

Evidence on Learning and Network Externalities in the Diffusion of Home Computers

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Abstract

In this paper we examine the importance of local spillovers – such as network externalities and learning from others – in the diffusion of home computers. We use data on 110,000 U.S. households in 1997. Controlling for many individual characteristics, we find that people are more likely to buy their first home computer in areas where a high fraction of households already own computers or when a large share of their friends and family own computers. Further results suggest that these patterns are unlikely to be explained by unobserved individual traits or by area features. When looked at in more detail, the spillovers appear to come from experienced and intensive computer users. They are not associated with the use of any particular type of software but do seem to be highly tied to the use of e-mail and the Internet, consistent with computers being part of an information or communication network.

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I. Introduction

In this paper we empirically examine the importance of local spillovers – such as network externalities and learning from others – in the diffusion of home computers.

Technology diffusion plays a central role in many theories of development and economic growth.¹ Some recent studies have singled out the diffusion of computers as an engine of growth and as a potential source of fundamental labor market changes.² And network externalities are of recurring interest in industrial organization and public economics.³

We employ a database on the computer ownership and purchase decisions of more than 110,000 U.S. households.⁴ Our notion is that people without computers may learn about the technology from their computer-owning friends and neighbors, or benefit from the size of the local computer "network", say because they can share software or communicate with one another. If so, there may be positive spillovers of existing computer owners on people considering adoption.

Because of the possibility that households have unobserved traits in common, establishing the existence of local spillovers is difficult. People who live in places where a high share of people already own computers may have a greater affinity for technology, even if they do not already own a computer, and therefore may be more likely to adopt. This problem of common traits pervades empirical work on local effects. We will employ several strategies to test whether unobserved common traits, or still other alternatives to network or learning benefits, can explain our findings.

¹ A few examples are Grossman and Helpman (1991), Parente and Prescott (1994), and Aghion and Howitt (1998, chapter 11).

² Discussions of computers as engines of growth can be found in Bresnahan and Trajtenberg (1995), Greenwood and Yorukoglu (1997), Helpman and Trajtenberg (1998), and Andolfatto and MacDonald (1998). The debate over the role of computers in the transformation of the labor market includes Krueger (1993), DiNardo and Pischke (1997), and Autor, Katz, and Krueger (1997). Friedberg (1997) examines the impact of computers on the retirement decisions of elderly workers.

³ Farrell and Saloner (1985) and Katz and Shapiro (1986) provide early analyses of network externalities. Economides (1996) surveys the more recent literature. Dybvig and Spatt (1983) give an overview of the implications for public economics.

⁴ For investigation into the adoption of computers by *firms*, see Bresnahan and Greenstein (1996) and Bresnahan, Stern, and Trajtenberg (1997).

The existence of learning or network externalities in computer adoption could have important policy implications. Learning externalities could mean that the rate of adoption is too slow and possibly justify subsidies to computer and Internet adoption.⁵ An important caveat, however, is that evidence of learning and network *spillovers* is not the same as evidence of learning and network *externalities*. The recipients of spillover benefits may compensate the providers (e.g., "I'll take you to lunch if you will show me how to use this computer"). In keeping with the existing literature, we will refer to learning and network externalities, but the distinction between externalities and spillovers matters for any policy discussion.

In the empirical results that follow, we find evidence consistent with local spillovers in home computer adoption. Using instruments, additional control variables, and a variety of tests and sample periods we find little evidence that the effects are the result of unobserved common traits within cities, local industry composition, availability of computer retailers, or peer pressure ("keeping up with the Joneses").

The data do suggest that the spillovers are concentrated in local areas and among family and friends. The spillovers appear to be greatest from experienced and intensive computer users. The spillovers do not appear to be tied to the use of any particular type of software (spreadsheets, word processors, graphics, games, family budgeting) but are highly tied to the use of e-mail and the Internet. This is consistent with the idea that the computer serves as a part of a local information or communication network.

The rest of the paper proceeds as follows. In section II we describe previous work in related areas. In section III we describe the dataset. In section IV we outline our empirical specification and present basic results on local spillovers. In section V we consider whether

⁵ See Hausman (1998) for a description and evaluation of the existing \$2.25 billion annual U.S. federal subsidy for public school and library Internet access financed by a special tax on phone service. There have been more ambitious (albeit geographically concentrated) subsidies as well, including the Blackburg Electronic Village program in Virginia and the Information Age Town program in Ennis, Ireland. In these cities attempts were made to put a computer in every household and school and to connect everyone to the Internet (for descriptions of these programs see Yaukey, 1997 and MacCarthaigh, 1997).

the results might arise from unobserved common traits. In section VI we investigate the nature of the spillovers try to identify the source of the network benefits. In section VII we conclude.

II. Related Literature

A. Learning and Network Externalities

The issues raised in this paper about learning from others and about network externalities are far from new. The literature in this area is voluminous (see the survey by Economides, 1996). As shown in early work by Farrell and Saloner (1985) and Katz and Shapiro (1986), network externalities can lead to suboptimally slow adoption, suboptimally fast adoption, or even adoption of an inferior technology.⁶ Examples of empirical work examining such issues includes Gandal (1994), Saloner and Shepard (1995), Berndt and Pindyck (1998), and Gowrisankaran and Stavins (1999).

Learning from others can also influence the spread of technology, as argued in Young (1991), Chari and Hopenhayn (1991), Lucas (1993), Jovanovic and MacDonald (1994), and Andolfatto and MacDonald (1998). In his classic study of the diffusion of hybrid corn in the U.S., Griliches (1957) found evidence consistent with late-adopters learning from early-adopters. More recent micro-level empirical studies of learning from others include Jaffe, Trajtenberg, and Henderson (1991), Irwin and Klenow (1994), Foster and Rosenzweig (1995), and Besley and Case (1997).

B. Local Conditions and Peer Groups

In the empirical work we will examine how local conditions affect the rate of computer adoption to test learning and network hypotheses. This is similar to the literature examining peer group effects on social outcomes such as crime and teen pregnancy. The presence of local spillovers has been investigated in various settings and using various methods by Case

⁶ The literature on herding and learning from the behavior of others, recently surveyed by Bikhchandani et al. (1998), contains similarities to both the learning and network literatures discussed here.

and Katz (1991), Evans, Oates and Schwab (1992), Case, Hines, and Rosen (1993), Borjas (1995), Glaeser, Sacerdote and Scheinkman (1996), and Katz, Kling and Liebman (1999), and Ludwig (1999).

This literature has generally found that local effects are important, but has also stressed that unobserved heterogeneity may bias empirical work toward finding local effects. Individuals may be more likely to do something when those around them are doing it because they share unobserved common traits. Evans et al. (1992), for example, show that instrumenting for selection into schools removes the entire estimated impact of peer groups for teen pregnancy. The problem of unobserved common traits and sorting is also known as Tiebout bias in the spirit of Tiebout (1956). Below we will use instruments and controls to try to assess the extent to which our results might come from unobserved common traits.

III. Data

The data we use come from a proprietary December 1997 mail survey by Forrester research called *Technographics 98*. Forrester is a marketing research company specializing in the information economy. The fieldwork for the survey was conducted by the NPD Group. NPD Group received filled-out questionnaires from more than 110,000 American households on their ownership patterns for computers and other electronic goods. The sampling methodology is proprietary but is meant to ensure a nationally representative sample. We found only modest differences when we cross-checked median income, age, and marital status for several states against data in the sample against data reported by the U.S. Bureau of the Census (1998). More details on the *Technographics* program can be found in Bernhoff, et al. (1998). Its purpose is to provide technology, communications, and consumer marketing companies with information for evaluating the consumer segments for their products. The Forrester data is widely respected in the industry and private sector companies pay significant amounts of money to get access to it.

For each respondent the dataset contains demographic information, including gender, race, income, education, age, marital status, whether they have children under 18, whether they use a computer at work, whether they run a business from home, and their state and broadly defined metropolitan area of residence.⁷ The dataset also contains information on how much they watch television, their ownership of various electronic goods, and even some attitude variables such as ratings from one to ten of how much they "like technology". All of this information is gathered in December of 1997.

For anyone with a computer in 1997, the survey also contains information on how many computers they have, how many they have ever had, when they bought their first computer, when they bought their (up to) three most recent computers, how often they use their computer and whether they have Internet access. For those without computers, the survey includes (self-reported) information on how likely they are to buy a computer in the next year and what share of their friends and family use computers.

Using this information we are able to calculate what fraction of people in a city had a computer last year (assuming no one moved) and what share of 1996 non-owners bought their first computer in 1997. We can also keep aging the data backward. We cannot get a true panel, however, because household information such as family composition is given only at the time of the survey.⁸

Table 1 provides average demographic characteristics for, respectively, households who owned a computer at the *start* of 1997, those who did not, households who bought their first home computer *during* 1997, and households who did not own through 1997.⁹ Compared to non-owners, owners at the beginning of 1997 were better educated, richer, and so on.

⁷ The respondents are divided into 208 metropolitan areas that are defined by the television market they reside in. These areas are generally larger than comparable SMSAs. The San Francisco area, for example, includes all of the Bay Area.

⁸ For a discussion of the potential problems with such retrospective data see Besley and Case (1993) or Hamermesh (1998).

⁹ The sample in the second group consists of those in the last two groups. No households reported going from owning at the beginning of 1997 to not owning at the end of 1997.

Likewise, among those not owning at the start of 1997, those adopting during 1997 were better educated, richer, etc. At the start of 1997, 39.7% of households reported owning; at the end of 1997, 44.6% of the households reported owning a home computer. By comparison, the Electronic Industries Association estimated 40% ownership during 1997, and the Current Population Survey 37%.

Figure 1 presents a map of end-of-1997 computer ownership rates by state. For the sample of 208 cities, Figure 2 plots the 1997 adoption rate (the percentage of households not owning at the beginning of 1997 who bought during 1997) against the fraction of households owning at the beginning of 1997. Regression coefficient? As shown, cities with high cumulative adoption rates through 1996 continue to have high adoption rates in 1997. The coefficient is .127 (standard error .025). This may simply owe to positively correlated demographics across households within a city, so we now proceed to examine the data on individual households.

IV. Empirical Specification and Basic Results

A. Empirical Specification

We concentrate on the dichotomous choice facing people who do not yet have a home computer at the start of the year of whether to buy a computer. For household i in year t , call this decision y_{it} , where $y_{it} = 0$ if the household does not adopt and $y_{it} = 1$ if the household adopts. If p_{it}^* is household i 's reservation price in year t and p_{it} is the market price facing household i in year t , then

$$y_{it} = 1 \text{ if } p_{it}^* \geq p_{it}$$

$$y_{it} = 0 \text{ if } p_{it}^* < p_{it} .$$

Consider a household that buys in year t . Since this is the first purchase, this is the first year in which the market price of a computer has been below the household's reservation price.

This may have come about because the market price fell, the household's reservation price rose, or some combination. We specify that

$$(1) \quad \text{Probability}(y_{it} = 1) = \lambda \text{CITY}\%_{t-1} + \beta x_i^o + x_{it}^u + c_{it}^u + u_{it} .^{10}$$

$\text{CITY}\%_{t-1}$ is the variable of interest. It is the fraction of households in the city having a computer in the previous year. If there are local learning and network externalities, then nonowners living in areas where owners are prevalent will be more likely to buy one (controlling for all other factors), leading to $\lambda > 0$.¹¹

The x_i^o are household observables. In the basic specification these are age, education, income, gender, race, marital status, the presence of children, whether the respondent uses a computer at work, and whether the respondent runs a business from home. There is no time subscript since we have this data for 1997 only.

The x_{it}^u represent household unobservables that are correlated with the $\text{CITY}\%_{t-1}$ but uncorrelated with x_i^o . Although families may not sort into cities based on their propensity to own computers, they may sort on characteristics that are correlated with that propensity. To contribute to x_{it}^u , however, the sorting must be over and above sorting on observables like income, age, education, or use of a computer at work. We have in mind something like technological sophistication that is correlated across households within cities but is not captured by the observables. Measurement error in x_i^o (e.g., errors in reported income, or the difference between permanent and current income) could also contribute to x_{it}^u .

¹⁰ We use a linear probability model for simplicity, particularly in the IV context. Our basic results were the same using a probit model.

¹¹ This model is analogous to epidemiology models in which an infectious disease spreads more quickly the larger the fraction of the population infected. In the marketing literature, this is known as the Bass (1969) model, and many examples of its use can be found in the survey by Mahajan, Muller and Bass (1993). The model is also very similar to the work of Glaeser (1997) on learning within cities and resembles work on local interactions such as Durlauf (1993), Brock and Durlauf (1995), and Lucas (1998).

The c_{it}^u are city-level unobservables such as the quality and price of Internet access, the price of computers, and the density of computer stores. It is important to note, though, that these differences may arise in response to city differences in computer ownership rates, and thus themselves represent network externalities. Finally, the u_{it} are idiosyncratic household unobservables and household unobservables that *are* correlated with the observables x_i^o .

The unobservable terms in (1) clarify the potential sources of bias in a regression of y_{it} on $CITY\%_{t-1}$ and x_i^o . In the spirit of the peer group literature, if the CITY% is positively correlated with the x_i^u , then the estimated local effect ($\hat{\lambda}$) will be biased upward. If people in Silicon Valley love technology, they may be more likely to own computers and to buy them even if they do not yet own them. This will spuriously make the spillover seem large. Similar biases arise when differences in city specific unobservables, c_i^u , are large. On the other hand, the estimates may be biased downward because of survivor bias (see Heckman and Singer, 1985). If the only people living in Silicon Valley who do not own computers in 1997 actually hate technology and will *never* buy a computer, this will create a downward bias in our estimated $\hat{\lambda}$. In either case, instrumenting is necessary.

B. Basic Results

We start by presenting the purely cross-sectional regression of household ownership on city ownership. The dependent variable is a $\{0,1\}$ for whether the individual owns a computer and the independent variable of interest is the mean ownership rates of other people in the metropolitan area. Column 1 of Table 1 shows that a household is more likely to own if other households in the same metro area own (coefficient .294, standard error .034), even controlling for household characteristics such as education, income, age, and whether a computer is used at work. This coefficient may be biased upward because of unobservable city features and correlated household observables. In contrast to this regression of ownership on contemporaneous ownership of others in the city, we now turn to regressions of *first-time adoption on lagged ownership* of others in the city.

We are more interested in the impact of lagged ownership on the probability of first-time adoption for two reasons. First, it may mitigate the bias from correlated household unobservables. By looking only at non-owners and asking if they are more likely to adopt if surrounded by more owners, we are isolating people who are demonstrably different from computer owners. Second and more important, the economic logic of learning and network externalities suggests that the stock should affect the flow. In the case of learning, a bigger stock means more owners from which to learn how to use and buy a computer, promoting adoption. In the case of network externalities, a bigger stock means a bigger network in which to participate.¹²

In column 2 of Table 2 we look at individuals who do not own computers at the end of 1996 and ask whether they are more likely to buy one in 1997 if there were many owners in their city in 1996. The estimated coefficient on local ownership rates, λ , is positive and highly significant, suggesting that local spillovers may be important. The t-statistic is 5.7.¹³ It is also economically important. The point estimate of .10 implies that, controlling for household-specific observables, a non-owner in a city with 10 percentage points higher computer ownership in 1996 has a 1 percentage point higher probability of making a purchase in 1997.¹⁴ This is substantial relative to the 1997 mean adoption rate for non-owners of 8%. The levels coefficient in column 1 of Table 2 is likely to be higher than the adoption coefficient in column 2 both because of the cumulative nature of the levels regression and because it is more susceptible to the bias from common unobservables across people.

The other coefficients listed in column 2 have predictable signs. Households with more income and education are more likely to buy their first computer. Using a computer at work, running a business from home, and having children in the household are also associated

¹² In fact, theory suggests that what should matter is the expected size of the network over the entire lifetime of the computer. This could also hold true for learning if adopters expect to be learning from other owners even after they have adopted. This is quite difficult to deal with appropriately, however, since the path of computer ownership will be strongly affected by the hard-to-predict future rate of decline of computer prices.

¹³ All of the standard errors in the paper are corrected for the fact that the CITY% does not vary by household.

¹⁴ We obtained very similar results using the share of people in a city who either own a computer at home or use a computer at work for CITY% (rather than just the share who own a computer at home).

with a higher probability of first purchase. Since this is a linear regression, the coefficients are marginal probabilities. For example, having a child between 6 and 17 in the household means a 4.2 percentage point higher probability of buying. The largest marginal effect (as well as the largest t-statistic) is on using a computer at work. This raises the probability of purchase by 7 percentage points, almost doubling it at the mean of the covariates.

Columns 3 through 5 estimate the same specification but for different years. Column 3 looks at the purchase decisions of non-owners in 1996 as a function of city ownership in 1995.¹⁵ Column 4 examines the decision in 1995 as a function of ownership in 1994. In both cases, the estimated coefficients remain large, positive and highly significant. The coefficient in 1996 is .145. The coefficient in 1995 is .138. In column 5 we examine the responses of 1997 non-owners to the question "How likely are you to purchase a computer in the next year?" (rated from 1 to 10) as a function of city ownership in 1997. Here we construct a pseudo-probability of buying by subtracting 1 and then dividing by 10. This gives a scale from 0 to .9. In this prospective purchase regression, increased ownership rates in a city are again associated with significantly higher probabilities of buying. The coefficient of .105 for prospective purchases is close to our baseline estimate of .104 for actual purchases.

The results in Table 2 show a robust coefficient on CITY%, consistent with local learning and network benefits. But this is also consistent with unobserved common traits between the new adopters and existing owners, so in the next section we test whether the coefficient can be attributed to this alternative hypothesis. Before doing so, however, we illustrate the quantitative importance of the spillover implied by the point estimate for 1997.

Summing equation (1) across households within a city yields

$$(2) \quad \frac{f_{ct}}{1-F_{c,t-1}} = \lambda F_{c,t-1} + \beta x_c^o + c_t^u + x_{ct}^u,$$

where f_{ct} is the fraction of city c households who buy their first computer in year t and $F_{c,t-1}$ (=

¹⁵ The demographics of the individual are given only in 1997 so we use the same values for earlier years.

$CITY\%_{t-1}$) is the fraction of households in city c who own a computer in year $t - 1$. i.e., f and F are the density and cumulative density, respectively, of computer adoption (first-time computer purchase). We then have

$$(3) \quad F_{ct} = F_{c,t-1} + f_{ct} = F_{c,t-1} + [1 - F_{c,t-1}] [\lambda F_{c,t-1} + \beta x_c^o + c_t^u + x_{ct}^u],$$

which is precisely the Bass (1969) diffusion model. Analogous to models of a contagious disease, the fraction of the city with a computer this year (F_{ct}) is equal to the fraction last year ($F_{c,t-1}$) plus the hazard rate for buying a computer ($f_{ct}/(1 - F_{c,t-1})$) times the fraction of the population which is at risk ($1 - F_{c,t-1}$). When $\lambda > 0$, the hazard is rising in the fraction of the population who own in the previous period.¹⁶

For non-owners, the 1997 hazard rate in our sample was approximately 8%. Given our baseline estimate of $\lambda = .10$ and an average $F_{c,t-1}$ across cities of 40% in 1996, $\lambda * F_{c,t-1}$ contributed 4 percentage points to the 8% hazard rate. The remaining terms ($\beta x_c^o + c_t^u + x_{ct}^u$) contributed the other 4 percentage points. Thus in 1997 one half of the adoption rate may have come from local spillovers. In short, if our coefficient reflects spillovers, then spillovers substantially affect the speed of diffusion.

V. Unobserved Common Traits

The most obvious alternative explanation for the positive coefficient on local ownership is that it represents nothing more than the existence of unobserved common traits across people in a city. In technologically sophisticated places where a large fraction of the population already owns computers, non-owners may also be more sophisticated and thus likely to buy computers, even controlling for observables like education and income. We take three approaches to get around the problem of unobserved common traits.

¹⁶ Notice that in this model computers can spread even if there are no spillovers, say because of falling prices interacting with the determinants of household reservation prices ($\beta x_i^o + c_{it}^u + x_{it}^u$).

A. Controls for Unobservable Sophistication

If the local spillover is coming from unobservable technological sophistication, then adding variables correlated with an individual's sophistication ought to reduce the coefficient on CITY %. In column 2 of Table 3 we add 23 additional controls to our original set of 10 demographic controls: 3 interactions of the demographic variables (income*education, education*age, and income*age); 7 dummies for ownership of other consumer electronics (satellite dish, big-screen TV, cordless phone, CD player, component stereo system, VCR, and answering machine); 3 "attitude toward technology" variables (self-ratings from one to ten of how well the statements "I like technology," "technology is important to me," and "I like to spend time learning about new technology products" describe the respondent's personality); 5 dummies for categories for hours of TV watching; and 5 dummies for wealth categories.

Of the 23 additional controls, 17 are statistically significant: nine at the 1% level, five more at the 5% level, and three more at the 10% level. The three individually most significant are ownership of a CD player (t-stat = 9.5), "technology is important to me" (t-stat 6.5), and ownership of a cordless phone (t-stat = 6.3). The inclusion of these extra, significant controls causes the coefficient on CITY% to fall only modestly from .1192 to .1107, with the standard error edging up from .0218 to .0220.¹⁷ Since we think these additional controls are likely to be correlated with a household's unobserved sophistication (and permanent income), the fact that the CITY% coefficient survives almost wholly intact makes us more (but far from fully) confident that the estimated CITY% coefficient does not merely reflect the correlation between CITY% and x_{it}^u .

The next regression is motivated by the debate over the impact of computers on wages. DiNardo and Pischke (1997) show that, while using a computer seems to raise wages substantially (Krueger, 1993), so does using a pencil and sitting down while working. Further,

¹⁷ Because many people have missing values for at least one of the additional variables, the sample size is smaller. Column (1) produces a baseline regression coefficient of .1192, compared to the .1043 reported with the larger sample in Table 2

controlling for pencil use and working while sitting often lowers the estimated wage effect of computers substantially. They argue that this casts doubt on a causal interpretation of the computer coefficient.

To apply this to our context, column 3 of Table 3 adds to the regression of column 2 the fraction of households in the city who own each of the seven consumer electronic goods. We do not think there are plausible learning or network benefits for computer adoption arising from widespread use of stereos, VCRs, and so on. Thus if these variables matter and lower the coefficient on the fraction of people owning computers, it would cast doubt on a spillover interpretation. We find in column 3, however, that the results change little. The local effect of computer ownership is still positive and significant (t-statistic of 3.4) and is very similar in magnitude (.1193 vs. .1107). Although we do not list the other coefficients for space reasons, none of the seven ownership fractions is significantly positive, and the same is true when we add them individually rather than collectively.¹⁸

To summarize, adding variables likely to be correlated with unobservable sophistication (and permanent income) does not change the estimated importance of spillovers.

B. Instrumental Variables

Our second strategy for dealing with household unobservables is instrumenting. Instruments must be relevant (correlated with the fraction of the city that owns computers) and valid (uncorrelated with the household's unobservables).

In column 4 of Table 3 we use the city means of the 10 household variables (education, income, age, and so on) that appear in the baseline regression in column 1. Positive local externalities mean that, conditional on its characteristics, a household should be more likely to buy its first computer if it is surrounded by households with observables favorable to computer ownership. For example, a childless household surrounded by households with kids should be

¹⁸ We also re-estimated the column 3 specification with dummies for 15 income categories, 5 education categories, and 3 age categories rather than single variables for income, education, and age. The coefficient actually rose a negligible amount, from .1193 to .1211 (.0348).

more likely to adopt than a childless household surrounded by childless households. Thus city means should be relevant instruments, and they are (the 1st-stage R^2 is 0.88).¹⁹

But are city mean observables (call them x^o) valid instruments, i.e., uncorrelated with household unobservables (x_i^u)? One might worry that they are positively correlated because, say, cities with lots of households with children are filled with the more technologically savvy. But note that x_i^u is orthogonal to x_i^o by construction. We defined x_i^u to be the component of household unobservables that is correlated with the CITY% *conditional* on a household's observables. We included the part of the unobservables that is correlated with a household's observables in another error term, u_i . We did this deliberately to clarify that, since household observables are included in the regression, correlation between unobservables and observables biases the β coefficients on the observables, but not the λ coefficient on the CITY%. For example, the coefficient on kids in the household should incorporate any correlation between a the household's sophistication and the presence of kids. For this reason, *city mean* observables would not be correlated with x_i^u simply because cities with kids tend to be filled with technological sophisticates. It would have to be that, controlling for whether a household includes kids, it is more savvy the higher the fraction of households in the city with kids.

As column 4 of Table 3 shows, using these 10 city mean variables as instruments gives a similar answer to OLS (.1145 vs. the .1043 in the comparable sample OLS estimate in column 2 of Table 3). The CITY% coefficient is still estimated quite precisely (t-statistic of 6.1). Importantly, we cannot reject the nine overidentifying restrictions at the five percent level (p-value of .16).²⁰ We obtained similar results (and also could not reject overidentifying restrictions) when we excluded various subsets of the instruments such as, respectively, the two work variable city means, the income and education city means, and the race city means. Thus, for example, adoption is more likely if a household is surrounded by households with

¹⁹ Case and Katz (1991) develop this insight and propose a likelihood ratio test of, in our example, whether the city means for the observables matter for individual decisions. When we did this test using our data, we easily rejected the hypothesis that there are no local effects.

²⁰ In testing the overidentifying restrictions, we take account of the fact that the data are grouped by city using the technique of Hoxby and Paserman (1998).

kids, controlling for whether the household has kids or not, as shown in column 5 of Table 3 (the coefficient is actually noticeably higher at .1676, with a standard error of .0531) .

To summarize, the instrumental variables results also suggest that unobserved common traits are not likely to be the source of the CITY% coefficient.

C. Spillovers by Type of Owner

Our third check for the importance of unobserved common traits uses information on who the existing owners are. Now, our basic regression shows that having high local ownership of computers makes non-owners more likely to adopt in the coming year. If this comes from common traits across owners and non-owners (such as technical sophistication), then the estimated spillovers should be largest from owners who are most like the non-owners. The network and learning spillover story, however, predicts something quite different. It predicts that experienced, heavy users should be the most important influence on potential adopters because they have the most information and are the most valuable members of a local network.

Using the information in the survey on how many computers a household has ever owned, we divide city ownership into two groups: people who have owned two or more computers in their lifetime (19% of all households at the end of 1996) and people who have owned only one in their lifetime (also 19% of all households at the end of 1996). We would expect non-owners to have more traits in common with people owning their first computers than with experienced owners, so if the unobservables bias explanation is correct there should be particularly high rates of adoption in places where there are many first time owners. On the other hand, if the spillover explanation is correct, since multiple-computer owners are likely to be better informed, have more software to share, and so on, the coefficient on experienced users should be larger. The results, presented in column 6 of Table 3, show that, in fact, multiple-lifetime-purchasers are substantially more influential. The coefficient for the fraction

of city households that are multiple-lifetime-purchasers is .123 (standard error .024, t-statistic 5.1), while the coefficient for single-purchasers is .061 (standard error .056, t-statistic 1.1).

Similarly, in column 7 of Table 3 we classify computer owners into two usage groupings. We define households who report using a computer more than 20 days per month as "heavy users" and those using it fewer than 20 days per month as "light users." From this we decompose the CITY% into the share of the city that owns a computer and uses it more than 20 days per month and the share that owns but uses it fewer than 20 days per month (these average 26% and 12% of households, respectively). Again, we expect the unobserved traits of the light users to be most like those of the non-owners, whereas any spillovers should be more important from the heavy users. Again, the results show that the coefficient is much larger on the group that is less likely to share unobserved common traits (.137 on heavy users versus -.007 on light users). Indeed, the light users have no significant spillover at all.

To summarize, each of our three approaches suggests that the estimated effect of city ownership on future adoption rates is not likely to be caused by common unobserved traits.

VI. Identifying the Type of Network

If the CITY % coefficient is a true spillover and not just a consequence of unobserved common traits, then we would like to know more about the channel and nature of the spillover. In this section we try to determine whether local schools are an important channel, whether computer adopters are trying to keep up with the Joneses who already own computers, whether local computer retailers and the local computer industry play a special role, and whether any externalities might operate through the use of e-mail and the Internet.

A. Local Schools

One potential explanation for the city ownership coefficient is that it is being driven by computer use in local schools. School districts in which lots of families own computers may, for example, draft curricula that encourage non-owning families to buy a computer. To see if

local schools are a possible network hub, in column 1 of Table 4 we run our baseline specification including only households *without school-age children* (hence the smaller number of observations and the absence of the children demographic control). The coefficient on city ownership (of all households, those with and without kids 6 to 17) is again significant and has a similar magnitude (.094 versus .104). Thus the school system cannot directly explain the local spillovers for these households. The school system may be an important conduit of learning and network benefits of computers, but this regression suggests those benefits are not restricted to families with children in school.

B. Peer Pressure and Keeping Up With the Joneses

One possible effect of local ownership on computer adoption, distinct from conventional learning or network externality stories, arises from a desire to show off and impress others, i.e., to "keep up with the Joneses." This type of local effect would have very different policy implications. In a world with learning from others and network benefits, it may be appropriate to subsidize adoption. In contrast, if people are buying computers just to show off, it may be appropriate to *tax* adoption.

Distinguishing "good" local influences from "bad" ones is quite difficult, but in column 2 of Table 4 we examine the computer use of people who bought their first computer in 1997. We might expect those buying computers just to keep up with the Joneses to use their computers less frequently once they own them. That is, if peer pressure causes our results, there might be a negative relationship between frequency of computer use by new buyers and the share of people in the city who already own computers. Higher CITY% places should have more peer pressure and more buying just for show (and thus lower use).

The results in column 2, where the dependent variable is the days per month that new buyers use their computers, indicate no significant relationship. New buyers do not use their

computers less frequently if they buy in places where there are many owners.²¹ The point estimate is small and insignificant. The coefficient is -0.13 with a standard error of 2.1, whereas the mean use for new buyers is 23.2 days. To us, this casts some doubt on the status interpretation.

C. Local Prices/Local Industries

Another explanation of our local effect is that cities where there are many computer owners may have large numbers of people working in the computer industry, or they may have lower computer store prices, denser networks of computer stores, and cheaper access to the Internet. This may increase the probability of buying and thus explain our coefficient.²² This local effect could itself arise because of network externalities. Cities with lots of computer-owning households may endogenously have a dense network of computer retailers and local phone numbers to access the Internet, benefiting new adopters.

To test this explanation we examine the geographic areas in more detail. Thus far we have been grouping households according to metropolitan area. For each person we also have the state they live in, and many of the metropolitan areas cross state boundaries. The New York City area, for example, includes people in New Jersey and Connecticut. We therefore create a narrower local area, the "city-state". This splits a city like New York into three

²¹ This is also contrary to what one would expect from differences in local prices, availability of computer retailers, and computer advertising. If adoption is high because (say) local prices are low, adopters should include more marginal buyers who will use their computer less intensively. What the learning and network stories predict is not as clear. More learning and a bigger local network should lead to more use by a given household, *conditional* on its ownership. But the spillovers should, like price differences, pull in more marginal buyers who will use their computer less intensively.

²² Fixed city price differences are actually not sufficient to generate a positive correlation between the CITY% and the adoption rate of *non-owners*. For non-owners, the local market price has up to now exceeded their reservation price. If the distribution of reservation prices across households is uniform between 0 and P (so that low price cities have higher ownership), then low price cities would need to have more rapidly falling computer prices in percentage terms. i.e. one would need price divergence, not just price level differences. If the distribution of reservation prices is non-uniform, however, then level differences in city prices could produce positive or negative local effects. (Similarly, city differences in P – the upper bound on the uniform distribution of household reservation prices – would not generate a positive correlation between the CITY% and the subsequent adoption rate of non-owners.)

different city-states: New York-New York, New York-New Jersey, and New York-Connecticut. We then create the fraction of ownership within each of these city-states.

In column 3 of Table 4 we repeat our standard regression but with the ownership shares both by city-state and by city. The evidence is quite clear that the local effect is concentrated at the more local, city-state level (coefficient .094 on the CITY-ST% vs. only .011 on the CITY%). In column 4 we add city dummies which should absorb any metropolitan area level differences in industry composition, Internet access, computer store availability, computer advertising, and so on. The coefficient on the local spillover remains large, positive and significant (the coefficient is .088 with a standard error of .033). Thus the local effect cannot be explained by differences in any citywide features.²³ To explain the results, prices et al. would have to differ systematically *within* metropolitan areas. Because of these results, in the regressions that follow we will use the CITY-ST% rather than the CITY%.

Of course, adjacent city-states may indeed differ in computer prices, Internet access, and so on. Fortunately, we are able to examine the issue in even greater detail using the information given by end-of-1997 non-owners on how likely they are to buy a computer in the next year (1998). These same respondents were also asked how many of their family and friends own personal computers (potential answers to the latter being "all", "most", "some", "very few", and "none").

Column 5 of Table 4 shows a regression of the reported likelihood of buying on dummies for the share of friends and family that use computers, the fraction of the city-state that own computers, and our standard list of household observables. The results show that, the larger the fraction of family and friends that own a computer, the higher the reported likelihood of a first purchase in the next year. Going from "none" to "all" of friends and family owning computers raises the reported likelihood by .21, a considerable amount relative

²³ A variant of the local price hypothesis is that the presence of computer owners affects adoption through the market for used computers. Cities with many owners may have lots of inexpensive or free old computers. Our data contains information for some respondents on the type of store in which they purchased their computer. Using this information we found the same spillovers from local ownership looking only at the decision to buy a *new* computer.

to the mean likelihood of .25. The friends and family dummies are highly significant, with t-statistics ranging from 9 to 40. Importantly, their inclusion renders the estimated spillover at the city-state level small and insignificant: .032 with a standard error of .019. (The regression with the same dependent variable but no friends and family dummies yielded a coefficient of .105, with a standard error of .026, in column 5 of Table 2.) Column 6 of Table 4 shows that adding city-state dummies does not materially change the estimated effects of ownership by friends and family. The fact that friends and family dummies eclipse the CITY-ST% represents, in our view, strong evidence against the view that unobserved city-state features (prices, Internet access, computer ads, computer stored density) explain the significance of the CITY-ST% in other regressions. For variables such as prices to explain the importance of the CITY%, the prices would need to be more specific to the household and its friends and family than to the city-state in which the household resides.

These "friends and family" results could, of course, also be explained by common unobserved traits among friends and family. Correlation with unobserved traits is surely responsible for much of the explanatory power of the friends and family dummies.²⁴ But the correlation between the friends and family dummies and the CITY% need not owe to common unobserved traits. Three lines of evidence presented in the previous section that cast doubt on the common traits interpretation of the CITY% coefficient. We think the evidence — taken together — suggests that friends and family variables render the CITY% coefficient insignificant because friends and family with computers are the *channel* by which the CITY% enters significantly. It seems plausible that the spillovers occur among friends and family members — precisely the people a household interacts with most. Friends and family may be

²⁴ Comparing the R²'s of around .26 in the regressions with friends and family variables (columns 5 and 6 of Table 4) to the R²'s of around .06 in other columns actually overstates their explanatory power. Columns 5 and 6 use a more continuous dependent variable (likelihood of buying for the first time in 1998 of 0, .1, ..., .9) than the other regressions (dependent variable equal to 1 for adopters, 0 for non-adopters), and hence naturally tend to have higher R²'s. The proper comparison is with column 5 of Table 2, which has the same dependent variable but no friends and family dummies. This shows that friends and family dummies push the R² up from .225 to .261. Thus, although they noticeably improve the fit, their tremendous significance also comes at the expense of the significance of the other demographic variables.

precisely the people with whom one exchanges most e-mail from home, most software used at home, etc.²⁵

D. Internet and e-Mail Networks

In Table 5 we present a series of regressions, each of which breaks the CITY-ST% into computer users who do and do not report using their computer frequently for specific activities. If there are networks associated with sharing software files, for example, we might expect that spreadsheet or word processing computer users would have more influence on new adopters than would owners who do not use those types of software. The first 5 columns of Table 5 reveal, however, that spillovers from computer owners are equally strong from users and non-users of word processing, spreadsheets, games, graphics, and family budgeting — precisely the types of software where file sharing might be prevalent.

Column 6 shows that spillovers do not appear to be from users of home computers for work. The spillovers appear larger from those who do not use their computers to do work at home than from those who do, although the difference is not significant at the 10% level.

More significantly, columns 7 and 8 of Table 5 are consistent with the view that computers are components of local communication and information networks. In these columns we look at users who frequently use the Internet and e-mail. People who frequently use these appear more influential. The coefficient on Internet households is .134 compared to only .046 on other households (the p-value that the coefficients are the same is .07). The coefficient on e-mail users is .144 versus .030 on those who do not use e-mail (p-value .03 on their equality). These are suggestive of local communications networks but are also consistent with local learning if Internet and e-mail users are more knowledgeable than other computer

²⁵ We do not know how geographically local friends and family typically are, but the learning and network stories involve interaction, not proximity per se. We do not give these friends and family results a more central place in the paper because friends and family computer ownership is only asked of households not owning a computer at the end of 1997. Thus, for example, we do not have this information for households who actually adopted a computer in 1997. Moreover, we cannot observe the fraction of friends and family who use computers intensively, etc., whereas we can observe the fraction of households in the city who do so.

owners, or are more active in communicating with others. We do not know what share of e-mail traffic goes to local users, or what fraction of web-browsing involves local-content websites.²⁶

VII. Conclusion

Using data on 110,000 U.S. households in 1997, we are finding evidence that local spillovers are important for household computer adoption: households are more likely to buy their first computer when a high fraction of people around them already own computers. Our point estimates imply that such spillovers could play a quantitatively important role in the spread of home computers, perhaps doubling the rate of adoption.

Applying a battery of tests, we found that this effect was robust and unlikely to be explained by common unobserved local traits or by alternative network explanations such as local prices, local industry composition, local schools, or peer pressure. The networks do not appear to be tied to any particular type of software nor to the use of an at-home computer for work. Instead, networks seem related to use of the Internet and e-mail, consistent with computers as the hub of local information and communications networks.

²⁶ When people with on-line access are asked what they usually do on-line, one top answer does not appear to be particularly local (42% usually visit reference sites), but five of the next six may or may not include significant local content (45% usually visit product or company websites, 37% check the weather, 29% read a daily newspaper, 25% usually visit a sports site, and 24% participate in on-line chats). A bigger local pool of on-line computer owners means, perhaps, endogenously greater supply of websites with local information.

Figure 1

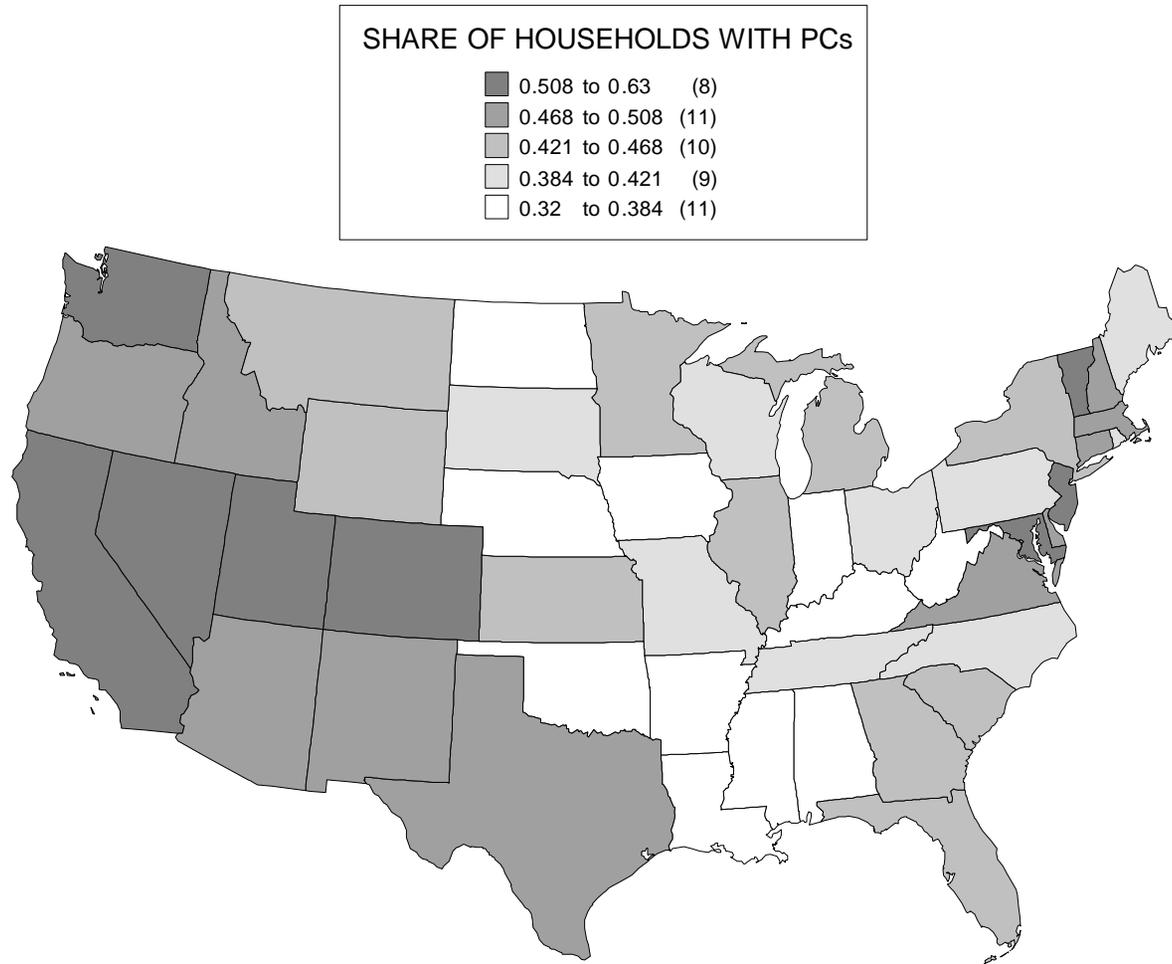


FIGURE 2:

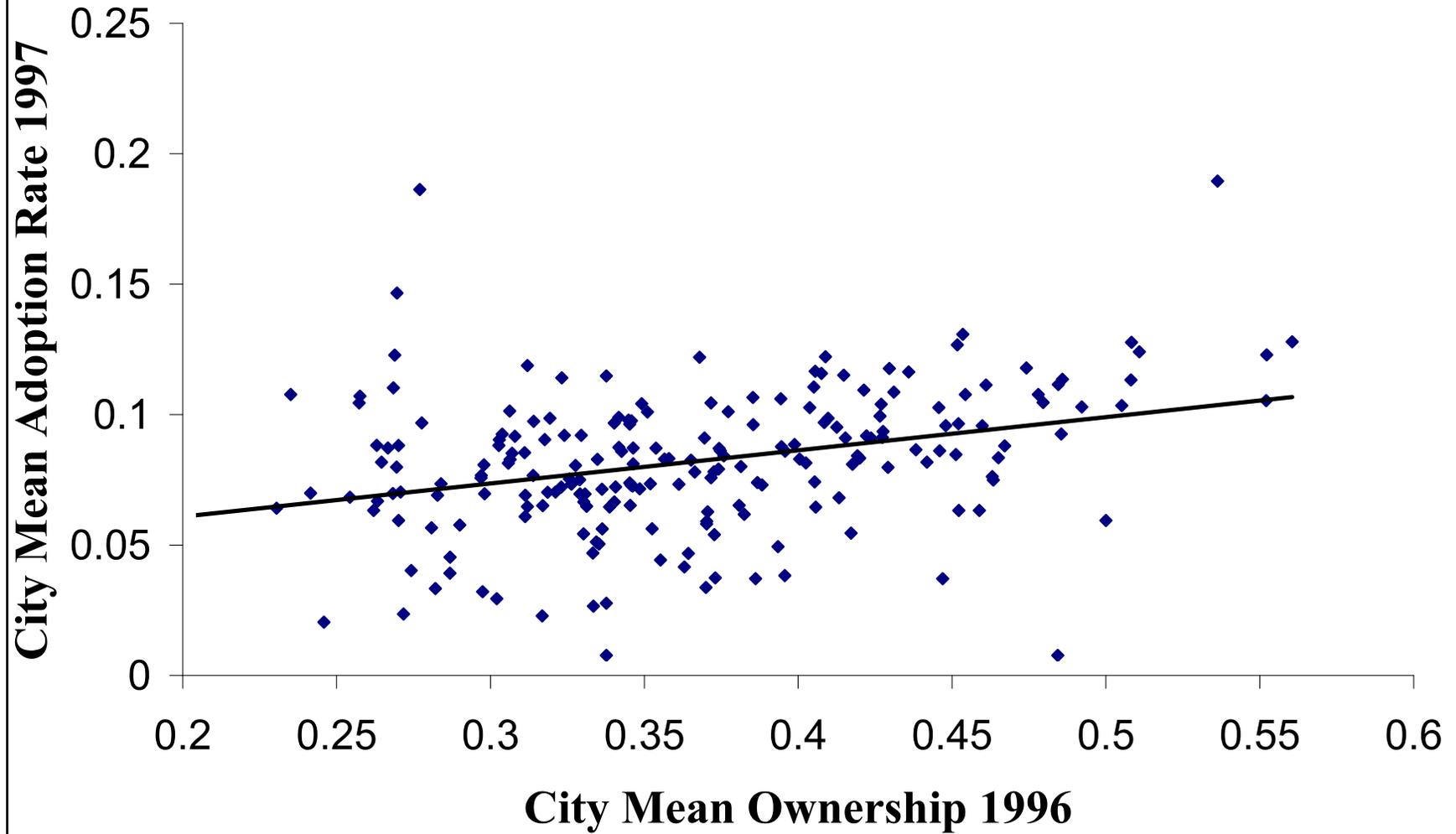


TABLE 1: DEMOGRAPHICS OF 1997 NON-OWNERS VS. 1997 OWNERS

Variable	Computer Owners at the Start of 1997	Those Not Owning at the Start of 1997	Those Adopting During 1997	Those Not Owning Through 1997
Income	53.7 (30.5)	31.4 (23.8)	44.2 (27.9)	30.3 (23.1)
Age	44.5 (13.2)	49.0 (15.3)	43.0 (13.5)	49.5 (15.4)
Education	14.5 (2.4)	12.8 (2.2)	13.6 (2.3)	12.8 (2.2)
Female	.466 (.499)	.589 (.492)	.522 (.500)	.594 (.491)
Single	.350 (.477)	.518 (.500)	.391 (.488)	.529 (.499)
Kids Age 6-17	.358 (.479)	.219 (.413)	.352 (.478)	.208 (.406)
Asian	.016 (.126)	.007 (.082)	.012 (.111)	.006 (.079)
Non-Asian Minority	.098 (.297)	.134 (.340)	.122 (.327)	.135 (.342)
Use a Computer at Work	.684 (.465)	.314 (.464)	.592 (.492)	.290 (.454)
Run a Business from Home	.181 (.385)	.090 (.286)	.164 (.370)	.083 (.276)
Number of Observations	40,472	61,399	4,967	56,432

Notes: Standard deviations are in parentheses. Education and age are in years, income is in thousands, and the other variables are in fractions of one.

TABLE 2: BASIC RESULTS AND ROBUSTNESS

	(1) Ownership 1997	(2) Adoption 1996-97	(3) Adoption 1995-1996	(4) Adoption 1994-95	(5) Likelihood 1997-98
CITY % (year t)	.2935 (.0338)				
CITY % (year t-1)		.1043 (.0182)	.1450 (.0165)	.1380 (.0203)	.1048 (.0264)
INCOME	.0030 (.0001)	.0009 (.0001)	.0013 (.0001)	.0013 (.0001)	.0016 (.0001)
AGE	-.0023 (.0001)	-.0009 (.0001)	-.0006 (.0001)	-.0003 (.0001)	-.0046 (.0001)
EDUCATION	.0356 (.0009)	.0059 (.0006)	.0097 (.0006)	.0096 (.0006)	.0098 (.0006)
FEMALE	-.0317 (.0025)	-.0051 (.0021)	-.0047 (.0020)	-.0022 (.0022)	-.0150 (.0026)
SINGLE	-.0759 (.0037)	-.0219 (.0024)	-.0225 (.0025)	-.0223 (.0024)	-.0015 (.0027)
KIDS	.1086 (.0051)	.0419 (.0041)	.0632 (.0041)	.0513 (.0034)	.1114 (.0041)
ASIAN	.0558 (.0117)	.0283 (.0184)	.0371 (.0180)	.0399 (.0133)	-.0348 (.0165)
NON-WHITE	-.0537 (.0052)	-.0122 (.0033)	-.0154 (.0032)	-.0151 (.0029)	.0730 (.0060)
WORK COMP	.2107 (.0042)	.0701 (.0033)	.0669 (.0031)	.0639 (.0030)	.0842 (.0033)
OWN BIZ	.1210 (.0036)	.0579 (.0043)	.0586 (.0050)	.0407 (.0040)	.0844 (.0045)
N	101,871	61,399	67,599	73,433	53,468
R ²	.270	.060	.082	.076	.225

Notes: Standard errors are in parentheses. The income and education coefficients are multiplied by 10 for presentation. Each regression is a linear probability model. Column 1 regresses ownership of the individual on the fraction of the city owning in the same year. Columns 2 through 4 regress the decision to buy a computer in the later year on the share of the city owning a computer in the previous year. Column 5 looks at the self-reported likelihood of buying a computer in the next year.

TABLE 3: CORRECTING FOR UNOBSERVABLES

	(1) OLS Baseline	(2) OLS More Controls	(3) OLS City % for Electronics	(4) IV City Means for Demog.	(5) IV City Means for Kids Only	(6) OLS Purchase Experience	(7) OLS Intensity Of Use
CITY %	.1192 (.0218)	.1107 (.0220)	.1193 (.0348)	.1145 (.0188)	.1676 (.0531)		
Demog.:	10 vars	10 vars 3 interactions	10 vars 3 interactions	10 vars	10 vars	10 vars	10 vars
Others:		7 goods 3 attitude vars TV Hrs, Wealth	Column (2) vars plus 7 CITY%				
2+ Comps CITY%						.1233 (.0243)	
1 Comp CITY%						.0607 (.0558)	
Heavy Use CITY%							.1368 (.0252)
Light Use CITY%							-.0068 (.0747)
N	35,144	35,144	35,144	61,399	61,399	61,399	61,399
R ²	.058	.073	.074			.060	.060
First stage R ²				.884	.156		

Notes: Standard errors are in parentheses. Variables are defined in the text. The instruments used in the IV estimates are listed at the top of the column. Coefficients are not listed for some of the variables as indicated in each column.

TABLE 4: IDENTIFYING THE TYPE OF NETWORK

Vars.	(1) No Kids	(2) Computer Use	(3) City- States	(4) City Dummies	(5) 1998 Friends	(6) 1998 City-ST Dums
CITY %	.0941 (.0185)	-.1280 (2.111)	.0110 (.0428)			
CITY-ST %			.0943 (.0390)	.0878 (.0333)	.0320 (.0190)	
Friends w/comp-- ALL					.2096 (.0105)	.2085 (.0106)
Friends w/comp-- MOST					.1992 (.0049)	.1998 (.0049)
Friends w/comp-- SOME					.1149 (.0047)	.1162 (.0048)
Friends w/comp-- VERY FEW					.0432 (.0047)	.0450 (.0047)
Demographics:	9 vars	10 vars	10 vars	10 vars	10 vars	10 Vars
CITY Dums:	No	No	No	Yes	No	No
CITY-ST Dums:	No	No	No	No	No	Yes
N	47,929	4,841	61,399	61,399	52,868	52,868
R ²	.051	.034	.060	.064	.261	.268

Notes: Standard errors are in parentheses. Variables are defined in the text. Column 1 restricts the sample to individuals with no children. Column 2 examines computer use measured in days per month. The dependent variable in columns 5 and 6 is the self reported likelihood of buying a computer in 1998.

TABLE 5: NETWORKS BY TYPE OF USE

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Word Processing	Spreadsheet	Games	Graphics	Family Budget	Work at home	Internet	e-mail
CITY-ST% (Frequently Use Feature)	.1057 (.0177)	.0925 (.0397)	.1104 (.0311)	.0991 (.0553)	.1196 (.0440)	.0641 (.0336)	.1343 (.0202)	.1435 (.0217)
CITY-ST% (Do Not Freq. Use Feature)	.0960 (.0670)	.1148 (.0359)	.0968 (.0366)	.1067 (.0275)	.0964 (.0289)	.1526 (.0380)	.0460 (.0393)	.0301 (.0399)
Other Variables	10 Vars	10 Vars	10 Vars	10 Vars	10 Vars	10 Vars	10 Vars	10 Vars
N	61,399	61,399	61,399	61,399	61,399	61,399	61,399	61,399
p-value on equality	.89	.74	.81	.92	.72	.17	.07	.03
R ²	.061	.064	.061	.061	.061	.061	.061	.061

Notes: Standard errors are in parentheses. Variables are defined in the text. Coefficients are not listed for some of the variables as indicated. The first row is the share of the city-state which owns a computer and frequently uses their computer for the purpose listed at the top of the column. The second row is the share of computer users who do not frequently use their computer for that purpose.

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