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WHEN NARRATIVES REPLACE EVIDENCE: EMPLOYMENT CLAIMS AND THE EPB MUNICIPAL BROADBAND NETWORK

Abstract: This POLICY BULLETIN evaluates claims that Chattanooga’s EPB municipal broadband network generated large employment gains in Hamilton County, Tennessee. First, this BULLETIN shows that the source paper invoked to justify the employment narrative explicitly finds no statistically significant employment effects in urban counties; Hamilton County is an urban county. Second, using modern causal inference methods, this BULLETIN directly evaluates labor-market outcomes in Hamilton County. Across employment and unemployment outcomes, no economically or statistically significant effects are found. Third, analysis of County Business Patterns data finds no evidence that the EPB network increased business establishment growth in Hamilton County. Finally, a replication of the original broadband study using appropriate staggered-adoption methods yields small, statistically insignificant effects of worsening employment outcomes. The evidence consistently indicates that EPB’s municipal broadband deployment did not generate measurable employment benefits in Hamilton County. When public borrowing, ratepayer cross-subsidization, and federal grants are justified using employment claims, identification errors are not academic—they translate directly into misallocation of public capital.

I. Background

In 2010, EPB—the municipal electric utility of Chattanooga, Tennessee—began the construction and operation of a fiber-optic network to serve the entirety of Hamilton County, Tennessee. As one of the largest and earliest municipally-owned fiber optic networks serving households, the EPB network became a “poster child” for government-owned broadband

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networks (“GONs”).¹ Also, EPB appears to have evaded the financial stress common to GONs, in part because the construction of the \$390 million municipal network received \$111.6 million in federal subsidies from the American Recovery and Reinvestment Act of 2009, a \$229 million subsidy from the city’s electric utility, and another \$50 million in loans from the electric utility.² Much of the Chattanooga municipal broadband network was, and in part continues to be, financed by U.S. taxpayers and EPB’s captive electric ratepayers.³

Given the huge investment funded by public borrowing, federal subsidies and cross-subsidies, a fundamental question is whether the investments made by taxpayers and ratepayers in the network produced any benefits to the county’s economy. EPB and city officials often tout the purported economic benefits of the network,⁴ and every few years EPB hires Bento Lobo, a finance professor at the University of Tennessee-Chattanooga, to quantify the economic benefits of the network.⁵ Lobo’s most recent report—released in December 2025 and co-authored with EPB employee William Plank—continues this tradition, claiming the municipal network has created or saved over 10,420 jobs over fifteen years.⁶ This report was a follow up to Lobo (2020) that made similar claims.⁷ These analyses do not look at employment trends in Hamilton County,

¹ G.S. Ford, *Why Chattanooga is not the “Poster Child” for Municipal Broadband*, PHOENIX CENTER POLICY PERSPECTIVE NO. 15-01 (January 20, 2015) (available at: <https://www.phoenix-center.org/perspectives/Perspective15-01Final.pdf>).

² G.S. Ford and R.A. Seals, *The Rewards of Municipal Broadband: An Econometric Analysis of the Labor Market*, 45 TELECOMMUNICATIONS POLICY 1-12 (2021); T.R. Beard, G.S. Ford, L.J. Spiwak, and M. Stern, *The Law and Economics of Municipal Broadband*, 73 FEDERAL COMMUNICATIONS LAW JOURNAL 1-98 (2021).

³ See G.S. Ford, *Electricity Rates And The Funding Of Municipal Broadband Networks: An Empirical Analysis*, 102 ENERGY ECONOMICS (October 2021) (available at: <https://www.sciencedirect.com/science/article/pii/S0140988321003613?dgcid=author>).

⁴ J. McGee, *Chattanooga Mayor: Gigabit Speed Internet Helped Revive City*, THE TENNESSEAN (June 14, 2016) (available at: <https://www.tennessean.com/story/money/2016/06/14/chattanooga-mayor-gigabit-speed-internet-helped-revive-city/85843196/?gnt-cfr=1&gca-cat=p&gca-uir=true&gca-epi=z115734d00----v115734d--64--b--64--&gca-ft=265&gca-ds=sophi>). G.S. Ford, *Questionable Economic Benefits Of Chattanooga’s Gig*, THE TENNESSEAN (August 16, 2016) (available at: <https://www.tennessean.com/story/opinion/contributors/2016/08/17/questionable-economic-benefits-chattanoogas-gig/88908270>); see also E. Rasure, *U.S. Recessions Throughout History: Causes and Effects*, Investopedia (March 20, 2025) (available at: <https://www.investopedia.com/articles/economics/08/past-recessions.asp>).

⁵ B.J. Lobo, *Ten Years of Fiber Optic and Smart Grid Infrastructure in Hamilton County*, Report funded by EPB (August 31, 2020) (available at: https://assets.epb.com/media/Lobo%20-%20Ten%20Years%20of%20Fiber%20Infrastructure%20in%20Hamilton%20County%20TN_Published.pdf); B.J. Lobo and W. Plank, *From Gig City to Quantum City: The Value of Fiber Optic Infrastructure in Hamilton County, TN 2011-2035*, Report funded by EPB (November 12, 2025) (available at: https://static.epb.com/media/documents/Lobo-Plank_2025_-_Nov_12_2025.pdf).

⁶ *Id.*

⁷ *Id.*

but are based on a paper by Lobo, Alam and Whitacre published in TELECOMMUNICATIONS POLICY in 2020 that claims to estimate the effect of high-speed broadband (100 Mbps) on unemployment rates in Tennessee over the years 2011 through 2015.⁸ However, the employment claims in Lobo (2020) and Lobo and Plank (2025) directly conflict with the cited research that shows no employment (or unemployment) benefits in urban counties.

In this BULLETIN, I briefly discuss how Lobo (2020) and Lobo and Plank (2025) misrepresent the findings in this earlier work. The results from Lobo, Alam and Whitacre (2020) (“LAW”) show no statistically significant effect on employment in urban counties, of which Hamilton County is one, with the paper noting explicitly the “(lack of an) urban effect.”⁹ This “no effect” result is confirmed in Ford and Seals (2021), also published in TELECOMMUNICATIONS POLICY, that estimates treatment effects across multiple labor market outcomes in Hamilton County using a Difference-in-Differences (“DID”) framework.¹⁰ Lobo and Plank (2025) do not mention this study.

Both LAW (2020) (indirectly) and Ford and Seals (2021) (directly) find a lack of an employment effect from the EPB network. While there is no empirical conflict to resolve, I add to the evidence by evaluating employment trends in Tennessee counties to determine whether this “lack of an effect” is robust in other empirical methods. The analysis shows no impact of the EPB network on employment or unemployment in the county, consistent with prior published research. Thus, Lobo’s (2020) and Lobo and Plank’s (2025) claims of employment benefits from the EPB municipal broadband network are empirically unsupported, and the claims are inconsistent with the source study upon which they rely. More broadly, to evaluate whether the EPB network boosted business establishment growth, I use County Business Pattern data in a DID framework to quantify whether business establishment growth in Hamilton County increased after the introduction of the EPB network. I find no evidence, across multiple control groups, that the EPB network had a positive effect on business establishment growth.

Finally, given that LAW (2020) used an incorrect specification of their DID model, I replicate the LAW study and apply a model specification suitable to data where the treatment is staggered over time. I find no effect of high-speed broadband treatment in either urban or rural counties. Consistent with the research on staggered treatment interventions, the mis-specified empirical model in LAW (2020) provides (substantially) biased estimates of the treatment effects.

⁸ B.J. Lobo, M.R. Alam, B.E. Whitacre, *Broadband Speed and Unemployment Rates: Data and Measurement Issues*, 44 TELECOMMUNICATIONS POLICY 1-15 (2020).

⁹ *Id.* at p. 11.

¹⁰ Ford and Seals, *supra* n. 2.

II. Lobo's Employment Claims

Regarding employment effects, Lobo and Plank (2025) is largely an extension of Lobo (2020), extrapolating the employment claims of the former another five years. The more recent report claims the EPB network – not broadband services generally – is responsible for creating or saving 10,420 jobs over fifteen years (on a labor force of approximately 202,000 persons).¹¹ The employment effect relies on a 2020 paper by LAW published in TELECOMMUNICATIONS POLICY that analyses the effect on unemployment rates and the availability of high-speed internet services in the state of Tennessee.¹² The empirical approach is based on a Difference-in-Differences (“DID”) model in a uniform treatment timing context (a 2x2 DID model). Uniform treatment timing means that all units are treated at the time, which is not the case in the broadband data used in the analysis. Counties are treated in different years, a reality covered extensively in the paper.

Around the time the LAW (2020) paper was published, numerous research papers showed that treating staggered treatment interventions as uniform (occurring at the same time everywhere) leads to incorrect estimates—a bias of unknown size and direction.¹³ That is, a negative effect may very well be positive when properly modeled. There is now a large and growing literature addressing staggered treatment timing, but the LAW (2020) paper cannot be faulted for the modeling error as this research was not yet mainstream at the time. Yet, the fact remains that all estimated effects from the paper are unreliable absent further research (which is provided *infra*).

There are several other questionable methodological choices in LAW's (2020) analysis,¹⁴ and the early broadband availability data used in the study were subject to substantial measurement

¹¹ Data available at: <https://fred.stlouisfed.org/series/TNHAMI5LFN>.

¹² *Id.*

¹³ See, e.g., C. de Chaisemartin and X. D'Haultfœuille, *Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects*, 110 AMERICAN ECONOMIC REVIEW 2964–2996 (2020); B. Callaway and P.H.C. Sant'Anna, *Difference-in-Differences With Multiple Time Periods*, 225 JOURNAL OF ECONOMETRICS 200–230 (2021); A. Goodman-Bacon, *Difference-in-Differences With Variation In Treatment Timing*, 225 JOURNAL OF ECONOMETRICS 254–277 (2021); L. Sun and S. Abraham, *Estimating Dynamic Treatment Effects In Event Studies With Heterogeneous Treatment Effects*, 225 JOURNAL OF ECONOMETRICS 175–199 (2021); J.M. Wooldridge, *Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators*, 69 EMPIRICAL ECONOMICS 175–199 (2025).

¹⁴ The “treatment” used by Lobo and Plank (2025) is defined using an arbitrary dichotomization of a continuous availability variable of 100 Mbps download speeds with a threshold of 50% availability. Dichotomizing a continuous variable in this way is bad form and is discouraged in empirical research. For one reason (among several), this approach assumes that 49.5% availability is materially different than 50.1% availability, which is implausible. Also, the data permit the treatment to switch on and off over time.

error,¹⁵ but I set these issues aside for now since the staggered treatment problem alone renders the results unreliable. For now, I turn first to how the results of the LAW (2020) paper are (improperly) used by Lobo (2020) and Lobo and Plank (2025) to estimate employment effects. Then, I use a proper empirical model to estimate the effects targeted in LAW (2020).

What does the LAW (2020) paper actually find? In analyzing the relationship between broadband availability and the unemployment rate, the paper divides counties into two groups “to study the differential effects of broadband speed on rural and urban counties.”¹⁶ Ignoring the county-level urban/rural distinction, the regression model indicates that the unemployment rate fell by 0.26 percentage points (“pp”), on average, for the high-speed broadband “treated” units. This relationship is a weighted average of urban and rural effects (a “pooled” effect).

When urban and rural counties are evaluated separately, the effect in rural areas is a 0.45pp reduction in the unemployment rate while in urban areas it is only a statistically insignificant 0.066pp reduction (SE = 0.115, $t = -0.57$). LAW (2020) concludes “the average effects for urban counties [] are not statistically significant,” there is a “(lack of an) urban effect,” and that “[b]etter quality broadband appears to have a disproportionately greater effect in rural areas.”¹⁷ Thus, the paper is clear that the employment benefits are restricted to rural counties. Given that Hamilton County is an urban county it is incorrect to claim that EFB boosted jobs in Hamilton County using the study. Yet, in both Lobo (2020) and in Lobo and Plank (2025), the employment effects for Hamilton County (an urban county) are based on the claim that “that high speed broadband reduces unemployment rates by 0.26 percentage points,” citing to the LAW (2020) result, despite the fact that paper explicitly acknowledges the “(lack of an) urban effect.”¹⁸ Moreover, the employment effect size (*i.e.*, number of jobs) is based on the multiplication of the 0.26pp estimate multiplied by the working age population and not the labor force (upon which the unemployment rate is based), thus overstating the jobs effect. Estimating the employment effects in Hamilton County, or any urban county, using the 0.26pp effect size scaled by the working age population is invalid, ignoring the actual findings in LAW (2020) and using the wrong scaling

¹⁵ The authors drop ten counties (about 10% of the sample) from the sample for what the authors describe as “implausible access data,” but they offer no explanation as to why some data in the same sample, collected by the same entity, is “implausible” but the rest of it is plausible.

¹⁶ Lobo, Alam and Whitacre, *supra* n. 8 at p. 12.

¹⁷ *Id.* at pp. 11, 1.

¹⁸ *Id.* at p. 11. The paper also mentions an “early adoption effect,” but this effect does not come from the empirical model; it comes from a naïve comparison of unweighted average unemployment rates between groups including an exclusion a year of data. There are no covariates, fixed effects, nothing. If these results were valid, then there would be no reason for an econometric model. This “early adoption” analysis fails to identify any meaningful effect; it is not clear what it is intended to measure. Also, the early adoption effect is averaged into the estimated coefficients of the empirical model (and could be estimated directly, with an appropriate staggered DID model), so adding it to the coefficients from the empirical model is double counting.

factor. The correct interpretation of LAW (2020) is that the employment effect in Hamilton County is no different than zero.

The zero effect in urban counties—specifically Hamilton County—has additional empirical support. Ford and Seals (2021) provide the first and only large-scale econometric evaluation of the labor-market effects of the EPB network.¹⁹ Using U.S. Census Bureau American Community Survey (“ACS”) microdata and a DID framework augmented with Coarsened Exact Matching (to ensure like-to-like comparisons), Ford and Seals (2021) estimate causal effects across nine labor-market outcomes, including employment, labor-force participation, wages, information technology employment, self-employment, and business income. Across many model specifications, they find no economically or statistically significant positive effects of the municipal broadband network on local labor-market performance. The authors conclude that, in markets where private broadband services are already widely available, municipal broadband overbuilds do not generate measurable employment or wage gains. Lobo and Plank (2025) do not mention these findings, relying instead on an incorrect interpretation of LAW (2020).

To summarize, LAW (2020) find that the employment effect of high-speed broadband in *urban* Tennessee counties is essentially zero. Ford and Seals (2021), likewise, find the employment effect of the EPB network is essentially zero. Yet, both Lobo (2020) and Lobo and Plank (2025) assert employment benefits in Hamilton County by misrepresenting past studies and omitting relevant studies that would challenge their conclusions. Next, additional analyses are provided to evaluate whether these effects (or lack thereof) can be confirmed, and whether the EPB network had any effect on business establishment growth.

III. Employment Trends in Hamilton County

The intent of Lobo (2020) and Lobo and Plank (2025) is to provide an estimate of the employment effect of the EPB network in Hamilton County, Tennessee, where it is located. With modern econometric methods, estimating the unique employment effects for Hamilton County, relative to a counterfactual of other counties, is straightforward. To do so, monthly employment counts for all Tennessee counties are obtained from the Bureau of Labor Statistics (“BLS”) over the years 2001 through 2019 (stopping short of the Covid Pandemic).²⁰ The treatment year is 2011 following Lobo (2020) and Lobo and Plank (2025). Other counties with municipal broadband networks are excluded from the sample.²¹ Annual county-level population data are from the

¹⁹ Ford and Seals, *supra* n. 2.

²⁰ Data available at: <https://www.bls.gov>.

²¹ Data available at: <https://tmepa.org/municipalbroadbandproviders>.

Bureau of Economic Analysis (“BEA”).²² Urban and rural counties are identified using the Rural-Urban Continuum Code (“RUCC”), which is the same classification used in LAW (2020).²³

This analysis focuses on Hamilton County specifically. Since the employment effect in the county cannot be measured without some sort of “but for” measure (a counterfactual) for the county if the EPB network was not deployed. There are several methods to establish a counterfactual. Given that only one county in the sample is treated (Hamilton County), the methods of Synthetic Control (“SC”) and Synthetic Difference-in-Differences (“SDID”) are sensible empirical approaches, as they are designed specifically for this scenario.²⁴ Both methods construct a weighted combination of control counties (those without a GON) to serve as a counterfactual for the treated county, but they differ in how they construct these weights. SC matches the treated unit’s pre-treatment outcome levels and covariates by weighting control units, essentially creating a synthetic twin that mimics the treated county’s pre-treatment trajectory. SDID extends this approach by incorporating both unit weights (like SC) and time weights. This dual-weighting structure makes SDID more robust to bias from time-varying confounders and generally produces more efficient estimates. Both methods rely on the parallel trends assumption, implying that the treated unit would follow the same trend as the counterfactual in the absence of treatment. Hypothesis tests are based on placebo effects, where the distribution of outcomes is constructed by assigning the treatment to the controls to get a distribution of effect sizes.

A. Employment Effect

I begin with the employment effect where the outcome is county total employment divided by 2010 working-age population to account for scale and to fix the denominator to the pre-treatment period to avoid endogeneity concerns regarding the treatment’s effect on population growth.²⁵ The estimated treatment effect is -0.0165 ($t = -0.44$), an effect which is negative, small

²² Data available at: <https://www.bea.gov>.

²³ Data (2013) available at: <https://www.ers.usda.gov/data-products/rural-urban-continuum-codes>.

²⁴ A. Abadie and J. Gardeazabal, *The Economic Costs of Conflict: A Case Study of the Basque Country*, 93 AMERICAN ECONOMIC REVIEW 113-132 (2003); A. Abadie, A. Diamond, and J. Hainmueller, *Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California’s Tobacco Control Program*, 105 JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION 493-505 (2010); A. Abadie, *Using Synthetic Controls: Feasibility, Data Requirements, and Methodological Aspects*, 59 JOURNAL OF ECONOMIC LITERATURE 391-425 (2021); D. Arkhangelsky, S. Athey, D.A. Hirshberg, G.W. Imbens, and S. Wager, *Synthetic Difference-in-Differences*, 111 AMERICAN ECONOMIC REVIEW 4088-4118 (2021).

²⁵ Even looking at the (natural log of) employment count without scaling, there is no employment effect. The effect size is -0.008 ($t = 0.15$) for SC and 0.0175 ($t = 0.36$) for SDID.

and statistically insignificant.²⁶ If anything, the employment-population ratio has fallen since the introduction of the EPB network.²⁷ At the working age population in 2010, the point estimate indicates that the EPB network reduced employment in Hamilton County by 3,749 jobs annually between 2011 and 2019 (about 2% below actual employment), but the wide confidence interval supports the null hypothesis that the network had “no effect” on employment.

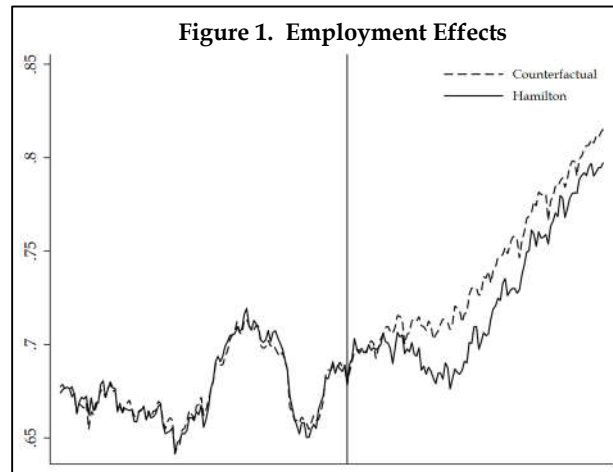


Figure 1 illustrates the employment trends from the SC method. The synthetic control fits the pre-treatment data well (even during a recession), but the outcome in Hamilton County is clearly below the counterfactual after the introduction of the EPB network. The EPB network did not increase employment in Hamilton County; if anything, it slowed employment growth.

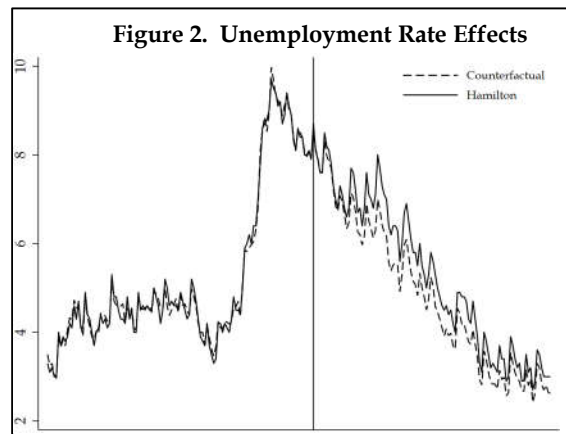
From SDID, which employs a different weighting structure on the counterfactual, the estimated treatment effect is 0.010 ($t = 0.29$), a positive but small and statistically insignificant effect. Again, there was no employment boost from the EPB network. These zero effects for Hamilton County, an urban county, are consistent with both LAW (2025) and Ford and Seals (2021).

²⁶ The mean of the outcome 0.65, so the effect size is about a 2.5% reduction in the outcome. The weighted control counties are: Carroll (0.0005), Davidson (0.6399), DeKalb (0.0009), Decatur (0.0636), Haywood (0.0911), Lake (0.00005), Loudon (0.0395), Marion (0.0695), Marshall (0.0002), Moore (0.0491), Roane (0.0451), and Warren (0.0006). Restricting the analysis to urban counts ($RUCC \leq 3$) provides similar results (-0.0134 , $t = -0.270$).

²⁷ Nearly identical results are obtained when the labor force in 2010 is used to scale the employment data (-0.017 , $t = -0.32$).

B. *The Unemployment Rate*

Turning the unemployment rate ($\times 100$), which was the outcome studied by LAW (2020), the SC method estimates a treatment effect of 0.466 ($t = 1.01$).²⁸ By the point estimate, the unemployment rate rose in Hamilton County relative to the counterfactual after the deployment of the EPB network, though the null hypothesis of “no effect” is not rejected at traditional significance levels. Figure 2 illustrates the results of the SC method. SDID produces a near equal treatment effect as SC (0.422, $t = 0.92$). There is no discernable effect on unemployment after the deployment of the EPB network. Hamilton County’s unemployment rate is comparable to the counterfactual.



Note, however, that the unemployment rate is defined as the share of unemployment persons in the (contemporary) labor force. Since the size of the labor force changes over time, this ratio introduces the potential for endogeneity (the network may increase the size of the labor force). Fixing the labor force at year 2010, the effect size from SC is 0.003 ($t = 0.81$) and from SDID is 0.005 ($t = 1.00$), both tiny and statistically insignificant effects. There is no effect from the EPB network on unemployment rate in Hamilton County.

Using appropriate empirical methods that focus exclusively on Hamilton County (rather than aggregating it with other counties that received high-speed broadband), this analysis shows that the EPB network failed to increase employment (or to reduce unemployment) in Hamilton County. Since Hamilton County is an urban county, the results are consistent with LAW (2020) and likewise match the results in Ford and Seals (2021). Whatever effects the EPB network has had, increasing employment is not one of them. Lobo’s (2020) and Lobo and Plank’s (2025) claims to the contrary have no empirical support and reflect a misapplication of the results in

²⁸ The weighted control counties are identical to the employment model.

LAW (2020), omitting the findings in Ford and Seals (2021), and conducting no new analysis focusing exclusively on Hamilton County.

IV. Business Establishments

A common argument for ultra-high speed broadband networks, at least historically, has been the attraction of new business establishments. To more thoroughly examine the EPB's economic impact, we can also test whether it boosted new business establishments. Using the Census Bureau's County Business Pattern data, the change in the number of business establishments in Hamilton County relative to other counties can be quantified. To do so, I gather from the data the number of business establishments by county in 2005, 2010, 2015, and 2020.²⁹ These are non-overlapping five-year averages from the American Community Survey ("ACS"), where the year indicates the final year of the sample.

Following standard practice in the empirical growth and regional economics literature,³⁰ growth in business establishments (E_{it} for county i at time t) is measured using log differences,

$$\Delta E_i = \ln(E_{it+k}/E_{it}) \quad (1)$$

which measures the continuously compounded (log) growth rate in business establishments between $t + k$ and t where t is 2010 and $k \in \{5, 10\}$. I then regress,

$$\Delta E_i = \beta + \delta H_i + \varepsilon_i \quad (2)$$

where H_i is an indicator for Hamilton County, Tennessee, and δ measures the log-point difference in establishment growth rate relative to the control counties.³¹ This is a DID model following the approach of Lee and Wooldridge (2025).³² The 2005 and 2010 data are used to evaluate the parallel paths assumption with (log) growth measured between 2005 and 2010. In none of the samples is

²⁹ Data available at: <https://www.census.gov/programs-surveys/cbp.html>.

³⁰ See, e.g., R.J. Barro and X. Sala-i-Martin, *ECONOMIC GROWTH* (2004); E.L. Glaeser, H. Kallal, J.A. Scheinkman, and A. Shleifer, *Growth in Cities*, 100 *JOURNAL OF POLITICAL ECONOMY* 1126-1152 (1992); J.M. Wooldridge, *ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA* (2010).

³¹ The percentage change in growth rates is $\exp(\delta) - 1$.

³² S.J. Lee and J.M. Wooldridge, *Simple Approaches to Inference with Difference-in-Differences Estimators with Small Cross-Sectional Sample Sizes*, Working Paper, Department of Economics, Michigan State University (2025); G.S. Ford, *Difference-in-Differences with Few Treated Units: The Lee-Wooldridge Approach in Stata*, Working Paper (September 29, 2025) (available at: <https://ssrn.com/abstract=5544598>).

the null hypothesis of a “zero-treatment effect” rejected (even at the 20% level); the assumption of parallel paths is plausible.

This regression model is applied to various samples, including: (1) all counties in the U.S. for which data is available; (2) all urban counties; (3) all counties with the same RUCC as Hamilton County (RUCC = 2); (4) only Tennessee Counties; (5) urban Tennessee counties (RUCC \leq 3); (6) Tennessee counties with the same RUCC code as Hamilton County; (7) counties in Tennessee-adjacent states; (8) adjacent urban counties; and (9) adjacent counties with the same RUCC code as Hamilton County.

Sample	Years 2010-2015			Years 2010-2020		
	δ	t-stat	Obs.	δ	t-stat	Obs.
1	0.023	0.26	3,133	0.076	0.57	3,133
2	0.005	0.94	825	0.026	0.45	825
3	0.009	0.15	378	0.031	0.28	378
4	0.041	0.59	95	0.061	0.62	95
5	0.021	0.38	42	0.020	0.22	42
6	0.031	0.60	15	0.053	0.77	15
7	0.042	0.60	831	0.084	0.75	831
8	0.016	0.24	343	0.028	0.26	343
9	0.019	0.27	108	0.032	0.30	108

*** 1% ** 5% *10%

Results for growth between year-pairs are summarized in Table 1. The results indicate that the (log) growth rate in establishments in Hamilton County are unremarkable for all samples and year pairs; in no case is the null hypothesis of “equal growth” rejected at anywhere near standard significance levels, whether in large and smaller samples. Due to the longer time span, the δ coefficients are mostly larger for the 2010-2020 period than the 2010-2015 period, but the t-statistics are all small. The null hypothesis of “equal growth” in establishments is never rejected at traditional significance levels. In all, it is “business-as-usual” in Hamilton County since the EPB network was deployed; the EPB network did not spur employment growth, reduce unemployment, or improve business establishment growth.

V. Replication and Re-estimation of the LAW (2020) Model

As noted above, the LAW (2020) study’s results are not reliable given the uniform-timing of the treatment specification of the DID model when the treatments are staggered over time. Since LAW (2020) does not support the conclusion that EPB boosted employment in urban counties (of which Hamilton County is one), and additional research methods in Ford and Seals (2021) also find zero labor market effects, it is worth evaluating whether any of the results in LAW (2020) survive correct estimation methods. Here, I attempt a replication of the LAW (2020) study, which is an interesting exercise in itself given the low-quality of the early broadband deployment data

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and several questionable methodological choices made in that research, and then estimate the treatment effects methods that account for the staggered treatment timing.

A. Data

Data on broadband deployment are obtained from the FCC's Form 477 data. As in LAW (2020), I use the Analyze Tables for the June-2011 through June-2014 data including the December variants of the same data. For December 2014 through December 2018, the block-level data are collapsed to the county level using 2010 population weights (as in LAW). There are few changes to the data: I extend the sample to 2018, I include both June and December data, and I use the Form 477 data for all periods past June-2014, where LAW used data from another source for the later years in the sample because the Form 477 data was yet to be released. As such, some changes in the estimates are expected, though they should not be large.

It appears, though it is not stated, that LAW used the June data only. In studying the data this choice is problematic because the June-2011 data appears to contain substantial measurement error; many counties that have very high availability rates for 100 Mbps broadband in December-2011 have none in June-2011, suggesting missing data in June-2011 (the first release of the data).³³ With these data being used to identify treated counties, the poor-quality data is a serious concern, with counties counted as untreated in June-2011 that were actually treated at the time. In fact, LAW acknowledged some peculiarities in the data, leading the authors to drop ten counties from the sample for "implausible data."³⁴ Yet, all the data are from the same source; LAW offer no explanation as to why some of the data from the same source are "plausible" while other data are "implausible," other than the pattern of broadband availability trends appeared particularly irregular. Despite these concerns, I use the data and drop the same counties for consistency.

As in LAW, a "treatment" is defined when broadband availability at 100 Mbps equals or exceeds 50% of the population. For several reasons, dichotomizing a continuous variable in this way is discouraged in empirical research.³⁵ Dichotomizing continuous variables destroys identifying variation, reduces statistical power, induces measurement error and functional-form

³³ G.S. Ford, *Challenges in Using the National Broadband Map's Data*, PHOENIX CENTER POLICY BULLETIN No. 27 (March 2011) (available at: <https://www.phoenix-center.org/PolicyBulletin/PCPB27Final.pdf>).

³⁴ Lobo, Alam and Whitacre, *supra* n. 8 at p. 6.

³⁵ See, e.g., J. Cohen, *The Cost of Dichotomization*, 7 APPLIED PSYCHOLOGICAL MEASUREMENT 249-253 (1983); R.C. MacCallum, S. Zhang, K.J. Preacher & D.D. Rucker, *On the Practice of Dichotomization of Quantitative Variables*, 7 PSYCHOLOGICAL METHODS 19-40 (2002); P. Royston, D.G. Altman & W. Sauerbrei, *Dichotomizing Continuous Predictors in Multiple Regression: A Bad Idea*, 25 STATISTICS IN MEDICINE 127-141 (2006); D.G. Altman & P. Royston, *The Cost of Dichotomizing Continuous Variables*, 332 BRITISH MEDICAL JOURNAL 1080-1081 (2006); J.D. Angrist & J.S. Pischke, *MOSTLY HARMLESS ECONOMETRICS: AN EMPIRICIST'S COMPANION* (2009); G.W. Imbens & D.B. Rubin, *CAUSAL INFERENCE FOR STATISTICS, SOCIAL, AND BIOMEDICAL SCIENCES: AN INTRODUCTION* (2015).

misspecification, creates arbitrary threshold dependence, and can generate biased and unstable estimates without any compensating benefit. In some cases, a continuous variable may have a “clinical” threshold, or perhaps the variable is already categorized (*e.g.*, income groupings), but these scenarios do not apply here. Here, the dichotomization assumes that a county with 49.9% broadband availability is materially different (untreated) than one with a 50% broadband availability (treated), which is implausible. Also, in Form 477 data, the treatment cuts on-then-off for some counties, an inconsistency that requires special attention. Nonetheless, I continue, marking a county as treated in the first period the threshold is reached.³⁶ In an alternative definition of the treatment, the later data is viewed as more reliable. Working backwards in time, the latest period for which availability falls below 50% ends the pre-treatment period with all prior periods labeled as untreated, irrespective of what earlier data indicates. While various “patches” to the data are imaginable, the threshold dichotomization is questionable with such “imputed” values.

Unemployment data from June and December are from the BLS. Unlike the broadband data, unemployment rate data are available for many years. Data on the demographic variables are from the Census Bureau’s ACS five-year estimates, also available for many years. I presume these five-year samples are the source data for the demographics in LAW (it is unstated) since only the five-year estimates include data for all counties. As in LAW, the demographic variables include the working age population, the share of racial minorities, the share of persons with at least a high school education, and (the natural log of) population density. Note that these data are rolling five-year averages, so the years are merged using the last year of the five-year samples. As rolling averages, these variables have little variation over time, explaining their limited statistical significance in LAW’s results (the fixed effects account for much of the variation). Only population density has much effect.

B. *Estimating of the Wrong Model*

In LAW, the regression model is,

$$y_{it+1} = \delta T_{it} \cdot P_{it} + \beta X'_{it-t} + \mu_i + \lambda_t + \varepsilon_{it} \quad (3)$$

where y_{it+1} is the unemployment rate ($\times 100$) at time $t + 1$, T_{it} indicates treated counties, P_{it} indicates the treatment period, X'_{it-t} is a lagged covariate vector, μ_i is a county fixed effect, λ_t is a time fixed effect, and ε_{it} is a presumably heteroskedastic and autocorrelated disturbance term. Standard errors are thus clustered at the county level. The δ coefficient is a potentially biased treatment effect given the staggered treatment timing.

³⁶ There are several other ways to address data irregularities, of which this is one.

The X_{it} are lagged to address “endogeneity,” though lags rarely sufficient to address that concern, especially when the variables are all persistent over time, as all these are. Lagging the treatment is an odd choice, since the treatment is no longer the treatment at all. The treatment indicator is, in this model, the year after a county reaches the 50% availability of 100 Mbps broadband; the level of availability used to quantify the treatment effect could be any value above 50%. As there is no coherent motivation to lag the treatment, I suspect the lag treatment provided better results than the contemporaneous treatment. Typically, lagged treatments are used to test anticipation effects and parallel paths (both sources of bias). Note that the data here include June and December data, so I use a one-year lag ($t - 2$).

Another difference in my data and LAW’s relates to data management. When LAW lags the treatment it costs them one year of data. There is absolutely no reason for this loss of data. The unemployment and demographic data span decades; only the broadband data are restricted to a few years. The unemployment data could be merged (using leads) with the broadband data without a loss of observations. Surrendering data for no reason is poor data management.

There is also a serious flaw in this specification of the DID model and it relates to staggered interventions. This two-way fixed effects regression framework identifies treatment effects by comparing outcomes between treated and untreated states. However, LAW (2020) include counties that are always treated in the estimation sample. These units contain no identifying variation – treatment status never changes – so they contribute nothing to identification. Instead, they effectively function as controls, and in doing so contaminate the comparison group (the untreated) by violating the parallel trends assumption, which mechanically biases the estimated treatment effects. I proceed, nonetheless.

Table 3. Replication of LAW (2020)

	<i>LAW</i> Coef (se)	<i>Replication</i> Coef (se)	
Pooled δ	-0.263** (0.101)	-0.241 (0.168)	-0.410** (0.162)
Covariates	Yes	Yes	No
Obs	425	1,360	1,360

*** 1% ** 5% * 10%

Turning to my replication effort of the incorrect model, the results for the urban/rural pooled δ coefficient are summarized in Table 3 with LAW’s results provided for reference. The pooled δ is -0.241, which compares favorably with LAW’s estimate of -0.263. However, the null hypothesis of “no effect” is not rejected even at the 10% level (but close, $p = 0.153$) despite the much larger sample size. Note that using y_{it} rather than y_{it-2} , the δ coefficient is -0.157 ($p = 0.382$); lagging the treatment indicator, as LAW does, increases both the coefficient and significance level. Excluding the covariates, which are nearly fixed effects, the effect size jumps to -0.410 with the null

hypothesis rejected at the 5% level. This result—unconditioned on the covariates—is included because it is a large, statistically significant effect, offering the most favorable assessment of the incorrect model specification to compare with a correct estimation method.

	<i>LAW</i> Coef (se)	<i>Replication</i> Coef (se)	
Urban δ	-0.066 (0.115)	0.077 (0.188)	-0.018 (0.191)
Rural $\Delta\delta$	-0.389** (0.157)	-0.438* (0.235)	-0.521** (0.231)
Covariates		Yes	No
Obs	425	1,360	1,360

*** 1% ** 5% * 10%

Table 4 provides the results when dividing the counties into their rural and urban counterparts. Note that the $\Delta\delta$ coefficient is added to the urban δ for the total effect in rural areas. Again, the results align, in large part, with those in LAW. There is no effect in urban areas and a large negative effect in rural counties, with the null hypothesis rejected at the 10% level or better for rural counties. The coefficient in the model with covariates is close to LAW's estimate for rural counties ($\Delta\delta = -0.389$ versus -0.438), but the coefficients are a bit different for the urban counties (though poorly estimated coefficients often vary widely across specifications and samples).

Despite some differences, this replication succeeds on many grounds—the point estimates and their patterns are comparable to those in LAW. The pooled effect, which is used in Lobo (2020) and Lobo and Plank (2025), is close in magnitude though not statistically different from zero. None of these estimated effects, however, tell us much about the effect of broadband on unemployment—they are estimated using the wrong type of model when the treatment timing is staggered over time.

Switching to the alternative treatment indicator (which assumes later data are better), the pooled δ coefficient (with covariates) is -0.096 ($p = 0.55$), the urban δ coefficient is 0.22 ($p = 0.36$) and the $\Delta\delta$ coefficient for rural counties is -0.53 ($p = 0.052$). This alternative treatment definition, which is as legitimate as the other definition, indicates no pooled treatment effect; only the $\Delta\delta$ coefficient is statistically significant (at near the 5% level). Clearly, how one defines the treatment in this setting, especially given the sometimes-erratic movement in availability measures between periods, has a material effect on the results. The Form 477 data, at least in the early years, are ill-suited to a threshold-dichotomization approach, a methodological choice that is typically ill-advised in any case.

C. Staggered Difference-in-Differences

These results from the model in Equation (3) are, however, unreliable. The treatment is staggered and not uniform. Counties are “treated” at different times. To get better estimates (though no estimate is reliable given the arbitrarily dichotomization of the broadband data and errors in the data), I estimate the treatment effects for staggered treatments using the approach of Wooldridge (2025) based on a two-way fixed Mundlak regression.³⁷ This procedure constructs cohort-time causal comparisons that are robust to staggered timing and dynamic treatment effects. A mean aggregate effect is constructed from the results for comparison to the results in LAW and those above. The not-yet-treated counties (including the never treated counties) are the controls. Always treated counties are excluded as they are only a source of bias.

	Coef (se)	Coef (se)
Pooled δ	0.140 (0.27)	0.152 (0.26)
Urban δ	0.041 (0.28)	0.165 (0.26)
Rural $\Delta\delta$	0.139 (0.28)	0.141 (0.27)
Covariates	Yes	No
Obs	1,232	1,232

*** 1% ** 5% * 10%

Results are summarized in Table 5. For the model including covariates, this aggregate mean effect is 0.140 ($p = 0.61$). In the incorrect model (Equation 3), the effect size was -0.241 and statistically significant at the 15% level; the correct effect size is positive, small and nowhere near statistically significant at traditional levels. The bias from the uniform-treatment model is so large that the sign on the treatment effect has changed from negative to positive. The effect in urban and rural counties is comparable (0.141 and 0.139, with $p \approx 0.6$ for both), so the large effect size for rural counties in the uniform-timing model is an anomaly of the incorrect model specification, perhaps due to rural counties being treated later, on average, and the mechanical violation of parallel trends.

Excluding the covariates, where the uniform-treatment model has a large, negative effect and statistically significant (-0.410), the staggered timing model has the mean aggregate effect of 0.152 ($p = 0.57$). Again, the uniform-treatment effect is severely biased and wrong signed. There is no

³⁷ J.W. Wooldridge, *Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Differences-in-Differences Estimators*, 69 *EMPIRICAL ECONOMICS* 2545-2587 (2025).

effect of the broadband treatment on the unemployment rate; if anything, it is positive. The effect size for urban counties is comparable to rural counties (0.165 versus 0.141, $p \approx 0.52$ for both).

This replication exercise yields three clear conclusions. First, I cannot replicate the *statistically significant* impact of the pooled effect of broadband treatment on the unemployment rate, despite using more data. The point estimate is close, but the null hypothesis cannot be rejected even at the 10% level. Second, the replication exercise revealed significant measurement errors in the Form 477 data, especially in June-2011 (and in the other periods as well). The early Form 477 data are unsuitable for this sort of analysis, especially when using an arbitrary dichotomization of the broadband data into a treatment indicator (which introduces several sorts of bias). Third, when estimating the effect sizes using an approach suited to the staggered treatment, the treatment effects are positive, though statistically insignificant. The incorrect specification of a staggered treatment using the uniform-timing model has its expected effects—substantial bias in the estimated treatment effect, so much so the sign of the effect is reversed.

VI. Conclusion

This analysis demonstrates that claims of large employment gains attributable to Chattanooga’s EPB municipal broadband network are not supported by empirical evidence. The employment figures advanced by Lobo (2020) and Lobo and Plank (2025) rest on a fundamental misrepresentation of the underlying academic research they cite, conflating rural effects with urban effects, and ignoring the fact the source study explicitly finds no statistically significant urban effect. In doing so, and for whatever reason, their reports convert a zero result into a positive claim. The latter report ignores conflicting research that does not support their claims.

Using modern causal inference methods specifically designed for single-unit treatments, this BULLETIN directly evaluates the labor market effects of the EPB network in Hamilton County, Tennessee. Across two employment outcomes and two (related) estimation strategies, the results are the same: the deployment of the EPB network did not increase employment and did not reduce unemployment relative to a credible counterfactual. The estimated effects are economically trivial and empirically indistinguishable from zero. The “job creation” narrative surrounding EPB’s network is not an inference drawn from observed labor market data, but a constructed extrapolation built on inappropriate aggregation and misapplied coefficients from a fundamentally flawed empirical study. Additionally, the growth in business establishments in the county is evaluated. The growth in the number of establishments in Hamilton County is average compared to other counties. Business establishment growth in Hamilton County is unremarkable since the EPB network was deployed.

An attempt to replicate Lobo, Alam and Whitacre (2020) met with some success: the coefficients and their patterns are similar, though I do not find a statistically significant effect at traditional levels for the combination of urban and rural counties. Yet, the empirical approach in Lobo, Alam and Whitacre (2020) is unsuited to the data; the treatment is not uniformly assigned

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but staggered over time. Using the same data and an appropriate estimation method, the treatment effect of high-speed broadband on unemployment is mildly positive though statistically indistinguishable from zero. High speed broadband, as defined in the study, did not impact employment, either in urban or rural counties.

This case illustrates a recurring problem in broadband infrastructure policy analysis: advocacy-driven benefit claims that are detached from counterfactual-based causal evaluation and the misapplication of empirical results. Economic development narratives cannot substitute for rigorous empirical identification. When public borrowing, ratepayer cross-subsidization, and federal grants are justified using employment claims, identification errors are not academic – they translate directly into misallocation of public capital.

In sum, the evidence presented here leads to a clear conclusion: whatever other objectives the EPB network may serve, employment or business growth in Hamilton County is not one of them. Claims to the contrary are unsupported by the data, inconsistent with the academic literature invoked to justify them, and contradicted by modern empirical evaluation.

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