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IS FASTER BETTER?

QUANTIFYING THE RELATIONSHIP BETWEEN BROADBAND SPEED AND ECONOMIC GROWTH

Abstract: In this BULLETIN, I aim to quantify the relationship between higher broadband speeds (10 Mbps versus 25 Mbps) and the growth rates in important economic outcomes in U.S. counties including jobs, personal income, and labor earnings. Doing so exposes the potential for severe selection bias in studies of broadband's economic impact, which is addressed in this study using Coarsened Exact Matching. Once balanced, the data reveal no economic payoff from the 15 Mbps speed difference between the years 2013 and 2015. I also revisit the Crandall, Lehr and Litan (2007) study on broadband's effect on employment to evaluate the possible impacts of selection bias, and conclude that the positive benefits of broadband reported in that particular study are likely spurious. The selection bias problem may infect other studies on the economic impacts of broadband Internet services.

I. Introduction

High-speed Internet connectivity (i.e., broadband) is seen as essential for modern life, whether consuming video and audio entertainment, interacting through social media, obtaining healthcare and education services, and a host of other activities. Many of these uses produce sizable private benefits, but significant social payoffs from broadband access have policy makers, and even private companies, across the globe seeking ways to encourage broadband availability and access.[1] Any effort to promote broadband access presumes the policymaker knows what broadband is, who has it and who does not, and what sorts of companies and technologies might get subsidized to expand availability and subscriptions. Before a policy to promote broadband can be implemented and administered, broadband must be defined.

In the United States, the definitional inquiry turns up in a number of significant places. Section 706(b) of the Telecommunications Act of 1996 requires the Federal Communications Commission ("FCC") to conduct an annual inquiry to determine "whether advanced telecommunications capability is being deployed to all Americans in a reasonable and timely fashion."[2] In reaching its determination, the FCC today defines *wireline* "broadband" using an upstream/downstream standard of 25/3 Mbps.[3] This 25/3 Mbps definition of "broadband" is not universally applied, however. To determine whether broadband providers are eligible for Connect America Fund ("CAF") subsidies to provide service in high-cost, rural areas, the agency set a 10/1 Mbps "broadband" standard, presumably to reduce subsidy obligations by introducing lower-speed, lower-cost technologies in the acceptable technology mix.[4] The lower

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1 T.R. Beard, G.S. Ford, and L.S. Spiwak, *Private Solutions to Broadband Adoption: An Economic Analysis*, 69 FEDERAL COMMUNICATIONS LAW JOURNAL 1-23 (2017) (available at: http://www.fclj.org/wp-content/uploads/2017/04/69.1.1-T.-Randolph-Beard-et-al.pdf).

2 47 U.S.C. § 1302(b).

3 *In the Matter of Inquiry Concerning the Deployment of Advanced Telecommunications Capability to All Americans in a Reasonable and Timely Fashion, and Possible Steps to Accelerate Such Deployment Pursuant to Section 706 of the Telecommunications Act of 1996, as Amended by the Broadband Data Improvement Act*, 2015 BROADBAND PROGRESS REPORT AND NOTICE OF INQUIRY ON IMMEDIATE ACTION TO ACCELERATE DEPLOYMENT, FCC 15-10, 30 FCC Rcd 1375 (rel. February 4, 2015) (available at: https://apps.fcc.gov/edocs_public/attachmatch/FCC-15-10A1.pdf) (hereinafter "*2015 Broadband Progress Report*"); *see also In the Matter of Inquiry Concerning the Deployment of Advanced Telecommunications Capability to All Americans in a Reasonable and Timely Fashion, and Possible Steps to Accelerate Such Deployment Pursuant to Section 706 of the Telecommunications Act of 1996, as Amended by the Broadband Data Improvement Act,* 2016 BROADBAND PROGRESS REPORT, FCC 16-6, 31 FCC Rcd 699 (rel. January 29, 2016) (hereinafter "*2016 Broadband Progress Report*") (available at: https://apps.fcc.gov/edocs_public/attachmatch/FCC-16-6A1.pdf). In effect, Section 706 grants the Commission self-determination regarding its relevance in regulating broadband markets. Regulators regulate, so the implications of being both judge and jury are predictable and often controversial. G.S. Ford and L.J. Spiwak, *Justifying the Ends: Section 706 and the Regulation of Broadband*, 16 JOURNAL OF INTERNET LAW 1 (January 2013) (available at: http://www.phoenix-center.org/papers/JournalofInternetLawSection706.pdf). On the Federalist implication of the FCC's use of Section 706, *see* L.J. Spiwak, *Federalist Implications of the FCC's Open Internet Order*, PHOENIX CENTER PERSPECTIVE No. 11-01 (February 8, 2011) (available at: http://phoenix-center.org/perspectives/Perspective11-01Final.pdf). The scope of the Commission's Section 706 authority is subject to significant legal debate. *See, e.g.*, L.J. Spiwak, *What Are the Bounds of the FCC's Authority over Broadband Service Providers? – A Review of the Recent Case Law,* 18 JOURNAL OF INTERNET LAW 1 (2015); L.J. Spiwak, *USTelecom and its Aftermath*, PHOENIX CENTER POLICY BULLETIN NO. 42 (June 2017) (available at: http://www.phoenix-center.org/PolicyBulletin/PCPB42Final.pdf). Discussion of the extent of that legal authority is beyond the scope of this paper, however.

4 *In the Matter of Connect America Fund ETC Annual Reports and Certifications Petition of USTelecom for Forbearance Pursuant to 47 U.S.C. § 160(c) from Obsolete ILEC Regulatory Obligations that Inhibit Deployment of Next-Generation*

threshold appears to be improving broadband deployment in rural areas.[5] Recently, the Commission considered a 10/1 Mbps definition of broadband for mobile wireless services for its Section 706 analysis.[6] To date, the Commission has not set a speed threshold for what constitutes "mobile broadband service." Individual U.S. states define "broadband" using myriad "speed" levels, most of which fall below the FCC's 25/3 Mbps standard.[7]

In most instances, the FCC's definition of "broadband" for its Section 706 analysis is irrelevant to actual economic outcomes—it is an unbinding constraint. There is no financial impact to a provider that fails to satisfy the standard set in the Section 706 analysis and no provider, to my knowledge, bases deployment decisions on the FCC's threshold.[8] Still, the FCC's definition of broadband in its Section 706 reports is controversial. Some broadband advocates fear that a 10/1 Mbps standard might leave rural Americans "behind economically."[9] In contrast, other industry commentators discount the spread, noting: (a) "[t]he difference between a 10 and a 25 Mbps connection is marginal, affecting how long a large download may take or how many

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Networks, REPORT AND ORDER, FCC 14-190, 29 FCC Rcd 15,644 (rel. December 18, 2014) (available at: https://apps.fcc.gov/edocs_public/attachmatch/FCC-14-190A1.pdf) at ¶ 2; *Justifying the Ends*, *supra* n. 3 at pp. 3-4.

5 B. Ayvazian, *Analyst Angle: 4G LTE leveraged for fixed wireless broadband in rural communities*, RCR WIRELESS (June 6, 2017) (available at: https://www.rcrwireless.com/20170606/analyst-angle/20170606wireless4g-lte-leveraged-for-fixed-wireless-broadband-in-rural-communities-tag10); *AT&T Launches Fixed Wireless Internet in Additional Counties in Mississippi to Better Serve Select Rural and Underserved Areas*, PNRNEWSWIRE (October 25, 2017) (available at: https://www.prnewswire.com/news-releases/att-launches-fixed-wireless-internet-in-additional-counties-in-mississippi-to-better-serve-select-rural-and-underserved-areas-300543224.html).

6 *2016 Broadband Progress Report, supra* n. 3 at ¶ 3 ("We retain our existing speed benchmark of 25 Mbps download/3 Mbps upload (25 Mbps/3 Mbps) for fixed services, but find that the current record is insufficient to set an appropriate speed benchmark for mobile service."); *In the Matter of Inquiry Concerning Deployment of Advanced Telecommunications Capability to All Americans in a Reasonable and Timely Fashion,* THIRTEENTH SECTION 706 REPORT NOTICE OF INQUIRY, FCC 17-109, 32 FCC Rcd 7029 (rel. August 8, 2017) (available at https://apps.fcc.gov/edocs_public/attachmatch/FCC-17-109A1.pdf).

7 Alabama, for instance, defines "broadband" as a service of 200 Kbps or greater. Ala. Code §37-2A-2. Colorado defines broadband as a service of 4 Mbps or greater. Colorado Revised Statutes §40-15-102(3.7).

8 The 10/1 Mbps standard for CAF subsidies is binding.

9 R. Souza, *Defining Down the Definition of Internet 'Service' Will Hurt Rural Economy,* PORTLAND PRESS HERALD (Oct. 12, 2017) (available at: http://www.pressherald.com/2017/10/12/maine-voices-defining-down-the-definition-of-internet-service-will-hurt-rural-economy). *See, also,* S. Lohr, *FCC Sharply Revises Definition of Broadband,* NEW YORK TIMES (January 29, 2015) (https://bits.blogs.nytimes.com/2015/01/29/f-c-c-sharply-increases-definition-of-broadband); *2015 Broadband Progress Report, supra* n. 3 at ¶ 2 ("sufficient" broadband capability will "drive American economic growth."); K. Finley, *Redefining 'Broadband' Could Slow Rollout in Rural Areas,* WIRED (August 30, 2017) (available at: https://www.wired.com/story/redefining-broadband-could-slow-rollout-in-rural-areas); K. McCarthy, *FCC Taps the Brakes on Fudging US Broadband Speed Amid Senator Fury*, THE REGISTER (September 6, 2017) (available at: https://www.theregister.co.uk/2017/09/06/fcc_extends_comment_on_broadband_fudge).

concurrent high-definition streams a household can run;"[10] (b) "most people don't often need speeds of hundreds of megabits. Two HD TV stream and plenty of surfing fit easily into 12 megabits;"[11] and (c) "[t]here aren't any common apps that require 25 Mbps."[12] Empirical evidence on the broad economic impacts of "speed" is scarce and often not well-matched to the present policy debate.[13] Whether there is an "economic payoff" from higher speeds is interesting and policy-relevant, but the equation remains largely unanswered.

Is there a payoff from a move from 10 Mbps to 25 Mbps? In this BULLETIN, I seek to fill the void in empirical analysis on broadband speeds and answer that very question.[14] To do so, I link county-level data on broadband speeds from the National Broadband Map ("NBM") to data on economic outcomes—including jobs, earnings, and total personal income—from the U.S. Bureau of Economic Analysis ("BEA"). Using this data, I test whether the growth rates in these important economic outcomes are larger for areas with higher-speed broadband services. This analysis offers two important, policy-relevant findings. First, under current conditions, there appears to be no broad economic payoff from higher-speed connections, at least when that difference is between download speeds of 10 Mbps and 25 Mbps and during the period 2013 through 2015.

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10 D. Brake, *A Policymaker's Guide to Rural Broadband Infrastructure*, Information Technology and Information Foundation (April 2017) at p. 8 (available at: http://www2.itif.org/2017-rural-broadband-infrastructure.pdf).

11 D. Burstein, *Hundreds of Thousands Go Wireless Only in Tokyo*, FASTNET (October 23, 2017) (available at: http://fastnet.news/index.php/97-fnn/592-hundreds-of-thousands-go-wireless-only-in-tokyo). *See also, The Dawn of 5G: Will Wireless Kill the Broadband Star*, FORBES (September 22, 2017) (available at: https://www.forbes.com/sites/washingtonbytes/2017/09/22/the-dawn-of-5g-will-wireless-kill-the-broadband-star/#32f8ae43fd7f) ("The 25 Mbps 'Wheeler Standard' looks like cooking the books. There aren't any common apps that require 25 Mbps."); H.G. Thompson Jr. and C. Garbacz, *Economic Impacts of Mobile Versus Fixed Broadband*, 35 TELECOMMUNICATIONS POLICY 999-1009 (2011) ("mobile broadband has an important direct effect on GDP, but fixed broadband has an effect no different than zero."); R. Kenny and C. Kenny, *Superfast Broadband: Is It Really Worth a Subsidy?*, 13 INFO 3-29 (2011) ("the authors conclude that the conventional wisdom that FTTH will bring substantial economic and societal benefits and therefore deserves a subsidy is, at best, much overstated.").

12 FORBES WASHINGTON BYTES (moderated by H. Singer), *The Dawn of 5G: Will Wireless Kill the Broadband Star*, FORBES (September 22, 2017) (quoting Richard Bennett) (available at: https://www.forbes.com/sites/washingtonbytes/2017/09/22/the-dawn-of-5g-will-wireless-kill-the-broadband-star/#32f8ae43fd7f).

13 A. Grimes, C. Ren and P. Stephens, *The Need for Speed: Impacts of Internet Connectivity on Firm Productivity*, 37 JOURNAL OF PRODUCTIVITY ANALYSIS, 187-207 (2012) (available at: https://ideas.repec.org/p/mtu/wpaper/09_15.html); I.K. Rohman and E. Bohlin, *Does Broadband Speed Really Matter for Driving Economic Growth? Investigating OECD Countries*, Unpublished Manuscript (2012) (available at: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2034284); Kenny and Kenny, *supra* n. 11.

14 For a more general analysis on speed, *see, e.g.*, I.K. Rohman and E. Bohlin, *Does Broadband Speed Really Matter for Driving Economic Growth? Investigating OECD Countries* (April 4, 2012) (available at: https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2034284); A. Grimes, C. Ren, and P. Stephens, *The Need for Speed: Impacts of Internet Connectivity on Firm Productivity*, 37 JOURNAL OF PRODUCTIVITY ANALYSIS 187-207 (2012).

Notably, my analysis does not evaluate the economic payoff of broadband more generally, or the payoff of technological advances that do much more than simply increase broadband speeds (e.g., 5G wireless).[15] Second, the empirical analysis reveals severe "selection bias," in that counties with higher-speed broadband are wholly unlike those with lower-speed broadband. Broadband services and upgrades *are not* randomly distributed, but rather such activity is systematic, presumably based on supply- and demand-side conditions. When conducting studies on broadband's economic impact, a strong caution is warranted: *a failure to grant selection bias fair weight could result in misleading inferences about broadband's economic impact*. As a demonstration, I revisit the early and frequently cited study by Crandall, Lehr, and Litan (2007) and find biased estimates of broadband's economic impact.[16] While I speculate other studies may suffer from the same bias, I do not (and cannot) conclude that there is no evidence of an economic payoff from broadband Internet service more generally.

This BULLETIN in outlined as follows: In the next section, I present my empirical model. Coarsened Exact Matching is used to address profound covariate imbalance between counties with mostly 25 Mbps service and those with mostly 10 Mbps service. Accounting for the differences between counties, I show there is no effect on economic outcomes from higher broadband speeds, at least for the outcomes evaluated over the sample period. In the following section, I provide additional evidence from a pseudo-treatment, where a statistical test is conducted to see if a future treatment affects past outcomes—a sure sign of confounding. For the original, unbalanced county data, the pseudo-treatment is statistically significant when it should not be, suggesting biased estimates of the treatment effect when ignoring selection bias. Next, I apply to the pseudo-treatment method to the Crandall, Lehr, and Litan (2007) study, and likewise find the pseudo-treatment is statistically significant, discrediting the results from that earlier study. Conclusions and policy recommendations are at the end.

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15 *See, e.g.,* Accenture Strategies, SMART CITIES (January 2017) (available at: https://newsroom.accenture.com/content/1101/files/Accenture_5G-Municipalities-Become-Smart-Cities.pdf) (estimating that deploying the next generation of high-speed 5G wireless networks could create up to three million jobs and add approximately $500 billion to U.S. GDP through direct and indirect potential benefits).

16 R. Crandall, W. Lehr, and R. Litan, *The Effects of Broadband Deployment on Output and Employment: A Cross-Sectional Analysis of U.S. Data*, THE BROOKINGS INSTITUTE: ISSUES IN ECONOMIC POLICY (July 2007) (available at: https://www.brookings.edu/research/the-effects-of-broadband-deployment-on-output-and-employment-a-cross-sectional-analysis-of-u-s-data).

II. Empirical Model

Is there a broad economic *societal* payoff from increasing broadband speeds from 10 Mbps to 25 Mbps, or are the benefits mostly *private* in nature (e.g., faster movie downloads)?[17] Almost all of the Internet's existing activities can be accomplished with 10 Mbps speeds or less. The FCC's own analysis indicates that social media, streaming audio, VoIP call, general browsing, email, even high-definition personal video calls can be done with less than 2 Mbps downloads speeds.[18] A 10 Mbps connection can handle most online education courses.[19] According to the Commission, and nearly every analysis of broadband speeds, the activities benefitting most from higher speeds is the consumption of video entertainment (either one- or two-way).[20] Certainly, higher speeds are helpful when it comes to data-heavy video content, but even so a 10 Mbps connection permits the streaming of multiple high-definition video streams.[21] Downloading a 4 GB high-definition video takes just under a minute to download on a 10 Mbps connection, but only a quarter-minute on a 25 Mbps connection.[22] Such a difference may prove a minor nuisance

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17 Recent evidence suggests consumers are willing to pay substantially more for higher speeds, though at a diminishing rate. *See, e.g.,* Y. Liu, J. Prince, and S. Wallsten, *Distinguishing Bandwidth and Latency in Households' Willingness-to-Pay for Broadband Internet Speed*, TECHNOLOGY POLICY INSTITUTE (August 2017) (available at: https://techpolicyinstitute.org/wp-content/uploads/2017/08/Distinguishing-Bandwidth-and-Latency-in-Households-Willingness-to-Pay-for.pdf).

18 FCC Broadband Speed Guide (available at: https://www.fcc.gov/reports-research/guides/broadband-speed-guide).

19 C. Sabo, *Need for Speed: How Fast Does Your Internet Connection Need to Be for Online Courses?*, Digital Quad - College of DuPage (March 17, 2016) (available at: http://digitalquad.cod.edu/quicktips/need-for-speed-how-fast-does-your-internet-connection-need-to-be-for-online-courses).

20 *Id.*; T. Moton, *How Much Internet Speed is Right for You?*, YAHOO (February 13, 2013) (available at: https://www.yahoo.com/news/news/choose-the-best-internet-speed-for-you-224440280.html); *How Much Internet Speed Do You Need? Data Speeds Deciphered!*, LEAP FROG SERVICES (March 4, 2017) (available at: https://leapfrogservices.com/how-much-internet-speed-do-you-need-data-speeds-deciphered); S. Layton, *How to Decide What Internet Speed You Need*, NERDWALLET (January 11, 2017) (available at: https://www.nerdwallet.com/blog/utilities/how-to-decide-what-internet-speed-you-need); C. Hoffman, *Should You Pay More for a Faster Internet Connection?*, HOW-TO GEEK (July 20, 2017) (available at: https://www.howtogeek.com/217627/htg-explains-should-you-pay-more-for-a-faster-internet-connection); D. Reisenger, *How Much Internet Speed Should You Really Pay For?*, TOM'S GUIDE (October 6, 2017) (available at: https://www.tomsguide.com/us/internet-speed-what-you-need,news-24289.html).

21 An end-user's speed is only one of many determinants of streaming quality. *See, e.g.,* S. Gustin, *This is Why Netflix Just Got So Blazingly Fast*, TIME (April 15, 2014) (available at: http://time.com/62903/netflix-comcast-speed-boost).

22 http://www.download-time.com.

to users, but whether such savings are sufficiently important to have broader economic implications is unclear.

Given the present debate over defining broadband using speed thresholds, whether there are broad economic payoffs from an increase from 10 Mbps to 25 Mbps perhaps deserves some attention. Broadband deployment within geographic boundaries for which there is data is dichotomous, so the question I seek an answer to in this BULLETIN is whether areas with *mostly* 25 Mbps broadband connections fare better economically than do areas with *mostly* 10 Mbps broadband connections? I measure broadband using the National Broadband Map's ("NBM") deployment estimates as of December 2013, the last end-of-year data available.[23] Economic activity is measured using data from the Bureau of Economic Analysis ("BEA"), including: (a) the number of jobs; (b) total personal income; and (c) total labor earnings.[24] Economic activity is measured as the percentage change in an outcome of interest between 2015 and 2013, or the two years following the measurement date for broadband. All data are measured at the county level.[25] The specific empirical question is whether the "speed" of the stock of broadband in a county at the end of 2013 has an impact on economic growth over the next two years. Of course, there are many plausible empirical tests of the effects of speed.

Table 1. Definition of Treated and Control Groups

	25 Mbps	10 Mbps	Observations
Treated Group (Higher Speed)	> 80%		1,001
Control Group (Lower Speed)	< 20%	> 80%	629
Omitted			1,494

Counties are divided into three groups, only two of which are retained for study (see Table 1). First, there is the "treatment" group including counties with at least 80% coverage of 25 Mbps service (by any technology).[26] Second, there is the "control" group, defined as counties with at least 80% coverage of 10 Mbps service, but less than 20% of coverage of 25 Mbps service. These percentage thresholds for service levels produce a significant difference in speed across two groups while maintaining adequate sample sizes. Finally, any county not qualifying for the either

23 National Broadband Map data is available at: https://www.broadbandmap.gov/data-download.

24 Economic outcome data is available at: https://www.bea.gov/itable/iTable.cfm?ReqID=70&step=1#reqid=70&step=25&isuri=1&7022=12&7023=7&7024=non-industry&7001=712&7029=12&7090=70.

25 BEA data from Virginia does not match up exactly with that from the NBM, so a few counties in Virginia are excluded from the sample.

26 The availability of 25 Mbps presumes at least an equivalent level of 10 Mbps service.

the treatment or control group is dropped from the sample.[27] In the U.S., there are a total of 3,124 counties (or equivalents). Applying the definitions above, there are 1,001 counties in the treatment group and 629 counties in the control group, the sum of which accounts for about half (52%) of all counties. For the treatment group, the average level of 25 Mbps service is 92.5% (with 99.1% having 10 Mbps service), while for the control group 94.5% of persons have 10 Mbps service but only 3.5% have 25 Mbps service.

A. *Causal Effects and Selection Bias*

We do not have experimental data, but rather observational data on economic outcomes and broadband collected by various state agencies. Since broadband deployment is not a random process, careful attention must be given to selection bias. Here, selection bias is caused by the non-random process by which counties receive the high-speed treatment so that the control group is not a sound proxy for the treated group (absent the treatment) and the "treatment" and "outcome" may be related through observed and potentially unobserved confounding factors.[28]

As explained by Imbens and Wooldridge (2009), when estimating the causal treatment effect in observational studies is the researcher must be alter to two key concepts stemming from selection bias: (1) unconfoundedness (or the conditional independence assumption) and (2) covariate overlap (or common support).[29] Unconfoundedness implies that, conditional on

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[27] These dropped counties are not necessarily counties with very poor broadband, as they may have moderate shares of 25 Mbps service.

[28] In regression analysis, this latter problem appears as a correlation between the regression residual and the treatment variable. Deployment decisions based on profitability do not imply redlining. Rather, redlining is a discriminatory practice based on ethnic composition. *See*, *e.g.*, D. D'Rozario and J.D. Williams, *Retail Redlining: Definition, Theory, Typology, and Measurement*, 25 JOURNAL OF MACROMARKETING 175-186 (2005) ("Retail redlining is a spatially discriminatory practice among retailers, of not serving certain areas, based on their ethnic-minority composition, rather than on economic criteria, such as the potential profitability of operating in those areas (at p. 175)."); *Federal Fair Lending Regulations and Statutes: Fair Housing Act, Consumer Compliance Handbook*, Federal Reserve Board, (Undated) at p. 1 (available at: https://www.federalreserve.gov/boarddocs/supmanual/cch/fair_lend_fhact.pdf) ("Redlining is the practice of denying a creditworthy applicant a loan for housing in a certain neighborhood even though the applicant may otherwise be eligible for the loan. [] The prohibition against redlining does not mean that a lending institution is expected to approve all housing loan applications or to make all loans on identical terms. Denying loans or granting loans on more-stringent terms and conditions, however, must be justified on the basis of economic factors and without regard to the race, color, religion, national origin, sex, or marital status of the prospective borrowers or the residents of the neighborhood in which the property is located."); G.S. Ford, *Challenges In Using The National Broadband Map's Dat*a, PHOENIX CENTER POLICY BULLETIN NO. 27 (March 2011) (available at: http://www.phoenix-center.org/PolicyBulletin/PCPB27Final.pdf). Descriptive statistics from the original data indicate that a higher proportion of African-Americans are in the treated group.

[29] G.W. Imbens and J.M. Wooldridge, *Recent Developments in the Econometrics of Program Evaluation*, 47 JOURNAL OF ECONOMIC LITERATURE 5-86 (2009).

observed covariates X, the treatment assignment probabilities are independent of potential outcomes. If we have a sufficiently rich set of observable covariates, then regression analysis including the variables X leads to valid estimates of causal effects. Since the X must be observed to be included in the regression model, this approach is often referred to as *selection on observables*. It is difficult to know and impossible to test whether the observed and included X are sufficient to guarantee unconfoundedness (so the regression error and treatment are uncorrelated), though some guidance is available through pseudo-treatment tests (as applied later).

The second factor to consider for the measurement of the causal effect is covariate overlap, which Imbens and Wooldridge (2009) observe is, after unconfoundedness, the "main problem facing the analyst."[30] This condition implies that the support of the conditional distribution of X for the control group overlaps completely with the conditional distribution of X for the treatment group. That is, the covariate distributions for the treated and untreated groups are sufficiently alike, thereby lending credibility to the extrapolations between the groups inherent in regression analysis. If the untreated observations are very different demographically and geographically from the treated, then the projection will be a poor one, which by Equation (5) suggests a bias in the estimated treatment effect. To remedy the problem of inadequate covariate overlap, researchers often apply matching methods.[31] These techniques aim to discard observations outside the region of common empirical support, rendering the treated and control counties sufficiently similar across economically relevant factors.

A little algebra illustrates the general problem of selection bias.[32] Let the potential outcome (Y) for county i be written as,

$$Y_i(t) = g_t(X_i) = g_t(X_{i1},\ldots,X_{ik}), \tag{1}$$

where t = 0, 1 (no treatment, treatment), g_t is as unknown function translating a set of k county-specific determinants X_i into the outcome of interest. If a county receives the treatment, then its outcome is $Y_i(1)$, and the treatment effect is

$$TE_i = Y_i(1) - Y_i(0), \tag{2}$$

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30 *Id.* at 43.

31 *Id.*; J.D. Angrist and J. Pischke, MOSTLY HARMLESS ECONOMETRICS (2008).

32 Angrist and Pischke (2008), *id.*, at Chapter 3; S.M. Iacus, G. King. G. Porro, *Causal Inference without Balance Checking: Coarsened Exact Matching*, Working Paper (June 26, 2008) (available at: https://ssrn.com/abstract=1152391), later published *Causal Inference without Balance Checking: Coarsened Exact Matching*, 20 POLITICAL ANALYSIS 1-24 (2012) (available at: https://gking.harvard.edu/files/political_analysis-2011-iacus-pan_mpr013.pdf)

where $Y_i(0)$ must be estimated for counties receiving the treatment (it is unobservable) using outcomes from a control group. The true treatment effect for the county (indicated by the *) is

$$TE_i^* = Y_i(1) - Y_i^*(0)\,, \tag{3}$$

where $Y_i^*(0)$ is the actual outcome for county i had it not received the treatment (rather than an estimate of that outcome). Of course, Y^* cannot be observed, since the treatment is given. Substitution of Equation (3) into Equation (2) yields,

$$TE_i = [TE_i^* + Y_i^*(0)] - Y_i(0)\,, \tag{4}$$

which from Equation (1) yields

$$TE_i = TE_i^* + [g_0(\widetilde{X}_i) - g_0(X_i)], \tag{5}$$

or,

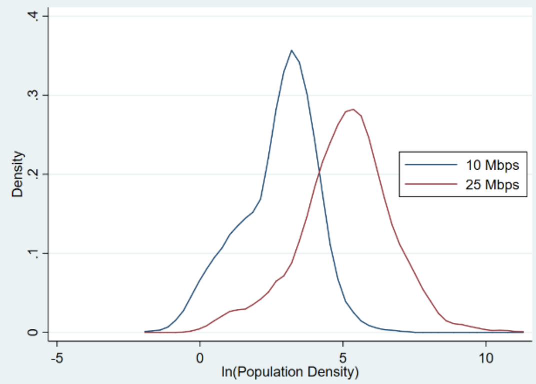
$$TE_i = TE_i^* + \varepsilon(\widetilde{X}_i, X_i)\,, \tag{6}$$

where $\widetilde{X}_i$ is the treated county's true X and X_i is from the control, and where $\varepsilon = Y_i^*(0) - Y_i(0)$ is the county-level treatment effect error. Equation (6) states that the estimated treatment effect for a county equals the *true* treatment effect plus an error term, ε. From Equation (4) or (5), this error equals the average difference in the *untreated* states (subscript 0) of those counties receiving the treatment and those that did not, the latter of which are used to project the outcome in the untreated state *for the treated*. These county-level treatment effects and errors are averaged across the sample to get the sample-average treatment on the treated, which in a random sample will equal the population average treatment effect.

B. *Differences between the Treated and Control Groups*

Treated counties are defined as those with mostly 25 Mbps broadband service, and the control group is counties with mostly 10 Mbps service. Between the two, covariate overlap is very poor, portending large errors in the estimates of treatment effects. For instance, Figure 1 provides the kernel densities of (the natural log of) population density for the treated and untreated samples for the year 2013. The distributions are obviously and substantially different. Population density in counties with predominately 25 Mbps service averages 603 persons per square mile, but only 32 persons-per-square-mile for counties with predominately 10 Mbps broadband service. That is nearly a 20-fold difference, and certain to affect broadband deployment across the two county groups as density is a key factor in determining costs. Density may also effect economic activity.[33]

Figure 1. Population Density



Such huge disparities are seen in other variables of interest. Table 2 provides descriptive details on the treatment and control groups. For the levels of the outcome variables, the differences in 2013 are titanic. Average population for the treated counties is 251,490, but a paltry 22,013 for the control group—a 10-fold difference. The average number of jobs in the treated group is 150,288, while only 10,605 in the control group. Similar size differentials are observed for total earnings as well as personal income (as noted above). Large differences are also seen in the share of persons with a college education (22% versus 14%), and household size (2.24 versus 2.07). The treated and control counties are very much different, risking large errors in the estimation of the treatment effect if the differences are left unaddressed. Before estimating the treatment effect, the lack of common support must be addressed.

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[33] There is a wide literature on this topic. *See*, *e.g.*, J. Rappaport, *A Productivity Model of City Crowdedness*, Federal RESERVE BANK OF KANSAS CITY RWP 06-06 (June 2006) (available at: http://citeseerx.ist.psu.edu/viewdoc/download?doi=10.1.1.366.4195&rep=rep1&type=pdf); S.S. Deole and A. El Gallaa, *Population Density, Optimal Infrastructure, and Economic Growth*, in THE ECONOMICS OF INFRASTRUCTURE PROVISIONING (A. Picot, *et al.*, eds.) (2015) at pp. 95-120.

Table 2. Descriptive Statistics Across Treated and Control Groups
(Year = 2013)

Variable	Treated (25 Mbps)	Control (10 Mbps)	Difference (%)
Treatment			
25 Mbps Broadband	0.925	0.035	
10 Mbps Broadband	0.991	0.945	
Outcomes			
Jobs (2013)	150,288	10,605	1,317%
Personal Income (2013)	12,060,905	788,098	1,530%
Earnings (2013)	8,960,766	452,080	1,982%
Other Factors			
Population (2013)	251,490	22,013	1,042%
Pop. Density (2013)	603.6	31.7	1,804%
College Educated (2013)	0.22	0.14	57%
Household Size (2013)	2.34	2.07	13%
Stat. Sig. 1% ***			

Here, the technique of Coarsened Exact Matching ("CEM") is used to improve covariate overlap, culling the total sample to render a sample of comparable counties.[34] The data are matched on population, population density, the percentage of adults with a college education, and household size. All but the college education variable (a percentage) is expressed in natural log form. [35] The improvements in balance are summarized in Table 3, where higher values of the $\mathcal{L}_1$ statistic indicate greater imbalance.

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[34] Iacus, *et al.* (2008), *supra* n. 32; M. Blackwell, M. Iacus, G. King, and G. Porro, *cem: Coarsened Exact Matching in Stata*, 9 THE STATA JOURNAL 524-546 (2009) (available at: https://gking.harvard.edu/files/gking/files/cemStata_0.pdf). A user's guide for CEM is offered in R. Firestone, *Evaluating Program Effectiveness: Key Concepts and How to Use Coarsened Exact Matching*, POPULATION SERVICES INTERNATIONAL (2015) (available at: http://www.psi.org/publication/evaluating-program-effectiveness-key-concepts-and-how-to-use-coarsened-exact-matching). Coarsened Exact Matching has been applied in some Internet research. *See, e.g.*, S.R. Cotten, G. Ford, S. Ford, and T.M. Hale, *Internet Use and Depression Among Retired Older Adults in the United States: A Longitudinal Analysis*, 69 JOURNALS OF GERONTOLOGY, SERIES B: PSYCHOLOGICAL SCIENCES AND SOCIAL SCIENCES 763–771 (2014); Sudulich, M. Wall and L. Baccini, *Wired Voters: The Effects of Internet Use on Voters' Electoral Uncertainty*, 45 BRITISH JOURNAL OF POLITICAL SCIENCE 853-881 (2015) (available at: https://www.exeter.ac.uk/media/universityofexeter/elecdem/pdfs/florence/Sudulich_Wall_Baccini_Wired_voters.pdf); B.N. Greenwood and R. Agarwal, *Matching Platforms and HIV Incidence: An Empirical Investigation of Race, Gender, and Socioeconomic Status*, 62 MANAGEMENT SCIENCE 2281-2303 (2015) (draft available at: http://questromworld.bu.edu/platformstrategy/files/2014/07/platform2014_submission_7.pdf).

[35] Sturge's rule chooses the cutpoints. Blackwell, *et al.* (2009), *id.* at p. 25.

Table 3. $\mathcal{L}_1$ Imbalance

Variable	Original Data	CEM Weighted
Population (2013)	0.639	0.151
Pop. Density (2013)	0.445	0.143
College Educated (2013)	0.344	0.194
Household Size (2013)	0.637	0.210
Multivariate	0.907	0.896
N: Treated, Control	1001, 629	435, 419

As shown in the table, for the individual variables balance is much improved by CEM. Multivariate $\mathcal{L}_1$ imbalance falls only from 0.906 to 0.896. With CEM weighting, sample sizes remain large with 435 treated counties and 419 control counties, even though the sample is much smaller than the full samples of 1,001 and 629 counties. The somewhat large reduction in sample size reflects the gross imbalances across counties with differing broadband deployments. This analysis demonstrates the critical nature of considering covariate balance in the empirical study of the effects of broadband services. Such imbalanced may arise in any measure of broadband availability or adoption, and across any geographic area under consideration.

Figure 2. Population Density, Balanced Sample

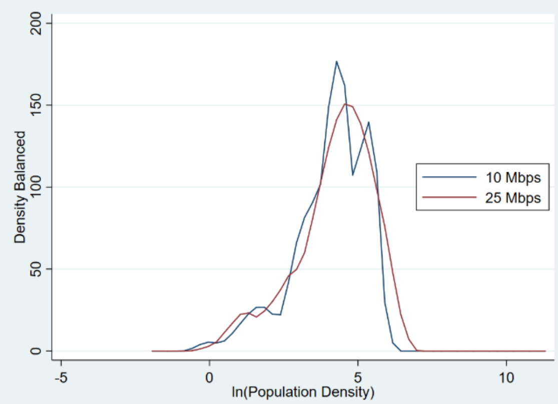

Figure 2 illustrates the kernel density for population density based on the balanced data. Average density across the two groups is now much closer than in the full sample (123.1 versus 97.7). Overlap for other factors of interest after CEM include college educated (16.2; 16.0), household size (2.21; 2.21), and population (66,684; 67,478). Overall, the matching procedures appears to have been a success.

C. *Treatment Effects*

I now turn to the estimation of the treatment effect. As noted above, economic outcomes are defined as the percentage growth rate in jobs, total personal income, and total labor earnings between 2015 and 2013 (or two years of growth). The treatment effects are estimated using the regression model,

$$\ln(Y_{i,t} / Y_{i,t-2}) = \delta T_{i,t-2} + \beta X_{i,t-2} + \lambda_j + e_i , \tag{7}$$

where the dependent variable is the percentage growth in the economic outcome of interest between 2015 and 2013 in county i, T_i equals 1 for the treatment group based on 2013 broadband data, X_i is a vector of county-specific covariates also measured in 2013, β is a vector of coefficients on those variables, λ is a set of coefficients on state-level (subscript j) fixed effects, and e is the econometric disturbance term. The treatment effect is measured by δ, which equals the difference in outcomes "caused" by the difference in broadband speeds. There are three outcomes of interest and thus three versions of Equation (7), all of which measure economic outcomes at the country level. To account for the correlation of the residuals across these three equations, they are jointly estimated using Seemingly Unrelated Regression ("SUR").[36] Hypothesis tests are based on robust standard errors.

Using the CEM-matched data involves estimating Equation (3) by weighted least squares. When using the exactly matched data, the inclusion of the X variables is unnecessary, but I have only approximately matched data.[37] Thus, the X are included in some regressions. Also, state fixed effects are included in some regression to account for many factors, including the fact the broadband data is collected at the state level by different organizations and methods.[38] Many of these fixed effects are statistically significant, though I suppress the results for expositional purposes.

Table 4 summarizes the results from the simplest regression model that includes only the treatment (T) as a regressor (excluding both X and the fixed effects λ). The influence of selection bias may be determined by comparing the results using the original and the matched data, the former possibly being biased. The first set of results in Table 4 are based on the original, unbalanced data. These results would—albeit falsely—suggest that higher-speed broadband causes much higher growth in economic outcomes. For instance, job growth in the treated group is 3.24% over the two-year window, yet only 1.48% for the control group (a difference of 1.77

[36] The maximum likelihood approach is employed and robust errors computed. W. Gould, J. Pitblado, and B. Poi, MAXIMUM LIKELIHOOD ESTIMATION WITH STATA (2010). I use the -mysureg- command in Stata 15.

[37] Blackwell, *et al.* (2009), *supra* n. 34.

[38] Including the state fixed effects in CEM results in very small sample sizes.

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percentage points). The difference between the two groups for total personal income is even larger, a 4-percentage point difference (7.33% versus 3.33%), and still larger for labor earnings (5.98% versus -0.31%). All these differences are statistically significant at the 1% level or better.

Table 4. Estimated Treatment Effects, Simplest Model

Original Data	Jobs	Income	Earnings
Treated	3.24%	4.47%	3.12%
Control	1.46%	0.55%	-3.17%
Difference	1.78***	3.92***	6.29***
Balanced Data			
Treated	2.27%	2.09%	-0.54%
Control	2.01%	2.90%	0.55%
Difference	0.25	-0.81	-1.09

Sig. Levels: 1% ***, 5% **, 10% * .

These large and statistically significant differences in growth rates vanish in the balanced data. In fact, two of the three treatment effects is negatively signed, and all are small. None of the treatment effects is statistically different from zero. Thus, statistical analysis of broadband speeds, once covariate imbalance is addressed, do not indicate positive economic gains from an additional 15 Mbps in service speed. Plainly, the original data, being very unbalanced, offers highly misleading inferences about the impact of broadband speeds on economic outcomes. Any sizable or statistically significant growth rate advantage for higher-speed broadband found in that data comes from estimation error and not a valid treatment effect.

Table 5. Estimated Treatment Effects, Full Model

Original Data	Jobs	Income	Earnings
Treated	2.68%	3.43%	1.41%
Control	2.38%	2.16%	-0.53%
Difference	0.30	1.27**	1.93**
Balanced Data			
Treated	1.99%	2.54%	-0.06%
Control	2.31%	2.43%	0.06%
Difference	-0.32	0.11	-0.12

Sig. Levels: 1% ***, 5% **, 10% * .

In Table 5, I include the covariates *X* in the regression model (population, population density, household size, and college education) as well as state-level fixed effects. Again, all regressors are in natural log form except for the college education variable. The estimated treatment effects from the original data are much smaller, but two of the outcomes continue to show statistically significant differences (income and earnings) at the 5% level. Including some observables in the regression helped reduce the bias in the treatment effect, but did not eliminate it for all outcomes. On jobs, the treatment effect is very small (0.30) and not statistically different from zero; so, merely including a few observable factors eliminates the jobs effect in the original data irrespective of balancing.

Turning to the balanced data, the treatment effects are much smaller than with the original data and none is statistically different from zero. Again, the treatment effects estimated from the original, unbalanced data arise from error and not from the influence of higher broadband speeds. These results suggests no broad economic payoff from an increase in broadband speeds from 10 Mbps to 25 Mbps, at least not for the measures of outcomes considered here (jobs, income, and earnings). The concern that a lack of an extra 15 Mbps will leave rural Americans "behind economically" is unsupported by the evidence.[39]

D. *Unconfoundedness*

Addressing covariate overlap eliminated any measureable economic impact from the speed differential between counties. As for the unconfoundedness assumption, there are no direct tests of it. However, there are indirect ways of assessing the validity of assumption. Rosenbaum (1987), Heckman and Hotz (1989), and Imbens and Wooldridge (2009) offer a simple approach that is sometimes available to test the null hypothesis of "no causal effect" in instances where the null is (almost) certain to be true.[40] When time series data is available, Imbens and Wooldridge (2009) propose to estimate the causal effect of the treatment on an outcome that is determined prior to the treatment (a pseudo-treatment), so that the treatment is known for certain to have no effect, since the future cannot determine the past.[41]

To see the logic of the pseudo-treatment, say, for instance, a researcher wants to know whether an educational program improves test scores. Participation in the program is voluntary. After the program, the researcher finds test scores for the treatment group (those taking the course) are 10% higher than the control group. This finding may lead the researcher to conclude the class was beneficial. However, what if some or all of those persons in the sample took the same test prior to the course? If the difference in test scores before the class was also 10%, then it

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[39] Souza, *supra* n. 9.

[40] P.R. Rosenbaum, *The Role of a Second Control Group in an Observational Study*, 2 STATISTICAL SCIENCE 292–306 (1987); J.J. Heckman and V. J. Hotz, *Choosing among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training*, 84 JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION 862–74 (1989); Imbens and Wooldrige (2009), *supra* n. 29 at p. 48-9.

[41] Imbens and Wooldridge, *id.* at p. 48.

is clear the course has no effect on test scores, but rather arises from some other factors more common in the treatment group than in the control group.

Table 6. Estimated Pseudo-Treatment Effects

Original Data	**Jobs**	**Income**	**Earnings**
Treated	4.01%	7.52%	8.24%
Control	1.20%	4.47%	4.57%
Difference	2.81***	3.05***	3.68***
Balanced Data			
Treated	2.70%	5.91%	6.05%
Control	2.41%	5.51%	5.35%
Difference	0.29	0.39	0.70
Sig. Levels: 1% ***, 5% **, 10% *.			

Since I have data on economic outcomes prior to the treatment, the pseudo-treatment approach is a viable option. I apply a pseudo-treatment by looking at economic outcomes long before the advent of very high-speed broadband. Specifically, outcomes are measured as the percent change in jobs, income, and earnings between 1998 and 2000. If the 25 Mbps treatment from 2013 is found to be positive and statistically significant, then this result is evidence that a confounding factor is polluting the estimated treatment effect (that is, the unconfoundedness assumption is unlikely to hold).

As shown in Table 6, the estimated treatment effects from the pseudo-treatment applied to the original, unbalanced data are all large and statistically significant at the 1% level or better.[42] The results suggest that there is indeed confounding. Looking at the result on job growth, the treatment effect is estimated to be 2.81 percentage points (significant at better than the 1% level). In other words, the estimate indicates that job growth between 1998 and 2000 is determined by speed differences between counties in 2013. None of the pseudo-treatments is statistically different from zero in the matched data and the effect sizes are much smaller than in the unbalanced data. Based on these results, I conclude that broadband is deployed (or upgraded) first in areas that tend to have higher economic activity, and this pattern leads to spurious findings of economic impacts from broadband deployment (in unbalanced data).[43]

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[42] The pseudo-treatment is also significant (at the 10% level or better) if the additional regressors are added to the model.

[43] Note, alternately, that the pseudo-treatments are not statistically significant in the balanced samples.

III. A Fresh Look at an Earlier Study

There are many studies of varying quality that attempt to quantify the relationship between broadband and aggregate measures of economic activity. A few of the more commonly cited studies from the earliest research on the topic include Ford and Koutsky (2005), Gillett, Lehr, and Osorio (2006), Crandall, Lehr, and Litan (2007), and Shideler, et al. (2009), all of which provide some supporting evidence of broadband's impact on broad measures of economic activity.[44] Many others studies have been published in recent years.[45] In light of the results above, it is perhaps worth looking again at this early evidence. I focus here on the ubiquitously cited Crandall, Lehr, and Litan (2007) study (hereinafter "Crandall Study"), primarily because of the availability of data and ease of replication.[46]

The Crandall Study is frequently cited to support the notion that broadband drives employment growth; indeed, citation to the study is now boilerplate in studies of broadband's effect on the economy.[47] While the study also looked at GDP growth, the results on that outcome were very weak and are uncommonly cited, so I focus on the employment data. Looking at state level broadband adoption and employment levels, the analysis occurs at a high level of aggregation. Alaska, Hawaii, and the District of Columbia were excluded from the sample. The full sample consists of only 48 data points.

The Crandall Study employs a simple model, estimated by least squares, to quantify the relationship between employment growth and broadband adoption by state:

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[44] G. Ford and T. Koutsky, *Broadband and Economic Development: A Municipal Case Study from Florida*, 17 REVIEW OF URBAN AND REGIONAL DEVELOPMENT ECONOMICS 216-229 (2005); S. Gillett, W. Lehr, C. Osorio, and M. Sirbu, *Measuring the Economic Impact of Broadband Deployment*, Final Report: National Technical Assistance, Training, Research, and Evaluation Project #99007-13829 (2006) (available at: http://www.eda.gov/PDF/MITCMUBBImpactReport.pdf); R. Crandall, W. Lehr, and R. Litan, *supra* n. 16; D. Shideler, N. Badasyan, and L. Taylor, *The Economic Impact of Broadband Deployment in Kentucky*, 3 REGIONAL ECONOMIC DEVELOPMENT 88-118 (2007) (available at: https://files.stlouisfed.org/files/htdocs/publications/red/2007/02/Shideler.pdf). A review of some of the early literature is provided in L. Holt and M. Jamison, *Broadband and Contributions to Economic Growth: Lessons from the U.S. Experience* (Aug. 14, 2008).

[45] For a summary of some of this work, *see* I. Bertschek, W. Briglauer, K. Huschelrath, B. Kauf, and T. Niebel, *The Economic Impacts of Telecommunications Networks and Broadband Internet: A Survey*, CENTER FOR EUROPEAN ECONOMIC RESEARCH DISCUSSION PAPER NO. 16-056 (2016) (available at: http://ftp.zew.de/pub/zew-docs/dp/dp16056.pdf).

[46] Ford and Koutsky (2009), *supra* n. 44 apply a type of matching algorithm within a difference-in-differences analysis.

[47] Google Scholar lists 257 citations to the study.

$$EMP_t / EMP_{t-l} = \delta B_i + \beta X_i + \lambda_i + e_i \,, \tag{8}$$

where *EMP* is total employment in the state in year *t* and *t* - *l* (where *l* is either 1, 2), *B* is broadband lines per capita, *X* is a set of covariates believed to impact employment growth (with coefficients β), λ is a set of regional fixed effects, and *e* is the disturbance term. The coefficient δ measures the impact of broadband on employment growth. The dependent variable measures the ratio of employment in for years 2004 to 2005, or 2003 to 2005. Regressors, for the most part, are from the base year. In addition to broadband lines per capita (BB LINES/CAP, which measures B_i), the regressors include average annual temperature (TEMP), small business tax climate indicator (TAX), union membership share of employment (UNION), share of college graduates in the adult population (EDUC), average hourly earnings for the nonfarm private sector (WAGE), and dummy variable for the nine census regions.[48] To the extent feasible, I rely on the same sources as the original study, closely replicating both the descriptive statistics and regression results.

48 Sources and descriptive statistics are provided in Crandall, *et al.* (2007), *supra* n. 16 at Table 1b.

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Table 7. Replication of Crandall Study, Employment Data

	Crandall Study		Replication	
	2005/2004	2005/2003	2005/2004	2005/2003
	(1)	(2)	(3)	(4)
Constant	0.994***	1.042***	1.002***	1.052***
	(39.44)	(22.89)	(46.64)	(24.76)
BB LINES/CAP	0.223**	0.593***	0.222**	0.571***
	(2.39)	(3.23)	(2.38)	(3.24)
TEMP	0.000	-0.000	-0.000	-0.000
	(0.01)	-(0.26)	(-0.44)	(-0.86)
TAX	0.004**	0.009**	0.004**	0.008**
	(2.46)	(2.91)	(2.02)	(2.59)
UNION	-0.000	0.000	-0.000	0.000
	(-0.42)	(0.40)	(-0.50)	(0.11)
EDUC	-0.102	-0.116	-0.096	-0.168
	(-1.11)	(-0.89)	(-1.07)	(-1.31)
WAGE	-0.001	-0.006*	-0.001	-0.004
	(-0.45)	(-1.74)	(-0.25)	(-1.41)
Regional FE	Yes	Yes	Yes	Yes
Adj. R^2	0.646	0.686	0.575	0.655
N	48	48	48	48

Stat. Sig. 1% ***, 5% **, 10% *.

Table 7 reproduces the strongest and most commonly referenced results from the Crandall Study from the study's Table 2 (at columns 2 and 4). For expositional purposes, I do not report the coefficients on the regional dummy variables. In the first column are the results for employment growth between 2004 and 2005. In the second column, the results are for employment growth over the years 2003 to 2005 (the coefficient measures two-years of growth so is larger). The coefficients on the broadband variable in both models (1) and (2) are positive and statistically different from zero, suggesting broadband increases employment growth. These results explain the popularity of the Crandall Study in broadband advocacy and research.

In Columns (3) and (4) of Table 7, my attempt at replication is summarized. While there are some differences in the results, they are minor and explained by some small discrepancies in the non-broadband regressors. The coefficients and t-statistics on the broadband coefficients are very similar, as are the coefficients and significance levels of the other covariates. In all, the replicated dataset is very close to the original and the estimates are validated using the same model. This is encouraging.

Given the small sample size and the continuous treatment variable, CEM is precluded. It is interesting to look, however, at the differences in the X_i at the extreme values of the continuous broadband variable. In Table 8, the means of the variables are offered, using samples drawn from the upper-and-lower 10% and 25% tails of BB/LINES from 2004. The differences for all the variables but TAX, and perhaps TEMP in the 25% tails, are quite large. The imbalance in these factors suggests the treatment effect in the Crandall Study may be estimated with error.

Table 8. Imbalance

	10% Tails		25% Tails	
	Low	**High**	**Low**	**High**
TAX	5.43	5.57	5.18	5.49
TEMP	49.02	52.81	50.82	52.38
UNION	7.38%	12.9%	8.29%	13.63%
EDUC	15.27%	22.31%	14.23%	20.13%
WAGE	14.54	19.61	14.93	18.89

Using employment data from 1995, 1996, 1997, and 1998, it is possible to evaluate the unconfoundedness assumption using pseudo-treatments. The late 1990's were the very earliest stages of broadband deployment. Certainly, we would not expect broadband adoption in years 2003 and 2004 to impact employment growth in these earlier years. In Table 9, I summarize the results from applying pseudo-treatments in the context of the Crandall Study's regression model. The null hypothesis is that broadband adoption in either 2003 or 2004 *does not* influence employment growth in the late 1990s. Of course, the null hypothesis is true. If the regression indicates otherwise, there is good reason to suspect the regression model has failed to satisfy the unconfoundedness assumption.

Table 9. Pseudo-Treatments for Crandall Study, Employment Data

	Crandall Study	Pseudo-Treatment	Pseudo-Treatment	Pseudo-Treatment
	2005/2004	**1998/1997**	**1997/1996**	**1996/1995**
BB LINES/CAP (2004)	0.223**	0.151**	0.254***	0.312***
	(2.39)	(2.21)	(3.62)	(3.17)
BB LINES/CAP (2003)		0.182**	0.320***	0.413***
		(2.27)	(4.03)	(3.70)
	2005/2003	**1998/1996**	**1997/1995**	
BB LINES/CAP (2004)	0.593***	0.417***	0.590***	
	(3.23)	(3.28)	(3.66)	
BB LINES/CAP (2003)	0.593***	0.429***	0.608***	
	(3.23)	(3.38)	(3.83)	

Stat. Sig. 1% ***, 5% **, 10% *.

The results in Table 9 reveal that the pseudo-treatments are all positive and similarly sized to the coefficients from the Crandall Study. The null hypothesis that broadband adoption in 2003 or 2004 does not "cause" employment growth in the late 1990's is easily rejected at standard significance levels in all cases.[49] That is, broadband adoption in the future causes employment growth in the past. Rejecting the null hypothesis in this case shows that there are factors driving both broadband and employment growth—confounders—that are not accounted for in the econometric model applied in the Crandall Study. Spurious correlation, not causation, is likely driving the results of the Crandall Study.

Between my analysis of the Crandall Study and the speed question above, it should be apparent that empirical studies on broadband's impact must carefully attend to the unconfoundedness assumption and covariate overlap. For instance, Stenberg, et al. (2007) simulate an experiment by using a matching procedure to create a control group, ensuring covariate overlap between the control and the treated group.[50] Pseudo-treatments should also be evaluated, since the potential for confounding broadband's impact with other factors seems likely, especially with broad measures of economic outcomes. Any study that has considered such factors by some appropriate means should be viewed with skepticism and not referenced for public policy purposes.

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49 Multiple years are used to demonstrate the result is not a random event (*i.e.*, a "fluke").

50 P. Stenberg, M. Morehart, S. Vogel, J. Cromartie, V. Breneman, and D. Brown, *Broadband Internet's Value for Rural America*, United States Department of Agriculture Economic Research Report No. 78 (August 2009) (available at: https://www.ers.usda.gov/webdocs/publications/46200/17045_err78fm_1_.pdf?v=41056).

IV. Conclusion

Do counties with mostly 25 Mbps broadband connections fare better economically than counties with mostly 10 Mbps broadband connections? I find no evidence of such an effect here, at least with respect to the growth in jobs, personal income, or labor earnings between 2013 and 2015. This analysis is, of course, limited to those time periods and the outcomes considered, and is dependent on the particular empirical model employed. While these are limitations, the empirical analysis attempts to answer a specific policy question of some importance, and upon doing so, is perhaps the sole evidence available on the presently important topic.

What my statistical analysis does find evidence of is profound differences in the economic character of counties with different broadband speeds. Selection bias appears to be a serious problem in quantifying broadband's economic impact. Broadband (and higher speed broadband) is not randomly distributed across geography, but rather is deployed in areas where the ratio of demand to costs is favorable, complicating the task of discovering broadband's influence on economic outcomes. Not only does my own analysis of broadband speeds demonstrate these empirical difficulties, but application of standard techniques reveals that the results of an earlier and frequently cited study by Crandall, Lehr, and Litan (2007) are probably spurious. In future research on broadband's impact on economic outcomes, whether at a macro or micro level, statistical techniques must be used to address both problems of confounding influences and the potentially large differences in the economic and demographic characteristics across geographic markets.