	Installation and Repair
51	Production	53	Transportation and material moving

ratio in the data, and keeps the dimensions of heterogeneity tractable (for a similar strategy, see occupational groups in Davis and Dingel, 2020).

Since the O*NET data is released typically twelve to sixteen months after the information is collected (the 2015 survey release pertains to 2014, the 2014 one to 2013 and so on), I match the t OES shares with the $t+1$ O*NET release. I also match O*NET data from the 2002 release (pertaining to 2001) to the 2000 OES survey, under the assumption that skill profiles for 2-digits occupations did not change substantially in that year. I do not use the 2001 release of O*NET because it is not constructed with criteria consistent with following years — in particular, O*NET surveys before 2002 were not employee-level but only addressed to "occupational experts". This matching procedure turns out to be inconsequential for the final results.

In my baseline analysis, I attribute to jobs between 1994 and 2000 the skill profile of their occupation as measured by O*NET in 2000. I exclude Teaching occupations (SOC2=25) and Agricultural Occupations (SOC2=45) because of the peculiar wage and wage growth distributions for workers employed in these occupations. These choices are immaterial with respect to my final results because of the very small number of teachers and agricultural workers who undergo layoff episodes.

A.2 Complete List of Skills from O*NET

- Active Learning
- Active Listening
- Complex Problem Solving
- Coordination
- Critical Thinking
- Equip. Maintenance
- Equip. Selection
- Installation
- Instructing
- Judgment/Decision Making
- Learning Strategies
- Financial Management
- Materials Management
- Personnel Management
- Mathematics
- Monitoring
- Negotiation
- Operation Monitoring
- Operation and Control
- Operations Analysis
- Persuasion
- Programming
- Quality Control Analysis
- Reading Comprehension
- Repairing
- Science
- Service Orientation
- Social Perceptiveness
- Speaking
- Systems Analysis
- Systems Evaluation
- Technology Design
- Time Management
- Troubleshooting
- Writing

Figure A.1: The O*NET Skills questionnaire

Instructions for Making Skills Ratings

These questions are about work-related skills. A **skill** is the ability to perform a task well. It is usually developed over time through training or experience. A skill can be used to do work in many jobs or it can be used in learning. You will be asked about a series of different skills and how they relate to *your current job*—that is, the job you hold now.

Each skill in this questionnaire is named and defined.

For example:

Writing	**Communicating effectively in writing as appropriate for the needs of the audience.**

You are then asked two questions about each skill:

A ***How important is the skill to the performance of your current job?***

For example:

How important is WRITING to the performance of *your current job*?

Not Important*	Somewhat Important	Important	Very Important	Extremely Important
1	2	3	4	5

Mark your answer by putting an **X** through the number that represents your answer.
Do not mark on the line between the numbers.

***If you rate the skill as Not Important to the performance of your job, mark the one [1] then skip over question B and proceed to the next skill.**

B ***What level of the skill is needed to perform your current job?***

To help you understand what we mean by **level**, we provide you with examples of job-related activities at different levels. For example:

What level of WRITING skill is needed to perform *your current job*?

	Write down a guest's order at a restaurant ↓		Write an email to staff outlining new directives ↓		Write a novel for publication ↓	
1	2	3	4	5	6	7
						Highest Level

Mark your answer by putting an **X** through the number that represents your answer.
Do not mark on the line between the numbers.

1 O*NET Skills Questionnaire

A.3 Earnings and wages in the NLSY79

In my baseline analysis, I drop all observations that pertain to teachers (SOC 25) because their wage is usually set through non-market mechanisms, for example by collective bargaining. I also drop agricultural occupations (SOC 45) because of their peculiarly seasonal wage volatility. The omission of agricultural occupations is inconsequential, given the small shares of employment these jobs represent. Results are robust to the inclusion of SOC 25 occupations as well.

I impute wages as follows, when missing: (i) where the hourly wage is missing but the annual wage is provided, I impute the hourly wage by dividing the annual wage by the number of paid weeks reported and then by the number of weekly hours worked. (ii) If either weeks or hours are missing but the worker responds negatively to the question about part-time status, I presume that the worker works 40 hours per week and 52 weeks a year. (iii) If the worker works part-time and no hourly wage is reported, the observation is dropped. This affects less than 0.1% of the sample, and the results are robust to my imputation procedure.

Finally, in all earnings and wage regressions, both the first and last percentile of the wage distribution are omitted from the analysis for robustness. Including them only reinforces my results but may give excessive importance to outliers. I also replicated the results while omitting the top and bottom 5% of the wage distribution and using log wages. Results are unchanged.

B Local skill remoteness

B.1 Vacancy and employment shares

While plausible, the choice of employment weights for local skill remoteness is not obvious and primarily reflects data constraints. Indeed, an attractive alternative is the share of *vacant* jobs by occupation and metropolitan area. However, a long and spatially comprehensive time series for occupation-city level vacancies is not available for the U.S.. Data on job openings is often limited to online sources only, has a limited level of detail, or is available only since the late 2000s.

Data on local job ads is available for the U.S. in two datasets: the Help Wanted Online (HWOL) and the Burning Glass (BG) data. In particular, the HWOL reports disaggregated vacancy counts only for the largest 45 metropolitan areas in the U.S. since 2005. BG data, instead, provides information on detailed occupations in all 382 metropolitan areas since 2010, but the level of vacancy posting is underestimated for low-skill occupations and the data is only available since 2010 (Hershbein and Kahn, 2018). On the other hand, employment shares are readily available from comprehensive data sources at a rather granular level (occupation-by-metro area) since the 1990s. Therefore, I choose to use employment shares in my main specification, as such data is available for all metropolitan areas at the chosen level of occupational detail since the early 1990s. For the available cities, occupations, and years, I find that the discrepancy between employment and job postings shares is not substantial, with an average linear correlation of over 0.7 in all years. Though I recognize that this empirical strategy is far from unrestrictive, I take comfort in the fact that it is ultimately empirically successful: the employment-weighted local skill remoteness of a worker's last job predicts a number of post-layoff outcomes of interest and reveals interesting heterogeneity in the consequences of layoff.

It is reassuring that local occupational employment shares and local occupational vacancy shares are strongly correlated in the cross-section and over time. Employment shares in the OES — the weights of choice in my baseline formulation of local skill remoteness — are strongly and positively correlated with vacancy shares in both HWOL and BG. For the cities, occupations, and years available in HWOL, the cross-sectional correlation between the vacancy- and employment-weighted skill remoteness measures is between 0.62 (2010) and 0.75 (2016), with an average of 0.74 over the whole period (2005-2016). The correlation using BG job postings data for 2010-2016 is also high, and equal to 0.75.

C Additional empirical results

C.1 Descriptive analysis: skill distances

I use skill scores from O*NET to construct pairwise skill distances between occupations. In my main specification, I use the Manhattan distance and denote the skill-distance between occupation i and occupation j by d_{ij} as in (1). Table C.1 shows the closest and farthest occupations for selected occupational groups, according to the skill distance from O*NET. To compute skill distances I use all the 35 skill dimensions available in the data, without imposing weights nor aggregation. This approach reflects the empirical evidence that occupations combine several, often diverse, types of skills at varying intensity. Broad skill groups such as cognitive and manual skills are not necessarily mutually exclusive: indeed, as Fernandez-Macias, Hurley, and Storrie (2012) document for European countries, I also find that many jobs display a balanced combination of cognitive, manual, and interpersonal skills.

Consider, for example, the jobs in table C.2. Anesthesiologists and economists are often thought of as prevalently cognitive occupations, and so they are. Yet skills related to social interaction (such as listening, speaking, or monitoring others) play an important role in the performance of these occupations. Machinists and masons tend to be classified as manual occupations, yet among the five top skills these workers use we find coordination, listening, and critical thinking. Similarly, occupations that are intensive in social skills (baristas or nurses, for instance) also use cognitive ones, such as coordination, monitoring, and critical thinking. In general, though some jobs are more pronouncedly cognitive (anesthesiologists) and other predominantly manual (masons), almost all occupations combine various skills in a non-trivial way and at different levels of complexity to produce services and goods (see table C.3). Therefore, I exploit the full richness of skill dimensions in O*NET and let the data speak to each skill dimension's relevance.

Table C.1: Skill-distances across selected occupational groups. Source: O*NET 2017.

Occupation	Closest	Farthest
Economist	Statisticians	Construction workers
Physicians and Pharmacists	Biologists	Cooks and Waiters
Machine Operators	Laborers and Material Movers	Chief executives
Masons	Production and Operating workers	Chief executives
Baristas and Bartenders	Housekeepers and Janitors	Architects and Engineers

Table C.2: Top 5 skills for different occupations: occupations combine several, often diverse, types of skills. Source: O*NET 2017.

Occupation	Skill	Occupation	Skill
Anesthesiologist	Critical Thinking	**Economist**	Active Listening
	Active Listening		Critical Thinking
	Decision Making		Mathematics
	Monitoring		Speaking
	Problem Solving		Writing
Machinist	Operation Monitoring	**Cement Mason**	Monitoring
	Critical Thinking		Active Listening
	Operation and Control		Coordination
	Active Listening		Critical Thinking
	Coordination		Decision Making
Barista	Active Listening	**Critical Care Nurse**	Active Listening
	Service Orientation		Service Orientation
	Social Perceptiveness		Critical Thinking
	Speaking		Monitoring
	Coordination		Reading

Table C.3: Level and importance of listening skills across occupations. Source: O*NET 2017.

Skill	Occupation	Level†	Importance†
Active listening	Anesthesiologist	78	61
	Economist	75	59
	Machinist	50	41
	Mason	53	34
	Barista	63	37
	Nurse	78	57

† min=0, max=100

C.2 Skills distances and occupational flows

Skill distances are highly predictive of cross-occupational flows. I interpret this evidence as confirming the validity of measuring workers' skill remoteness starting from distances between jobs in the skill space. Specifically, I find that the distance between the two occupations d_{ij} is inversely related to the probability of ij worker flows, with an elasticity very close to -1. In other words, the larger the skill distance between two occupations, the smaller the probability a worker transitions to be employed from one to the other.

To show the negative relationship between skill distances and cross-occupational flows, I construct two different measures of occupational change rates using longitudinally-linked CPS micro-data 1994-2016. The first measure considers all workers that were employed in occupation i at month t and employed in occupation $j \neq i$ at month $t+1$. The rate is computed as a percentage of all workers employed in i at t. I denote these monthly flow rates by f_{ij}. As an alternative measure, I compute generalized flow rates that consider all workers employed in occupation i at month t and employed in occupation $j \neq i$ at any of the months between $t+1$ and $t+3$. Again, the rate is computed as a percentage of all workers employed in i at t. I denote these generalized flow rates by $\tilde{f}_{ij}$. Notice that f_{ij} excludes workers who experience an occupational change episode through a spell of non-employment, while $\tilde{f}_{ij}$ does not.

To investigate the relationship between skill distances and flows, I run a series of regressions of log occupational flows (either monthly or generalized) on log skill distances. The estimated elasticities are in table C.4. In columns 1 and 3, I test the

Table C.4: Skill distances are highly predictive of cross-occupational flows: a 1% increase in the distance between occupations is associated with a 1% decrease in the worker flow between them. Standard errors clustered by outgoing occupation i. Source: O*NET and CPS 1994-2017.

	(1)	(2)	(3)	(4)
	ln(flow)	ln(flow)	ln(generalized flow)	ln(generalized flow)
ln(distance)	-1.068**	-1.086**	-1.053**	-1.160**
	(0.0968)	(0.0637)	(0.102)	(0.0761)
i FE	No	Yes	No	Yes
j FE	No	Yes	No	Yes
R-squared	0.227	0.802	0.204	0.799

Significance levels : $\dagger = 10\%$ $* = 5\%$ $** = 1\%$

relationship between skill distances and flow rates (monthly and generalized) and find that it is negative, with an elasticity tightly estimated around -1 in both cases, and a contribution of skill distances to the variance of flows rates of at least 20%. In columns 2 and 4, I add fixed effects for the outgoing and receiving occupations to the baseline specification: I find that the estimated elasticity of cross-occupational flows to skill distances is again estimated tightly around -1. I conclude that a 1% increase in the distance between two occupations is associated with a 1% decrease in the worker flow between them. This result is robust across several empirical specifications, thus emphasizing how, even when I allow for differential means for the outgoing and receiving occupations, skill distances between jobs are a major determinant of cross-occupational worker mobility.

C.3 Unemployment duration

Table C.5: Distribution of unemployment duration, in weeks, by the local skill-remoteness of the lost job. Source: CPS-DWS 1994-2017. Recession years are 2001, and 2007-10.

Percentile	All years		Recessions	
	Skill-central	Skill-remote	Skill-central	Skill-remote
1%	1	1	1	1
5%	1	1	1	1
10%	2	2	2	2
25%	4	4	5	5
Median	12	12	13	14
Mean	23.4	24.9	26.4	27.0
75%	30	34	39	39
90%	60	63	64	65
95%	104	104	91	104
99%	119	119	117	119

In the CPS, one can measure the separation rate of workers whose last job was either skill-remote or skill-central *at the monthly frequency.* This implies that short unemployment spells may go undetected if they start after the interview date at month m and end before the interview date at month $m+1$. One hypothesis could be that such short unemployment spells are much more likely for workers separating from skill-central jobs. Then, the rates of job destruction could actually be the same for skill-central and skill-remote jobs, but the job-finding rate faster for workers laid-off from skill-central ones. The CPS evidence on separation rates alone cannot distinguish between this scenario and one in which skill-remote jobs are, in fact, destroyed at a faster rate in recessions than skill-central ones.

While theoretically possible, in the data there is little evidence of differential incidence of short unemployment spells by skill remoteness at layoff. In the CPS, every unemployed person is asked the duration of the current unemployment spell — many of these spells last less than 4 weeks, in fact. The overall distribution of unemployment duration, however, does not differ in any appreciable way between laid-off workers whose last job was above or below the median local skill remoteness. This statement is especially true for short unemployment spells, as detailed in table C.5. In both groups, about 25% of laid-off workers have unemployment duration of at most 4 weeks, and the median is 12 weeks in both cases. At longer duration we do see some differences, but they are relatively small. In recessions, that is in years 2001, and 2007-09, the picture is similar with no difference at short unemployment spells and some longer durations for skill-remote workers who have long unemployment spells. This is consistent with the evidence in section 4.3, as you noted, that shows little difference in hours worked after layoff between workers who lost a skill-central or a skill-remote job.

C.4 Migration and occupational change

Of those workers that change city, approximately 1 in 2 also change occupation. For migrants who also change occupation after moving to a different metro-area, the total change in remoteness is the sum of two components. First, the change in local skill remoteness of the old occupation between the old and new city, the "spatial component". Second, the change in local skill remoteness between the old and the new occupation in the new city, the "occupational component". Then, the overall change in local skill remoteness can be written as follows, where "prime" denotes post-migration variables:

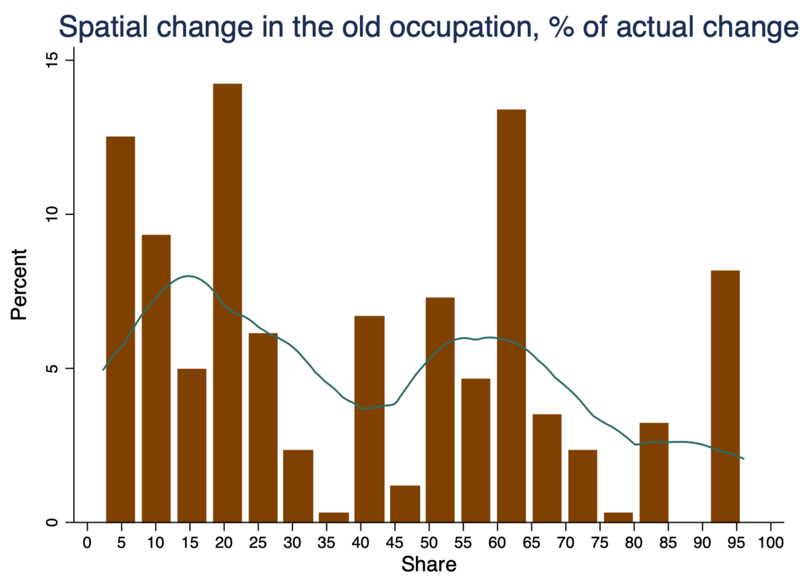
$$\begin{aligned} &\mathcal{R}_{oc} - \mathcal{R}_{o'c'} = \\ &= (\mathcal{R}_{oc} - \mathcal{R}_{oc'}) - (\mathcal{R}_{o'c'} - \mathcal{R}_{oc'}) \end{aligned}$$

On average, $(\mathcal{R}_{oc} - \mathcal{R}_{oc'})$ — the spatial change in the skill remoteness of the old occupation — accounts for 40% of the overall average decline (table C.6). This, however, masks a more varied distribution, with multiple peaks at 5, 20, and 60 percent (figure C.1).

Table C.6: Summary stats for the spatial change in the old occupation between the old and new city $\mathcal{R}_{oc} - \mathcal{R}_{oc'}$, expressed as a percentage of actual post-migration skill remoteness change $\mathcal{R}_{oc} - \mathcal{R}_{o'c'}$.

Percentiles	Value
1%	2.319
5%	2.330
10%	3.579
25%	17.166
Median	39.130
Mean	39.695
75%	62.595
90%	83.444
95%	91.647
99%	93.651

Figure C.1: For migrants who change occupation after moving to a different metro-area, the distribution of the spatial component $\mathcal{R}_{oc} - \mathcal{R}_{oc'}$ has multiple peaks.



C.5 NLSY79 regressions: details and additional figures

Table C.7 and table C.8 (regression equation (4) in main text):

$$earnings_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it}$$

Table C.9 and table C.10 (regression equation not reported in main text):

$$\frac{earnings_{it}}{earnings_{it_{-1}}} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it}$$

Table C.13 and table C.14 (regression equations (5) and (6) in main text):

$$hours_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it}$$

$$wage_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it}$$

Table C.15 and table C.16 (regression equation not reported in main text):

$$\begin{aligned} changed\_occ_{it} &= \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} \\ &\quad + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it} \end{aligned} \tag{10}$$

Table C.17 and table C.18 (regression equation (8) in main text):

$$earnings_{it} = \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{exceed}_{it_0} + \epsilon_{it}$$

Table C.19 and table C.20 (regression equation not reported in main text):

$$\begin{aligned} changed\_cbsa_{it} &= \alpha_i + \beta^{(1)}\mathbf{X}_{it} + \beta^{(2)}\mathbf{X}_{it_0} \\ &\quad + \textstyle\sum_{m=-12}^{48} \gamma_m \mathbf{I}_{\{t=t_0+m\}} + \sum_{m=-12}^{48} \delta_m \mathbf{I}_{\{t=t_0+m\}} \texttt{above}_{it_0} + \epsilon_{it} \end{aligned} \tag{11}$$

Figure C.2: Regression (4): skill remoteness at layoff is *negatively* associated with monthly earnings after layoff. Overall, in the course of 4 years after layoff, workers who were laid-off from a locally skill-remote job recoup 6% less per month of their previous earnings than workers who were laid-off from a locally skill-central job. Source: NLSY79, laid-off workers sample, 1994-2014.

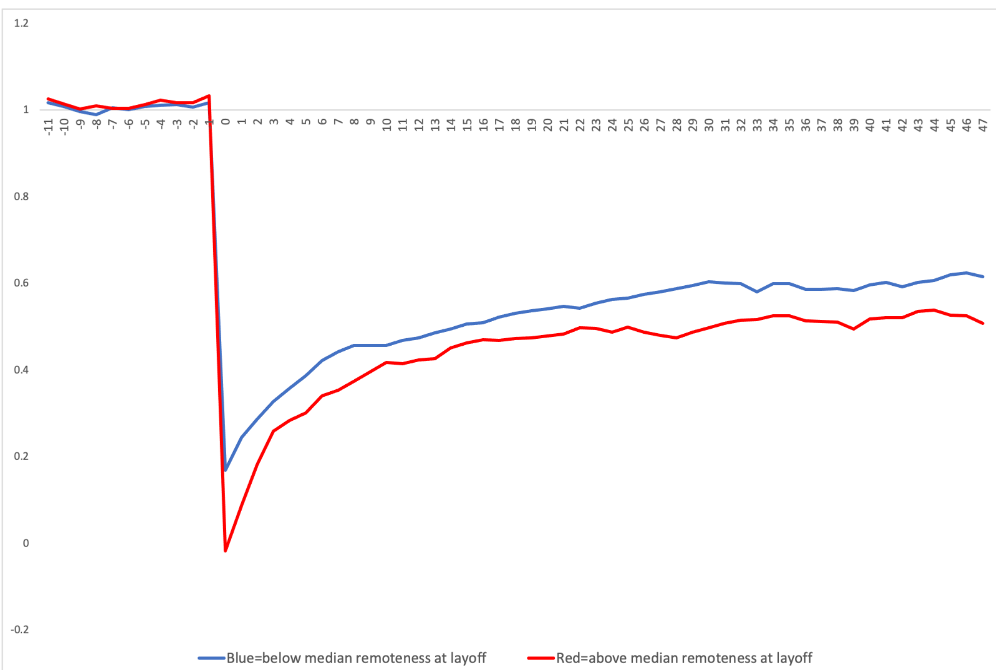


Table C.7: Detailed coefficients from earnings regression (4) (table 1/2).

m	γ_m	SE (γ_m)	δ_m	SE (δ_m)	earnings loss from δ_m (% of pre-layoff avg.)
-11	43.40	45.93	22.24	71.16	–
-10	23.05	44.38	13.23	69.39	–
-9	-6.99	44.33	12.59	67.52	–
-8	-25.13	43.07	49.73	65.26	–
-7	15.07	41.15	-4.48	63.26	–
-6	3.23	38.36	6.16	60.21	–
-5	23.21	35.36	8.60	55.80	–
-4	29.29	32.98	29.80	52.56	–
-3	34.18	28.37	11.35	47.80	–
-2	19.61	25.68	22.60	44.39	–
-1	42.18	23.27	41.71	39.02	–
0	-2,067.58	47.35	-491.18	78.34	23.76
1	-1,879.78	49.68	-416.53	80.53	22.16
2	-1,772.93	50.17	-285.51	81.66	16.10
3	-1,673.44	50.37	-189.34	81.83	11.31
4	-1,597.38	51.01	-203.57	81.95	12.74
5	-1,524.19	49.92	-233.78	81.54	15.34
6	-1,435.72	50.09	-223.99	81.74	15.60
7	-1,385.49	50.88	-241.60	81.30	17.44
8	-1,349.93	51.12	-224.60	81.28	16.64
9	-1,348.89	50.35	-171.28	80.01	12.70
10	-1,348.88	51.06	-116.29	80.14	8.62
11	-1,320.80	49.20	-151.33	76.93	11.46
12	-1,307.86	49.52	-142.76	76.25	10.92
13	-1,277.13	51.99	-165.54	78.57	12.96
14	-1,256.80	53.41	-124.42	79.58	9.90
15	-1,228.21	53.15	-122.91	78.20	10.01
16	-1,220.16	53.80	-113.49	80.38	9.30
17	-1,186.78	53.74	-149.67	78.35	12.61
18	-1,163.81	54.33	-161.70	79.83	13.89
19	-1,149.98	54.64	-172.07	78.57	14.96

Table C.8: Detailed coefficients from earnings regression (4) (table 2/2).

m	γ_m	SE (γ_m)	δ_m	SE (δ_m)	earnings loss from δ_m (% of pre-layoff avg.)
20	-1,139.35	56.21	-170.36	79.09	14.95
21	-1,124.53	58.86	-173.74	80.07	15.45
22	-1,135.39	59.28	-129.32	80.14	11.39
23	-1,107.67	60.62	-159.79	80.24	14.43
24	-1,086.58	60.77	-202.37	80.71	18.63
25	-1,077.92	61.24	-180.11	79.45	16.71
26	-1,056.90	62.72	-231.62	81.40	21.92
27	-1,042.63	64.50	-265.71	83.51	25.48
28	-1,024.27	66.49	-297.30	86.68	29.03
29	-1,006.89	66.00	-281.18	84.70	27.93
30	-986.03	65.75	-278.14	84.61	28.21
31	-993.30	66.79	-244.35	84.68	24.60
32	-995.13	67.98	-225.82	85.35	22.69
33	-1,042.92	69.38	-171.73	84.88	16.47
34	-993.78	69.00	-198.47	85.62	19.97
35	-997.11	69.50	-195.64	86.11	19.62
36	-1,026.38	71.31	-196.12	87.62	19.11
37	-1,026.14	72.03	-200.35	86.93	19.52
38	-1,024.31	73.06	-205.14	87.94	20.03
39	-1,036.55	72.62	-234.41	90.19	22.61
40	-1,004.24	72.96	-207.96	88.97	20.71
41	-989.75	73.41	-214.69	89.86	21.69
42	-1,014.75	73.72	-191.55	90.01	18.88
43	-988.11	74.65	-180.77	87.72	18.29
44	-976.64	75.83	-185.39	89.54	18.98
45	-944.71	78.08	-245.61	90.73	26.00
46	-934.89	78.98	-258.58	91.89	27.66
47	-954.46	80.02	-283.67	95.58	29.72
Cumulative earnings loss	-57,227.03		-10,111.49		17.67

Table C.9: Detailed coefficients from earnings regression (4), in terms of pre-layoff earnings (table 1/2).

m	γ_m	SE (γ_m)	δ_m	SE (δ_m)
-11	0.018	0.018	0.008	0.028
-10	0.009	0.018	0.005	0.028
-9	-0.003	0.018	0.005	0.027
-8	-0.010	0.017	0.019	0.026
-7	0.006	0.017	-0.002	0.025
-6	0.002	0.015	0.002	0.024
-5	0.010	0.014	0.003	0.022
-4	0.012	0.013	0.011	0.021
-3	0.014	0.011	0.004	0.019
-2	0.008	0.010	0.009	0.018
-1	0.017	0.009	0.016	0.016
0	-0.831	0.019	-0.186	0.031
1	-0.755	0.020	-0.157	0.032
2	-0.713	0.020	-0.106	0.033
3	-0.673	0.020	-0.068	0.033
4	-0.642	0.020	-0.074	0.033
5	-0.613	0.020	-0.086	0.033
6	-0.577	0.020	-0.083	0.033
7	-0.557	0.020	-0.090	0.032
8	-0.543	0.021	-0.084	0.032
9	-0.542	0.020	-0.062	0.032
10	-0.542	0.020	-0.041	0.032
11	-0.531	0.020	-0.055	0.031
12	-0.526	0.020	-0.051	0.030
13	-0.513	0.021	-0.061	0.031
14	-0.505	0.021	-0.044	0.032
15	-0.493	0.021	-0.044	0.031
16	-0.490	0.022	-0.040	0.032
17	-0.477	0.022	-0.055	0.031
18	-0.468	0.022	-0.060	0.032
19	-0.462	0.022	-0.064	0.031

Table C.10: Detailed coefficients from earnings regression (4), in terms of pre-layoff earnings (table 2/2).

m	γ_m	SE (γ_m)	δ_m	SE (δ_m)
20	-0.458	0.023	-0.063	0.032
21	-0.452	0.024	-0.065	0.032
22	-0.456	0.024	-0.047	0.032
23	-0.445	0.024	-0.059	0.032
24	-0.436	0.024	-0.076	0.032
25	-0.433	0.025	-0.068	0.032
26	-0.424	0.025	-0.088	0.032
27	-0.419	0.026	-0.102	0.033
28	-0.411	0.027	-0.115	0.035
29	-0.404	0.026	-0.108	0.034
30	-0.396	0.026	-0.107	0.034
31	-0.399	0.027	-0.094	0.034
32	-0.400	0.027	-0.086	0.034
33	-0.419	0.028	-0.065	0.034
34	-0.399	0.028	-0.076	0.034
35	-0.401	0.028	-0.074	0.034
36	-0.412	0.029	-0.074	0.035
37	-0.412	0.029	-0.076	0.035
38	-0.411	0.029	-0.078	0.035
39	-0.416	0.029	-0.090	0.036
40	-0.403	0.029	-0.079	0.036
41	-0.398	0.029	-0.082	0.036
42	-0.408	0.030	-0.073	0.036
43	-0.397	0.030	-0.068	0.035
44	-0.392	0.030	-0.070	0.036
45	-0.379	0.031	-0.094	0.036
46	-0.376	0.032	-0.100	0.037
47	-0.383	0.032	-0.109	0.038

Table C.11: Testing for non-linearities: columns (1), (2), (3) report coefficients from regression (4), but using indicators for the job exceeding the 75th, 90th, and 50th percentile, respectively. Negative effects associated with losing a job exceeding higher skill remoteness percentiles are generally larger than those associated with losing a job which is more skill-remote than the median.

m	**90th pct**	**75th pct**	**50th pct**
1	-984.25	-908.69	-491.18
	(182.36)	(117.04)	(78.34)
2	-784.35	-778.26	-416.53
	(195.34)	(121.24)	(80.53)
3	-554.36	-616.34	-285.51
	(191.56)	(120.73)	(81.66)
4	-556.19	-510.03	-189.34
	(191.95)	(120.20)	(81.83)
5	-394.05	-455.47	-203.57
	(193.98)	(122.55)	(81.95)
6	-360.79	-500.60	-233.78
	(188.47)	(121.41)	(81.54)
7	-288.21	-472.54	-223.99
	(194.76)	(234.87)	(81.74)
8	-322.78	-455.68	-241.60
	(195.40)	(123.66)	(81.30)
9	-372.76	-454.67	-224.60
	(198.95)	(123.67)	(81.28)
10	-341.38	-419.09	-171.28
	(202.94)	(124.78)	(80.01)
11	-336.30	-417.81	-116.29
	(198.77)	(126.03)	(80.14)
12	-350.71	-450.33	-151.33
	(206.62)	(121.45)	(76.93)
N_{pct}	201	566	1072
N	2009	2009	2009

Table C.12: Comparing the earnings losses associated with exceeding median local skill remoteness at layoff and one standard deviation decrease in the local share of the occupation at layoff. Regression specification includes both, own occupation employment share has unitary standard deviation.

Months from layoff	Skill remoteness	SE	Own occup. share	SE
0	-583.07	85.07	-200.67	40.87
1	-514.35	87.91	-214.72	43.11
2	-364.62	88.83	-170.77	42.70
3	-257.14	88.38	-149.68	43.10
4	-272.04	89.71	-148.38	44.70
5	-290.45	89.42	-128.41	43.69
6	-276.78	89.48	-117.26	44.39
7	-292.24	88.79	-108.99	44.59
8	-262.65	88.84	-82.95	45.27
9	-211.04	87.27	-84.39	44.59
10	-154.16	87.36	-81.79	44.81
11	-187.24	83.29	-77.86	43.41
12	-179.80	82.75	-79.50	43.27

Table C.13: Detailed coefficients from wage and hours worked regression (5) and (6) (table 1/2). Note that sample restrictions are such that only full-time, full-year workers are selected before layoff, hence coefficients on hours are missing for that period.

	Hourly wage (in \$)				Hours worked (per month)			
m	γ_m	SE (γ_m)	δ_m	SE (δ_m)	γ_m	SE (γ_m)	δ_m	SE (δ_m)
-11	-1.95	0.22	-0.02	0.36	–	–	–	–
-10	-1.79	0.22	-0.30	0.35	–	–	–	–
-9	-2.00	0.22	-0.06	0.34	–	–	–	–
-8	-1.89	0.22	0.01	0.32	–	–	–	–
-7	-1.42	0.21	-0.36	0.31	–	–	–	–
-6	-1.34	0.20	-0.33	0.30	–	–	–	–
-5	-1.18	0.19	-0.19	0.29	–	–	–	–
-4	-1.03	0.19	-0.07	0.28	–	–	–	–
-3	-0.68	0.15	-0.06	0.24	–	–	–	–
-2	-0.36	0.13	0.01	0.21	–	–	–	–
-1	-0.11	0.11	0.02	0.18	–	–	–	–
0	-11.11	0.25	-1.94	0.41	-127.57	1.73	-1.62	2.62
1	-10.08	0.25	-1.54	0.41	-116.88	1.91	-1.01	2.79
2	-9.49	0.25	-1.17	0.40	-110.76	2.00	0.95	2.90
3	-9.01	0.26	-0.62	0.41	-105.07	2.06	4.73	2.94
4	-8.53	0.26	-0.75	0.40	-100.32	2.10	3.23	2.93
5	-8.13	0.25	-0.92	0.40	-96.77	2.11	3.12	2.89
6	-7.71	0.25	-0.82	0.40	-92.63	2.16	1.31	2.92
7	-7.44	0.26	-0.86	0.40	-89.67	2.16	1.05	2.93
8	-7.16	0.26	-0.89	0.40	-87.00	2.16	2.00	2.95
9	-7.11	0.26	-0.63	0.39	-85.89	2.20	3.22	2.96
10	-7.01	0.26	-0.42	0.39	-85.02	2.26	5.57	2.94
11	-6.86	0.26	-0.56	0.38	-83.70	2.25	5.23	2.93
12	-6.73	0.26	-0.52	0.37	-83.19	2.28	5.24	2.90
13	-6.53	0.27	-0.60	0.37	-82.47	2.33	5.55	2.94
14	-6.38	0.27	-0.40	0.39	-80.96	2.35	6.10	2.93
15	-6.17	0.27	-0.56	0.38	-78.36	2.34	4.13	2.96
16	-6.04	0.28	-0.64	0.39	-77.48	2.36	3.38	2.96
17	-5.88	0.28	-0.71	0.39	-76.54	2.41	1.64	2.99
18	-5.79	0.28	-0.77	0.39	-75.32	2.45	0.50	3.05
19	-5.63	0.28	-0.86	0.39	-73.68	2.45	-0.63	3.03

Table C.14: Detailed coefficients from wage and hours worked regression (5) and (6) (table 2/2).

	Hourly wage (in \$)				Hours worked (per month)			
m	γ_m	SE (γ_m)	δ_m	SE (δ_m)	γ_m	SE (γ_m)	δ_m	SE (δ_m)
20	-5.48	0.29	-1.00	0.39	-73.88	2.51	-0.07	3.03
21	-5.39	0.30	-1.01	0.40	-74.61	2.58	1.90	3.07
22	-5.45	0.30	-0.71	0.40	-73.95	2.59	2.19	3.09
23	-5.34	0.30	-0.89	0.40	-72.84	2.64	1.58	3.09
24	-5.30	0.30	-1.00	0.40	-71.65	2.63	-0.06	3.08
25	-5.20	0.30	-0.95	0.39	-71.63	2.63	1.36	3.05
26	-5.13	0.30	-1.08	0.40	-70.57	2.63	0.01	3.09
27	-5.09	0.30	-1.22	0.41	-70.31	2.65	-0.63	3.13
28	-4.94	0.31	-1.41	0.42	-69.18	2.70	-2.06	3.19
29	-4.78	0.31	-1.35	0.41	-68.11	2.67	-1.36	3.10
30	-4.71	0.31	-1.34	0.41	-67.26	2.70	-1.91	3.13
31	-4.77	0.31	-1.13	0.41	-66.69	2.76	-1.48	3.14
32	-4.73	0.32	-1.11	0.42	-67.02	2.83	-0.75	3.18
33	-4.89	0.33	-0.92	0.42	-69.63	2.96	1.26	3.25
34	-4.69	0.32	-1.02	0.42	-67.09	2.94	0.24	3.26
35	-4.73	0.32	-0.99	0.42	-66.96	2.92	-0.33	3.30
36	-4.89	0.33	-0.88	0.43	-68.96	2.98	0.73	3.37
37	-4.85	0.33	-0.93	0.42	-69.79	3.06	2.59	3.37
38	-4.79	0.33	-1.03	0.42	-68.43	3.04	0.86	3.36
39	-4.87	0.34	-1.22	0.43	-68.30	3.04	-1.64	3.40
40	-4.67	0.34	-1.17	0.42	-67.64	3.04	0.02	3.40
41	-4.56	0.34	-1.21	0.42	-66.42	3.02	-0.83	3.37
42	-4.75	0.34	-0.97	0.43	-66.32	3.07	-0.76	3.39
43	-4.56	0.34	-1.04	0.42	-65.32	3.08	-0.45	3.36
44	-4.52	0.35	-0.95	0.43	-64.14	3.09	-0.38	3.37
45	-4.48	0.35	-1.08	0.42	-62.01	3.08	-3.03	3.35
46	-4.55	0.35	-0.89	0.43	-61.94	3.13	-3.21	3.42
47	-4.51	0.36	-1.16	0.44	-62.71	3.19	-2.85	3.49

Figure C.3: Regression for wage and hours worked (5) and (6). Wage losses account for most of the earnings losses associated with skill remoteness at layoff. Source: NLSY79, laid-off workers sample, 1994-2014.

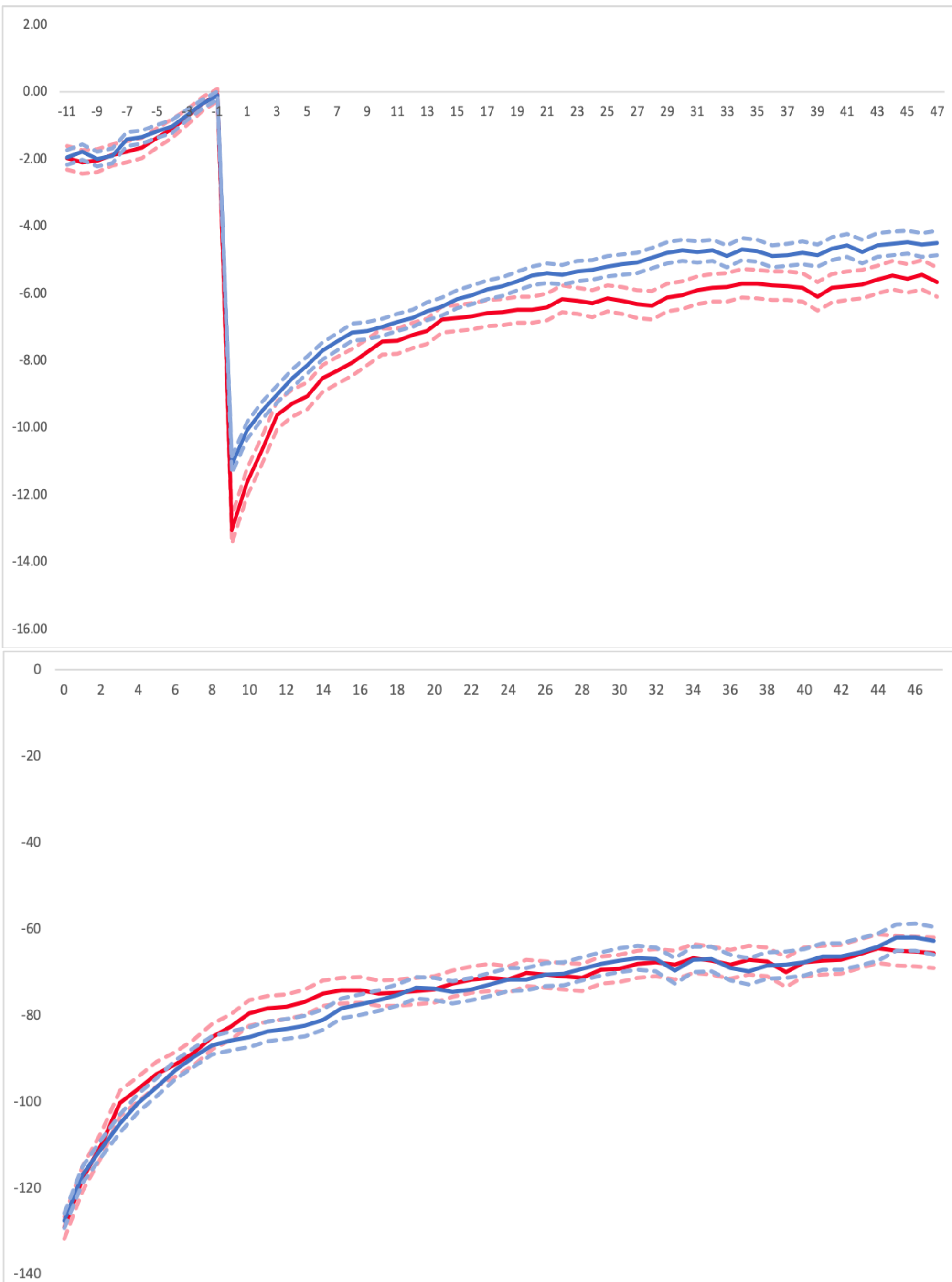


Table C.15: Detailed coefficients from occupational change regression (10) (table 1/2).

m	γ_m	SE (γ_m)	SE (remote)	SE (central)	% Change (remote over central)
0	0.00	0.00	0.00	0.00	—
1	0.14	0.12	0.01	0.01	16.19%
2	0.19	0.17	0.01	0.01	10.66%
3	0.22	0.20	0.02	0.01	13.25%
4	0.28	0.22	0.02	0.01	22.83%
5	0.29	0.26	0.02	0.01	15.41%
6	0.32	0.28	0.02	0.01	12.73%
7	0.34	0.30	0.02	0.01	14.27%
8	0.36	0.32	0.02	0.01	13.55%
9	0.38	0.33	0.02	0.01	15.55%
10	0.39	0.34	0.02	0.01	12.88%
11	0.41	0.35	0.02	0.01	14.98%
12	0.42	0.36	0.02	0.01	17.68%
13	0.43	0.37	0.02	0.01	16.09%
14	0.44	0.38	0.02	0.01	16.53%
15	0.44	0.39	0.02	0.01	12.53%
16	0.45	0.41	0.02	0.01	10.76%
17	0.46	0.42	0.02	0.02	10.32%
18	0.47	0.43	0.02	0.02	9.99%
19	0.48	0.44	0.02	0.02	10.07%

Table C.16: Detailed coefficients from occupational change regression (10) (table 2/2).

m	γ_m	SE (γ_m)	SE (remote)	SE (central)	% Change (remote over central)
20	0.48	0.44	0.02	0.02	8.92%
21	0.48	0.45	0.02	0.02	7.85%
22	0.49	0.45	0.02	0.02	8.62%
23	0.50	0.45	0.02	0.02	10.25%
24	0.50	0.46	0.02	0.02	8.96%
25	0.51	0.46	0.02	0.02	10.15%
26	0.52	0.47	0.02	0.02	11.45%
27	0.53	0.47	0.02	0.02	11.74%
28	0.54	0.49	0.02	0.02	9.52%
29	0.54	0.49	0.02	0.02	10.26%
30	0.54	0.50	0.02	0.02	8.48%
31	0.55	0.50	0.02	0.02	9.12%
32	0.55	0.50	0.02	0.02	10.36%
33	0.56	0.50	0.02	0.02	10.52%
34	0.57	0.51	0.02	0.02	11.45%
35	0.57	0.52	0.02	0.02	10.74%
36	0.58	0.53	0.02	0.02	9.17%
37	0.58	0.53	0.02	0.02	8.21%
38	0.59	0.54	0.02	0.02	8.03%
39	0.59	0.55	0.02	0.02	8.29%
40	0.60	0.54	0.02	0.02	10.34%
41	0.60	0.55	0.02	0.02	8.99%
42	0.60	0.55	0.02	0.02	8.31%
43	0.60	0.56	0.02	0.02	7.04%
44	0.61	0.56	0.02	0.02	8.51%
45	0.61	0.57	0.02	0.02	8.19%
46	0.62	0.56	0.02	0.02	9.52%
47	0.62	0.57	0.02	0.02	8.66%
48	0.63	0.57	0.02	0.02	9.31%

Table C.17: Detailed coefficients *on skill distance after occupational change* from earnings regression (8) (table 1/2).

m	γ_m	SE
0	–	–
1	-77.57	66.61
2	-26.51	59.72
3	-49.18	57.16
4	-85.56	53.27
5	-69.31	53.65
6	-70.15	51.66
7	-99.37	51.78
8	-73.07	49.93
9	-68.02	49.37
10	-45.63	46.42
11	-70.24	46.62
12	-76.34	47.41
13	-76.75	48.34
14	-62.38	46.98
15	-75.48	46.67
16	-84.84	46.11
17	-101.74	47.23
18	-98.91	46.62
19	-120.29	46.56
20	-91.79	44.73
21	-89.79	43.66
22	-97.75	43.78
23	-82.71	43.21

Table C.18: Detailed coefficients *on skill distance after occupational change* from earnings regression (8) (table 2/2).

m	γ_m	SE
24	-85.16	44.43
25	-83.22	42.83
26	-67.28	43.27
27	-63.44	43.03
28	-67.03	42.53
29	-85.43	41.85
30	-63.75	40.91
31	-66.13	39.79
32	-72.09	40.21
33	-67.59	40.56
34	-59.39	41.20
35	-38.40	40.42
36	-33.49	38.93
37	-23.81	38.00
38	-33.29	36.08
39	-19.31	35.28
40	-15.57	35.11
41	-20.24	33.30
42	-21.49	33.97
43	-20.10	33.35
44	-27.49	33.10
45	-43.06	32.89
46	40.84	20.16
47	34.09	16.83

Table C.19: Detailed coefficients from migration propensity regression (11) (table 1/2).

m	Coefficient	SE
0	–	–
1	0.00	0.00
2	0.00	0.00
3	0.00	0.00
4	0.00	0.00
5	0.01	0.00
6	0.01	0.01
7	0.00	0.01
8	0.01	0.01
9	0.01	0.01
10	0.01	0.01
11	0.01	0.01
12	0.01	0.01
13	0.02	0.01
14	0.02	0.01
15	0.02	0.01
16	0.03	0.01
17	0.03	0.01
18	0.03	0.01
19	0.03	0.01

Table C.20: Detailed coefficients from migration propensity regression (11) (table 2/2).

m	Coefficient	SE
20	0.02	0.01
21	0.02	0.01
22	0.03	0.01
23	0.03	0.01
24	0.02	0.01
25	0.02	0.01
26	0.03	0.01
27	0.02	0.01
28	0.02	0.01
29	0.02	0.01
30	0.03	0.01
31	0.03	0.01
32	0.03	0.01
33	0.03	0.01
34	0.03	0.01
35	0.03	0.01
36	0.03	0.01
37	0.02	0.01
38	0.03	0.01
39	0.02	0.01
40	0.03	0.01
41	0.03	0.01
42	0.03	0.01
43	0.03	0.01
44	0.03	0.01
45	0.03	0.01
46	0.02	0.01
47	0.03	0.01
48	0.02	0.01