Internet Job Search and Unemployment Durations

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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. Which types of persons looked for work on line, and did searching for work on line help these workers find new jobs faster? We address these questions using measures of internet job search among unemployed workers in the December 1998 and August 2000 CPS Computer and Internet Supplements, matched with job search outcomes from subsequent CPS files. 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.

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"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, Sept. 19, 2002.

1. Introduction

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. jobseekers reported that they regularly used the internet to look for jobs; one in ten employed persons said they regularly looked for other jobs on line. 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 (Mortensen 2000), higher average match quality (Krueger 2000), a reduction of noncompetitive wage differentials (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 on line? Second is to estimate the effect on search

[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 on-line 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 websites.

[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.

outcomes, for an individual worker, of incorporating the internet into his or her job search strategy. We are 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.[4]

In order to answer our questions we use measures of internet search derived from the December 1998 and August 2000 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.[5] 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 jobseekers[6]; thus we cannot ascertain from our data whether internet search produces better job matches. 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. Brown and Goolsbee 2002 in life insurance markets; Brynjolfsson and Smith 2000 on book and CD markets, and Carlton and Chevalier 2001 on

[3] For example, 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] The internet has, of course, affected firms' recruiting strategies as well as workers' search strategies. To the extent that vacancies posted on line can only be discovered by job-searching on line, such changes in firms' recruiting strategies should increase the estimated effectiveness of internet job search in this paper.
[5] See Skuterud (2001) for a recent analysis of trends in on-the-job search using the occasional CPS surveys that do collect this information.
[6] CPS wage information is of course only available for persons who find new jobs, and who are in the outgoing rotation groups. Further, a credible analysis of re-employment wages also requires controls for *pre-unemployment* wages, a restriction which reduces the sample to non-useful levels.

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, Holzer (1987, 1988), Bortnick and Ports (1992), Osberg (1993) and Addison and Portugal (2001) compare the job-finding rates of unemployed workers using a variety of search methods. 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. 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 unsurprising 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.

2. Data and Descriptive Statistics

As noted, our data on internet job search come from the December 1998 and August 2000 Computer and Internet Use Supplements to the Current Population Survey. 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 jobseekers[7] looking for work on line 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).[8]

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 re-interviewed. 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 Madrian and Lefgren 1999); details about our procedure are available from the authors.[9]

To be in our sample, a person had to be unemployed according to the official Bureau of Labor Statistics definition in a Computer/Internet supplement month (December 1998 or August

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[7] All unemployed workers not expecting to be recalled to their former employer are classified by the BLS as "jobseekers".

[8] 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 siginficantly higher, suggesting that the internet may have contributed to an increase in total OJS.

[9] See an earlier version of this paper posted at http://www.econ.ucsb.edu/~pjkuhn/pkhome.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 compared to 91.5 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 re-employed in the month following the censoring. There was very little change.

2000), yielding a sample of 4139 persons.[10] 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.[11] 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 jobseeker could email resumes to employers or fill out an on-line 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 3 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. In most cases, unemployed workers who look for jobs on line 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, somewhat less than the 3.75-month “retrospective duration” of the non-internet searchers. Internet searchers resided in states with somewhat lower unemployment rates than other unemployed workers, and had previously worked in occupations with considerably lower unemployment rates. They 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). Internet job searchers 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

[10] Our sample includes a small group of persons who were never matched with an observation 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.

looked for work on line 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 of internet and non-internet methods.

By construction, no one in our sample was working 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 re-interviewed at least once after the date in which we observe their internet search activity, the same share, 55.9 percent, were seen in re-employment 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 re-employed, compared to 53.3 percent of other unemployed workers. This difference, like the difference in re-employment at *any* time after the Supplement date, is statistically significant. On the surface, Table 2 thus seems to suggest that internet search facilitates re-employment, at least if one allows a few months to elapse for this method to yield results.

3. 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, Z_i; a vector of 9 endogenously-chosen

[11] We conducted some analyses 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.

"traditional" search methods, 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 "re-employment" is given by[12]:

$$R_i = \theta Z_i + \gamma IJS_i + \delta M_i + \mu_i \,. \tag{1}$$

Let individual i's total cost of using the internet to look for work be:

$$C_{i0} = b_0 Z_i + \varepsilon_{i0} \tag{2}$$

and the cost of using "traditional" job search method j (j = 1,…9) be:

$$C_{ij} = b_j Z_i + c_j IJS_i + \varepsilon_{ij} \tag{3}$$

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 search methods to maximize $U_i = kR_i - \sum_j C_{ij}$ (where k is a scaling parameter converting search outcomes into dollars), they will use the internet for job search when $\varepsilon_{i0} < k\gamma - b_0 Z_i$, and will use method j when $\varepsilon_{ij} < k\delta_j - b_j Z_i - c_j IJS_i$.

Given the above structure, equation (1) can be consistently estimated by OLS (or its single-equation equivalent) as long as the vector of shocks to search costs, ε_i, is uncorrelated with shocks to re-employment rates, μ_i, where the latter may include a permanent person-specific effect, i.e. unobserved "re-employability". Identifying the parameter of interest –γ—in the presence of correlation between ε_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 dataset. 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.

[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.

Finally, in the case where ε_i and μ_i are uncorrelated, consider estimating equation (1) excluding measures of "traditional" search methods, 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:

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

where $\phi_j = -c_j$, i.e. the marginal effect of internet use on the use of "traditional" search method j for a total-search-cost-minimizing individual, estimated from a linear probability model for each of the 9 "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 in the ensuing tables.

4. 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 on line 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 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 re-employment 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*. Columns 1 and 2 also show that internet job search tends to be used in conjunction with public employment agencies, private employment agencies, sending resumes, using ads, and "other active" measures. In fact, while not always statistically significant, use of *every* "traditional" search method is positively associated with internet search, suggesting a complementarity rather than substitutability between these methods.

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 3, we present

[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 on-line 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 re-employment within a month, within two months, or at any time after internet search activity is observed. (In the latter specification, 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.

estimates 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 re-employed –a result that mirrors the common finding of declining re-employment hazards in duration studies.[15] Workers on layoff are more likely to be re-employed 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 re-employed than those who did neither. Persons whose last job was in the private sector fared better in re-employment 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 re-employed 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 re-employed than single women. Married men are however much more likely to be re-employed than married women, possibly reflecting greater geographical search constraints among married women (Crossley, Jones and Kuhn, 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 re-employment for three such methods: 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.

[15] In a previous version of this paper we modelled the effects of left-censoring in our duration data more formally, using a technique introduced by 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, so that these "sector" indicators do not simply subdivide the group who entered unemployment directly from a job.

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 re-employment 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. As noted, if –as our data suggest-- internet and other job search methods are complementary, this latter result is especially strong, since no effect is seen even when we allow workers to adjust their use of "traditional" methods when the internet is incorporated into their search strategy. 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.

5. 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 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 re-employment occurred; in some cases (the gap between the two four-month CPS observation "windows"), we only know that re-employment occurred during an eight-month period. This makes continuous-time duration models highly inappropriate. For 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 re-employment, $\lambda(\tau)$, 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 X_i and estimated parameters β:

$$\lambda(\tau) = \lambda_0(\tau) \cdot \exp(X_i\beta) \tag{5}$$

From assumption (1) it follows that (see Kiefer 1988, pp. 664-665):

$$\log \Lambda_0(t_i) = -X_i\beta + \mu_i \tag{6}$$

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:

$$F(\delta_a + X_i\beta) - F(\delta_b + X_i\beta), \tag{8}$$

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

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

[18] The cdf 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.

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 5 (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 $\underline{V_i}$ 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 $\underline{V_i}$ 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 $\overline{V_i}$ 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 δ be a Tx1 coefficient vector corresponding to the "cut points" between the above intervals. Because the elements of δ correspond to the log of the integrated baseline hazard at the upper end of each interval, and because δ 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:

[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 re-employed during the first interval $\underline{V_i}$ is a vector of zeroes.

[22] If the observation is right-censored, $\overline{V_i}$ is a vector of zeroes.

$$\log L = \sum_{Cens=L} \log[F(\overline{V}_i \delta + X_i \beta)] + \sum_{Cens=0} \log\left[F(\overline{V}_i \delta + X_i \beta) - F(\underline{V}_i \delta + X_i \beta)\right] + \sum_{Cens=R} \log\left[1 - F(\underline{V}_i \delta + X_i \beta)\right]. \quad (9)$$

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 re-employed 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 (5) for the same set of control variables (*X*) used in Table 3 (not all coefficients are reported to save space). 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 re-employment hazards (longer remaining unemployment durations) after that date. Re-employment 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 re-employment. One interesting difference from Table 3 is that the positive partial correlation between home internet *access* and re-employment rates becomes statistically signficant. 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 on line actually entered re-employment more slowly than those who did not, during the period after we observe whether they search on line. Incorporating all the available information on durations in our sample therefore only strengthens the case against an unemployment-reducing effect of internet job search; this is true whether we consider total effects of internet job search, allowing the use of other methods to adjust optimally to the use of the internet, or partial effects that hold all other methods fixed.

6. Discussion

According to our best estimates, internet job search is more common among workers with observed characteristics that are usually associated with faster re-employment. At the same time, holding these observed characteristics constant, unemployment durations are *longer* among workers who look for work on line 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 the signals it sends to employers. Workers might still use this method, however, either because it is so much easier and cheaper than "traditional" methods or because they were 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. A third possibility is that internet job search *does* speed re-employment, 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' re-employability.

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 jobseekers, 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.[23] But what of the possibility of negative selection into internet search on unobservables? We can think of at least four mechanisms that could generate this. First, as suggested by Holzer (1987) in another context,

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[23] 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.

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.[24] Second, and related, is the possibility of private information about re-employability: 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. Third, our data do not allow us to control for UI receipt. If internet searchers are more likely to apply and qualify for UI, this omitted variable might also account for their longer durations.[25]

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 on line.[26]

In sum, unemployed internet job searchers do not become re-employed more quickly than observationally-equivalent unemployed persons who do not look for work on line. 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

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Other omittted variables include children, income and previous unemployment though it is unclear in what direction these might bias our results.

[24] In particular, Holzer suggests that minority youth disproportionately use formal and anonymous job search networks in part due to low access to informal contacts in the world of work, and that their reliance on formal methods in part explains their lower job-finding rates.

[25] We thank an anonymous referee for this suggestion.

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

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 re-employment. Since internet search companies often make both claims simultaneously, some re-evaluation of these claims may be necessary.

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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 on line		Fraction looking for work on line, given home internet access[1]	
	1998	2000	1998	2000	1998	2000
Employed						
- at work	.347	.521	0.071	0.113	0.159	0.183
- absent	.339	.611	0.070	0.105	0.166	0.151
Unemployed						
- on layoff	.165	.396	0.048	0.103	0.176	0.207
- jobseeker	.223	.394	0.150	0.255	0.495	0.541
Not in LF						
- retired	.122	.238	0.003	0.005	0.023	0.021
- disabled	.105	.204	0.014	0.022	0.104	0.097
- other	.319	.465	0.038	0.063	0.090	0.117
Total	.294	.457	0.055	0.089	0.146	0.165

Notes:
1. Does not equal the ratio of previous columns because some individuals without home internet access search on line.

Table 2: Sample means by internet search activity.

	Internet Search Yes	Internet Search 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[1]	0.298	0.289	0.291
Employed 2 months after Computer/Internet Supplement[1]	0.413	0.365	0.375
Employed 12 months after Computer/Internet Supplement[1]	0.646	0.533	0.559*
Observed in Employment, in any post-supplement month[2]	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% 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 3279 non-internet searchers. 1. Share of persons observed at that date 2. Share of all observations

Table 3: Probit Estimates of Internet Search Determinants and Outcomes

Dependent Variable	Looked for Work on Line			Employed One Year Later?		
	(1)	(2)	(3)	(4)	(5)	(6)
Internet job search			0.062	0.035	0.031	-0.005
			(0.095)	(0.107)	(0.097)	(0.109)
Retrospective duration	-0.003	0.007	-0.027*	-0.027*	-0.029*	-0.029*
	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
2000 supplement	0.429*	0.194*	-0.100	-0.105	-0.108	-0.115
	(0.052)	(0.058)	(0.075)	(0.076)	(0.076)	(0.077)
On layoff	-0.109	-0.109	0.328*	0.328*	0.316*	0.315*
	(0.089)	(0.100)	(0.135)	(0.135)	(0.138)	(0.137)
State unemployment rate	0.027	0.015	0.040	0.041	0.034	0.034
	(0.027)	(0.030)	(0.039)	(0.039)	(0.039)	(0.040)
Occupation unemployment rate	-0.087*	-0.062*	-0.044*	-0.044*	-0.044*	-0.043*
	(0.016)	(0.017)	(0.021)	(0.021)	(0.021)	(0.021)
Worked before unemployment	0.164*	0.225*	0.414*	0.415*	0.418*	0.420*
	(0.082)	(0.091)	(0.122)	(0.122)	(0.123)	(0.123)
School before unemployment	0.324	0.281*	0.271*	0.266*	0.258*	0.251*
	(0.084)	(0.092)	(0.121)	(0.121)	(0.122)	(0.122)
Lost job	0.069	0.071	0.027	0.027	-0.010	-0.010
	(0.080)	(0.089)	(0.123)	(0.123)	(0.125)	(0.125)
Temporary job	-0.047	-0.009	-0.195	-0.191	-0.246	-0.241
	(0.096)	(0.105)	(0.142)	(0.142)	(0.145)	(0.145)
Private sector	0.281*	0.244*	0.422*	0.419*	0.440*	0.437*
	(0.110)	(0.121)	(0.148)	(0.148)	(0.149)	(0.149)
Public sector	0.319*	0.302*	0.089	0.087	0.132	0.130
	(0.135)	(0.148)	(0.195)	(0.195)	(0.196)	(0.196)
Self-employed	0.378*	0.413*	0.153	0.151	0.219	0.216
	(0.163)	(0.180)	(0.241)	(0.241)	(0.242)	(0.243)
Age 16-25	0.561*	0.458*	0.571*	0.572*	0.584*	0.585*
	(0.121)	(0.135)	(0.160)	(0.160)	(0.161)	(0.161)
Age 26-35	0.446*	0.451*	0.466*	0.471*	0.443*	0.450*
	(0.116)	(0.129)	(0.153)	(0.154)	(0.155)	(0.156)
Age 36-45	0.311*	0.288*	0.507*	0.511*	0.511*	0.516*
	(0.115)	(0.128)	(0.149)	(0.149)	(0.151)	(0.151)
Age 46-55	0.372*	0.355*	0.242	0.243	0.241	0.243
	(0.119)	(0.132)	(0.155)	(0.155)	(0.157)	(0.157)
Male	-0.033	-0.162*	-0.187*	-0.190*	-0.175	-0.179
	(0.063)	(0.069)	(0.094)	(0.095)	(0.095)	(0.095)
Married	0.140	-0.075	0.032	0.027	0.031	0.024
	(0.104)	(0.117)	(0.151)	(0.151)	(0.153)	(0.153)
Married male	0.180	0.315*	0.336*	0.339*	0.319*	0.322*
	(0.105)	(0.118)	(0.154)	(0.154)	(0.157)	(0.157)
Spouse employed	-0.057	-0.059	-0.081	-0.082	-0.077	-0.079
	(0.093)	(0.104)	(0.135)	(0.135)	(0.137)	(0.137)
Primary school	-1.602*	-1.125	-0.295	-0.286	-0.264	-0.251
	(0.210)	(0.233)	(0.204)	(0.205)	(0.207)	(0.208)
Incomplete high	-1.163*	-0.839*	-0.443*	-0.439*	-0.414*	-0.407*
	(0.094)	(0.104)	(0.144)	(0.144)	(0.147)	(0.147)
Complete high	-0.859*	-0.532*	-0.200	-0.190	-0.170	-0.160
	(0.077)	(0.085)	(0.126)	(0.127)	(0.129)	(0.130)
Incomplete college	-0.361*	-0.190*	-0.087	-0.083	-0.061	-0.055
	(0.080)	(0.088)	(0.136)	(0.136)	(0.138)	(0.138)
Associate degree	-0.270*	0.005	0.153	0.157	0.171	0.177
	(0.110)	(0.122)	(0.187)	(0.187)	(0.190)	(0.190)
Black	-0.270*	0.026	-0.267*	-0.260*	-0.280*	-0.272*
	(0.070)	(0.079)	(0.095)	(0.095)	(0.096)	(0.097)
Hispanic	-0.225*	0.069	0.008	0.016	0.007	0.017

	(0.087)	(0.097)	(0.114)	(0.115)	(0.116)	(0.117)
Home owner	0.080	-0.172*	0.071	0.067	0.077	0.071
	(0.052)	(0.059)	(0.078)	(0.079)	(0.079)	(0.080)
Immigrant	-0.010	-0.064	0.011	0.008	0.057	0.051
	(0.088)	(0.096)	(0.117)	(0.117)	(0.118)	(0.118)
Contact employer	0.094	0.092			0.159*	0.160*
	(0.053)	(0.058)			(0.079)	(0.079)
Contact public employment agency	0.196*	0.338*			0.257*	0.262*
	(0.062)	(0.069)			(0.097)	(0.098)
Contact private employment agency	0.283*	0.284*			0.247	0.249
	(0.091)	(0.101)			(0.153)	(0.153)
Contact friend/relative	0.029	0.068			-0.146	-0.143
	(0.074)	(0.082)			(0.111)	(0.111)
Contact school employment agency	0.048	0.085			-0.245	-0.243
	(0.141)	(0.158)			(0.220)	(0.220)
Sent resumes	0.358*	0.372*			0.220*	0.220*
	(0.051)	(0.056)			(0.077)	(0.077)
Check union	0.049	0.016			-0.139	-0.142
	(0.167)	(0.186)			(0.292)	(0.292)
Used ads	0.304*	0.340*			-0.158	-0.157
	(0.067)	(0.075)			(0.108)	(0.108)
Other active	0.441*	0.363*			0.281	0.284
	(0.102)	(0.112)			(0.179)	(0.180)
Constant	-1.407*	-2.119*	-0.517	-0.531	-0.744*	-0.766*
	(0.226)	(0.252)	(0.308)	(0.309)	(0.320)	(0.322)
Home Internet Access		1.468*		0.052		0.069
		(0.062)		(0.096)		(0.097)
Log likelihood	-1685.04	-1369.23	-840.15	-840.00	-827.22	-826.97
N	4139	4139	1344	1344	1344	1344

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

Table 4: Re-employment 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	-2038.50	-2031.89	-2026.83	-2021.58

Notes: Regressions include all controls used in Table 3. The sample size for all specifications is 4139.