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

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Abstract

Much research on the "digital divide" has presumed 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 change 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.

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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 intergroup differences, more recent research attempts to explain such differences statistically, and has also explored 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 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 paper 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 policy makers share this faith. For example, the Statement of Findings for Illinois's 2000 "Eliminate the Digital Divide" Act noted the existence of a "digital divide" and asserted as settled fact that citizens who have mastered and have access to "the tools of the new digital technology" had "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).

But although we have learned a lot 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, and therefore an important topic for research.

The digital divide is also 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 in an era in which rapid technological change has become the norm. Charles 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 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 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.

Such studies have not determined whether such increments in power are converted into higher earnings, however. Indeed, sociologists who study inequality have rarely asked 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 has addressed Internet use, however. Moreover, most economic studies of effects of technology use on earnings have exhibited two shortcomings. First, they usually have employed cross-sectional data. Second, they have assumed technology use influences income through a single mechanism – *i.e.*, that any non-spurious effects of technology use on income reflect increases in human capital and productivity.

Some economists have called for employing longitudinal data and using other means to counteract effects of reciprocity bias (DiNardo and Pischke 1997; Card and DiNardo 2002) inherent in (but not limited to) cross-sectional designs. 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. Thus earnings advantages that appear to be caused by the use of new technology may instead 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 competitors *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 those with less well-trained employees (Acemoglu 2002), producing 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 in turn 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 exclusively 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 (for example, prior use or use outside the workplace), and which 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).

Assessing confidently the impact of Internet use on earnings, then, requires that we:

(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; and

(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 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. The CPS has conducted periodic surveys of respondents' use of communications technologies, as well as taking multiple measures of respondents' incomes. CPS 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. Thus it was possible to explore the impact of Internet use on earnings change over a thirteen-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 and 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 non-users; and 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. By 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 and/or competence. We use measures of Internet use from the 2000 and 2001 CPS Internet modules to divide our sample into groups of non-users, consistent users, adopters (2000 non-users who were users in 2001), and disadopters (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 Internet use at work, and therefore less likely to be a function of unmeasured employer characteristics that influence both workplace technology and wages. We believe that ours is the first earnings study to use separate measures 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 21st century. We first look at the relative earnings gains of consistent Internet users, new adopters, and *dis*adopters (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. But first 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 has focused on mechanisms that link technology use to worker productivity and thence to earnings (summarized below under the heading of "human capital/productivity-enhancement"), we describe additional mechanisms that link technology use, respectively, to better labor-market information and social networks ("social capital/information-hoarding") and to the 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 necessary skills quickly? Finding information and communicating with other people on-line, 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 (for example, 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 (hyperlinks, URLs, search engines) through which one can do so; (3) learn the basics of on-line search (*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 websites; (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 contents, use of abbreviations and emoticons, and so on) (Warschauer 2003; Van Dijk 2005, ch. 5).

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 on-line (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 northeastern county, with years of experience and intensity of use 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 and/or experience.

<u>Internet Use as a Form of Human Capital Leading to Enhanced Productivity</u>

In some occupations in some industries, workers who can use the Internet effectively may perform better than those who cannot, and 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 classic study of the effects of computer use on earnings (1993; Autor, Katz and Krueger 1998; in the U.K., Dickerson and Green 2004) reported that workers who used computers earned 17 to 20 percent more than workers who did not. 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 only adopt new technologies that require them to increase the ratio of skilled to unskilled workers 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 only provide job training for new salespersons offsite over the Internet. The ability to retrieve information on-line is an important part of many workers' daily routine: 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 *Occupational Outlook* includes "conduct research on the Internet" in the job description for secretaries (Levy and Murnane 2004:4). Some workers, for example 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 on-line.

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 whose occupants benefit from technological change is not skill *per se* but impediments to routinization. Drivers for many trucking fleets, for example, employ 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. Thus ability to use Internet (or Intranets based on Internet technology) may 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).

<u>Internet Use as a Source of Social Capital</u>

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 resumés: 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 (Hampton and Wellman 2000; Fountain 2005). Online communications may also complement rather than substitute for face-to-face relationships. For example, the first author interviewed a sales rep 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 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 2000. Using similar CPS data, Kuhn and Skuterud (2004) found no contribution of Internet searching to job placement. By 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).

<u>Internet Use as Cultural Resource and Signal</u>

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 upon (and contributed to) the motor craze of the early 20th 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, adaptable 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 make it easier for people to form relations with high-status others and lead gatekeepers to evaluate them favorably (Bourdieu 1989; 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) reported that some employers used Internet job postings to weed out low-quality applicants, who they presumed would not be on-line. An experimental study of the impact of race and other factors on employer responses to otherwise randomized resumés reported that (fictional) applicants with e-mail addresses on their resumes received significantly more calls for interviews than similar applicants without them (Bertrand and Mullainathan 2004).

<u>Temporal specificity of causal mechanisms.</u>

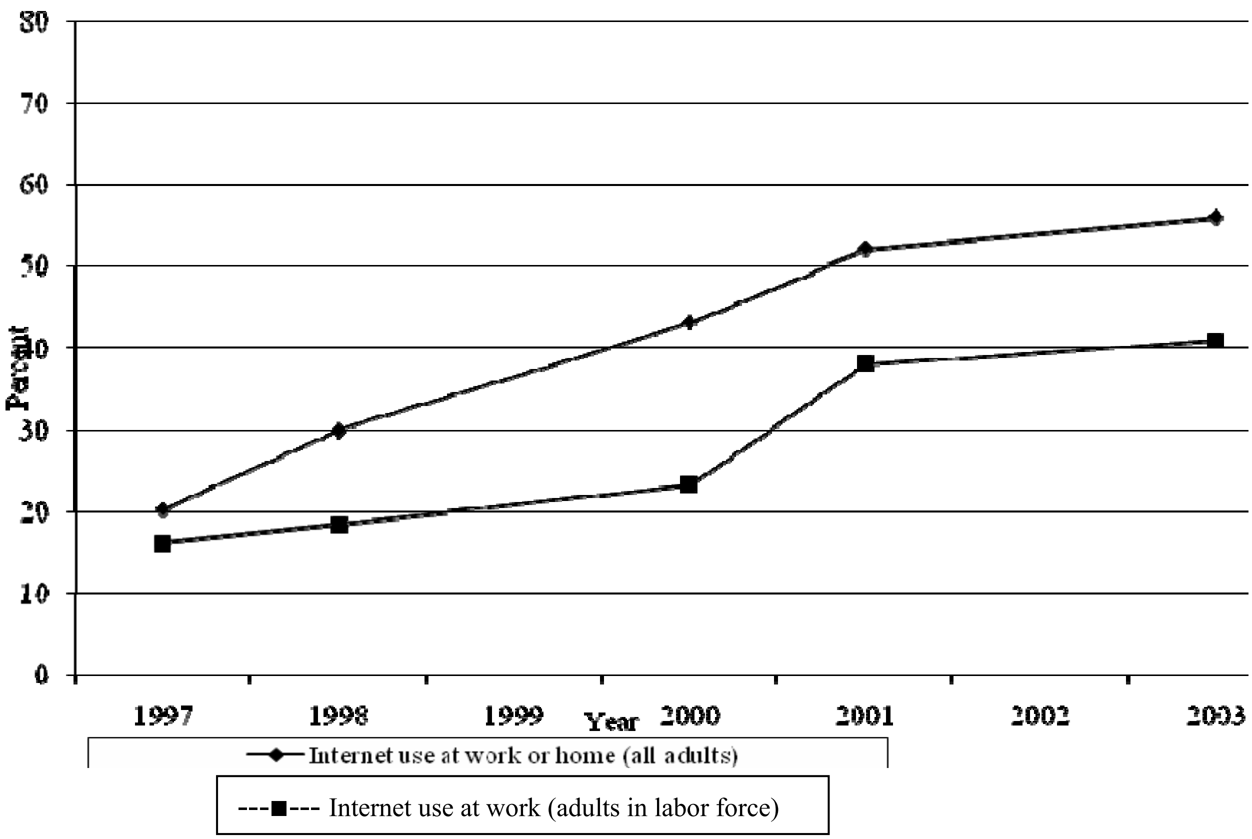
Rewards to technological competence are likely to change systematically over the life-cycle of a technological innovation. Our data were collected at the end of a period of very rapid diffusion, just as the rate of increase was beginning to decline. Figure 1 describes change between 1997 and 2003 in the percentage of all non-institutionalized Americans aged 18 or older who reported using the Internet at any location, and in 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 percent 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 capital investments necessary to exploit such competence; and then decline as relevant skills saturate the workforce. Even if the ability effectively to find information or to engage in transactions online enhances worker productivity, such skill will no longer give a worker a competitive edge if 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 would become 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, and results should not be generalized to later periods.

Hypotheses

The three mechanisms described above do not map neatly onto specific indicators of Internet use; but we can nonetheless 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 it on the job will benefit. By contrast, in so far as the Internet operates through social-capital enhancement or signaling, using the Internet as home may independently affect earnings. Indeed, such benefits could even be derived from past Internet use by workers who are no longer on-line.

Figure 1. Internet Use, 1997 - 2003 (*source: CPS*)



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

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

Not all forms of Internet use will have equally strong effects, however. 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 we have reviewed on growth in the efficacy of use over time, we predict that:

> *Hypothesis 2. The net earnings of workers who used the Internet in 2001 but not 2000 grew faster than those of non-users but less quickly than those of more experienced Internet users.*

Workers who stopped using the Internet between 2000 and 2001 may also have benefited from signaling effects and social-capital effects, but, as current nonusers, lack the human-capital advantage derived from using new technology at work. Therefore, we expect that

> *Hypothesis 3. The net earnings of workers who used the Internet in 2000 but not in 2001 grew fasters than those of non-users but less quickly than those of more experienced Internet users.*

In so far 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 those who used it at work. Using the Internet both at home and at work is likely to be especially advantageous: Work use nets human-capital benefits; use on one's own may be more influential for signaling. Workers are freer to peruse job postings and to expand networks through casual interaction at home. And search and other skills are likely honed through technology use at home as well as work. For all these reasons, we anticipate that

> *Hypothesis 4: Net earnings of workers who used the Internet only at home or only at work grew faster than those of non-users but less quickly than earnings of workers who used the Internet at work* and *at 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 sixteen months. Every month, one eighth of the sample is replaced by new households with similar characteristics. This rotating sampling design permits comparison 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 twelve 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 has 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 2001 households, 15,758 had also participated in the 2000 supplement, yielding 37,288 individual records. After excluding non-civilians, respondents who were under eighteen, over sixty-five, or outside the labor force, and respondents who reported variable hours worked or 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 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 2001 rates of Internet use by sociodemographic categories for persons in our sample.[5] (Because the analysis is restricted to employed persons aged 18 to 65, usage rates are higher than for the population at large.) 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 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 twenty-six and thirty-five years of age reported the highest usage of any age cohort (74 percent); those between ages fifty-six and sixty-five 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 women, 55 percent for men). The difference reflects the advantage for women in at-work Internet use (48 vs. 41 percent) and suggests that increased workplace technology use was responsible for eliminating a gender gap that advantaged men 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 Mid-West and the Northeast (73 percent), and workers who lived in MSAs were online more than nonmetropolitan residents (72 percent *vs.* 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, compared to 89 percent of professionals, going on-line.

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. More than 98 percent were connected at home *or* work, the rest going on-line at a library, community center, school, or friend's or relative's home.

Table 1. Group-Specific Rates of 2001 Internet use (unweighted counts)[a]

	N	Anywhere (%)	Work (%)	Home (%)	Work and home (%)	Work or home (%)
r						
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
Am. Indian	100	48.0	26.0	33.0	14.0	45.0
Hispanic	772	41.3	24.74	31.9	16.6	40.0
Age						
18 - 25	830	72.2	27.4	58.8	19.8	66.4
26 - 35	1,953	73.8	47.4	59.4	34.8	71.9
36 - 45	2,991	71.4	46.8	58.5	35.1	70.2
46 - 55	2,628	70.4	47.6	56.4	34.4	69.6
56 - 65	1,044	57.7	39.2	44.4	26.3	57.2
Education						
Less than HS	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
Mid-West	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 - Non-durable	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 & 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
Personal Services	218	54.1	21.6	45.0	13.8	52.8
Entert. & Rec. Services	116	69.8	33.6	61.2	27.6	67.2
Hospitals	525	75.2	45.3	60.8	31.8	74.3
Med. Serv. (exc. 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 (incl. Clerical)	1,529	79.1	53.2	58.2	33.6	77.8

	N	Anywhere (%)	Work (%)	Home (%)	Work and home (%)	Work or home (%)
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

[a] Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001, employed persons aged 18 to 65..

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 use 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 2001 Earnings (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), a group that included 55 percent of the sample; 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, 2000 users who were nonusers in 2001, or Y-N [for Yes-No]) (7 percent). (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 of the total. 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^2), 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]

Table 2. Regression of 2001 logged wages on general Internet use[a]

	N	I		II		III		IV	
Internet 2000 - 2001: Y - Y[b]	5,156	0.148	***	0.087	***	0.065	***	0.061	***
		(0.011)		(0.011)		(0.012)		(0.015)	
		0.132		*0.078*		*0.058*		*0.055*	
Internet 2000 - 2001: N - Y	1,471	0.070	***	0.051	***	0.040	**	0.036	*
		(0.014)		(0.013)		(0.013)		(0.017)	
		0.046		*0.033*		*0.026*		*0.024*	
Internet 2000 - 2001: Y - N	625	0.086	***	0.063	***	0.046	**	0.046	**
		(0.018)		(0.018)		(0.017)		(0.017)	
		0.039		*0.028*		*0.021*		*0.020*	
Computer Use 2001: Networked and Non-Networked	7,331							0.005	
								(0.015)	
								0.004	
Intercept		0.419	***	0.189	***	0.436	***	0.435	***
		(0.028)		(0.056)		(0.059)		(0.059)	
N		9,446		9,446		9,446		9,446	
Adjusted R^2		0.486		0.529		0.555		0.555	

*** p<0.001; ** p<0.01; * p<0.05; † p<0.1; one-tailed tests for Internet use coefficients, two-tailed tests for all other variables

[a] Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001. Income and hours worked were obtained from Basic CPS data collected in Sep., Oct., and Nov., 2000 and 2001. The analysis excludes non-civilians, respondents under eighteen and over sixty-five years of age, those 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 1controls 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.

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

The remaining net earnings advantage of continuous users is statistically significant at p<.001 (one-tailed), consistent with Hypothesis 1. The advantages of adopters and disadopters were significant at p<.01. In dollar terms (based on coefficients in Model 3), the advantage of continuous Internet use for a median earner (relative a comparable non-user) amounted to $0.96 per hour, while the wage premium for a median earner who adopted Internet use in 2001 was $0.58. Median earners who ceased using the Internet between waves received a $0.67 wage premium relative to comparable non-users. Consistent with hypothesis 2, Wald tests for difference in coefficients (available upon request) indicate that continuous users gained significantly more than 2001 adopters (p<.05). In these analyses, however, hypothesis 3 was disconfirmed, as effects for continuous users and *dis*adopters were 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 non-users. These gains persisted despite the inclusion of numerous control variables. They were also independent of the

effects of any unmeasured characteristics of worker and job, the effects of which were fully incorporated in logged earnings as measured in fall 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 continue 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 reflected only unmeasured differences between Internet users and other workers that influenced the rate of earnings growth,, or if they reflected 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 it on the job. This was not the case. If the association between Internet use and earnings *only* reflected 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 n wages. This, too, was not the case.

Results appear in Table 3. Separate dichotomous variables represent respondents who used the Internet at home *and* work in at least one year (37 percent of all workers), respondents who only used the Internet at home (26 percent), and those who only used the Internet at work (13 percent). Nonusers (23 percent) were 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. Models control for lagged income, so coefficients indicate influence on *net* wages over a period of approximately 13 months.

All groups of users earned significantly more (net 2000 earnings and other controls) than nonusers in 2001 (Model 1). Those who used the Internet at home *and* work gained the highest returns (unstandardized coefficient of .198); followed by those who used the Internet at work but not at home (coefficient of .115); and those who used it at home but not at work (.066). Controlling for race and Hispanic ethnicity, gender, age (and age^2), 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 of work-only use by 17 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.

Table 3. Regression of 2001 logged wages on Internet use at home and work[a]

	N	I		II		III		IV	
Internet: Home and Work (2000 or 2001)[b]	3,486	0.198	***	0.124	***	0.094	***	0.094	***
		(0.011)		(0.012)		(0.013)		(0.016)	
		0.172		*0.108*		*0.082*		*0.082*	
Internet: Home Only (2000 or 2001)	2,414	0.066	***	0.038	**	0.036	**	0.036	**
		(0.012)		(0.012)		(0.011)		(0.014)	
		0.052		*0.030*		*0.028*		*0.028*	
Internet: Work Only (2000 or 2001)	1,186	0.115	***	0.086	***	0.060	***	0.060	***
		(0.014)		(0.014)		(0.015)		(0.017)	
		0.069		*0.051*		*0.036*		*0.036*	
Computer Use 2001: Networked and Non-Networked[c]	7,331							0.000	
								(0.014)	
								0.000	
Intercept		0.477	***	0.252	***	0.463	***	0.463	***
		(0.028)		(0.056)		(0.058)		(0.059)	
N		9,446		9,446		9,446		9,446	
Adjusted R^2		0.493		0.532		0.556		0.556	

*** p<0.001; ** p<0.01; * p<0.05; † p<0.1; one-tailed tests for Internet use coefficients, two-tailed tests for all other variables

[a] Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001. Income and hours worked were obtained from Basic CPS data collected in Sep., Oct., and Nov., 2000 and 2001. The analysis excludes non-civilians, respondents under eighteen and over sixty-five years of age, those 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 1controls 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. Model 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. Coefficients are followed by standard errors in parentheses and beta weights in italics.

[b] "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 (2000 or 2001)" refers to respondents who used the Internet at home but not work in at least one of the two waves (and did not use the Internet at work in the other wave). "Internet: Work (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 (2000 or 2001)" or "Internet: Home (2000 or 2001)", based on their Internet usage in 2001.

[c] Computer use variable measures use anywhere (home, work, or other locations).

Consistent with Hypothesis 4, all user groups increased earnings significantly more than did nonusers. Also consistent, Wald tests indicated that returns to workers who used the Internet at home *and* at work were significantly greater than for those who used it 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, $0.88 for those who used it at work but not at home, and $0.52 for home-only users.

We ran an additional model with the same covariates as in model 3, but with fifteen detailed categorical measures indexing Internet use or nonuse at home and work by year, with nonusers omitted. Although Ns for many categories were quite small, the overall pattern of results (available upon request) reinforced those in model 3. Workers who used the Internet at home *and* 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); 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 who used the Internet at both locations in 2000 but at neither in 2001. Overall, respondents who used the Internet at home *and* work, and especially 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 results of efforts 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 remained positive and highly significant in every specification. Bias from selectivity on unobserved variables, 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 remained significant in all specifications (though the significance level declined in the proxy and change-score specifications), instilling confidence that home Internet use boosted earnings even for respondents who did not use it at work. Benefits to adopters were reduced, especially in the change-score model, but also remained significant. Work-only users' earnings gains were strongly significant in every specification but the change-score model. Given reasons to question the specification of that model (see Appendix), we are disinclined to reject the hypothesis that work-only use matters on that basis alone.

3. Earnings differences between disadopters and nonusers became insignificant in the proxy specification and declined to marginal significance in the change-score model Thus positive returns for *disadopters* appear to be artifacts of the CPS's use of proxy respondents and perhaps of selection bias.

To summarize: Internet users earned more than nonusers, especially if they used it in both years, the labor market rewarded Internet use at home and at work, and workers who went on-line 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]

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, computers users who did *not* use the Internet were more likely to be women, nonwhite and non-Asian, less highly educated, somewhat older, and employed in blue-collar or retail occupantions. (Table available upon request.)

The effects of computer and Internet use are difficult to disentangle. Bresnahan, Brynjolfsson and Hitt (2002) argue that, by the late 1990s, 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 reported 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) reported 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 *intra*net) 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 Table 2 and Model 3 of Table 3. 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 only enhanced earnings when computers were connected to networks that enabled users to go on-line. Web users' earnings were higher than those of non-users, 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 to workers who used the Internet in both years and to those who used the Internet at work and at home are robustly significant across a wide range of model specifications. Workers who only used the Internet 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 on-line between 2000 and 2001 also earned more, though the effects were 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. We found earnings gains not only for workers currently using the Internet on the job, but also for workers who used the technology at home but not at work. Such effects are consistent with a variety of mechanisms. Some are plausibly connected to productivity enhancement (*e.g.*, if home users acquire information that makes them better workers); whereas other mechanisms may enhance workers' wages without necessary benefiting employers (*e.g.*, by giving workers superior information about available jobs or providing noisy signals for characteristics employers value). Taking our results as a whole, we suspect that the 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 2000 workplace use) or .064 (for 2001 workplace use), and missing the value added by use of the Internet at home and by persistent as opposed to short-term use at work.

This study also leaves several questions unresolved:

1. A high priority is identifying more clearly the relative roles of different mechanisms in linking technology use to earnings. 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 their on-line tasks, and the potential payoff to their employers of excellent performance (and the potential cost of mistakes). Benefits from enhanced information about the job market should be most visible among job recent job changers and in occupations for 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 meter. 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 on-line 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, *e.g.*, 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 on-line, 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. Nonetheless, selection on unmeasured personal characteristics is always possible. 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 fact that home Internet use exerted an independent impact on wages indicates that Internet effects cannot be reduced to results of employer decisions. Nonetheless, more research at the firm level is needed.

4. Attention is also due to the way in which careers at work and careers in technology use interact. Some technology effects on earnings may be reflect one-time results of critical events (*e.g.,* locating a good job match on-line, 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 were 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 this paper's findings demonstrate that, to quote those infamous spam e-mails and on-line ads, one can "make money surfing the Web"? Not necessarily: It is important to understand these results in historical context. It is likely that 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. This seems to have been the case for computer use: 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 that the authors found puzzling, but which we interpret as reflecting the impact of the Internet's growth between 1997 and 2001. It seems likely that returns to Internet familiarity as cultural capital declined when the Internet bubble burst, and that human-capital returns to Internet use will decline as workers with skills necessary to use the Internet productively become more plentiful. Social-capital/information effects may be more enduring, for even as the diffusion of search skills reduces Internet users' edge in finding out about job opportunities, widespread Internet use may improve the quality of job/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 R&D head John Seely Brown (Brown and Thomas 2006) has argued that massively multiplayer online games are incubators of critical workplace skills: "The day may not be far off," he speculated, "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.

Appendix: Corrections for Peculiarities of the CPS and Tests for Robustness of Findings to Different Model Specifications

We have 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 have also noted that the potential for endogeneity and selectivity bias complicates estimating effects of technology use on earnings. There is no magic wand that enables analysts to detect endogeneity bias and assessments must rely on theory as well as statistical tools (Moffitt 2005). The analyses reported above we 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 2001 earnings 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 2001 earnings (compared to models without these controls, which are not reported), but did not alter substantive conclusions.

CPS Issues: Proxy Responses. When household members are unavailable, the CPS typically asks an available household member to answer on their behalf. Almost two thirds (65 percent) of the cases had proxy responses in at least one wave. Research has shown that proxy responses may be unreliable for some purposes (Kojetin and Mullin 1995). To address this possibility, we controlled for the 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 Appendix Table 4, 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 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, 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 *under*estimates of Internet users' earnings advantage.

Appendix Table 4. Robustness checks

	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)[b]	8. P. Score Matching (either year)[b]
Internet 2001 / Either Year[c]							0.051 ***	0.052 ***
							(0.009)	(0.011)
Internet 2000 - 2001: Y - Y	0.078 ***	0.106 ***	0.028 ***					
	(0.012)	(0.018)	(0.006)					
Internet 2000 - 2001: N - Y	0.048 ***	0.053 *	0.022 *					
	(0.014)	(0.023)	(0.012)					
Internet 2000 - 2001: Y - N	0.050 **	0.034	0.032 *					
	(0.018)	(0.030)	(0.018)					
Internet: Home and Work (2000 or 2001)[d]				0.115 ***	0.149 ***	0.032 ***		
				(0.013)	(0.019)	(0.008)		
Internet: Home (2000 or 2001)				0.042 ***	0.046 *	0.025 **		
				(0.012)	(0.020)	(0.009)		
Internet: Work (2000 or 2001)				0.070 ***	0.084 ***	0.022 †		
				(0.015)	(0.023)	(0.013)		
CPS Proxy Responses (2001 or 2001)[e]		0.003			0.005			
		(0.018)			(0.017)			
Internet 2000 - 2001: Y - Y x CPS Proxy Dummy		-0.044 *						
		(0.021)						
Internet 2000 - 2001: N - Y x CPS Proxy Dummy		-0.008						
		(0.028)						
Internet 2000 - 2001: Y - N x CPS Proxy Dummy		0.024						
		(0.037)						
Internet H & W x CPS Proxy Dummy					-0.055 *			
					(0.022)			
Internet H x CPS Proxy Dummy					-0.008			
					(0.024)			
Internet W x CPS Proxy Dummy					-0.022			
					(0.028)			
Intercept	0.883 ***	0.897 ***		0.928 ***	0.919 ***			
	(0.057)	(0.058)		(0.056)	(0.058)			
N	9,446	9,446	9,446	9,446	9,446	9,446	4,078	3,146
Adjusted R^2	0.520	0.522	0.015	0.523	0.524	0.015		

*** p<0.001; ** p<0.01; * p<0.05; † p<0.1; one-tailed tests for Internet use coefficients, two-tailed tests for all other variables

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

Propensity scores were estimated on a common support subsample using the single-nearest-neighbor method without replacement within a caliper of 0.005. 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 imptuation-wage interaction. The treatment in the first model is Internet use in 2001; the treatment in the second is Internet use in 2000 or 2001 or both.

"Internet 2001 / Either Year", "Internet 2000-2001: Y-Y", "Internet 2000-2001: N-Y", "Internet 2000-2001: Y-N" measure Internet use anywhere.

"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 (2000 or 2001)" refers to respondents who used the Internet at home but not work in at least one of the two waves (and did not use the Internet at work in the other wave). "Internet: Work (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 (2000 or 2001)" or "Internet: Home (2000 or 2001)", based on their Internet usage in 2001.

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.

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, 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.* In so far as being on-line is rewarded (even absent a change of state), consistent users have greater skill than new users, and adopting carries a benefit different than the cost of disadoption, conventional change-score analyses will yield misleading results.

Therefore we 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 App. Table 4, models 3 and model 6) were smaller than in other models, but the effects of consistent Internet use, Internet use at home and at 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 ($p<.10$) 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 do not regard them as preferable to results of models using other specifications. The advantages of the change-score model come at substantial cost due to model-specification problems. Two examples: Education continues to boost earnings throughout adulthood rather than spending its effect as soon as it is acquired. Yet the change-score specification only controls for the effects of years of education acquired in the previous year, treating Ph.D.s 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 quite different from instrumental-variable analysis, which requires the use of instruments that predict the treatment without predicting the outcome in order to correct for selection bias based on *unobservables*. By 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, Imai, King, and Stuart (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 0.05). We report two models: one using a binary measure of 2001 Internet use in any location (Appendix Table 4, 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 0.005.

The propensity score matching analyses (reported in the last two column of Appendix Table 4) 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 2000/2001). Based on these analyses, we find little evidence that the impact of Internet use on earnings is seriously inflated by selectivity bias related to observed characteristics.

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Appendix Table 1. Descriptive statistics for all variables used in the analysis

Variable	N[b]	Mean	Std. Dev.	Min	Max
Internet use (N = 9,446)					
Internet 2000 - 2001: Yes - Yes[a]	5,156	0.55	0.50	0.00	1.00
Internet 2000 - 2001: No - Yes	1,471	0.16	0.36	0.00	1.00
Internet 2000 - 2001: Yes - No	625	0.07	0.25	0.00	1.00
Internet 2000 - 2001: No - No	2,194	0.23	0.42	0.00	1.00
Home Internet Use 2000	4,588	0.49	0.50	0.00	1.00
Work Internet Use 2000	2,688	0.28	0.45	0.00	1.00
Home Internet Use 2001	5,342	0.57	0.50	0.00	1.00
Work Internet Use 2001	4,211	0.45	0.50	0.00	1.00
Labor force (N = 9,446)					
Income 2000 (not logged, 2000 $)	9,446	693.75	500.43	18.64	#####
Income 2001 (not logged, 2000 $)	9,446	711.22	519.27	17.50	#####
Typical hours worked 2000	9,446	41.09	9.53	2.00	166.00
Typical hours worked 2001	9,446	40.80	9.15	2.00	108.00
Wage 2000 (not logged, 2000 $)	9,446	16.54	10.72	2.62	155.50
Wage 2001 (not logged, 2000 $)	9,446	17.12	11.59	2.62	308.53
Union membership	1,637	0.17	0.38	0.00	1.00
Sociodemographic variables (N = 9,446)					
Male	4,793	0.51	0.50	0.00	1.00
Black	836	0.09	0.28	0.00	1.00
American Indian	100	0.01	0.10	0.00	1.00
Asian	342	0.04	0.19	0.00	1.00
Hispanic		0.08	0.27	0.00	1.00

Variable	N	Mean	Std. Dev.	Min	Max
Industry (N = 9,446)					
Agriculture, Extractive	170	0.02	0.13	0.00	1.00
Construction	581	0.06	0.24	0.00	1.00
Manufacturing (Durable Goods)	935	0.10	0.30	0.00	1.00
Manufacturing (Non-Durable Goods)	589	0.06	0.24	0.00	1.00
Transportation	413	0.04	0.20	0.00	1.00
Communications	160	0.02	0.13	0.00	1.00
Utilities and Sanitary Services	149	0.02	0.12	0.00	1.00
Wholesale Trade	397	0.04	0.20	0.00	1.00
Retail Trade	1,225	0.13	0.34	0.00	1.00
Finance, Insurance, Real Estate	655	0.07	0.25	0.00	1.00
Business and Repair Services	501	0.05	0.22	0.00	1.00
Personal Services	218	0.02	0.15	0.00	1.00
Entertainment and Rec. Services	116	0.01	0.11	0.00	1.00
Hospitals	525	0.06	0.23	0.00	1.00
Medical Services (non-Hospital)	471	0.05	0.22	0.00	1.00
Educational Services	1,109	0.12	0.32	0.00	1.00
Social Services	203	0.02	0.15	0.00	1.00
Other Professional Services	437	0.05	0.21	0.00	1.00
Public Administration	592	0.06	0.24	0.00	1.00
Occupation (N = 9,446)					
Executive, Admin. Managerial	1,487	0.16	0.36	0.00	1.00
Professional Specialty		0.19	0.39	0.00	1.00

	772						1,775				
Age	9,446	41.67	10.99	18.00	65.00	Technicians and Related	391	0.04	0.20	0.00	1.00
Age squared	9,446	#####	911.84	324.00	#####	Sales	879	0.09	0.29	0.00	1.00
Age: 18 - 25	830	0.09	0.28	0.00	1.00	Administrative Support (incl. Clerical)	1,529	0.16	0.37	0.00	1.00
Age: 26 - 35	1,953	0.21	0.40	0.00	1.00	Service	991	0.10	0.31	0.00	1.00
Age: 36 - 45	2,991	0.32	0.47	0.00	1.00	Precision Product., Craft, Repair	1,069	0.11	0.32	0.00	1.00
Age: 46 - 55	2,628	0.28	0.45	0.00	1.00	Mach. Operat., Fabricators, Laborers	483	0.05	0.22	0.00	1.00
Age: 56 - 65	1,044	0.11	0.31	0.00	1.00	Transportation & Material Moving	405	0.04	0.20	0.00	1.00
Education: Less than high school	693	0.07	0.26	0.00	1.00	Handlers, Equip. Cleaners, Helpers, Laborers	326	0.03	0.18	0.00	1.00
Education: High school degree	4,752	0.50	0.50	0.00	1.00	Farming, Forestry, Fishing	111	0.01	0.11	0.00	1.00
Education: Associate degree	1,037	0.11	0.31	0.00	1.00	*Occupational Skill Requirements (N = 8,540)*					
Education: College degree	1,960	0.21	0.41	0.00	1.00	Interpersonal Relationships	8,540	3.36	0.87	0.31	4.71
Education: Advanced degree	1,004	0.11	0.31	0.00	1.00	Strength	8,540	5.50	2.09	0.87	11.29
Married	6,326	0.67	0.47	0.00	1.00	Getting Information	8,540	3.91	0.67	0.63	4.89
Geographic location (N = 9,446)						Independence	8,540	3.06	1.38	0.01	4.61
Region: Northeast	2,092	0.22	0.42	0.00	1.00	Achievement/Effort	8,540	2.92	1.33	0.01	4.80
Region: Mid-West	2,601	0.28	0.45	0.00	1.00	Social Perceptiveness	8,540	3.08	0.89	0.24	4.65
Region: South	2,686	0.28	0.45	0.00	1.00	Analyzing Data	8,540	2.93	0.78	0.42	4.61
Region: West	2,067	0.22	0.41	0.00	1.00	Updating/Using Relevant Knowledge	8,540	3.91	0.67	0.63	4.89
Metropolitan Area: No	2,077	0.22	0.41	0.00	1.00	*Proxy and Imputation Controls*					
Metropolitan Area: Yes	7,336	0.78	0.42	0.00	1.00	Proxy Responses (2000 or 2001)	9,446	0.65	0.48	0.00	1.00
Metropolitan Area: Not Identified	33	0.00	0.06	0.00	1.00	Imputed Hours or Earnings (2000 or 2001)	9,446	0.46	0.50	0.00	1.00

[a] Unless otherwise indicated, all variables contain 2001 data.
[b] Ns for categorical variables indicate number of positive responses.

Appendix Table 2. Regression of 2001 logged wages on Internet use by year of use[a]

	N	I		II		III		IV	
Internet and Computer Use Variables									
Internet 2000 - 2001: Y - Y[b]	5,156	0.148	***	0.087	***	0.065	***	0.061	***
		(0.011)		(0.011)		(0.012)		(0.015)	
		0.132		*0.078*		*0.058*		*0.055*	
Internet 2000 - 2001: N - Y	1,471	0.070	***	0.051	***	0.040	**	0.036	*
		(0.014)		(0.013)		(0.013)		(0.017)	
		0.046		*0.033*		*0.026*		*0.024*	
Internet 2000 - 2001: Y - N	625	0.086	***	0.063	***	0.046	**	0.046	**
		(0.018)		(0.018)		(0.017)		(0.017)	
		0.039		*0.028*		*0.021*		*0.020*	
Computer Use: Networked and Non-Networked	7,331							0.005	
								(0.015)	
								0.004	
Labor Market Variables									
Wage 2000 (log)[c]	9,446	0.821	***	0.668	***	0.591	***	0.591	***
		(0.011)		(0.011)		(0.012)		(0.012)	
		0.815		*0.663*		*0.586*		*0.586*	
Union	1,637			0.030	**	0.060	***	0.060	***
				(0.011)		(0.011)		(0.011)	
				0.021		*0.041*		*0.041*	
Sociodemographic Variables									
Male	4,793			0.123	***	0.106	***	0.106	***
				(0.008)		(0.009)		(0.009)	
				0.111		*0.095*		*0.095*	
Black	836			-0.061	***	-0.050	***	-0.049	***
				(0.015)		(0.014)		(0.014)	
				-0.031		*-0.025*		*-0.025*	
American Indian	100			-0.010		0.005		0.004	
				(0.039)		(0.038)		(0.038)	
				-0.002		*0.001*		*0.001*	
Asian	342			-0.015		-0.012		-0.012	
				(0.022)		(0.021)		(0.021)	
				-0.005		*-0.004*		*-0.004*	
Hispanic	772			-0.041	**	-0.027	†	-0.027	†
				(0.016)		(0.015)		(0.015)	
				-0.020		*-0.014*		*-0.013*	
Age	9,446			0.022	***	0.020	***	0.020	***
				(0.003)		(0.003)		(0.003)	
				0.446		*0.403*		*0.403*	
Age squared	9,446			-0.000	***	-0.000	***	-0.000	***
				(0.000)		(0.000)		(0.000)	
				-0.394		*-0.354*		*-0.354*	
Education: < High School	693			-0.132	***	-0.121	***	-0.121	***
				(0.017)		(0.016)		(0.017)	
				-0.062		*-0.057*		*-0.057*	
Education: Associate Degree	1,037			0.071	***	0.040	**	0.040	**
				(0.013)		(0.013)		(0.013)	
				0.040		*0.022*		*0.022*	
Education: Bachelor's Degree	1,960			0.183	***	0.143	***	0.143	***
				(0.011)		(0.012)		(0.012)	

		0.134		*0.105*		*0.105*	
Education: Advanced Degree	1,004	0.245	***	0.216	***	0.215	***
		(0.015)		(0.017)		(0.017)	
		0.136		*0.120*		*0.120*	
Married	6,326	0.040	***	0.032	***	0.032	***
		(0.009)		(0.009)		(0.009)	
		0.034		*0.027*		*0.027*	
Region: Midwest	2,601	-0.017		-0.020	†	-0.020	†
		(0.011)		(0.011)		(0.011)	
		-0.014		*-0.016*		*-0.016*	
Region: South	2,686	-0.016		-0.019	†	-0.019	†
		(0.011)		(0.011)		(0.011)	
		-0.013		*-0.016*		*-0.016*	
Region: West	2,067	-0.011		-0.012		-0.012	
		(0.012)		(0.012)		(0.012)	
		-0.008		*-0.009*		*-0.009*	
Metropolitan: Yes	7,336	0.066	***	0.066	***	0.066	***
		(0.010)		(0.010)		(0.010)	
		0.050		*0.049*		*0.049*	
Metropolitan: Not Identified	33	0.086		0.096		0.096	
		(0.067)		(0.065)		(0.065)	
		0.009		*0.010*		*0.010*	
Industry Variables							
Agriculture, Extractive[d]	170			-0.005		-0.005	
				(0.036)		(0.036)	
				-0.001		*-0.001*	
Construction	581			0.009		0.009	
				(0.021)		(0.021)	
				0.004		*0.004*	
Manufacturing (Non-Durable)	589			-0.006		-0.006	
				(0.020)		(0.020)	
				-0.002		*-0.002*	
Transportation	413			-0.019		-0.019	
				(0.024)		(0.024)	
				-0.007		*-0.007*	
Communications	160			0.041		0.041	
				(0.032)		(0.032)	
				0.010		*0.010*	
Utilities and Sanitary Services	149			0.056	†	0.056	†
				(0.033)		(0.033)	
				0.013		*0.013*	
Wholesale Trade	397			-0.001		-0.001	
				(0.023)		(0.023)	
				0.000		*0.000*	
Retail Trade	1,225			-0.115	***	-0.115	***
				(0.018)		(0.018)	
				-0.070		*-0.070*	
Finance, Insurance, Real Estate	655			0.012		0.012	
				(0.020)		(0.020)	
				0.006		*0.006*	
Business and Repair Services	501			-0.052	*	-0.052	*
				(0.021)		(0.021)	
				-0.021		*-0.021*	
Personal Services	218			-0.135	***	-0.135	***

		(0.029)		(0.029)	
		-0.037		*-0.037*	
Entertainment and Rec. Services	116	-0.101	**	-0.101	**
		(0.037)		(0.037)	
		-0.020		*-0.020*	
Hospitals	525	-0.022		-0.022	
		(0.022)		(0.022)	
		-0.009		*-0.009*	
Medical Services (non-Hospital)	471	-0.022		-0.021	
		(0.023)		(0.023)	
		-0.008		*-0.008*	
Educational Services	1,109	-0.148	***	-0.147	***
		(0.019)		(0.019)	
		-0.086		*-0.086*	
Social Services	203	-0.158	***	-0.158	***
		(0.030)		(0.030)	
		-0.041		*-0.041*	
Other Professional Services	437	-0.034		-0.034	
		(0.023)		(0.023)	
		-0.013		*-0.013*	
Public Administration	592	0.003		0.003	
		(0.021)		(0.021)	
		0.001		*0.001*	
Occupation Variables					
Executive, Admin. Managerial	1,487	0.200	***	0.200	***
		(0.015)		(0.015)	
		0.132		*0.132*	
Professional Specialty	1,775	0.173	***	0.173	***
		(0.016)		(0.016)	
		0.122		*0.122*	
Technicians and Related	391	0.135	***	0.136	***
		(0.022)		(0.022)	
		0.049		*0.049*	
Sales	879	0.086	***	0.086	***
		(0.018)		(0.018)	
		0.045		*0.045*	
Service	991	-0.030	†	-0.029	†
		(0.017)		(0.017)	
		-0.017		*-0.016*	
Precision Product., Craft, Repair	1,069	0.111	***	0.112	***
		(0.017)		(0.017)	
		0.063		*0.064*	
Mach. Operat., Fabricators, Laborers	483	-0.016		-0.015	
		(0.022)		(0.022)	
		-0.006		*-0.006*	
Transportation & Material Moving	405	0.049	*	0.049	*
		(0.023)		(0.023)	
		0.018		*0.018*	
Handlers, Equip. Cleaners, Helpers, Laborers	326	-0.079	**	-0.079	**
		(0.024)		(0.024)	
		-0.026		*-0.026*	
Farming, Forestry, Fishing	111	-0.132	**	-0.131	**
		(0.044)		(0.044)	
		0.026		*-0.026*	

CPS Proxy Response and Imputation Controls									
Proxy Responses (2001 or 2001)[e]	6,112	-0.015	†	-0.029	**	-0.025	**	-0.025	**
		(0.009)		(0.009)		(0.009)		(0.009)	
		-0.013		*-0.025*		*-0.022*		*-0.022*	
Imputed Hours or Earnings (2000 or 2001)[f]	4,370	1.083	***	1.051	***	0.984	***	0.984	***
		(0.040)		(0.039)		(0.038)		(0.038)	
		0.973		*0.945*		*0.885*		*0.885*	
Imputation Dummy x Wage 2000 (log)	9,446	-0.410	***	-0.397	***	-0.372	***	-0.372	***
		(0.015)		(0.014)		(0.014)		(0.014)	
		-1.019		*-0.986*		*-0.925*		*-0.372*	
Intercept		0.419	***	0.189	***	0.436	***	0.435	***
		(0.028)		(0.056)		(0.059)		(0.059)	
N		9,446		9,446		9,446		9,446	
Adjusted R^2		0.486		0.529		0.555		0.555	

*** p<0.001; ** p<0.01; * p<0.05; † p<0.1; one-tailed tests for Internet use coefficients, two-tailed tests for all other variables

[a] Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001. Income and hours worked were obtained from Basic CPS data collected in Sep., Oct., and Nov., 2000 and 2001. The analysis excludes non-civilians, respondents under eighteen and over sixty-five years of age, those 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. Coefficients are followed by standard errors in parentheses and betas weights in italics.

[b] Omitted categories are: Internet 2000 - 2001: N - N, Education: H.S. / Some College, Region: Northeast, Metropolitan: No, Industry: Manufacturing (Durable Goods), and Occupation: Administrative Support (including Clerical). Internet and computer use variables measure use anywhere (home, work, or other locations).

[c] Wages for 2000 and 2001 have been converted to 2000 dollars using the CPI-U (http://www.bls.gov/ro9/9210.pdf)

[d] Some industry categories have been aggregated to increase sample sizes: ind_agric includes agriculture, forestry, and mining, while ind_pserv includes household and non-household private services.

[e] 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.

[f] The imputation binary variable is positive for cases in which earnings or hours values were imputed by the Bureau of Labor Statistics.

Appendix Table 3. Regression of 2001 logged wages on Internet use at home and work[a]

	N	I		II		III		IV	
Internet and Computer Use Variables									
Internet: Home and Work (2000 or 2001)[b]	3,486	0.198	***	0.124	***	0.094	***	0.094	***
		(0.011)		(0.012)		(0.013)		(0.016)	
		0.172		*0.108*		*0.082*		*0.082*	
Internet: Home Only (2000 or 2001)	2,414	0.066	***	0.038	**	0.036	**	0.036	*
		(0.012)		(0.012)		(0.011)		(0.014)	
		0.052		*0.030*		*0.028*		*0.028*	
Internet: Work Only (2000 or 2001)	1,186	0.115	***	0.086	***	0.060	***	0.060	***
		(0.014)		(0.014)		(0.015)		(0.017)	
		0.069		*0.051*		*0.036*		*0.036*	
Computer Use: Networked and Non-Networked	7,331							0.000	
								(0.014)	
								0.000	
Labor Market Variables									
Wage 2000 (log)[c]	9,446	0.796	***	0.657	***	0.585	***	0.585	***
		(0.011)		(0.012)		(0.012)		(0.012)	
		0.790		*0.651*		*0.580*		*0.580*	
Union	1,637			0.038	***	0.064	***	0.064	***
				(0.011)		(0.011)		(0.011)	
				0.026		*0.043*		*0.043*	
Sociodemographic Variables									
Male	4,793			0.127	***	0.106	***	0.106	***
				(0.008)		(0.009)		(0.009)	
				0.115		*0.095*		*0.095*	
Black	836			-0.061	***	-0.050	***	-0.050	***
				(0.014)		(0.014)		(0.014)	
				-0.031		*-0.025*		*-0.025*	
American Indian	100			-0.008		0.005		0.005	
				(0.038)		(0.038)		(0.038)	
				-0.001		*0.001*		*0.001*	
Asian	342			-0.016		-0.013		-0.013	
				(0.021)		(0.021)		(0.021)	
				-0.005		*-0.004*		*-0.004*	
Hispanic	772			-0.043	**	-0.028	†	-0.028	†
				(0.016)		(0.015)		(0.015)	
				-0.021		*-0.014*		*-0.014*	
Age	9,446			0.021	***	0.019	***	0.019	***
				(0.003)		(0.003)		(0.003)	
				0.416		*0.384*		*0.384*	
Age squared	9,446			-0.000	***	-0.000	***	-0.000	***
				(0.000)		(0.000)		(0.000)	
				-0.368		*-0.337*		*-0.337*	
Education: < High School	693			-0.131	***	-0.123	***	-0.123	***
				(0.017)		(0.016)		(0.016)	
				-0.062		*-0.058*		*-0.058*	
Education: Associate Degree	1,037			0.069	***	0.039	**	0.039	**
				(0.013)		(0.013)		(0.013)	
				0.039		*0.022*		*0.022*	
Education: Bachelor's Degree	1,960			0.169	***	0.138	***	0.138	***

		(0.011)		(0.012)		(0.012)	
		0.123		*0.101*		*0.101*	
Education: Advanced Degree	1,004	0.227	***	0.208	***	0.208	***
		(0.015)		(0.017)		(0.017)	
		0.126		*0.116*		*0.116*	
Married	6,326	0.038	***	0.030	***	0.030	***
		(0.009)		(0.009)		(0.009)	
		0.032		*0.026*		*0.026*	
Region: Midwest	2,601	-0.018		-0.021	†	-0.021	†
		(0.011)		(0.011)		(0.011)	
		-0.014		*-0.017*		*-0.017*	
Region: South	2,686	-0.017		-0.020	†	-0.020	†
		(0.011)		(0.011)		(0.011)	
		-0.014		*-0.016*		*-0.016*	
Region: West	2,067	-0.012		-0.013		-0.013	
		(0.012)		(0.012)		(0.012)	
		-0.009		*-0.009*		*-0.009*	
Metropolitan: Yes	7,336	0.065	***	0.065	***	0.065	***
		(0.010)		(0.010)		(0.010)	
		-0.049		*0.049*		*0.049*	
Metropolitan: Not Identified	33	0.093		0.098		0.098	
		(0.067)		(0.065)		(0.065)	
		0.010		*0.010*		*0.010*	
Industry Variables							
Agriculture, Extractive[d]	170			-0.001		-0.001	
				(0.036)		(0.036)	
				0.000		*0.000*	
Construction	581			0.013		0.013	
				(0.021)		(0.021)	
				0.006		*0.006*	
Manufacturing (Non-Durable)	589			-0.007		-0.007	
				(0.020)		(0.020)	
				-0.003		*-0.003*	

App. Table 3 (con.)

Transportation	413			-0.012		-0.012	
				(0.024)		(0.024)	
				-0.005		*-0.005*	
Communications	160			0.037		0.037	
				(0.032)		(0.032)	
				0.009		*0.009*	
Utilities and Sanitary Services	149			0.053		0.053	
				(0.033)		(0.033)	
				0.012		*0.012*	
Wholesale Trade	397			-0.000		-0.000	
				(0.023)		(0.023)	
				0.000		*0.000*	
Retail Trade	1,225			-0.109	***	-0.109	***
				(0.018)		(0.018)	
				-0.066		*-0.066*	
Finance, Insurance, Real Estate	655			0.012		0.012	
				(0.020)		(0.020)	
				0.005		*0.005*	

Business and Repair Services	501	-0.053	*	-0.053	*
		(0.021)		(0.021)	
		-0.021		*-0.021*	
Personal Services	218	-0.130	***	-0.130	***
		(0.029)		(0.029)	
		-0.035		*-0.035*	
Entertainment and Rec. Services	116	-0.095	*	-0.095	*
		(0.037)		(0.037)	
		-0.019		*-0.019*	
Hospitals	525	-0.015		-0.015	
		(0.022)		(0.022)	
		-0.006		*-0.006*	
Medical Services (non-Hospital)	471	-0.013		-0.013	
		(0.023)		(0.023)	
		-0.005		*-0.005*	
Educational Services	1,109	-0.147	***	-0.147	***
		(0.019)		(0.019)	
		-0.085		*-0.085*	
Social Services	203	-0.153	***	-0.153	***
		(0.030)		(0.030)	
		-0.040		*-0.040*	
Other Professional Services	437	-0.035		-0.035	
		(0.023)		(0.023)	
		-0.013		*-0.013*	
Public Administration	592	0.001		0.001	
		(0.021)		(0.021)	
		0.001		*0.001*	
Occupation Variables					
Executive, Admin. Managerial	1,487	0.196	***	0.196	***
		(0.015)		(0.015)	
		0.129		*0.129*	
Professional Specialty	1,775	0.171	***	0.171	***
		(0.016)		(0.016)	
		0.121		*0.121*	
Technicians and Related	391	0.134	***	0.134	***
		(0.022)		(0.022)	
		0.048		*0.048*	
Sales	879	0.088	***	0.088	***
		(0.018)		(0.018)	
		0.046		*0.046*	
Service	991	-0.021		-0.021	
		(0.017)		(0.017)	
		-0.012		*-0.012*	
Precision Product., Craft, Repair	1,069	0.121	***	0.121	***
		(0.017)		(0.017)	
		0.069		*0.069*	
Mach. Operat., Fabricators, Laborers	483	-0.003		-0.003	
		(0.022)		(0.022)	
		-0.001		*-0.001*	
Transportation & Material Moving	405	0.060	**	0.060	**
		(0.023)		(0.023)	
		0.022		*0.022*	
Handlers, Equip. Cleaners, Helpers, Laborers	326	-0.069	**	-0.069	**
		(0.024)		(0.024)	

						-0.023		*-0.023*	
Farming, Forestry, Fishing	111					-0.125	**	-0.125	**
						(0.044)		(0.044)	
						-0.024		*-0.024*	
CPS Proxy Response and Imputation Controls									
Proxy Responses (2001 or 2001)[e]	6,112	-0.011		-0.027	**	-0.025	**	-0.025	**
		(0.009)		(0.009)		(0.009)		(0.009)	
		-0.021		*-0.023*		*-0.021*		*-0.021*	
Imputed Hours or Earnings (2000 or 2001)[f]	4,370	1.057	***	1.035	***	0.975	***	0.975	***
		(0.040)		(0.039)		(0.038)		(0.038)	
		0.877		*0.930*		*0.877*		*0.877*	
App. Table 3 (con.)									
Imputation Dummy x Wage 2000 (log)	9,446	-0.401	***	-0.391	***	-0.369	***	-0.369	***
		(0.015)		(0.014)		(0.014)		(0.014)	
		-0.917		*-0.972*		*-0.917*		*-0.917*	
Intercept		0.477	***	0.252	***	0.463	***	0.463	***
		(0.028)		(0.056)		(0.058)		(0.059)	
N		9,446		9,446		9,446		9,446	
Adjusted R^2		0.493		0.532		0.556		0.556	

*** p<0.001; ** p<0.01; * p<0.05; † p<0.1; one-tailed tests for Internet use coefficients, two-tailed tests for all other variables

[a] Source: Current Population Survey Internet and Computer Use Supplement 2000 and 2001. Income and hours worked were obtained from Basic CPS data collected in Sep., Oct., and Nov., 2000 and 2001. The analysis excludes non-civilians, respondents under eighteen and over sixty-five years of age, those 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. Coefficients are followed by standard errors in parentheses and beta weights in italics.

[b] Omitted categories are: Internet 2000 - 2001: N - N, Education: H.S. / Some College, Region: Northeast, Metropolitan: No, Industry: Manufacturing (Durable Goods), and Occupation: Administrative Support (including Clerical). "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 (2000 or 2001)" refers to respondents who used the Internet at home but not work in at least one of the two waves (and did not use the Internet at work in the other wave). "Internet: Work (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 (2000 or 2001)" or "Internet: Home (2000 or 2001)", based on their Internet usage in 2001. Computer use variable measures use anywhere (home, work, or other locations).

[c] Wages for 2000 and 2001 have been converted to 2000 dollars using the CPI-U (http://www.bls.gov/ro9/9210.pdf)

[d] Some industry categories have been aggregated to increase sample sizes: ind_agric includes agriculture, forestry, and mining, while ind_pserv includes household and non-household private services.

[e] 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.

[f] The imputation binary variable is positive for cases in which earnings or hours values were imputed by the Bureau of Labor Statistics.

Endnotes

[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 differentiated in many other ways, of course. Jung, Qui and Kim (2001) note that Internet users vary markedly on several dimensions of intensity and scope of use, and have produced and index of "internet connectedness" to tap such differences. DiMaggio, Hargittai, Shafer and Celeste (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 one that is beyond the scope of this paper and the capacity of existing data.

[3] Individual earnings data in the CPS are only collected 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 was in the fourth month of his or her rotation in August, 2000, and only one third were in their sixteenth 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' reported 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, respectively, 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 diluted slightly the effects of Internet use for those respondents who used it in 2000 but not in 2001, but that 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' movement 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.

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

they used it at work and/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"), or use 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 at 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.

[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 Appendix Table 2. For a list of control variables use din each model, see the source 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 included a proxy flag indicating that another household member answered on behalf of the respondent, as well as a multiplicative term for imputation X 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 use is highly correlated with job skill requirements, and if these requirements are associated with change in wages, then omitting them from the model may lead us to overestimate the impact of Internet use on net wages. Therefore we ran an additional model (not reported here but available upon 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 Occupatinoal Information Netowrk (O*NET), successor to the Dictionary of Ocucpational Titles, and made applicable to the CPS occupation codes by a multi-step crosswalk developed by the second author (details available upon request). Replacing categorical measures of occupation with these new measures left coefficients for Internet use indicators virtually unchanged.

[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 that they possess; that, in other words, home users have higher net earnings because their technological skills enable them to insist upon a higher reservation price. 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.