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ECONOMIC BENEFITS OF FIBER DEPLOYMENT: A REVIEW OF THE BRATTLE GROUP STUDY

Abstract: The Infrastructure Investment and Jobs Act provided over $42 billion in subsidies for broadband deployment via the Broadband Equity, Access, and Deployment ("BEAD") program. Although BEAD funds are supposed to be disbursed on a technology-neutral basis, the Biden Administration mandated a preference for fiber deployment, even when alternative technologies—including satellite broadband—are the more efficient mechanism to serve certain high-cost areas. With the recent election of Donald Trump to the presidency and his focus on ensuring that taxpayer dollars are spent effectively, the government's preference for fiber now faces significant uncertainty. In this context, the Brattle Group released a commissioned study claiming substantial economic benefits from fiber deployment based on a Differences-in-Differences ("DID") regression model. In this BULLETIN, I describe several methodological problems in the Brattle Group's approach, including a failure to evaluate key assumptions of DID models and the mis-specification of the regression models, and then evaluate their study's findings using the same data sources. I find key identifying assumptions of the DID estimator are unmet, meaning the Brattle Group's estimates are uninformative, and model mis-specification is shown to produce outsized estimates. An attempt to address parallel paths suggests fiber has no meaningful effects on the studied outcomes. These findings suggest that, for now, policymakers should explore diverse approaches beyond fiber networks for delivering broadband to Americans, avoiding subsidizing fiber when modern cable and other networks are available, including the potential of next-generation satellite technologies.

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I. Introduction

Given the exceedingly slow rollout of Broadband Equity, Access, and Deployment ("BEAD") funding and the host of partisan-motivated appendages to the broadband subsidy plan by the Biden Administration,[1] the National Telecommunications Information Administration ("NTIA") is taking heat from nearly all fronts.[2] Among other complaints, although BEAD funds are supposed to be distributed on a technology-neutral basis, the Biden Administration adopted a preference for fiber deployment even though alternative distribution technologies—such as satellite broadband—provide a more cost-effective alternative, particularly for hard to serve rural areas. With the election of Donald Trump to the presidency, many analysts therefore expect some changes in BEAD implementation.[3] New Street's Blair Levin, for example, suggested that under the Trump Administration "the BEAD program is likely to see a delay in spending [] and a reduction in expenditure for fiber."[4] These expectations may prove correct.

Commerce Secretary Howard Lutnick recently stated that the Trump Administration is considering modifying the BEAD Program back to a "tech-neutral approach that is rigorously driven by outcomes, so states can provide internet access for the lowest cost." Thus, it appears that part of the new administration's policies will be to rely more on modern Low Earth Orbit ("LEO") satellite broadband—including Elon Musk's StarLink and Amazon's Project Kuiper (soon to be launched)—to serve very high-cost, rural areas that may have otherwise been serviced by fiber networks at enormous expense.[5] As a result, Trump's win may be bad news for

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1 *See, e.g.*, G.S. Ford, *Middle-Class Affordability of Broadband: An Empirical Look at the Threshold Question*, PHOENIX CENTER POLICY BULLETIN NO. 61 (October 2022) (available at: https://phoenix-center.org/PolicyBulletin/PCPB61Final.pdf).

2 M. Abarinova, *2025 Hasn't Been "Year of Execution" for BEAD*, FIERCENETWORK (June 26, 2024) (available at: https://www.fierce-network.com/broadband/heres-lowdown-whats-going-bead); P. McLAughlin, *Senator Ted Cruz Takes Aim at BEAD Program*, CABLING INSTALLATION & MAINTENANCE (November 26, 2024) (available at: https://www.cablinginstall.com/cable/article/55246223/senator-ted-cruz-takes-aim-at-bead-program); K. Griffis, *Trump Taps Critic of Broadband Expansion Plan to Oversee Program*, YAHOO!News (February 4, 2025) (available at: https://www.yahoo.com/news/trump-tap-critic-biden-broadband-014935164.html).

3 M. Abarinova, *With Trump Now in Office, What About BEAD?* FIERCENEWORK (January 22, 2025) (available at: https://www.fierce-network.com/broadband/trump-now-office-what-about-bead); C. Teale, *The $42B Question: What's Next for Federal Broadband Funding?*, GOVERNMENT EXECUTIVE (November 26, 2024) (available at: https://www.govexec.com/technology/2024/11/42b-question-whats-next-federal-broadband-funding/401317).

4 M. Dano, *Under Trump, Satellites Could Steal Fiber's BEAD Bonanza*, LIGHTREADING (November 6, 2024) (available at: https://www.lightreading.com/satellite/under-trump-satellites-could-steal-fiber-s-bead-bonanza).

5 *Statement from U.S. Secretary of Commerce Howard Lutnick on the BEAD Program*, U.S. Department of Commerce Press Release (March 5, 2025) (available at: https://www.commerce.gov/news/press-releases/2025/03/statement-us-secretary-commerce-howard-lutnick-bead-program); P. Haggin, *Commerce to Overall "Internet for All" Plan,*

Footnote Continued…

subsidized fiber deployment and, in turn, the fiber network providers and their suppliers seeking those subsidies.

Perhaps motivated by these turn of events, Frontier Communications and the Fiber Broadband Association asked the Brattle Group to estimate some economic impacts of fiber deployment (hereinafter, the "Brattle Group Study" or "Study").[6] Combining data from the American Community Survey ("ACS") and broadband deployment data from the Form 477 and National Broadband Map, the Brattle Group Study uses a difference-in-differences ("DID") model to estimate the effects of fiber deployment on three outcomes: (1) the employment-to-population ratio; (2) median home values; and (3) median household income. The Brattle Group Study concludes that deploying fiber to all non-fiber households, which they find increases home values and median household incomes, offers a net present value ("NPV") economic benefit of $3.42 trillion, or an annual benefit of about $171 billion, and create 380,000 new jobs.

Fiber deployment's effect on outcomes like employment, home prices, and incomes is a challenging problem—mainly, fiber deployment (the treatment) is not randomly assigned and there is likely a spatial element to the analysis (requiring special techniques).[7] The Brattle Group Study, however, adopts a rather simple approach and offers few details. First, the Brattle Group Study provides scarcely any description of the data used in the analysis. Second, the Study's definition of "urban" puts less than 20% of Americans in urban areas, whereas most traditional measures of urbanicity have 80% of Americans living in urban areas. Adding to the lack of detail, the Study does not state how the "urban" variable is defined (requiring an author inquiry). Third, the regression model is incorrectly specified, assuming additive rather than multiplicative effects for home values and household income and using an incorrect specification for what appears to be an unbalanced panel. Fourth, despite recognizing the importance of the parallel paths

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Expanding Starlink Funding Prospects, WALL STREET JOURNAL (March 4, 2025) (available at: https://www.wsj.com/politics/policy/commerce-to-overhaul-internet-for-all-plan-expanding-starlink-funding-prospects-74664efc); *see also* Testimony of Phoenix Center Chief Economist Dr. George S. Ford before the House of Representatives Committee on Energy and Commerce - Subcommittee on Oversight and Investigations, Hearing on "Closing the Digital Divide: Overseeing Federal Funds for Broadband Deployment" (May 10, 2023) (available at: https://phoenix-center.org/Ford-Testimony-ClosingDigitalDivide-20230510.pdf).

[6] P. Sanyal, C. Bazelon, Y. Paek, and D. Beemon, *Economic Benefits of Fiber Deployment*, The Brattle Group, Commissioned by Frontier Communications and the Fiber Broadband Association (November 20, 2025) (available at: https://fiberbroadband.org/wp-content/uploads/2024/11/2024.11.20-Benefits-of-Fiber-Deployment-Brattle-FINAL.pdf).

[7] Spatial regression likely requires the weighting matrix to be constructed using all tracts and not the restricted sample used in here or in the Brattle Group Study. Constructing such large spatial matrix could be challenging (larger than 70,000 x 70,000) to construct.

assumption in DID analysis, the Study does not evaluate it.[8] Fifth, it seems likely that a spatial regression model is required.[9] Sixth, the estimation methods are ill-suited to fiber deployment, which is a continuous variable that rises somewhat steadily over time. Dichotomizing a continuous variable to facilitate simple analysis, as is done in the Brattle Group Study, is widely held to be a practice to avoid.[10] Finally, whether higher home prices constitute an unbridled benefit is a dubious proposition, as high home prices embed a transfer from buyers to sellers, a transfer of income from renters to owners, an increase in wealth inequality, and can have other deleterious effects that are unaccounted for.[11]

While the lack of detail on the data used in the Brattle Group Study frustrates an exact reproduction of its empirical analysis, all of these data are publicly available and the empirical methods are familiar. In this BULLETIN, broadband deployment and ACS data are used to replicate the Brattle Group Study's analysis, expanding the analysis to evaluate parallel trends. The Brattle Group Study's findings do not survive this analysis. Foremost, the parallel paths assumption is unsupported, so the DID estimates in the Brattle Group Study are unreliable as they incorporate the effects of different trends. Also, the effect sizes in the Brattle Group Study are grossly overstated due to the misspecification of the regression model. Absent more reliable evidence, policymakers should continue to investigate alternative, more efficient means than fiber networks for expanding broadband access to Americans in high-cost areas, including modern LEO satellite services that serve users at a fraction of the cost of fiber. Five years of LEO service costs $10,690 (in current monthly fees of $120 plus $349 in equipment), whereas fiber often costs far more than that to deploy and still requires users to pay a monthly fee.[12] As these LEO networks are more fully deployed, including the upcoming launch of Amazon's Project Kuiper,

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8 *Brattle Group Study*, *supra* n. 6 at p. 34 ("assess the necessary assumption that the treatment and control groups have parallel trends before the treatment").

9 *See*, *e.g.*, W. Kang, *Housing Price Dynamics and Convergence in High-Tech Metropolitan Areas*, 51 QUARTERLY REVIEW OF ECONOMICS AND FINANCE 283-291 (2011); P. Simlai, *Estimation of Variance of housing Prices Using Spatial Conditional Heteroskedasticity (SARCH) Model with an Application to Boston Housing Price Data*, 54 QUARTERLY REVIEW OF ECONOMICS AND FINANCE 17-30 (2014); J. Kim and P. Goldsmith, *A Spatial Hedonic Approach to Assess the Impact of Swine Production on Residential Property Values*, 42 ENVIRONMENTAL AND RESOURCE ECONOMICS 509-534 (2009); M. Fletcher, P. Gallimore, and J. Mangan, *Heteroskedasticity in Hedonic House Price Models*, 17 JOURNAL OF PROPERTY RESEARCH 93-108 (2000).

10 *See*, *e.g.*, N.V. Dawson and R. Weiss, *Dichotomizing Continuous Variables in Statistical Analysis: A Practice to Avoid*, 32 MEDICAL DECISION MAKING 225-226 (2012); D.L. Streiner, *Breaking up is Hard to Do: The Heartbreak of Dichotomizing Continuous Data*, 47 CANADIAN JOURNAL OF PSYCHIATRY 262-266 (2022).

11 *See*, *e.g.*, J.H. Choi, L. Goodman, and B. Bai, *Four Ways Today's Higher Home Prices Affect the Larger Economics*, Urban Institute (October 11, 2018) (available at: https://www.urban.org/urban-wire/four-ways-todays-high-home-prices-affect-larger-economy).

12 Prices available at: https://www.starlink.com/us.

capacity will rise, and prices will fall. Also, fiber should not be subsidized by anyone as it is wasteful, particularly where modern cable networks (capable of gigabit speeds) are already available.

II. A Review of the Brattle Group Study

The Brattle Group Study attempts to estimate some economic benefits of fiber availability using the 2-by-2 difference-in-differences estimator ("DID"), the now standard approach for estimating causal inference in modern econometrics. The 2x2 DID estimator requires two groups and two time periods; there is a treatment group (T) and a control group (C) of which neither receives the treatment in period 1 and only the treatment group receives the treatment in period 2. The DID estimator (δ), which is simply the *difference in the differences* in outcomes for each group between periods, is,

$$\delta = (Y_1^T - Y_0^T) - (Y_1^C - Y_0^C) = \Delta Y^T - \Delta Y^C . \tag{1}$$

Central to the validity of the DID estimator (δ) is that the change in Y for the control group between periods equals that of the treatment group if the treatment group were untreated (the parallel paths or common trends assumption), and that the treatment status of one unit does not "spillover" to the potential outcomes of other units. The Brattle Group Study mentions the parallel paths assumption but provides no evaluation of its plausibility (I do so here). Also required of the DID estimator is the Stable Unit Treatment Value Assumption ("SUTVA"). SUTVA requires that a treatment effect influences the outcomes of those treated and no others, but fiber deployment has (in the past, at least) led to economic migration, hurting one area to the benefit of another, and fiber deployment in one neighborhood may affect prices in another. Also, the Brattle Group Study devotes a few paragraphs to the "existence of externalities implies that increasing the current access to fiber broadband has spillover effects on economic activity."[13] Such spillovers, where the untreated are affected by the treatment, violates SUTVA.

Notably, the 2x2 DID framework of Equation (1) does not fit the reality of fiber deployment. Fiber deployment is a continuous variable (0% to 100%) and fiber is regularly deployed to new places. Rather than employ an empirical method suited to these data, the Brattle Group Study manipulates these data to comport with this simple 2x2 DID approach using the following procedure. First, the Brattle Group Study dichotomizes the continuous fiber deployment variable by defining the treatment group as census tracts where fiber is deployed to at least 15% of blocks within a tract. There are two concerns here. Dichotomizing a continuous variable (fiber deployment) is ill advised and the 15% cutoff is arbitrary (treating 14.9% as entirely different than 15%, which it is not). Also, it is unclear whether the 15% cutoff is based on a share of population,

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13 *Brattle Group Study*, *supra* n. 6 at p. 11.

households, or just blocks (ignoring population), though the description of these data suggests the latter. Second, to create the required two time periods the treatment date is defined as year 2017-2018 and all census tracts with fiber before 2017 are excluded from the sample so no tracts are treated in the first period. Third, the treatment group is defined as census tracts with at least 15% fiber as of 2017 or 2018. Fourth, a control group is defined as census tracts without fiber through 2021. This definition is inconsistent with the treatment indicator since the treatment group has more than 15% fiber coverage while the control group has no fiber, leaving a gap between 0% and 15%. I suppose these are excluded from the sample, though it is unclear. Fifth, both the control and treated census tracts have a positive deployment of cable model service, presumably to demonstrate the unique contribution of fiber and not broadband generally. Logically, we would expect the fiber premium to be smaller (if not zero) when quality cable modem service is available. In 2018, for example, the mean download speed for fiber networks was 890 Mbps but cable download speeds were a close 848 Mbps. No customer could tell the difference. Finally, only census tracts in the control or treatment groups are retained in the sample.

The Brattle Group Study employs census tract level data on the outcomes and fiber deployment. The DID estimator δ is estimated by least squares regression using the simple regression model,

$$Y_{it} = \delta T_{it} \cdot P_{it} + \alpha_0 + \alpha_1 T_{it} + \alpha_2 P_{it} + \varepsilon_{it}, \qquad (2)$$

where Y_{it} is the outcome of interest, T_{it} is an indicator for the treatment group, P_{it} is an indicator for the treatment period, and ε_{it} is random disturbance term. If Y_{it} is in linear form, then α_0 is the mean outcome for the control group and $\alpha_0 + \alpha_1$ is the mean outcome for the treated group in the pre-treatment period, and $(\alpha_0 + \alpha_2)$ is the mean outcome for the control group in the treatment period and $(\alpha_0 + \alpha_1 + \alpha_2 + \delta)$. Simple algebra reveals that the δ coefficient is the DID estimator of Equation (1), and that the counterfactual for the treatment group is $(\alpha_0 + \alpha_1 + \alpha_2)$. When the Brattle Group Study reports the percentage effects of the treatment, it incorrectly uses α_0 as the counterfactual thus overstating these percentage effects by a sizable degree given the positive values from α_1 and α_2.

Notably, this model is mis-specified for an unbalanced panel (though a two-way fixed effects regression is appropriate). While the Study does not indicate whether the panel is balanced or unbalanced, another failure to adequately describe these data, the variations in sample sizes across outcomes suggest an unbalanced panel. Also, the Study does not indicate how the standard errors are calculated (whether standard, robust, or cluster robust), which is another significant omission.

The Brattle Group Study uses unspecified years of ACS data to estimate a DID model, though presumably part of these data are pre-treatment and part post-treatment. While the Brattle Group Study references 2023 data from the ACS, the five-year estimates for that year were not released until December 12, 2024, months after the Study was released. Five-year data are required for tract-level data. The Study suggests annual data from 2014-2021 are used, but this use is problematic with five-year ACS data as only year 2021 is clear of the pre-treatment data. My efforts to replicate sample sizes in the Study were unsuccessful.

A few things are worth mentioning before turning to the regressions. First, the "urban, non-urban" distinction is not the same as a "urban, rural" distinction. Here, "urban" is defined as any tract with at least 7,500 persons per square mile (though this is not stated in the Study); a non-traditional definition that places only about 20% of households in urban areas, whereas about 80% of households are in urban areas by more standard definitions.[14] No explanation of the choice of threshold is provided, and it is a peculiar one; a search of the literature found no use of this cutoff in published research). Atlanta, Georgia, the nation's sixth largest metropolitan area, is classified as "non-urban" by this definition (though some tracts may be labeled urban depending on what year is used to compute density). Only 12 of the 50 largest cities (not tracts) had population densities at 7,500 per square mile, so this is a peculiar choice for "urban."[15] The definition implies "very" urban, which makes the non-urban areas a mix of urban and rural areas by more traditional definitions.

Second, the home value and household income dependent variables enter the model linearly rather than in natural log form. This specification assumes, for instance, that home values increase over time by the same fixed amount irrespective of the base value (an additive effect), rather than increase by some percentage (a multiplicative effect), the latter of which is plainly more sensible. There is no reason to expect a house valued at $10,000 to increase by the same amount over five years as a house valued at $1,000,000, but the Brattle Group Study assumes this to be true. Likewise, the treatment effect is the same regardless of home value. Empiricists typically use the natural log transformation of variables like home values, wages, incomes, prices, and so forth, where multiplicative rather than additive effects are more likely. As demonstrated below, these data support multiplicative effects. The log transformation has other advantages as

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[14] While the square mileage of a census tract is constant, population varies by year, so population density also varies by year. The Brattle Group Study does not say what year's population is used to define "urban." If density is calculated each year, then census tracts may switch from one group to the other.

[15] E. Kober, *How Large Cities Can Grow Denser and Flourish: What the 2020 Census Reveals About Urban Sprawl*, Manhattan Institute (January 20, 2022) (available at: https://manhattan.institute/article/how-large-cities-can-grow-denser-and-flourish-what-the-2020-census-reveals-about-urban-sprawl).

well. Assuming additive effects when they are multiplicative can overstate (or understate) effect sizes, as detailed below, in part because the counterfactual cannot be accurately constructed.

Table 1. Brattle Group Study Regression Results

	Emp-Pop	Urban Home Value	HH Income	Emp-Pop	Rural Home Value	HH Income
Post	0.0404***	107,333***	13,097***	0.0261***	44,660***	10,675***
Treated	0.0205***	81,339***	-4,113***	0.0284***	38,423***	3,953***
Treated · Post (δ)	0.00313	41,201***	2,050	0.00107	27,061***	1,613**
Constant	0.656***	241,736***	52,354***	0.679***	192,827***	52,260***
N	10,356	9,763	10,304	50,652	49,310	50,381
HH No Fiber	5,876,121	5,876,121	5,876,121	50,061,637	50,061,637	50,061,637

* p<0.10, ** p<0.05, *** p<0.01

The Brattle Group Study's regression results are presented in Table 1. The effects on the employment-to-population ratio in "urban" and "non-urban" areas are both statistically no different than zero. As for home prices, the results suggest home prices in fibered areas increased by $41,201 while in non-urban areas prices increased by $27,061, both relative to the counterfactual. The Brattle Group Study reports that fiber increased home values by 14% to 17% based on these values, but the calculation is incorrect. The 17% for urban areas divided the δ coefficient by the α_0 coefficient [= 42,201/241,736], whereas the actual counterfactual is ($\alpha_0 + \alpha_1 + \alpha_2$), so the implied percentage price increase is actually 9.6% in urban areas and 4% in non-urban areas (versus 14%), so the Brattle Group Study grossly overstates the percentage effects by using the incorrect counterfactual. (As described later, the incorrect functional form also leads to a large overstatement of the effect size.) The δ coefficient for household income in urban areas is not statistically different from zero (the null hypothesis of "no effect" is not rejected), though positive and statistically different from zero in non-urban areas. Of the six estimated DID coefficients, only half of them are statistically different from zero.

Another concern arises because the Brattle Group Study does not indicate whether the samples are balanced or unbalanced. The sample sizes do vary across outcomes and this discrepancy suggests these data may be unbalanced. If these data are indeed unbalanced, then the regression specification in Equation (2)—which relies on separate treated and post dummies plus their interaction—may produce biased coefficient estimates compared to a two-way fixed effects regression approach. This bias arises through several mechanisms. First, when observations are missing non-randomly, the simple treated and post dummies cannot adequately control for group-specific or time-specific factors. If the missing observations differ systematically from those observed (*e.g.*, they represent particularly large or small values relative to group means), then this introduces a type of selection bias. Second, the "post" and "treated" dummies may permit distortions in calculating the control and treatment group means in either the pre- or post-periods. Such distortions directly affect parameter estimates and can produce spurious treatment effects. Absent proper documentation of the data, the Brattle Group's findings should be interpreted with caution.

III. Data

The Brattle Group Study provides scarce details about the data used in its analysis. As for the ACS data, with outcomes measured at the census tract level, it must be the case (though unstated) that the five-year ACS data are used. At a minimum, estimating the regression equation requires at least one five-year sample ending in or before 2016 and at least one five-year sample after 2016, with the latter excluding any data from years prior to 2017 (the first treatment year). The five-year sample from 2021 begins in 2017, so is a sensible choice.

To replicate the Brattle Group Study, I use three non-overlapping years of five-year ACS data including 2011 (2007-2011), 2016 (2012-2016), and 2021 (2017-2021).[16] The 2011 data allow for the assessment of common trends, which is supported if the change in the outcomes is equal between the control and treatment groups between 2011 and 2016. Following the Brattle Group Study, three outcomes are extracted from the ACS at the census tract level: (1) the employment-to-population ratio (ages 20-64); (2) median home values; and (3) median household income.[17] The Brattle Group Study describes the employment-to-labor force ratio is the more "commonly cited" statistic but do not use it even though these data are available.[18] I include this ratio in my analysis. Also, data on median rents are included in the analysis, since rising home values are associated with higher rents. A crosswalk between 2010 and 2020 census tract cartography is used to link these data from before-and-after the 2020 census.[19] Alaska and Hawaii are excluded as they are unique markets. Some of these data are top- and bottom-coded and I remove any data at these boundaries.

Fiber deployment data from Form 477 data are collected for years 2016, 2018, and 2021, and collapsed to the census tract level using the FCC's population data for years 2016, 2018, and 2020.[20]

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16 The 2023 ACS data were released on December 12, 2024, so they were unavailable for the Brattle Group Study.

17 Data available at: https://data.census.gov. The series are S2301, B25077, and S1901.

18 Brattle Group Study, *supra* n. 6 at Table 2.

19 Data available at: https://www.nhgis.org/geographic-crosswalks#download. Tracts from 2020 are collapsed to 2010 tracts using household weights.

20 Data available at: https://www.fcc.gov/staff-block-estimates. December data are used for 2016 and 2018 and June data for 2021 since 2010 census cartography was used in June-2021 but not December-2021.

Any census tract with fiber in 2016 is dropped from the sample. Following the Brattle Group Study, all census tracts with *no fiber* in 2016, 2018, or 2021, but with positive amounts of cable modem service, constitute the control group.[21] Census tracts with positive amounts of cable modem service and with fiber coverage of 15% or more in 2018 (and at least as much in 2021) are marked as treated (receiving the treatment in either 2017 or 2018). The final sample is restricted to the control and treatment groups and only tracts with data in all years are included for a balanced panel.

Table 2. Urban, Non-Urban Populations

	Urban	Non-Urban
Census Bureau 2010	80.7%	19.3%
Census Bureau 2020	80.0%	20.0%
RUCA 2010; WRHRC	84.5%	15.5%
FCC 14th Broadband Report	79.2%	20.8%
Brattle Group Study	16.9%	83.1%

The 2010 Rural-Urban Commuting Area codes ("RUCA") is used to mark urban and rural tracts.[22] The method proposed by the University of Washington's Rural Health Research Center ("WRHRC") is used to define urban and rural areas based on the secondary RUCA code.[23] Table 2 summarizes the urban-rural shares from different sources and methods.[24] All the urban shares are quite similar across sources at about 80% urban. The Brattle Group Study is an outlier, with only 17% of the population in urban areas. Such a stark contrast from standard methods warrants explanation but none is provided. For the RUCA, about 71% of census tracts are assigned to the most metropolitan classification (RUCA = 1), so it is difficult to justify a "urban" definition containing only 17% of the population. The USDA defines "urban" as a population density of 1,000 persons per square mile and adjoining an area with at least 500 persons per square mile.[25]

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21 Brattle Group Study, *supra* n. 6 at p. 12 ("we define the control group as census tracts that do not have fiber access in any year of the sample").

22 Data available at: https://www.ers.usda.gov/data-products/rural-urban-commuting-area-codes/documentation.

23 *Using RUCA Data*, Rural Health Research Center, University of Washington (last visited December 9, 2024) (available at: https://depts.washington.edu/uwruca/ruca-uses.php).

24 Data available at: https://www.census.gov/newsroom/blogs/random-samplings/2022/12/redefining-urban-areas-following-2020-census.html; https://www.census.gov/programs-surveys/geography/guidance/geo-areas/urban-rural/2020-ua-facts.html; https://www.hrsa.gov/rural-health/about-us/what-is-rural/data-files; https://docs.fcc.gov/public/attachments/FCC-21-18A1.pdf (at. Fig 1).

25 *Rural Classifications – What is Rural?*, US Department of Agriculture Economic Research Service (January 8, 2025) (available at: https://www.ers.usda.gov/topics/rural-economy-population/rural-classifications/what-is-rural#:~:text=In%20general%2C%20urban%20areas%20had,designating%20census%20blocks%20as%20urban.).

Rather than departing entirely from the Brattle Group Study's "urban" measures, I construct a three-category urban variable. Similar to the Brattle Group Study, I define the most dense areas as urban areas (as defined by the RUCA) with population density (in 2021) of at least 7,500 persons per square mile. The least dense areas are defined as rural areas by the RUCA code. A middle-density group is the remainder.

Table 3. Sample Statistics by Urban Classification

	Very Urban (*U*)	Middle Urban (*M*)	Rural Areas (*R*)
Tracts	1,359	9,238	2,355
Treatment Share	59.4%	46.2%	16.5%

Summary statistics for the sample constructed here are provided in Table 3. The sample size for each year is large with 12,952 total census tracts for each period (and three times that for the pooled sample). There are 5,446 (42.2%) treated census tracts and 7,486 (57.8%) control tracts. The treatment applies more often in the urban classifications, as expected, indicating some selection bias is the assignment of the treatment.

IV. Empirical Analysis

A regression model is specified that permits the estimation of the effect sizes for urban and rural areas jointly along with an assessment of parallel paths. For the three urban classifications there are treatment indicators for the treatment groups in the post-treatment period D_i^U, D_i^M and D_i^R and three false treatment indicators F_i^U, F_i^M and F_i^R. The two-way fixed effects regression model is,

$$\begin{aligned} Y_{it} = {} & \delta_U D_i^U + \delta_M D_i^M + \delta_R D_i^R + \gamma_U F_i^U + \gamma_M F_i^M + \gamma_R F_i^R + \\ & + \mu_i^U + \left(\lambda_t^U + \lambda_t^M + \lambda_t^R\right) + \varepsilon_{it} \end{aligned} \tag{3}$$

where the μ_i and λ_i are tract and year (by group) fixed effects for each group and ε_{it} is the disturbance term. Standard errors are clustered at the tract level. The δ_k coefficients are the DID estimators and the γ_k coefficients measure whether the control and treatment groups follow a common path prior to the treatment. If the γ_k coefficients are non-zero, then the parallel paths assumption is unsupported.

A. *Additive vs. Multiplicative Effects*

Regarding home values and household income, the regression models in the Brattle Group assume additive rather than multiplicative effects. The additive effect assumption, when the effects are in fact multiplicative, can lead to severely biased estimates. Looking back to Table 1, the coefficient on the *Post* variable (the mean change in home values between the two periods) is $107,333 in urban areas. The additive effect assumption implies that between the pre- and post-treatment periods home values rose by $107,333 whether the median home value was initially $10,000 or $1,000,000, which is implausible.

To evaluate whether the effects are additive or multiplicative, the change in (nominal) home values between 2016 and 2021 are calculated at varying values in 2016 and are presented in Table 4.[26] For homes valued between $0 and $100,000 in 2016, the mean increase in value is $21,801 on a base of $74,451 (a 29.3% increase). For homes valued between $100,000 and $200,000 in 2016, the base value is $144,681 and change in value is about twice as large as the first group (a percentage change of 29.3%). Homes valued between $300,000 and $400,000 in 2016 have a mean base value about five times larger than the first group, and the increase in value between the two periods is likewise about five times larger (a change of 30.3%). Finally, for values between $700,000 and $800,000, with a base value about ten-times larger than the first group, the change in value is $194,829, which is also about ten-times larger than the first group (a 30.2% change). The level changes vary in rough proportion (about 30%) to the base value. Plainly, the change in home values is multiplicative and not additive. A natural log transformation of the dependent variable is a better specification.

Table 4. Additive or Multiplicative Effects

Median Home Value Range	Change in Value (2016 to 2021)	Value 2016	% Change
$0 to 100,000	21,801	74,451	29.3%
$100,000 to 200,000	42,449	144,681	29.3%
$300,000 to 400,000	74,361	245,556	30.3%
$400,000 to 500,000	106,044	347,078	30.6%
$500,000 to 600,000	134,150	445,031	30.1%
$600,000 to 700,000	152,018	550,684	27.6%
$700,000 to 800,000	194,829	644,176	30.2%
$800,000 to 900,000	203,347	745,735	27.3%
$900,000 to 1,000,000	233,403	846,487	27.6%

A Monte Carlo simulation of a DID model illustrates the problem with treating multiplicative relationships as additive. Say there are two equal sized groups (treated, control) and two periods (pre, post treatment) with 1,000 observations in each period. A y variable is generated by exponentiating a random normal variable $N(12, 0.5)$ to mimic the home value data from the ACS. Treated units are assumed to have 30% higher mean y than the control group and y increases 30%

26 These data include all available ACS data and not just the restricted sample.

in the second period for both groups. The treatment effect is zero. These data are generated 1,000 times and the model is estimated with y and $\ln(y)$ as the dependent variable and the results are recorded. A 5% alpha level is assumed. When $\ln(y)$ is the dependent variable, the treatment effect is estimated to be zero with a 5.3% rejection rate, so a model with $\ln(y)$ provides the correct result. Alternately, estimating the model using the level of y as the dependent variable produces a treatment effect of about $21,352 with a rejection rate of 100%. Assuming additive effects when they are multiplicative leads to a biased estimate of the treatment effect and grossly overstates the rejection rate. This simulation suggests the DID estimates from the Brattle Group Study, which assumes additive effects for home values and median income, are biased and too large.

B. *Fiber's Effect on Economic Outcomes*

Using the data described above, Equation (3) is estimated using the non-overlapping five-year ACS data for years 2011, 2016, and 2021. The δ coefficient is the DID estimator and the γ coefficient is an assessment of parallel trends. To support parallel trends, γ should equal zero; if not, then the δ coefficient is unidentified and biased. These data include all tracts in the sample ($N = 89{,}481$) and estimates for urban and rural areas are provided in Table 5. Two additional outcomes are included: (1) the employment-to-labor-force ratio, which the Brattle Group Study describes as more traditional than the employment-to-population ratio; and (2) the median gross rent for rental properties. For median home values, household income, and median rent, the natural log transformation is applied so $e^{\delta} - 1$ measures the percent change in the outcome from the fiber treatment. All the F-statistics for the model are statistically significant at the 1% level.

Table 5. Regression Results

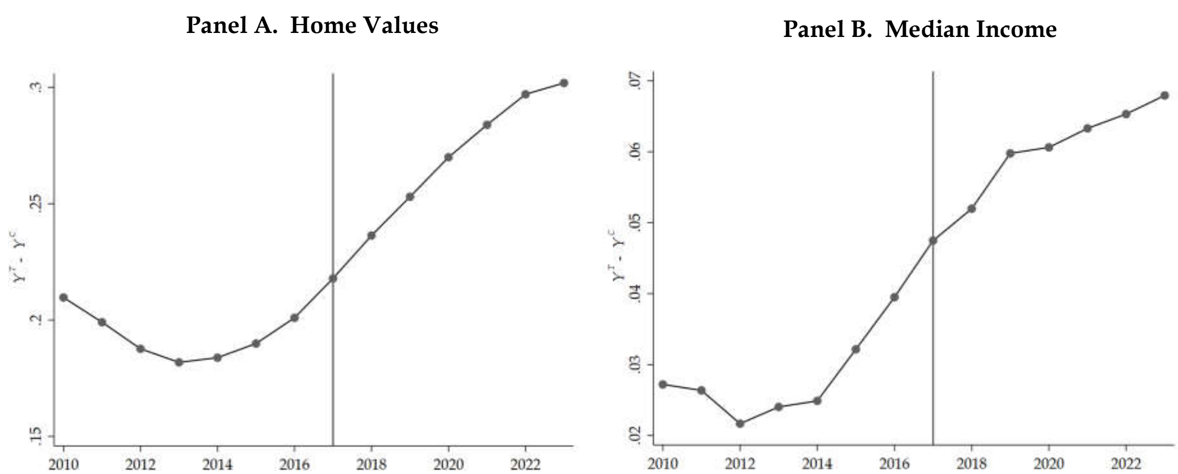
	Very Urban		Middle Urban		Rural	
Outcome	δ	γ	δ	γ	δ	γ
Emp-Pop Ratio	0.0032	0.0175***	0.0051***	0.0067***	0.0020	0.0125**
	(0.88)	(3.21)	(3.33)	(3.49)	(0.48)	(2.52)
Emp-Labor Force Ratio	-0.0006	0.0033	-0.0006	0.0078***	-0.0073***	0.0111***
	(0.20)	(0.81)	(-0.52)	(5.72)	(-4.69)	(3.22)
ln(Median Home Value)	0.0956***	0.0547***	0.0683***	0.0080**	0.0106*	0.0231**
	(7.71)	(4.32)	(16.20)	(2.06)	(1.93)	(2.36)
ln(Median Rent)	0.0330***	0.0345***	0.0322***	0.0056	0.0066	0.0111***
	(4.30)	(5.05)	(9.04)	(1.55)	(1.21)	(1.34)
ln(Median HH Income)	0.0335***	0.0384***	-0.0145***	0.0134***	-0.0149**	0.0153*
	(2.89)	(3.44)	(3.63)	(3.75)	(-2.69)	(1.69)
N = 38,856	*n* = 4,077		*n* = 27,714		*n* = 7,065	

* p<0.10, ** p<0.05, *** p<0.01

The problems with the DID analysis are plain—almost all the γ coefficients, which measure the changes in the outcomes between the treatment and control tracts prior to the treatment, are statistically different from zero. In only two cases are the γ coefficients statistically insignificant, including once when the δ is also not statistically significant and once for median rent (though the t-statistic is 1.55). The evidence weighs heavily against the parallel paths assumption, as the outcomes between the treatment and control groups deviated prior to the treatment. The control group is not a good counterfactual for the treatment group and the δ coefficients are not identified and presumably a biased measure of the treatment effect. The result is unsurprising. Selection bias would be expected with fiber deployment; fiber is not randomly deployed (it is deployed more heavily, for example, in more densely populated areas).[27] Given the statistical significance of the γ coefficients, none of these δ coefficients are reliable measures of the treatment effect, so a discussion of the results is pointless.

The parallel paths problem is seen clearly by looking at the log differences in means (percentage) between the two groups by year for all the available five-year ACS data (years 2010-2023). Since these five-year estimates overlap, these values represent a five-year moving average. Note that the post-treatment data are not clear of the pre-treatment years until 2021.

Figure 1. Trends in Home Values Differences



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[27] T.R. Beard and G.S. Ford, *Digital Discrimination: Fiber Availability and Speeds by Race and Income*, PHOENIX CENTER POLICY PAPER No. 58 (September 2022) (available at: https://phoenix-center.org/pcpp/PCPP58Final.pdf); T.R. Beard, G.S. Ford, and L.J. Spiwak, *Digital Discrimination Under Disparate Impact: A Legal and Economic Analysis*, 48 TELECOMMUNICATIONS POLICY 102853 (2024).

Figure 1 illustrates the difference in log median home values and median income for the entire sample.[28] When satisfying parallel paths, the trend through 2016 (all years prior to the treatment) should have zero slope (a flat line). Yet, the figure shows that the percentage difference in homes values has a positive slope in the pre-treatment years beginning in 2013, so the median home values between the treatment and control groups are departing from each other long before the fiber treatment, and the trend continues consistently across the treatment years. The same is true for median incomes, with a positive trend beginning around 2013. The parallel trends assumption is not supported, and nothing remarkable happens around the treatment date.

C. *Addressing Parallel Trends*

One empirical approach to address a lack of parallel trends identified as above is to include a unit-specific time trend in the regression.[29] The approach allows each unit (*e.g.*, census tract) to have its own linear trend that would have continued absent treatment. Arguably, these trends can adjust for pre-existing differential trends between treatment and control groups, but there are shortcomings of this approach. First, it assumes that any pre-existing non-parallel trends were linear and would have continued linearly absent treatment. If the true counterfactual trends are non-linear, then this assumption could be violated. Second, including unit-specific trends makes the model more demanding of these data in that there needs to enough pre-treatment periods to credibly estimate these trends. Third, the approach can reduce statistical power and make estimates more sensitive to model specification.

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28 A similar pattern, with a rising trend around 2013, is observed among the three urban groups.

29 J.D. Angrist and J. Pischke, MASTERING METRICS (2009) at Ch. 5.

Table 6. Regression Results with Tract Trends

	Very Urban		Middle Urban		Rural	
Outcome	δ	γ	δ	γ	δ	γ
Emp-Pop Ratio	-0.0330**	0.0054*	-0.0113**	-0.0010	-0.0156	-0.0014
	(-2.43)	(1.81)	(-2.33)	(-0.96)	(-1.26)	(-0.50)
Emp-Labor Force Ratio	-0.0124	-0.0028	-0.0146***	-0.0005	-0.0137***	-0.0003
	(-1.24)	(-1.20)	(-4.02)	(-0.53)	(-1.52)	(-0.17)
ln(Median Home Value)	-0.0042	0.0033	0.0560***	0.0067***	-0.0076	-0.0103*
	(-0.17)	(0.45)	(7.30)	(3.28)	(-0.36)	(-1.84)
ln(Median Rent)	-0.0068	0.0049	0.0265***	0.0021	0.0098	0.0091
	(-0.43)	(1.10)	(3.15)	(0.86)	(0.46)	(1.31
ln(Median HH Income)	-0.0071	0.0086	-0.0111	0.0009	-0.0078	0.0058
	(-0.28)	(1.20)	(-1.25)	(0.38)	(-0.35)	(0.889)
N = 64,760	*n* = 6,795		*n* = 46,190		*n* = 11,775	

* p<0.10, ** p<0.05, *** p<0.01

Considering these caveats, Equation (3) is augmented with tract-specific time trends for urban and rural areas. To provide a better estimate of the trend, the pre-treatment data includes data from 2013 through 2016 (expanding the sample size), and the treatment period data are from 2021. As shown in Figure 1, a linear trend is perhaps plausible between 2013 and 2016. The expanded pre-treatment data sets the trend on smoothed data. Results are summarized in Table 7.

These results present a very different story than that offered by the Brattle Group Study. For the employment-to-population ratio, in both urban groups the δ coefficient is negative and statistically different from zero; fiber reduces employment, though the γ coefficient for very urban areas is marginally statistically significant (at the 10% level). Switching to the employment-to-labor-force ratio, the δ coefficient is negative and statistically significant for the middle urban group and its γ coefficient is zero. As for home values, there is no fiber premium in either the very urban or rural groups, and the γ coefficients are statistically insignificant at the 5% level or better. Home prices rise in the middle urban group, but the γ coefficient remains statistically different from zero. Rents rise in the middle urban group and parallel trends have some support. None of the δ or γ coefficients are statistically different from zero for median incomes, so the added trends appear to have resolved the pre-trends issue. These results do not provide a strong (or even weak) case for the expense of deploying fiber networks, and concerns about parallel paths remain in some cases.

D. *Summary*

The purpose of this analysis is to address what I see as several problems with the Brattle Group Study and to see if the Study's results hold up. They do not. There is no reason to expect fiber to have much effect in areas where cable model service is already available (a sample restriction imposed in the Brattle Group Study) as cable modem service meets the needs of almost all users. Where broadband is not available, then we might expect some effects from broadband deployment, but that is a response to broadband service generally and not fiber specifically. Selection bias is likely a bigger problem between areas that have and do not have broadband than between areas with and without fiber, thus complicating empirical analysis. Of course, the results presented here are specific to these data and empirical methods used, and I have used the same data sources and empirical methods as did the Brattle Group Study, albeit correcting for misspecification and a non-traditional definition of "urban," as well as evaluating parallel trends. The Study's discussion of spillovers suggests SUTVA may not apply to these data. Working with Form 477 data, especially when collapsing these data from the native block data to census tracts, requires numerous choices, and my approach may differ from that of others.[30]

V. Conclusion

Given the billions of dollars in BEAD subsidies on the line, the recent Brattle Group Study may be perceived as an attempt by the fiber industry to dissuade the Trump Administration from abandoning the Biden Administration's stated preference for fiber. The Study reports a $3.24 trillion fiber bonus to the nation, a number large enough to support the policy but also to draw skepticism. In this BULLETIN, I have reviewed the Brattle Group's empirical analysis to evaluate its veracity. Reproduction was precluded by the Brattle Group Study's nearly complete lack of details about these data. In my view, the Brattle Group Study contains several material empirical problems—these data are poorly described, the parallel paths assumption is not evaluated (despite mentioning the requirement), effects are assumed as additive rather than multiplicative, the regression is mis-specified if the panel is unbalanced, a failure to consider spatial regression models, a peculiar measure of urbanicity, the likely violation of SUTVA, among other concerns.

Here, I report estimates of the effects of fiber availability using an empirical approach consistent with that of the Brattle Group and include sufficient data to evaluate the parallel paths assumption. The parallel paths assumption is unsupported; the outcomes between the treatment and control groups all deviate prior to the treatment. An attempt to address parallel paths suggests the effect of fiber is likely zero on home values and incomes and may be negative for employment. With several empirical shortcomings of the Brattle Group's methods left unaddressed, further empirical analysis of this issue is warranted. For now, policymakers should explore diverse approaches beyond fiber networks for delivering broadband to Americans,

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30 The census block data are first collapsed by block and then to the tract level using population weights. Another approach would be to use household weights. Using broadband data after June-2021 would require a crosswalk between census cartography, which requires further adjustments.

avoiding subsidizing fiber when modern cable and other networks are available and including examining the potential of next-generation satellite technologies.