Internet Job Search and Unemployment Durations

By Peter Kuhn and Mikal Skuterud*

Using the December 1998 and August 2000 CPS Computer and Internet Supplements matched with subsequent CPS files, we ask which types of unemployed workers looked for work on line and whether Internet searchers became reemployed more quickly. In our data, Internet searchers have observed characteristics that are typically associated with shorter unemployment spells, and do spend less time unemployed. This unemployment differential is however eliminated and in some cases reversed when we hold observable characteristics constant. We conclude that either Internet job search is ineffective in reducing unemployment durations, or Internet job searchers are negatively selected on unobservables. (*JEL* J64, C41)

"*Using CareerBuilder® to find a job is like driving in the carpool lane.*"
—Half-page ad for an Internet job site in the *Los Angeles Times,* Friday, March 1, 2002 (p. C5).

"*Think Monster for the best resumes, the best candidates.*"
—Monster.com Web site, September 19, 2002.

After decades of stability, the technologies used by workers to locate new jobs began to change rapidly with the diffusion of Internet access in the late 1990's. As early as August 2000, one in four unemployed U.S. job seekers reported that they regularly used the Internet to look for jobs; one in ten employed persons said they regularly looked for other jobs online. The use of Internet job and recruiting sites is generally free of cost for workers and much cheaper for firms than traditional print advertisements. In addition, these services offer firms and workers the promise of instant access to a much larger number of possible matches than traditional channels, as well as the potential for the exchange of much more detailed information about both worker and job attributes.[1]

Not surprisingly, economists have begun to speculate on the potential effects of the above developments on labor markets. For example, commentators have argued that the higher contact rate, lower cost, and greater information content provided by this technology could lead to lower frictional unemployment (Dale T. Mortensen, 2000), higher average match quality (Alan B. Krueger, 2000), a reduction of noncompetitive wage differentials (David H. Autor, 2001), and an amplification of ability-related wage differentials (Kuhn, 2000). If even some of these claims are correct, the advent of Internet job search will have important implications for both labor- and macroeconomic policy.[2]

This article has two main goals. The first is to examine the incidence and diffusion of Internet job search: who looks for work online? Second is to estimate the effect on search outcomes, for an individual worker, of incorporating the Internet into his or her job search strategy. We are

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[1] For example, at firms' request WebHire will check the following worker credentials: Social Security numbers; current and previous addresses; references; education; criminal, civil, and bankruptcy court records; driving and credit reports; and workers' compensation claims. Also offered are online skills and personality testing. The combination of Internet application procedures and traditional database management software also dramatically simplifies the process of searching through submitted resumes for appropriate matches. Finally, workers can now gain much more information about working conditions and job requirements from job boards and company Web sites.

[2] One potentially relevant aspect of labor market policy is the rationale for government-provided job matching services such as the states' Employment Services. Macro policy implications could follow from any change in the natural unemployment rate caused by Internet job search technology.

of course well aware that even if Internet search has private, individual benefits, it does not follow that the equilibrium effects of introducing this technology on unemployment rates, wages, and other outcomes are socially beneficial.[3] However, since in most equilibrium models, some "first-order," or private effects are a necessary condition for *any* general equilibrium effect to occur, the questions posed in this paper seem to be the right ones to ask first.

In order to answer our questions we use measures of Internet search derived from the December 1998 and August 2000 Current Population Survey (CPS) Computer and Internet Use Supplements, matched with job search outcomes from all subsequent CPS files that contain some of the same survey respondents. Throughout our analysis we focus on the search methods and outcomes of unemployed persons only. This is because the regular monthly CPS does not collect data on *non*-Internet job search by employed persons.[4] Thus, for those with jobs, CPS data does not allow one to distinguish Internet job search activity from the decision to look for work in the first place. We also restrict our attention to one particular outcome of the job search process—jobless duration. In part, this is driven by data considerations: in the CPS, job quality (i.e., wage) information is not available for a sufficient sample of job seekers; thus we cannot ascertain from our data whether Internet search produces better job matches.[5] For many policy purposes, however, unemployment durations are the outcome of greatest interest, justifying our focus here.

This paper contributes to an emerging literature on the effects of Internet technology on product market performance (e.g., Jeffrey R. Brown and Austan Goolsbee, 2002, in life insurance markets; Erik Brynjolfsson and Michael D. Smith, 2000, on book and CD markets; and Dennis W. Carlton and Judith A. Chevalier, 2001, on various consumer goods). To our knowledge ours is the only study of the effects of Internet technology on the functioning of the labor market. The current paper also contributes to an older literature on the relative effectiveness of different job search methods. For example, Harry J. Holzer (1987, 1988), Steven M. Bortnick and Michelle H. Ports (1992), Lars Osberg (1993), and John T. Addison and Pedro Portugal (2001) compare the job-finding rates of unemployed workers using a variety of search methods. Jonathan M. Thomas (1997) focuses specifically on the effectiveness of public employment agencies. Finally, our work also relates to a recent literature on the "digital divide," which asks whether differential access to computer or Internet technology aggravates inequality along various dimensions (e.g., Robert W. Fairlie, 2001).

In our data, we find that Internet job searchers are better educated, previously worked in occupations with lower unemployment rates, and had several other characteristics which are usually associated with shorter unemployment durations. Thus it is not surprising that, overall, Internet searchers had shorter unemployment durations than workers who did not use the Internet to locate new jobs. Once observable differences between Internet and other searchers are held constant, however, we find no difference in unemployment durations, and in some specifications even significantly longer durations among Internet searchers.

We conclude that either (a) Internet job search is ineffective in reducing unemployment durations or (b) Internet job searchers are adversely selected on unobservable characteristics; further research is needed to disentangle these two possibilities. In either case, however, Internet search firms who simultaneously claim to employers that their applicants are positively selected (on hard-to-observe characteristics) *and* to their applicants that Internet search will reduce their search time are making claims that are inconsistent with our evidence.

I. Data and Descriptive Statistics

As noted, our data on Internet job search come from the December 1998 and August

[3] For example, Kevin Lang (2000) has suggested an asymmetric-information model in which a reduction in the costs of applying to jobs can be Pareto-worsening, in part by reducing the average match quality in every firm's applicant pool.

[4] See Skuterud (2001) for a recent analysis of trends in on-the-job search using Canadian data, in addition to the occasional CPS surveys that do collect this information.

[5] CPS wage information is of course only available for persons who find new jobs, and who are in the outgoing rotation groups. Further, in our opinion a credible analysis of reemployment wages also requires controls for *pre-unemployment* wages, a restriction which reduces the sample to nonuseful levels.

TABLE 1—FRACTION OF PERSONS WITH INTERNET ACCESS AND ENGAGING IN INTERNET JOB SEARCH, BY LABOR FORCE STATUS, DECEMBER 1998 AND AUGUST 2000

	Fraction with home Internet access		Fraction looking for work online		Fraction looking for work online, given home Internet access[a]	
	1998	2000	1998	2000	1998	2000
Employed						
—at work	0.347	0.521	0.071	0.113	0.159	0.183
—absent	0.339	0.611	0.070	0.105	0.166	0.151
Unemployed						
—on layoff	0.165	0.396	0.048	0.103	0.176	0.207
—job seeker	0.223	0.394	0.150	0.255	0.495	0.541
Not in labor force						
—retired	0.122	0.238	0.003	0.005	0.023	0.021
—disabled	0.105	0.204	0.014	0.022	0.104	0.097
—other	0.319	0.465	0.038	0.063	0.090	0.117
Total	0.294	0.457	0.055	0.089	0.146	0.165

[a] Does not equal the ratio of previous columns because some individuals without home Internet access search online.

2000 Computer and Internet Use Supplements to the CPS. These supplements included the following question: "Do(es) (you) (any one) REGULARLY use the Internet ··· to search for jobs?" As always, the regular monthly CPS survey in these months also asked unemployed individuals which out of a list of nine "traditional" job search methods they used.

Internet job search rates in these two surveys, classified by labor force status, are shown in Table 1. As already noted, the fraction of unemployed job seekers[6] looking for work online was 25.5 percent in August 2000, up from 15.0 percent in November 1998, less than two years earlier. As Table 1 also shows, much of this increase was associated with a large rise in home Internet access among unemployed persons (from 22.3 to 39.4 percent), but Internet use for job search conditional on Internet access also rose over this period. By August 2000, *regular* Internet job search was also surprisingly common among the employed (around 11 percent) and among labor force nonparticipants, at least those who were neither retired nor disabled (around 6 percent).[7]

In order to measure the job-finding success of Internet versus other job searchers, we matched observations in the December 1998 Supplement with the same persons in the ten subsequent CPS regular monthly surveys (January–March 1999, September 1999 through March 2000) in which some of the same individuals were reinterviewed. Similarly the August 2000 survey was matched with September–November 2000, and May through November 2001. Matching was done using established methods (see, for example, Brigitte Madrian and Lars Lefgren, 1999); details about our procedure are available from the authors.[8]

To be in our sample, a person had to be unemployed according to the official Bureau of

[6] All unemployed workers not expecting to be recalled to their former employer are classified by the Bureau of Labor Statistics (BLS) as "job seekers."

[7] Kuhn and Skuterud (2000) compare these recent rates of on-the-job *Internet* job search (IJS) to historical measures of on-the-job search (OJS) via any method. They are significantly higher, suggesting that the Internet may have contributed to an increase in total OJS.

[8] See an earlier version of this paper posted at: http://www.econ.ucsb.edu/~pjkuhn/Data/DataIndex.html. Only 10.4 percent of observations were not matched in any month after the Supplement date. The match rate for Internet searchers and others were very similar. For example, in January 1999 the match rate for Internet searchers is 93.6 percent compared to 91.5 percent for those not reporting Internet search in the previous month. In order to assess the possibility that our results might be driven by Internet searchers who were not matched because they moved to take jobs, we replicated our entire analysis treating all individuals whose spells were censored due to a failure to match as becoming reemployed in the month following the censoring. There was very little change.

Labor Statistics definition in a Computer/Internet supplement month (December 1998 or August 2000), yielding a sample of 4,139 persons.[9] According to this definition, unemployed persons must not be working, and *either* "on layoff" from a job to which they expected to be recalled *or* searching for work using at least one of nine recognized "active" methods.[10] These methods are listed in Table 2; the most common are "contacted employer directly," "sent resumes/filled applications," and "contacted public employment agency." It is noteworthy that these "traditional" measures of job search activity—used for decades by the BLS to define unemployment—could themselves involve Internet use, in which case they may be natural complements with Internet search. For example, a job seeker could e-mail resumes to employers or fill out an online job application form. Because of the possibility of complementarities (and, of course, substitutabilities), interpretation of the Internet search coefficient in a search outcome regression requires some care, as is discussed in Section II below.

Sample means of all the variables used in our regressions are presented in Table 2, separately for unemployed persons who searched for a new job on the Internet and those who did not.[11] In most cases, unemployed workers who look for jobs online have observable characteristics that are usually associated with greater job search success than other unemployed workers. For example, in the Computer/Internet Supplement month, the average unemployed Internet searcher had already been unemployed for 3.44 months, significantly less than the 3.75 month "retrospective duration" of the non-Internet searchers. Internet searchers resided in states with lower unemployment rates than other unemployed workers, and had previously worked in occupations with considerably lower unemployment rates, though the former difference is not statistically significant. Internet searchers were more likely to have been employed prior to the current unemployment spell, were much better educated, and were more likely to be in their "prime" working ages (26–55) (versus under 26 or over 55). They were less likely to be black, Hispanic, or immigrant and more likely to be homeowners than other unemployed persons. Finally, on average, unemployed workers who looked for work online were *more* likely, not less likely, to use "traditional" job search methods than other unemployed workers. In all, they used an average of 2.17 "traditional" search methods, compared to 1.67 for other unemployed workers, suggesting an overall complementarity between Internet and non-Internet methods.

By construction, no one in our sample was employed in the month in which we observe whether their job search strategy incorporated the Internet (December 1998 or August 2000). The fraction of our sample observed in employment at various points after these dates is reported near the bottom of Table 2. For example, among those individuals whose labor market status was observed one month after the Supplement date (i.e., in January 1999 or September 2000), 29.1 percent were employed. Two months after the Supplement date, 37.5 percent were employed, and a year later 55.9 percent were employed. If we pool all individuals who were reinterviewed at least once after the date in which we observe their Internet search activity, the same share, 55.9 percent, were seen in reemployment at some time after the Supplement date.

Comparing Internet job searchers with other unemployed workers, essentially no difference in employment rates is evident one or two months after an individual's Internet job search activity is observed. A year later, however, 64.6 percent of unemployed Internet searchers are reemployed, compared to 53.3 percent of other unemployed workers. This difference, like the difference in reemployment at *any* time after the Supplement date, is statistically significant. On the surface, Table 2 thus seems to suggest that Internet search facilitates reemployment, at least if one allows a few months to elapse for this method to yield results.

[9] Our sample includes the small group of persons who were never matched after those dates. While these observations contribute no information on unemployment durations, they do contribute information on the determinants of Internet search, and are retained in our analysis for that reason.

[10] We conducted some robustness checks that excluded workers expecting recall, as well as some analyses that included marginally attached workers (nonparticipants who engaged in passive job search only). In neither case were the results substantially different.

[11] The data and do-files used to produce Table 2 and all subsequent tables are posted at: http://www.econ.ucsb.edu/~pjkuhn/Data/DataIndex.html.

TABLE 2—SAMPLE MEANS BY INTERNET SEARCH ACTIVITY

	Internet search		
	Yes	No	Total
Retrospective duration	3.440	3.749	3.684*
2000 Supplement	0.637	0.477	0.510*
On layoff	0.107	0.093	0.096
State unemployment rate	4.312	4.370	4.358
Occupational unemployment rate	3.681	4.723	4.506*
Worked prior to unemployment	0.619	0.507	0.530*
School prior to unemployment	0.208	0.215	0.213
Lost job	0.323	0.240	0.258*
Temporary job	0.115	0.117	0.117
Private sector	0.792	0.794	0.794
Public sector	0.115	0.070	0.079*
Self-employed	0.047	0.034	0.036
Age 16–25	0.302	0.408	0.386*
Age 26–35	0.240	0.211	0.217
Age 36–45	0.219	0.199	0.203
Age 46–55	0.180	0.108	0.123*
Male	0.484	0.498	0.495
Married	0.421	0.302	0.326*
Male and married	0.203	0.135	0.150*
Spouse employed	0.307	0.213	0.233*
Primary school	0.006	0.072	0.058*
Incomplete high school	0.098	0.296	0.255*
Completed high school	0.241	0.368	0.342*
Incomplete college	0.234	0.139	0.158*
Associate degree	0.084	0.039	0.048*
Black	0.117	0.210	0.191*
Hispanic	0.079	0.168	0.149*
Home owner	0.602	0.515	0.533*
Immigrant	0.100	0.133	0.126*
Contacted employer directly	0.653	0.643	0.645
Contacted public employment agency	0.250	0.191	0.203*
Contacted private employment agency	0.116	0.057	0.069*
Contacted friends or relatives	0.151	0.128	0.133
Contacted school employment center	0.044	0.022	0.027*
Sent resumes/filled applications	0.603	0.456	0.487*
Checked union/professional registers	0.033	0.018	0.021*
Placed or answered ads	0.221	0.120	0.141*
Other active search method	0.099	0.038	0.051*
Number of traditional search methods	2.171	1.674	1.777*
Internet access at home	0.801	0.202	0.326*
Employed in the month following the Computer/Internet Supplement[a]	0.298	0.289	0.291
Employed 2 months after Computer/Internet Supplement[a]	0.413	0.365	0.375
Employed 12 months after Computer/Internet Supplement[a]	0.646	0.533	0.559*
Observed in employment, in any post-Supplement month[b]	0.614	0.545	0.559*
Number of months observed	2.805	2.611	2.651*

Notes: * Indicates if means are statistically different at a 5-percent significance level which is obtained by regressing each variable on a constant and the Internet search dummy variable. Sample sizes are 860 Internet searchers and 3,279 non-Internet searchers.

[a] Share of persons observed at that date.

[b] Share of all observations.

II. Conceptual Framework

To help interpret our estimates of the effect of Internet job search activity, suppose that an outcome of job search, R_i [for example the log of the integrated baseline hazard—see equation (6)] is a linear function of a vector of exogenous observables, $\mathbf{Z}_i$; a vector of nine endogenously chosen "traditional" search methods, $\mathbf{M}_i$; an indicator variable for the use of Internet methods, IJS_i; and a random term μ_i. In other words, the production function for "reemployment" is given by:[12]

$$(1) \qquad R_i = \theta\mathbf{Z}_i + \gamma IJS_i + \boldsymbol{\delta}\mathbf{M}_i + \mu_i.$$

If individual i's total cost of using the Internet to look for work is:

$$(2) \qquad C_{i0} = \mathbf{b_0}\mathbf{Z}_i + \varepsilon_{i0}$$

and individuals choose IJS_i (=0 or 1) to maximize $kR_i - C_{i0}$ (where k is a scaling parameter converting search outcomes into dollars), they will use the Internet for job search when $\varepsilon_{i0} < k\gamma - \mathbf{b_0}\mathbf{Z}_i$. Next, let the cost of using "traditional" job search method j ($j = 1, \cdots 9$) be:

$$(3) \qquad C_{ij} = \mathbf{b_j}\mathbf{Z}_i + c_j IJS_i + \varepsilon_{ij}.$$

According to this formulation, use of the Internet can be either complementary ($c_j < 0$) or a substitute ($c_j > 0$) with "traditional" methods such as sending resumes. If workers choose a vector of traditional search methods to maximize $U_i = kR_i - \sum_j C_{ij}$, they will use method j when $\varepsilon_{ij} < k\delta_j - \mathbf{b_j}\mathbf{Z}_i - c_j IJS_i$.

Given the above structure, equation (1) can be consistently estimated by ordinary least squares (OLS) (or its single-equation equivalent) as long as the vector of shocks to search costs, $\boldsymbol{\varepsilon}_\mathbf{i}$, is uncorrelated with shocks to reemployment rates, μ_i, where the latter may include a permanent person-specific effect, i.e., unobserved "reemployability." Identifying the parameter of interest—γ—in the presence of correlation between $\boldsymbol{\varepsilon}_\mathbf{i}$ and μ_i requires an instrument—i.e., a variable that enters (2) but not (1); unfortunately we do not have credible candidates for such a variable in our data set. In the absence of such an instrument, our priors when we started this research were that "abler" workers would have lower Internet use costs, implying that single-equation estimates of (1) will *overstate* the productivity of Internet job search.

Finally, in the case where $\boldsymbol{\varepsilon}_\mathbf{i}$ and μ_i are uncorrelated, consider estimating equation (1) excluding measures of "traditional" search methods, $\mathbf{M}_i$. Approximating the distribution of ε_{ij} by a uniform distribution (without loss of generality with density 1), the omitted-variable bias formula implies that:

$$(4) \qquad \hat{\gamma} = \gamma + \sum_{j=1}^{9} \delta_j \phi_j,$$

where $\phi_j = -c_j$, i.e., the marginal effect of Internet use on the use of "traditional" search method j, estimated from a linear probability model for each of the nine "traditional" search methods. Equation (4) thus defines two Internet search effects of potential interest: the "direct" effect (γ), and the total effect ($\hat{\gamma}$). The former gives the effect of Internet search on outcomes holding all other search methods fixed; the latter gives its effect when all other search methods are adjusted optimally to the adoption of Internet search, allowing for both substitutabilities and complementarities among methods. Since these are both interesting questions, we shall present results for both specifications below.

III. Probit Analysis

As suggested by equation (1), any credible analysis of both the determinants and effects of Internet search would, of course, control for observable differences between unemployed workers who look for work online and those who do not. To that end, Table 3 reports estimates of probit models for Internet job search, as well as for the outcomes of job search. Regressors include characteristics of the individual, his/her unemployment spell, and the individual's activity before entering the current unemployment spell. Throughout Table 3, we

[12] Note that this framework does not allow for heterogeneity in the marginal effectiveness of Internet search across individuals. If anything, ignoring the possibility that individuals choose those search methods whose idiosyncratic productivity effects are the greatest implies that our estimates in this paper will *overstate* the effectiveness of Internet search for a randomly selected individual.

TABLE 3—PROBIT ESTIMATES OF INTERNET SEARCH DETERMINANTS AND OUTCOMES

	Looked for work online		Employed one year later?			
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Internet job search			0.062 (0.095)	0.035 (0.107)	0.031 (0.097)	−0.005 (0.109)
Retrospective duration	−0.002 (0.005)	0.010 (0.005)	−0.027* (0.007)	−0.027* (0.007)	−0.029* (0.007)	−0.029* (0.007)
2000 Supplement	0.408* (0.051)	0.166* (0.056)	−0.100 (0.075)	−0.105 (0.076)	−0.108 (0.076)	−0.115 (0.077)
On layoff	−0.067 (0.087)	−0.050 (0.097)	0.328* (0.135)	0.328* (0.135)	0.316* (0.138)	0.315* (0.137)
State unemployment rate	0.020 (0.026)	0.012 (0.029)	0.040 (0.039)	0.041 (0.039)	0.034 (0.039)	0.034 (0.040)
Occupation unemployment rate	−0.088* (0.016)	−0.062* (0.016)	−0.044* (0.021)	−0.044* (0.021)	−0.044* (0.021)	−0.043* (0.021)
Worked before unemployment	0.189* (0.080)	0.247* (0.089)	0.414* (0.122)	0.415* (0.122)	0.418* (0.123)	0.420* (0.123)
School before unemployment	0.333 (0.083)	0.280* (0.091)	0.271* (0.121)	0.266* (0.121)	0.258* (0.122)	0.251* (0.122)
Lost job	0.118 (0.078)	0.130 (0.086)	0.027 (0.123)	0.027 (0.123)	−0.010 (0.125)	−0.010 (0.125)
Temporary job	−0.015 (0.094)	0.028 (0.103)	−0.195 (0.142)	−0.191 (0.142)	−0.246 (0.145)	−0.241 (0.145)
Private sector	0.258* (0.108)	0.221* (0.119)	0.422* (0.148)	0.419* (0.148)	0.440* (0.149)	0.437* (0.149)
Public sector	0.265* (0.133)	0.241* (0.145)	0.089 (0.195)	0.087 (0.195)	0.132 (0.196)	0.130 (0.196)
Self-employed	0.316* (0.160)	0.330* (0.176)	0.153 (0.241)	0.151 (0.241)	0.219 (0.242)	0.216 (0.243)
Age 16–25	0.554* (0.119)	0.448* (0.132)	0.571* (0.160)	0.572* (0.160)	0.584* (0.161)	0.585* (0.161)
Age 26–35	0.462* (0.114)	0.473* (0.127)	0.466* (0.153)	0.471* (0.154)	0.443* (0.155)	0.450* (0.156)
Age 36–45	0.333* (0.113)	0.310* (0.125)	0.507* (0.149)	0.511* (0.149)	0.511* (0.151)	0.516* (0.151)
Age 46–55	0.384* (0.117)	0.369* (0.129)	0.242 (0.155)	0.243 (0.155)	0.241 (0.157)	0.243 (0.157)
Male	−0.055 (0.062)	−0.175* (0.068)	−0.187* (0.094)	−0.190* (0.095)	−0.175 (0.095)	−0.179 (0.095)
Married	0.143 (0.103)	−0.054 (0.114)	0.032 (0.151)	0.027 (0.151)	0.031 (0.153)	0.024 (0.153)

TABLE 3—*Continued.*

	Looked for work online		Employed one year later?			
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Married male	0.218	0.346*	0.336*	0.339*	0.319*	0.322*
	(0.104)	(0.115)	(0.154)	(0.154)	(0.157)	(0.157)
Spouse employed	−0.088	−0.099	−0.081	−0.082	−0.077	−0.079
	(0.092)	(0.102)	(0.135)	(0.135)	(0.137)	(0.137)
Primary school	−1.708*	−1.252	−0.295	−0.286	−0.264	−0.251
	(0.208)	(0.231)	(0.204)	(0.205)	(0.207)	(0.208)
Incomplete high school	−1.243*	−0.926*	−0.443*	−0.439*	−0.414*	−0.407*
	(0.092)	(0.101)	(0.144)	(0.144)	(0.147)	(0.147)
Complete high school	−0.918*	−0.589*	−0.200	−0.190	−0.170	−0.160
	(0.075)	(0.082)	(0.126)	(0.127)	(0.129)	(0.130)
Incomplete college	−0.418*	−0.250*	−0.087	−0.083	−0.061	−0.055
	(0.079)	(0.086)	(0.136)	(0.136)	(0.138)	(0.138)
Associate degree	−0.303*	−0.035	0.153	0.157	0.171	0.177
	(0.109)	(0.119)	(0.187)	(0.187)	(0.190)	(0.190)
Black	−0.266*	0.030	−0.267*	−0.260*	−0.280*	−0.272*
	(0.069)	(0.078)	(0.095)	(0.095)	(0.096)	(0.097)
Hispanic	−0.253*	0.036	0.008	0.016	0.007	0.017
	(0.086)	(0.095)	(0.114)	(0.115)	(0.116)	(0.117)
Home owner	0.072	−0.177*	0.071	0.067	0.077	0.071
	(0.052)	(0.058)	(0.078)	(0.079)	(0.079)	(0.080)
Immigrant	−0.048	−0.103	0.011	0.008	0.057	0.051
	(0.087)	(0.094)	(0.117)	(0.117)	(0.118)	(0.118)
Contact employer					0.159*	0.160*
					(0.079)	(0.079)
Contact public employment agency					0.257*	0.262*
					(0.097)	(0.098)
Contact private employment agency					0.247	0.249
					(0.153)	(0.153)
Contact friend/relative					−0.146	−0.143
					(0.111)	(0.111)
Contact school employment agency					−0.245	−0.243
					(0.220)	(0.220)
Sent resumes					0.220*	0.220*
					(0.077)	(0.077)
Check union					−0.139	−0.142
					(0.292)	(0.292)
Used ads					−0.158	−0.157
					(0.108)	(0.108)

TABLE 3—*Continued.*

	Looked for work online		Employed one year later?			
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
Other active					0.281	0.284
					(0.179)	(0.180)
Home Internet access		1.430*		0.052		0.069
		(0.060)		(0.096)		(0.097)
Constant	−0.908*	−1.509*	−0.517	−0.531	−0.744*	−0.766*
	(0.214)	(0.238)	(0.308)	(0.309)	(0.320)	(0.322)
Log-likelihood	−1,738.72	−1,425.16	−840.15	−840.00	−827.22	−826.97
N	4,139	4,139	1,344	1,344	1,344	1,344

Notes: Standard errors are in parentheses. The reference category for activity before unemployment (worked or school) is neither worked nor attended school before unemployment.
* Indicates significance at the 5-percent level.

present specifications of each equation with and without a control for home Internet access. In the Internet-search probits, home Internet access clearly has a strong estimated effect, but it is possible (especially among the unemployed) that Internet access was obtained in order to assist with job search, making specifications without this control of some interest. Likewise, while we do not believe home Internet access has a causal effect on the job-finding rate—what should matter is whether the Internet is *used* for job search—we can think of plausible arguments for and against controlling for Internet access in the reemployment probits.[13]

Looking first at the determinants of Internet search in columns (1) and (2) of Table 3, most of the results from the univariate comparisons in Table 2 are confirmed. For example, Internet search grew rapidly between 1998 and 2000, was more common in occupations with low unemployment rates, among young and well-educated workers, and among persons who entered unemployment either from work or school. Since all of these characteristics are usually associated with shorter unemployment spells, this confirms our strong impression of *positive selection on observables.* Particularly noteworthy is the highly significant negative correlation in column (1) between being black or Hispanic and using the Internet to look for work, suggesting a "digital divide" along racial and ethnic lines. Both of these gaps, however, completely disappear in column (2), indicating that the gap is entirely attributable to racial/ethnic differences in home Internet access, rather than any differential tendency to use the Internet for job search conditional on access.

Turning now to the effects of Internet search on unemployment durations, do the apparently beneficial effects of Internet search in the Table 2 means *also* survive controlling for observable differences between Internet searchers and others? As a first step in answering this question, the remaining columns of Table 3 present probit estimates of the probability an unemployed individual is employed 12 months after we observe their Internet job search activity in the CPS Computer/Internet Supplement. We focus on 12 months because this is where the largest apparent Internet effect was observed in Table 2.[14] As discussed in Section II, we present es-

[13] On the "for" side, home Internet access may be correlated with other unobserved characteristics (for example wealth, which in turn is correlated with past employment) that do affect job-finding rates. On the other hand, home Internet access is a very powerful predictor of online search among the unemployed, and much of the variation in home access may be driven by genuinely exogenous differences in the rate of Internet diffusion across space, time, and income groups; in this case controlling for access could be discarding a large amount of useful variation.

[14] Similar analyses were performed for reemployment within a month, within two months, or at any time after Internet search activity is observed. (In the latter specifica-

timates with and without controls for the use of "traditional" search methods, identifying respectively the "direct" and "total" effects of incorporating the Internet into one's job search strategy.

Effects of the "control" variables in Table 3's employment probits are generally in line with expectations. For example, we see that individuals with high retrospective durations are less likely to be reemployed—a result that mirrors the common finding of declining reemployment hazards in duration studies.[15] Workers on layoff are more likely to be reemployed than those not expecting to be recalled to their former employer. A high occupational unemployment rate depresses job-finding rates, and individuals who worked or went to school immediately before the onset of their current unemployment spell are much more likely to be reemployed than those who did neither. Persons whose last job was in the private sector fared better in reemployment than those whose last job was in the public sector or in self-employment, or who did not work just prior to the current unemployment spell.[16] Younger workers are reemployed more quickly; less-educated and black workers more slowly. Although the effect is not quite significant at conventional levels, single men are less likely to be reemployed than single women. Married men are however much more likely to be reemployed than married women, possibly reflecting greater geographical search constraints among married women (Thomas S. Crossley et al., 1994).

The remaining variables in Table 3 are controls for the use of other, "traditional" job search methods. Interestingly, when these variables are included [columns (5) and (6) only] we detect significant positive effects on reemployment for three of them: direct employer contact, "sent resumes," and public employment agencies, which incidentally are also the search methods most commonly used by unemployed persons in our data. For the remaining methods, no statistically significant effects on the job-finding rate are detected.

Most surprising, and of greatest interest to us here, is the Internet job search coefficient in Table 3. In contrast to the univariate results in Table 2, Table 3 shows that adding the Internet to one's job search strategy appears *not* to increase reemployment rates. This is true whether or not we hold constant an individual's Internet access from home, and whether or not we allow the use of "traditional" search methods to be adjusted optimally when an Internet search component is introduced. In sum, when we control for the positive selection of Internet job searchers on observed characteristics, no evidence of an unemployment-reducing effect of Internet search is evident in our data.

IV. Duration Analysis

While Table 3 certainly suggests that incorporating the Internet into one's job search strategy is ineffective in reducing jobless durations, one reason why this conclusion might be premature is an inefficiency in the estimation procedure. In particular, *any* probit focusing on a worker's labor force status at only a single date—in the above case 12 months after his/her search activity is observed—discards a considerable amount of information on the actual duration of unemployment. It is therefore possible that those probits might fail to reveal a true, beneficial effect of Internet job search.

To address this issue, we estimate a duration model that incorporates all the available information in the CPS about a worker's jobless spell following the Supplement date. Of course, the information available to us on durations in the CPS is highly discrete: at best, we only know the month in which reemployment occurred; in some cases (the gap between the two four-month CPS observation "windows"), we only know that reemployment occurred during an eight-month period. This makes continuous-time duration models highly inappropriate. For

tion, we added a control for the number of months in which the individual is observed after the Supplement month.) We also replaced the state unemployment rate by a state fixed effect. In all cases, the results were similar to those in Table 3: whenever even a relatively parsimonious set of demographic controls are used, the Internet search coefficient is either insignificant or negative.

[15] In a previous version of this paper we modeled the effects of left-censoring in our duration data more formally, using a technique introduced by Tony Lancaster (1979): essentially we condition each observation's contribution to the likelihood function on the fact that it lasted long enough to be observed in our sample. There was very little change in the results.

[16] Note that in a substantial number of cases the individual's last job preceded a spell of nonparticipation; these "sector" indicators apply to these individuals as well as to persons who entered unemployment directly from a job.

this reason we develop and estimate a discrete-time hazard model that takes into account the particular features of CPS duration data (i.e., potentially large failure "windows" whose structure varies across observations), while still allowing for a fully flexible form of the baseline hazard function.[17]

We begin, as is common, by assuming the hazard rate into reemployment, $\lambda(\tau)$, is separable into a baseline component that depends on elapsed duration $\lambda_0(\tau)$, and a component that depends on a linear combination of observed characteristics $\mathbf{X}_i$ and estimated parameters $\boldsymbol{\beta}$:

$$\lambda(\tau) = \lambda_0(\tau) \cdot \exp(\mathbf{X}_i\boldsymbol{\beta}). \tag{5}$$

From assumption (1) it follows that (see Nicholas M. Kiefer, 1988, pp. 664–65):

$$\log \Lambda_0(t_i) = -\mathbf{X}_i\boldsymbol{\beta} + \mu_i \tag{6}$$

where the random variable $\Lambda_0(t_i)$ is the integrated baseline hazard up to each observation's realized duration, i.e.;

$$\Lambda_0(t_i) = \int_0^{t_i} \lambda_0(\tau)d\tau \tag{7}$$

and where μ_i follows a type-1 extreme-value distribution.[18] Thus the transformed duration variable, log $\Lambda_0(t_i)$,—which is monotonically increasing in t_i—can be thought of as the dependent variable in a linear regression.

Suppose now that a particular unemployment spell is known to have ended between two dates, $t_a > t_b$. Defining $\delta_a \equiv \log \Lambda_0(t_a)$ and $\delta_b \equiv \log \Lambda_0(t_b)$, the likelihood of such a spell is just:

$$F(\delta_a + \mathbf{X}_i\boldsymbol{\beta}) - F(\delta_b + \mathbf{X}_i\boldsymbol{\beta}), \tag{8}$$

where F is the c.d.f. of μ_i. Durations known only to have ended after, say, t_a (i.e., right-censored durations) have a likelihood of $1 - F(\delta_a + \mathbf{X}_i\boldsymbol{\beta})$; durations known to have ended between $t = 0$ and, say, t_b, have a likelihood of $F(\delta_b + \mathbf{X}_i\boldsymbol{\beta})$.[19]

In our data, job searchers are observed no more frequently than once per month. Recognizing this discreteness, we divide the set of possible jobless durations into disjoint intervals.[20] Denote the number of such intervals by $T + 1$; in the results reported in Table 4 (which focus on post-Supplement durations only), we used eight intervals: 0–1, 1–2, 2–3, 3–10, 10–11, 11–12, 12–13, and more than 13 months. For some of our observations (for example, those persons observed as unemployed in one month and employed the next), we know in exactly which of these intervals their unemployment spell ended. Others are right-censored, due to attrition or rotation out of the sample. For yet others (including, but not limited to, persons who were not matched in a period before they are first observed in employment) we know only that they became employed at some point within a set of adjacent intervals.

To allow for the latter types of observations, define $\underline{\mathbf{V}_i}$ as a 1xT vector of "lower bound" dummy variables (think of these as applying, in order, to each of the $T + 1$ intervals defined above except the highest one). Set $\underline{\mathbf{V}_i}$ equal to zero for all intervals except the one *preceding* the interval in which worker i's unemployment spell is known to have ended.[21] Define $\bar{\mathbf{V}}_i$ as a 1xT vector of upper bound dummy variables, equal to zero for all intervals except the one *during which* we knew the unemployment spell ended.[22] Finally, let $\boldsymbol{\delta}$ be a Tx1 coefficient vector corresponding to the "cut points" between the above intervals. Because the elements of $\boldsymbol{\delta}$ correspond to the log of the integrated baseline hazard at the upper end of each interval, and because $\boldsymbol{\delta}$ is estimated, this procedure allows for an unrestricted baseline hazard function.

Putting all the above together, the log likelihood for the entire sample can be expressed as:

[17] Existing discrete-time hazard models, such as Bruce D. Meyer's (1990) require the structure of intervals to be the same across observations.

[18] The cumulative distribution function (c.d.f.) for the extreme-value distribution is given by $F(\mu_i) = \exp(-\exp(-\mu_i))$.

[19] Unlike observed durations which must be positive, note that the transformed durations and the error term μ_i occupy the entire real line.

[20] An Appendix describing how we constructed unemployment durations from the matched CPS files is available from the authors. See footnote 8.

[21] If the observation is right-censored this is the interval before it became right-censored; if the observation became reemployed during the first interval $\underline{\mathbf{V}_i}$ is a vector of zeroes.

[22] If the observation is right-censored, $\bar{\mathbf{V}}_i$ is a vector of zeroes.

TABLE 4—REEMPLOYMENT HAZARD ESTIMATES

	(1)	(2)	(3)	(4)
Internet search	−0.198*	−0.350*	−0.170*	−0.309*
	(0.073)	(0.084)	(0.074)	(0.086)
Retrospective duration	−0.029*	−0.028*	−0.030*	−0.030*
	(0.005)	(0.005)	(0.005)	(0.005)
2000 Supplement	0.310*	0.275*	0.310*	0.280*
	(0.061)	(0.061)	(0.061)	(0.062)
On layoff	0.150	0.148	0.186	0.183
	(0.096)	(0.096)	(0.097)	(0.097)
State unemployment rate	−0.087*	−0.093*	−0.083*	−0.088*
	(0.030)	(0.030)	(0.031)	(0.031)
Occupation unemployment rate	−0.005	−0.002	−0.005	−0.002
	(0.016)	(0.016)	(0.016)	(0.016)
Worked before unemployment	−0.007	0.010	−0.005	0.014
	(0.098)	(0.098)	(0.099)	(0.099)
School before unemployment	−0.094	−0.094	−0.097	−0.093
	(0.102)	(0.102)	(0.102)	(0.102)
Lost job	−0.213*	−0.224*	−0.200*	−0.212*
	(0.093)	(0.093)	(0.093)	(0.093)
Temporary job	−0.052	−0.045	−0.026	−0.021
	(0.111)	(0.111)	(0.112)	(0.113)
Private sector	0.076	0.054	0.090	0.067
	(0.133)	(0.134)	(0.134)	(0.134)
Public sector	0.366*	0.347*	0.360*	0.343*
	(0.169)	(0.170)	(0.170)	(0.171)
Self-employed	0.386	0.354	0.355	0.327
	(0.205)	(0.206)	(0.207)	(0.207)
Home Internet access		0.292*		0.263*
		(0.081)		(0.082)
Controls for "traditional" job search methods	No	No	Yes	Yes
Log-likelihood	−2,038.50	−2,031.89	−2,026.83	−2,021.58

Notes: Regressions include all controls used in Table 3. The sample size for all specifications is 4,139.

$$(9)\quad \log L = \sum_{Cens=L} \log\left[F(\bar{\mathbf{V}}_{\mathbf{i}}\boldsymbol{\delta} + \mathbf{X}_i\boldsymbol{\beta})\right] + \sum_{Cens=0} \log\left[F(\bar{\mathbf{V}}_{\mathbf{i}}\boldsymbol{\delta} + \mathbf{X}_i\boldsymbol{\beta}) - F(\underline{\mathbf{V}}_{\mathbf{i}}\boldsymbol{\delta} + \mathbf{X}_i\boldsymbol{\beta})\right] + \sum_{Cens=R} \log\left[1 - F(\underline{\mathbf{V}}_{\mathbf{i}}\boldsymbol{\delta} + \mathbf{X}_i\boldsymbol{\beta})\right].$$

where *Cens* = L, 0, and R indicates the observation is left-censored, not censored, or right-censored, respectively. [Note that we refer to observations that became reemployed in the first month of their unemployment spell as left-censored because the transformed duration variable, $\log \Lambda_0(t_i)$, has no lower bound for this group.]

Table 4 presents the values of β that maximize (9) for the same set of control variables ($\mathbf{X}$) used in Table 3 (not all coefficients are reported). Note that a positive coefficient in Table 4 indicates a positive effect on the hazard rate, so that coefficient signs and significance but not magnitudes are comparable with Table 3. That said, Table 4 results for the "control" variables are very similar to those in Table 3. For example, persons who are far into their unemployment spells (i.e., with high retrospective durations in the Supplement month) have lower reemployment hazards (longer remaining unemployment durations) after that date. Reemployment rates were higher in the 2000 Supplement, reflecting the tighter aggregate labor market conditions prevailing around the time of that survey. High state unemployment rates retard reemployment. One interesting difference from Table 3 is that the positive partial correlation between home Internet *access* and reemployment rates becomes statistically significant. The most surprising finding from Table 3, however, is that Internet job search now appears to be not simply ineffective, but in fact significantly *counterproductive.* In other words, holding constant observable characteristics of the person and the previous duration of the unemployment spell, persons who searched for work online actually entered reemployment more slowly than those who did not during the period after we observe whether they search online. We conclude that the absence of an estimated beneficial effect of Internet search in Table 3's reemployment probits cannot be attributed to the inefficiency of that estimation procedure. Instead, incorporating all the available information on durations in our sample only strengthens the case against an unemployment-reducing effect of Internet job search.[23]

V. Discussion

According to our data, Internet job search is more common among workers with observed characteristics that are usually associated with faster reemployment. At the same time, holding these observed characteristics constant, unemployment durations are not shorter, and possibly even longer among workers who look for work online than among workers who do not. What explains this? One possibility, of course, is that Internet search is in fact counterproductive at the individual level, perhaps because of certain signals it sends to employers. Workers might still use this method, however, because it is easier and cheaper than other methods, or because they are unaware of these drawbacks. Alternatively, Internet job search might significantly improve search outcomes on dimensions such as job quality that we cannot measure here, which could more than compensate for an estimated increase in search time.[24] A third possibility is that Internet job search *does* speed reemployment, but that (despite the relatively rich set of observables available in this data) our results are contaminated by selection into Internet search on unobservable worker characteristics that are correlated with the workers' reemployability.

Our priors when we started this research, in fact one of our chief concerns, was that Internet searchers would be positively selected on unobservables, as they are on observables. Clearly, if we were to maintain our belief in this plausible notion that, for example, Internet searchers are likely to be more motivated or better connected than other job seekers, then our estimates in Table 4 *exaggerate* the benefits of Internet job search, thus strengthening the case that Internet job search does not reduce unemployment durations.[25] But what of the possibility of negative selection into Internet search on unobservables? We can think of at least four mechanisms that

[23] Inspection of the coefficients for "traditional" search methods reveals that three of these methods—public employment agencies, "sent resumes," and "checked union and professional registers"—also had significant, counterproductive estimated effects on reemployment in the Table 4 hazard models. We discuss possible interpretations of these results in the following section.

[24] Theoretical results in Kenneth Burdett and Jan Ondrich (1985) suggest that this is unlikely. If we think of Internet search as raising the offer arrival rate in a standard sequential search model, then a relatively weak condition (log-concavity of the wage-offer density) guarantees that it will reduce unemployment durations.

[25] Since we have no measures of advance notice of job loss, one example of positive selection would involve a greater amount of pre-unemployment search among Internet searchers—search which could yield job offers during the period in which we observe workers. Other omitted variables include children, income, and previous unemployment though it is unclear in what direction these might bias our results.

could generate this. First, as suggested by Holzer (1987) in a different context, persons who use formal and anonymous job search channels (such as the Internet) may be doing so because their informal contacts and social networks are poor.[26] Second, and related, is the possibility of private information about reemployability: persons using a larger number of search methods—including the Internet—may do so in *response* to private information that their search prospects are particularly poor.[27] Third, our data do not allow us to control for receipt of unemployment insurance (UI), or health status. If Internet searchers are more likely to apply and qualify for UI, or are using the Internet to search because of health or disability limitations, these omitted variables might also account for their longer durations.[28]

Finally, especially among workers with home Internet access, Internet job search strikes us as a very low-cost job search method. The costs of engaging in it are therefore unlikely to screen out individuals with only a very marginal interest in finding a new job. This source of adverse selection is apparently a major concern for practitioners currently working in the Internet recruiting industry. In a personal interview, a professional recruiter informed us that he avoids Internet job boards altogether because of a concern about negative selection. This is echoed by a recruiting executive quoted in Autor (2001), who observed that Internet job boards are populated with four types of resumes: "the unhappy (and thus probably not a desirable employee); the curious (and therefore likely to be a 'job-hopper'); the unpromotable (probably for a reason); and the unemployed (probably for a worse reason)." It is also echoed in the development of software tools such as "resume spiders" and "resume robots," whose main aim is to circumvent job boards by trolling the Internet for "passive" job seekers who have *not* decided to look for work online.[29]

In sum, unemployed Internet job searchers do not become reemployed more quickly than observationally equivalent unemployed persons who do not look for work online. A number of factors, including simple ineffectiveness of Internet job search methods and negative selection on unobservables, could account for this finding. While disentangling these remaining possibilities remains an important topic for further research, our results in this paper are clearly inconsistent with a scenario in which Internet searchers are positively selected (on hard-to-observe characteristics) *and* in which Internet search speeds reemployment. Since Internet search companies often make both claims simultaneously, some reevaluation of these claims may be necessary.

[26] Reinforcing this interpretation is the fact that the three other search methods that were associated with longer durations in the hazard models—see footnote 23—are also formal and anonymous in nature (compared, for example, to direct employer contact and using friends and relatives).

[27] In particular, it is possible that unemployed persons who have already located and arranged to start a job (a) are no longer looking for work, and (b) are likely to transit into employment very quickly. Since such short-term effects are more likely to be captured in our Table 4 hazard estimates than the Table 3 probits for employment a year later, this may help explain why other search methods also appear to be less productive in the hazard models. For evidence on time lags between "finding" and starting a job, see Crossley and Kuhn (1999).

[28] We thank anonymous referees for these suggestions.

[29] See Kuhn (2003) for a more detailed description of these industry developments.

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