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Technical Appendix

Does Broadband Boost Local Economic Development?

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Existing research on the effect of broadband on job growth and labor market outcomes has not yielded definitive conclusions, in part because of differences in methods. The literature has tended to use broadband measures at the state or county level, even though infrastructure availability and quality can vary by neighborhood (e.g., Crandall, Lehr, and Litan, 2007; Van Gaasbeck et al., 2007). Previous work has also not fully considered alternative factors that might be correlated with broadband availability and economic growth, and omitting important variables could produce misleading results (e.g., Osorio, 2006; Shideler et al., 2007). Finally, previous work has not assessed whether broadband availability could be the effect, rather than the cause, of job growth: Broadband providers could build up their infrastructure or provide service in response to realized or anticipated growth in population or employment, rather than the other way around.

Several recent papers have tried to assess how broadband affects economic development. Four empirical issues distinguish these papers from each other: (1) how broadband is measured; (2) which economic development outcomes are measured; (3) which control variables are included; and (4) which geographic level is the unit of analysis. These four issues are related: For instance, some broadband measures are available only for states, not ZIP codes or counties, so the choice of broadband measure can determine the geographic level of analysis. This section reviews several papers, highlighting these issues, and then suggests why some approaches are preferable to others. Overall, most studies find that broadband has a positive relationship with employment and establishment growth, yet the relationship between broadband and income (or wages) is mixed—consistent with the results we report.

Crandall, Lehr, and Litan (2007) measure broadband using FCC data on broadband lines in use per capita. These broadband data are reported annually for each state by the FCC and reflect broadband adoption by households and businesses. Their outcome measures include employment and output (GDP) growth for the state overall as well as for individual sectors (e.g. manufacturing, finance) in the state, over a one- or two-year period. Control variables include average annual temperature, a measure of the business tax burden, unionization, average wages, level of education, and Census region. They find that broadband adoption is positively correlated with employment growth but not output growth for states overall; the effect on employment and GDP is positive for a minority of economic sectors. They speculate that the different effects on employment and output growth could be due to the Bureau of Economic Analysis's estimation process of annual state output, which is less precise than employment measures.

Van Gaasbeck et al. (2007) also use adoption as their broadband measure in their study of counties in California. They use proprietary residential broadband adoption estimates from a household survey by Scarborough, a market research firm, and construct an annual panel of broadband adoption and economic development outcomes (employment, establishments, and output).[1] Their main finding is that higher

[1] They do not report the sample size in Scarborough's survey, but the considerable annual fluctuations—upward and downward—in broadband adoption for many smaller regions within California suggest small samples with large standard errors.

broadband adoption is associated with higher employment and fewer establishments in the full model with county fixed-effects. Intending to test for endogeneity, they find that the effects on the key outcome variables are unchanged when using the one-year lag of broadband adoption instead of contemporaneous broadband adoption, though this result is hard to interpret because they omit contemporaneous adoption, and of course broadband adoption could be the effect of predicted future growth.

Crandall, Lehr, and Litan (2007) and Van Gaasbeck et al. (2007) suggest that adoption may better reflect the extent to which broadband can affect a local economy than availability since broadband affects economic and social outcomes only if adopted. However, adoption is even more plausibly endogenous with respect to economic growth than availability is since the decision to adopt broadband, conditional on availability, could be the outcome of economic growth rather than the cause. Furthermore, from a policy perspective, increasing broadband availability is more feasible—whether through regulation, subsidy, or direct provision—than increasing broadband adoption since adoption depends on a range of demand factors, some of which are beyond the easy reach of public policy. Studies looking at broadband availability have a better chance to offer policymakers a clearer picture of whether broadband policy can affect local economic growth. Also, adoption measures exist only for states and the largest metropolitan areas, whereas availability measures exist down to the ZIP code level. While labor markets are as large as a metropolitan area or small state, broadband infrastructure requires detailed geographic analysis: Firms can hire workers that live ten miles away, but they cannot easily get broadband service if the nearest infrastructure network ends ten miles away.

Several other papers use broadband availability measures instead of broadband adoption measures. Osorio (2006) assesses whether broadband provision by a municipal electric utility (MEU) affects employment and output; he uses a matching estimator, matching places based on past employment trends and composition, residential demographics, and private-sector broadband provision. He finds a positive effect of MEU broadband provision on the number of establishments but not on employment, average salaries, or the number of high-tech establishments. Shideler, Badasyan, and Taylor (2007) measure county-level broadband availability in Kentucky based on broadband providers' proprietary infrastructure data, collected and mapped by ConnectKentucky. They regress county-level total employment growth and sector employment growth on broadband availability, controlling for past growth, education, unemployment, and road density. The effect of broadband is positive and statistically significant for total employment and for employment in the construction, administrative/support/waste services, and information sectors.

Gillett et al. (2006) define broadband availability as whether the FCC reported at least one broadband provider with subscribers in a ZIP code in December 1999. Controlling for past employment growth, sectoral composition, and demographics, they find a positive, statistically significant effect of broadband availability on employment, the number of establishments overall, and the number of establishments in high-tech sectors. Gillett et al. also perform a state-level analysis using number of broadband lines per capita (as did Crandall, Lehr, and Litan, 2007), but after finding few significant effects they dismiss the state-level analysis as too coarse to reveal the effect of broadband. They conclude that states are too large a geography for the analysis of broadband on economic development: Even small states show considerable intra-state variation in broadband availability and adoption.

Stenberg et al. (2009) find faster employment growth in counties with better broadband availability but no effect on per capita income growth in most years. To measure broadband availability at the county level, they, like Gillett et al. (2006), use FCC Form 477 provider counts by ZIP code, which they aggregate to the county level. Unlike Gillett et al., they use the continuous provider count level rather than a binary measure indicating at least one provider in the area. As we do, below, they argue that the continuous provider count measure gives additional information about the extent of broadband availability. They estimate their effects for rural counties only, using a matching estimator; they match using similar variables to the economic and demographic controls used in other studies.

Another possible outcome of broadband deployment is increased telecommuting, but little data on telecommuting exists. Song, Orazem, and Singh (2006) look empirically at the relationship between broadband and telecommuting, which the authors proxy with a UCLA survey question about "using the Internet for work from home." The authors attribute the urban-rural gap in telecommuting largely to the greater availability of broadband in urban areas, which seems to contradict their finding that the effect of county-level broadband availability on county-level telecommuting is not statistically significant.[2] In addition to a questionable interpretation of their results, their study has two flaws. First, broadband availability can vary considerably within a county, so it would be better to focus on availability at the ZIP code level. Second, having a formal agreement with an employer to telecommute regularly is not synonymous with using the Internet to work from home, which encompasses everything from a formal agreement to work from home to bringing work home occasionally.

The literature—despite a near-consensus that broadband is positively related to employment growth—leaves many open questions. By controlling for or matching on factors such as past employment growth, the literature takes into account that geographic variation in broadband availability or adoption could be correlated with factors—such as the industry mix or residential education level—that could also affect economic growth. But the literature has not assessed the possibility that increased broadband availability or adoption is the effect, rather than only the cause, of economic growth, or that both broadband availability and employment growth are the effect of some other cause. Rational broadband providers will offer service where demand is already high or is expected to be high. Although several studies raise this issue, and Gillett et al. (2006), Van Gaasbeck et al. (2007), and Stenberg et al. (2009) are careful to caution that their results do not imply causality, none instruments for broadband availability or adoption in order to identify a causal effect or tries to infer causality using more informal approaches. Without considering reverse causality, it is hard to draw any policy implications from the finding that broadband and economic growth are positively correlated.

The second important open question is what any positive relationship between broadband and local employment growth (which other studies typically find) means for residents. While some studies look at income or wages, none considers the effect on employment relative to working-age population. Considering these outcomes reveals whether employment growth actually raises the likelihood of employment among residents, or whether the population grows as well and leaves the employment rate unchanged. Employment growth with population growth could still meet the policy objective of economic development, though it has different implications for households than employment growth that significantly exceeds population growth. Considering population growth also turns out to be useful in assessing causality, as the results will show.

A third open question is heterogeneous effects: Broadband's effect on economic outcomes could depend on local conditions. If broadband allows firms to substitute electronic communication for face-to-face interactions with

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[2] Table 2 of their paper seems to show that the relationship between the number of broadband providers in a county and using the Internet (column 1) to work from home is not statistically significant at the 5 percent level, though they report these results to be statistically significant in the text on pp. 14–15.

suppliers or customers, businesses could weigh other location considerations more heavily, such as the skill level of the workforce, other local amenities, or inexpensive land; if so, then places exhibiting these characteristics might experience a larger effect on employment due to broadband than other places. None of the studies discussed explores how broadband interacts with local conditions to affect economic outcomes.[3]

A fourth open question is the effect of broadband on home-based work, including telecommuting, bringing work home informally, or operating a home-based business. Incorporating these into the analysis of broadband and economic development would provide a fuller picture of how broadband affects the range of business and household behaviors and outcomes.

This report addresses all of these open questions.

Broadband and National Growth

A related academic literature considers the effect of broadband on national economic activity. Whereas the research discussed above compares economic outcomes in states, counties, or ZIP codes with better and worse broadband access, measuring the effect of broadband on national economic activity requires different methods. One approach is to compare broadband and economic activity internationally, but it is far more challenging to compare broadband availability, speeds, prices, or quality across countries than across jurisdictions within the United States. For instance, although the Organisation for Economic Co-operation and Development (OECD) ranks the United States 15th out of 30 countries for broadband subscribers per capita, Wallsten (2009) and others argue that this often-repeated ranking understates broadband adoption in the United States, and international comparisons using broader measures of information and communication technology generally rank the United States much more favorably.[4]

An alternative approach to measuring the national economic impact of broadband involves estimating cost savings from increased business efficiency and estimating increases in consumer well-being as measured by willingness to pay for broadband.[5] Estimates of the economic benefit of broadband are highly sensitive to methods and assumptions. One widely cited study estimates a range of $32–350 billion per year in consumer surplus nationally, depending on assumptions about the shape of the consumer demand curve (Crandall, Jackson, and Singer, 2003). Recent work by Greenstein and McDevitt (2009), using alternative assumptions and focusing on the marginal effect of broadband relative to dial-up, estimates the annual increase in GDP in the range of $8.3–10.6 billion, of which $4.8–6.7 billion is an increase in consumer surplus—far below even the lower bound of earlier work. Much of the difference in estimates hinges on assumptions about the elasticity of demand for broadband; Greenstein and McDevitt do not count the high willingness-to-pay among broadband's "biggest fanatics" with very inelastic demand, whereas the higher estimates of consumer surplus in Crandall, Jackson, and Singer assume some users value broadband at over $300/month. Furthermore, Greenstein and McDevitt estimate consumer surplus of switching from dial-up to broadband,

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[3] Although not a study of broadband per se, Forman, Goldfarb, and Greenstein (2009) look at the effect of business Internet adoption and county-level wage growth, and they do include interactions between their Internet measure and county-level start-year income, education, and share of businesses in information-technology-intensive industries. Their main conclusion is that business information technology usage raised wages more in counties that started with higher incomes and therefore contributes to the divergence of wages across counties.
[4] Current OECD international broadband comparisons are available at www.oecd.org/sti/ict/broadband. The ranking of broadband subscribers per capita in December 2008 is available in Table 1d: www.oecd.org/dataoecd/21/35/39574709.xls, viewed October 8, 2009.
[5] Consumers' willingness to pay for broadband isn't, on its own, a reason for public investment in broadband. Kolko (in press) points out that when consumers adopt broadband, they're more likely to increase consumption of entertainment activities (for example, downloading music) than activities leading to outcomes that coincide with public policy goals (for example, searching for a job online).

whereas Crandall, Jackson, and Singer appear to estimate the consumer surplus of broadband relative to not having any Internet service.

Data Description

In our analysis, we combine data from four main sources: broadband data from the FCC's annual Form 477 report; employment and business data from the National Establishment Time-Series; demographic data from the Census; and household data on telecommuting and other forms of working at home from Forrester Research. We link these datasets using U.S. Postal Service ZIP codes or the Census equivalent, ZCTAs (ZIP code tabulation areas). Below we describe each data source and then the process for linking them together using ZIP codes and ZCTAs.

FCC Form 477

The FCC's Form 477 data report the number of broadband providers with subscribers in each ZIP code, semiannually back to 1999. Providers offering broadband services at 200 kilobits per second or faster are included, which covers telephone-line-based DSL, cable modems, wireless, satellite, and power-line technologies. The FCC uses these data to assess the extent of broadband rollout in the United States.

The ZIP code provider count data have been criticized on several grounds. First, the FCC data reflects geographic patterns in subscriptions, not availability. Provider counts, therefore, could understate availability in areas where service is available but no one subscribes. This situation describes satellite broadband, which is available to nearly every household with a clear view of the southern sky but accounts for a very small share of high-speed subscriptions.[6] Second, many ZIP codes cover large geographic areas, and providers with a subscriber in a ZIP code might not offer service throughout the ZIP code. This could overstate broadband availability if the FCC data are interpreted to mean that an entire ZIP code is served by a provider. Third, the FCC data include providers who serve business or residential customers (or both), which could overstate the level of availability if the data are interpreted to mean availability for residential customers. Fourth, providers reporting service include those that buy or lease telecommunications facilities from wholesalers who might themselves offer retail service as well. Thus, the provider counts might reflect the level of market competition better than the extent of physical infrastructure if that infrastructure is shared among multiple providers.[7]

The FCC ZIP code provider counts, therefore, are hard to interpret. They are, at best, an imperfect measure of some combination of the deployment of broadband infrastructure, the extent of residential availability, and the depth of market competition. Furthermore, the published FCC ZIP code data include no information on the technology, price, or speed of service, which would be essential for understanding geographic differences in broadband markets within the United States.[8] Nonetheless, "there is no practical alternative to using the FCC data in assessing broadband availability" (Flamm, 2006). Policymakers use the FCC data to describe the broadband landscape, and although some reports are careful to mention that the FCC data do not reflect whether

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[6] Satellite accounts for less than 1 percent of all high-speed lines (FCC 2006). Because satellite offers slower speeds for higher monthly fees and requires costlier hardware, satellite is not considered an adequate substitute for the main wireline broadband technologies (cable and DSL); and satellite subscriptions might be more prevalent where cable and DSL are unavailable.
[7] The Government Accountability Office (2006) offers a fuller description and assessment of the FCC Form 477 data.
[8] Data on price and speed are also essential for understanding international differences in broadband services. Ultra-high-speed fiber service, for instance, is far more prevalent in Korea and Japan than in the U.S., as reported by the Organisation for Economic Co-operation and Development, www.oecd.org/sti/ict/broadband.

broadband is available to every residence in a ZIP code, the data are nonetheless used as a measure of broadband deployment (California Public Utilities Commission, 2006). Numerous academic papers have relied on the FCC Form 477 data, both to assess the effects of broadband on other outcomes and also to describe the factors that affect broadband availability.

Despite these difficulties of interpretation, the ZIP code provider count data provide useful information on the extent of broadband availability over time. Kolko (forthcoming) argues that although the FCC provider count is an imperfect measure of residential broadband availability, there is nonetheless a reliable relationship between the number of providers in a ZIP code and the estimated extent of residential broadband availability. Based on residential adoption data and adjusting for household characteristics, availability increases with the number of providers, especially at low numbers of providers. ZIP codes with multiple providers could have more widespread availability if not every provider covers all parts of the ZIP code. Also, if providers within a ZIP code do overlap geographically, then the number of providers could also reflect the extent of competition. Since the FCC does not publicly report on price, speed, or quality of service at the ZIP code level, we cannot identify the mechanism by which introducing an additional broadband provider affects the market for broadband services.[9] But since broadband policies often work by adding providers to an area—sometimes directly with public provision and sometimes indirectly through subsidization or regulation—using the number of providers as a proxy for broadband availability is meaningful in a policy context.

Kolko (forthcoming) shows that the relationship between provider count and availability is neither binary nor linear but rather non-linear: The marginal provider implies a bigger increase in the share of households within a ZIP code with broadband availability at low provider counts than at higher provider counts, suggesting that a logarithmic model is more appropriate. Other studies have used different functional forms of the provider count measure. Gillett et al. (2006), Prieger (2003), and Flamm (2006), using FCC Form 477 data, construct a binary measure using FCC provider counts to indicate whether a ZIP code had any broadband subscribers. Grubesic (2006) and Grubesic and Murray (2004) use the FCC provider count as a continuous variable and assume a linear relationship between broadband provider count and other variables; Stenberg et al. (2009) use provider count as a continuous variable and, in one model, include both a linear and squared term.

National Establishment Time Series

Data on employment and businesses come from the National Establishment Time-Series (NETS) database, a longitudinal panel of business establishments. The NETS is based on the Dun & Bradstreet register of nearly all U.S. businesses and includes annual information on the number of employees, 6-digit NAICS industry, and exact street address, including ZIP code. The NETS data cover the entire United States over the period 1992–2006. The NETS has important advantages over other data sources used to estimate the effect of broadband on economic development. Because the NETS reports exact street address and is not based on confidential government data, we can generate exact employment counts by ZIP code and detailed industry. Government sources like County Business Patterns suppress information for many industry-county and industry-ZIP code cells to preserve business confidentiality. Also, because the data are establishment-level and longitudinal, we can decompose employment changes into those due to births and deaths of establishments, relocations, and the expansion and

[9] Kolko (forthcoming) finds that broadband monthly subscription price does not vary with the number of providers in a ZIP code.

contraction of existing establishments, to assess whether any observed relationship between broadband and growth is due solely to jobs moving around from migration.[10]

Forrester Research

Forrester Research, a technology consultancy, surveys households about their technology adoption and behaviors. Surveys are conducted annually by mail, in English only, typically covering 60,000–100,000 households each year, with approximately one-sixth of respondents participating in successive years' surveys. Between 2001 and 2007, the surveys ask respondents whether they "bring work home to do outside normal business hours" and "have a formal arrangement with an employer to work from home one or more days per week" (i.e., as a telecommuter), and have a home-based business. The surveys also report respondents' ZIP codes, as well as standard household demographic variables.

Other Data Sources

Numerous other data sources provide information at the ZCTA and county level:

- Educational attainment, median household income, and population are available at the ZCTA level in the 2000 Census. In 1990, the Census did not report summary data for ZCTAs. To estimate 1990 ZCTA Census data, we assigned year-2000 Census tracts to ZCTAs using Mable/Geocorr (http://mcdc2.missouri.edu/websas/geocorr2k.html). Then, we used the Neighborhood Change Database—which recalculates 1970, 1980, and 1990 Census summary data using year-2000 tract definitions—to construct tract-based estimates of ZCTA-level summary data for the 1990 and 2000 Censuses.
- Climate variables at the county level come from the USDA Economic Research Service.
- Employment at businesses, employment among residents, working-age population, payroll, and household income at the county level come from the U.S. Bureau of Labor Statistics and Census Bureau.
- Population, income, education, and housing characteristics at the county level come from the 1990 and 2000 Census and the 2006 American Community Survey, as well as from Census Bureau mid-decade estimates.
- Slope and road density at the ZCTA level are generated using ESRI mapping data in ArcGIS software.

Combining Datasets

Most of the empirical analysis is at the ZCTA level; the individual-level analysis of telecommuting and home-based work activities also relies on ZCTA-level broadband availability. A major challenge of this research, therefore, was to combine data from multiple sources that use ZIP codes and ZCTAs, the Census approximation of ZIP codes.[11]

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[10] See Kolko and Neumark (2007) for a longer description and quality assessment of the NETS database.

[11] Two studies discussed above also use FCC provider count data and faced similar challenges of combining ZIP code level data with data reported at other geographic levels. Gillett et al. (2006) merged datasets based on FCC ZIP codes, USPS ZIP codes, and Census Bureau ZCTAs and analyzed those that matched fully; it appears from their Appendix II that they did not include employment data in USPS ZIP codes (reported in ZIP code Business Patterns) that lacked a ZCTA with the same number, as well as unique and P.O. Box ZIP codes, and it is unclear how they handled employment changes in USPS ZIP codes that changed over time. Stenberg et al. (2009) aggregated the FCC ZIP code provider counts to the county level and matched them to other data sources using counties.

The NETS reports business addresses with USPS ZIP codes. Forrester reports households' USPS ZIP codes. ZIP codes are a USPS designation intended to facilitate mail delivery, not data gathering and reporting, and they do not follow any of the geographies that the Census Bureau uses for reporting economic and demographic data (Census blocks, Census tracts, counties, etc.). USPS ZIP codes include standard, unique, and P.O. Box ZIP codes. Standard ZIP codes typically cover a neighborhood, region, or other geographic area and include numerous residences, businesses, or both. Unique ZIP codes serve a single institution, such as a large company or public institution, and typically include few or no residences. P.O. Box ZIP codes are often assigned to a post office location. Whereas standard ZIP codes are typically represented by a "polygon" shape (to use the Geographic Information System terminology), unique and P.O. Box ZIP codes are typically represented geographically by a "point" with no land area. Because residential measures like population density or median household income are important explanatory variables in the analysis of ZIP-code-level data, unique and P.O. Box ZIP codes pose a problem if they have no residents or land area. USPS ZIP codes also pose a problem for research because they can split, merge, or be renumbered over time, and there is no comprehensive, public listing of all changes over multiple years.

Because USPS ZIP codes do not correspond to Census blocks, tracts, or other geographic designations, the Census created ZIP code Tabulation Areas (ZCTAs) in 2000, which are groupings of Census blocks that approximate USPS ZIP codes. Most USPS unique and P.O. Box ZIP codes do not correspond to a Census ZCTA; most USPS standard ZIP codes do. The Census reports data for ZCTAs but not for USPS ZIP codes.

FCC Form 477 data report broadband provider count data for a proprietary ZIP code approximation that is based on USPS ZIP codes: the "TANA Inc./GDT Inc. Dynamap/ZIP code Boundary and Inventory Files," produced by TeleAtlas, a mapping company, for use with GIS software. These ZIP codes are not identical to either USPS ZIP codes or Census ZCTAs.

Research requires using a single geographic definition that is consistent over time and common to all data sources, in order to look at the same geographic area. It was therefore necessary to create a correspondence between (1) all USPS ZIP codes that appear in the NETS or Forrester data during the years 1992–2006, (2) all ZIP code designations that appear in the FCC data during the years 1999–2006, and (3) Census 2000 ZCTAs. Because ZCTAs do not change over time and because they all cover a geographic area, we chose ZCTAs as our common geography and used the following steps described below to convert USPS ZIP codes, which are reported in NETS and Forrester data, and FCC ZIP codes over all years, to 2000 ZCTAs.

First, we used the NETS to identify all ZIP code changes and create a consistent ZIP code list over the time period 1992–2006. Without accounting for ZIP code redefinitions, employment changes will be incorrectly measured. For instance, if ZIP code A is split into two equally sized new ZIP codes, half of which is still called A and half is named B, then one needs to be sure not to misinterpret this as the movement of jobs from a declining neighborhood to a new, booming neighborhood. Fortunately the NETS allows an easy method for tracking ZIP code re-numberings, merges, and splits. Because the database is longitudinal, we can observe whether an individual establishment's ZIP code changes over time. A change in ZIP code could either reflect an establishment actually moving to a location with a different ZIP code or it could reflect the establishment remaining at an address where the ZIP code changed.[12] Using the NETS, we looked at all establishments that existed over the entire period of 1992–2006 and identified ZIP code changes based on whether (1) the plurality of establishments in

[12] Looking at street address changes could distinguish between actual moves and ZIP code changes, but matching millions of string variables (street addresses) is computationally demanding, and there are many misspellings and alternative spellings of street addresses in the database.

a 1992 ZIP code reported a different ZIP code in 2006, or (2) the plurality of establishments in a 2006 ZIP code reported a different ZIP code in 1992. With this set of changes, we grouped together ZIP codes that appeared to be re-numbered, split, or merged over this time period. Of all ZIP codes for which the NETS reports some employment in 1992 or 2006, 7 percent of the ZIP codes were assigned to a ZIP code group of two or more ZIP codes. For instance, among establishments existing in 1992 and 2006, the vast majority of establishments with a ZIP code of 02146 in 1992 (Brookline, MA, just west of Boston) had ZIP codes of 02445 or 02446 in 2006. And the vast majority of establishments in ZIP codes 02445 and 02446 in 2006 were in ZIP code 02146 in 1992. A closer look at the NETS shows a sharp drop in establishments and employment between 1997 and 1998 in ZIP code 02146 and a correspondingly sharp rise in establishments and employment in ZIP codes 02445 and 02446 at the same time. Our algorithm groups ZIP codes 02146, 02445, and 02446 together in the same ZIP code group, and this ZIP code group is a much closer approximation to the same geographic area over time than any one of those ZIP codes individually. In this manner, we constructed a set of consistent ZIP codes and ZIP-code groupings for the time period.

Second, we created a correspondence between USPS ZIP codes and Census ZCTAs using ESRI maps in ArcGIS. To do this, we overlaid ZIP code and ZCTA maps using GIS software. We did this first for standard polygon ZIP codes and then for point P.O. Box and unique ZIP codes. For standard ZIP codes, we have ESRI polygon ZIP code maps for 1999–2006, except 2001, and a polygon map of 2000 ZCTAs. For each year, we combined that year's ZIP code map with the ZCTA map to determine, for each ZIP code, how much of its area is in which ZCTAs. We then used Census 2000 block group population data to weight these areas and determine, for each ZIP code, how much of its population is in which ZCTAs. We then combined this information across ZIP code groups—created using the NETS in the previous step—and across years to match each ZIP code group to a best match ZCTA. Each ZIP code group can only match to one ZCTA, though many ZIP code groups can match to the same ZCTA. For point ZIP codes, the method is very similar. We have ESRI point ZIP code maps for 1999–2006, excluding 2001, and the polygon ZCTA map. For each year, we again combined that year's ZIP code map with the ZCTA map; but since the ZIP codes are points, there was no need to calculate shares of ZIP codes in different ZCTAs; all of the ZIP code is necessarily in a single ZCTA. We then determined best matches as with polygon ZIP codes. Having matched ZIP code groups to best match ZCTAs separately for polygon and point ZIP code groups, we combined the results into a single ZIP code-ZCTA best match correspondence. With this method, 97–99 percent of ZIP codes in the NETS, depending on the year, were matched to ZCTAs; these successful ZIP code matches accounted for more than 99.99 percent of employment in every year. Our ZIP code-ZCTA correspondence also covered more than 99.5 percent of the ZIP codes that appear in any year in the FCC Form 477 data.

Third, we used several methods to assign the few still-unmatched ZIP codes from the NETS and FCC to ZCTAs. For ZIP codes that appear in the NETS, we have the street address, city, and state for most businesses in those ZIP codes, and we geocoded those establishments using both ESRI's ArcGIS software and the Google Maps geocoding service. Overlaying the geocoded establishment points with the ZCTA polygon map allowed us to assign some of the hitherto unmatched ZIP codes to the ZCTA that the greatest share of establishments in a ZIP code or a ZIP code group (from the first step) matched to. For ZIP codes that remained unmatched to a ZCTA, we checked the place name associated with businesses in those ZIP codes; ZIP codes for which all place names correspond to the same ZCTA (according to the Mable/Geocorr engine) were matched that way. After all these steps, only a handful of ZIP codes remained unmatched, and we assigned these last few ZIP codes to ZCTAs manually by looking up street names and places in ArcGIS and identifying the nearest ZCTA.[13] With these final matches, we arrived at a correspondence between ZIP codes and ZCTAs that allowed us to assign every ZIP code appearing in the NETS or the FCC data to a Census 2000 ZCTA. [14]

Analytical Approach

Our analysis uses three baseline empirical specifications:

1. ZCTA-level employment changes based on business location, using NETS data.
2. County-level household labor market outcomes, including employed residential population, total population, working-age population, the employment rate, median income, and average pay per employee, using Census and other data.
3. Individual-level analysis of changes in telecommuting and operating a home-based business, using Forrester data.

Each specification relates the dependent variable to the change in the number of broadband providers, from the FCC Form 477 data.

ZCTA-Level Employment

The first specification looks at the relationship between employment growth and broadband availability.

$$(1)\quad ln\left(\frac{emp_i^{t+1}}{emp_i^t}\right) = \alpha + \beta \ln(BB_i^{t+1} - BB_i^t + 1) + \Phi X_i^t \ln(BB_i^{t+1} - BB_i^t + 1) + \Psi X_i^t + \Lambda Z_i^t + \mu ln\left(\frac{emp_i^t}{emp_i^{t-1}}\right) + \tau \ln(BB_i^t - BB_i^{t-1} + 1) + \varepsilon_i$$

The dependent variable is employment growth, and BB refers to the number of broadband providers in a ZCTA at a point in time. The independent variable of interest is the change in broadband providers. Employment growth in ZCTA *i* between time *t* and time *t*+1 is a function of the change in the number of broadband providers in ZCTA *i* and ZCTA-level controls, some of which (the Xs) are interacted with the change in broadband providers and others of which (the Zs) are not. Lagged values of employment growth and broadband expansion are also included as controls.

Time Period

We focus on the time period 1999–2006 for several reasons. The FCC reports ZIP code provider counts starting in 1999, and Forrester surveys about home-based work are available starting in 2001. The NETS covers the entire time period 1992–2006, and Census/BLS data on employment, pay, income, and population are available for years prior to 1999. Focusing on the 1999–2006 period allows us to use prior trends in employment growth from the NETS. Furthermore, this period is more relevant than earlier years (1992–1999) as a guide to what broadband expansion induced by current policies means for economic development. The relationship between broadband (or any technology) and economic outcomes could change over the course

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[13] We used this manual process for all unmatched ZIP codes in the NETS with at least five establishments in any one year. We chose to ignore ZIP codes that were unmatched by our algorithm and that had fewer than five establishments in every year because some of these ZIP codes might have been data entry errors in the NETS and because the manual matching process was time-consuming. These ZIP codes account collectively for at most 2,000 employees in any one year out of total national employment of well over 100 million.
[14] See Census documentation on the relationship between ZCTAs and ZIP codes at www.census.gov/geo/ZCTA/zcta.html. Also, Gillett et al. (2006) offer a detailed explanation of reconciling the FCC and Census ZIP code definitions.

of the technology's diffusion.[15] Policies designed to bring broadband to still-underserved areas are at the end of the diffusion process of broadband at current broadband speeds, so the most recent historical experience is a better guide to what might happen in the future than earlier historical experience.

Because the FCC began reporting provider counts at the ZIP code level in 1999, we assume the number of broadband providers in 1992 to be zero in all ZCTAs, so the level of providers in 1999 is assumed to equal the change in providers from 1992 to 1999.[16] This assumption will allow us to control for earlier trends in broadband expansion, check for a lagged effect of broadband on employment growth, and check for whether broadband expansion in the earlier period appeared to anticipate later employment growth.

We also use this specification for the 1992–1999 period to assess how broadband expansion in its early stages related to economic development. But with t=1992 and t+1=1999, we cannot control for lagged employment growth because the NETS begins in 1992.

Broadband Measure

We use the log of the change in broadband providers to proxy for broadband availability. In a related working paper, Kolko (forthcoming) infers broadband availability at the ZIP code level using FCC provider counts and Forrester broadband adoption data, showing a monotonic but non-linear relationship between the number of broadband providers and the extent of broadband availability in the ZIP code. The log form reflects this non-linear relationship better than either a binary form, which reflects whether a ZCTA has at least one broadband provider, or a linear form, which assumes the effect of the marginal provider on availability is uniform regardless of the number of existing providers. We add one to the change in providers so as not to drop from the logged value those ZCTAs where the number of providers stayed constant between t and $t+1$.

Control Variables

Controls include factors (the Xs) that could influence the relationship between broadband provision and growth. ZCTA-level population density, ZCTA-level median household income, and the log of metropolitan area population (or county population for ZCTAs outside metropolitan areas) are included in the Xs; these interactions are potentially important from a policy perspective since lower-income and rural areas are less well-served by broadband providers and are the main focus of efforts to close the digital divide. County-level education attainment—measured as the percent of adults ages 25 years or older with a bachelor's degree—is also included to assess whether broadband availability is a complement for the skill level of the local workforce. Because ZCTAs are almost always smaller than a labor market, and firms draw their workforce not just from within their ZCTA but from the local labor market, we use county-level rather than ZCTA-level education. Finally, we include interactions to assess whether broadband encourages businesses or households to locate in places that are desirable: These measures

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[15] It is ambiguous whether the economic effects, theoretically, might be bigger earlier or later in the diffusion process. Early adopters, by taking advantage of a technology first, could grow to a scale that is hard for later adopters to compete with. However, the cost of adopting a new technology can fall over time with technological improvements and knowledge from lessons learned by the earlier adopters, which could make economic benefits higher for later adopters.

[16] The FCC reports the number of high-speed lines in use (i.e., broadband subscribers, residential and business) back only to December 1999, when there were 2.5 million, compared with 82.5 million in December 2006 and 132.8 million in June 2008. Kolko (2000) reports that the initial growth of commercial Internet usage began around 1994: In January of that year, only 6,653 ".com" Internet domain names were registered, compared with over one million four years later. Forman, Goldfarb, and Greenstein (2009) also argue that commercial Internet usage most likely did not affect local economic activity before 1995. Gillett et al. (2006) note that few communities had broadband before December 1996 (p. 14).

include (1) a county-level climate index from the USDA that captures mild temperatures, sunny winters, and dry summers; and (2) a county-level measure of the share of housing that is vacation or seasonal homes, reflecting how recreation-oriented a place is.

Additional controls include several measures used in the urban growth literature to predict local economic growth. One control is a shift-share measure intended to predict employment growth between t and $t+1$, calculated as the weighted average of 3-digit NAICS industry growth (indexed by j) in the rest of the country (i.e., excluding ZCTA i), where the weights are industry shares in ZCTA i at time t. $Emp_{i,j}$ equals employment in ZCTA i in industry j, and emp_i equals employment in ZCTA i across all industries.

$$predictedgrowth_i^{t \to t+1} = \sum_j \left(\frac{\sum_{k \neq i} emp_{k,j}^{t+1}}{\sum_{k \neq i} emp_{k,j}^{t}} * \frac{emp_{i,j}^{t}}{emp_i^{t}} \right)$$

Intuitively, this predicted growth measure reflects the level of employment growth in ZCTA i if each industry within the ZCTA grew at the same rate as in the rest of the country. In addition, we include the diversity of ZCTA employment as measured by the similarity of the ZCTA's industry mix to that of the nation, and we include average establishment size in the ZCTA. [17] These employment variables are all generated from the NETS database. We also include the density of major roads in the ZCTA as a proxy for transportation costs: Areas with better transportation could grow faster, and since broadband infrastructure often follows existing transportation rights-of-way, road density, and broadband availability could also be positively correlated. Models for the 1999–2006 period include further controls (in the Zs) for the log of broadband providers in 1999 and 1992–1999 employment growth. The lagged growth variable captures the prior trend in employment growth: If past ZCTA-level growth is positively correlated with future growth due to a pre-existing growth trend, and if broadband providers expand service in areas exhibiting persistent growth, then omitting lagged growth would bias the relationship between broadband expansion and employment growth upward.

Weighting, Samples, and Instrumental Variable Strategy

We report results weighted by employment in ZCTA i at time t. ZCTAs have no inherent economic meaning (they don't approximate a labor market, for instance) and no political meaning, so there's no justification for giving them all equal weight, particularly since many ZCTAs have very little employment. Standard errors are clustered at the county level to reflect likely correlation of errors between nearby ZCTAs.

In addition to looking at overall employment growth for all ZCTAs in each time period, we also look at growth in specific industries or sectors; growth due to relocation, expansion and contraction, and births and deaths of establishments; and growth over the period 1999–2006 in ZCTAs with 0 or 1–3 providers in 1999. This set of ZCTAs excludes the early adopters of broadband and better represents the unserved and underserved areas that are the target of government programs to support broadband infrastructure.

In an improvement over earlier work on broadband and economic development, we investigate the causality of the relationship using an instrumental variable strategy. Reverse causality is an obvious concern: Plausible

[17] Glaeser et al. (1992) and Henderson, Kuncoro, and Turner (1995) use diversity measures in their classic studies of local industry growth. Glaeser et al. (1992) also include average establishment size as a measure of competition as another explanatory factor affecting local industry growth.

as it may be that improved broadband availability leads to economic growth, it is also plausible that local economic growth—either past or forecasted—gives providers an incentive to make broadband more widely available. A good candidate instrument must be correlated with broadband availability without being independently correlated with economic growth, aside from its indirect effect through the relationship between broadband availability and economic growth.

As an instrument for broadband availability, we use the average slope of the land in ZCTA *i*. Broadband providers face higher costs to extend service in areas with steeper terrain.[18] In the results section, below, we confirm that slope and provider count have a statistically significant, negative relationship over the period 1999–2006, holding constant other explanatory variables used in the model. The exclusion restriction holds if slope has no direct effect on economic growth independent of its relationship with broadband availability. Several possibilities arise as to why slope might affect economic activity outside of an indirect effect on broadband availability. First, workers might value steeper terrain as an amenity offering views or recreational activities, so they would accept lower wages and raise the level of employment in steeper areas, and slope would be positively correlated with growth. To handle this potential source of bias, we control for two amenity measures—climate and share of housing that is vacation homes: Mountainous areas have drier climates, which enters positively into the climate index, and mountain areas that are high-amenity due to recreational opportunities (e.g., skiing) would have a higher share of housing as vacation homes. Second, steeper terrain might raise transportation costs, lowering the level or growth of employment relative to flatter areas. To handle this potential source of bias, we control for road density as a proxy for transportation costs. Third, slope could affect the economic base: Steeper areas tend to be farther from coasts, where transportation, warehousing, and other goods-related industries often cluster. Slope could also be correlated with the location of natural resource industries such as mining or forestry. We control for the local industry mix using the shift-share predicted-growth measure, which captures local employment growth differences due to exogenous national shifts in the growth rates of local industries.[19]

County-Level Household Outcomes

For household outcomes, we use specification (1), but with the county as the unit of analysis rather than the ZCTA. We examine several labor market measures: employment from the NETS, and employed population, total and working-age population, the employment rate (employed population divided by working-age population), median household income, and average pay per employee from the Census Bureau and the Bureau of Labor Statistics. Employed population (Census) refers to the number of county residents who are employed anywhere, whereas employment measures (NETS) reflect employment by workplace location, not employee residence. Whereas the most geographically disaggregated analysis possible—here, ZCTAs—is appropriate for business location because businesses need broadband infrastructure at their exact location, county-level analysis is more appropriate for household outcomes because counties better approximate a household's labor market.

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[18] Personal communication with Mark Guttman of CostQuest, a consultancy whose practice includes modeling the costs and benefits to telecommunications firms of providing broadband service, August 15, 2008. Prieger (2003) notes that the FCC's Hybrid Cost Proxy Model considers terrain as a factor that influences the cost of providing local telecommunications services. The Government Accountability Office (2006) reports that broadband providers, state regulators, and other stakeholders said "infrastructure build-out can be difficult in mountainous and forested areas because these areas may be difficult to reach or difficult on which to deploy the required equipment. Conversely, we were told that "flat terrain constitutes good geography for telecommunications deployment" (p.19).

[19] Kolko (forthcoming) shows that broadband subscription prices as reported by households are uncorrelated with the number of broadband providers in a ZIP code, so the relationship between slope and broadband availability is not driven by prices broadband providers charge.

The dependent variable is expressed as the log of the ratio between the value at times *t* and *t*+1. For the change in broadband availability, we use the employment-weighted average of ZCTA-level availability for ZCTAs in county *c*. The Xs include county-level education, median income, population density, metropolitan area population, climate, and housing share of vacation homes—the same variables as in specification (1). Instead of the predicted employment growth, average establishment size, and industrial diversity measures in specification (1), which are predictors specifically of employment growth, here the Zs include 2-digit NAICS sector shares, a more flexible specification for looking at a wider range of labor market outcomes.[20] As before, when looking at the 1999–2006 period, we include in the Zs the 1992–1999 change in the outcome variable and the 1999 provider count. Road density, like broadband provider count, is the employment-weighted average for ZCTAs in the county. For the 1992–1999 regressions, we include prior trends of the dependent variable from 1979 or 1980 to 1990 or 1992, depending on data availability.

Individual-Level Home-Based Work

Here we use individual-level regressions, based on Forrester survey data, of telecommuting, having a home-based business, bringing work home, and employment.

(2) $outcome_s^t = \alpha + \beta \ln(BB_i^t + 1) + \Phi X_i^t \ln(BB_i^t + 1) + \Psi X_i^t + \Lambda Z_s^t + \varepsilon_s$

Unlike specification (1), where the dependent variable is a change, here in specification (2) the dependent variable is the level of an outcome variable for individual *s*, who lives in ZCTA *i* at time *t*. Forrester data are matched to the ZCTA-level broadband provider count data and other ZCTA controls in X, which—as before—include ZCTA-level household income and population density, county-level educational attainment, climate score, housing share of vacation homes, and metropolitan area population. Here, the additional control variables—the Zs—include individual age, education, race and ethnicity, family structure variables, and household income and assets—all in categories using saturated sets of dummies. The Forrester data are an unbalanced panel: Many respondents participate in multiple years of the survey, though typically in just two or three years. We estimate model (3) with person-ZCTA fixed-effects in order to difference out time-invariant observed and unobserved individual characteristics, thus estimating for a given person the effect of the change in broadband provider count in their ZCTA on the likelihood of each outcome. The person-ZCTA fixed effects model identifies the relationship only off of households in the same location in multiple years (though including movers as well turns out not to affect the results). Also included are year dummies to capture general economic conditions and slight variations in survey-question wording in a couple of years. The outcomes include employment, having a formal telecommuting relationship with an employer, informally bringing work home, and having a home-based business. Each is a binary variable.

[20] For outcomes other than employment growth, sector share controls are more appropriate than the predicted employment growth control used in model (1). When employment growth is the outcome, the predicted employment growth variable has the advantage of being constructed from 3-digit NAICS industries and, in effect, controls for detailed industry mix with a minimal loss of degrees of freedom.

Regression Results

Broadband Availability

Detailed regression results are presented in Tables A1 through A7. Before examining the effect of broadband on economic outcomes, the first results examine factors associated with increased broadband availability, as measured by the ZCTA-level provider count changes. Because broadband investment involves fixed costs that are shared among subscribers, factors influencing the number of potential subscribers—such as household income and residential density—should be correlated with broadband providers.

Broadband availability in 2006—equal to the change in broadband providers over the 1992–2006 period—is positively correlated with residential income and education levels, both of which raise the demand for broadband, as well as road density. The correlation between ZCTA road and population density is 0.83; omitting road density results in a positive, statistically significant coefficient on population density. Finally, slope is negatively correlated with broadband availability, consistent with the expectation that broadband infrastructure buildout is more expensive on steeper terrain.[21]

Because slope will be the instrument for broadband expansion, Table A1 both describes the factors associated with broadband availability and presents the first-stage results for the instrumental variable estimation. Columns 2 through 5 are the first-stage, where the dependent variable is the increase in broadband providers over the period 1999–2006; recall that the analysis focuses on this time period, rather than the entire 1992–2006 period from column (1). The coefficient on slope is negative and statistically significant, and although the p-value for the F-test is well-below .05, the F-statistic is small relative to alternative specifications. The relationship between slope and broadband expansion in 1999–2006 is quite sensitive to the weighting by ZCTA employment, as the comparison with column (3) shows: Unweighted, the magnitude of the slope coefficient is 2.5 times larger, and the F-statistic is much larger. As explained above, because ZCTAs have no inherent economic or political meaning, we prefer to weight by employment throughout the analysis, though this comes at the cost of some first-stage power because the ZCTAs with steep terrain and less broadband expansion in 1999–2006 tend to have low employment. The coefficient on slope becomes less sensitive to whether the regression is weighted by removing ZCTAs with low and high employment: Looking only at ZCTAs with 1999 employment between 200 and 7000—roughly the 20th to 80th percentiles of ZCTAs by employment—the coefficient on slope is similar and the F-statistic is high regardless of weighting (see columns 4 and 5). Another issue is that slope is not a valid instrument for broadband expansion for the 1992–1999 period because it is uncorrelated with the 1999 provider count in the weighted model (column 6) and *positively* related to broadband expansion in the unweighted model (column 7). However, the 1999–2006 period is more relevant for broadband policy and is the focus of the analysis.

[21] Flamm (2006), GAO (2006), Grubesic (2003), Gillett and Lehr (1999) and others also find greater broadband availability in higher-density and higher-income areas. Prieger (2003) reports no statistically significant effect of income on broadband availability when education and numerous other controls are included.

TABLE A1
Broadband providers and ZCTA characteristics (first stage for 2SLS)

Dependent variable: change in broadband provider count; OLS

	(1) 1992–2006	(2) 1999–2006	(3) 1999–2006	(4) 1999–2006	(5) 1999–2006	(6) 1992–1999	(7) 1992–1999
Slope (ZCTA)	-0.00398***	-0.00434***	-0.0114***	-0.00961***	-0.0119***	0.000212	0.00447***
	(0.00114)	(0.00162)	(0.000850)	(0.000955)	(0.000908)	(0.00127)	(0.000927)
Road density (ZCTA)	9.416***	5.539**	3.576	-6.613**	-5.615*	22.07***	15.14***
	(1.582)	(2.449)	(2.869)	(2.947)	(2.929)	(2.810)	(2.339)
Percent bachelor's degree (county)	0.629***	0.683***	0.514***	0.255**	0.211**	0.946***	0.524***
	(0.126)	(0.160)	(0.105)	(0.106)	(0.0945)	(0.169)	(0.114)
Log population density (ZCTA)	0.00311	0.0118	0.0278***	0.0312***	0.0200***	-0.00145	0.0554***
	(0.00532)	(0.00756)	(0.00493)	(0.00621)	(0.00543)	(0.00786)	(0.00555)
Log median HH income (ZCTA)	0.0759***	-0.0155	0.0196	0.0245	0.0855***	0.233***	0.192***
	(0.0159)	(0.0205)	(0.0158)	(0.0217)	(0.0202)	(0.0248)	(0.0192)
Log population (MSA/county)	-0.00915*	-0.0144**	-0.00335	-0.00860	0.000538	0.0350***	0.0107**
	(0.00532)	(0.00692)	(0.00489)	(0.00560)	(0.00478)	(0.00793)	(0.00462)
Broadband change (1992–1999)		-0.264***	-0.301***	-0.236***	-0.277***		
		(0.0249)	(0.0104)	(0.00979)	(0.00867)		
Employment growth (1992–1999)		0.146***	0.0812***	0.113***	0.0882***		
		(0.0305)	(0.0102)	(0.0138)	(0.0118)		
Weighted by employment?	Yes	Yes	No	Yes	No	Yes	No
ZCTAs included	All	All	All	Employment 200–7000	Employment 200–7000	All	All
Observations	26717	26721	26721	15210	15210	26717	26717
R-squared	0.479	0.268	0.445	0.239	0.279	0.527	0.503
F-test of slope=0	12.09	7.12	178.42	101.29	172.91	0.03	23.24
p-value for F-test of slope=0	0.0005	0.0076	0	0	0	0.86	0

NOTES: Each column represents a separate regression. Slope and road density from 2003 ESRI shape files. Percent bachelor's degree, population density, household income, and metropolitan/county population measured in 2000 for 1999–2006 regressions and in 1990 for 1992–1999 regressions. Additional controls included in all columns: predicted employment growth for the time period covered by the dependent variable, road density, climate, vacation home share, competition, and diversity, all as described in the text. All regressions report robust standard errors clustered on county. *=10% level sig.; **=5% level sig.; ***=1% level sig.

Broadband and Overall Local Employment Growth

The analysis of broadband and employment growth—following model (1) above—appears in Tables A2 through A5. All of these results include controls for road density, educational attainment, population density, household income, metropolitan (or county) population, climate desirability, and housing share of vacation homes; three additional controls for employment growth are included as well: predicted growth based on the ZCTA industry mix and national industry growth, the inverse of average establishment size, and the diversity of the local industry mix. Prior employment growth and prior broadband expansion in 1992–1999 are included when employment growth 1999–2006 is the dependent variable, unless otherwise noted. All are weighted by start-year ZCTA employment, unless otherwise noted.

The baseline specification is a regression of employment growth in 1999–2006 on the change in broadband providers 1999–2006 (Table A2, column 1). To interpret the magnitude of the coefficients, keep in mind that a one-unit change in the log of the difference in providers corresponds roughly to an increase from zero to 1–3 providers or from 1–3 providers to 4 providers; a two-unit change corresponds roughly to an increase from zero to 7 providers. A one-unit change is associated with employment growth that is 6 percentage points higher over the seven-year period, which corresponds to the coefficient of 0.0636 in column 1. Standardizing this coefficient, a one-standard-deviation change in broadband corresponds to a 0.085 standard deviation change in employment.[22] The insignificant coefficient on broadband expansion in 1992–1999 indicates that there are not long lags in the relationship between broadband expansion and employment growth, controlling for the contemporaneous broadband expansion. Restricting the model to ZCTAs with zero or 1–3 providers in 1999, the 1999–2006 broadband expansion coefficient remains positive, significant, and of similar magnitude (column 2). Since this set of ZCTAs excludes the early broadband adopters, this coefficient represents the relationship between broadband and employment growth in the types of areas targeted by public broadband policy.

The relationship between broadband expansion and employment growth in 1992–1999 is positive, statistically significant, and smaller than in the later period: 0.0350 in column 3 versus 0.0636 in column 1. Although the 1992–1999 model excludes prior employment growth as a control since the NETS data start in 1992, the exclusion of prior growth in the 1999–2006 model leaves the relationship unchanged (not shown); the 1999–2006 model also showed no significant lagged effect of broadband expansion.

The final two columns of Table A2 begin to explore the causality of the relationship between broadband expansion and employment growth. One possible mechanism for reverse causality is that population growth could encourage both broadband provision—since residential customers are the bulk of broadband subscribers—and employment growth, since some industries serve local populations. Column 4 includes county population growth in 1999–2006 as a control.[23] Although employment growth and broadband expansion are measured at the ZCTA level, most businesses serving local populations probably have a larger customer footprint than a ZCTA, which in urban areas is often a small

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[22] The standard deviation of the 1999–2006 provider count change is 0.358, and the standard deviation of employment growth 1999–2006 is 0.268—both weighted by 1999 ZCTA employment.
[23] County population for intercensal years are Census Bureau annual estimates, based on numerous administrative data sources.

neighborhood and averages roughly 10,000 people.[24] Including population growth lowers the coefficient on broadband expansion by roughly one-fifth, remaining statistically significant: Population growth does not account for most of the relationship between employment growth and broadband expansion. The industry-level analysis in the next section explores this further.

The last point from Table A2 is that there is no relationship between employment growth in the later period and broadband expansion in the earlier period. Having already discounted the possibility of a long lagged effect of broadband expansion 1992–1999 on employment growth 1999–2006 from column 1, the lack of relationship between later employment growth and earlier broadband expansion in column 5 suggests that broadband expansion was not greater in areas with future employment growth. Although this does not establish the direction of causality between broadband expansion and employment growth, it is evidence that broadband expansion did not occur in anticipation of (accurately predicted) later employment growth.

TABLE A2
Employment growth and broadband, 1992–1999 and 1999–2006

Dependent variable: employment growth (NETS); OLS

	(1) Employment, 1999–2006	(2) Employment, 1999–2006	(3) Employment, 1992–1999	(4) Employment, 1999–2006	(5) Broadband, 1992–1999
BB provider change, 1999–2006	0.0636***	0.0601***		0.0499***	
	(0.0119)	(0.0180)		(0.0118)	
BB provider change, 1992–1999	0.0105	-0.00214	0.0350***	0.00773	
	(0.0101)	(0.0163)	(0.0126)	(0.00974)	
Population growth, county, 1999–2006				0.394***	
				(0.0647)	
Employment growth, 1999–2006					-0.00938
					(0.0193)
ZCTAs included	All	0 or 1–3 Providers in 1999	All	All	All
Observations	26721	23760	26717	26720	26717
R-squared	0.254	0.267	0.152	0.270	0.527

NOTES: Each column represents a separate regression. All columns include controls: predicted employment growth for the time period covered by the dependent variable, road density, percent bachelor's degree, population density, household income, and metropolitan/county population, climate, vacation home share, competition, and diversity, all as described in the text. Employment growth 1992–1999 included as a control for regression with 1999–2006 employment as dependent variable. Interactions are reported at the means of the control variables. Note that some 1990 control variables could not be generated for 4 ZCTAs using the consistent-tract-matching method described in the text. All regressions report robust standard errors clustered on county. *=10% level sig.; **=5% level sig.; ***=1% level sig.

[24] The Census has not published population estimates or any other data for ZCTAs after 2000.

Broadband and Employment Growth across Industries and Places

Table A3 presents results for employment growth in specific industries and due to different employment dynamics, as well as for establishment growth. For the period 1999–2006, broadband expansion is positively associated with establishment growth as well as employment growth, while average establishment size is negative.[25] Across employment dynamics, broadband expansion has a positive, statistically significant relationship with growth due to births and deaths and expansions and contractions in 1999–2006. The magnitude of the coefficient in the cross-ZCTA moves regression is the smallest of the employment dynamics, suggesting that the broadband-growth relationship is not driven by the relocation of jobs from lower broadband areas to higher broadband areas.[26]

The relationship between broadband expansion and employment growth varies by industry. Table A3 presents the coefficients from separate regressions of growth in each sector on broadband expansion and the full set of controls. Column 1 does not include the control for county population growth and therefore corresponds to Table A2, column 1; column 2 includes the county population growth variable and therefore corresponds to Table A2, column 4. The relationship is strongest for utilities; information; finance and insurance; professional, scientific, and technical services; management of companies and enterprises; and administrative and business support services. In these sectors, the same increase in broadband availability is associated with at least 12 percentage point higher employment growth. The relationship was not statistically significant at the 5 percent level in mining and public administration. With the population growth variable, the relationships with growth in manufacturing, educational services, and arts, entertainment, and recreation become insignificant.

As described in the main text, the sectors whose growth is more strongly associated with broadband expansion are generally those that are more technology-intensive. We measure technology-intensity of industries in two ways: inputs and occupations. The input-based measure equals the ratio of the value of technology inputs to the value of industry output, where technology inputs include Internet publishing, telecommunications services, data processing, and related services (the output of NAICS industries 516–518). These data come from the 2002 Input-Output Use Tables, published by the Bureau of Economic Analysis. The occupation-based measure equals the share of industry workers in "computer specialist occupations" (Standard Occupational Code 15-1000, Bureau of Labor Statistics); these include programmers, software engineers, and database and network administrators, but not workers involved in physical computer assembly or telecommunications installation and repair. As Table 4 in the main text shows, there is considerable overlap in the two measures. The correlation across sectors is 0.72.

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[25] The relationship between average establishment size and broadband expansion is positive in 1992–1999 (not shown in the tables), suggesting that the benefits of broadband might have favored larger firms earlier and smaller firms more recently, which could arise if early adopters of a technology face higher fixed costs of adoption whereas the cost of adopting the technology falls as the technology diffuses, raising the relative benefit to smaller firms.

[26] The cross-ZCTA migration employment change in 1999–2006 has the smallest magnitude of the three dynamics with standardized betas as well. The standard deviation of employment growth over the period is 0.17 for births minus deaths, 0.12 for expansions minus contractions, and 0.10 for migration. Employment growth for these dynamics is measured as the net employment change for the period divided by total ZCTA employment in the start-year. Cross-ZCTA migration is more common than cross-state migration, which Kolko and Neumark (2007) found to be minimal. The weaker relationship for migration does not, by itself, rule out the possibility that the growth associated with broadband expansion is a zero-sum game: Broadband expansion could theoretically change the geographical distribution of establishment births without changing the overall national level.

TABLE A3
Employment growth in specified establishments, 1999–2006

Employment from NETS; all results OLS

	(1) Coefficient	Standard Error	(2) Coefficient	Standard Error
Employment growth in all establishments	0.0636***	(0.0119)		
Establishment growth	0.0547***	(0.00995)		
Average establishment size change	-0.0377***	(0.0101)		
EMPLOYMENT GROWTH IN:				
Establishment births and deaths	0.0441***	(0.00667)		
Establishment expansions and contractions	0.0112***	(0.00435)		
Establishment cross-ZCTA moves	0.00616*	(0.00315)		
Sectors not tied to population (see below)	0.0899***	(0.0166)		
Sectors tied to population (see below)	0.0626***	(0.0105)		
INDUSTRY SECTORS (NAICS code):				
Agricultural, forestry, fishing, and hunting (11)	0.116***	(0.0422)	0.0918**	(0.0431)
Mining (21)	0.0657	(0.0590)	0.0339	(0.0604)
Utilities (22)	0.167***	(0.0592)	0.147**	(0.0601)
Construction (23)	0.118***	(0.0226)	0.0957***	(0.0229)
Manufacturing (31–33)	0.0625**	(0.0252)	0.0384	(0.0243)
Wholesale trade (42)	0.0708***	(0.0204)	0.0474**	(0.0213)
Retail trade (44–45)	0.0654***	(0.0157)	0.0443**	(0.0172)
Transportation and warehousing (48–49)	0.0862***	(0.0283)	0.0609**	(0.0287)
Information (51)	0.120***	(0.0236)	0.0995***	(0.0252)
Finance and insurance (52)	0.148***	(0.0241)	0.117***	(0.0236)
Real estate and rental and leasing (53)	0.102***	(0.0192)	0.0717***	(0.0198)
Professional, scientific, and technical services (54)	0.164***	(0.0193)	0.139***	(0.0182)
Management of companies and enterprises (55)	0.408***	(0.0765)	0.341***	(0.0763)
Administrative and business support services (56)	0.141***	(0.0272)	0.110***	(0.0265)
Educational services (61)	0.0610***	(0.0218)	0.0423*	(0.0227)
Health care and social assistance (62)	0.0736***	(0.0204)	0.0513***	(0.0198)
Arts, entertainment, and recreation (71)	0.0566**	(0.0259)	0.0367	(0.0275)
Accommodation and food services (72)	0.0993***	(0.0182)	0.0773***	(0.0208)
Other services (81)	0.0707***	(0.0167)	0.0497***	(0.0170)
Public administration (92)	0.00538	(0.0403)	-0.0163	(0.0397)
Control for county population growth 1999–2006?	No		Yes	

NOTES: Each ROW and each pair of columns presents the coefficient estimate and standard error from a regression of employment growth on contemporaneous broadband expansion and the basic set of controls, as described in the notes to Table A2. Controls also include employment growth 1992–1999 and employment share 1992 for the specified sector, in the industry-sector regressions.

TABLE A4
Employment growth and broadband: heterogeneous effects

Dependent variable: employment growth (NETS); OLS

	(1) 1999–2006	(2) 1999–2006	(3) 1992–1999
Broadband provider change, 1999–2006	0.0576***	0.0588***	
	(0.0116)	(0.0226)	
Broadband provider change, 1992–1999	0.00740	0.00106	0.0416***
	(0.01000)	(0.0162)	(0.0109)
Broadband provider change (1999–2006 for cols. 1 & 2) interacted with:			
Share bachelor degree (county)	0.103	0.161	0.326***
	(0.132)	(0.226)	(0.116)
Log population density (ZCTA)	-0.0314***	-0.0364***	-0.0105**
	(0.0101)	(0.0104)	(0.00490)
Median HH income (ZCTA)	0.0352	0.0636	0.00324
	(0.0291)	(0.0449)	(0.0244)
Log population (MSA or county)	0.0193*	0.0242*	-0.0262***
	(0.0114)	(0.0132)	(0.00589)
Climate desirability index	0.00514	0.00409	-0.000404
	(0.00341)	(0.00535)	(0.00312)
Share vacation homes (county)	0.0279	-0.0573	-0.366***
	(0.0964)	(0.101)	(0.122)
ZCTAs included	All	0 or 1–3 Providers in 1999	All
Observations	26721	23760	26717
R-squared	0.258	0.273	0.163

NOTES: Each column represents a separate regression. Main effect of broadband, plus interactions, shown in table. Main effects of the interaction variables are not shown but included in all models, as well as controls for road density, predicted employment growth, competition, and diversity, as described in the text. Employment growth 1992–1999 included as a control for regression with 1999–2006 employment as dependent variable. Interactions are reported at the means of the control variables. Education, density, income, and population measured in 2000 for 1999–2006 regressions and in 1990 for 1992–1999 regressions. All regressions report robust standard errors clustered on county. *=10% level sig.; **=5% level sig.; ***=1% level sig.

A third description of sectors is the extent to which the geographic distribution of their employment follows that of population, measured as the sum of absolute values of the differences between a ZCTA's share of national sector employment and a ZCTA's share of population (a standard dissimilarity index). The lowest values—sectors where the geographic distribution of employment most closely mimics the distribution of population—are retail, construction, and "other services," which consists largely of personal services. Those whose employment is most closely tied to population do not. This is further evidence that the relationship between broadband expansion and employment growth is not primarily driven by population growth; if it were, we would expect to see the strongest relationship between broadband expansion and employment growth in the sectors where growth is most tied to population growth.

Grouping together the sectors whose locations are most tied to the population—retail (NAICS 44–45) and consumer services (NAICS 6, 7, and 8)—moving from 0 to 1–3 broadband providers (or equivalent increases in the log change) is associated with a 6.3 percentage point increase in employment growth. (See Table A3.)

For sectors whose locations are less tied to the population distribution—manufacturing (NAICS 3), wholesale trade (NAICS 42), and finance, information, real estate, and business services (NAICS 5)—broadband availability is associated with a 9.0 percentage point increase in employment growth.[27] The difference between these estimates is somewhat informative as a lower bound of the effect of broadband if the relationship between broadband and employment in retail and consumer services were entirely due to exogenous population growth that caused both broadband expansion and employment growth. However, this difference is only a lower bound. Even if broadband does not cause employment growth in retail or consumer services directly, a positive effect of broadband on employment growth in business services, manufacturing, and other industries could induce population growth (people follow jobs) and therefore indirectly cause some employment growth in retail and consumer services.

Up to this point, the analysis has not considered heterogeneous effects of broadband on employment growth based on ZCTA characteristics. Including interactions between the broadband provider count and ZCTA-, county-, and metropolitan-level variables reveals that broadband has a stronger relationship with employment growth in some areas than others. Table A4 shows that broadband expansion is associated with higher employment growth in lower-density ZCTAs, both in the later period (columns 1 and 2) and in the earlier period (column 3). The interaction coefficients in column 1 imply that a ZCTA with density at the 90th percentile—with all other characteristics at their means—would exhibit no statistically significant relationship between broadband expansion and employment growth. However, density and metropolitan population are positively correlated, and a ZCTA with density at the 90th percentile and metropolitan population typical for a high-density ZCTA would still exhibit a positive, statistically significant relationship between broadband expansion and employment growth.[28] The negative, statistically significant coefficient on the population density interaction is consistent with the claim that electronic communication, which broadband facilitates, can substitute for face-to-face communication, which is more prevalent and lower-cost in higher density areas.

The county education attainment interaction is statistically significant for the period 1992–1999 only, suggesting that the effect of broadband is a complement with general worker skills in fostering employment growth for areas where broadband was adopted earlier; we return to this finding below in the discussion of household outcomes.

Does Broadband Expansion Cause Employment Growth?

Results discussed above in this appendix and in the main text were suggestive evidence of causality in the direction of broadband expansion leading to employment growth. In Table A2, we showed that earlier broadband expansion does not appear to anticipate later employment growth. Also, including population growth as a control did little to change the relationship between broadband expansion

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[27] For these estimates only, I omit sectors whose locations are strongly tied to the availability of natural resources or geographic features. Their geographic distribution often diverges from the population distribution, but that does not mean they are more "footloose" industries in the way that many business services are. I also omit public administration.

[28] The estimated coefficient for density at the 90th percentile and other interacted variables at their means is 0.0089 with a standard error of 0.014; the estimated coefficient for density at the 90th percentile and metropolitan area population at the 75th percentile is 0.0340 with a standard error of 0.014, which is statistically significant at the 5% level. The correlation between log ZCTA population density and log metropolitan area (or county) population is 0.73.

and employment growth, making us discount the most plausible alternative to broadband expansion causing employment growth, which was that both could be caused by population growth.

To assess causality in the contemporaneous positive relationship between broadband expansion and employment growth, we instrument for broadband expansion using slope and present the second stage results in Table A5. Recall from the first-stage results in Table A1 that slope is a valid instrument for broadband only in 1999–2006 and is a stronger instrument when the model is unweighted. Yet, we prefer weighting all models in this report because ZCTAs lack inherent economic or political meaning, and ZCTAs—unlike Census tracts, which are designed to have roughly uniform population—vary hugely in population and employment. Accordingly, the second-stage results are highly sensitive to weighting. The weighted, full-sample 2SLS estimate in Table A5, column 1, is positive, significant, but implausibly high: 0.636 is ten times the OLS estimate, and the standard error is twenty times that of the OLS estimate. The unweighted 2SLS estimate is essentially zero. Following the strategy in the first stage, we repeat the second-stage estimation, limiting the sample to ZCTAs with a narrower distribution of employment.[29] Columns 3 and 4 include only ZCTAs with employment between 200 and 7000, and—as in the first stage—the second-stage results are somewhat less sensitive to weighting. Weighted, the estimate with this restricted set is 0.325; the unweighted estimate is 0.127. Both estimates remain well above the OLS estimates for these samples, which are reported in the last row of the table.

TABLE A5
Employment growth and broadband, 1999–2006, second stage

Dependent variable: employment growth (NETS); 2SLS

	(1) Employment, 1999–2006	(2) Employment, 1999–2006	(3) Employment, 1999–2006	(4) Employment, 1999–2006
BB provider change, 1999–2006	0.636***	0.00925	0.325***	0.127***
	(0.238)	(0.0447)	(0.0649)	(0.0485)
BB provider change, 1992–1999	0.162**	0.00797	0.0835***	0.0515***
	(0.0692)	(0.0147)	(0.0183)	(0.0152)
Weighted by employment?	Yes	No	Yes	No
ZCTAs included	All	All	Employment 200–7000	Employment 200–7000
Observations	26721	26721	15210	15210
R-squared		0.150	0.130	0.165
BB provider change, 1999–2006 (OLS)	0.0636***	0.0566***	0.0823***	0.0654***
	(0.0119)	(0.00645)	(0.0103)	(0.00762)

NOTES: Each column represents a separate regression. All columns include controls: predicted employment growth for the time period covered by the dependent variable, road density, percent bachelor's degree, population density, household income, and metropolitan/county population, climate, vacation home share, competition, diversity, and employment change 1992–1999, all as described in the text. All regressions report robust standard errors clustered on county. *=10% level sig.; **=5% level sig.; ***=1% level sig.

[29] Excluding ZCTAs with little employment in an unweighted regression seems an appropriate balance between not giving undue influence to ZCTAs of little economic importance and choosing a model that gives slope more power as an instrument for broadband expansion.

The 2SLS results are suggestive—though by no means definitively—of a causal effect of broadband on employment growth. Aside from the unweighted full sample model, the 2SLS results are positive and statistically significant although the estimates balloon upward relative to the OLS estimates. As argued above, the most plausible source of upward bias in the 2SLS estimate would arise from slope being positively correlated with transport costs, transport costs being negatively correlated with employment growth, and slope being negatively correlated with broadband expansion over the 1999–2006 period: Thus, the portion of broadband expansion variation explained (positively) by flatness could bias the 2SLS upward, since flat land would have lower transport costs and therefore faster growth. But road density is included as a control, which reduces the possibility of this upward bias.

It is also possible that endogeneity could bias the OLS estimates downward during this time period since broadband expansion in 1999–2006 is negatively correlated with broadband expansion in 1992–1999 (see Table A1, columns 2–5). Areas expected to have higher employment growth might have induced more broadband expansion in 1992–1999 if broadband expanded first in the places that were expected to be most profitable. Broadband expansion in 1999–2006 would therefore have been in places where slower growth was anticipated, leading to a downward endogeneity bias. This would be consistent with the higher 2SLS estimates, though the magnitudes of the 2SLS estimates are still unconvincingly high.[30]

Broadband and Household Outcomes

Employment growth is only one of many possible economic effects of broadband. If broadband increases economic output in a ZCTA, households' employment and income outcomes could change throughout the labor market area. We now turn to county-level relationships between broadband availability and economic outcomes, following specification (2) above. Table A6 presents results for employment in businesses located in the county (from the NETS), employment among county residents, total population, the working-age population, the share of working-age adults employed, average pay per employee, and median household income. Employment in county businesses can differ from employment among county residents since some employees work at businesses outside their county of residence. For each outcome variable, Table A6 reports the coefficient estimate on contemporaneous broadband expansion for 1999–2006—both for all counties and restricted to counties with on average 2 or fewer providers in 1999—and for 1992–1999.[31]

The effect on employment from the NETS at the county level is positive, significant, and only slightly smaller in magnitude than the ZCTA-level analysis for 1999–2006: 0.0534 versus 0.0636 in Table A2, column 1, with a bigger gap for the county-level and ZCTA-level results for counties with low broadband availability in 1999. Turning to household outcomes, during 1999–2006 broadband expansion is associated with larger increases in employed residents (Census), population, and the working-age

[30] The 2SLS results for sector employment growth (corresponding to the OLS results in Table A3) and for interactions with local characteristics (corresponding to the OLS results in Table A4) both had even higher standard errors relative to OLS and coefficient estimates with even less plausible magnitudes. While endogeneity is a concern not just for overall employment growth but also for other specifications, it appears that slope is not a sufficiently strong instrument to yield meaningful results for these other specifications.

[31] In the county model, the broadband provider count is a weighted average of the county's component ZCTAs. Since the ZIP code level provider count of 1–3 is coded as 2, the county-level cutoff of 2 or fewer corresponds to the ZCTA-level cutoff from earlier tables of zero and 1–3.

population, but no statistically significant change in the employment rate or average pay.[32] Broadband expansion is associated with a decrease in household income despite no change in average pay per employee. The primary differences for counties with low broadband availability in 1999 (column 2) is that broadband expansion has no statistically significant effect on population employed and a negative relationship with the employment rate, significant at the 10 percent level.

TABLE A6
Broadband and household labor market outcomes

Dependent variable is change in: (all results OLS)	(1) 1999–2006	(2) 1999–2006, county broadband provider count <=2	(3) 1992–1999
Employment (NETS)	0.0534***	0.0288**	0.0367**
	(0.0111)	(0.0121)	(0.0160)
Employed residents (Census)	0.0422***	0.0171	0.0654***
	(0.0131)	(0.0135)	(0.0121)
Population	0.0166**	0.0130**	0.0376***
	(0.00742)	(0.00542)	(0.00948)
Working-age population	0.0247***	0.0182***	0.0446***
	(0.00839)	(0.00628)	(0.0102)
Employment rate (Census)	-0.0118	-0.0143*	0.0199**
(employed residents/working age pop.)	(0.00947)	(0.00754)	(0.00796)
Average pay per employee	0.0110	0.0119	0.0488***
	(0.00846)	(0.00846)	(0.00846)
Median income	-0.0242**	-0.0166***	0.0499***
	(0.00984)	(0.00581)	(0.00954)
N (for all regressions in column)	3086	2772	3086

NOTES: The unit of observation is the COUNTY. Each CELL represents a separate regression. Shown is the coefficient on broadband provider count change for 1999–2006 (columns 1 and 2) or 1992–1999 (column 3). For income and poverty rate, column (3) represents the change 1989–1999. All regressions include controls for county-level road density, education, income, metro area (or county) population, the climate index, vacation home share, and employment shares in each 2-digit NAICS industry. Controls for 1999–2006 regressions include broadband change 1992–1999; controls for all regressions include lagged growth of the dependent variable, with the exception of the regression for NETS employment 1992–1999 since the NETS database begins in 1992. All regressions report robust standard errors. *=10% level sig.; **=5% level sig.; ***=1% level sig.

[32] As noted above, the effects on employment and employed residents could differ because the employment measure from the NETS is based on place of employment, and the employed-residents measure from the Census is based on place of residence. Some people commute across county lines and therefore would be included in different counties for employment in the NETS and employed residents in the Census. In counties with relatively little cross-county commuting – where more than 2/3 of employed residents work in the county and more than 2/3 of employment is county residents –the coefficient on employment (NETS) is 0.0384 and on employed residents (Census) is 0.0398, both statistically significant and nearly identical to each other. For counties where fewer than 2/3 of employed residents work in the county or where fewer than 2/3 of employment is employed residents, the coefficient on employment is 0.0752 (t-stat=3.7) and the coefficient on employed residents is 0.0194 (t-stat=0.99). This suggests that the employment growth due to broadband expansion in counties with cross-commuting does not necessarily benefit residents and is consistent with an elastic labor supply.

The main difference between the later period and the earlier period is that broadband expansion in 1992–1999 is associated with a positive and statistically significant increase in the employment rate, average pay per employee, and median household income. The relationship with employment (NETS) is positive and significant in both periods. As discussed in the main text, in the earlier years of broadband, computer-literate workers might have been in scarce supply, so employers paid more for the same skills earlier that they did in later years; furthermore, early adopters might have demanded more advanced skills if, to integrate a nascent technology into their business processes, these early adopter businesses had to develop more applications in-house, whereas later adopters could rely more on off-the-shelf mass-market applications that workers with more modest technology skills could use. The stronger complementarity in the earlier period between broadband and higher-skilled workers, relative to the later period, is consistent with the positive interaction between county education level in 1992–1999 and broadband expansion, in Table A4, column 3, which could also reflect a greater benefit of broadband to businesses with access to higher-skilled labor.

The last outcomes in the analysis are individual-level measures of telecommuting, bringing work home, and having a home-based business. Changes in the number of broadband providers have no effect on the likelihood of telecommuting, bringing work home, or having a home-based business (Table A7, first row). There is similarly no statistically significant effect for any of these outcomes when restricting the sample to people in ZCTAs with zero or 1–3 providers in 1999 (Table A7, second row). Even when restricting the sample to respondents with a bachelor's degree (row three) or in managerial or professional occupations (row four), who might be the respondents whose work is most conducive to working independently at home, broadband expansion has no relationship with changes in the likelihood of doing any of the home-based work activities. The likelihood of working for an employer, however, has a positive and statistically significant relationship with the number of broadband providers (Table A7, column 4), except for respondents in managerial or professional occupations. One cannot compare this with the lack of relationship between broadband expansion and the employment rate estimates at the county level in Table A6, for two reasons. First, the geographic level of analysis is different, so broadband expansion in one's own ZCTA might have different effects on household employment than broadband expansion in one's own county. Second, the individual-level analysis controls for individual characteristics and conditions on being in the same ZCTA in multiple years: since broadband expansion was associated with both employment and population growth (Table A6), it may be that longer-term residents, who are overrepresented in the Forrester sample with fixed individual-ZCTA effects, have a better employment outcomes than newcomers who constitute the population growth that is also associated with broadband expansion.

TABLE A7
Working at home and broadband, 2001–2006

Dependent variable (Forrester data): (all results OLS, respondent-ZCTA fixed effects)	(1) Telecommute	(2) Bring work home	(3) Have home-based business	(4) Work for an employer
Independent variable is log broadband providers; regression includes:				
All respondents	-0.000625	0.00451	0.00107	0.0144***
	(0.00157)	(0.00324)	(0.00268)	(0.00346)
Respondents in ZCTAs < 3 broadband providers in 1999	-0.000748	0.00414	0.00168	0.0112***
	(0.00170)	(0.00363)	(0.00312)	(0.00409)
Respondents with a bachelor's degree	0.00107	0.00824	0.000739	0.0103**
	(0.00323)	(0.00671)	(0.00463)	(0.00514)
Respondents in managerial or professional occupations	-0.000304	0.00511	-0.00365	0.00571
	(0.00337)	(0.00723)	(0.00488)	(0.00543)

NOTES: Each CELL represents a separate regression. For the full sample, N=166179; number of groups=68579. All regressions include controls (using saturated dummies) for individual- or household-level age, education, income, assets, household size, age of children, race, ethnicity, and year fixed-effects. All regressions include respondent-ZCTA fixed effects; only respondents participating in multiple years of the survey and reporting the same ZCTA contribution to the estimation. Respondents-ZCTAs that appear in only one annual Forrester survey are dropped from the count of observations and number of groups. Main effects of the interaction variables are differenced out because they are constant across years for each respondent-ZCTA observation.

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