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Does the internet help the unemployed find jobs?

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ABSTRACT

This study examines the effect of internet job search (IJS) on job-finding rates among unemployed job seekers during the rapid expansion of the internet from the mid-1990s to the early 2010s. To address endogenous selection into IJS, I use an instrumental variables (IV) strategy exploiting the rise of IJS within occupations over time, which varied across occupations depending on pre-internet exposure to computers at work. The analysis sample includes unemployed workers from the December 1998, August 2000, September 2001, October 2003, and July 2011 Current Population Survey (CPS) Computer and Internet Use Supplements and the September 1992 Basic Monthly CPS, longitudinally matched with their employment outcomes from the subsequent monthly CPS files. The IV estimates indicate that IJS increased the 15-month job-finding rate by 12.9 percentage points (25.1% relative to the mean). Results from placebo exercises and various specification checks support a causal interpretation of the estimated effects. Additionally, the effectiveness of IJS remained stable over time throughout the analysis period.

1. Introduction

Using the internet for job search and recruitment saw a rapid expansion after the advent of the internet in the mid-1990s. In the US, large commercial job sites, such as Monster.com, CareerBuilder.com, and Hotjobs.com, started in 1994–1996. America's Job Bank was founded by the US government in 1995. Craigslist.org, a well-known non-profit advertisement site that includes job posts, was launched in 1995. During the late 1990s and early 2000s, a number of niche job boards and social networking sites, such as LinkedIn, were introduced. The proportion of unemployed workers searching for jobs online increased sixfold from 13% in 1998 to 79% in 2015 (see Fig. 1). Nowadays, the internet provides essential resources and tools for job seekers, and online job search has become even more prevalent with the rise of social media and mobile devices (Cumming et al., 2022; Pew Research Center, 2015; Zhao, 2019). On the employer side, already 94% of the Global 500 companies were recruiting online by 2003, up from only 29% in 1998 (Nakamura et al., 2009).

This study examines the effect of internet job search (IJS) on the employment prospects of unemployed job seekers during the rapid expansion of the internet from the mid-1990s to the early 2010s. While studies demonstrate that the diffusion of the internet substantially improved search efficiency in the product (Brown and Goolsbee, 2002) and marriage (Bellou, 2015) markets, research on internet advantages in job search processes is limited and mixed. Among the few studies focusing on IJS in the US labor market, Kuhn and Skuterud (2004) and Kuhn and Mansour (2014) find that until the early 2000s, unemployment durations among online job seekers were not shorter than their observably similar counterparts who only used offline search methods. Among young workers in the late 2000s, however, Kuhn and Mansour (2014) show that IJS was associated with shorter unemployment spells. I extend the seminal studies of Kuhn and Skuterud (2004) and Kuhn and Mansour (2014) by addressing endogenous selection into IJS and identifying the effectiveness of IJS.

The analysis sample includes unemployed workers aged 16 or above from the December 1998, August 2000, September 2001, October 2003, and July 2011 Current Population Survey (CPS) Computer and Internet Use Supplements and the September 1992 Basic Monthly CPS. Utilizing the CPS survey structure, I longitudinally link individuals from these six baseline surveys with their employment outcomes up to 15 months after, which are observed in subsequent waves of the monthly CPS. Given that the internet was unavailable for general use until 1994, respondents from the September 1992 CPS serve as the pre-intervention sample, in which no one engaged in IJS activities.

As individuals freely decide whether to use the internet for job search, IJS status would be confounded with job seekers' observable and unobservable characteristics. To address this selection problem, I use an instrumental variables (IV) strategy exploiting the rise of IJS within occupations over time, which varied across occupations depending on pre-internet exposure to computers at

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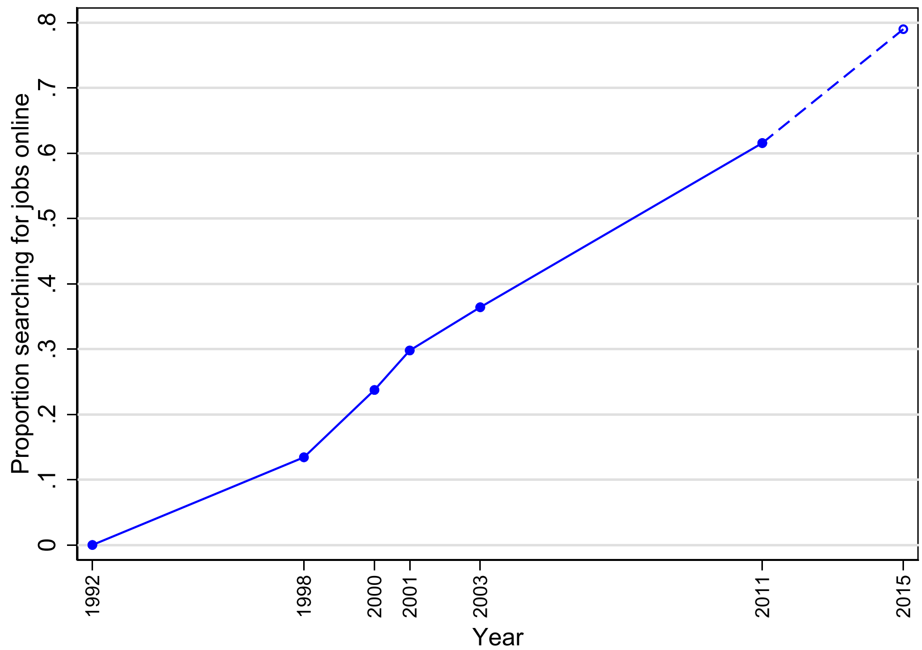

Fig. 1. Proportion of Unemployed Workers Searching for Jobs Online.
Notes. Calculations using data from the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements. The 2015 statistic is from Pew Research Center (2015).

work. The instrument is constructed by multiplying a time trend with the occupation-specific computer use rate at work before the emergence of the internet, which is computed using data from the October 1993 CPS School Enrollment Supplement. This empirical strategy is effectively a difference-in-differences IV design in that I exploit variation within occupations over time before and after the intervention in 1994. The instrument strongly predicts differential growth in IJS adoption across occupations.

The key identifying assumption of this IV strategy is that pre-internet computer use intensity cannot predict within-occupation *changes* in job-finding rates without IJS expansion. This assumption would be violated if changes in labor market conditions were systematically correlated with pre-internet computer use intensity across occupations. To address this concern, I perform multiple internal validity checks. First, I conduct a placebo exercise illustrating that the instrument has no predictive power for employment outcomes before internet expansion. Second, I show that the IV estimates are robust to a range of specification checks, especially to the inclusion of control variables representing concurrent labor demand and supply conditions. Lastly, I show that the instrument is unrelated to the probability of switching an occupation, which may exclusively affect the job-finding rate. Results from these exercises suggest that the instrument is unconfounded with preexisting or contemporaneous occupation-specific trends in employment outcomes and thus unlikely to violate the exclusion restriction.

I find that online job seekers are substantially more likely to be employed at various follow-up points than those only searching offline. The IV estimates show that IJS raises the probability of finding a job within the 15-month follow-up period by 12.9 percentage points (25.1% relative to the average employment probability of 0.514). As results from the aforementioned placebo exercise and various specification checks support a causal interpretation of the estimated effects, the IV estimates provide evidence that IJS is effective in facilitating (re-)employment opportunities for the unemployed and reduced search frictions in the labor market. However, the benefits of IJS do not appear in the ordinary least squares (OLS) regression controlling for observable characteristics only. Thus, the difference between the IV and OLS estimates suggests that online job seekers are negatively selected on unobservables. Moreover, I document that IJS effectiveness and selection patterns remained stable over time throughout the analysis period.

This paper contributes to the under-studied literature on online job search and its effectiveness. Kuhn and Skuterud (2004) and Kuhn and Mansour (2014) highlight that online job seekers are positively selected on observables and appear to be negatively selected on unobservables. Only a few studies circumvent this endogeneity problem. Among them, two studies focusing on the expansion of a specific search engine find mixed results. Bagues and Labini (2009) study AlmaLaurea, a job search engine founded in 1994 by a consortium of Italian universities to facilitate the school-to-work transition of college graduates. Their analysis shows that AlmaLaurea lowered the unemployment rate among graduates of AlmaLaurea member universities. On the other hand, Kroft and Pope (2014) find that the expansion of Craigslist between 2005 and 2007 had no measurable impact on local area unemployment rates in the US. More recently, Bhuller et al. (2021), Denzer et al. (2021), and Gürtzgen et al., 2021 exploit geographic and temporal variation in broadband roll-outs in Germany and Norway. They show that better access to the internet has enhanced both IJS usage and employment prospects among the unemployed. This paper is differentiated from the recent prior works in three aspects. First, the endogeneity of IJS behaviors is addressed at the individual level. Second, I provide more direct evidence of the effectiveness of IJS in the context of the US labor market. Lastly, I examine whether IJS effectiveness and selection patterns have changed over time.

The remainder of this paper is organized as follows. Section 2 describes the construction of the analysis sample and key variables from the CPS data. Section 3 explains the IV estimation strategy used to address the selection problem in the IJS status and measure the effect of IJS on job-finding rates.

Section 4 presents the results of IV estimation and internal validity checks. Section 5 discusses the mechanisms and implications of the findings. Section 6 concludes.

2. Data and analysis sample

This section outlines the construction of the analysis sample and key variables. Further details are provided in Sections S1 and S2 of the online appendix.

The main analysis sample includes the unemployed from the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements and the September 1992 Basic Monthly CPS.[1] The internet use supplements surveyed whether the respondents had used the internet to search for work.

To examine employment outcomes, I utilize the CPS rotation group structure and longitudinally match individuals in the 1992/09, 1998/12, 2000/08, 2001/09, 2003/10, and 2011/07 data with the same survey respondents in the ten subsequent Basic Monthly CPS files.[2] The CPS sample is split into eight rotation groups, each entering the CPS in the same month. Respondents are interviewed for 4 consecutive months, not interviewed for the next 8 months, and re-interviewed for another 4 months before exiting the CPS. Based on this survey structure, each person's employment status can be tracked until a maximum of 15 months after the initial survey.[3] Table A1 documents which CPS files are linked from month 0 to months 1–3 and 9–15. For example, respondents from the 2011/07 CPS (i.e., month 0) are merged with their employment outcomes in 2011/08–2011/10 (i.e., months 1–3) and 2012/04–2012/10 (i.e., months 9–15). Those first interviewed in 2011/07 were followed up in the next 3 months until 2011/10, out of the survey for 8 months, and interviewed again for another 4 consecutive months from 2012/07 to 2012/10. Those interviewed in 2011/07 for the fourth time were interviewed again in 2012/04–2012/07, after the 8-month break. Note also that the entire 15-month follow-up period of the 1992/09 CPS respondents falls before 1994 (with 1993/12 being month 15). Therefore, their employment outcomes are unaffected by the emergence of the internet.

The analysis sample includes unemployed workers aged 16 or above at baseline surveys in 1992/09, 1998/12, 2000/08, 2001/09, 2003/10, and 2011/07, whose employment outcomes are observed at least once in subsequent monthly surveys.[4] This yields a sample of 15,655 individuals, 4136 of whom have searched for jobs through the internet.[5]

To construct an instrumental variable exploiting pre-internet exposure to computers at work, I use data from the October 1993 CPS School Enrollment Supplement and calculate the fraction of employed workers using a computer at work by detailed CPS occupation category. Table A2 lists the calculated values of computer use intensity for each occupation in 1993. As the analysis sample is restricted to unemployed workers from the six waves of the CPS, their occupations are classified based on the last job held as of these baseline surveys and linked with the occupation-specific computer use rate calculations. The computer use rate at work cannot be linked to those who have never had a job nor to those who served in the armed forces just before becoming unemployed. Among 15,655 unemployed workers in the analysis sample, 1121 never had a job, and 59 served in the armed forces before the current unemployment spell. For the former, the computer usage rate at work is set to zero.[6] The average computer use rate across all occupations (i.e., 0.452) is used for the latter.

Table 1 reports summary statistics of variables used in the analysis. Panel A shows the IJS status at the baseline survey (i.e., month 0) and employment outcomes after 1–15 months. Statistics from post-internet (i.e., 1998/12, 2000/08, 2001/09, 2003/10, and 2011/07) data suggest that online job seekers are more likely to be employed in months 9–15 than those searching offline only.

Panel B presents background characteristics measured in the baseline surveys. The unemployed workers who look for work online tend to have observable characteristics associated with a higher likelihood of employment than those who do not engage in online job search activities, as pointed out by Kuhn and Skuterud (2004), Fountain (2005), Stevenson (2009), and Kuhn and Mansour (2014). First, regarding demographic and socioeconomic status, online job seekers are more likely to be of prime working age, better educated, less likely to be Black or Hispanic, and more likely to be married. Second, online job seekers are positively selected on worker characteristics: a higher fraction of online job seekers are voluntary job quitters and have worked prior to the current unemployment spell compared to their offline-only counterparts.[7] Third, online job seekers are more likely to use each traditional job search method compared to offline searchers.[8] Lastly, online job seekers are more concentrated in occupations where computer use was traditionally more prevalent, and have better access to the internet at home.

3. Empirical strategy

I estimate the effect of IJS on employment propensities using the following linear probability model:

$$y_i = \alpha + \beta IJS_i + \mathbf{X}_i'\boldsymbol{\gamma} + \delta_k + \lambda_t + \varepsilon_i, \tag{1}$$

[1] The analysis is restricted to the civilian noninstitutional population aged 16 or above, which is the universe of labor market statistics published by the Bureau of Labor Statistics (BLS).

[2] Madrian and Lefgren (2000) describe the CPS longitudinal design and propose approaches to match respondents across CPS surveys.

[3] Unlike employment status, each worker's wage cannot be tracked month to month as the CPS collects wage information only among those surveyed for the fourth and eighth times. This makes the analysis of wage outcomes very challenging owing to insufficient observations in the analysis sample.

[4] Under the CPS rotation group system, up to 75% of households at month 0 can be matched to month 1; 50% to months 2 and 12; 37.5% to months 11 and 13; 25% to months 3, 10, and 14; and 12.5% to months 9 and 15. Actual match rates are usually lower than the maximum possible rate due to sample attrition.

[5] The July 2013 and July 2015 CPS supplements also collected information on IJS status but are excluded from the main analysis for the following reasons. In the 2013/07 supplement, IJS status was not inquired to those in the two rotation groups who were interviewed for the first or the fifth time, making employment outcomes in months 3 and 15 unavailable. In the 2015/07 supplement, IJS status was surveyed only among randomly selected internet users. Hence, using the 2015/07 data, it is infeasible to construct a representative sample of unemployed workers, including both online and offline searchers. Furthermore, occupational categories in both 2013/07 and 2015/07 data are based on the 2010 Census occupational classification system, which is very different from the 1990 Census occupational classification system used in the main analysis sample. Converting the 2010 occupation codes to the 1990 codes is inadequate, creating substantial measurement errors in the instrumental variables. Section S4 in the online appendix provides further details and discusses the results from the extended sample including the 2013/07 and 2015/07 data.

[6] In Section 4.3, I discuss results from alternative imputation methods or a sample excluding the never-worked.

[7] Compared to offline searchers in the same survey year, online job seekers have occupations with lower unemployment rates by 0.8–1.9 percentage points. Pooling all survey years as in Table 1 makes this pattern unclear because online job seekers are disproportionately from more recent data and the occupational unemployment rates are higher in 2011 than in the earlier survey years before the Great Recession.

[8] The 1992/09 CPS lists six traditional job search methods: 1) contacted employer directly, 2) contacted public employment agency, 3) contacted private employment agency, 4) contacted friends or relatives, 5) placed or answered ads, and 6) other search methods. To ensure that the search method categories are consistent with the 1992/09 file, additional search methods listed in the 1998/12, 2000/08, 2001/09, 2003/10, and 2011/07 CPS are reclassified as other search methods. They include contacted school/university employment center, sent out resumes/filled out application, checked union/professional registers, other active, looked at ads, attended job training programs/courses, and other passive. Note that job search methods categorized in the CPS may or may not involve using the internet.

Table 1
Summary Statistics.

	Pre-internet: 1992/09		Post-internet: 1998/12, 2000/08, 2001/09, 2003/10, 2011/07					
			Offline job searchers		Online job searchers			
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean diff. (7)	SE (8)
A. Internet job search status and employment outcomes								
Search for jobs online	0.000	[0.000]	0.000	[0.000]	1.000	[0.000]	1.000	(0.000)
Employed within 15 months	0.504	[0.500]	0.519	[0.500]	0.515	[0.500]	-0.004	(0.012)
Employed in month 1	0.270	[0.444]	0.285	[0.451]	0.225	[0.417]	-0.060	(0.010)***
Employed in month 2	0.330	[0.470]	0.358	[0.479]	0.329	[0.470]	-0.029	(0.014)**
Employed in month 3	0.358	[0.480]	0.386	[0.487]	0.386	[0.487]	0.001	(0.021)
Employed in month 9	0.495	[0.501]	0.489	[0.500]	0.567	[0.496]	0.077	(0.031)**
Employed in month 10	0.502	[0.500]	0.517	[0.500]	0.557	[0.497]	0.040	(0.023)*
Employed in month 11	0.504	[0.500]	0.514	[0.500]	0.570	[0.495]	0.056	(0.019)***
Employed in month 12	0.508	[0.500]	0.504	[0.500]	0.575	[0.495]	0.071	(0.015)***
Employed in month 13	0.527	[0.500]	0.501	[0.500]	0.599	[0.490]	0.098	(0.018)***
Employed in month 14	0.506	[0.500]	0.510	[0.500]	0.642	[0.480]	0.131	(0.021)***
Employed in month 15	0.519	[0.500]	0.520	[0.500]	0.616	[0.487]	0.096	(0.030)***
B. Background characteristics								
Age								
Age 16–25	0.332	[0.471]	0.320	[0.467]	0.229	[0.420]	-0.091	(0.019)***
Age 26–35	0.268	[0.443]	0.202	[0.402]	0.246	[0.431]	0.044	(0.010)***
Age 36–45	0.200	[0.400]	0.207	[0.405]	0.227	[0.419]	0.020	(0.011)*
Age 46–55	0.120	[0.325]	0.153	[0.360]	0.197	[0.397]	0.044	(0.009)***
Education								
Some high school	0.209	[0.407]	0.252	[0.434]	0.089	[0.284]	-0.163	(0.011)***
High school graduate	0.556	[0.497]	0.541	[0.498]	0.496	[0.500]	-0.045	(0.018)**
Some college	0.052	[0.221]	0.047	[0.212]	0.097	[0.296]	0.050	(0.005)***
College graduate	0.132	[0.338]	0.091	[0.287]	0.312	[0.464]	0.221	(0.019)***
Black	0.177	[0.382]	0.192	[0.394]	0.148	[0.356]	-0.044	(0.008)***
Hispanic	0.105	[0.306]	0.167	[0.373]	0.088	[0.284]	-0.079	(0.008)***
Male	0.525	[0.499]	0.509	[0.500]	0.469	[0.499]	-0.039	(0.022)*
Married	0.419	[0.493]	0.362	[0.481]	0.419	[0.493]	0.058	(0.015)***
Married male	0.218	[0.413]	0.175	[0.380]	0.201	[0.401]	0.026	(0.014)*
Spouse employed	0.285	[0.451]	0.294	[0.456]	0.360	[0.480]	0.065	(0.013)***
Home owner	0.568	[0.495]	0.562	[0.496]	0.573	[0.495]	0.011	(0.011)
Duration of unemployment (in weeks)	18.48	[23.39]	17.44	[24.94]	24.69	[29.55]	7.249	(0.963)***
Quit job	0.115	[0.320]	0.106	[0.308]	0.130	[0.336]	0.023	(0.007)***
On layoff	0.113	[0.316]	0.166	[0.372]	0.058	[0.234]	-0.107	(0.011)***
Status before unemployment								
Worked (not including those on layoff)	0.517	[0.500]	0.451	[0.498]	0.634	[0.482]	0.183	(0.016)***
Attended school	0.046	[0.210]	0.163	[0.370]	0.158	[0.365]	-0.005	(0.017)
Did something else	0.324	[0.468]	0.220	[0.414]	0.150	[0.357]	-0.070	(0.009)***
Sector								
Private sector	0.777	[0.416]	0.805	[0.396]	0.812	[0.390]	0.008	(0.023)
Public sector	0.088	[0.283]	0.075	[0.264]	0.108	[0.310]	0.032	(0.011)***
Self-employed	0.028	[0.164]	0.046	[0.209]	0.044	[0.204]	-0.002	(0.005)
State unemployment rate (%)	7.675	[1.378]	5.402	[1.788]	6.485	[2.331]	1.083	(0.148)***
Occupational unemployment rate (%)	7.701	[2.987]	6.320	[3.036]	6.210	[3.321]	-0.110	(0.276)
Traditional job search methods								
Contacted employer directly	0.652	[0.476]	0.524	[0.499]	0.569	[0.495]	0.045	(0.012)***
Contacted public employment agency	0.196	[0.397]	0.165	[0.371]	0.236	[0.425]	0.072	(0.007)***
Contacted private employment agency	0.081	[0.273]	0.052	[0.222]	0.114	[0.318]	0.062	(0.007)***
Contacted friends or relatives	0.217	[0.413]	0.136	[0.343]	0.206	[0.405]	0.071	(0.009)***
Placed or answered ads	0.369	[0.483]	0.109	[0.312]	0.219	[0.413]	0.110	(0.007)***
Other search methods	0.044	[0.206]	0.547	[0.498]	0.782	[0.413]	0.235	(0.012)***
Number of traditional search methods used	1.559	[1.014]	1.532	[1.102]	2.126	[1.208]	0.594	(0.032)***
Internet access at home	0.000	[0.000]	0.330	[0.470]	0.834	[0.372]	0.504	(0.014)***
Occupational computer use rate in 1993[a]	0.320	[0.290]	0.283	[0.269]	0.473	[0.301]	0.190	(0.022)***
N	3,775		7,744		4,136			

Notes. The baseline characteristics are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements. The employment outcomes are from the subsequent Basic Monthly CPS files matched with the baseline surveys. The number of observations for the employment outcomes varies by follow-up months owing to the rotation group design of the CPS. Standard deviations are reported in brackets. Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] The pre-internet computer use at work by occupation is computed using data from the October 1993 CPS School Enrollment Supplement.

where IJS_i indicates whether person i searches for work online at the baseline survey (conducted in year t = 1992, 1998, 2000, 2001, 2003, or 2011). The outcome variable y_i is a dummy on whether person i becomes employed in a follow-up period, which is 1–15 months after the baseline survey. The regression model includes fixed effects for person i's occupation k (δ_k) and baseline survey year t (λ_t), which are essential in my IV design that exploits variation within occupations over time. The vector $\mathbf{X}_i$ controls for person i's demographic characteristics, socioeconomic status, worker characteristics, activities prior to unemployment, and labor market conditions. All of these covariates are observed in the baseline survey.[9] ε_i is the error term representing the remaining unobserved determinants of the employment outcome.

Because of endogenous selection into IJS, the OLS estimates of the regression model (1) are unlikely to reveal the causal effect of IJS on employment outcomes. To isolate potentially exogenous variation in individuals' IJS status, I employ an IV strategy that exploits within-occupation growth in IJS during the rapid expansion of the internet in the 1990s and 2000s. The instrument projects IJS growth based on pre-internet exposure to computers across occupations. The idea behind this instrument is that individuals with more computer-intensive occupations adopted the internet and IJS earlier and faster than those with less computer-intensive occupations. As the only source of variation in this instrument is the interaction between time and pre-internet computer use intensity across occupations, whether an individual adopted IJS more quickly than others does not affect the projected IJS growth.

IV specification. Based on this idea, I specify my instrument for IJS_i as $C93_k \times \lambda_t$, where $C93_k$ is the proportion of workers using a computer at work in occupation k in 1993 and λ_t represents a set of time dummies. When I present the instrument using indicator functions, the first-stage equation is expressed as follows:

$$IJS_i = \mu + \sum_{l>1994} \theta_l (C93_k \times 1[t=l]) + \mathbf{X}_i'\boldsymbol{\pi} + \delta_k + \lambda_t + u_i, \qquad (2)$$

where $1[t=l]$ indicates whether the baseline survey year is l. As I exploit variation within occupations over time before and after the intervention (i.e., the introduction of the internet to the general public), my empirical strategy is effectively a difference-in-differences IV (DiD-IV) method.[10] In this DiD-IV design, years before 1994 serve as the pre-intervention period. Thus, the dummy for the survey year 1992 is omitted from the regression as a reference year. Standard errors in all regression analyses are clustered at the occupation-by-year level, corresponding to the level of variation in the instrument.[11]

In Section 4.3, I show that qualitatively similar IV estimates are obtained from alternative specifications for the time trend component in the instrument, such as a linear function (i.e., $C93_k \times t$) or higher order polynomials (i.e., $C93_k \times f(t)$).[12] $C93_k \times \lambda_t$ is used in the main analysis because it employs the most flexible time trend specification and generally yields more conservative estimates than linear or polynomial specifications. Results using $C93_k \times t$ are also reported in the subsequent regression tables for its simplicity and ease of interpretation.

First stage. Figures 2 and A1 plot the expansion of IJS from 1992 to 2011 across occupations with various degrees of pre-internet exposure to computers at work, $C93_k$, illustrating the first-stage relationship. Six selected occupations are presented in Fig. 2 and all occupations in Fig. A1. The intensity of pre-internet computer use varies considerably across occupations with $C93_k$ ranging from 0.6% to 98.4%. More importantly, this variation well predicts the differential growth of IJS adoption across occupations. Occupations with a higher fraction of computer users in the pre-internet days exhibit a more rapid increase in the share of online job seekers after the internet became available for general use in 1994. This pattern is depicted in the fanning-out trajectories in Figs. 2 and A1.

For example, before the introduction of the internet, nearly 90% of technicians used computers at work, while less than 1% of private household service workers did so. During the expansion of the internet from pre-1994 to 2011, the fraction of online job seekers grew rapidly to 0.967 among the former and at a much slower rate to 0.318 among the latter. This pattern is more pronounced up to the early 2000s: in 2001, the proportion of unemployed workers searching for jobs online was 0.688 among the former, while staggering at 0.095 among the latter. The average computer use rate at work across all occupations was 45.2% in 1993, close to 42.7% and 49.0% among protective service workers and teachers, respectively. From pre-1994 to 2011, protective service work and the teaching profession saw a steady increase in online job searchers to 0.561 and 0.609, respectively.

Identification assumption. Using the interactions between time and pre-internet computer use intensity as IV for IJS in Eqs. (1) and (2) relies on the assumption that pre-internet exposure to computers at work had no other effect on changes in employment outcomes except through IJS expansion. Note that any level differences across occupations with low and high intensity of initial computer use are captured by occupation fixed effects and thus would not invalidate the identification strategy. The DiD-IV design also allows for any time-varying factors that commonly affect workers in all occupations, such as economic growth or business cycles, which are captured by the survey year fixed effects. Therefore, the only potential threats to the identification strategy are factors that are confounded with initial computer use intensity and exclusively affect within-occupation *changes* in job-finding rates. To address the concern of violating the exclusion restriction, I conduct multiple internal validity checks in Section 4.2, showing that the instrument is unlikely to be confounded with preexisting or contemporaneous occupation-specific trends in employment outcomes.

4. Results

In this section, I estimate the effect of IJS on the job-finding rate using both OLS and IV methods and examine the internal validity of the empirical strategy. I mainly focus on whether unemployed workers find a job within 15 months and also present whether they are employed in months 1, 2, and 12. The estimated IJS effects for all follow-up months are reported in the online appendix.[13]

4.1. Main results

In Table 2, I first present OLS estimates of the IJS effect on employment probability with and without controlling for covariates $\mathbf{X}_i$. Although controlling for observable characteristics would not sufficiently resolve endogeneity in IJS status, the OLS estimates are presented for comparison with the IV estimates. Panel A shows that the OLS estimates conditional on various control variables are small in magnitude with mixed signs. During the 15-month follow-up period, the difference in the job-finding rate be-

[9] In addition to occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state.

[10] A canonical example using a DiD-IV design is Duflo (2001), who estimates returns to schooling by instrumenting educational attainment with interactions between cohort dummies and intensity of the Indonesian school construction program.

[11] Abadie et al. (2017) emphasize that whether or at what level standard errors should be clustered depends on the experimental design. If treatment assignment is made across certain groups, clustering at this group level can be justified in general.

[12] When $C93_k \times t$ or $C93_k \times f(t)$ is used, my IV strategy becomes similar in spirit to that of Acemoglu and Johnson (2007), where life expectancy is instrumented with predicted mortality constructed as a deterministic function of time and pre-intervention mortality rates from various diseases.

[13] The IV estimates for follow-up months 9–11 and 13–15 may be subject to a weak instrument bias. See footnote [17] for further details.

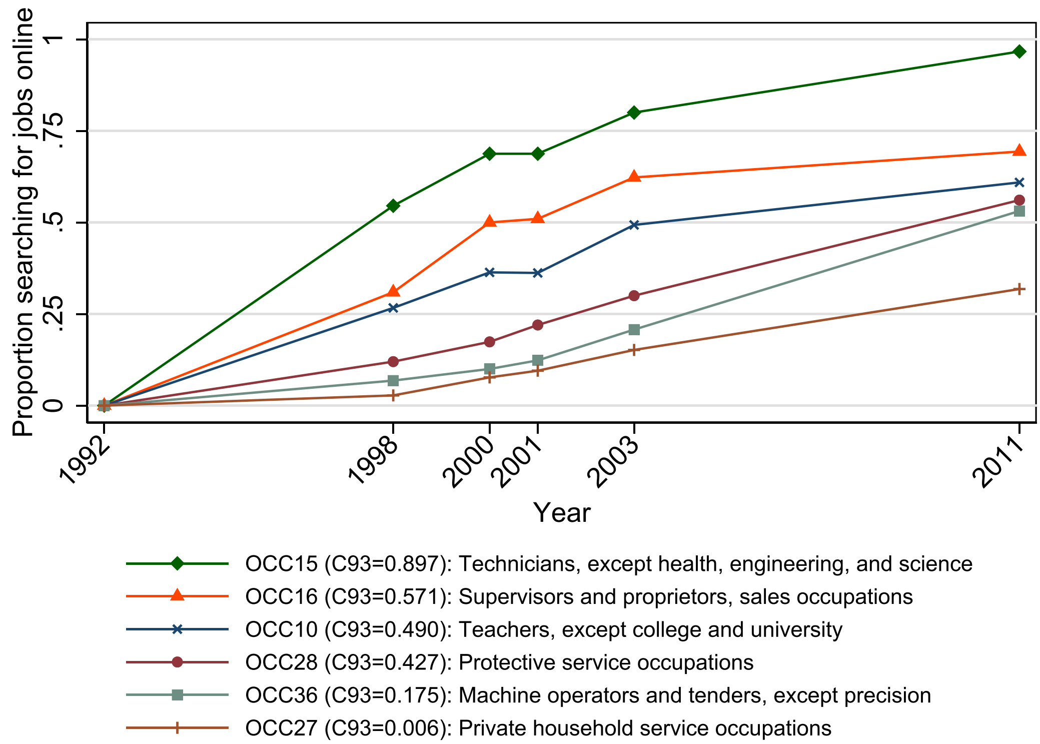

Fig. 2. Proportion of Unemployed Workers Searching for Jobs Online, by Occupation (Selected Occupations).
Notes. Online job search status is from the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements. $C93$ denotes the 1993 computer use rate at work in each occupation from the October 1993 CPS School Enrollment Supplement. Occupation codes follow the 1990 Census occupational classification.

Table 2
OLS Estimates of Internet Job Search Effects on Employment Probability.

Dependent variable:	Employed within	Employed in		
	15 months (1)	month 1 (2)	month 2 (3)	month 12 (4)
A. OLS with controls				
IJS	0.018*	-0.022**	-0.014	0.056***
	(0.010)	(0.009)	(0.015)	(0.015)
Adjusted R^2	0.079	0.081	0.083	0.101
B. OLS without controls				
IJS	0.027***	-0.026***	-0.005	0.093***
	(0.010)	(0.010)	(0.014)	(0.016)
Adjusted R^2	0.022	0.029	0.026	0.028
N	15,655	13,613	8,679	6,301
Mean of Y	0.514	0.265	0.344	0.524

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. In panels A and B, occupation and year fixed effects are included in the regressions. In panel B, the regressions also control for unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

tween observably comparable online and offline searchers is 0.018 on average. When individual characteristics are not held constant in panel B, estimated differences tend to be higher than those from controlled OLS regressions. The OLS results confirm that internet job searchers are positively selected on observables as in Kuhn and Skuterud (2004) and Kuhn and Mansour (2014).[14]

Table 3 presents IV estimates and describes the main finding of this study: online job seekers are substantially more likely to find a job at various follow-up points than those who only search offline. Panel A reports estimates using $C93_k \times \lambda_t$ as instruments. Overall, IJS raises the employment probability by 12.9 percentage points (with p-value $= 0.047$) during the 15-month follow-up period, which is 25.1% relative to the mean of 0.514.[15] The IV specification using $C93_k \times t$ in panel B tends to provide larger estimates. The estimated effect of searching for jobs online is substantial, considering that average job-finding rates among offline searchers are 28.0%, 34.9%, and 50.5% in months 1, 2, and 12, respectively, and 51.4% during the entire 15-month follow-up period.

In the online appendix, Figure S2 shows that the point estimate for each follow-up month, except months 10–12, is similar to the benchmark estimate for the entire follow-up period. Even the smaller point estimates for months 10–12 are not statistically distinguishable from the benchmark estimate, as standard errors are large. The IJS effects are imprecisely estimated, especially for months 3 and 9–15, owing to smaller sample sizes. As noted in

[14] Based on duration analyses controlling for observable characteristics of job seekers, Kuhn and Skuterud (2004) and Kuhn and Mansour (2014) document that IJS did not reduce unemployment spells during the late 1990s and early 2000s, and Kuhn and Mansour (2014) report stronger benefits among young unemployed workers in the late 2000s. In the online appendix, I compute an OLS version of Kuhn and Skuterud (2004)'s results using CPS data from December 1998 and August 2000. I demonstrate that similar OLS estimates are obtained even when I add the September 1992 CPS data to Kuhn and Skuterud (2004)'s analysis sample and control for year and occupation fixed effects as in Eq. (1) (see Table S1).

[15] In Figure S1, I additionally control for home internet access and/or traditional job search methods, as in Kuhn and Skuterud (2004) and Kuhn and Mansour (2014), and obtain similar estimates to the main specification.

Table 3
IV Estimates of Internet Job Search Effects on Employment Probability.

Dependent variable:	Employed within	Employed in		
	15 months (1)	month 1 (2)	month 2 (3)	month 12 (4)
A. IV: $C93_k \times \lambda_t$				
IJS	0.129**	0.130**	0.105	0.043
	(0.065)	(0.056)	(0.075)	(0.093)
First-stage *F*-statistic	37.07	32.62	32.86	28.12
B. IV: $C93_k \times t$				
IJS	0.266**	0.255***	0.177	0.003
	(0.112)	(0.087)	(0.125)	(0.162)
First-stage *F*-statistic	39.93	35.60	27.67	32.61
N	15,655	13,613	8,679	6,301
Mean of *Y*	0.514	0.265	0.344	0.524

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. *C*93 denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. The first-stage *F*-statistics are computed by implementing the weak instrument test by Montiel Olea and Pflueger (2013). Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Section 2, less than 50% of individuals in month 0 can be matched to their employment outcomes after month 2 due to the 4-8-4 rotation group design of the CPS.

The first-stage *F*-statistics reported in Table 3 are computed following Montiel Olea and Pflueger (2013), who provide a weak instrument test robust to heteroskedasticity, autocorrelation, and clustering.[16] They propose rejecting the null hypothesis for weak instruments when the *F*-statistic is greater than 23.1, which is much higher than 10—the conventional rule of thumb established by Stock and Yogo (2005) for cases with homoskedastic and serially uncorrelated errors. As the computed *F*-statistics are above the threshold of 23.1, I can reject the null hypothesis that the instrument, $C93_k \times \lambda_t$ or $C_k^{93} \times t$, is weak.[17]

In the online appendix, Table S2 presents the first-stage relationship between the instrument and IJS.[18] The first-stage estimates using $C_k^{93} \times \lambda_t$ in panel A imply that unemployed individuals who worked in an occupation with a 10 percentage point higher pre-internet computer use rate experienced an additional increase in IJS by 5.59 and 3.36 percentage points from 1992–2001 and 1992–2011, respectively.[19] Those in an occupation with a computer use rate one standard deviation (i.e., 0.294) above would have a 16.4 percentage point higher IJS rate in 2001, which is over one-third of a standard deviation of the 2001 IJS rate (i.e., 45.9%), and a 9.9 percentage point higher IJS rate in 2011, which is approximately one-fifth of a standard deviation of the 2011 IJS rate (i.e., 48.7%). Therefore, the estimated first-stage effect on IJS status is quite large.

4.2. Internal validity

In my IV strategy, a major potential threat to identification is violation of the exclusion restriction. Occupation-specific changes in labor market circumstances may become another channel besides IJS through which the instrument affects job-finding rates. If labor market conditions evolved more favorably for workers in more computer-intensive occupations, IV estimates would be biased upwards. To address this concern, I demonstrate that the instrument is unlikely to be confounded with preexisting or contemporaneous occupation-specific trends in employment outcomes.

Placebo exercises using pre-internet data. I conduct placebo exercises on the reduced-form regression using data from the pre-internet period. Before 1994, the internet was unavailable to the general public. Consequently, no first-stage relationship exists between the instrument and IJS, and reduced-form effects should also be zero unless the instrument is confounded with preexisting trends in employment outcomes. For comparison, Table 4 presents reduced-form estimates from the main analysis sample.

In Table 5, the pre-internet data are collected from the September 1983, December 1989, August 1991, and September 1992 Basic Monthly CPS and matched with their subsequent monthly CPS files.[20] The placebo sample is designed to mimic the first four waves of the main analysis sample from the 1992/09, 1998/12, 2000/08, and 2001/09 CPS in terms of time coverage and time interval between surveys.[21] Unlike the positive and significant reduced-form effects during the main analysis period (1992–2011) in Table 4, there is no evidence of such positive relationship between the instrument and employment status in the pre-internet sample (1983–1992). Panel A shows that coefficients on the instruments $C93_k \times \lambda_t$ exhibit no systematic pattern and are not statistically distinguishable from zero either individually or jointly at the 10% significance level. Moreover, in panel B, the coefficient on the instrument $C93_k \times t$ is precisely zero for most employment outcomes or, if statistically significant, negative and very close to zero (e.g., for employment in month 12). These results suggest the lack of preexisting trends in employment outcomes systematically related to pre-internet occupational computer use intensity in 1993.

In the online appendix, I repeat a placebo test on another pre-internet sample from the September 1989, September 1990, September 1991, and September 1992 Basic Monthly CPS, matched with their subsequent monthly CPS files. For this exercise, a shorter time frame is used to focus on labor market conditions during the start of the information revolution in the early 1990s. Moreover, the baseline survey months are fixed to preclude the possibility of occupation-specific seasonality. The reduced-form estimates are reported in Table S3 and are qualitatively similar to those in Table 5. In Figure S3, I also show that employment growth from 1989 to 1993 is unrelated to the level of computer use at work in 1993 across occupations, corroborating the placebo results.

Exploration of contemporaneous occupation-specific trends in employment outcomes. Even in the absence of preexisting trends, labor market conditions may have evolved differently by occupational pre-internet computer usage during the analysis period.

[16] I use the STATA command `weakivtest` by Pflueger and Wang (2015) to calculate the first-stage *F*-statistic, which is referred to as the effective *F*-statistic in Montiel Olea and Pflueger (2013).

[17] The IV estimates for follow-up months 9–11 and 13–15 in Figure S2 may suffer from a weak instrument problem and need to be interpreted with caution. In particular, $C93_k \times \lambda_t$ becomes a weak instrument with the first-stage *F*-statistic ranging from 8.65 to 19.92 in estimation samples with fewer observations, where employment status in month 9, 10, 11, 13, 14, or 15 is not missing.

[18] The coefficient estimates are similar in all columns of Table S2 because observations contributing to the regressions in columns (2)–(4) are subsets of observations used for the regression in column (1).

[19] In panel B using $C_k^{93} \times t$, the first-stage estimate of 0.020 suggests that unemployed individuals in an occupation with a 10 percentage point higher pre-internet computer use rate experienced a 0.2 percentage point additional increase in IJS rate each year.

[20] See Table A1 and Section S3 in the online appendix for further details.

[21] The placebo sample also covers the period of rapid growth in computer use at work and skill-biased technological change in the 1980s (Krueger, 1993; Autor et al., 1998).

Table 4
Reduced-Form Estimates on Employment Probability.

Dependent variable:	Employed within	Employed in		
	15 months	month 1	month 2	month 12
	(1)	(2)	(3)	(4)
A. IV: $C93_k \times \lambda_t$				
$C93 \times 1[t = 1998]$	0.120***	0.081*	0.106**	0.163**
	(0.040)	(0.042)	(0.054)	(0.074)
$C93 \times 1[t = 2000]$	0.097**	0.085**	0.107**	0.075
	(0.041)	(0.042)	(0.048)	(0.064)
$C93 \times 1[t = 2001]$	0.072*	0.058*	0.074	0.017
	(0.037)	(0.032)	(0.047)	(0.054)
$C93 \times 1[t = 2003]$	0.048	0.077**	0.023	0.043
	(0.033)	(0.030)	(0.040)	(0.054)
$C93 \times 1[t = 2011]$	0.120***	0.100***	0.087*	0.022
	(0.042)	(0.030)	(0.046)	(0.059)
F-statistic for joint significance test	2.881	2.647	1.873	1.156
	[0.015]	[0.023]	[0.099]	[0.331]
Adjusted R^2	0.079	0.081	0.083	0.100
B. IV: $C93_k \times t$				
$C93 \times t$	0.005**	0.005***	0.004	0.000
	(0.002)	(0.002)	(0.002)	(0.003)
Adjusted R^2	0.079	0.081	0.083	0.100
N	15,655	13,613	8,679	6,301
Mean of Y	0.514	0.265	0.344	0.524

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. *p*-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
Placebo Exercise: Reduced-Form Estimates on Employment Probability, Using CPS 1983–1992.

Dependent variable:	Employed within	Employed in		
	15 months	month 1	month 2	month 12
	(1)	(2)	(3)	(4)
A. IV: $C93_k \times \lambda_t$				
$C93 \times 1[t = 1989]$	0.041	0.031	0.040	-0.036
	(0.031)	(0.035)	(0.046)	(0.051)
$C93 \times 1[t = 1991]$	0.012	0.026	0.039	-0.093**
	(0.026)	(0.033)	(0.036)	(0.039)
$C93 \times 1[t = 1992]$	-0.031	-0.053	-0.030	-0.059
	(0.027)	(0.036)	(0.043)	(0.039)
F-statistic for joint significance test	2.157	2.802	1.564	2.010
	[0.095]	[0.041]	[0.200]	[0.114]
Adjusted R^2	0.067	0.053	0.062	0.091
B. IV: $C93_k \times t$				
$C93 \times t$	-0.001	-0.002	-0.000	-0.008**
	(0.003)	(0.004)	(0.004)	(0.004)
Adjusted R^2	0.067	0.053	0.062	0.092
N	14,739	12,719	8,108	5,729
Mean of Y	0.496	0.253	0.330	0.510

Notes. Data are from the September 1983, December 1989, August 1991, and September 1992 Basic Monthly CPS, matched with their subsequent monthly CPS files. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. *p*-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For example, the change in labor demand may have been more dramatic in more computer-intensive occupations during the dot-com bubble and burst between the mid-1990s and early 2000s. On the supply side, workers in occupations with a higher pre-internet computer usage may have become more positively selected in terms of skill levels and thus more employable over time due to enhanced technology-skill complementarity. In these situations, my IV strategy may overstate the positive effect of IJS on the job-finding rate.

To address this concern, I first compare the estimated coefficients on IJS across specifications, with and without covariates capturing concurrent labor market conditions or individual char-

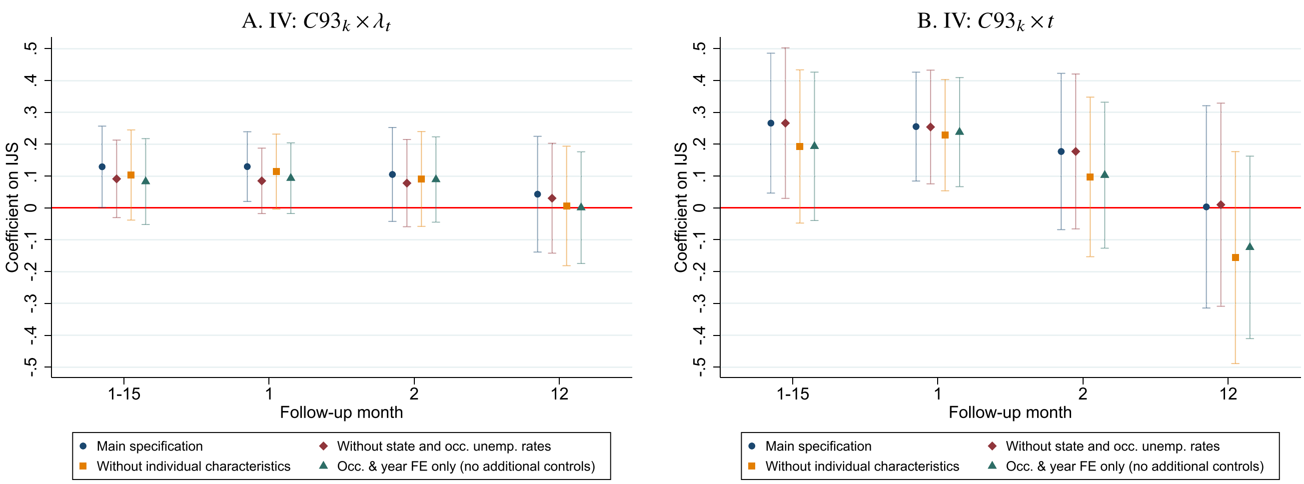

Fig. 3. IV Estimates of Internet Job Search Effects on Employment Probability, from Various Specifications.
Notes. Each bullet shape represents the coefficient estimate on *IJS* from a separate IV regression estimating Eq. (1). Vertical spikes around each point estimate represent the 95% confidence interval, clustered at the occupation-by-year level. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. Besides occupation and year fixed effects, the main specification controls for unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state.

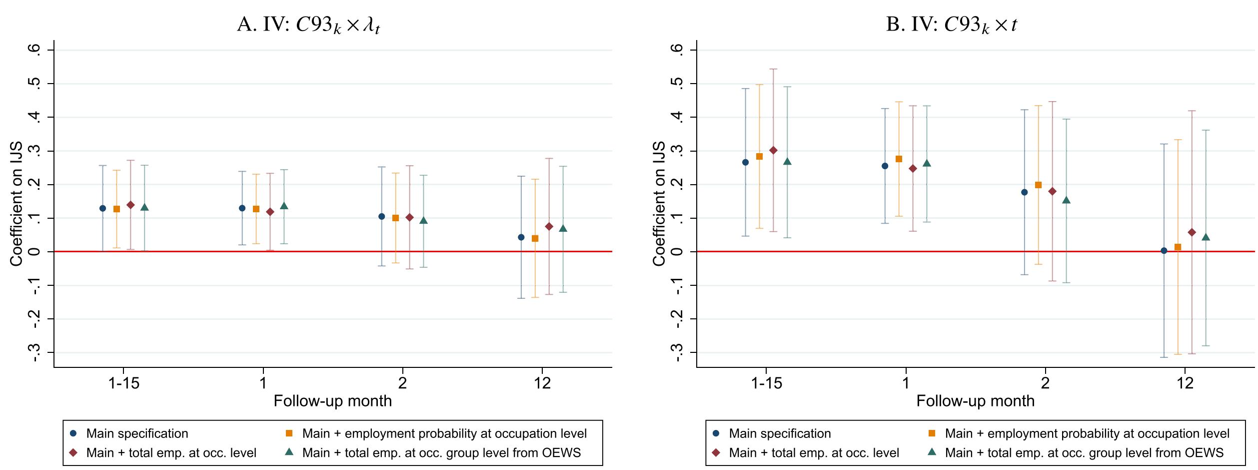

Fig. 4. IV Estimates of Internet Job Search Effects on Employment Probability, Controlling for Occupational Employment.
Notes. Each bullet shape represents the coefficient estimate on *IJS* from a separate IV regression estimating Eq. (1). Vertical spikes around each point estimate represent the 95% confidence interval, clustered at the occupation-by-year level. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. Besides occupation and year fixed effects, the main specification controls for unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. The total employment at the occupation group level is from the 1991–1993, 1998, 2000, 2001, May 2003, and May 2011 OEWS.

acteristics. Figure 3 shows that the IV estimates remain stable when state and occupational unemployment rates are dropped from the regression. Without individual covariates, such as demographic characteristics, socioeconomic status, and worker characteristics, the IV estimates change little (in panel A using $C93_k \times \lambda_t$) or tend to be smaller (in panel B using $C93_k \times t$), excluding the possibility of accelerated positive selection of workers in more computer-intensive occupations.

In Fig. 4, I provide further evidence that the instrument is unconfounded with within-occupation changes in labor demand by showing that the IV estimates are robust to additionally controlling for occupational employment. Given the lack of job vacancy data by occupation for the 1990s,[22] I use data from the CPS and Occupational Employment and Wage Statistics (OEWS) to compute employment probability and total employment at the occupation

[22] For example, the BLS's Job Openings and Labor Turnover Survey (JOLTS) began in 2000 and has been the official source of job openings. The JOLTS data do not provide an occupation breakdown of jobs. The proprietary job postings data constructed by Emsi Burning Glass are available from 2001 onward. The Conference Board has been producing the Help Wanted Online (HWOL) data since 2005.

Table 6
Reduced-Form Estimates on the Probability of Finding a Job in the Same Occupation.

Dependent variable:	Finding a job in the same occ. within 15 months	
	Main sample (1992–2011) (1)	Placebo sample (1983–1992) (2)
A. IV: $C93_k \times \lambda_t$		
$C93 \times 1[t = 1998]$	0.032	
	(0.051)	
$C93 \times 1[t = 2000]$	-0.067	
	(0.059)	
$C93 \times 1[t = 2001]$	-0.064	
	(0.051)	
$C93 \times 1[t = 2003]$	-0.046	
	(0.057)	
$C93 \times 1[t = 2011]$	0.032	
	(0.059)	
$C93 \times 1[t = 1989]$		-0.063
		(0.057)
$C93 \times 1[t = 1991]$		0.009
		(0.061)
$C93 \times 1[t = 1992]$		-0.027
		(0.054)
F-statistic for joint significance test	1.472	0.678
	[0.199]	[0.567]
Adjusted R^2	0.135	0.125
B. IV: $C93_k \times t$		
$C93 \times t$	0.000	-0.002
	(0.003)	(0.006)
Adjusted R^2	0.135	0.125
N	7,552	6,520
Mean of Y	0.393	0.409

Notes. In column (1), data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. In column (2), data are from the September 1983, December 1989, August 1991, and September 1992 Basic Monthly CPS, matched with their subsequent monthly CPS files. In both columns (1) and (2), the estimation is conducted on those who have found a job during the 15-month follow-up period and whose occupation is not missing both in month 0 and after becoming employed. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on lay-off, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. p-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

level as a proxy for occupational labor market tightness.[23,24] The estimated coefficient on IJS is insensitive to adding one of the occupational employment measures in the IV regression.[25,26]

Lastly, I show that pre-internet computer use intensity is unrelated to over-time changes in the probability of finding a job within the same occupation. One concern motivating this exercise is that for those who worked in an occupation with higher exposure to computers, it may have become easier to find a job by switching to another occupation. If easier job switching and, consequently, a higher job-finding rate are attributable to increased demand for computer-accustomed workers rather than to IJS growth, this is a possible scenario in which the instrument might directly affect the employment outcome. Table 6 reports the estimated reduced-form effects on the probability of finding a job within the same detailed occupational category for the analysis period (1992–2011) in column (1), in comparison with the estimates for the placebo period (1983–1992) in column (2).[27] In both columns, the coefficients on the instruments are statistically indistinguishable from zero at any conventional level of significance for both IV specifications, $C93_k \times \lambda_t$ and $C93_k \times t$.[28] This provides another piece of evidence against violation of the exclusion restriction during the analysis period.

[23] Both CPS and OEWS have their own caveats in counting the number of jobs. As the CPS is "person-based" and focuses on the main job of respondents, many of the secondary jobs are not considered. The OEWS is "job-based" but does not cover unincorporated self-employed, agriculture, or private household jobs. See Abraham and Spletzer (2010) for a comparison of occupational employment between CPS and OEWS.

[24] It is very difficult to harmonize the occupation codes used by OEWS and CPS/Census, and no crosswalk is available especially for the 1990s. To reduce measurement errors from manual reclassification, I convert the OEWS occupation codes to 14 broad occupation groups instead of 45 detailed occupation categories in the 1990 Census occupational classification system. Section S4 in the online appendix provides further details on how occupational employment is constructed using the OEWS data.

[25] Controlling for occupational employment in logs instead of levels also gives similar IV estimates.

[26] The missing values of the occupational employment measures among veterans or the never-worked are imputed with the sample averages across all occupations.

[27] The estimation is conducted on unemployed workers 1) who have found a job during the 15-month follow-up period and 2) whose occupation is not missing both in month 0 and after becoming employed.

[28] This pattern is confirmed by an occupation-level analysis presented in Figure S4.

The results of the aforementioned internal validity checks suggest that the positive IJS effect estimates from the proposed IV method are unlikely to be driven by differential changes in labor market conditions across occupations.

4.3. Robustness checks

In this section, I present IV estimates from alternative specifications for the instrument and from interval regressions that consider censored unemployment spells. I also discuss whether and how the results change when the pre-internet computer use rate at work among the never-worked is handled differently instead of being imputed with zero.

IV specifications. Table A3 compares IV estimates from alternative IV specifications: $C93_k \times t$, $C93_k \times f(t)$, and $C93_k \times \lambda_t$. When I impose a functional form assumption on the time component of the instrument using a linear, quadratic, or cubic time trend, IV estimates remain qualitatively similar but become larger in magnitude compared to the preferred specification using a set of time dummies. In most cases, the preferred specification, $C93_k \times \lambda_t$, provides the most conservative IJS effect estimates, and the linear specification, $C93_k \times t$, returns the largest estimates.

Censored regression. For the main analysis, I have estimated the linear probability model in Eq. (1) instead of a duration model because dealing with endogenous regressors is easier when both the outcome (i.e., employment status) and treatment (i.e., IJS status) processes are linear. When the outcome and treatment variables originate from nonlinear and nonnormal processes, particularly from different statistical families, tractable multivariate distributions for a joint model of outcome and treatment often do not exist (Cameron and Trivedi, 2005). Duration outcomes with binomial treatment are a typical example with no analytic joint distribution.

Nevertheless, the linear probability analysis of employment status observed within a certain period or in a specific follow-up month has limitations. Notably, the linear probability model used in the main analysis does not capture that unemployment spells are censored in the CPS: 1) unemployment may have continued after individuals stopped being followed up by the CPS, and 2) individuals may have been employed during the 8-month gap between the two 4-month CPS survey windows. To incorporate these features from the CPS survey structure, I examine the timing of employment using a censored regression model in Table 7. Column (1) shows that online job seekers stay unemployed slightly longer than their observably similar counterparts who do not search online, suggesting negative selection into IJS on unobservables. In columns (2) and (3), the outcome and first-stage equations are jointly estimated using the maximum likelihood estimation to address selection into IJS, following the literature on limited dependent variable models with endogenous regressors (e.g., Angrist, 2001). Results suggest that IJS accelerated the timing of employment by 0.6–1.3 months (13.7–32.3% relative to the mean of 4.7 months until employment), which is consistent with the main findings from the IV analysis using the linear probability model.

Alternative treatments of the never-worked. For those who never had a job, the computer use rate at work in 1993, and thus the instrument, is missing and imputed with zero in the main analysis. Table 8 presents the IV estimates of the IJS effect on the 15-month job-finding rate using alternative imputed values of $C93$ for the never-worked or from an alternative estimation sample excluding the never-worked. The reduced-form estimates are reported in Table A4. In columns (2)–(4), I impute $C93$ with 0.053, 0.108, and 0.175, respectively, instead of zero, for those who never had a job. These values are the pre-internet computer use rate at work in occupations 31, 29, and 36, which display over-time trajectories of the IJS rate close to the never-worked group (see Fig. A1). The IV and reduced-form estimates remain similar when the nonzero values are used for imputation. In column (5), the restricted sample without the never-worked tends to yield smaller IV and reduced-form estimates, suggesting that IJS is more effective for those who never had a job before. A possible explanation is that IJS may be more beneficial to job seekers who do not have career networks and thus have limited access to insider information on job opportunities (Cumming et al., 2022).

Table 7
Censored Regression Estimates of Internet Job Search Effects on Unemployment Duration since Month 0.

	Censored reg.	Censored reg. with IV	
		$C93_k \times \lambda_t$	$C93_k \times t$
	(1)	(2)	(3)
IJS	0.074**	-0.646**	-1.329***
	(0.037)	(0.254)	(0.436)
Log likelihood	-13,542	-19,565	-19,668
N	15,655	15,655	15,655
Mean of *Y*	4.682	4.682	4.682
Median of *Y*	2.0	2.0	2.0

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5. Discussion

This section discusses the underlying mechanisms and implications of the findings in relation to the existing literature.

Theoretical framework. Online job search and recruitment tools improve match efficiency and reduce search costs by facilitating information exchanges between workers and firms.[29] Specifically, online job boards make learning about and applying for jobs easier and cheaper. Internet technology also enhances a firm's ability to announce vacancies and reach job candidates.

Labor market search theory suggests that greater search efficiency and lower search costs can have both positive and negative effects on the employment probability among job seekers (e.g., Mortensen and Pissarides, 1999; Pissarides, 2000). More potential matches between firms and workers are expected to create more hires and increase job-finding rates among the unemployed.[30] However, workers may become more selective regarding the quality of matches when considering more, faster, and cheaper potential matches. The consequent increase in reservation wages will then lower the job-finding rate.

Concerning the strength of the two competing effects, Gürtzgen et al., 2021 predict that internet expansion would most likely raise the job-finding rate, referring to

[29] Autor (2001), Bhuller et al. (2021), and Gürtzgen et al., 2021 provide an overview of the expected impacts of internet expansion on various labor market outcomes.

[30] In addition, Bhuller et al. (2021) point out that reduced recruitment costs are equivalent to enhanced productivity for firms and would increase the number of job vacancies per job seeker, leading to a higher job-finding rate.

Table 8
IV Estimates of Internet Job Search Effects on Employment Probability, by Alternative Treatments of the Never-Worked.

Dependent variable:	Employed within 15 months				
	Main sample (1)	Impute with $C93 = 0.053$ (2)	Impute with $C93 = 0.108$ (3)	Impute with $C93 = 0.175$ (4)	Without the never-worked (5)
A. IV: $C93_k \times \lambda_t$					
IJS	0.129**	0.119*	0.108*	0.093	0.056
	(0.065)	(0.062)	(0.060)	(0.057)	(0.054)
First-stage *F*-statistic	37.07	37.06	36.73	36.26	35.07
B. IV: $C93_k \times t$					
IJS	0.266**	0.254**	0.242**	0.226**	0.184*
	(0.112)	(0.110)	(0.109)	(0.107)	(0.108)
First-stage *F*-statistic	39.93	41.24	42.10	42.59	42.72
N	15,655	15,655	15,655	15,655	14,534
Mean of *Y*	0.514	0.514	0.514	0.514	0.522

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. *C*93 denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Column (1) reproduces the main IV estimates in Table 3. In columns (2)–(4), *C*93 for those without work experience is imputed with the value of *C*93 in occupation categories 31, 29, and 36, respectively. In column (5), the estimation sample excludes individuals who never worked before the current unemployment spell. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. The first-stage *F*-statistics are computed by implementing the weak instrument test by Montiel Olea and Pflueger (2013). Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Van den Berg (1994)'s theoretical work. Regarding the impact of increased job offer arrival rates on the employment probability, Van den Berg (1994) shows that the positive effect from increased employment opportunities dominates the negative effect through increased reservation wages and prolonged job search for virtually all wage offer distributions used in the literature.

Interpretation of IV results. The IV results in Section 4.1 indicate that IJS substantially improved search effectiveness among unemployed individuals in the US labor market during the internet expansion from the mid-1990s to the early 2010s. This is in line with recent empirical evidence from Germany and Norway that access to broadband internet generated more employment among job seekers and reduced search frictions in the labor market (Bhuller et al., 2021; Denzer et al., 2021; Gürtzgen et al., 2021).

My IV estimates cannot be directly compared to the estimates from these studies since they focus on the effect of internet availability on employment prospects and provide only suggestive evidence of the effectiveness of online job search. Nevertheless, I can conduct a back-of-the-envelope calculation using the estimates from Bhuller et al. (2021), who measure the effects of internet access on the probability of finding a job within 2 years (0.016) and on the likelihood of using the internet to browse job advertisements (0.088). An IV estimate of 0.182 (= 0.016/0.088) can be obtained when the former is regarded as the reduced-form estimate and the latter as the first-stage estimate, treating broadband availability as the instrument.[31] Despite the difference in the population of interest and variation exploited, the back-of-the-envelope calculation result is within the range of my IV estimates between 0.129 and 0.266 for the 15-month job-finding rate in Table 3.

Bias in OLS estimates. A comparison between the OLS and IV results reveals that the OLS estimates are downward biased, suggesting negative selection into IJS on unobserved characteristics.[32] The following are two possible reasons for negative selection. First, the drastically reduced cost of job searches on the internet may have widened the pool of job seekers to nonserious or less qualified applicants (Autor, 2001). Because reading online ads, searching for job listings on the web, or posting a resume on online job boards is almost free, even individuals with low expected benefits (i.e., low employment chances) may become willing to engage in such online job search activities.

Second, online job seekers may have poor social networks and informal contacts compared to their observably similar counterparts who only search offline. Specifically, job seekers with a weaker social network mainly resort to formal search channels, and online job search had remained disproportionately formal until 2008 (Gürtzgen et al., 2021; Kuhn and Mansour, 2014).[33] If the benefits of informal channels are concentrated in early unemployment stages and depleted relatively quickly (Van den Berg and Van der Klaauw, 2006), the downward bias in OLS estimates would be especially severe at the beginning of the 15-month follow-up period. Indeed, the OLS estimates for month 9 or later are not only more distinctively positive and higher but also closer to the benchmark IV estimate (i.e., 0.129 from $C93_k \times \lambda_t$ and 0.266 from $C93_k \times t$), compared to the OLS estimates for months 1 and 2 (see Table 2 panel A, Table 3, and Figure S2).

IJS effects over time and across subgroups. Given the dramatic increase in IJS from the mid-1990s to the early 2010s, the effectiveness and selection pattern of IJS may have changed over time. In Table 9, I explore this possibility by examining whether the IV and OLS estimates of the IJS effects in more recent years (2011) differ from the early expansion period (1998–2003). Although the IV estimates tend to be larger in 2011 than in 1998–2003, the IJS effects in the two periods are not statistically distinguishable.[34] In addition, the OLS estimates are similar across years and remain

[31] This estimate implies a 27.6% increase relative to the average employment probability of 0.659 in the Norwegian sample analyzed by Bhuller et al. (2021).

[32] Gürtzgen et al., 2021 point out that male and young online job seekers especially appear to be negatively selected.

[33] According to Kuhn and Mansour (2014)'s calculation using the 2008 wave of the National Longitudinal Survey of Youth 1997 (NLSY97), among the 12 active and passive job search methods, "sending out resumes or applications" and "looking at advertisement" were the most internet-intensive, while "contacting friends or relatives" was the least internet-intensive.

[34] As shown in Figure S5, the 95% confidence interval for the IJS effect in 2011 overlaps with that in 1998–2003 for all the follow-up months.

Table 9
Heterogeneous Effects of Internet Job Search on Employment Probability, by Year Group.

Dependent variable:	Employed within	Employed in		
	15 months	month 1	month 2	month 12
	(1)	(2)	(3)	(4)
A. IV: $C93_k \times \lambda_t$				
$IJS \times 1[t = 1998, 2000, 2001, 2003]$	0.123*	0.126**	0.106	0.044
	(0.065)	(0.055)	(0.075)	(0.093)
$IJS \times 1[t = 2011]$	0.296**	0.260***	0.209	-0.032
	(0.127)	(0.095)	(0.143)	(0.170)
χ^2-statistic testing *IJS* effects	2.484	2.993	0.756	0.300
identical across year groups	[0.115]	[0.084]	[0.385]	[0.584]
B. IV: $C93_k \times t$				
$IJS \times 1[t = 1998, 2000, 2001, 2003]$	0.185**	0.171***	0.156**	0.166
	(0.072)	(0.063)	(0.079)	(0.117)
$IJS \times 1[t = 2011]$	0.352***	0.300***	0.260*	0.078
	(0.130)	(0.098)	(0.146)	(0.179)
χ^2-statistic testing *IJS* effects	2.254	2.671	0.730	0.385
identical across year groups	[0.133]	[0.102]	[0.393]	[0.535]
C. OLS				
$IJS \times 1[t = 1998, 2000, 2001, 2003]$	0.020*	-0.019*	-0.007	0.051***
	(0.012)	(0.011)	(0.017)	(0.018)
$IJS \times 1[t = 2011]$	0.010	-0.032**	-0.036	0.069***
	(0.018)	(0.015)	(0.026)	(0.023)
F-statistic testing *IJS* effects	0.255	0.599	0.922	0.353
identical across year groups	[0.614]	[0.440]	[0.338]	[0.553]
Adjusted R^2	0.079	0.081	0.083	0.101
N	15,655	13,613	8,679	6,301
Mean of *Y*	0.514	0.265	0.344	0.524

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. *C93* denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. *p*-values in brackets. * $p < .10$, ** $p < .05$, *** $p < .01$

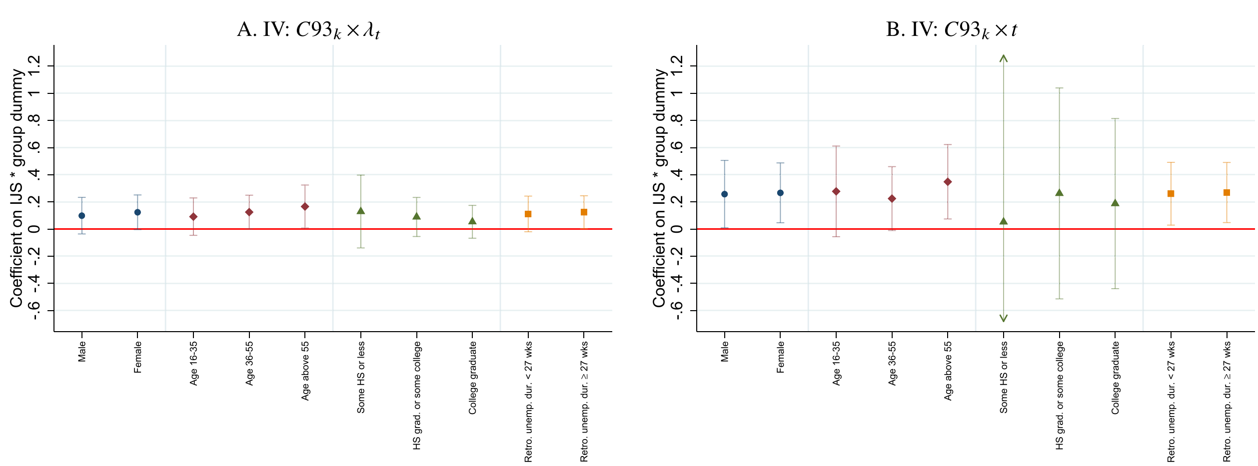

Fig. 5. Heterogeneous Effects of Internet Job Search on Employment Probability, by Various Subgroups.
Notes. A set of the same bullet shapes represent the coefficient estimates on the interaction terms between *IJS* and group dummies from an IV regression estimating Eq. (3). Different types of bullet shapes are from separate regressions. Vertical spikes around each point estimate represent the 95% confidence interval, clustered at the occupation-by-year level. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. *C93* denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. In panel B, the 95% confidence interval of the IJS effect for those with some high school education or less is [-2.181, 2.287], which is beyond the range of the horizontal axis.

substantially smaller than the IV estimates. This suggests no significant change in the selection pattern. Therefore, IJS has constantly been effective in securing jobs for one and a half decades since the introduction of the internet.

Additionally, I examine whether the benefits of IJS are heterogeneous by demographic characteristics, education level, or retrospective unemployment duration. Fig. 5 presents estimated coefficients (i.e., β_g's) on the interaction terms between the IJS status and subgroup dummies in the following regression specification:

$$y_i = \alpha + \sum_g \beta_g IJS_i \times 1[G_i = g] + \mathbf{X}_i'\boldsymbol{\gamma} + \delta_k + \lambda_t + \varepsilon_i, \tag{3}$$

where $1[G_i = g]$ indicates whether person i belongs to group g. For example, I include two interaction terms between the IJS status and each sex dummy in the regression to allow IJS effects to differ between men and women. I find no evidence of heterogeneous IJS effects by sex, age, education, or long-term unemployment status.[35,36] Although less precisely estimated, subgroup-specific IJS effects are generally similar in magnitude to the average effect for all unemployed workers in Table 3. Also, I cannot reject the null hypothesis that the IJS effects are identical across subgroups at the 10% significance level.

6. Conclusion

This study investigates whether using the internet for job search helps unemployed workers find jobs. To address the self-selection of online job seekers, I employ an IV strategy based on the fact that IJS was adopted more rapidly among individuals with occupations that had greater exposure to computers at work prior to the emergence of the internet. The results of my analysis of unemployed workers from the 1992/09, 1998/12, 2000/08, 2001/09, 2003/10, and 2011/07 CPS indicate that online job seekers are substantially more likely to be employed during the 15-month follow-up period than those who search offline only. Therefore, IJS effectively raised job-finding rates among the unemployed during the internet expansion from the mid-1990s to the early 2010s. The IV results also suggest that, between the two competing channels, the positive effect of improved search efficiency dominates the negative effect of increased reservation match quality and prolonged job search. The effectiveness of IJS remained stable throughout the analysis period.

The following are two limitations of this study. First, the IV analysis of individual outcomes identifies the partial equilibrium effect of IJS on the job-finding rate. To address the general equilibrium question of whether improved employment prospects for online job seekers lead to a reduction in aggregate unemployment, search externalities on non-internet searchers should also be considered at the labor market level, which is beyond the scope of this paper. Second, this study focuses on (re-)employment outcomes among the unemployed mainly due to data limitations. Whether the internet advantage can be extended to other outcomes (e.g., wages) or different groups of workers (e.g., the employed) remains an important subject for future research.

Declaration of competing interest

I acknowledge research funding from the Korea Sanhak Foundation and Hanyang University in Seoul, South Korea. The funding source had no involvement in the conduct of the research or preparation of the manuscript. I have no competing financial interests or personal relationships that have influenced the work reported in this paper. IRB approval was not needed for the project, as all data are non-identifiable.

CRediT authorship contribution statement

Eleanor Jawon Choi: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

Data availability

The data used in my analyses are from the Current Population Survey (CPS) and the Occupational Employment and Wage Statistics (OEWS) in the United States. The CPS data are publicly available from the National Bureau of Economic Research: https://www.nber.org/research/data/current-population-survey-cps-data-nber. The IPUMS CPS harmonizes a subset of variables in the raw CPS files to facilitate cross-time comparisons and provides the integrated datasets at https://cps.ipums.org/cps/. The OEWS data are publicly available from the Bureau of Labor Statistics: https://www.bls.gov/oes/tables.htm.

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[35] The BLS defines long-term unemployment as being jobless for 27 weeks or longer.

[36] The IJS effects are also similar by race (Black vs. non-Black) or ethnicity (Hispanic vs. non-Hispanic).

Appendix

Table A1
Longitudinally Linked CPS Files.

Month 0	Month 1	Month 2	Month 3	Month 9	Month 10	Month 11	Month 12	Month 13	Month 14	Month 15
1983/09	1983/10	1983/11	1983/12	1984/06	1984/07	1984/08	1984/09	1984/10	1984/11	1984/12
1989/12	1990/01	1990/02	1990/03	1990/09	1990/10	1990/11	1990/12	1991/01	1991/02	1991/03
1991/08	1991/09	1991/10	1991/11	1992/05	1992/06	1992/07	1992/08	1992/09	1992/10	1992/11
1992/09	1992/10	1992/11	1992/12	1993/06	1993/07	1993/08	1993/09	1993/10	1993/11	1993/12
1998/12	1999/01	1999/02	1999/03	1999/09	1999/10	1999/11	1999/12	2000/01	2000/02	2000/03
2000/08	2000/09	2000/10	2000/11	2001/05	2001/06	2001/07	2001/08	2001/09	2001/10	2001/11
2001/09	2001/10	2001/11	2001/12	2002/06	2002/07	2002/08	2002/09	2002/10	2002/11	2002/12
2003/10	2003/11	2003/12	2004/01	2004/07	2004/08	2004/09	2004/10	2004/11	2004/12	2005/01
2011/07	2011/08	2011/09	2011/10	2012/04	2012/05	2012/06	2012/07	2012/08	2012/09	2012/10
2013/07	2013/08	2013/09	2013/10	2014/04	2014/05	2014/06	2014/07	2014/08	2014/09	2014/10
2015/07	2015/08	2015/09	2015/10	2016/04	2016/05	2016/06	2016/07	2016/08	2016/09	2016/10

Used for main analysis
Used for placebo exercise
Added for construction of extended sample

Table A2
Pre-Internet Computer Use at Work, by Occupation.

Category Number	CPS detailed occupation	Proportion using computers at work (*C*93)
1	Administrators and officials, public administration	0.862
2	Other executive, administrators, and managers	0.657
3	Management related occupations	0.872
4	Engineers	0.895
5	Mathematical and computer scientists	0.984
6	Natural scientists	0.873
7	Health diagnosing occupations	0.490
8	Health assessment and treating occupations	0.596
9	Teachers, college and university	0.725
10	Teachers, except college and university	0.490
11	Lawyers and judges	0.646
12	Other professional specialty occupations	0.581
13	Health technologists and technicians	0.554
14	Engineering and science technicians	0.691
15	Technicians, except health, engineering, and science	0.897
16	Supervisors and proprietors, sales occupations	0.571
17	Sales representatives, finance, and business service	0.732
18	Sales representatives, commodities, except retail	0.560
19	Sales workers, retail and personal services	0.298
20	Sales related occupations	0.231
21	Supervisors - administrative support	0.868
22	Computer equipment operators	0.965
23	Secretaries, stenographers, and typists	0.862
24	Financial records, processing occupations	0.801
25	Mail and message distributing	0.259
26	Other administrative support occupations, including clerical	0.744
27	Private household service occupations	0.006
28	Protective service occupations	0.427
29	Food service occupations	0.108
30	Health service occupations	0.175
31	Cleaning and building service occupations	0.053
32	Personal service occupations	0.102
33	Mechanics and repairers	0.304
34	Construction trades	0.079
35	Other precision production occupations	0.347
36	Machine operators and tenders, except precision	0.175
37	Fabricators, assemblers, inspectors, and samplers	0.175
38	Motor vehicle operators	0.121
39	Other transportation occupations and material moving	0.143
40	Construction laborer	0.036
41	Freight, stock and material handlers	0.158
42	Other handlers, equipment cleaners, and laborers	0.126
43	Farm operators and managers	0.137
44	Farm workers and related occupations	0.059
45	Forestry and fishing occupations	0.029
46	Armed forces last job, currently unemployed	0.452
N/A	Never worked	0.000

Notes. Data are from the October 1993 CPS School Enrollment Supplement. The detailed occupations based on the 1990 Census occupational classification are used. For those who served in the armed forces just before becoming unemployed, the computer use rate at work is imputed with the average across all 45 occupations (i.e., 0.452). For those who never had a job, the computer use rate at work is set to zero.

Table A3
IV Estimates of Internet Job Search Effects on Employment Probability, by Various IV Specifications.

Dependent variable:	Employed within	Employed in		
	15 months (1)	month 1 (2)	month 2 (3)	month 12 (4)
A. Linear time trend: $C93_k \times t$				
IJS	0.266**	0.255***	0.177	0.003
	(0.112)	(0.087)	(0.125)	(0.162)
First-stage F-statistic	39.93	35.60	27.67	32.61
B. Quadratic time trend: $C93_k \times f(t)$				
IJS	0.160**	0.163***	0.126*	0.110
	(0.069)	(0.059)	(0.076)	(0.105)
First-stage F-statistic	67.65	62.60	66.16	46.82
C. Cubic time trend: $C93_k \times f(t)$				
IJS	0.148**	0.158***	0.116	0.068
	(0.068)	(0.059)	(0.076)	(0.100)
First-stage F-statistic	53.61	49.67	47.21	37.58
D. Time dummies: $C93_k \times \lambda_t$				
IJS	0.129**	0.130**	0.105	0.043
	(0.065)	(0.056)	(0.075)	(0.093)
First-stage F-statistic	37.07	32.62	32.86	28.12
N	15,655	13,613	8,679	6,301
Mean of Y	0.514	0.265	0.344	0.524

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. The first-stage F-statistics are computed by implementing the weak instrument test by Montiel Olea and Pflueger (2013). Robust standard errors in parentheses are clustered at the occupation-by-year level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4
Reduced-Form Estimates on Employment Probability, by Alternative Treatments of the Never-Worked.

Dependent variable:	Employed within 15 months				
	Main sample (1)	Impute with $C93 = 0.053$ (2)	Impute with $C93 = 0.108$ (3)	Impute with $C93 = 0.175$ (4)	Without the never-worked (5)
A. IV: $C93_k \times \lambda_t$					
$C93 \times 1[t = 1998]$	0.120***	0.118***	0.116***	0.112***	0.096**
	(0.040)	(0.040)	(0.040)	(0.040)	(0.040)
$C93 \times 1[t = 2000]$	0.097**	0.092**	0.086**	0.077*	0.048
	(0.041)	(0.041)	(0.041)	(0.040)	(0.037)
$C93 \times 1[t = 2001]$	0.072*	0.067*	0.061*	0.053	0.028
	(0.037)	(0.037)	(0.036)	(0.035)	(0.033)
$C93 \times 1[t = 2003]$	0.048	0.045	0.040	0.033	0.016
	(0.033)	(0.033)	(0.032)	(0.032)	(0.030)
$C93 \times 1[t = 2011]$	0.120***	0.118***	0.114***	0.109***	0.092**
	(0.042)	(0.042)	(0.042)	(0.042)	(0.042)
F-statistic for joint	2.881	2.743	2.609	2.436	1.849
significance test	[0.015]	[0.019]	[0.025]	[0.035]	[0.103]
Adjusted R^2	0.079	0.079	0.079	0.079	0.081
B. IV: $C93_k \times t$					
$C93 \times t$	0.005**	0.005**	0.005**	0.005**	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Adjusted R^2	0.079	0.079	0.079	0.079	0.081
N	15,655	15,655	15,655	15,655	14,534
Mean of Y	0.514	0.514	0.514	0.514	0.522

Notes. Data are from the September 1992 Basic Monthly CPS and the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements, matched with their subsequent monthly CPS files. $C93$ denotes the 1993 computer use rate at work in each occupation, calculated using data from the October 1993 CPS School Enrollment Supplement. Column (1) reproduces the reduced-form estimates in Table 4. In columns (2)–(4), $C93$ for those without work experience is imputed with the value of $C93$ in occupation categories 31, 29, and 36, respectively. In column (5), the estimation sample excludes individuals who never worked before the current unemployment spell. Besides occupation and year fixed effects, control variables include unemployment duration (in weeks), state and occupational unemployment rates, and dummies for age (16–25, 26–35, 36–45, 46–55), education (some high school, high school graduate, some college, college graduate), Black, Hispanic, male, married, married male, spousal employment, home ownership, job quitters, on layoff, status before unemployment (worked, attended school), sector (private, public, self-employed), industry, and state. Robust standard errors in parentheses are clustered at the occupation-by-year level. p-values in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

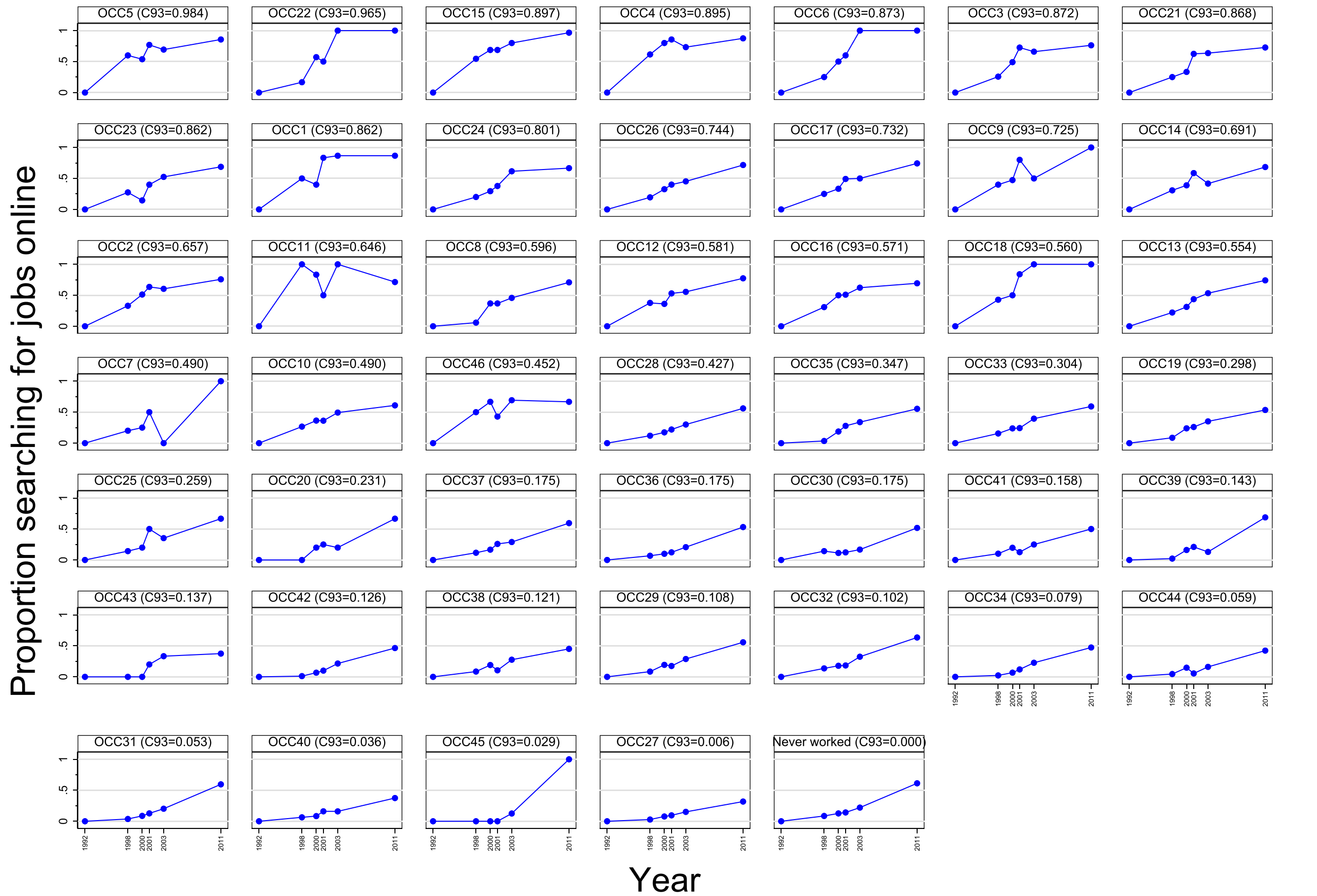

Fig. A1. Proportion of Unemployed Workers Searching for Jobs Online, by Occupation (All Occupations).
Notes. Online job search status is from the December 1998, August 2000, September 2001, October 2003, and July 2011 CPS Computer and Internet Use Supplements. *C*93 denotes the 1993 computer use rate at work in each occupation from the October 1993 CPS School Enrollment Supplement. Occupation codes follow the 1990 Census occupational classification.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.infoecopol.2023.101017

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