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Make Money Surfing the Web?
The Impact of Internet Use
on the Earnings of U.S. Workers

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Much research on the "digital divide" presumes that adults who do not use the Internet are economically disadvantaged, yet little research has tested this premise. After discussing several mechanisms that might produce differences in earnings growth between workers who do and do not use the Internet, we use data from the Current Population Survey to examine the impact of Internet use on changes in earnings over 13-month intervals at the end of the "Internet boom." Our analyses reveal robustly significant positive associations between Web use and earnings growth, indicating that some skills and behaviors associated with Internet use were rewarded by the labor market. Consistent with human-capital theory, current use at work had the strongest effect on earnings. In contrast to economic theory (which has led economists to focus exclusively on effects of contemporaneous workplace technology use), workers who used the Internet only at home also did better, suggesting that users may have benefited from superior access to job information or from signaling effects of using a fashionable technology. The positive association between computer use and earnings appears to reflect the effect of Internet use, rather than use of computers for offline tasks. These results suggest that inequality in access to and mastery of technology is a valid concern for students of social stratification.

With the emergence of the Internet as a popular means of communication and information retrieval in the mid-1990s, policy-makers and scholars became concerned about the "digital divide"—the emerging gulf between people with access to the Internet and those without. The literature on the digital divide has grown in size and sophistication: Whereas early work simply documented and tracked inter-group differences, more recent research attempts to explain such differences statistically. Recent research also explores digital inequality *within* the online population in extent and types of use, autonomy of use, and the effectiveness with which desired information can be retrieved (DiMaggio et al. 2004).

Much of this work is motivated by a faith that access to the Internet and the ability to use it effectively is an important form of human capital that influences labor-market success. An early study of the digital divide warned that "the consequences to American society" of racial inequality in Internet access "are expected to be severe" and noted that "the Internet may provide for equal opportunity . . . but only for those with access" (Hoffman and Novak

1998:390). A more recent article makes a similar point: "The 'Digital Divide' may have serious economic consequences for disadvantaged minority groups as information technology skills become increasingly important in the labor market" (Fairlie 2004).

Many policymakers share this faith. For example, the Statement of Findings for Illinois's 2000 "Eliminate the Digital Divide" Act notes the existence of a "digital divide" and asserts as settled fact that citizens who have mastered and have access to "the tools of the new digital technology" have "benefited in the form of improved employment possibilities and a higher standard of life," whereas those without access to and mastery of the technology "are increasingly constrained to marginal employment and a standard of living near the poverty level" (Illinois General Assembly 2000, Section I–5).

Although we have learned much about the nature and causes of inequality in access to and use of the Internet, we know surprisingly little about such inequality's effects on individual mobility. To be sure, there are other reasons to worry about the digital divide: Internet use is becoming necessary for certain kinds of social and political participation and for access to some private markets and government services (Fountain 2001). Ultimately, however, the expectation that people without Internet access are disadvantaged in their pursuit of good jobs and adequate incomes is a central basis for concern about the digital divide. Determining whether this expectation is justified is therefore an important priority for research.

In an era in which rapid technological change has become the norm, the digital divide is significant for students of social stratification as an example of what many believe to be the increasingly important influence of technological access and know-how on social inequality. Tilly (2005:118, 120), for example, asks, "To what extent and how does unequal *control* over the production and distribution of knowledge generate or sustain" inequality? He contends that control over information, science, and "media for storage and transmission of capital, information and scientific-technical knowledge" are "newly prominent bundles of value-producing resources" that have displaced ownership of the material means of production as the primary bases of intergroup inequality.

LIMITATIONS OF EXISTING RESEARCH ON THE EFFECTS OF TECHNOLOGY USE ON EARNINGS

Research on organizations suggests that command of new technologies increases the power and centrality to the labor process of those who possess it. For example, Barley (1986) reported that the introduction of CT scanners in hospital radiology labs enhanced the status and autonomy of technicians trained to use them, empowering such technicians in their relations with senior radiologists, to whom the new methods were unfamiliar. Kapitzke (2000) found similar dynamics when computers were introduced into public schools.

Researchers have not determined, however, whether such increments in power are converted into higher earnings. Indeed, sociologists who study inequality rarely ask whether variation in access to or command of new technologies influences individual life chances. Economists have addressed this question more thoroughly and have found positive impacts of computer use on earnings (Krueger 1993). Very little economic research, though, addresses Internet use. Moreover, most economic studies of effects of technology use on earnings exhibit two shortcomings. First, they usually employ cross-sectional data. Second, they assume that technology use influences income through a single mechanism, specifically the increase in human capital and productivity.

Some economists have called for employing longitudinal data and using other means to counteract the effects of bias inherent in (but not limited to) cross-sectional designs (Card and DiNardo 2002; DiNardo and Pischke 1997). The obvious problem is reciprocity bias: workers may adopt a new technology because they are better paid (and can therefore afford it) rather than being paid better because they use the technology. Cross-sectional studies are also vulnerable to three kinds of selectivity bias. First, employers may choose their highest-quality workers to implement new technologies. Earnings advantages that appear to be caused by the use of new technology may thus reflect unmeasured variation in human capital (Entorf and Kramarz 1997). Second, successful firms with slack resources may adopt new technologies sooner than their less successful competi-

tors *and* pay their employees higher wages (Domes, Dunne, and Troske 1997). Third, firms with skilled (and highly paid) workers can more easily implement technological changes requiring an educated work force than can those with less well-trained employees (Acemoglu 2002). This produces additional opportunities for spurious correlation between technology use and earnings.

The second problem with existing research is that economists have restricted their hypothesis-testing to a single mechanism: technology use increases human capital, which in turn boosts productivity, which leads to higher wages. From a sociological perspective, this view is unnecessarily narrow. Earnings may be determined not only by productivity (correctly appraised) but also by efforts of groups or networks of workers to monopolize access to certain skills (monopolistic closure [Weber 1978:336]), to use social ties to receive disproportionate access to desirable jobs (opportunity hoarding [Tilly 1998]), or to employ culturally embedded status cues to signal virtue and ability (cultural capital [Bourdieu 1986]). (Economists refer to such devices as "rent-seeking" but regard them as less central and ubiquitous features of labor markets than do most sociologists.)

Because of their preoccupation with earnings increases caused by workplace productivity enhancement, economists' empirical efforts have focused almost exclusively on examining the impact on earnings of *current technology use in the workplace.* By contrast, we believe that an exclusive focus on the human-capital/productivity-enhancement mechanism produces three kinds of mischief. First, it leads one to neglect two other mechanisms by which workers may gain earnings advantages: social-capital/information-hoarding (i.e., the use of technology to gain privileged access to information about desirable jobs) and cultural-capital/signaling (i.e., the use of technology to signal positive qualities that the worker may or may not possess). Second, an exclusive emphasis on human-capital/productivity-enhancement leads analysts to rely on measures of technology use—current use at work—for which the potential for endogeneity related to employer decisions is greatest, and to neglect measures of technology use that are less likely to be affected by employers (e.g., prior use or use outside

the workplace) and that may affect earnings independently.[1] Third, the focus on current Internet use neglects research indicating that experience leads to more effective use, which suggests that returns to current users should be higher for those with more accumulated experience (Eastin and LaRose 2000; Hargittai 2003).

A confident assessment of the impact of Internet use on earnings requires that we do the following: (1) Go beyond cross-sectional analyses to examine the influence of technology use on earnings change over time. (2) Control for as many individual differences that may be associated with both earnings and technology use as possible, including occupation and industry characteristics. (3) Distinguish between types of Internet use and include independent measures of Internet use at home and in the past, as well as measures of current Internet use on the job.[2]

We take the following three steps to accomplish these goals:

1. *Panel data.* We exploit a fortuitous feature of the Current Population Survey (CPS) to produce a panel with two measures of both Internet use and earnings. Through 2003, the CPS conducted periodic surveys of respondents' use of communications technologies and took multiple measures of respondents' incomes. CPS

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[1] Note that we do not question the importance of the human-capital/productivity-enhancement mechanism. Indeed, we shall argue (on empirical grounds) that it is probably the most important mechanism connecting Internet use to earnings. Nonetheless, we believe that attention to other mechanisms is necessary both to assess the full effect of technology use on earnings and to gain leverage over potential reciprocity bias.

[2] Internet use is, of course, differentiated in many other ways. Jung, Qiu, and Kim (2001) note that Internet users vary markedly on several dimensions of intensity and scope of use, and they produced an index of "Internet connectedness" to tap such differences. DiMaggio and colleagues (2004) likewise distinguish several dimensions of variation among Internet users (degree of access and freedom from surveillance, quality of available technology, skill, social support, and type of use) that they view as predictive of rewards. Exploring such variation is a worthy objective but it is beyond the scope of this article and the capacity of existing data.

empanels respondents for a span of 16 months. Two of their periodic surveys of communications-technology use, in 2000 and 2001, captured several thousand employed respondents toward the beginning and end of their periods of empanelment. It was thus possible to explore the impact of Internet use on earnings change over a 13-month interval. To our knowledge this is the first study to exploit this feature to study the over-time effects of Internet use on earnings.

2. *Controls for other factors affecting income.* Including lagged wages in a wage-determination model helps to correct for selectivity bias, but other factors may influence both technology use and the rate at which wages rise. It is therefore important to include a variety of additional controls and to employ additional means of correcting for possible selectivity bias. The CPS sample's large size enables us to explore differences in the effects of Internet use associated with industry, occupation, and job-specific skill requirements, as well as educational attainment, union membership, gender, race and Hispanic ethnicity, marital status, age, and place and region of residence. We also employ propensity-score matching to address sample selection bias based on observable characteristics of Internet users and nonusers. We use change-score models to address selectivity on unobserved characteristics, the effects of which are not incorporated in the lagged term.

3. *Distinguishing among types of Internet use.* Almost all economic accounts posit that technology-linked wage gains reflect enhanced productivity due to the use of the new technology at work. In contrast, we argue that Internet use may also contribute to earnings by enhancing access to labor-market information and by serving as a signal of status or competence. We use measures of Internet use from the 2000 and 2001 CPS Internet modules to divide our sample into groups of nonusers, consistent users, adopters (nonusers in 2000 who were users in 2001), and *dis*adopters (Internet users in 2000 but not 2001). We also use the CPS to compare the impact on earnings, respectively, of Internet use at work and at home. The latter is less likely to be a product of firm-level decisions than is Internet use at work, and therefore it is less likely to be a function of unmeasured employer characteristics that influence both workplace technology and wages. We believe this is the first earnings study to use both separate meas-

ures of technology use at work and at home, and separate measures of Internet use at two points in time.

The CPS data offer substantial purchase on the relationship between Internet use and earnings for U.S. workers at the turn of the twenty-first century. We first look at the relative earnings gains of consistent Internet users, new adopters, and disadopters (compared to never-users) between 2000 and 2001. Next we explore the effects of Internet use at home as compared to Internet use in the workplace. Finally, after testing several model specifications to examine the models' robustness to differing assumptions, we evaluate the hypothesis that gains result from computer use per se rather than from Internet use. First, though, we discuss in more detail the mechanisms—human-capital/productivity-enhancement, social-capital/information-hoarding, and cultural-capital/signaling—that might lead us to expect, and enable us to explain, an association between Internet use and wages.

EXPLAINING THE RELATIONSHIP BETWEEN INTERNET USE AND EARNINGS

Why might we expect to find positive empirical associations between Internet use and earnings (and, more generally, between technology use and socioeconomic achievement)? Whereas most work in economics focuses on mechanisms that link technology use to worker productivity and thence to earnings (summarized below under the heading of "Internet Use as a Form of Human Capital . . . "), we describe additional mechanisms that link technology use to better labor-market information and social networks ("social capital/information-hoarding") and to a worker's ability to establish a positive face (Goffman 1955) before potential and actual employers ("cultural capital/signaling").

SKILL ONLINE

We begin by anticipating an objection from Internet-savvy academic readers to our focus on long-term use and home use. Even if new employees have not used the Internet at home or in a previous job, can they not pick up the necessary skills quickly? Finding information

and communicating with other people online, after all, is not rocket science.

This objection underestimates the strangeness of cyberspace to neophytes, the difficulty of mastering online search and communication skills for workers without previous experience, and the range of competencies that Internet use entails. New users must (1) understand graphic conventions prevalent in web design (e.g., the difference between a list and a drop-down menu) and learn the cues that make it easy for experienced users to tell one from the other; (2) acquire a mental map of the Internet as a "space" across which one can "navigate," and master the instrumentalities (e.g., hyperlinks, URLs, search engines) through which one can do so; (3) learn the basics of online searches (e.g., generating queries that are neither too broad nor too narrow, using Boolean operators to refine a search); (4) acquire information about the uses and reputations of major Web sites; (5) develop skill in distinguishing between trustworthy online information sources and amateurish or misleading sites; and (6) master the pragmatics of online communicative competence (e.g., knowing when it is appropriate to contact a stranger or participate in an online forum, the appropriate formality of address, appropriate message length and content, and use of abbreviations and emoticons) (Van Dijk 2005, chap. 5; Warschauer 2003).

Not surprisingly, research demonstrates that new users are less effective and more scattered in their use of the Internet than more experienced users. A psychological study of Internet use concluded that most people take at least two years to become competent at finding information online (Eastin and LaRose 2000). The most comprehensive sociological study of online skills (Hargittai 2003) found low and variable levels of skill in a random sample of Internet users from a socially heterogeneous county in the Northeast, with years of experience and intensity of use serving as strong predictors of the success and rapidity with which subjects completed a variety of online tasks. In other words, research indicates that effective use of the Internet requires significant training or experience.

INTERNET USE AS A FORM OF HUMAN CAPITAL LEADING TO ENHANCED PRODUCTIVITY

In some occupations in some industries, workers who can use the Internet effectively may perform better than those who cannot, and they will therefore have privileged access to desirable jobs, be rewarded more generously for their performance, or both. According to human-capital theory, a wage premium for Internet use would reflect productivity gains that result from improved access to information, faster and more efficient communication, greater access to learning opportunities, or higher job satisfaction leading to greater job commitment. Krueger's (1993) classic study of the effects of computer use on earnings reported that workers who used computers earned 17 to 20 percent more than those who did not (see also, Autor, Katz, and Krueger 1998; in the United Kingdom, Dickerson and Green 2004). Two rare studies of Internet users, employing cross-sectional data on workplace Internet use, reported a 13.5 percent premium in 1998 (Goss and Phillips 2002) and a 14 percent premium (controlling for computer use) in 2001 (Freeman 2002). The economic theory of skill-biased technological change suggests that such wage premiums are temporary, because employers adopt new technologies that require them to increase the ratio of skilled to unskilled workers only when the former are relatively plentiful (Acemoglu 2002), and saturation of demand eventually causes returns to flatten or decline.

In many technologically oriented industries, familiarity with the Internet is necessary to obtain a job in the first place. For example, some auto-parts distributors provide job training for new salespersons only offsite over the Internet. The ability to retrieve information online is an important part of many workers' daily routines: secretaries, for example, use the Internet to retrieve contact information, find references to research reports, organize meetings, and locate statistical data. Indeed, the Department of Labor's *Occupational Outlook* includes "conduct research on the Internet" in the job description for secretaries (Levy and Murnane 2004:4). Some workers, such as customer service representatives who respond to online inquiries, or purchasing agents who trawl through business-to-business ecommerce sites, may spend most of their working time online.

The theory of skill-biased change implies that highly educated workers are most likely to benefit from new technologies. But as Autor, Levy, and Murnane (2003) note, the critical feature of jobs in which occupants benefit from technological change is not skill per se but impediments to routinization. Drivers for many trucking fleets, for example, use the Internet to receive information about route changes, report deliveries, and maintain contact with their home offices (Nagarajan, Bander, and White 1999). Police departments frequently issue officers laptops to report and receive information about crimes and other matters over dedicated wireless networks (Downs 2006). Some universities require custodial staff to log in for assignments at the beginning of the work day. The ability to use the Internet (or intranets based on Internet technology) may thus be necessary even for blue-collar or service workers if their jobs cannot easily be routinized.

Technology use also may be associated with higher wages if firms invest in worker human capital when they implement new technologies. For example, implementation of online inventory-management plans may be associated with intensive employee training and skill-enhancing reorganization of the labor process (Fernandez 2001).

INTERNET USE AS A SOURCE OF SOCIAL CAPITAL

The Internet may also intervene in the earnings-determination process by facilitating the expansion and exploitation of social networks (Lin 2001). Internet users may benefit from three kinds of social-capital enhancement. First, they can use the Internet to search online job listings or post their résumés. A 2006 survey reports that almost one in four workers who use computers at work have used them to search for new jobs (Hudson Employment Index 2006). Such workers are likely to learn about many more openings than would otherwise come to their attention. Second, when online activities lead workers to expand their personal social networks, incidentally created new ties may provide access to informal information about job opportunities within or outside the firm (Fountain 2005; Hampton and Wellman 2000). Online communications may also complement rather than substitute for face-to-face relationships.

For example, the first author interviewed a sales representative who found a better job when a professional acquaintance he had not seen in years stumbled upon his résumé on an online employment site. Third, employees with large, accessible professional networks may use technology to employ these networks in ways that benefit their employers: for example, getting useful information, contacting clients, or setting up collaborative ventures.

Efforts to assess the impact of Internet search methods on employment outcomes have focused on low-income job-seekers and yielded inconsistent results. In a study of 662 unemployed persons tracked by CPS in 1998 and 2000, Fountain (2005) found that Internet searchers were more likely to find jobs in 1998 but not in 2000. Using similar CPS data, Kuhn and Skuterud (2004) found no contribution of Internet searching to job placement. In contrast, a 2003 study of Florida welfare recipients who had moved into the labor market reported that Internet search intensity (but not offline search intensity) was significantly associated with both earnings and benefits (McDonald and Crew 2006).

INTERNET USE AS CULTURAL RESOURCE AND SIGNAL

Throughout modern history, new technologies have galvanized the popular imagination, entered into everyday language and literature, and provided prisms through which actors have experienced and interpreted their times. In the age of railways, the locomotive was a metaphor for driving force. Henry Adams famously used the "dynamo" in his *Autobiography* to symbolize American society during the industrial revolution. Children's author James Braden's *Auto Boys* series drew on (and contributed to) the motor craze of the early twentieth century. Sinclair Lewis's *The Flight of the Hawk* documented the heady social world of aviation as that technology emerged in the 1920s. In each instance, cultural enthusiasm accompanied financial speculation to create a boom with material and symbolic dimensions. The commercialization of the Internet in the second half of the 1990s reproduced this pattern once again (Castells 2001; Turner 2006).

Significant emerging technologies possess a cachet that marks their users as capable, adapt-

able, and well informed. When this occurs, a wage premium may reflect both the symbolic value that employers attach to familiarity with the technology and the personal qualities (competence, resourcefulness, intelligence) of which they take it to be a signal (Weiss 1995), especially where direct evidence of those qualities is difficult to come by. Technology use may also serve as a kind of "cultural capital": familiarity with high-status objects or activities that makes it easier for people to form relations with high-status others and leads gatekeepers to evaluate them favorably (Bourdieu 1986; DiMaggio 2004).

Our data were collected toward the end of the Internet boom (but before the Internet bust), when many Americans regarded the Internet as a transformative force that would ignite explosive economic growth. Internet use had spread widely in the population (our analyses of CPS data indicate that approximately seven in ten employed American adults used the Internet at some location in 2001), but the technology was not so common in the workplace that it could be taken for granted (just 45 percent used it at work). Some of the Internet's prestige may have

attached itself to workers who seemed knowledgeable about the new technology.

Some evidence supports the view that employers regarded Internet users as especially able. Niles and Hanson (2003:1236) report that some employers used Internet job postings to weed out low-quality applicants, whom they presumed would not be online. An experimental study of the impact of race and other factors on employer responses to otherwise randomized résumés reports that (fictional) applicants with e-mail addresses on their résumés received significantly more calls for interviews than similar applicants without them (Bertrand and Mullainathan 2004).

Temporal specificity of causal mechanisms. Rewards to technological competence are likely to change systematically over the lifecycle of a technological innovation. Our data were collected at the end of a period of very rapid diffusion, just as the rate of increase was beginning to decline. Figure 1 describes the change between 1997 and 2003 in the percentage of all non-institutionalized Americans age 18 or older who reported using the Internet at
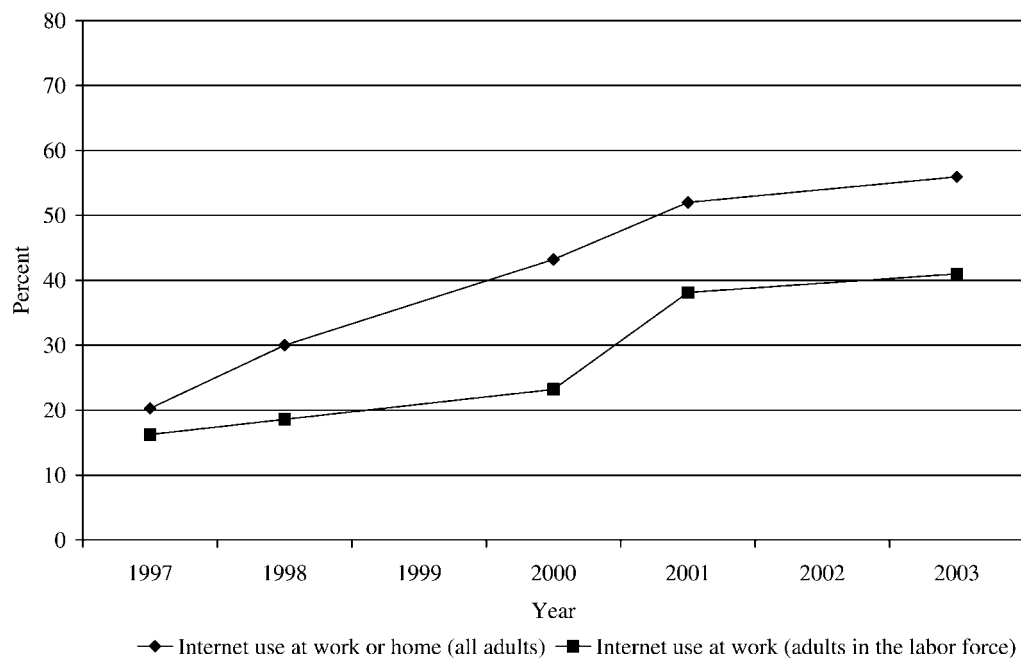

Figure 1. Internet Use, 1997 to 2003

Source: Current Population Survey Internet and Computer Use Supplements, 1997, 2000, 2001, and 2003.

any location and the percentage of employed Americans who reported using the Internet at work. Penetration in the U.S. population grew slowly through 1997 (not shown), then took off, rising from 20 percent in 1997 to 52 percent in 2001. Internet use in the workplace, by contrast, grew slowly until 2000, jumped sharply from 23 to 38 percent between 2000 and 2001, and then leveled off.

The effects of competence in a new technology should initially increase as firms make the capital investments necessary to exploit such competence. The effects should then decline as relevant skills saturate the workforce. Even if the ability to find information effectively or to engage in transactions online enhances worker productivity, such skill will no longer give a worker a competitive edge once everyone has it (Aghion and Howitt 2002). The same is true of the competitive aspect of the social-capital mechanism (although a general improvement in worker-job matches through more effective information diffusion could lead to higher wages overall). Similarly, the efficacy of Internet use for signaling is also likely to have been time-limited. By 2002, the Internet boom had turned into a bust, and Internet use became common even among moderately educated workers. Being conversant with the latest technology may always serve as a form of cultural capital in some work settings, but particular technologies may move in and out of fashion relatively quickly. Consequently, the analyses that follow reflect the way that labor markets operated in 2000 and 2001; results should not be generalized to later periods.

HYPOTHESES

The three mechanisms described above do not map neatly onto specific indicators of Internet use. Nonetheless, we can use information about the impact of different indicators to derive insights into the relative importance of each. If Internet use raises income by boosting productivity, only workers who use the Internet on the job will benefit. By contrast, insofar as the Internet operates through social-capital enhancement or signaling, using the Internet at home may independently affect earnings. Indeed, workers who are no longer online could derive such benefits from past Internet use.

For all of the reasons described above, we anticipate the following:

Hypothesis 1: The net earnings of Internet users rose faster in the period under observation than the earnings of workers who did not use the Internet.

Not all forms of Internet use will have equally strong effects. We anticipate that the net earnings of workers who used the Internet in 2000 *and* 2001 rose faster than workers who reported using it in only one of those years. Based on research on growth in the efficacy of use over time, we predict the following:

Hypothesis 2: The net earnings of workers who used the Internet in 2001 but not 2000 grew faster than the earnings of nonusers but less quickly than the earnings of more experienced Internet users.

Workers who stopped using the Internet between 2000 and 2001 may have benefited from signaling effects and social-capital effects. As current nonusers, though, they lack the human-capital advantage derived from using new technology at work. We therefore expect the following:

Hypothesis 3: The net earnings of workers who used the Internet in 2000 but not in 2001 grew faster than the earnings of nonusers but less quickly than the earnings of persistent Internet users.

Insofar as social-capital and cultural-capital mechanisms operate to link technology use to higher earnings, we would expect to see benefits for workers who used the Internet at home, as well as for those who used it at work. Using the Internet both at home and at work is likely to be especially advantageous. Use at work nets human-capital benefits; use on one's own may be more influential for signaling. Workers have more freedom to peruse job postings and to expand networks through casual interaction at home. And skills in searching and other activities are likely honed through technology use at home as well as at work. These points lead us to our final hypothesis:

Hypothesis 4: Net earnings of workers who used the Internet only at home or only at work grew faster than the earnings of nonusers but less quickly than earnings of

workers who used the Internet at both work and home.

DATA

We rely on data from the Current Population Survey (CPS), a monthly household survey fielded continually by the Bureau of the Census and based on stratified probability samples of the non-institutionalized U.S. population. Each household in the CPS is interviewed in two sequences of four consecutive months, separated by an eight-month hiatus, for a total of eight interviews over 16 months. Every month, one-eighth of the sample is replaced by new households with similar characteristics. This rotating sampling design permits comparisons of households across time, as three-quarters of respondents are the same in any two consecutive months and half of the respondents are the same after 12 months. This design feature, combined with the large sample size, makes the CPS uniquely useful for our purposes.

In addition to core employment and demographic modules, the CPS periodically included special supplements on information and communications technology. We take advantage of the fact that the CPS included such supplements in August, 2000 and September, 2001. Data on technology use were collected between August 13 and August 19, 2000 and again between September 16 and September 22, 2001. The 2000 wave comprised 47,673 households and 121,745 individual responses, while the 2001 wave comprised 56,634 households and 143,300 individual responses. Of the households in 2001, we found that 15,758 had also participated in the 2000 supplement, yielding 37,288 individual records. After excluding non-civilians, respondents under age 18 and over 65, those outside the labor force, respondents who reported variable hours worked, and those who earned less than half of the federal minimum wage, 9,446 individual cases remained.[3]

Although the CPS's panel structure makes it uniquely appropriate, it is not perfect. The major limitation is that it cannot be used to estimate the relationship between Internet use, job change, and earnings.[4] Nor does it include data

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third were in their 16th month (or had their eighth interview) in September 2001. Hence, we were forced to rely on earnings data collected in the months immediately following the Internet supplements. In 2000, all of our income data were collected after the August information technology module, in September, October, and November; in 2001, two-thirds were collected after the administration of the September 2001 information technology module, in October and November. This feature of the CPS data requires us to assume that respondents' typical weekly income would have been the same in the month of the Internet supplement as it was a month or two later. Chow tests on coefficients from separate analyses of earnings data collected in September, October, and November gave us confidence in this assumption. Coefficients for dummy variables representing (a) Internet use in both 2000 and 2001 and (b) Internet use in 2001 but not 2000 were virtually identical across pairs of months. Coefficients for a dummy variable representing the relatively few respondents who reported using the Internet in 2000 but not in 2001 were different (the hypotheses that the effects for the September and November samples were the same as for the October sample were rejected with probabilities of $p = .028$ and $p = .030$, respectively) but non-monotonically so, with effects on income positive and significant for the September and November samples but negative and non-significant for the October sample. Based on these analyses, we believe that using income measured in October and November 2001 as a proxy for September income may have slightly diluted the effects of Internet use for those respondents who used the Internet in 2000 but not in 2001, but it did not materially affect the conclusions of this study.

[4] During the period spanned by our panel, the CPS only collected detailed job change information in its February 2000 Job Tenure and Occupational Mobility Supplement. Because our data span August to November 2000 and September to November 2001, and because the job change question refers to the previous year, it is impossible to know if the change occurred before or after the collection of our first-period data. The basic CPS survey that accompanies the Internet use supplements does gather information about the respondents' movements into and out of broad occupation and industry categories, but these items fail to capture the majority of job changes that occur within occupations and industries.

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[3] Individual earnings data in the CPS are collected only from outgoing rotation groups (households completing their fourth or eighth interview), which constitute one-fourth of the respondents in any given month. None of the respondents who took part in both waves of the Internet supplement were in the fourth month of their rotations in August 2000, and only one-

on employers. The CPS also suffers from excessive use of imputation and proxy respondents, but our ability to control for the effects (see the Appendix) of these features renders these problems manageable.

Table 1 reports rates of Internet use in 2001 by sociodemographic categories for persons in

our sample.[5] (Because the analysis is restricted to employed persons ages 18 to 65, usage rates are higher than for the population at large.)

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[5] Respondents were asked, "Does anyone in this household use the Internet from home?" (2000) or

Table 1.   Group-Specific Rates of Internet Use in 2001 (unweighted counts)

	N	Anywhere (percent)	Work (percent)	Home (percent)	Work and Home (percent)	Work or Home (percent)
Gender						
Male	4,793	66.7	41.1	55.6	31.6	65.1
Female	4,653	73.7	48.2	57.5	33.5	72.2
Race/Ethnicity						
White	8,168	72.3	46.1	59.0	34.2	70.8
Black	836	52.9	32.2	35.9	17.3	50.7
Asian	342	67.5	44.7	56.4	34.5	66.7
American Indian, Aleut, or Eskimo	100	48.0	26.0	33.0	14.0	45.0
Hispanic	772	41.3	24.7	31.9	16.6	40.0
Age						
18 to 25	830	72.2	27.4	58.8	19.8	66.4
26 to 35	1,953	73.8	47.4	59.4	34.8	71.9
36 to 45	2,991	71.4	46.8	58.5	35.1	70.2
46 to 55	2,628	70.4	47.6	56.4	34.4	69.6
56 to 65	1,044	57.7	39.2	44.4	26.3	57.2
Education						
Less than high school	693	23.8	7.5	18.2	3.6	22.1
High school	4,752	62.8	32.1	49.4	20.7	60.8
Associate	1,037	76.0	45.6	60.6	31.9	74.3
College	1,960	89.7	70.8	74.0	55.8	88.9
Advanced	1,004	92.9	77.1	78.8	63.6	92.3
Region						
Northeast	2,092	72.9	44.0	60.3	32.6	71.7
Midwest	2,601	72.7	45.8	58.4	33.6	70.6
South	2,686	65.8	42.9	51.9	30.5	64.3
West	2,067	70.0	46.0	56.4	33.8	68.6
Metropolitan Status						
Metropolitan	7,336	72.1	46.6	58.8	34.7	70.7
Non-metropolitan	2,077	63.6	37.7	48.8	25.0	61.4
Not identified	33	60.6	30.3	48.5	24.2	54.6
Industry						
Agriculture, forestry, fishing, and mining	170	52.4	23.5	44.7	17.1	51.2
Construction	581	50.3	19.3	43.6	14.1	48.7
Manufacturing–durable	935	64.5	43.2	51.3	31.4	63.1
Manufacturing–nondurable	589	61.1	39.7	50.4	30.6	59.6
Transportation	413	60.3	25.2	49.6	17.7	57.1
Communications	160	86.9	68.1	65.0	46.9	86.3
Utilities and sanitary services	149	69.1	49.7	59.1	39.6	69.1
Wholesale trade	397	67.5	45.1	52.6	31.7	66.0
Retail trade	1,225	61.2	24.8	51.7	17.3	59.2
Finance, insurance, and real estate	655	83.4	65.3	63.5	46.0	82.9
Business, auto, and repair services	501	73.7	51.7	59.3	39.7	71.3

(continued on the next page)

Table 1. *(continued)*

	N	Anywhere (percent)	Work (percent)	Home (percent)	Work and Home (percent)	Work or Home (percent)
Personal services	218	54.1	21.6	45.0	13.8	52.8
Entertainment and recreation services	116	69.8	33.6	61.2	27.6	67.2
Hospitals	525	75.2	45.3	60.8	31.8	74.3
Medical services (excluding hospitals)	471	65.6	30.8	52.9	21.0	62.6
Educational services	1,109	84.8	63.8	69.3	49.8	83.4
Social services	203	69.0	38.9	49.8	23.2	65.5
Other professional services	437	89.9	75.1	72.3	57.9	89.5
Public administration	592	81.6	64.2	61.0	44.3	80.9
Occupation						
Executive, administrative, and managerial	1,487	88.4	73.7	70.8	56.6	88.0
Professional specialty	1,775	89.1	69.8	74.1	55.7	88.2
Technicians and related support	391	82.9	55.5	65.5	39.6	81.3
Sales	879	72.4	43.1	59.4	31.2	71.3
Administrative support (including clerical)	1,529	79.1	53.2	58.2	33.6	77.8
Service	991	48.3	13.4	40.2	8.7	44.9
Precision production	1,069	52.5	20.3	44.1	13.8	50.5
Mach. operat., assemb., and inspect.	483	38.7	11.8	32.3	7.5	36.7
Transportation and material moving	405	40.3	5.4	34.6	2.2	37.8
Farming, forestry, and fishing	111	30.6	9.9	26.1	7.2	28.8
Handlers, equip. cleaners, laborers	326	42.0	8.3	34.4	4.0	38.7
Total	9,446	70.2	44.6	56.6	32.5	68.6

Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001, employed persons, ages 18 to 65.

More than two-thirds (70 percent) of the respondents reported using the Internet in 2001, up from 61 percent in 2000. Usage varied by race, with whites reporting the highest rates (72 percent), followed by Asian Americans or Pacific

"Does anyone in this household connect to the Internet from home?" (2001). In 2000, they (or the person who answered on their behalf) were then asked a series of questions about how they used the Internet at home and whether they used the Internet at work or at another location. Persons were coded as using the Internet in 2000 if they reported any use at home (in response to a list that ended with use for "any other purpose"), at work, or at another location outside the home. In 2001, the survey asked point blank about use of any kind at home as well as use at work or another location outside the home. Again, persons who used the Internet at any of these locations were coded as using the Internet. Respondents who reported (or were reported as) using the Internet at home for any of the options (including "any other") in 2000, or who reported (or were reported as) using the Internet at home in 2001, were coded as home users. Those who reported using the Internet at work were coded as workplace users.

Islanders (68 percent), African Americans (53 percent), and American Indians, Aleuts, or Eskimos (48 percent). Hispanics reported a lower rate than non-Hispanics, 41 and 73 percent respectively. Respondents between ages 26 and 35 reported the highest usage of any age cohort (74 percent); those between ages 56 and 65 used the Internet the least (58 percent). Women were more likely than men to use the Internet (74 percent compared to 67 percent), an advantage among working Americans that contrasts with CPS figures for the entire adult public (55 percent for both women and men). The difference reflects women's advantage in at-work Internet use (48 versus 41 percent) and suggests that increased workplace technology use was responsible for eliminating a gender gap that gave men an advantage during the Internet's early years (Ono and Zavodny 2003).

Consistent with previous research, educational attainment was strongly and positively associated with Internet use, with rates ranging from 24 percent for workers who had not finished high school to 93 percent for those with advanced degrees. Usage rates were lowest in the South (66 percent) and highest in the

Midwest and the Northeast (73 percent). Workers who lived in Metropolitan Statistical Areas (MSAs) were online more than non-metropolitan residents (72 versus 64 percent).

Internet use rates varied notably by industry, ranging from just 50 percent in the construction trades to 90 percent in "other professional services." (Not surprisingly, rates for agricultural and personal-service workers were near the bottom, whereas rates in the communications and education industries were close to the top.) Variation was even greater among occupations, with just 31 percent of laborers in extractive industries going online, compared to 89 percent of professionals.

Finally, 81 percent of Internet users (57 percent of all respondents in the labor force) used the Internet at home, 64 percent (45 percent of all respondents) used it at work, and 46 percent (33 percent of all respondents) used it at home *and* at work. Over 98 percent of users were connected at home or work; the rest went online at a library, community center, school, or friend's or relative's home.

RESULTS

We first present OLS regression models in which the dependent variable is logged hourly earnings in 2001.[6] We compare change in wages of Internet nonusers to continuous Internet users, adopters, and disadopters. Then we compare nonusers to workers who used the Internet only at work, only at home, and at home *and* work. After reporting results of several robustness tests, we distinguish the impact of Internet use from that of using stand-alone computers.

DOES INTERNET USE SIGNIFICANTLY PREDICT EARNINGS IN 2001 (NET OF 2000 EARNINGS)?

Internet use is measured at any location in 2000 and 2001. Separate dummies represent respondents who used the Internet in both years (Y–Y [for Yes–Yes] in Table 2; 55 percent of the sam-

ple); adopters, those who did not use the Internet in 2000 but did in 2001 (N–Y [for No–Yes]; 16 percent of respondents); and disadopters, users in 2000 who were nonusers in 2001 (Y–N [for Yes–No]; 7 percent of respondents). (Consistent with other studies, but contrary to popular belief, the Internet user population is characterized by considerable flux [Katz and Aspden 1997; Lenhart et al. 2003]. The proportion of disadopters in our sample of employed persons is lower than that for the CPS as a whole.) The omitted category includes respondents who reported using the Internet in neither year (23 percent). Because all models control for lagged (2000) income, coefficients indicate effects on *net* wages over a period of approximately 13 months.[7]

Positive effects of Internet use on earnings are significant and robust to the inclusion of a wide range of controls. Model 1 includes the Internet use measures and lagged wages.[8] The effects of all kinds of Internet use are highly significant, but the coefficient for respondents who used the Internet in both periods exceeds those for recent adopters or disadopters. Model 2 adds controls for race and Hispanic ethnicity, gender, age (and age squared), marital status, educational attainment, region of residence, and metropolitan residence, reducing the impact of continual use by 41 percent and the advantage of both adopters and disadopters by about 27 percent. Adding controls for industry and occupation (Model 3) reduces the effect for continual users by another 25 percent, for adopters by 22 percent, and for disadopters by 27 percent.[9]

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[6] Earnings were reported for the primary job. Hourly employees could report hourly earnings. Others reported hours worked in a typical week and typical weekly earnings. The latter was divided by the former to yield an hourly wage.

[7] Coefficients for controls are reported in Table S2 of the Online Supplement on the *ASR* Web site: http://www2.asanet.org/journals/asr/2008/toc062.html. For a list of control variables used in each model, see the note for Table 2.

[8] All models also include controls for dichotomous variables indicating (a) whether data on hours or earnings were imputed in either wave and (b) whether the case includes a proxy flag indicating that another household member answered on behalf of the respondent, as well as a multiplicative term for imputation*earnings (to correct for underestimation of the lagged earning effect and for the possibility that control variables might be overestimated). See the Appendix for a thorough discussion.

[9] Both industry and occupation categories include numerous job titles that are heterogeneous with respect to the skill required of incumbents. If Internet

Table 2. Regression of Logged Wages in 2001 on Internet Use at Any Location

	N	Model 1	Model 2	Model 3	Model 4
Internet 2000 to 2001: Y–Y[a]	5,156	.148***	.087***	.065***	.061***
		(.011)	(.011)	(.012)	(.015)
		.132	*.078*	*.058*	*.055*
Internet 2000 to 2001: N–Y	1,471	.070***	.051***	.040**	.036*
		(.014)	(.013)	(.013)	(.017)
		.046	*.033*	*.026*	*.024*
Internet 2000 to 2001: Y–N	625	.086***	.063***	.046**	.046**
		(.018)	(.018)	(.017)	(.017)
		.039	*.028*	*.021*	*.020*
Computer use 2001: networked and non-networked	7,331				.005
					(.015)
					.004
Intercept		.419***	.189***	.436***	.435***
		(.028)	(.056)	(.059)	(.059)
N		9,446	9,446	9,446	9,446
Adjusted R^2		.486	.529	.555	.555

Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001.

Notes: Income and hours worked were obtained from Basic CPS data collected in September, October, and November, 2000 and 2001. The analysis excludes non-civilians, respondents under age 18 and over 65, people out of the labor force, those with varying weekly work hours, and those who earned less than half of the federal minimum wage in 2000 or 2001. Control variables are omitted in the interest of parsimonious presentation. Model 1 controls include earnings in 2000 (logged), proxy and imputed response dummies, and an earnings by imputation interaction. Model 2 controls include those from Model 1, plus union membership, gender, race and ethnicity, age, education, marital status, metropolitan status, and region. Models 3 and 4 controls include those from Model 2 plus industry and occupation. Coefficients are followed by standard errors in parentheses and beta weights in italics. Y–Y refers to respondents who used the Internet at any location in both 2000 and 2001. N–Y refers to those who used it in 2001 but not in 2000 (adopters). Y–N refers to respondents who used the Internet in 2000 but not in 2001 (disadopters).

[a] Internet and computer use variables measure use anywhere (home, work, or other locations).

* $p < .05$; ** $p < .01$; *** $p < .001$ (one-tailed tests for Internet use coefficients, two-tailed tests for all other variables).

use is highly correlated with job skill requirements, and if these requirements are associated with changes in wages, then omitting them from the model may lead us to overestimate the impact of Internet use on net wages. We therefore ran an additional model (not reported here but available on request) in which occupation dummies were replaced by a novel set of occupational skill ratings: skill in managing interpersonal relations, exerting physical strength, obtaining information, working independently, having a strong achievement orientation, being socially perceptive, analyzing data, and updating and using relevant knowledge. Skill ratings were obtained from the Occupational Information Network (O*NET), successor to the Dictionary of Occupational Titles, and made applicable to the CPS occupation codes by a multistep crosswalk developed by the second author (details available on request). Replacing categorical measures of occupation with these new measures left coefficients for Internet use indicators virtually unchanged.

The remaining net earnings advantage of continuous users is statistically significant at $p < .001$ (one-tailed), consistent with Hypothesis 1. Adopters' and disadopters' advantages are significant at $p < .01$. In dollar terms (based on coefficients in Model 3), the median earner who used the Internet in both years was paid $.96 per hour more than a comparable nonuser. The wage premium for a median earner who adopted Internet use in 2001 was $.58; and median earners who stopped using the Internet between waves received a premium of $.67. Consistent with Hypothesis 2, Wald tests for difference in coefficients (available on request) indicate that continuous users gained significantly more than 2001 adopters ($p < .05$). In these analyses, however, Hypothesis 3 is disconfirmed, as effects for continuous users and disadopters are not significantly different. As

noted below, robustness tests suggest that the disadopter coefficient is inflated.

We draw three tentative lessons from these results:

1. Internet users gained significantly more in earnings than nonusers. These gains persisted despite the inclusion of numerous control variables. They were also independent of the effects of unmeasured characteristics of worker and job, the effects of which were incorporated in logged earnings as measured in the fall of 2000.
2. The advantage of workers who used the Internet in both years over recent adopters indicates that experience and accumulated skill mattered.
3. The fact that disadopters continued to do better than workers who never used the Internet may be attributable to some combination of cultural-capital effects, job skills or information acquired before disadoption, and unmeasured correlates of disadopter status that influenced the slope of earnings between August 2000 and September 2001.

If social-capital/information-hoarding and cultural-capital/signaling mechanisms provide an income advantage to Internet users, then workers who use the Internet at home but *not* at work should also do better than workers who do not use the Internet at all. It is also important to assess the effects of Internet use at home because home use is far less likely to be influenced by employer decisions than is Internet use at work. We explore this possibility in the next section.

DID INTERNET USE AT HOME INDEPENDENTLY BOOST EARNINGS?

If the effects of Internet use reflect only unmeasured differences that influence the rate of earnings growth between Internet users and other workers, or if they reflect a cultural-capital or signaling effect rather than enhanced productivity, we would expect workers who used the Internet only at home to boost their net wages as much as those who used the Internet on the job. This was not the case.

If the association between Internet use and earnings reflects *only* a tendency for wealthy firms to implement new technologies first *and* to pay high wages to their employees, then workplace Internet use would make all the difference and Internet use at home would have little effect on wages. This, too, was not the case.

Results appear in Table 3. Separate dichotomous variables identify respondents who used the Internet at home *and* work in at least one year (37 percent of all workers), respondents who used the Internet only at home (26 percent), and those who used the Internet only at work (13 percent). Nonusers (23 percent) are the omitted category. For the sake of parsimony, the few respondents who used the Internet only at a location other than home or work are omitted. The models control for lagged income, so coefficients indicate influence of Internet use on net wages over a period of approximately 13 months.

All groups of users earned significantly more (net earnings in 2000 and other controls) than nonusers in 2001 (Model 1). Those who used the Internet at home *and* work gained the highest returns, followed by those who used the Internet at work but not at home, and those who used it at home but not at work. Controlling for race and Hispanic ethnicity, gender, age (and age squared), marital status, educational attainment, region of residence, and metropolitan residence (Model 2) reduces the coefficient for Internet use at home *and* work by 37 percent. The effect of home-only use declines by 42 percent, and that of work-only use by 25 percent. Introducing controls for industry and occupation (Model 3) further reduces the home *and* work effect by 24 percent, the work-only coefficient by 30 percent, and the home-only effect by just 5 percent. All remain positive and statistically significant.

Consistent with Hypothesis 4, all user groups increased earnings significantly more than did nonusers. Also consistent, Wald tests indicate that returns to workers who used the Internet at home *and* at work were significantly greater than for those who used the Internet only at work ($p < .01$) or only at home ($p < .001$). Wage premiums (relative nonusers) amounted to $1.40 per hour for median earners who used the Internet at home and work, $.88 for those who used it at work but not at home, and $.52 for home-only users.

We ran an additional model with the same covariates as in Model 3, but with 15 detailed categorical measures indexing Internet use or nonuse at home and work by year, with nonusers omitted. Although Ns for many categories are quite small, the overall pattern of results (available on request) reinforces those in Model 3. Workers who used the Internet at home *and*

Table 3. Regression of Logged Wages in 2001 on Internet Use at Home and Work

	N	Model 1	Model 2	Model 3	Model 4
Internet: home and work (2000 or 2001)[a]	3,486	.198***	.124***	.094***	.094***
		(.011)	(.012)	(.013)	(.016)
		.172	*.108*	*.082*	*.082*
Internet: home only (2000 or 2001)	2,414	.066***	.038**	.036**	.036**
		(.012)	(.012)	(.011)	(.014)
		.052	*.030*	*.028*	*.028*
Internet: work only (2000 or 2001)	1,186	.115***	.086***	.060***	.060***
		(.014)	(.014)	(.015)	(.017)
		.069	*.051*	*.036*	*.036*
Computer use 2001: networked and non-networked[b]	7,331				.000
					(.014)
					.000
Intercept		.477***	.252***	.463***	.463***
		(.028)	(.056)	(.058)	(.059)
N		9,446	9,446	9,446	9,446
Adjusted R^2		.493	.532	.556	.556

Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001.

Notes: Income and hours worked were obtained from Basic CPS data collected in September, October, and November, 2000 and 2001. The analysis excludes non-civilians, respondents under age 18 and over 65, people out of the labor force, those with varying weekly work hours, and those who earned less than half of the federal minimum wage in 2000 or 2001. Control variables are omitted in the interest of parsimonious presentation. Model 1 controls include 2000 earnings (logged), proxy and imputed response dummies, and an earnings x imputation interaction. Model 2 controls include those from Model 1, plus union membership, gender, race and ethnicity, age, education, marital status, metropolitan status, and region. Models 3 and 4 controls include those from Model 2 plus industry and occupation. Coefficients are followed by standard errors in parentheses and beta weights in italics.

[a] "Internet: home and work (2000 or 2001)" refers to respondents who used the Internet both at home and at work in at least one of the two waves of the survey. "Internet: home only (2000 or 2001)" refers to respondents who used the Internet at home but not at work in at least one of the two waves (and did not use the Internet at work in the other wave). "Internet: work only (2000 or 2001)" refers to respondents who used the Internet at work but not at home in at least one of the two waves (and did not use the Internet at home in the other wave). Respondents who used the Internet at home in one wave and at work in the other were classified based on their Internet usage in 2001.

[b] The computer use variable measures use anywhere (home, work, or other locations).

* $p < .05$; ** $p < .01$; *** $p < .001$ (one-tailed tests for Internet use coefficients, two-tailed tests for all other variables).

work in both 2000 and 2001 (unstandardized coefficient of .116), at home in 2000 and home *and* work in 2001 (.110), and at work in 2000 and home *and* work in 2001 (.112) gained the most. The only groups whose earnings did not increase significantly more than nonusers were those who used the Internet only at work and only in 2000 (likely due to unmeasured job change); those who used the Internet only at home and only in 2001 (whose skills were poorly developed and for whom any signaling value was belated); and those who used the Internet at both locations in 2000 but at neither in 2001. Overall, respondents who used the Internet at home *and* work, including those who added a

location between 2000 and 2001, did better than those who used it at work alone.

ARE THESE RESULTS ROBUST TO DIFFERENT SPECIFICATIONS?

In this section, we summarize the results of efforts both to correct for CPS's use of imputation and proxy responses and to employ change-score and propensity-score–matching models to address issues of endogeneity and selectivity. A more detailed account appears in the Appendix.

1. The effects of persistent Internet use and of Internet use at home and work remain positive and highly significant in every specification. Bias

from selection on unobserved factors, which the change-score analysis suggests may inflate coefficients, and bias introduced by CPS's use of proxy responses, which lead to underestimates of Internet-use coefficients (except for disadopter status), run in opposite directions. Propensity-score matching indicates that selection on observed variables is not a problem.

2. The effects of home-only use remain significant in all specifications (though the significance level declines in the proxy and change-score specifications), reinforcing our confidence that home Internet use boosted earnings even for respondents who did not use the Internet at work. Benefits to adopters are reduced, especially in the change-score model, but they also remain significant. Work-only users' earnings gains are strongly significant in every specification but the change-score model. Given reasons to question the specification of that model (see the Appendix), we are disinclined to reject the hypothesis that work-only use matters on that basis alone.

3. Earnings differences between disadopters and nonusers are insignificant in the proxy specification and only marginally significant in the change-score model. Positive returns for disadopters thus appear to be artifacts of the CPS's use of proxy respondents and perhaps of selection bias.

To summarize the findings: Internet users earned more than nonusers, especially if they used the Internet in both years. The labor market rewarded Internet use at home and at work, and workers who went online at home *and* work did best of all. These results are inconsistent with the view that Internet effects are artifactual because they reflect the characteristics of firms rather than workers (in which case additive effects of home use would be weak or nonexistent). They are also inconsistent with the view that Internet use boosts wages entirely through its effect on technology-use–driven workplace productivity gains (in which case use at home, but not at work, would have no effect). The effects appear to be real, but the mechanisms that connect technology use to earnings are more numerous and complex than standard human-capital theories would predict.[10]

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[10] One can salvage the human-capital explanation for home users by arguing that they only take jobs that do not permit them to benefit directly from their Internet skills if those jobs provide especially high returns to some other skill they possess. In other

DID INTERNET USE HAVE AN IMPACT OVER AND ABOVE THAT OF COMPUTER USE ALONE?

Most computer users also use the Internet. Might the impact of Internet use represent no more than the familiar effects of computer use on earnings (Krueger 1993)? In 2001, 72 percent of workers who used a computer at work used the Internet there as well (Hipple and Kosanovich 2003). Of the computer users in our sample, 89 percent also used the Internet. (The figure is higher because it includes computer and Internet use at any location.) Compared to Internet users, computer users who did *not* use the Internet were more likely to be women, non-white and non-Asian, less educated, somewhat older, and employed in blue-collar or retail occupations (table available from the authors on request).

The effects of computer and Internet use are difficult to disentangle. Bresnahan, Brynjolfsson, and Hitt (2002) argue that, by the late 1990s, the reported effects on labor markets of computers were largely effects of *networked computing* rather than stand-alone computers. Kim's (2003) cross-sectional analysis of 1997 CPS data reports a positive impact of Internet use on hourly wages even after controlling for computer use on the job. Also using cross-sectional data, Bertschek and Spitz (2003) report stronger effects of Internet use than of more routine forms of IT use (including PCs) on earnings in a West German sample.[11]

Consistent with these findings and arguments, we hypothesize that Internet and intranet use add to workers' earning power independent of using computers for spreadsheet management, word processing, or other conventional office activities. To address this issue, we added dummy variables for computer use at any location in 2001 to Model 3 of Tables 2 and 3.

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words, home users have higher net earnings because their technological skills enable them to insist on a higher reservation wage. This explanation is plausible, but preferring it to the social-capital or signaling accounts requires a commitment to neoclassical theory that we do not share.

[11] They also reported stronger effects, with appropriate controls, of using a laptop on the job. We suspect that being trusted with a laptop is a proxy for employee autonomy and employer confidence.

Results appear in Table 2, Model 4 (for year-of-use Internet measures) and Table 3, Model 4 (for home and work Internet-use measures). Controlling for computer use has only a slight effect on the statistically significant coefficients of persistent Internet use, adoption, and disadoption, and no effect on the coefficients for Internet use at home, work, and home *and* work. In both models the coefficient for computer use itself is tiny and insignificant. These results suggest that the effect of Internet use on earnings is independent of computer use and that, as computer use has become ubiquitous, networked computing has succeeded stand-alone functions as the basis of computer users' earnings advantage.

SUMMARY AND CONCLUSIONS

Between 2000 and 2001, U.S. workers who used the Internet increased their earnings at a faster rate than their offline counterparts. These benefits were independent of computer use, which enhanced earnings only when computers were connected to networks that enabled users to go online. Web users' earnings were higher than those of nonusers, even controlling for earnings a year earlier, and with controls for age, gender, race, ethnic background, educational attainment, marital status, region and metropolitan residence, union membership, occupational category, occupation-level job skill demands, and industry. Results indicating an advantage for workers who used the Internet in both years and for those who used the Internet at work *and* at home are robustly significant across a wide range of model specifications. Workers who used the Internet only at home and not at work were also rewarded, indicating that not all of the effect on earnings reflects either direct enhancements to workplace productivity or the results of employer investments. Workers who used the Internet only at work, or who began going online between 2000 and 2001, also earned more, though the effects are smaller and less robust.

These results indicate the value of looking beyond workplace Internet use and suggest that human-capital/productivity enhancement may not be the only mechanism responsible for Internet users' earnings advantage. The earnings gains for workers who used the technology at home but not at work may result from several mechanisms. Some such mechanisms are plausibly connected to productivity enhancement (e.g., if home users acquire information that makes them better workers); whereas others may enhance workers' wages without necessarily benefiting employers (e.g., by giving workers superior information about available jobs or providing noisy signals for characteristics that employers value). Taking our results as a whole, we suspect that human-capital/productivity-enhancement is probably the most important, but not the only, mechanism through which technology affects earnings. Had we taken the human-capital model for granted and measured Internet use only in the workplace (using the covariates in Model 3), we would have underestimated the impact of Internet use on wages, reporting a single unstandardized coefficient of .033 (for workplace use in 2000) or .064 (for workplace use in 2001) and missing the value added by home Internet use and by persistent, as opposed to short-term, use at work.

This study also leaves several questions unresolved:

1. Identifying more clearly the relative roles of different mechanisms in linking technology use to earnings is a high priority. Better data on jobs, employers, and career histories could make this possible. For example, human-capital returns to Internet use should be a function of experience with technology, the non-routineness of online tasks, and the potential payoff to employers of excellent performance (and the potential cost of mistakes). Benefits from enhanced information about the job market should be most visible among recent job changers and in occupations in which employers compete to attract and retain workers. Signaling effects should be most important in industries that are youth-oriented and value employees who are au courant; and for occupations in which performance is difficult to measure. Given adequate data, one could specify a combination of interactions that could yield more detailed conclusions about the relative role of these mechanisms. More detailed data on what workers actually do online at work and at home would also provide greater purchase on this issue, especially combined with better data on jobs, making it possible to identify more precisely the skills that the labor market rewards. Case studies of particular workplaces (see Fernandez 2001 for an excellent example) and interviews with human resources administrators could also be useful in this regard.

2. These analyses are restricted to men and women already in the labor market. As job listings migrate online, mastery of Internet technology may become increasingly important for getting a job in the first place. The impact of Internet use on job acquisition is an especially important priority for students of poverty.

3. We are reasonably sanguine about our success in addressing issues of endogeneity and selection bias by controlling for lagged wages and a wide range of personal attributes, and by using propensity-score matching to control for selection on observables and change-score models to correct for selection on unobservables. Selection on unmeasured characteristics is always possible, however. Because the CPS does not provide firm-level or detailed job data, potential effects of unmeasured job characteristics and employer policies are of special concern. To be sure, the independent impact of home Internet use on wages indicates that Internet effects cannot be reduced to results of employer decisions. Nonetheless, more research at the firm level is needed.

4. The interaction between workers' careers and their histories of technology use also deserves attention. Some technology effects on earnings may reflect one-time results of critical events (e.g., locating a good job match online, being available when one's firm introduces a new system, or interviewing with a gatekeeper enamored of the tech boom). With observations at only two times, and lacking information on job changes, we are unable to distinguish among workers who first gained access to the Internet at work, some nonusers who had been users in the past, and workplace Internet users who honed their skills at home or school. Longer-term panel studies or retrospective technological life-history interviews would provide a more detailed understanding of how such histories influence and are shaped by workplace experience.

5. Developing a comprehensive theoretical framework for approaching the impact of technology on life chances represents a final priority. Understanding the circumstances under which technologies disrupt or reinforce existing patterns of inequality is particularly important. Taxonomies are needed that define dimensions of variation among technologies that influence both their rate of diffusion and their impact on occupational attainment and earnings. Salient characteristics may include the accessibility of the technology to persons without higher education, its utility for non-workplace activities, the relevance of skills developed at home to the workplace, and the extent and nature of network externalities in adoption.

Do the findings in this article demonstrate that, to quote those infamous spam e-mails and online ads, one can "make money surfing the Web"? Not necessarily. These results must be understood in historical context. Two features of the period in which the data were collected—the lingering cultural cachet of the Internet and the fact that the percentage of workers who used the Internet was lower than it would become—may have inflated the impact of Internet use on earnings relative to what we may find in the future. Computer use appears to have followed a similar trajectory: Valletta and MacDonald (2004) report that the earnings premium associated with computer use for college-educated workers increased from the 1980s through 1993, turned downward in 1997, but rose sharply in 2001—a finding the authors found puzzling, but which we interpret as reflecting the impact of the Internet's growth between 1997 and 2001. It is likely that the value of familiarity with the Internet as cultural capital diminished when the Internet bubble burst, and that human-capital returns will decline as workers with skills required to use the Internet productively become more plentiful. Social-capital and information effects may endure, for even as diffusion of search skills reduces Internet users' edge in learning about job opportunities, widespread Internet use may improve the quality of job and worker matches (Autor 2001).

Even if the Internet premium declines, as we believe it will, new technologies will arise from which some workers will extract an advantage. Technologist and former Xerox research and development head John Seely Brown (Brown and Thomas 2006) argues that massively multiplayer online games are incubators of critical workplace skills. "The day may not be far off," he speculates, "when companies receive résumés that include a line reading 'level 60 tauren shaman in *World of Warcraft.*' The savviest employers will get the message." Whether or not this specific prophecy comes to pass, students of social stratification should more routinely take unequal access to and mastery of technology into account in explaining individual-level outcomes.

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nologies to social inequality includes work on the future of the digital divide in the United States and on the ways in which socioeconomic differences structure the uses to which people put the Internet. Other research interests include cultural conflict in the United States from 1965 to the present, modeling schematic heterogeneity in social attitude data, the audience for classical music, and economic networks in colonial America.

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APPENDIX

CORRECTIONS FOR PECULIARITIES OF THE CPS AND TESTS FOR ROBUSTNESS OF FINDINGS TO DIFFERENT MODEL SPECIFICATIONS

We mentioned two peculiarities of the Current Population Survey: the use of imputed values and proxy respondents. In this appendix we describe how we dealt with those issues. We also noted that the potential for endogeneity and selectivity bias complicates estimating effects of technology use on earnings. No magic wand enables analysts to detect endogeneity bias; assessments must rely on theory as well as statistical tools (Moffitt 2005). The analyses reported above dealt with endogeneity by controlling for past income and for many respondent characteristics that might be correlated with both earnings and Internet use. We also used measures of Internet use logically unrelated to current employer choices, as well as measures likely to reflect work demands. Below we describe two additional analytic methods, change-score analysis and the propensity-score–matching method (Winship and Morgan 1999).

CPS ISSUES: IMPUTATION. The CPS imputes values for hours worked (30.3 percent of the sample for at least one of the two years) and earnings (42.8 percent of the sample for at least one of the two years), with almost half of all respondents (46.3 percent) having at least one imputed value over the two waves. Imputation reduces the lagged effect of earnings, lowering the correlation between 2000 and 2001 earnings to .62, as compared to .86 for only those cases for which earnings estimates are unaffected by imputation. Imputation also threatens to inflate the impact of other variables in the model if such measures are positively correlated with the (unmeasured) difference between true and imputed lagged earnings. To address this potential problem, we controlled for the main effect of imputation on earnings in 2001 and for the interaction between imputation and lagged earnings in all the models reported in Tables 2 and 3. As expected, the slope of the lagged effect was reduced for cases with imputed values in all models. Including these controls also modestly reduced the impact of measures of Internet use on earnings in 2001 (compared to models without these controls, which are not reported), but it did not alter substantive conclusions.

CPS ISSUES: PROXY RESPONSES. When household members are unavailable, the CPS typically asks an available member to answer on their behalf. Almost two-thirds (65 percent) of the cases had proxy responses in at least one wave. Research shows that proxy responses may be unreliable for some purposes (Kojetin and Mullin 1995). To address this possibility, we controlled for direct effects on earnings of proxy responses by placing a dichotomous control in all models reported in Tables 2 and 3. We also ran additional models (reported in Table A, columns 2 and 5) with interactions between proxy status and Internet use measures. In these models, the effects of Internet use in both years (column 2) rose by 36 percent (from .078 to .106), with slopes for proxy respondents significantly flatter than those for consistent users who responded themselves. Coefficients for adopters increased marginally, whereas effects for disadopters declined by 32 percent and were no longer significant. The effect of using the Internet at home *and* work (column 5) increased by 30 percent (to .149). The coefficient for work-only Internet use increased by 20 percent,

Table A.   Robustness Checks of Logged Hourly Wages in 2001

	1. Base Model	2. CPS Proxy Responses	3. Change Score	4. Base Model	5. CPS Proxy Responses	6. Change Score	7. P. Score Matching (2001)[a]	8. P. Score Matching (either year)[a]
Internet 2001 / either year[b]							.051*** (.009)	.052*** (.011)
Internet 2000 to 2001: Y–Y	.078*** (.012)	.106*** (.018)	.028*** (.006)					
Internet 2000 to 2001: N–Y	.048*** (.014)	.053* (.023)	.022* (.012)					
Internet 2000 to 2001: Y–N	.050** (.018)	.034 (.030)	.032* (.018)					
Internet: home and work (2000 or 2001)[c]				.115*** (.013)	.149*** (.019)	.032*** (.008)		
Internet: home only (2000 or 2001)				.042*** (.012)	.046* (.020)	.025** (.009)		
Internet: work only (2000 or 2001)				.070*** (.015)	.084*** (.023)	.022 (.013)		
CPS proxy responses (2001 or 2001)[d]		.003 (.018)			.005 (.017)			
Internet 2000 to 2001: Y–Y × CPS Proxy Dummy		–.044* (.021)						
Internet 2000 to 2001: N–Y × CPS Proxy Dummy		–.008 (.028)						
Internet 2000 to 2001: Y–N × CPS Proxy Dummy		.024 (.037)						
Internet H & W × CPS Proxy Dummy					–.055* (.022)			
Internet H × CPS Proxy Dummy					–.008 (.024)			
Internet W × CPS Proxy Dummy					–.022 (.028)			
Intercept	.883*** (.057)	.897*** (.058)		.928*** (.056)	.919*** (.058)			
N	9,446	9,446	9,446	9,446	9,446	9,446	4,078	3,146
Adjusted R^2	.520	.522	.015	.523	.524	.015		

(continued on next page)

Table A.   *(continued)*

Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001. Income and hours worked were obtained from Basic CPS data collected in September, October, and November, 2000 and 2001.

Notes: The dependent variable is logged hourly wages in 2001. Control variables are omitted in the interest of parsimonious presentation. They include: 2000 wage (logged), union membership, gender, race and ethnicity, age, education, marital status, metropolitan status, region, industry, and occupation. As in all other tables, the dependent variable is the log of wages in 2001. All coefficients are unstandardized and are followed by standard errors in parentheses. Results of the base models vary somewhat from those presented in Tables 2 and 3 because they do not control for dichotomous imputation or proxy measures.

[a] Propensity scores were estimated on a common support subsample using the single-nearest-neighbor method without replacement within a caliper of .005. The reported coefficient corresponds to the average treatment effect. In addition to labor market and sociodemographic controls (listed above), the outcome model included an imputation dummy and an imputation-wage interaction. The treatment variable in the first model is Internet use in 2001; the treatment variable in the second is Internet use in 2000 or 2001 or both.

[b] "Internet 2001 / either year," "Internet 2000 to 2001: Y–Y," "Internet 2000 to 2001: N–Y," and "Internet 2000 to 2001: Y–N" refer to Internet use at any location.

[c] "Internet: home and work (2000 or 2001)" refers to respondents who used the Internet both at home and at work in at least one of the two waves of the survey. "Internet: home only (2000 or 2001)" refers to respondents who used the Internet at home but not at work in at least one of the two waves (and did not use the Internet at work in the other wave). "Internet: work only (2000 or 2001)" refers to respondents who used the Internet at work but not at home in at least one of the two waves (and did not use the Internet at home in the other wave). Respondents who used the Internet at home in one wave and at work in the other were classified as either "Internet: work only (2000 or 2001)" or "Internet: home only (2000 or 2001)," based on their Internet usage in 2001.

[d] The proxy binary variable is positive for cases in which labor force information was collected from proxy respondents or from a combination of self and proxy respondents in either 2000 or 2001.

* $p < .05$; ** $p < .01$; *** $p < .001$ (one-tailed tests for Internet use coefficients, two-tailed tests for all other variables).

whereas the coefficient for home-only use rose only marginally. These analyses suggest that significant advantages reported for Internet disadopters may be artifacts of the CPS's reliance on proxies. In other respects, however, the use of proxy respondents appears to produce underestimations of Internet users' earnings advantage.

TESTING FOR ENDOGENEITY BIAS: CHANGE-SCORE MODELS. Change-score models assess the strength of association between differences in the independent variables measured at two points in time and changes in income. Like the mathematically equivalent two-panel fixed effects model, they dramatically reduce the possibility of endogeneity error by eliminating from the analysis all characteristics of a respondent that did not change between 2000 and 2001, including stable unmeasured differences in personality, ambition, physical appearance, and other attributes (Stock and Watson 2007).

Conventional change-score (or fixed-effect) models are inappropriate for our purposes because only 23 percent of respondents either adopted or discontinued Internet use between the two waves, meaning that a conventional change-score analysis would exclude 77 percent of the sample. To make matters worse, it would compare adopters and disadopters to non-changers, lumping together consistent nonusers (the omitted category in the analyses thus far) with consistent users (who stood to benefit from their continued use). Compounding this problem further, previous research indicates that consistent users should derive *more* benefit than new users, because they use the technology more effectively. Indeed, recent adopters are precisely the people one would expect to benefit *least.* Insofar as being online is rewarded (even absent a change of state), consistent users have greater skill than new users, and adoption carries a benefit different than the cost of disadoption. Conventional change-score analyses will hence yield misleading results.

We therefore altered the change-score model to include the dummy variables for Internet use employed in the previous analyses, rather than employing a single-change measure. All other variables in the model,

including the dependent variable (log earnings), were expressed in terms of change scores between 2000 and 2001.

Coefficients for Internet use (reported in Table A, Models 3 and 6) were smaller than in other models, but the effects of consistent Internet use, Internet use at home *and* work, and home Internet use remained strongly significant. Coefficients for adopters and disadopters, though comparable in size to those for consistent users, were only marginally significant ($p < .1$) due to larger standard errors; and the coefficient for the effect of Internet use at work, but not at home, became nonsignificant. The results of the change-score analyses, then, confirm the main findings from the OLS models, with significant earnings advantages accruing to persistent Internet use, use at home *and* work, and use at home. At the same time, marginally significant coefficients for adopters and disadopters and, especially, insignificant effects for work-only users raise questions about findings for those categories.

We take these results seriously, but we do not regard them as preferable to the results of models using other specifications. The advantages of the change-score model come at substantial cost due to model-specification problems. For example, education continues to boost earnings throughout adulthood rather than spending its effect as soon as it is acquired. Yet the change-score specification controls only for the effects of years of education acquired in the previous year, treating PhDs and high school dropouts as indistinguishable with respect to incremental earning power, if their educational attainment was the same in 2001 as in 2000. Similarly, economic theory leads us to expect voluntary job change to occur only when workers' skills are more highly rewarded in a new job. Yet in the change-score specification, every job change that crosses industry or occupational boundaries must take a negative value for the exited industry or occupation and positive value for the entered one. Given such problems, we regard the results of the change-score analysis as informative but not dispositive.

TESTING FOR SELECTION BIAS ON OBSERVABLES: PROPENSITY-SCORE MATCHING. To test for possible sample selection bias on the basis of respondents' observed characteristics, we used the propensity-score–matching method, which approximates an experimental condition by pairing respondents who received a treatment (using the Internet) with those who did not, based on the respondents' probability of selection into the treatment group. (Note that this is very different from instrumental-variable analysis, which corrects for selection bias based on *unobservables* by using instruments that predict the treatment without predicting the outcome. In contrast, propensity-score matching uses predictors of both the treatment selection and the outcome to generate propensity scores on the basis of which matched pairs of respondents differing only in their observed reception of treatment are divided into treatment and control groups.) We used the MatchIt module designed by Ho and colleagues (2004) for the R statistical package to estimate the propensity scores and ensure that the selection model was balanced (i.e., that the standardized biases for all coefficients were less than .05). We report two models: one using a binary measure of Internet use in 2001 in any location (Table A, Model 7) and one using a binary measure of Internet use in 2001 or 2000 in any location (Model 8). We do so (instead of including multiple indicators of Internet use in different years or at different locations) because propensity-score estimation uses logit or probit regression, which requires a single dichotomous dependent variable, to generate propensities (of Internet use) used to match treatment and control cases. The resulting scores were matched using the nearest neighbor method without replacement within a caliper of .005.

The propensity-score–matching analyses (reported in the last two columns of Table A) yielded estimates of the effect of Internet use in 2001 on earnings of .051, and of Internet use in either year on earnings of .052, each statistically significant at the $p < .001$ level. Each was statistically indistinguishable from the estimates from comparable non-matched models (.048 for 2001 and .057 for 2000 to 2001). Based on these analyses, we find no evidence that the impact of Internet use on earnings is seriously inflated by selectivity bias related to observed characteristics.

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