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MEASURING BROADBAND'S ECONOMIC IMPACT

William H. Lehr[1], Carlos A. Osorio[2], Sharon E. Gillett[3], Marvin A. Sirbu[4]

[1]Massachusetts Institute of Technology

[2]Massachusetts Institute of Technology

[3]Massachusetts Institute of Technology

[4]Carnegie Mellon University

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William H. Lehr
Carlos A. Osorio
Sharon E. Gillett
Massachusetts Institute of Technology

Marvin A. Sirbu
Carnegie Mellon University

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ABSTRACT

Does broadband matter to the economy? Numerous studies have focused on whether there is a digital divide, on regulatory impacts and investment incentives, and on the factors influencing where broadband is available. However, given how recently broadband has been adopted, little empirical research has investigated its economic impact. This paper presents estimates of the effect of broadband on a number of indicators of economic activity, including employment, wages, and industry mix, using a cross-sectional panel data set of communities (by zip code) across the United States. We match data from the FCC (Form 477) on broadband availability with demographic and other economic data from the US Population Censuses and Establishment Surveys. We find support for the conclusion that broadband positively affects economic activity in ways that are consistent with the qualitative stories told by broadband advocates. Even after controlling for community-level factors known to influence broadband availability and economic activity, we find that between 1998 and 2002, communities in which mass-market broadband was available by December 1999 experienced more rapid growth in (1) employment, (2) the number of businesses overall, and (3) businesses in IT-intensive sectors. In addition, the effect of broadband availability by 1999 can be observed in higher market rates for rental housing in 2000. We compare state-level with zip-code level analyses to highlight data aggregation problems, and discuss a number of analytic and data issues that bear on further measurements of broadband's economic impact. This analysis is perforce preliminary because additional data and experience are needed to more accurately address this important question; however, the early results presented here suggest that the assumed (and oft-touted) economic impacts of broadband are both real and measurable.

I. Introduction

Does broadband matter to the economy? Compared to the volume of empirical research that has focused on the value of specific policies for promoting broadband,[1] relatively little has focused on the value of broadband itself. Widespread availability and use of cost-effective, always-on, faster-than-dialup access to the Internet is a relatively recent phenomenon in the U.S., with the first commercial deployments appearing only in the second half of the 1990's, and about a third of U.S. households subscribing to broadband by 2004.[2] National economic data is only now becoming available to examine whether broadband actually does act on the economy in ways that to date have been generally assumed – accelerating growth, expanding productivity, and enhancing the quality of life.

Empirical estimates of broadband's economic impact are an essential input to the development of broadband-related public policies. Most obviously, the value of broadband can inform estimates of the potential benefits obtainable from federal, state, and local government investments that directly or indirectly subsidize broadband deployment or use. Examples of such investments – in place or proposed – include targeting of Universal Service Funds toward broadband; the broadband loan program of the U.S. Department of Agriculture; "digital divide" grants and technology-led economic development programs; and municipally-led broadband networks.

Furthermore, quantifying the value of broadband also focuses public attention on telecommunications policy more generally. Stakeholders that are directly involved certainly understand the financial impact of public policies such as the Supreme Court's *Brand X* decision, the FCC's order making DSL an information service, state prohibitions on municipal networking, and debates over video franchising for next generation networks. To the general public, however, such issues can easily seem inscrutable and arcane. By defining the stakes involved for the economy as a whole, an estimate of broadband's impact also helps inform analyses of the public interest.

The challenges inherent in developing reliable estimates of broadband's value are reflected in the progression of empirical work to date. The first generation of studies appeared in 2001-2, before broadband had been significantly adopted in the U.S., and was correspondingly hypothetical and forward-looking. As a report from the U.S. Department of Commerce (DoC) put it at the time:

> Because broadband technologies are so new (and continue to evolve), there are no definitive studies of their actual impact on regional economic growth and tech-led economic development. Of course that never prevents economists and technologists from speculating or estimating. (U.S. Department of Commerce, 2002, p. 9)

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[1] (Wallsten, 2005) provides a recent entry into this stream of research, finding little impact from most state-level policies, but bigger impact from right-of-way reforms. Quite a bit of literature debates the merits of unbundling policies, such as (Crandall and Alleman, 2002).

[2] These estimates are based on 2004 U.S. penetration estimates from the Pew Internet Project, Nielsen/Net Ratings, eMarketer, the OECD, ITU, and FCC, and the authors' calculations based on the varying figures reported by these organizations.

A well-known report from this period was prepared for Verizon by Criterion Economics (Crandall & Jackson, 2002), and developed several forward-looking models to estimate broadband's economic impact. The study estimated that broadband, acting through changes to consumer's shopping, commuting, home entertainment and health care habits, would contribute an extra $500 billion in GDP by 2006. Other forward-looking studies from the time include the New Millenium Research Council's estimate of 1.2m jobs to be created from the construction and use of a nationwide broadband network (Pociask, 2002), and a Brookings Institution report estimating that "failure to improve broadband performance could *reduce* U.S. productivity growth by 1% per year or more." (Ferguson, 2002).

By 2003, studies started becoming available based on the real broadband experiences of individual communities. Early studies in this vein include a case study prepared for the U.K.'s Department of Trade and Industry of a municipal fiber network built in 2000-1 in South Dundas, Ontario (Strategic Networks Group, 2003), and a study comparing Cedar Falls, Iowa, which launched a municipal broadband network in 1997, against its otherwise similar neighboring community of Waterloo (Kelley, 2003). Each of these studies found positive economic impacts from the local government investment. More recently, (Ford & Koutsky, 2005) compared per capita retail sales growth in Lake County, Florida, which invested in a municipal broadband network that became operational in 2001, against ten Florida counties selected as controls based on their similar retail sales levels prior to Lake County's broadband investment. They found that sales per capita grew almost twice as fast in Lake County compared to the control group.

Given the passage of time, increased availability and adoption of broadband in the U.S., and newer data from the biennial business Census, it is now possible to begin looking for broadband's economic impacts more generally (for example, not restricted to public broadband investments) and at a larger geographic scale. The present study extends previous work by constructing a cross-panel data set at the zip-code level for the entire United States. We compare various economic outcome measurements in different communities (by zip code) based on when broadband became available in the community, controlling for other factors known to affect broadband availability and levels of local economic activity. The panel combines Census data on business activity from the 1990s through 2002, and community demographics through 2000, with a broadband availability indicator developed from the FCC's publicly available Form 477 data.[3]

Measuring the economic impact of broadband is difficult and confronts the same types of measurement challenges that led to the so-called Productivity Paradox of Information Technology (IT), best articulated by Robert Solow's famous quip that "we see computers everywhere but in the productivity statistics." Broadband does not act on the economy by itself, but in conjunction with other IT (primarily consisting of computers and software during the period studied here) and associated organizational changes (Brynjolfsson, Hitt, and Yang 2003; Lichtenberg and Lehr 1998). As with computers, the effects of broadband may be strongest in

[3] This data reports broadband available from all types of providers, and does not distinguish between public- vs. private-sector provision, or among broadband technologies. The data also reports the number of providers in each zip code, in a limited way: the number of broadband operators who provide service in a zip code is listed if the number is four or more, and replaced with an asterisk ("*") if the number is between one and three. Because of this limitation, this study does not use competitive information in its broadband indicator.

non-farm, non-manufacturing industries, where productivity improvements are typically less well captured by economic data.

A particular challenge for this study is that data to distinguish localities by their actual use of broadband – which would seem to be a pre-requisite for most types of economic impact – is not generally available. For example, the FCC's Form 477 data only distinguishes among communities by their broadband availability, and provides no metrics of broadband adoption or use below the state level. The prospective studies referenced above suggest that broadband should make individuals and businesses more productive through behaviors such as online procurement and telecommuting, but national data is generally not available to observe these behaviors at the local level.[4] Section II of this paper discusses these measurement and data availability challenges in more detail, while Section III discusses the empirical specifications adopted to deal with them.

Our results are summarized in Section IV. Specifically, we estimate that between 1998 and 2002, communities in which mass-market broadband was available by December 1999 experienced more rapid growth in (1) employment, (2) the number of businesses overall, and (3) businesses in IT-intensive sectors. In addition, the effect of broadband availability by 1999 can be observed in higher market rates for rental housing in 2000. We compare state-level with zip-code level analyses to highlight data aggregation problems and discuss a number of analytic and data issues that complicate measurement of the economic impact of broadband. This analysis is perforce preliminary because additional data and experience are needed to more accurately address this important question; however, the early results presented here suggest that the assumed (and oft-touted) economic impacts of broadband are both real and measurable.

II. Study Design

The essence of this study's design is to differentiate geographic areas by their availability and/or use of broadband, then look at economic indicators for these areas over a long enough period to see if consistent deviations from the secular trend are observable, controlling for other factors known to distinguish among the areas. We conducted the analysis using both zip codes and states as the relevant geographic areas, based on the FCC's reporting of broadband availability by zip code, and lines in service (a better proxy than availability for actual broadband use) by state. In this section we first discuss the theoretical basis for hypotheses regarding the impact of broadband on specific indicators of economic activity. We then turn our attention to the question of available data for the construction of broadband metrics, economic indicators, and control variables at the zip code and state levels.

A. Theoretical Basis for Broadband's Economic Impacts

Broadband does not act on the economy in isolation, but as a complement to other information technologies. In the pre-2003 period studied here, broadband typically consisted of always-on, faster-than-dialup access to the Internet, with the user's experience typically

[4] As (Ford & Koutsky, 2005, p. 4) put it: "One difficulty … is the general lack of sufficient economic and demographic data to analyze changes in a community's economic fortunes. Broadband service is a relatively recent phenomenon, and local economic data is often not collected on a regular basis for a detailed econometric analysis."

mediated by software running on a personal computer. User studies such as Rappoport, Kridel and Taylor (2002) have demonstrated how the convenience and responsiveness of broadband led people to use it more intensively than its narrowband (dialup) predecessor. Broadband is a critical enabler for the use of computer-based applications that need to communicate.

Adoption of broadband-enabled IT applications can affect the economy by changing the behaviors and productivity of both firms and individuals. Studies such as Forman, Goldfarb and Greenstein (2005), Bresnahan, Brynjolfsson, and Hitt (2002), and Brynjolfsson and Hitt (1996) have focused on changes to firm behavior, finding that these generally lie on a spectrum, with the highest payoffs in enhanced productivity appearing in the firms that commit most intensively to integration of IT into new business processes. For example, Forman et al distinguish between “IT using” and “IT enhancing” firms. The former simply adopt existing Internet applications to make current business processes more productive: for example, they use email and web browsing to raise the quality and lower the costs of gathering market intelligence and communicating with suppliers and customers. The latter develop and integrate more complex “e-business” applications, such as CRM and ERP, that can enable whole new business processes and models, such as automated online supply chain management and online sales into geographically distant markets. To the extent that the availability and use of broadband fosters either type of IT adoption and usage by firms, we would expect productivity improvements and other associated economic impacts to follow.

Other studies have focused on the effects of IT on individual workers. For example, Autor, Levy and Murnane (2003) found that IT tends to complement workers that perform nonroutine problem-solving and complex communication tasks, but substitutes for workers who perform cognitive and manual tasks that can be accomplished by following explicit rules. While both effects could be expected to increase productivity, the overall effect on employment is ambiguous and would depend on the mix of different types of jobs in the economy.

While much of the IT productivity literature has focused on workplace usage, much of the focus of broadband policy has been on residential deployments. Broadband at home may of course be used for leisure pursuits, but it can also be expected to affect the economy both directly and indirectly. For many knowledge workers, a residential broadband connection is a prerequisite for working at home (enabling productive use of non-traditional working hours, flexible work arrangements, or remote employment), or for establishment of a home-based business, such as an individual consultancy (contributing to new business formation). Less directly, expanded broadband availability at home may raise the quality of the labor force, for example through improved access to educational opportunities via distance education programs, thus making a locale more attractive to potential employers. Similarly, home-based access may improve quality of life, for example by enabling more participation in community and civic activities, making a locale more attractive to potential residents. Somewhat more directly, home-based access may enable more effective (i.e. online) job hunting, reducing unemployment by making labor markets more efficient. It may also make workers more productive by reducing the overall time needed for them to fulfill non-work obligations, e.g. via online bill payment, shopping, telemedicine, and so forth, as Crandall and Jackson (2001) envisioned. As with firm usage of IT, however, the overall effect of home-based broadband usage on local economic indicators is not obvious *a priori*. While online banking and shopping may make local workers more productive, it is also likely to put competitive pressure on local banks and retail stores, leading to ambiguous effects on the number of local jobs.

Most of these hypothesized impacts on firm and individual productivity are not measurable directly. Broadband availability varies by community, but statistics are not tallied at the community level to measure local output (GDP) or use of capabilities like e-commerce and telemedicine. To create hypotheses testable with available data, we focus instead on how broadband is likely to change other indicators that describe local economies.[5] Potential indicators to test for positive impacts from broadband include:

- Employment opportunities (e.g. measured by employment rate, share of high-skilled/high-wage jobs in community, wage rates, and self-employment);
- Wealth (e.g. measured by personal income, housing values, or rents);
- Skills/quality of local labor force (e.g. measured by educational attainment, drop-out rates, or share of work-force in more skilled jobs); and
- Community participation and quality of life (e.g. measured by voting participation, mortality rates, or local prices).

Our ability to test the complete list of indicators is limited by the frequency of data collection for different types of Census data, and geographic unit limitations for other types of data (e.g., voting participation by zip code). For most indicators, it is reasonable to expect that broadband's impacts will be felt only after some time lag. Broadband has to be not only available (step 1), but adopted (step 2) and then used (step 3) to provoke the kinds of individual and firm behavior changes discussed above as enhancing productivity and economic activity. While the expected length of that process may vary depending on the particular indicator, for most indicators it is not reasonable to expect to see impacts in the most recent decennial (2000) Census data, given that the FCC's earliest measurement of community broadband availability was only taken at the end of 1999. This reality particularly limited our ability to test broadband's impacts at the zip-code level on workforce-related indicators such as self-employment, the share of white-collar workers, educational attainment levels, and per capita expenditures on public assistance.[6] The one exception is our use of 2000 rent as a wealth indicator, justified because only broadband availability (not its actual use) should be necessary to influence the value of rental housing, and the effect should be immediate.

Despite these limitations on workforce and societal impacts, the use of biennial business Census data (for which 2002 data is the most recent available at the time of this writing) does allow testing of broadband's impacts on five key indicators of business activity: (1) total employment, (2) wages, and (3) the number of business establishments (used as a rough proxy for firms), as well as indicators of industry mix along (4) sector and (5) size dimensions. In particular, we examine broadband's effect on the share of business establishments in IT-intensive

[5] While all of these changes are expected to have a positive impact on total surplus, the direction of their effect on specific indicators (e.g. employment or wages) may be ambiguous, as discussed above.

[6] The Census also conducts a "Computer and Internet Use" survey every 3 years. However, this survey is based on a national sample (thereby undersampling in rural areas), and therefore does not have enough observations to use with the full set of national zip codes used in this study.

industry sectors[7] (interesting in its own right, but also as a proxy for the skill level of jobs in the community), and the establishment size mix (i.e. what share are small vs. large). The next section discusses these indicators in further detail.

B. Data Availability

Table 1 summarizes the sources used to construct the zip code and state-level data sets[8]. Most of the variables are straightforward, other than the broadband metric which we discuss below. Definitions and summary statistics are presented in Table 2 for all the variables used in the zip code analysis, and Table 3 for the state level analysis. In the zip code analysis, statistics are reported for both the full sample of zip codes, and the sub-sample that results from matching across all the variables. Because results did not differ substantially for the full and sub-samples, we simplify the analysis by using the consistent sub-sample throughout.

Ideally, we would be able to differentiate among communities by their actual use of broadband. However, the FCC's Form 477 data does not provide any indication of broadband adoption or use at the zip code level. At the state level, the FCC reports the number of broadband lines in service, segmented by lines serving residences and small businesses vs. those serving larger businesses, government, or other institutions. We convert the mass-market (residential and small business) segment[9] to a statewide penetration rate and use this broadband indicator to test for effects at the state level, because in theory penetration should provide a better indicator for broadband's impact than simple availability. In practice, however, the state level is too coarse an aggregate, as we discuss in more detail below.

At the zip code level, the broadband metric "BB99" is based on availability and is defined as 1 if the zip code had broadband by the end of 1999, and 0 otherwise. We adopt the FCC's "high-speed" classification to define broadband: any line with a speed higher than 200 Kilobits per second (Kbps) in at least one direction.[10] Although we do not expect availability to serve as a perfect proxy for broadband use, this metric is the best available.

BB99 represents a simplification along several dimensions of the Form 477 data provided by the FCC. First, we do not use information about the number of broadband providers, especially since the actual number is not publicly available when the total is between 1 and 3, as noted above.[11] We simply record the zip code as having broadband if any providers serve it. Second, we do not differentiate among communities that got broadband in periods post-1999 because we expect, as discussed above, that economic impacts will be observed with a time lag

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[7] Establishments in IT intensive sectors were defined by taking the work of Forman, Goldfarb and Greenstein (2002, 2004, 2005) and coding the 3-digit level NAICS codes that reported an "enhancement adoption rate" of at least 18%. This accounts for approximately 21% of the labor in the U.S.

[8] All tables are available in section VIII.

[9] This segment represents about 75% of the lines in service in 2000, the first year that lines in service were reported.

[10] Presumably, most availability represents consumer-grade broadband like cable modem and DSL. Leased lines, fiber to the premises, and satellite and fixed wireless are also included in the Form 477 reporting.

[11] Because of this data limitation, it is not possible to test whether competitive intensity affects broadband's economic impacts.

after broadband becomes available. Since most of our economic indicators are measured in 2002, we wanted to ensure as long and consistent a lag as possible across communities. Complete consistency is unfortunately not possible, because we know from the large proportion of the zip codes that first appear in the 1999 data collection, and the relatively high penetrations in 2000 in a few states (e.g., California, Connecticut and Massachusetts) that in fact many of these areas had broadband available in earlier periods as well (Tables 4 & 5). It is unfortunate that this early variability is not available in the data, since again it limits the analyses that can be conducted.

The Form 477 data on broadband availability by zip code has some well-known limitations which are discussed further in Flamm (2004), Grubesic (2004), and Prieger (2003). It is worth noting here that the data is especially problematic in rural areas, which form the bulk of the zip codes where BB99=0.[12] The problems arise because of a minimum reporting threshold that tends to understate rural availability,[13] and the assignment of broadband availability to an entire zip code if a broadband bill is sent anywhere in the zip code, which tends to overstate availability in the geographically larger zip codes found in rural areas (Table 6).[14]

III. Empirical Specifications

As discussed above, we lack any simple summary statistic with which to measure total economic activity (e.g., "total output" or "GDP") by community. Therefore, we examine a collection of economic variables for which we could reasonably expect to see a measurable impact of broadband (employment, wages, rent, and industry structure or mix). For each category of variables, we used a slightly different set of independent and dependent variables, and tested three approaches:

(1) Analyze the impact of broadband at the state level. Although in general we expect states to be too high a level of geographic aggregation to show interesting results (broadband variation within states is typically higher than among them), we conducted this analysis because the FCC data provides information on the total number of broadband consumers (i.e. penetration) only at the state level.[15] Thus testing at the state level was necessary to conduct any analysis

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[12] Flamm (2004) also discusses the challenges of inferring which zip codes had no broadband, since these are not reported directly by the FCC. The complete list of zip codes was created by matching: (1) all the zips reported by the FCC's releases of the Form 477 between December of 2000 and 2004 (29,281 zip codes), (2) the list of zip codes from the US Census Bureau's Zip Code Tabulation Areas (ZCTAs) for 2000 (33,178 zip codes), and (3) the list of zip codes from the US Census Zip Code Business Patterns for 1994, 1998, 2000, and 2002 (40,581 zip codes). This created a sample of 32,481 zip codes (22,390 of which present data for all variables, see Table 2), 17,889 of which were shown as getting broadband by 12/99. All others were coded as not having broadband (BB99=0) by 12/99.

[13] The FCC data on broadband availability and penetration are based only on reports by providers with more than 250 lines in a state. The data may thus underestimate the availability of broadband in rural communities covered by smaller independent LECs or cable franchisees, whose total subscribership falls below the reporting threshold.

[14] For example, data provided by the Vermont Public Service Commission to the FCC in docket WC 04-141 shows many zip codes in which broadband is available in only a small portion of the zip code. See http://gullfoss2.fcc.gov/prod/ecfs/retrieve.cgi?native_or_pdf=pdf&id_document=6516215235, p. 22.

[15] As discussed above, at the zip code level, the data indicates only availability: *i.e.* whether one or more broadband providers are serving customers within the zip code.

using penetration as a broadband metric. State-level analysis also provides an important connection to previous research on IT's economic impacts.[16]

(2) Explore the impact of broadband using community (zip-code) level data with exogenous controls; and

(3) Extend the community-level analysis with a matched sample analysis as the means to control for non-broadband, unobserved effects.

We have a time series panel dataset and are thus looking for variations in the secular trend of an economic indicator as a function of broadband availability or penetration.

Our zip code regressions generally take the form:

$$Y(t)=a+\alpha Y(0)+ X\beta+\gamma BB+e \qquad \text{(Eq1)}$$

where,

- Y(.) is the economic variable of interest, for example, the share of establishments in IT intensive industries.
- X are control regressors for differences in community characteristics of the different zip codes
- BB=1 if community had Broadband in 1999 and 0 otherwise; and
- e are error terms.

Typically, Y(0) corresponds to 1998, prior to the known availability of broadband, and Y(t) is measured in 2002, the latest year for which we have data from the Business Patterns survey. Since we are controlling for Y(0), we interpret γ as the impact of BB on the level of change in dependent variable Y(.) over the interval [0,t].

Zip codes vary widely in size, population, and other economic characteristics. Under these circumstances treating the impact of broadband as fixed additive amount may not be realistic. Treating the impact as a multiplier may make more sense, thus reducing the problem of heteroskedasticity. Accordingly, we may use $y(.) \cong \ln Y(.)$ in place of Y(.). This is consistent with the following structural model:

$$Y(t)=AY(0)^{\alpha}e^{rt} \qquad \text{(Eq2)}$$

where

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[16] Daveri and Mascotto (2002) study the effect of computer diffusion at home and work on the growth rate of gross state product (GSP) per employed population. They conclude that, while there is an affect at aggregate level, most of the impact comes from states where the contribution to GSP of IT-producing and non-IT manufacturing sectors is above the US average. When these states are excluded from the sample, the authors find no evidence of an impact of IT on productivity acceleration.

$$r = r^* + \gamma BB + X\beta + e \qquad \text{(Eq3)}$$

and e are distributed log-normally and t is defined by construction so that t=1 corresponds to 4 years after t=0.

Strictly speaking, if we view r as a growth rate, then we would expect A=1 and α=1. We can force α=1 by transforming our dependent variable to

$$\ln(Y(t)/Y(0)) = g(t) = a + X\beta + \gamma BB + e \qquad \text{(Eq4)}$$

where $a = \ln A + r^* = r^*$ if A=1.

When using equation 4, γ is interpreted as an increment to the growth rate of the dependent variable due to availability of broadband.

As noted in Table 2 above, we consider the impact of broadband on 6 different economic variables. Where the dependent variable is measured as a share (share of small establishments, share of establishments in IT-intensive industries) we use the specification in equation 1. For salaries, employment and number of establishments, we use g(t) = ln(Y(t)/Y(0)) as the dependent variable as in equation 4. For median rents, we use a specification based on equation 2. We do this because the unconstrained value of α that we estimate is far from equal to 1 and so it did not seem appropriate to force it to be =1 as in equation 4.

At the state level, we have data on the actual number of broadband lines in use. We normalize this data to a penetration rate by dividing the number of residential and small business lines by the number of households and small businesses in the state. Across the states, as shown in Table 5, penetration varies from near zero to as high as 22% by 2002. Because broadband will be adopted within a state first by those who get the greatest benefit, and we expect later adopters within a state will realize a lesser benefit, we do not expect our dependent variables to be linearly related to statewide broadband penetration. Consequently, at the state level, we modify our equations to incorporate both linear and quadratic terms for the impact of broadband penetration.

We know from the studies of Flamm (2004), Grubesic (2004), Prieger (2003), Gabel and Huang (2003), Gabel and Kwan (2000), and Gillett and Lehr (1999) that the decision by providers to deploy broadband is not unrelated to economic characteristics of the community, such as income and population density. As a result, if we look solely for an association between broadband availability and our economic variables, it may be hard to distinguish the direction of causality. In each equation, we introduce control variables in an attempt to separate the effects of broadband from the a priori economic characteristics of the community (zip code).

We are limited in the kinds of controls we can use by the availability of data at a zip code level over the relevant time periods. However, we have, for each equation we have estimated, identified a number of controls which improve our confidence in our estimates. We use the same controls in the regressions at the zip code level and the state level with one difference: at the zip code level we also include state dummies to account for fixed effects by state.

When analyzing data at the zip code level there is an alternative approach to the issue of controls and direction of causality when looking for the impact of broadband. Within our sample, a majority of zip codes had broadband available in 1999. These zip codes are on average in higher density, more urban areas, with greater proportions of college graduates, and higher growth rates in income and labor force. If we see differences in economic growth in communities with and without broadband, how do we know it is because of the lack of broadband, and not some other characteristic of the communities? We could try and take the (minority) set of zip codes that did not have broadband in 1999 and match them, using key economic characteristics, to a subset of the communities which did have broadband in 1999 in order to identify, insofar as possible, a "matched" sample. Then, if our dependent variable varies systematically between the two groups, we can infer that it must be due to the presence or absence of broadband.

Stata's NNMATCH function provides a method for selecting a control group to compare with a treatment group using a series of independent variables. It tries to identify a control group which has the same mean and variance across the independent variables as the treatment group—*i.e.* is statistically similar (Abadie et al. 2004). This is done by using nearest neighbor matching across these variables. In the case of our paper, we have used 1-to1 matching, which means the program has matched each control observation to the closest observation in the treatment group.[17] The function then estimates the average treatment effect on a dependent variable of being in one or the other group. In our analysis, we have assumed heteroskedastic standard errors, and used the robust option of nnmatch.

In some cases, it is not possible to find a control group which matches on all the characteristics of the treatment group. For example, if all the zip codes without broadband were rural, and only a small fraction of the zip codes with broadband were rural, it might not be possible to find a comparable number of rural zip codes among the "haves" group to match as a control with the non-broadband group. Thus, on a statistical measure such as degree of urbanness, the treatment group and the control group would not be truly similar along that dimension. Notwithstanding these difficulties, for each of our dependent variables, in addition to the regressions, we have used nnmatch to estimate whether broadband has a significant impact at the zip code level.

IV. Results

As we discussed earlier, state-level regressions are provided to test penetration as a broadband indicator and provide a point of reference to the earlier research; however, we do not believe these regressions are very meaningful because they represent too high a level of aggregation. There are likely too many countervailing forces affecting the various economic variables and within-state differences in broadband availability are likely much greater than cross-state differences. Consequently, we believe that the results of our zip code sample regressions are much more meaningful. In each case, we start with the simplest regression with a dummy variable which is 1 if broadband was available in the community by December 1999, and zero otherwise. We then add regressors to control for non-broadband exogenous influences that

[17] In most datasets, we find the treatment group to be smaller than the universe without the treatment, so the matching is done with respect to the smaller group. In this case, however, the set of zip codes without broadband was smaller than the group that got it by 12/99, which made the model results more complicated to interpret. Tests with NNMATCH showed no difference in results if the treatment was assigned to one group or the other. For this reason, we defined our treatment group as the one getting broadband by December of 1999 (i.e., BB99=1 forms the treatment group).

could be expected to impact the growth of the economic variable of interest. Although we ran multiple versions of these more complex regressions, we report only what we consider the best versions of these.[18] All of the zip code regressions were run with robust errors to control for heteroskedasticity in our data. Finally, we ran the matched sample regressions as a final method of controlling for exogenous effects. Because broadband was first deployed (as would be expected) in more urban, denser, and richer communities where it is reasonable to believe broadband service might be more profitable to providers, the demographics of broadband "haves" (by December 1999) and "have-nots" (after December 1999 or not at all) are systematically different. The "have-nots" represent a much smaller sample of communities and are typically more rural. Thus, the matched sample results attempt to compare a sample of otherwise similar "haves" to "have not" communities (where otherwise similar is determined relative to the exogenous regressors included in the standard regressions). As one can see from examining the results (Tables 9C through 14C*)* and, as will be discussed shortly, in a number of cases, the "have nots" sample was simply too different from the "haves" to be able to generate an acceptable match. However, when it is possible to construct a well-matched sample and these results are significant, they provide additional support for our zip-code results.

Our results are generally consistent with the view that broadband enhances economic activity, helping to promote job creation both in terms of the total number of jobs and the number of establishments in communities with broadband (see Table 7). The positive impact on establishment growth was higher for larger establishments and for IT intensive sectors of the economy. We did not observe a significant impact of broadband on the average level of wages, but we do observe that residential property values (proxied by the average level of rent paid for housing) are higher in broadband-enabled communities. These results are discussed further in the following sub-sections.

Another way to see the results is to compare the sample means for communities with and without broadband ("haves" vs. "have nots") as of December 1999 (Table 8). This comparison shows that the mean growth in rent, salaries, employment, number of establishments, and share of establishments in IT-intensive sectors were all higher in the communities with broadband, while only the share of small establishments declined. The regression results discussed below test this intuition further by adding additional controls to account for non-broadband influences that might account for these differences.

A. Employment

Our first group of results (Table 9) examines the impact of broadband availability on total employment in each community. As explained earlier, theory does not provide strong guidance *a priori* as to the expected impact of broadband on total employment. On the one hand, broadband might stimulate overall economic activity resulting in job growth; while on the other hand, broadband might facilitate capital-labor substitution, resulting in slower job growth. Furthermore, we might anticipate that broadband would have asymmetric effects by industry sector and for occupation mix. These additional share effects might result in ambiguous changes in the direction of total employment growth.

[18] For example, we do not include variables that were consistently insignificant (e.g., population density).

In the state-level regressions (here and in subsequent sub-sections except where noted), we use state-level data on broadband penetration as a measure of broadband use. This is appropriate in those cases where it seems reasonable to believe that it is broadband use (rather than simply its availability) that produces the economic impact. As discussed in Section III above, because we expect a saturation effect, when we use penetration in the state-level regressions, we also include the square of penetration as an additional regressor.

In the state-level regressions for employment (Table 9A), it initially looks as if broadband penetration might have a positive impact on employment growth which diminishes as penetration gets higher (thus, demonstrating the hypothesized saturation effect), but the relevant coefficients are not significant (regression 9A1). However, when additional regressors are added to control for such exogenous effects as the growth in employment from 1994 to 1998 (gEmp9498) and a dummy variable to account for urbanization (dUrban), the signs on the broadband variables are reversed and remain insignificant (regression 9A4). This is not surprising and points to the problems with using state-level data already discussed. Simply, it offers too high a level of aggregation – combining too many separate and potentially re-enforcing or countervailing forces (as suggested by the theory) – to permit us to observe a measurable impact.

However, when we turn to the zip code regressions (Table 9B) and matched sample regressions (Table 9C), we find a substantial positive impact for broadband availability on the growth in total employment. Progressing from simple (9B1) to more complex regressions (9B4), we observe that the magnitude of the estimated broadband effect declines. Nevertheless, it remains significant and positive. Regression 9B4 suggests that the availability of broadband added over 1 percent to the employment growth rate in the typical community (coefficient on BB99 is 0.01045). We also observe that the controls (gEmp9498 and dUrban) are significant and have positive signs as expected.

This result is also supported by the matched sample results (Table 9C). The match appears reasonable and the impact of broadband on employment appears slightly higher in the results (0.014426), suggesting that broadband increased employment by almost 1.5 percent.

B. Wages

Perhaps the most likely place to expect to see an impact of broadband would be on wages. If one believes that broadband enhances productivity in a number of ways, it is reasonable to expect that some of the benefits of these effects would be captured by workers. Additionally, perhaps the most extensive empirical literature that exists has focused on the positive effects of IT for wages and employment mix effects. Finally, one might expect that these wage effects might be observed in the economic data more quickly than shifts in employment mix (by occupation or by industry sector) or the number of firms (reflecting entry and exit into the community).

Thus, we initially approached the analyses of community wage data (measured as total payroll associated with all businesses in the community) with the hope of finding significant measurable impacts. Unfortunately, although some of the simplest regressions looked promising (10A1), as soon as we included appropriate exogenous controls, the sign of the coefficient on broadband changed signs (10A4) and became insignificant.

The coefficients on the controls have the expected signs. The growth in salary 1994 to 1998 (grSalary9498), the share of the population with college degrees in 2000 (pcollege2K), the growth of labor from 1990 to 2000 (grLaboe90s), the share of establishments that are in IT intensive sectors in 1998 (pIT98), and the urbanization dummy (dUrban) all have positive and significant coefficients. After controlling for these effects, we do not observe any additional significant effect attributable to broadband.

C. Rent

The third group of regressions we run look at the impact of broadband on rental rates as reported in the 2000 Census. Our measure of broadband availability only tells us whether a community has broadband by December 1999 or not, it does not tell us how long the community has had broadband. However, it seems reasonable that if broadband has an effect on rental rates, that effect ought to be observed relatively quickly. Since broadband is desirable, we would expect to see the availability of broadband resulting in higher rental rates.

The results reported in Tables 11A and 11B support the conclusion that rental rates were significantly higher in 2000 in communities that had broadband. The most meaningful zip-code regression shows that rental rates were almost 7 percent higher (coefficient on BB99 is 0.06557) for broadband communities (11B4). The state-level results (11A4) are consistent with the zip-code results, but for reasons already discussed, we do not place much stock in these. By contrast, the matched sample results show a significant negative impact of broadband on rents; however, in attempting to create a matched sample of zip codes with broadband in 1999 which is similar along the independent variables to the set of zip codes without broadband, we are unable to construct such a matched sample with equivalent levels of family income growth. Our attempts to find such a match reveal that otherwise comparable zip codes with broadband all had significantly higher levels of two control variables: family income growth and labor force growth. Because the matched set is not fully comparable, no conclusions should be drawn from this approach as to whether broadband availability affects rents.

D. Industry Structure and Mix

The last group of results we will discuss relate to the impact of broadband on industry structure and the mix of businesses by industry sector and size. These results are reported in Tables 12 though 14. Table 12 looks at the growth in the total number of establishments; Table 13 looks at the growth in the share of firms that are in IT intensive sectors; and Table 14 looks at the share of firms that are small (10 or fewer employees). We discuss each of these in turn.

First, looking at Table 12, we see that broadband has a significant positive effect on the growth in the number of business establishments, increasing growth, by almost one-half of a percent (BB99 coefficient is 0.00483) from 1998 to 2002 in the best zip code regression (12B4). This positive effect is retained in the matched sample regressions, but is two and half times larger (Table 12C), although again, labor force growth is imperfectly matched. The state-level regressions also support this result (12A4). Moreover, in the zip-code regressions, the controls have the appropriate positive sign: growth in number of establishments from 1994 to 1998 (grEst4998), urbanization dummy (dUrban), and the growth in labor force from 1990 to 2000 (grLabor90s).

Second, turning to Table 13, we see that the share of firms in IT intensive sectors is higher in broadband communities. In the best of the zip code regressions, the share of establishments that are in IT intensive sectors increased by an additional one half percent between 1998 and 2002 in communities that had broadband by December 1999 (13B4). This is a large effect and it is hardly surprising since we would expect there to be a positive feedback process underlying this observation. That is, IT intensive sectors are the most likely to demand and use broadband services, and if availability is an issue, IT intensive firms are more likely to expand operations in locales with broadband. This effect complements the positive effect we observe on total employment. This result is supported by matched sample regression (13C), although the magnitude of the effect is reduced by almost half. The state-level regressions (13A4) show conflicting results that suggest that broadband's impact on the change in the share of firms in a state that are in IT intensive sectors is negative for low penetration and becomes positive only for relatively high penetration.[19] These results are not very interesting because almost all of the variability in the share of IT intensive firms is already explained by the share of IT intensive firms in 1998.

Third, and in some ways most interesting, our data provides some suggestive results as to the impact of broadband on firm organization and the size of business establishments. One theory is that the availability of enhanced communication services facilitates more geographically distributed types of firm organization ("death of distance"). If true, this could explain why the number of establishments per 2000 population is higher in broadband communities (pEst02 is 0.030 in "haves" v. 0.024 in "have nots," see Table 8). Additionally, broadband might lower entry barriers for new firms and may encourage the growth of self-employment. Since most of these establishments are likely to be quite small, we might expect to see faster growth in the number of small establishments in broadband enabled communities.

Table 14 shows results of estimating the impact of broadband on the change in the share of firms that are small (less than 10 employees) between 1998 and 2002. The state-level results are consistent with the hypothesis explained in the previous paragraph (14A4), but are not significant, and since these are state-level regressions we do not place much stock in them in any case. When we turn to the zip-code regressions, however, we observe a significant effect that is contrary to our expectation. We observe that the share of firms that are small declined in broadband enabled communities relative to non-broadband communities by over one percent (14B4). In the overall sample, the relative size mix of establishments declined only slightly (sample means for psm98 and psm02 were 0.792 and 0.790, respectively) however, the decline was greater in broadband communities. The matched sample results in Table 14C are significant and consistent with the zip-code results.

When we tried to explore this further by looking at regressions with the number of establishments per population or using different measures of the size composition, the regressions failed to indicate a measurable impact for broadband.

Because we cannot control for the growth in the relative number of firms by different size classes (we observe only the number of establishments by industry sector and size class), our data do not really allow us to infer the impact of broadband on firm organization. To address this

[19] That is, the coefficient on broadband penetration is -0.27606 and on broadband penetration squared is 2.61798 (13A4), so the overall impact of broadband is negative for any penetration level below 11 percent.

question, it may be more appropriate to use enterprise-level data like the data used by Greenstein, Forman et al. (2005).

V. Conclusions

The analysis presented in this paper represents a first attempt to measure broadband's impact by applying controlled econometric techniques to national-scale data. The results support the view that broadband access *does* enhance economic growth and performance, and that the assumed (and oft-touted) economic impacts of broadband are real and measurable. We find that between 1998 and 2002, communities in which mass-market broadband became available by December 1999 experienced more rapid growth in (1) employment, (2) the number of businesses overall, and (3) businesses in IT-intensive sectors. This is supported both by simple and matched sample regressions. While the available data (4) does not demonstrate statistically significant impacts on wages, the effects of broadband availability by 1999 can also be observed in (5) higher market rates for rental housing (a proxy for property values) in 2000 on simple regression results. Table 15 summarizes the estimated magnitude of impacts resulting from our analysis at the zip code level, after controlling for other community-level factors known to affect both broadband availability and economic outcomes, including income, education, and urban vs. rural character.

This analysis is perforce preliminary because additional data and experience are needed to more accurately address the fundamental question of how broadband affects the economy. The magnitude of impacts estimated by our models are larger than we expected. We interpret our results cautiously, in light of the methodological challenges inherent in disentangling causality in any study of the relationships between infrastructure availability and economic development. While we have employed two approaches (of many possible, as discussed further below) to address the causality issue, further research would be required to reject entirely the interpretation that the differences observed in our data reflect fundamental differences in the communities that got broadband first, rather than the effect of broadband on those communities. While we have therefore not proven that broadband causes economic impact, neither have we proven that it doesn't. At the least, the data demonstrate a clear association between broadband and positive economic outcomes.

Several possible approaches could be followed to advance beyond the first stage of analysis presented in this study. One approach would be to develop instrumental variables that might better control for the early presence of broadband. For example, if data were available to identify the local cable franchisee and/or ILEC in 1998, and this data predicted well the availability of broadband by 1999, such a variable could provide a control for early broadband availability that is not itself also related to economic growth. If an appropriate instrument could be developed, and the analysis continued to show better economic outcomes associated with broadband availability, then the inference that broadband actually causes those outcomes would be more strongly supported.

Interpretation of causality from the present study is also limited by the lack of penetration data to use as a metric of actual broadband use at the zip code level. Ascribing economic outcomes to broadband's influence is more believable if it is clear that broadband is actually being used in the community, not just available. This limitation could be addressed by developing an estimate of penetration at the zip code level, for example derived by observing differences in broadband penetration at the state level and fitting them to a model that predicts penetration as a function of demographic, regulatory, and other factors characterizing each state. Given a well-fitting model, broadband penetration could be reasonably estimated for each zip code, and incorporated into the economic impact models described in this paper. However, previous work suggests that realizing such a model could be problematic.[20]

Another possible avenue for refinement would be to use firm-level data to take a more micro-level view of broadband's impacts on the conduct of business within and between enterprises. This approach could be especially valuable for gaining a deeper understanding of broadband's impact on the size of firms and its relation to growth of different industry sectors (NAICS codes). Progression to this type of study in the case of broadband would parallel the development of studies on the so-called productivity paradox of IT. In that literature, studies within the firm added valuable insight into factors that interacted with each other to produce economic impact from computerization. Similar results could be expected in a study of broadband's impact since, like computers in general, we do not expect broadband to act in isolation to enhance productivity, but rather to act as part of a constellation of factors including related information technologies, innovative business practices, and more flexible organizational structures. The present study is relatively crude in attempting to relate broadband availability directly to economic performance. Futures studies could examine more intervening variables and concomitant investments to better characterize the firms and individuals who adopt broadband.

Ultimately, the case for broadband as a cause of positive economic outcomes will rely on the accumulated results of many studies conducted using a variety of approaches. The passage of time will make more and different forms of data available, enabling the application of various rigorous methodological approaches to the estimation of broadband's impact. New business census data will become available annually, and data in the next decennial census (2010) will make it more feasible to look at broadband's impact on workforce-related indicators such as self-employment and the share of white-collar workers. The spread of broadband (and related data collection) in more countries will make cross-national impact studies more feasible over time. In addition, recent enhancements in the broadband availability data collected by the FCC through Form 477 will eventually make it possible to test for different magnitudes of impact based on different levels of broadband (e.g. "big" broadband such as fiber-to-the-home vs. "little" broadband such as DSL) supplied in any given area.[21]

The present study has several clear implications for policy-makers. The most obvious and important implication is that broadband *does* matter to the economy. Policy makers who

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[20] See, for example, the models developed in Flamm and Chaudhuri (2005) to estimate broadband and narrowband demand elasticity based on national-scale survey data.

[21] In November 2004, the FCC revised the Form 477 reporting requirements and extended the data collection program for five more years. The new rules apply to data as of June 30, 2005, reported on Form 477 as of September 1, 2005. Under the new rules, *all facilities-based* providers will have to report their broadband deployments regardless of their reach or size, and provide more detailed information on speed and types of services offered. By removing the 250 lines threshold that previously exempted small-scale carriers from providing information, this change will address one of the two reporting issues that led to particularly unreliable data in rural areas, as discussed above. Further information about the revised reporting requirements is available at http://hraunfoss.fcc.gov/edocs_public/attachmatch/DOC-254115A1.pdf, http://www.fcc.gov/broadband/data.html, and http://www.fcc.gov/formpage.html#477.

have been spending their time or money promoting broadband should take comfort that their efforts and investments are not in vain. Many significant public policy reforms and programs are in place or under consideration at the federal, state, and local levels to ensure competitive availability of broadband to all U.S. citizens, stimulate ongoing investment in broadband infrastructure, and facilitate the education and training that small business and residential customers need to make effective use of broadband's capabilities. Such policies are indeed aimed at important goals. Broadband is clearly related to economic well-being and is thus a critical component of our national communications infrastructure.

Local policy-makers in particular may wish to understand whether the economic advantages conferred by broadband are temporary (i.e. growth in the early "have" communities came at the expense of the early "have nots") or longer-lasting (i.e. broadband stimulated growth of the overall economic pie). If the advantages are temporary, then the benefits to be gained from local public investments to speed broadband availability will be muted once neighboring communities catch up. On the other hand, if broadband affects the base growth rate of the local economy, then the benefits from getting it sooner will continue to compound into the future. Because the present study only looks at one time period, it cannot address this important question directly. The results of our study can be seen as consistent with either hypothesis. Once broadband is available to most of the country, however, differences in economic outcomes are likely to depend more on how broadband is used than on its basic availability. The implication for policy makers is that a portfolio of broadband-related policy interventions that is reasonably balanced (i.e., also pays attention to demand-side issues such as training[22]) is more likely to lead to positive economic outcomes than a single-minded focus on availability.

Finally, the present study highlights the fundamental role that government data plays in shaping our understanding of how communications technologies and policies relate to national economic performance. As discussed above, public data about broadband focuses primarily on the supply side (availability), especially at the local level. Economic performance, however, also depends on demand-side factors such as broadband adoption and use. Such factors are of course competitively sensitive. Given how important broadband appears to be to the economy, however, the time has come for policy makers to engage in a dialogue with industry and develop reasonable ways to measure more of the broadband indicators that matter.

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[22] See Strover, Chapman, and Waters (2003) for further discussion of the factors beyond availability that are needed to result in actual use of broadband-related technologies.

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VIII. Tables

Table 1: Data Sources

Type of Data	Description	Availability	Source
Business Activity Indicators	Used for employment, establishments, wages (payroll), industry sector and size mix. Reported at zip code level; aggregated for state-level analysis.	Collected annually; most recent data from 2002. Industry sectors coded by SIC (1994-7) and NAICS (1998-2002).	U.S. Census Bureau -ZIP Code Business Patterns (ZCBP)[23]
Demographic Indicators / Controls	Used for income, rent, educational attainment, and # of households. Reported at both zip code and state level. Also used to compute % of population in urban areas for state-level analysis.	Collected every 10 years; most recent data from 2000.	(1) U.S. Census Bureau - 2000 Decennial Census (2) GeoLytics – CensusCD ("1990 Long form in 2000 boundaries")[24]
Geographic Controls	Used to indicate how urban or rural a zip code is, based on its population and proximity to metropolitan areas.	Computed every 10 years; most recent coding from 2003.	Economic Research Service, U.S. Department of Agriculture - Urban Influence Code (UIC)[25]
Broadband Metrics	Reports number of high-speed Internet providers by zip code, and number of lines in service by state.	Collected every 6 months (end of June and December) since 12/1999.	U.S. Federal Communications Commission - Form 477 databases[26]

[23] See http://www.census.gov/epcd/www/zbp_base.html

[24] See http://www.census.gov/main/www/cen2000.html for data from the US Census Bureau, and http://www.geolytics.com/USCensus,Census-1990-Long-Form-2000-Boundaries,Products.asp for GeoLytics data. Use of the GeoLytics CD simplified the matching and aggregation of data for changes across zip codes between 1990 and 2000.

[25] See http://www.ers.usda.gov/Data/UrbanInfluenceCodes/ . The rationale for the UIC is based on growth-pole and central place theory, and the effect that an area's geographic context has on its development, as discussed in Parr (1973), North (1975), and Polenske (1988).

[26] These data and reports are available at http://www.fcc.gov/wcb/iatd/comp.html

Table 2: Summary Statistics for Variables Used in Zip Code Level Analysis

Category	Variable	Full Sample Obs	Full Sample Mean (Std. Dev)	Sub-Sample (N=22,390) Mean (Std. Dev)	Description	Source
Dependent Variables	lnRent2K	30,659	6.167 (0.373)	6.218 (0.351)	Median Housing Rent, 2000 (Ln)	US Census, 2000 Decennial Census
	LnrSalary	27,421	0.066 (0.199)	0.068 (0.160)	Ratio of Average Salaries of 2002/1998 (Ln)	US Census, 2002 and 1998 ZCBP
	ptotIT02	27,659	0.233 (0.112)	0.226 (0.090)	Share of Establishments in IT-Intensive Sectors, 2002	US Census, 2002 ZCBP
	lnrEmplo	26,877	0.047 (0.389)	0.038 (0.316)	Ratio of Employment , 2002/1998 (Ln)	US Census, 2002 and 1998 ZCBP
	psm02	31,405	0.802 (0.131)	0.790 (0.098)	Share of Establishments with less than 10 Employees, 2002	US Census, 2002 ZCBP
	lnrEst	31,210	0.047 (0.273)	0.045 (0.171)	Ratio of Establishments 2002/ 1998 (Ln)	US Census, 2002 and 1998 ZCBP
Broadband	BB99	32,325	0.544 (0.498)	0.671 (0.470)	=1 if Zip Code had at least 1 broadband line by December 1999, =0 otherwise	FCC, Form 477 Database
Control Variables	dUrban	32,325	0.542 (0.498)	0.620 (0.485)	=1 if Zip Code in Urban Area (UIC=1,2,3), 0=otherwise	USDA, Economic Research Service
	gEmp9498	27,348	0.325 (5.525)	0.387 (6.072)	Growth Rate in the Number of Employees 1994 --1998	US Census, 1994 and 1998 ZCBP
	grColl90s	30,359	7.986 (80.522)	8.822 (80.180)	Growth Rate in the Number of People (25+) with College Degree or Higher,1990--2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	grEst9498	30,786	0.197 (3.119)	0.148 (1.195)	Growth Rate in the Number of Establishments, 1994 - 1998	US Census, 1994 and 1998 ZCBP
	grFInc90s	31,579	0.762 (44.808)	0.867 (53.213)	Growth Rate in Median Family Income, 1990--2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	grLabor90s	31,579	4.997 (63.978)	5.026 (66.064)	Growth of the Civilian Employed Labor Force ,1990 --2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	grpIT9800	26,954	0.044 (0.273)	0.038 (0.249)	Growth Rate of Share of Establishment in IT-Intensive Sectors ,1998 - 2000	US Census, 1998 and 2000 ZCBP
	grSalary9498	26,203	0.202 (0.378)	0.191 (0.319)	Growth Rate of Average Salary, 1994 -- 1998	US Census, 1994 and 1998 ZCBP
	lnRent90	31,528	5.838 (0.443)	5.902 (0.414)	Median Housing Rent, 1990 (Ln)	GeoLytics, 1990 Decennial Census
	pcollege2K	31,181	18.511 (13.622)	19.697 (13.662)	Share of Population (25+) with College Degree or Higher, 2000	US Census, 2000 Decennial Census
	pIT98	27,441	0.227 (0.110)	0.219 (0.088)	Share of Establishments in IT-Intensive Sectors, 1998	US Census, 1998 ZCBP
	psm98	31,436	0.804 (0.131)	0.792 (0.097)	Share of Establishments with fewer than 10 Employees, 1998	US Census, 1998 ZCBP

Table 3: Summary Statistics for Variables Used at State Level Analysis

Category	Variable	Mean (Std. Dev)	Description	Source
Dependent Variables	LnRent00	6.315 (0.171)	Median Housing Rent, 2000 (Ln)	US Census, 2000 Decennial Census
	lnrSalary	0.132 (0.018)	Ratio of Average Salaries of 2002/1998 (Ln)	US Census, 2002 and 1998 ZCBP
	ptotIT02	0.268 (0.024)	Share of Establishments in IT-Intensive Sectors, 2002	US Census, 2002 ZCBP
	psmall02	0.738 (0.021)	Share of Establishments with fewer than 10 Employees, 2002	US Census, 2002 ZCBP
	LnrEmplo	0.039 (0.037)	Ratio of Employment 2002/1998 (Ln)	US Census, 2002 and 1998 ZCBP
	lnrEst	0.034 (0.032)	Ratio of # Establishments 2002/1998 (Ln)	US Census, 2002 and 1998 ZCBP
Broadband	BBAvailHU99	0.864 (0.106)	% of Housing Units located in zip codes with available broadband by December 1999	FCC, Form 477 Database; US Census, 2000 Decennial Census
	BBPen00	0.035 (0.022)	No. lines for residential and small firms, divided by total number of housing units and business establishments with fewer than 10 employees	FCC, Form 477 Database; US Census, 2000 Decennial Census, 2000 ZCBP
	SqBBPen00	0.002 (0.002)	Squared term of BBPen00	FCC, Form 477 Database; US Census, 2000 Decennial Census, 2000 ZCBP
Independent Variables	gEmp9498	0.125 (0.044)	Growth Rate in the Number of Employees 1994 -- 1998	US Census, 1994 and 1998 ZCBP
	grcollege90s	0.387 (0.137)	Growth Rate in the Number of People (25+) with College Degree or Higher 1990 -- 2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	pcollege2K	23.765 (4.347)	Share of Population (25+) with College Degree or Higher, 2000	US Census, 2000 Decennial Census
	grEst9498	0.074 (0.043)	Growth Rate in the Number of Establishments 1994 - 1998	US Census, 1994 and 1998 ZCBP
	grFamInc90s	0.401 (0.070)	Growth Rate in Median Family Income 1990 -- 2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	grLabor90s	0.147 (0.109)	Growth of the Civilian Employed Labor Force 1990 -- 2000	US Census, 2000 Decennial Census; GeoLytics, 1990 Decennial Census
	grpIT9800	0.006 (0.010)	Growth Rate of Share of Establishment in IT Intensive Sectors 1998 -- 2000	US Census, 1998 and 2000 ZCBP
	grSalary9498	0.177 (0.039)	Growth Rate on Average Salary 1994 – 1998	US Census, 1994 and 1998 ZCBP
	LnRent90	6.064 (0.234)	Median Housing Rent, 1990 (Ln)	GeoLytics, 1990 Decennial Census
	psmall98	0.742 (0.021)	Share of Establishments with less than 10 Employees, 1998	US Census, 1998 ZCBP
	ptotIT98	0.258 (0.023)	Share of Establishments in IT-Intensive Sectors, 1998	US Census, 1998 ZCBP
	pUrbHousing00	0.703 (0.153)	Share of Urban Housing Units 2000	US Census, 2000 Decennial Census
	pUrbPop00	0.714 (0.149)	Share of Urban Population 2000	US Census, 2000 Decennial Census

Table 4: Number and Share of Zip Codes with Broadband December 1999-December 2002

Date	Zip Codes with Broadband	Cumulative %
December-1999	17,683	54.44%
June-2000	2,725	8.39%
December-2000	1,970	6.07%
June-2001	2,026	6.24%
December-2001	910	2.80%
June-2002	957	2.95%
December-2002	894	2.75%
No Broadband by December 2002	5,316	16.37%
Total	32481	100.00%

Source: the authors, based on data from FCC Form 477 and US Census Bureau's Decennial Census and Zip Code Business Patterns

Table 5: State Level Penetration of Broadband Lines among Residential and Small Establishments Users 2000-2002

State	2000	2001	2002	State	2000	2001	2002
Alabama	1.60%	5.95%	10.03%	Montana	1.49%	2.67%	4.13%
Alaska	0.20%	16.18%	18.62%	Nebraska	6.70%	9.11%	14.98%
Arizona	6.21%	10.26%	15.26%	Nevada	5.87%	10.73%	15.81%
Arkansas	2.14%	5.16%	7.79%	N.Hampshire	6.87%	10.96%	16.12%
California	8.20%	13.17%	19.96%	New Jersey	6.88%	15.00%	12.91%
Colorado	4.70%	8.19%	13.86%	New Mexico	2.62%	3.46%	6.30%
Connecticut	7.04%	12.43%	20.04%	New York	6.06%	12.77%	21.77%
Delaware	0.68%	6.70%	12.55%	N. Carolina	2.26%	8.46%	14.31%
D.C.	5.03%	9.92%	13.71%	N. Dakota	1.90%	1.68%	6.18%
Florida	3.33%	10.17%	15.92%	Ohio	3.51%	7.47%	12.68%
Georgia	1.98%	9.78%	16.00%	Oklahoma	2.73%	6.64%	11.62%
Hawaii	*	*	*	Oregon	4.34%	8.59%	15.89%
Idaho	2.39%	2.39%	8.77%	Pennsylvania	1.94%	5.84%	9.73%
Illinois	3.60%	6.46%	12.19%	Rhode Island	6.29%	13.06%	17.66%
Indiana	0.88%	3.79%	6.46%	S. Carolina	2.02%	6.32%	11.00%
Iowa	4.27%	6.03%	8.75%	S. Dakota	3.20%	2.45%	4.89%
Kansas	5.40%	10.15%	15.62%	Tennessee	3.04%	8.00%	12.94%
Kentucky	0.69%	2.59%	4.35%	Texas	4.95%	8.81%	14.16%
Louisiana	2.10%	7.71%	12.53%	Utah	3.70%	7.94%	13.39%
Maine	3.67%	6.88%	9.71%	Vermont	2.27%	6.55%	9.36%
Maryland	1.67%	10.15%	14.84%	Virginia	2.68%	8.47%	13.18%
Massachusetts	9.29%	16.24%	21.10%	Washington	6.51%	11.43%	16.01%
Michigan	2.73%	8.80%	13.32%	West Virginia	0.63%	3.56%	8.38%
Minnesota	4.79%	8.32%	14.33%	Wisconsin	2.40%	6.58%	12.80%
Mississippi	0.34%	2.37%	5.96%	Wyoming	*	2.87%	5.61%
Missouri	3.12%	6.47%	9.30%	Total	3.61%	7.91%	12.46%

Source: the authors, based on data from FCC Form 477 and US Census Bureau's Decennial Census and Zip Code Business Patterns

Table 6: Average Zip Code Area by Level of UIC

dUrban	UIC	Average Area (sq. miles)	UIC Definition
1	1	28.89	In large metro area of 1+ million residents
	2	67.29	In small metro area of less than 1 million residents
	3	86.69	Micropolitan adjacent to large metro
0	4	91.64	Noncore adjacent to large metro
	5	82.5	Micropolitan adjacent to small metro
	6	107.81	Noncore adjacent to small metro with own town
	7	134.02	Noncore adjacent to small metro no own town
	8	134.28	Micropolitan not adjacent to a metro area
	9	131.9	Noncore adjacent to micro with own town
	10	167.47	Noncore adjacent to micro with no own town
	11	233.06	Noncore not adjacent to metro or micro with own town
	12	225.83	Noncore not adjacent to metro or micro with no own town
	Average	86.41	

Source: the authors, based on USDA and US Census[27]

Table 7: Broadband Impact on Growth of Selected Economic Variables[28]
(+/-=growth higher/lower in broadband communities; *=significant at 90% or above)

	State[29]	Zip	Matched Panel
Employment	-/+*	+*	+*
Wages	+/-	-	-
Rental rates	+*	+*	-*
Establishment	+/-*	+*	+*
IT-intensive share of establishments	-/+*	+*	+*

[27] See http://www.census.gov/geo/ZCTA/zcta.html for US Census Zip Code Tabulation Areas and http://www.ers.usda.gov/Data/UrbanInfluenceCodes/ for USDA's UIC.

[28] Dependent variable is growth rate from 1998-2002, with exception of rental rates, which are 1990-2000

[29] First sign refers to broadband penetration, second sign to square of broadband penetration.

Table 8: Means for Communities with (and without) Broadband by Dec99

Categories	Variable	With Broadband by Dec 99 (N=15,020) Mean (Std. Dev.)	With No Broadband by Dec 99 (N=7,370) Mean (Std. Dev.)
Dependent Variables	lnRent2K	6.306 (0.341)	6.039 (0.298)
	LnrSalary	0.072 (0.131)	0.059 (0.206)
	ptotIT02	0.240 (0.088)	0.195 (0.088)
	lnrEmplo	0.049 (0.263)	0.015 (0.401)
	psm02	0.768 (0.087)	0.834 (0.102)
	lnrEst	0.054 (0.150)	0.027 (0.204)
Independent Variables	dUrban	0.739 (0.438)	0.374 (0.483)
	URinfl03	2.882 (2.632)	5.294 (3.253)
	gEmp9498	0.434 (7.356)	0.289 (1.315)
	grColl90s	11.526 (96.28)	3.310 (24.549)
	grEst9498	0.169 (1.428)	0.104 (0.425)
	grFInc90s	1.046 (64.969)	0.501 (0.370)
	grLabor90s	6.487 (79.518)	2.046 (18.969)
	grpIT9800	0.030 (0.193)	0.053 (0.334)
	grSalary9498	0.180 (0.243)	0.212 (0.432)
	lnRent90	5.995 (0.403)	5.711 (0.369)
	pcollege2K	22.387 (14.684)	14.211 (9.096)
	pEst98	0.029 (0.133)	0.024 (0.042)
	pIT98	0.232 (0.085)	0.191 (0.087)
	psm98	0.772 (0.086)	0.832 (0.102)

9. Employment - Table 9A: Employment - State Level Regressions

	(9A1) LnrEmplo	(9A2) LnrEmplo	(9A3) LnrEmplo	(9A4) LnrEmplo
BBPen00	0.44262	-0.39993		-0.45585
	[0.88115]	[0.79912]		[0.81443]
SqBBPen00	-0.73487	7.61773		7.43397
	[10.00182]	[8.99095]		[9.07825]
gEmp9498		0.42372	0.3912	0.41257
		[0.10915]***	[0.10807]***	[0.11250]***
pUrbPop00			0.03577	0.01914
			[0.03221]	[0.03961]
Constant	0.02467	-0.013	-0.03534	-0.02295
	[0.01621]	[0.01716]	[0.02399]	[0.02689]
Observations	48	48	48	48
R-squared	0.0531	0.2947	0.2801	0.2985

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9B: Employment - Zip Code Regressions

	(9B1) lnrEmplo	(9B2) lnrEmplo	(9B3) lnrEmplo	(9B4) lnrEmplo
BB99	0.03344	0.0333		0.01045
	[0.00515]***	[0.00515]***		[0.00560]*
gEmp9498		0.00094	0.00075	0.00075
		[0.00036]***	[0.00031]**	[0.00031]**
dUrban			0.0585	0.05548
			[0.00493]***	[0.00507]***
…				
Constant	0.01512	0.01485	0.04361	0.03571
	[0.00468]***	[0.00468]***	[0.03040]	[0.03070]
Observations	22390	22390	22390	22390
R-squared	0.0025	0.0028	0.0271	0.0273

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 9C: Employment - Zip Code nnmatch regressions

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	.0144264	1.94	0.052

	Variable	Treatment BB99=1 Mean	Treatment BB99=1 Std. Dev.	Control BB99=0 Mean	Control BB99=0 Std. Dev.
Dep. Variable	lnrEmplo	0.0379408	0.238360	0.0329223	0.3475896
Independent Variables	gEmp9498	0.1832633	3.193463	0.1627447	0.9055611
	URinfl03	2.74577	2.341581	2.746226	2.342017

10. Wage Regressions - Table 10A: State Level Salary Regressions

	(10A1)	(10A2)	(10A3)	(10A4)
	lnrSalary	lnrSalary	lnrSalary	lnrSalary
BBPen00	0.34782	0.42969		0.54628
	[0.42041]	[0.44506]		[0.41635]
SqBBPen00	-0.47119	-0.85803		-2.55233
	[4.77196]	[4.84982]		[4.58457]
grSalary9498		-0.04846	-0.08287	-0.15117
		[0.08110]	[0.07780]	[0.08060]*
grcollege90s			0.07534	0.07657
			[0.04023]*	[0.03871]*
pcollege2K			0.00282	0.00243
			[0.00074]***	[0.00074]***
grLabor90s			-0.08908	-0.09298
			[0.04953]*	[0.04814]*
pUrbPop00			-0.0274	-0.04813
			[0.02390]	[0.02514]*
pITfirms98			0.06221	0.11477
			[0.16172]	[0.15833]
Constant	0.1204	0.12675	0.06724	0.07492
	[0.00773]***	[0.01318]***	[0.03122]**	[0.03042]**
Observations	48	48	48	48
R-squared	0.1389	0.1458	0.3153	0.3971

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10B: Zip Code Salary Regressions

	(10B1)	(10B2)	(10B3)	(10B4)
	LnrSalary	LnrSalary	LnrSalary	LnrSalary
BB99	0.01328	0.00932		-0.00269
	[0.00263]***	[0.00253]***		[0.00284]
grSalary9498		-0.12272	-0.12484	-0.12504
		[0.01042]***	[0.01056]***	[0.01059]***
grColl90s			-0.00001	-0.00001
			[0.00001]	[0.00001]
pcollege2K			0.00082	0.00083
			[0.00009]***	[0.00010]***
grLabor90s			0.00003	0.00003
			[0.00001]**	[0.00001]**
dUrban			0.00429	0.00493
			[0.00252]*	[0.00259]*
pIT98			0.02275	0.02443
			[0.01586]	[0.01604]
…				
Constant	0.05957	0.08564	0.08206	0.08359
	[0.00241]***	[0.00297]***	[0.01355]***	[0.01364]***
Observations	22390	22390	22390	22390
R-squared	0.0015	0.0614	0.0772	0.0773

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10C: Zip Code Salary nnmatch regressions

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	.0003026	0.08	0.938

		Treatment BB99=1		Control BB99=0	
	Variable	Mean	Std. Dev.	Mean	Std. Dev.
Dep. Variable	LnrSalary	0.0684275	0.1435969	0.0682071	0.1882474
Independent Variables	grSalary9498	0.1904726	0.3090776	0.1904069	0.3222383
	grColl90s	8.707712	80.1020	7.0213	49.18039
	pcollege2K	19.69874	13.62371	19.57184	13.54363
	grLabor90s	4.957307	65.87055	3.784536	29.32728
	URinfl03	3.673292	3.06596	3.683296	3.060917

11. Rent Regressions - Table 11A: State Level Rent Regressions

	(11A1)	(11A2)	(11A3)	(11A4)
	LnRent00	LnRent00	LnRent00	LnRent00
BBAvailHU99	0.94869	0.27693		0.29616
	[0.19152]***	[0.07635]***		[0.09454]***
LnRent90		0.6333	0.70058	0.71233
		[0.03474]***	[0.04779]***	[0.04370]***
grFamInc90s			0.26186	0.25361
			[0.13617]*	[0.12408]**
grLabor9200			0.23264	0.23224
			[0.06576]***	[0.05991]***
pUrbHousing00			0.10155	-0.08143
			[0.05463]*	[0.07674]
Constant	5.49514	2.23559	1.8596	1.66449
	[0.16676]***	[0.18804]***	[0.30911]***	[0.28840]***
Observations	48	48	48	48
R-squared	0.3478	0.9222	0.9441	0.9547

Standard errors in brackets
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 11B: Zip Code Rent Regressions

	(11B1)	(11B2)	(11B3)	(11B4)
	lnRent2K	lnRent2K	lnRent2K	lnRent2K
BB99	0.26704	0.10341		0.06557
	[0.00445]***	[0.00507]***		[0.00390]***
lnRent90		0.57686	0.41784	0.40158
		[0.01315]***	[0.01646]***	[0.01646]***
grFInc90s			0.00007	0.00007
			[0.00002]***	[0.00002]***
grLabor90s			0.00016	0.00015
			[0.00007]**	[0.00006]**
dUrban			0.16388	0.14939
			[0.00550]***	[0.00512]***

…

Constant	6.03934	2.7445	3.73793	3.78442
	[0.00348]***	[0.07570]***	[0.10080]***	[0.09939]***
Observations	22390	22390	22390	22390
R-squared	0.1278	0.5439	0.6165	0.6227

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table11C: Zip Code Rent nnmatch regressions

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	-0.020979	-4.68	0.000

		Treatment BB99=1		Control BB99=0	
	Variable	Mean	Std. Dev.	Mean	Std. Dev.
Dep. Variable	lnRent2K	6.227739	.3418756	6.184831	.3376517
Independent Variables	lnRent90	5.901608	.4138795	5.901871	.4118173
	grFInc90s	0.8652419	53.04271	0.4759079	.323962
	grLabor90s	4.979731	65.78181	3.964122	29.44959
	URinfl03	3.671208	3.061892	3.677066	3.059775

12. Total Establishments - Table 12A: Total Establishments - State Level Regressions

	(12A1)	(12A2)	(12A3)	(12A4)
	lnrEst	lnrEst	lnrEst	lnrEst
BBPen00	1.12032	0.41932		0.19639
	[0.76148]	[0.40444]		[0.42569]
SqBBPen00	-8.83193	-1.20117		-0.06339
	[8.64342]	[4.58608]		[4.84344]
grEst9498		0.6161	0.51694	0.51294
		[0.05633]***	[0.11529]***	[0.11916]***
grLabor90s			0.03182	0.03725
			[0.04525]	[0.04880]
pUrbPop00			0.05317	0.03633
			[0.01648]***	[0.02019]*
Constant	0.00987	-0.02436	-0.04674	-0.04213
	[0.01401]	[0.00798]***	[0.01176]***	[0.01340]***
Observations	48	48	48	48
R-squared	0.0865	0.7376	0.7628	0.7740

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 12B: Total Establishments - Zip Code Regression

	(12B1)	(12B2)	(12B3)	(12B4)
	lnrEst	lnrEst	lnrEst	lnrEst
BB99	0.02625	0.02552		0.00483
	[0.00268]***	[0.00268]***		[0.00287]*
grEst9498		0.01122	0.00959	0.00957
		[0.00468]**	[0.00401]**	[0.00401]**
dUrban			0.04425	0.04285
			[0.00262]***	[0.00271]***
grLabor90s			0.00006	0.00006
			[0.00001]***	[0.00001]***
…				
Constant	0.02725	0.02608	0.03908	0.03542
	[0.00238]***	[0.00243]***	[0.02072]*	[0.02077]*
Observations	22390	22390	22390	22390
R-squared	0.0052	0.0114	0.0626	0.0627

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 12C: Total Establishments - Zip Code nnmatch regression

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	0.0123135	3.37	0.001

		Treatment BB99=1		Control BB99=0	
	Variable	Mean	Std. Dev.	Mean	Std. Dev.
Dep. Variable	lnrEst	0.0480704	0.1595301	0.0361407	0.1881027
Independent Variables	grEst9498	0.1381222	1.145844	0.1260622	0.6235826
	grLabor90s	4.610127	63.30446	3.935026	28.74862
	URinfl03	3.599211	3.018462	3.603401	3.014999

13. Establishments in IT Intensive Sectors - Table 13A: Establishments in IT Intensive Sectors - State Regressions

	(13A1) ptotIT02	(13A2) ptotIT02	(13A3) ptotIT02	(13A4) ptotIT02
BBPen00	0.68198	-0.14742		-0.27606
	[0.54717]	[0.11538]		[0.08941]***
SqBBPen00	-3.60893	0.76199		2.61798
	[6.21081]	[1.28341]		[1.01144]**
ptotIT98		1.06976	1.0274	1.03108
		[0.03346]***	[0.03715]***	[0.03414]***
grcollege90s			0.00163	0.00271
			[0.00930]	[0.00849]
pcollege2K			-0.00014	-0.00003
			[0.00017]	[0.00016]
grLabor90s			0.01454	0.0169
			[0.01142]	[0.01051]
pUrbPop00			-0.00281	-0.00043
			[0.00572]	[0.00574]
grpIT9800			0.21154	0.21862
			[0.06271]***	[0.05792]***
Constant	0.25037	-0.00356	0.00476	0.004
	[0.01007]***	[0.00821]	[0.00725]	[0.00675]
Observations	48	48	48	48
R-squared	0.1299	0.9641	0.9778	0.9825

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 13B: Establishments in IT Intensive Sectors - Zip Code Regressions

	(13B1) ptotIT02	(13B2) ptotIT02	(13B3) ptotIT02	(13B4) ptotIT02
BB99	0.04463	0.00994		0.00594
	[0.00125]***	[0.00089]***		[0.00085]***
pIT98		0.84724	0.86345	0.85988
		[0.00541]***	[0.00598]***	[0.00609]***
grColl90s			0.00001	0.00001
			[0.00000]***	[0.00000]***
pcollege2K			0.00065	0.00062
			[0.00003]***	[0.00003]***
dUrban			0.00314	0.00174
			[0.00075]***	[0.00076]**
grpIT9800			0.0795	0.07963
			[0.00242]***	[0.00241]***
…				
Constant	0.19566	0.03319	0.01977	0.01641
	[0.00103]***	[0.00112]***	[0.00496]***	[0.00508]***
Observations	22390	22390	22390	22390
R-squared	0.0539	0.7055	0.7619	0.7626

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 13C: Establishments in IT Intensive Sectors - Zip Code nnmatch regressions

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	.0028547	1.99	0.046

		Treatment BB99=1		Control BB99=0	
	Variable	Mean	Std. Dev.	Mean	Std. Dev.
Dep. Variable	ptotIT02	0.2254862	0.0889145	0.22155	0.0898961
Independent Variables	pIT98	0.2190703	0.0874809	0.2180745	0.0862081
	grColl90s	8.685352	80.12625	6.908195	51.86047
	pcollege2K	19.69035	13.58824	19.51451	13.51298
	URinfl03	3.666503	3.05856	3.681733	3.056761
	grpIT9800	0.0381206	0.2466281	0.0360131	0.2491051

14. Small Establishments - Table 14A: Small Establishments - State Level Regressions

	(14A1)	(14A2)	(14A3)	(14A4)
	psmall02	psmall02	psmall02	psmall02
BBPen00	0.0625	0.24637		0.12979
	[0.51854]	[0.12967]*		[0.12543]
SqBBPen00	-0.70207	-2.73645		-1.7089
	[5.88580]	[1.47174]*		[1.40863]
psmall98		0.95164	1.00245	1.00152
		[0.03655]***	[0.03993]***	[0.04015]***
grcollege90s			0.01559	0.01374
			[0.00556]***	[0.00579]**
pcollege2K			-0.00023	-0.0002
			[0.00023]	[0.00025]
ptotIT98			0.01027	0.00199
			[0.04994]	[0.05073]
pUrbPop00			0.01172	0.01364
			[0.00724]	[0.00793]*
Constant	0.73681	0.02759	-0.01768	-0.0179
	[0.00954]***	[0.02735]	[0.03334]	[0.03353]
Observations	48	48	48	48
R-squared	0.0003	0.9349	0.9522	0.9459

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 14B: Small Establishments - Zip Code Regression

	(14B1) psm02	(14B2) psm02	(14B3) psm02	(14B4) psm02
BB99	-0.06545	-0.01574		-0.01324
	[0.00139]***	[0.00103]***		[0.00110]***
psm98		0.81688	0.80843	0.79594
		[0.00541]***	[0.00555]***	[0.00583]***
pIT98			-0.04825	-0.04339
			[0.00605]***	[0.00601]***
grColl90s			0	0
			[0.00000]	[0.00000]
pcollege2K			-0.00001	0.00005
			[0.00003]	[0.00003]*
dUrban			-0.00952	-0.00694
			[0.00096]***	[0.00096]***
…				
Constant	0.83439	0.15403	0.15994	0.17797
	[0.00120]***	[0.00476]***	[0.00839]***	[0.00887]***
Observations	22390	22390	22390	22390
R-squared	0.0990	0.6958	0.6983	0.7013

Robust standard errors in brackets. State dummies are not shown in table. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 14C: Small Establishments - Zip Code nnmatch regression

		Coefficient	z-statistic	P>|Z|
N=22,390	BB99	-0.015714	-9.02	0.000

	Variable	Treatment BB99=1 Mean	Treatment BB99=1 Std. Dev.	Control BB99=0 Mean	Control BB99=0 Std. Dev.
Dep. Variable	psm02	0.7875819	0.0944401	0.8049239	0.0993516
Independent Variables	psm98	0.792165	0.0957805	0.7944902	0.0954684
	pIT98	0.2191602	0.087082	0.217224	0.0848717
	grColl90s	8.698381	80.10981	6.833522	51.23653
	pcollege2K	19.70571	13.60049	19.34939	13.2098
	URinfl03	3.666324	3.058514	3.699777	3.044652

Table 15: Impact (Controlled) on Economic Variables in Broadband-enabled communities (i.e., had broadband by Dec99)

	Zip Code Sample
Employment (Jobs)	Employment growth rate about 1% higher, 1998 - 2002.
Wages	No statistically measurable impact observed in data by 2002.
Property Values	Housing rental rates over 6% higher in 2000.
Business Establishments (proxy for firms)	Almost 0.5% higher rate of growth in the number of establishments, 1998 - 2002.
Industry Mix	Over 0.5% increase in the share of establishments in IT-intensive sectors, 1998 – 2002. About 1% reduction in share of small establishments.