P E R S P E C T I V E S

PHOENIX CENTER FOR ADVANCED LEGAL & ECONOMIC PUBLIC POLICY STUDIES

Are Broadband Prices Rising?
The Perils of Naive Price Comparisons

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Affordability has emerged as a defining political issue of the moment,[1] and broadband internet service affordability is no exception—especially with the expiration of low-income broadband subsidy programs such as the Affordable Connectivity Program ("ACP") in 2024. Policymakers and advocates frequently cite current broadband prices as evidence that affordability challenges are worsening, justifying calls for expanded subsidies and regulatory intervention.

But are broadband prices actually rising? Recent analyses of the Federal Communications Commission's ("FCC") Urban Rate Survey ("URS") data have reached starkly different conclusions. For example, John Horrigan of the Benton Institute offered the headline claim that the URS data revealed that broadband prices increased 4.8% in real terms from 2024 to 2025.[2] Horrigan attributes this increase to the changing "composition of the URS sample," where the shift in the 2025 sample toward higher-speed services "pulled the average upward for 2025 relative to 2024." Given the change in sample composition, a simple difference in means of 4.8% is a biased estimate of the price change; that is, the figure does not equal the true price change even in large samples. In fact, the 4.8% differences become a -1.6% difference simply by excluding speed tiers from 2025 that were absent from the 2024 data; Horrigan's claimed price increase is fully the result of sample composition.

Using the same data, Arthur Menko (2025) on behalf of USTelecom reports that real broadband prices fell 8.7%.[3] Menko limits his analysis to download speeds between 100 and 940 Mbps, restricting the sample of prices to FCC-defined "broadband services" (≥ 100 Mbps) and to the speed tiers typically purchased by consumers. Menko also adjusts prices to account for provider market shares and modality shares and limits the sample to the larger cable and telecom providers. While excluding portions of the URS data, this is a reasonable approach given the changing sample composition and this focus produces an estimate of what most typical consumers are likely paying for service.

Horrigan's headline statistic of a 4.8% price increase is based on two errors: (1) a lack of common support in speed (compositional shifts); and (2) an improper price comparison methodology (arithmetic rather than geometric means). Fixing both errors indicates prices fell or were stable (statistically speaking) between years.

In large part, this divergence in estimates of price changes stems from a fundamental measurement challenge well-known to economists. When the mix of available broadband services changes rapidly—with providers adding faster tiers, retiring slower plans, and upgrading speeds at existing price points—simple price comparisons become misleading. This fact is well-documented for broadband services by several researchers.[4] Higher average prices may reflect consumers purchasing better services rather than price increases for comparable offerings. Determining whether broadband is becoming less affordable requires carefully distinguishing between these two forces: true price changes for like-for-like services versus sample compositional shifts in the products consumers can purchase.

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Moreover, the FCC's data are incomplete in that they exclude the low-income discount plans offered by many broadband providers. For example, Comcast's Internet Essentials program provides a 75 Mbps plan for $14.99.[5] AT&T's "Access" program offers speeds up to 100Mbps (or better for fiber) for $30 per month, with no contract, no deposit, and no installation fees.[6] Verizon's Forward program offers $30 plan for varying speeds.[7] Consequently, the URS data cannot be used to evaluate price offers for low-income households, either for price changes or availability.

In this PERSPECTIVE, I use the FCC's URS data to compare prices between 2024 and 2025 by applying various empirical methods and by constructing a Laspeyres-style price index to hold the "quality" of service constant. I find no evidence of price increases in real broadband prices. Estimated price changes across several methods find statistically significant price declines of about 7% to 9%—a material decline in prices over a single year. Thus, when analyzing the URS data, sample composition demands a researcher's full attention. Here, a matched cell method is used to estimate the pure price effect, and it is this effect that is most relevant for policy analysis.

... year-to-year changes in observed average prices from URS reflect not only changes in the prices of comparable services but also shifts in the mix of services available to consumers. Distinguishing between these forces is essential for drawing meaningful inferences about underlying price trends.

Data

The sample used here includes the FCC's URS data for years 2024 and 2025, which is a pooled cross section.[8] While the FCC describes the survey design properties, the agency fails to include most of the relevant variables reflecting this design.[9] The URS does include an importance weight variable to "ensure the contributions of each response properly represent the service plans that consumers in urban tracts possibly receive."[10]

For the most part, the primary sampling unit (not provided) consists of provider, technology, and census tract. The tract indicators in the sample are masked (perhaps due to proprietary concerns) limiting the inclusion of additional tract level data and making year-to-year linkages of tracts infeasible. Stratum are described but not provided. While the sample weight is sufficient to obtain point estimates of price changes, the associated standard errors must reflect sample design to be useful. Absent the full details on sample design, I cluster standard errors to approximate, as best as may be, the URS's sample design by clustering the standard errors. In portions of the analysis, state-level geography is used because tracts cannot be linked between years due to the masking of tract codes. These constraints are entirely consequences of the information provided by the sample and its supporting documentation.

Price is measured as Total Charges, including the Monthly Charge and any additional required charges. The Consumer Price Index ("CPI") (for June of each year) is used to convert nominal price (total charge) to real price (base 2025).[11] My analysis looks only at real prices. Notably, the URS data do not include the low-income discount plans offered by many providers (*e.g.*, Internet Essentials, which provides a 75 Mbps plan at $14.99), so not much can be said about how price changes in the data may affect low-income households.

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Download speeds between years do not fully overlap, with the 2024 data capped at 5,000 Mbps and the 2025 data at 10,000 Mbps. Since price changes cannot be calculated between years for non-overlapping speed tiers, I limit my analysis to speeds present in both years (*i.e.*, common support), excluding speeds exceeding 5,000 Mbps ($n = 422$), about 3.4% of the 2025 sample, for much of the analysis. As shown below, these few observations fully account for Horrigan's 4.8% price increase. Also, a large share (26%) of download speeds are below the 100 Mbps level the FCC defines as "broadband service," and about 3.8% are below 10 Mbps. I retain these observations, since the FCC has subsidized connections at lower speeds in the past, but speeds below 100 Mbps are not by the FCC's definition considered "broadband services," and thus these prices are not "broadband prices" as currently accepted.

Compositional Bias in Price Indices

Simple comparisons of average broadband prices from the URS over time are misleading (biased) because the composition of offered services in the samples change. This change in composition has both a random (*i.e.*, sampling variation) and systematic component (shifts in speeds available). In the broadband market, providers frequently introduce higher-speed tiers, retire legacy plans, or upgrade speeds at unchanged prices—a systematic component.

Table 1. Download Speed by Year

Year	Mean	Min	Max
2024	673.1	2	5,000
2025	1,154.4	2	10,000
With Common Support			
2024	673.1	2	5,000
2025	798.6	2	5,000

As summarized in Table 1, the mean broadband speed is nearly twice as large in 2025 than in 2024, and the range of download speeds does not overlap.[12] In 2024, the top download speed is 5,000 Mbps but in 2025 it is 10,000 Mbps. Of course, these very high speeds have much higher prices, on average, and are of interest primarily to specialized commercial applications. Horrigan ignores this compositional shift. The means are much closer when restricting the data to the range of common support in speed. Thus, year-to-year changes in observed average prices from URS reflect not only changes in the prices of comparable services but also shifts in the mix of services available to consumers. Distinguishing between these forces is essential for drawing meaningful inferences about underlying price trends.

Formally, let P_{it} be the observed price for offer i at time t and X_{it} be a vector of service characteristics (speed tier, technology type, provider, state). The naïve approach (used above) to measure price trends compares sample means:

$$\overline{P}_{it} = \frac{1}{N_t}\sum_{i=1}^{N_t} P_{it}\ , \tag{1}$$

which is just a simple average over i in each t. For two time periods (0, 1), the year-over-year means difference is then,

$$\Delta\overline{P}_{it} = \overline{P}_1 - \overline{P}_0\ . \tag{2}$$

This change in the average observed price between two years can be written as the sum of two components: (1) a pure price effect, reflecting changes in prices holding service characteristics fixed, and (2) a composition effect, reflecting changes in the distribution of services offered over time,

$$\begin{aligned}\Delta\overline{P} = E[P_{1i} - P_{0i} \mid X_i = x] + \\ E[P_0(X_{1i}) - P_0(X_{0i})]\end{aligned} \tag{3}$$

where the first term is the pure price change (the item of interest) and the second term is a compositional effect (a source of bias). When the distribution of service characteristics changes over time (*e.g.*, more fiber, faster speeds), the composition effect contaminates inference about price changes. In the case of broadband services, the sample composition shifts toward higher-speed services, so the naïve average price comparison will tend to overstate price increases or understate price declines. This fact is why the Census Bureau diligently tries to maintain comparable quality over time when computing price indexes.

This decomposition parallels the logic of the Blinder–Oaxaca framework commonly used in labor economics, where differences in outcomes are separated into components attributable to changes in coefficients versus changes in covariates.[13] Notably, separating the pure price effect from the compositional bias is not accomplished by including the X_{it} as covariates in a regression model to account for the changing X_{it} between years. Multivariate regression may help attenuate the bias, but it need not eliminate it.

Notably, the URS data do not include the low-income discount plans offered by many providers (e.g., Internet Essentials, which provides a 75 Mbps plan at $14.99), so not much can be said about how price changes in the data may affect low-income households.

The Naïve, Incorrect Approach

To replicate Horrigan's estimates, I start with the simplest, most naïve approach. All of the data are included and unconditional (real) mean prices are computed for and compared between 2024 and 2025. This comparison involves taking the mean price for each year and computing the percent difference. This difference, as computed by Horrigan, is available from the regression model,

$$P_{it} = \beta + \delta T_{it} + \varepsilon_{it}\ , \tag{4}$$

where P_{it} is the price for offer i at time t, T_{it} is an indicator for 2025, and ε_{it} is the random disturbance term. The percent change in price between years is simply δ/β. Hypothesis tests are available in this means-difference regression. Standard errors are clustered on state and provider.

For the full sample, the β coefficient is 91.55 and the δ coefficient is 4.37, so the percent change in price is 4.8% ($p = 0.51$). This result reproduces Horrigan's estimate; the price change is not statistically significant. However, imposing common support in download speeds (restricting to speeds ≤ 5,000 Mbps), the δ coefficient is -1.44 (p = 0.68), for a price decrease of 1.6%. Horrigan's statistically insignificant 4.8% price increase arises purely from sample composition on download speed, capturing prices for download speeds exceeding 5,000 Mbps in 2025 absent from the 2024 data. Plainly, Horrigan's approach confounds compositional shifts with price changes.

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There is another problem with Horrigan's approach. Horrigan computes the percentage price change as the arithmetic mean of price relatives: $E[(P_1/P_0) - 1]$. This approach is problematic for several reasons. First, it lacks the symmetry property essential for price measurement. That is, a price that increases 10% and then decreases 10% does not return to its starting point under arithmetic averaging [(1.10 × 0.90) - 1 = -1%], creating spurious trends. Second, price relatives are bounded below by -100% but unbounded above, inducing positive skewness that biases arithmetic means upward—large price increases receive disproportionate weight relative to equivalent percentage decreases. Third, the arithmetic mean violates time-reversal and circularity properties fundamental to index number theory.

The Bureau of Labor Statistics ("BLS") and the broader price measurement literature address these issues by working in logarithmic space, computing $E[\ln(P_1/P_0)]$ and exponentiating to obtain the geometric mean of price relatives.[14] The geometric mean is symmetric (invariant to the direction of comparison), bounded, and satisfies the axiomatic properties required of proper price indices. As Fisher (1922) demonstrated, the geometric mean provides a theoretically superior aggregation of heterogeneous price changes.[15] For moderate price changes, the arithmetic and geometric means are similar; however, in the presence of large price variation or compositional shifts—both present in the URS data—the arithmetic means can substantially overstate price increases.

Recomputing Horrigan's estimate using the geometric mean, as is standard in price comparisons, uses the regression,

$$\ln P_{it} = \beta + \delta T_{it} + \varepsilon_{it}\,, \tag{5}$$

where the natural log of price is the dependent variable. The percentage change in prices between years is $e^{\delta} - 1$. For the full sample, the percentage change in price of 1.7% (p = 0.74), which is much smaller than 4.8%. With common support in download speeds, the δ coefficient is -2.0% (p = 0.56).

Horrigan's headline statistic of a 4.8% price increase is based on two errors: (1) a lack of common support in speed (compositional shifts); and (2) an improper price comparison methodology (arithmetic rather than geometric means). Fixing both errors indicates prices fell or were stable (statistically speaking) between years. Yet, more analysis is required to get a good estimate of price trends.

Conditioning on Covariates

This lack of common support in download speeds between years is a sample composition effect, but only one of many sources of sample composition bias. Before turning to the estimation of the pure price effect (*i.e.*, a Laspeyres price index), consider a regression approach that addresses the sources (but not the weights) of sample composition. The reported prices in the URS sample may be characterized by download speed, geography, provider, and modality—the composition of which changes between years. Conditioning on covariates by adding speed (s), state (c), provider (p), and modality (m) fixed effects to Equation (5),

$$\ln P_{it} = \delta T_{it} + \lambda_s + \mu_c + \theta_p + \phi_m + \varepsilon_{it}, \qquad (6)$$

with clustered standard errors on state and provider. Including all speeds, the percentage change in price is -8.7% ($p = 0.034$).[16] Excluding the ultra-high speeds from the 2025 data, the percentage change in price is again -8.7% ($p = 0.033$). In both cases, the price decline is -8.7% (as expected given λ_s), so conditioning on download speeds resolves the lack of common support. This approach is the familiar "hedonic" method used for CPI estimates.[17] Notably, the result matches Menko's estimate (-8.7%), though by different methods.

These estimates may still be infected with sample composition problems. I turn now to the pure price effect.

The Pure Price Effect

To isolate the pure price effect, I implement a matched-cell decomposition that compares prices only for download speeds, providers, modalities, and states that are observed in both periods.[18] This procedure is closely analogous to a Laspeyres price index, which measures price changes holding the initial consumption bundle fixed.[19] In effect, the 2024 price structure is a counterfactual for the null hypothesis of "no price change" for identical speeds (group), states, providers, and modalities.

By using base-year weights, the resulting estimate answers a well-defined question: *how would average prices have changed if consumers continued to purchase the same mix of services available in the base year?*

The mean price P_{gt} is computed for each cell g in year t, summing weights, and the 2024 mean prices and weights are projected onto matching cells in 2025. There are 1,614 such cells in 2024 of which 1,373 match across years (54.3% match). The matched-cell log price change is,

$$\Delta \overline{P}_g = \ln\left(\overline{P}_{g,2025} \,/\, \overline{P}_{g,2024}\right), \qquad (7)$$

where the denominator on the right-hand side is the counterfactual. Regressing ΔP_g on a constant term using the 2025 on a constant term and weighting by the 2024 weights, where the estimated coefficient is exponentiated to get the percent price change.[20] Standard errors are clustered on state × provider.

In contrast, a naïve (composition allowing) comparison of weighted average cell prices across years implicitly allows the service mix to vary, combining price changes with shifts toward higher-speed or higher-priced offerings. The difference between the naïve estimate and the matched-cell Laspeyres-style estimate therefore provides a direct measure of the compositional contribution to observed price movements.

To begin, I target the more popular plans that consumers buy by limiting the included cells to those with at least 100 observations in each year for matched cells and cap download speeds at 1,000 Mbps. For the more consumer-oriented speed tiers, the mean pure price effect is -8.4% ($p < 0.01$, $n = 876$ cells), and the naïve price effect is -7.3% ($p < 0.01$). The composition bias is about 15% of the observed price difference. This estimate is again comparable to Menko's estimate of 8.7%, so Menko's sample restrictions and weighting get close to the pure price effect, though his estimate does not target the pure price effect and may reflect compositional bias.

Recall that the estimated price change when conditioning on covariates on the raw data (an 8.7% reduction, see above) is very close to the pure price reduction. Including covariates in a regression framework may be sufficient to recover meaningful price changes over time, though different samples may provide different results.

Table 2. Pure Price Change by Speed

Speed Group	Coef	% Diff
25 Mbps	-0.250***	-22.1%
50 Mbps	-0.025***	-2.5%
100 Mbps	-0.005	-0.5%
200 Mbps	-0.031***	-3.1%
300 Mbps	-0.014*	-1.4%
500 Mbps	-0.140**	-13.1%
1,000 Mbps	-0.096***	-9.1%

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

Table 2 summarizes the pure price effects by speed group. The pure price effect is negative for all download speed cells, and the price changes are statistically significant at the 10% level or better for all but one speed cell (100 Mbps). The largest decline is for download speeds of 25 Mbps (-22%), a speed level that no longer qualifies as a broadband service, with larger changes also at the increasingly popular speed tiers of 500 Mbps or above.

This cell-matching approach applies exact matching to download speeds, so 100 Mbps is treated differently than 105 Mbps. As an alternative, I create bins of speeds to rope in nearby speed levels of comparable "quality."[21] The estimated pure price effect is -7.1% ($p < 0.01$), which is slightly smaller than before, and the naïve price effect is -4.6% ($p < 0.01$), so composition bias rises to 54%, with variations in the speed within bins between years to blame.

Expanding to the full sample as a robustness check (with speed ≤ 5,000 Mbps), the estimated pure price effect is -7.7% ($p < 0.01$, $n = 1{,}333$). The naïve estimate ($n = 5{,}074$) is -2.1% ($p = 0.11$).[22] From Expression (3), the bias from sample composition is 4.4 percentage points (67%), a large bias relative to the more restricted sample. Plainly, sample composition is important when analyzing the URS data.

Restricting the sample to speeds no greater than 1,000 Mbps, the pure price effect is 8.1% ($p < 0.01$) with a naïve estimate of 6.2% ($p < 0.01$). Estimating the pure price effects across technologies from this broader sample, cable broadband prices fell 8.9% ($p < 0.01$), fiber prices fell 4.8% ($p < 0.01$), fixed wireless prices fell 12.8% ($p < 0.01$), and DSL prices fell 1.8% ($p < 0.01$), with naïve changes of -6.7%, -1.5%, -21.4%, and -0.8%, respectively. The naïve approach can be very different from the pure price effect, sometimes larger (fixed wireless) and sometimes smaller.

Table 3. Pure Price Change by Technology

Speed Group	Coef	% Diff
Cable	-0.094***	-8.9%
Fiber	-0.049***	-4.8%
Fixed Wireless	-0.137	-12.8%
DSL	-0.018***	-1.8%
All Modalities	-0.084*	-8.1%

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

Using Equation (6) to get the price effect when conditioning on covariates (ϕ_m is redundant), cable prices fell 9.2% ($p < 0.10$), fiber prices fell 5.8% ($p < 0.05$), fixed wireless prices fell 12.1% ($p < 0.05$), and DSL prices fell 2.0% ($p < 0.01$). In large part, the regression approach provides comparable estimates of the price effect, though they are measured less precisely. Naïve price effects, however, are unreliable.

Table 3. Pure Price Change by Speeds

Speed Group	Coef	% Diff
Cable		
100 Mbps	-0.022**	-2.2%
300 Mbps	-0.154***	-14.3%
500 Mbps	-0.099***	-9.4%
Fiber		
300 Mbps	-0.009	-0.9%
300 Mbps	-0.094***	-9.0%
1,000 Mbps	-0.079***	-7.6%

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

Limiting the included cells to those with at least 100 observations in each year for matched cells and capping download speeds at 1,000 Mbps, prices changes at speeds for cable and fiber are summarized in Table 4. Larger price declines are observed for the higher speeds more typical of the current market environment. At 300 Mbps, for example, cable prices are down 14.3% and fiber prices are down 9.0%.

Services Consumer Use

The URS data do not include a measure of subscriptions to service tiers, so a true mean difference in prices paid by consumers is unavailable. To get a better feel for what speed tiers consumers use, and how prices have changed for these services, I retrieve measured speeds from Ookla's Open Data program for the second quarter of 2024 and 2025, restricting the data to the U.S. states.[23] These Ookla data, which are best in class, are a convenience sample and subject to measurement error.[24]

From these data, I calculate the shares of speed measurements within a 10% window of the speed groups in Table 2. I then construct a consumption-weighted price index by combining URS sampling weights (which roughly reflect market population) with Ookla speed tier shares (which reflect actual consumer usage patterns). The combined weight for each cell equals the URS population weight multiplied by the Ookla consumption share, yielding the expected number of consumers experiencing that price change. This provides an estimate of the average price experienced by actual broadband consumers rather than the average across available service offerings.

The matched cell approach is applied to the data with the download speed restrictions and the combined weight (based on 2024 shares). The pure price effect -6.7% ($p < 0.01$), smaller than the 8.4% but still large. The naïve estimate, based on the respective annual weights, is -4.5% ($p < 0.01$), with a compositional bias of 33% of the pure price effect.

Discussion

Setting aside the meaningless simple comparison of means between years that ignores obvious compositional changes between years, there is no evidence of broadband price increases. Broadband prices as measured in the URS are lower in 2025 than in 2024, and the price decrease is in the 7% to 9% range.

At a minimum, regression adjustment is required—including at least download speeds—though the estimated effects tend to be mildly biased upward by a small amount relative to the pure price effect. Matched cell methods estimate the pure price effect, which is most relevant for policy analysis. Naïve price differences are biased, and often severely so, and thus have zero policy relevance.

These findings have direct policy relevance: claims of rising broadband prices—used to justify expanded subsidies and regulation—are not supported by properly measured price trends.

Conclusion

Simple comparisons of average prices over time can be misleading in broadband markets, where providers routinely upgrade speeds, retire legacy tiers, and introduce higher-quality offerings without proportional changes in nominal prices. This PERSPECTIVE evaluates recent broadband price trends using a decomposition framework that separates changes in prices for comparable services from changes in the composition of services offered.

Several complementary empirical methods are used. All reasonable attempts to address sample composition indicate broadband prices are falling. Applying a matched-cell, base-year–weighted approach analogous to a Laspeyres price index—the most policy relevant comparison—reveals that prices for like-for-like broadband services of the sort consumers typically buy declined materially over the study period by 8.4%, even as naïve averages suggest smaller reductions. Re-weighting the data to approximately match speed tier consumption patterns, the estimated like-for-like price reduction is 6.7%. These pure price effect estimates produce higher mean price reductions than do naïve approaches, a divergence that reflects compositional shifts toward higher-speed services and underscores the importance of index-style methods when analyzing markets characterized by rapid product evolution. For these two years, regression adjustment alone provides an estimate of -8.7%, which is very close to the pure price effect.

As broadband technologies and service tiers continue to change, careful attention to composition effects will remain essential for accurate measurement of price trends and informed policy debate. Method matters. Given the prominence of affordability concerns in current policy discussions, these methodological distinctions have direct implications for regulatory design. These findings have direct policy relevance: claims of rising broadband prices—used to justify expanded subsidies and regulation—are not supported by properly measured price trends.

NOTES:

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1 G. Iacurci, *Affordability Is A Buzzword Right Now – These Charts Show Why*, CNBC (December 24, 2025) (available at: https://www.cnbc.com/2025/12/24/affordability-is-a-buzzword-right-now-these-charts-show-why.html); J. Allen and M. Dixon, *Trump Turns To Progressives For Ideas On Affordability*, NBC News (January 13, 2025) (available at: https://www.nbcnews.com/politics/donald-trump/trump-progressives-affordability-rcna253633).

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3 A. Menko, *2025 Broadband Pricing Index,* USTelecom (November 19, 2025) (available at: https://ustelecom.org/wp-content/uploads/2025/11/2025-BPI.pdf).

4 For an earlier analyses of this topic, *see* G.S. Ford, *Are Broadband Prices Declining? A Look at the FCC's Price Survey Data*, PHOENIX CENTER POLICY PERSPECTIVE No. 20-07 (October 26, 2020) (available at: https://www.phoenix-center.org/perspectives/Perspective20-07Final.pdf); S. Greenstein and R. McDevitt, *Evidence of a Modest Price Decline in US Broadband Services*, 12 INFORMATION ECONOMICS AND POLICY 200-211 (2011); K. Flamm and C. Herrera, *Price and Quality Change in U.S. Broadband Service Markets*, Working Paper (August 15, 2017) (available at: https://ssrn.com/abstract=2757429 or http://dx.doi.org/10.2139/ssrn.2757429).

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7 A. Aquirre, *How To Get Low-Income Internet Through Verizon*, HIGHSPEEDINTERNET.COM (Aug 6, 2024) (available at: https://www.highspeedinternet.com/resources/verizon-low-income-internet); *How Verizon Forward Works*, Verizon (last visited February 19, 2025) (available at: https://www.verizon.com/discounts/verizon-forward).

8 Data available at: https://www.fcc.gov/economics-analytics/industry-analysis-division/urban-rate-survey-data-resources. The years reported in the data are one year after collection.

9 *Methodology: 2026 Urban Rate Survey – Fixed Broadband Service*, Federal Communications Commission (2026).

10 The sample weighting methodology changed between years, but 2024 includes an additional weight (Weight2) that is consistent between years. I use these consistent weights in all analysis (as does Horrigan, but not Menko who constructs his own weights).

11 Data available at: https://fred.stlouisfed.org/series/CPIAUCSL. The CPI is June 2024 is 313.131 and in 2025 is 321.5.

12 The weighted mean download speed is 617 Mbps in 2024 and 1,167 Mbps in 2025.

13 N. M. Fortin, T. Lemieux, and S. Firpo, *Decomposition Methods in Economics*, in O. Ashenfelter and D. Card (Eds.), HANDBOOK OF LABOR ECONOMICS, Volume 4B, 1–102 (2011).

14 *Handbook of Methods - CPI Calculation*, Bureau of Labor Statistics (last visited January 15, 2026) (available at: https://www.bls.gov/opub/hom/cpi/calculation.htm) ("Most item strata use the geometric mean index formula"); *Consumer Price Index FAQ*, Bureau of Labor Statistics (last visited January 15, 2026) (available at: https://www.bls.gov/cpi/additional-resources/chained-cpi-questions-and-answers.htm)("Since January 1999, a geometric mean formula has been used to calculate most basic indexes within the CPI").

15 I. Fisher, THE MAKING OF INDEX NUMBERS: A STUDY OF THEIR VARIETIES, TESTS, AND RELIABILITY (1922).

16 Singletons are dropped in estimation (< 30 units).

NOTES CONTINUED:

[17] *Consumer Price Index, A Review of Hedonic Price Adjustment Techniques for Products Experiencing Rapid and Complex Quality Change*, Bureau of Labor Statistics (last visited January 15, 2026) (available at: https://www.bls.gov/cpi/quality-adjustment/hedonic-price-adjustment-techniques.htm).

[18] Census tracts are too fine for this analysis and cannot be linked across years. Limiting the groups to download speed and a state or provider groups does not change the results by much. Download speed appears to be the determining factor.

[19] Fisher, *supra* n. 15.

[20] This approach is comparable to differences-in-differences estimation with few clusters proposed by Lee and Wooldridge (2025), though here the sample weights are important. 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, *Differences-in-Differences with Few Treated Units: The Lee-Wooldridge Approach in Stata*, Working Paper (September 29, 2025) (available at: https://ssrn.com/abstract=5544598 or http://dx.doi.org/10.2139/ssrn.5544598).

[21] The cut points are: 0, 10, 25, 50, 75, 100, 150, 200, 250, 300, 350, 400, 450, 500, 550, 600, 700, 800, 900, 1000, 1250, 1500, 2000, 2500, 4000, 5001.

[22] This small coefficient for the cell data is comparable to that estimated from Equation (4) on when imposing common support in speed.

[23] Data available at: https://registry.opendata.aws/speedtest-global-performance.

[24] *See*, *e.g.*, J. Saxon and D.A. Black, *What We Can Learn from Selected, Unmatched Data: Measuring Internet Inequality in Chicago*, 98 COMPUTERS, ENVIRONMENT AND URBAN SYSTEMS 101874 (2022); G.S. Ford, *A Comparative Analysis of Fixed Broadband Speeds in Cities Across the World*, PHOENIX CENTER POLICY BULLETIN No. 54 (February 2022) (available at: https://www.phoenix-center.org/PolicyBulletin/PCPB54Final.pdf).