Tackling the Digital Divide?

Evaluating the Effectiveness of the Affordable Connectivity Program

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Abstract

The Affordable Connectivity Program (ACP) was an unprecedented expansion in the magnitude of broadband subsidies. While the ACP clearly reduced broadband costs for connected households, its effect on broadband subscription rates is unclear. In this paper, we employ a zip-code-level regression model using ACP claims and ACS data to estimate the effect of ACP deployment on new broadband subscriptions. We find consistently non-significant results. Our results suggest that the ACP likely resulted in fewer new subscriptions than prior estimates based on qualitative survey data.

Introduction

Internet connectivity has become a cornerstone of modern society. Participation in social, economic, and educational opportunities is increasingly conditioned on broadband internet. However, disparities in access persist, particularly among low-income and marginalized communities. To bridge this digital divide, the US government turned to Lifeline, a 1984 program originally designed to provide phone service subsidies. In its modern form, Lifeline offers up to one monthly subsidy per qualifying low-income household for either a wireline, wireless phone service or broadband bundle.

However, the COVID-19 pandemic made the problem of disparate access to virtual resources far more urgent. The Emergency Broadband Benefit (EBB) was launched in May 2021 to reduce the negative impact of a sudden shift towards virtual services on disconnected low-income households. This program temporarily subsidized both device acquisition and connection at the household level, using similar eligibility criteria to Lifeline, but substantially higher monthly payments—$35 a month per household instead of $9.25[1]. By the end of 2021, it was replaced and extended by the Affordable Connectivity Program (ACP), which kept the same subsidy but revised and expanded eligibility criteria to include SNAP status, WIC and FPHA aid, SSI recipients, and Medicaid. However, the primary criteria remained an income-based criteria set at 200% of the poverty line by household. The ACP was originally funded through 2024. That funding has since expired, and the future of the ACP is contingent on Congressional approval of additional funding for the program.

Current research suggests that the ACP was effective at allocating government funding to connected low-income households, thereby making broadband more affordable for recipients. However, this study design only captures a portion of the benefits the ACP offers. While subsidizing existing broadband subscriptions may help defray costs of broadband and is generally beneficial to eligible households, subsidizing low-income populations who already receive broadband does not reduce the digital divide, and thus does not achieve the presumptive policy goals of broadband policy. This is particularly true because subsidies taking the form of fixed discounts on individual subscriptions may be partially absorbed by plan price increases.

The most compelling potential benefit of the ACP would be the positive effect of broadband connectivity on households who would otherwise not subscribe to broadband internet. In other words, the question most relevant to determining the success of the ACP is whether the ACP incentivizes new households to purchase broadband subscriptions and keeps customers currently subscribed to broadband. This study seeks to determine whether the ACP increased broadband uptake rates.

[1] In tribal areas, the ACP benefit was expanded to $75/month.

Background and Literature Review

The 2021 Infrastructure Investment and Jobs Act earmarked $65 billion towards broadband internet initiatives, aiming to bridge the gap in access and affordability for underserved communities and tackle the persistent "digital divide." The FCC defines broadband internet as internet access with a minimum of 25 Mbps download speed and 3 Mbps upload speed. This minimum standard of connectivity is necessary to access much of the modern internet. In the US, approximately 80% of the population has access to broadband.[2] The World Bank defines broadband more broadly, including any fixed internet connection with at least 256 kbps broadband speed. Under this definition, broadband penetration in high-income nations is also approximately 80% on average, while developing nations average 35% access.[3] In the Infrastructure Act, Congress asserted broadband's importance, noting:

> (1) Access to affordable, reliable, high-speed broadband is essential to full participation in modern life in the United States; (2) The persistent "digital divide" in the United States is a barrier to the economic competitiveness of the United States and equitable distribution of essential public services, including health care and education; (3) The digital divide disproportionately affects communities of color, lower-income areas, and rural areas, and the benefits of broadband should be broadly enjoyed by all.[4]

These observations, and the legislative priorities they reflect, justify an in-depth analysis of the benefits of broadband access and the impact of different policies on broadband uptake.

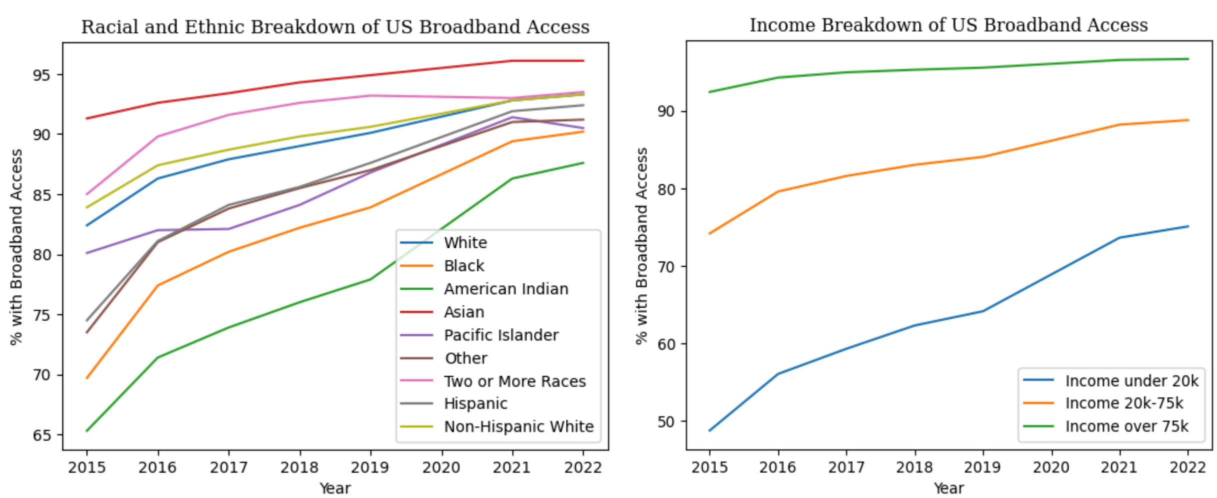

Figure 1: US broadband access by income, racial, and ethnic Group. For both charts, 2020 data is interpolated due to missing data from the pandemic.

The proliferation of broadband has enabled new modes of job seeking, enhanced business recruitment processes, and fostered a digitally savvy workforce. Some research also identifies

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[2] See ACS data below.
[3] World Bank 2024
[4] Congress 2021

marginal benefits to broadband: researchers at the Federal Reserve Bank of Richmond summarize current research as showing that "increased broadband in rural areas is linked to increased job and population growth, higher rates of new business formation and home values, and lower unemployment rates." [5]

However, isolating this effect as a causal result of expanded broadband, rather than a response to increased demand or improved infrastructure resulting from increased prosperity, is difficult. The World Bank estimates that a 10 percentage point increase in broadband penetration can lead to a 1.2% jump in real per capita GDP growth in developed economies, but this estimate stems purely from a linear regression of GDP growth against broadband penetration, beginning GDP, foreign investment, and educational factors. Therefore, it structurally ignores the possibility that countries with higher broadband penetration had superior infrastructure or more favorable natural conditions that enabled growth. Indeed, the same paper finds that while fixed broadband increases GDP (significant at the 10% level), mobile broadband decreases it (significant at the 1% level), which is wildly unlikely unless increases in mobile broadband reflect poor infrastructure compared to increases in fixed broadband.[6] Researchers at CESifo attempt to correct this issue by modeling broadband rollout as occurring primarily on existing fixed voice and TV networks, and thus avoiding infrastructure endogeneity. In order to address demand effects, they use this infrastructure model as an instrumental variable to predict the spread of broadband, which is then used in a second-stage model to predict the effects of broadband penetration on GDP growth. This produces an estimate of between 0.9 and 1.5 percentage points of GDP growth per 10 percentage point increase in broadband penetration for OECD countries, suggesting that marginal increases in broadband penetration do result in economic benefits[7]

Domestically, the economic repercussions of restricted broadband access exacerbate racial disparities. Currently, Black, American Indian, and Latinx populations, who have lower average incomes, are nearly twice as likely to be without broadband access compared to their white counterparts.[8] Deutsche Bank predicts that by 2045, the digital divide could leave over half of the Black and Latinx population inadequately prepared for 86 percent of jobs[9]

Broadband access also brings non-economic benefits: telehealth, education, and social services can be deployed virtually to connected regions. For instance, 62% of rural participants turned to online sources for COVID-19 information, and 13% used the Internet to consult with medical professionals.[10] In total, these benefits amount to substantial social and economic good. A cost-benefit analysis of rural broadband installation in Indiana observed three to four-fold returns

[5] Marre 2020.
[6] Minges 2015.
[7] Czernich, Falck, Kretschmer, and Woessmann, 2009.
[8] Marshall & Raune, 2021.
[9] Walia & Ravindran, 2020.
[10] McClain, Vogels, Perrin, Sechopoulos, and Rainie, 2021.

on investment, not including state and local governments' cost savings on medical expenditures and additional tax revenues from increased incomes.[11]

Recent research examining ACP enrollment found that decisions to enroll are influenced not only by individual or household income levels but also by community-level demographic factors.[12] These include housing costs, racial makeup, the presence of anchor institutions like public libraries, and population density. The findings reveal that while household eligibility remains a significant predictor of ACP enrollment, social and community-wide indicators are also important. Notably, minority groups tend to enroll at higher rates, yet areas with large populations of older residents and households led by foreign-born individuals see lower enrollment, possibly due to misinformation about eligibility, concerns about sharing data with government agencies, and potential digital literacy or awareness challenges, especially among those over 65. Intriguingly, the study finds that the presence of a local library branch is a positive influence on ACP enrollment, even when controlled for other socioeconomic and demographic factors, suggesting that such institutions may facilitate access to the program.

One potential obstacle to the effectiveness (or cost-effectiveness) of the ACP is how internet service providers adjust prices in response to the subsidy. Because eligibility for the ACP is often rolled directly into plans offered by providers, providers could increase their prices to directly offset the subsidy effect while leaving the market unchanged. This could substantially reduce the effectiveness of the ACP. One study attempts to analyze this effect using the FCC's new Broadband Facts Data to compare plan price, quality, and ACP participation. This study finds that ACP participation has a significant positive effect on pre-subsidy price before controlling for plan quality and type, which becomes a significant *negative* effect on pre-subsidy price when controlling for plan quality.[13] The authors claim that this reflects companies passing cost savings onto consumers. However, these results do not have a plausible causal explanation, and the authors make no attempt to provide one, instead focusing on the relatively small magnitude of the effect.

However, the study does not clearly distinguish ACP eligibility and ACP participation. This is extremely important, because the uncontrolled positive correlation between ACP eligibility and price, coupled with the positive correlation identified between plan quality and price, casts doubt on the assumption that primarily affordable, ACP-targeted plans are identified as ACP-eligible on the FCC broadband scorecard. The study also raises concerns about the reliability of the data due to issues such as inconsistent, non-standardized formats and the absence of machine-readable information, which complicated data cleaning. It also highlights the lack of detailed data on the technology types used for internet service delivery, which could affect cost variations. These methodological flaws are all the more concerning given that both authors are affiliated with Ready.net, a services contractor for broadband providers.

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[11] Grant, Tyner, & DeBoer, 2018.
[12] Horrigan, Whitacre, & Galperin, 2023.
[13] Schieberl & Ahmadi, 2023.

Comparatively little research exists analyzing the empirical total effect of ACP enrollment on broadband uptake.

Research from the Benton Institute examines the relationship between ACP enrollment in 2022 and broadband adoption, using data from the American Community Survey (ACS) and the Universal Service Administrative Company (USAC) for county-level analysis. This study found that by 2022, 12.5% of broadband users in the studied regions were enrolled in the ACP, an increase from 8% at the end of 2021.[14] Further statistical analysis revealed a positive and significant correlation between the growth in broadband-of-any-type subscriptions from 2021 to 2022 and levels of ACP enrollment. This analysis suggests that the ACP has contributed to expanding broadband penetration that began in the United States during the pandemic but omits important data: the analysis only includes the most populous counties, meaning the rural counties most likely to lack broadband connectivity are likely neglected.

The FCC surveyed ACP recipients to attempt to determine the impact of the ACP.[15] As expected, this survey found that over 70% of recipients use the ACP-subsidized internet for critical everyday activities like telehealth, education, and job interviews. However, the survey also found that 47% of recipients had no broadband access prior to receiving the ACP benefit. This is not consistent with ACS data on broadband subscriptions. Per ACP Claims Tracker data (see below), 23.2 million households were enrolled in the ACP. Attributing 100% of the increase in broadband subscribers from 2020 to 2022 to the ACP results in an increase of less than 8 million households (per ACS data, see below), which is far less than 47% of ACP participants. Moreover, this assumption is a significant overestimate, as broadband participation surged during 2020 due to pandemic-era effects. Therefore, survey data is not a reliable metric for determining whether the ACP increased subscriptions.

A study from the Benton Institute seeks to identify and isolate drivers of ACP subscriptions. This study finds that on a zip code level, households opting into ACP subsidies are correlated with economic distress and minority status.[16] However, because this study does not isolate the effects of ACP enrollment on broadband, it fails to determine the effect of the ACP as more than a general welfare program.

Another comprehensive review attempts to quantify the financial benefits of the ACP, finding that every ACP subsidy dollar generates nearly double in financial impacts.[17] Households with discounted internet report up to $2,200 in annual income boosts, while benefits from purchasing choice and convenience amount to about $1,285 per household annually. With 23 million households enrolled, the ACP generates $16.23 billion in benefits versus an $8.45 billion subsidy cost, resulting in a

[14] Horrigan, 2023.
[15] FCC, 2023.
[16] Horrigan, 2023.
[17] Horrigan, 2024.

claimed benefit-to-cost ratio of 1.92 to 1. However, this study uses survey data to estimate uptake increases, taking the average of an FCC and an NTIA survey to produce a final estimate that 22% of ACP recipients received broadband as a result of the ACP. This design attributes approximately 100% of the net gain in internet subscriptions between 2019 and 2022 to the ACP, which began at the end of 2021. Approximately 90% of this gain occurred in the 2019-2021 period, predating the ACP. Horrigan claims that the ACP may have helped "lock in" these gains, but does not justify attributing a substantial portion, let alone all, of these gains to the ACP. The claim that without the ACP, all pandemic-era broadband aids would have been lost is particularly unlikely given the steady long-term trend of increasing broadband penetration.

A separate study investigates the impact of affordable broadband plans offered by specific internet service providers (ISPs), mandated as part of merger deals, on connectivity.[18] Through empirical analysis, the study reveals varying degrees of success across ISPs, with Frontier Communications demonstrating a modest increase in connectivity among low-income households. However, the overall effectiveness of regulatory interventions remains questionable, emphasizing the importance of rigorous monitoring and accountability mechanisms.

These studies all fail to directly test whether broadband adoption increased as a result of ACP subsidies. Thus, there is a substantial literature gap in understanding whether the ACP meaningfully increased broadband participation among affected areas. Because this empirical question is central to evaluating the ACP's success, this study attempts to fill that research gap.

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[18] Galperin, 2022

Methodology

For all experiments, zip code level datasets sourced from the ACP Enrollment and Claims Tracker (compiled by the USAC, the permanent administrator of FCC services) and the American Community Survey were used. Zip codes with incomplete information were excluded. American Community Survey 5-year broadband subscription data from the end of 2022 and 2021 were compared by zip code, as neither 2023 data nor 1-year estimates were yet available. End of 2022 ACP data was used for estimating treatment. This data appeared to have significant measurement issues: 1,925 out of 30,309 zip codes showed more households receiving ACP grants than the total number of households in the zip codes. Some had dramatically more implausible funding amounts, with thousands of dollars allocated per resident of the zip code. Given the ACP's design, these measurements point to either high levels of fraud or high levels of measurement error. Zip codes with more ACP subscriptions than households were also excluded from the data set. Because many EBB-eligible broadband plans became ACP-eligible broadband plans when the EBB was phased out in favor of the ACP, treatment levels did not begin at 0 when the ACP began. Instead, January 2022 data on ACP subscriptions (immediately after ACP rollout) were compared to December 2022 data to determine the increase in ACP treatment over 2022.

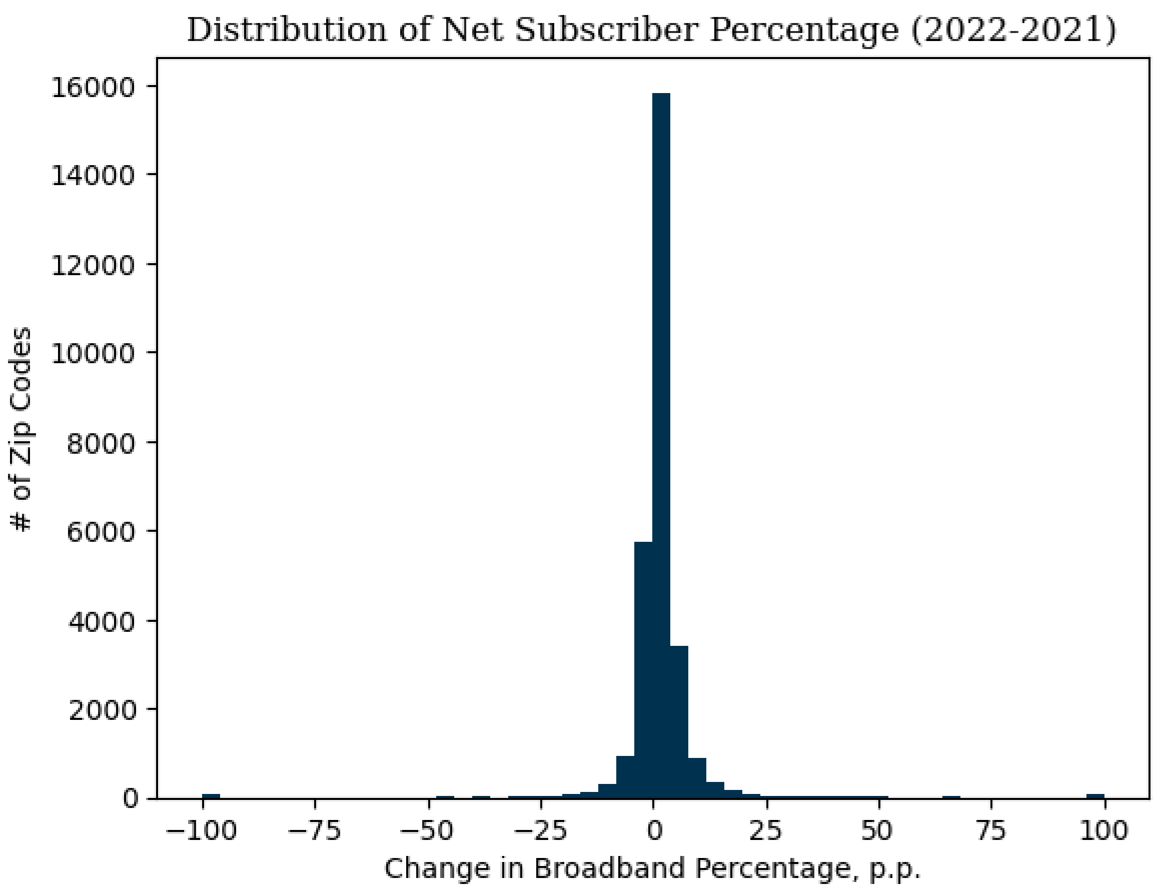

Figure 2: Distribution in broadband uptake over 2022.

Given the availability of zip-code-level nationwide data on both ACP subsidies and broadband participation, an experiment design based around instrumental variables appeared natural. However, identifying an appropriate instrumental variable proved impossible. For a household to become enrolled in the ACP, two separate conditions must be met: first, the household must be eligible for ACP, and second, the household must elect to participate in the ACP (potentially through automatic enrollment via ISPs). Because ACP subsidies were often extended as a part of existing low-income plans from broadband carriers, the second condition is inextricably linked to the dependent variable of broadband uptake: any exogenous variable influencing ACP enrollment would almost certainly directly affect broadband enrollment as well. Therefore, IV selection was limited to the first condition.

However, the ACP rollout was relatively uniform across the country, partially because the administration of plans through ISPs meant regional differences in program administration did not cause major delays. The two main exceptions were the high-cost (NTIA-designated areas with high broadband infrastructure costs) and tribal area expansions of the ACP. Beyond the obvious issues with endogeneity in both of these programs, both were conducted close enough to the end of funding for the ACP that their effects could not be analyzed with currently publicly available data. Therefore, IV selection was limited to household eligibility at the household level, rather than the regional level. However, the program eligibility criteria were designed as multifaceted proxies for socioeconomic status (i.e. household income relative to the poverty line, recipients of various welfare programs, etc.). Socioeconomic status both intuitively and empirically affects broadband uptake, so these eligibility criteria could not be used as independent variables. Therefore, a covariate-based difference-in-differences approach was used for the main study. However, because the primary criteria for ACP eligibility is based on a sharp cutoff at 200% of the federal poverty guideline, a regression discontinuity based approach was also pursued.

Unfortunately, neither income nor broadband uptake data is publicly available at the household level. Zip-code-level data is not sufficient to analyze broadband uptake by income, as under any reasonable set of assumptions, household income is distributed around a zip code mean, so even if the ACP had a strong effect on uptake fully segregated by income level, no discontinuity would result. However, data on the percentages of households with broadband subscriptions in the sub-20k, 20k-70k, and >70k income ranges were available. Because the vast majority of ACP-eligible households would fall into the first two bins, the closest mirror to a regression discontinuity design would be a comparison of the broadband uptake in the first two bins to broadband uptake in the third bin over the ACP period. However, because this approach doesn't produce a sufficient trend across income levels to identify a discontinuity, it collapses to a comparison of differences in uptake between high- and low-income households in prior years, which is significantly confounded. Therefore, it is not sufficient for causal inference (see results below).

Defining a binary treatment variable in the context of our data is challenging because we do not observe the treatment and control group characteristics within each zip code. Propensity-score-matched differences in differences were used as a first-pass attempt at causal inference. To obtain a treatment variable from the continuous funding variable, ACP funding by zip code was binned by quartile, and the top and bottom quartiles were taken as treatment and control groups. After introducing covariates and performing propensity score matching (PSM), a difference-in-differences-based design yielded a positive point estimate on the effect of the ACP (approximately 0.5 percentage points) Repeating this process with the top and bottom deciles used as treatment and control groups yielded a substantially larger point estimate (approximately 1.9 percentage points). While this difference supports the hypothesis that the benefit of the ACP was concentrated in high-treatment subpopulations (and thus that the ACP was plausibly effective), it suggests that the estimated size of the effect depends strongly on the researcher's choice of bins. Because of this, and because categorizing each zip code as either belonging to the treated or control group reduces the power of our study, this approach was discarded in favor of a continuous approach.

We employed a continuous treatment design based around a first-difference estimator with covariates. This experiment used ACS data for demographic variables by zip code, including the number of households in each zipcode falling below 200% of the federal poverty line. Because the ACP took effect in January 2022, for all pretreatment variables, 2021 5-year ACS estimates were used. The ideal continuous treatment variable would have been the percentage of ACP-eligible households that received ACP subsidies. However, ACP household eligibility data was not available at the zip-code level. Based on our literature review, the current academic standard for determining eligibility data was LISC's rural claims tracker, which uses household income relative to the poverty line as a proxy for eligibility.[19] In order to ensure consistency, we re-created this dataset using ACS data and treated households falling below 200% of the federal poverty line as eligible. More than 1/3rd of zip codes had more households receiving the ACP subsidy than those falling under the 200% of the poverty line criterion. While this could be an extension of the aforementioned issues with ACP data writ large, it more strongly suggests that the 200% of poverty eligibility threshold does not meaningfully capture the large number of ACP qualifying criteria. Therefore, the treatment variable employed was the increase in the percentage of households (regardless of eligibility) in a zip code that received the ACP subsidy from January 2022 to December 2022 (to account for the presence of the EBB at the beginning of the study period), and the 200% eligibility threshold data was re-introduced as a covariate. Our outcome variable is the percentage change in internet subscribers from 2021 to 2022 in each zip code. We hypothesize that as the percentage of households that received ACP grants increases, we would expect a higher percentage of internet subscribers.

Other covariates are median household income (from ACS), RUCA1 (urbanization score), the total population, and the percent of the population where the ISP offers 25 Mbps up 3 Mbps down

[19] LISC

Internet plans at their location in each zip code (broadband fabric data). The first four were sourced from census and ACS, measured at the last interval before December 2021. Broadband availability was sourced from FCC Form 477 data in December 2021.[20]
Next, we evaluate the assumptions necessary for casual influence for propensity-score based studies on observational data. Stable Unit Treatment Value Assumption (SUTVA)[21] is a strong, but fairly well-justified, assumption here: ACP grants in one zip code are fairly unlikely to affect uptake rates in other zip codes, although social contagion could be a relatively minor factor. Uniformity assumptions partially break down when we account for the fact that ACP grants are administered with significant heterogeneity in terms of amount ($30 per month, $75 per month if tribal, or $100 one-time device subsidy). However, because the $100 device subsidy is fully independent from the other portions of the program, the heterogeneity with tribal areas is the only main concern. However, the data used for ACP subscriptions suggests that this is a very minor problem: for every zip code included in the study, the amount of monthly ACP funding authorized at the end of 2022 was between 32 and 37 dollars per claimed subscription reported in the same period (in other words, the vast majority of households appear to have received the standard benefit)[22]. We expect the overlap assumption, which states that every zip code has a positive probability of receiving any proportion of ACP grants within a certain range given the covariates, to hold for our study. The conditional independence assumption (CIA), which claims that given a set of covariates the potential outcomes are independent of the treatment assignment, is a strong assumption, and likely the weakest portion of our study: there are almost certainly relationships between ACP uptake and broadband uptake on the zip-code level not included in our covariates. Further research to isolate and better control for these relationships is necessary.

Several regressions with increasingly complex specifications were performed. First, the first difference estimator on our treatment with no covariates was used as a first-pass estimate of ACP effects. We also cluster robust standard errors by zip code to account for within zip code correlation. Second, control variables and interaction terms were added to determine how they affect treatment determination. Through this regression, we estimate the average causal effect of a one percentage point increase in the proportion of households receiving ACP grants on the percentage change in internet subscribers, given the values of covariates.

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[20] Form 477 data comes from forms filed by individual ISPs as to their service ranges. This data is used to target FCC broadband expansion programs.
[21] STUVA is a prerequisite for valid casual influence that requires treatment in one region (zip code) to be independent of treatment in every other region. If this is violated, causal inference requires substantially more complex designs, as different zip codes influence each other.
[22] Some tribal areas may not be included in the zip code data.

Results

Initial discontinuity analysis produced inconclusive results. Comparing low and high-income broadband uptake over pre- and post-ACP periods revealed a continuation of a long-term trend, but did not suggest the ACP had any effect on this trend: indeed, broadband uptake among high-income groups was more similar to broadband uptake around low-income groups than ever before. Because broadband uptake is consistently higher among low-income groups, this would have suggested a negative effect of the ACP, which is implausible. Therefore, a more nuanced zip-code-level analysis is necessary.

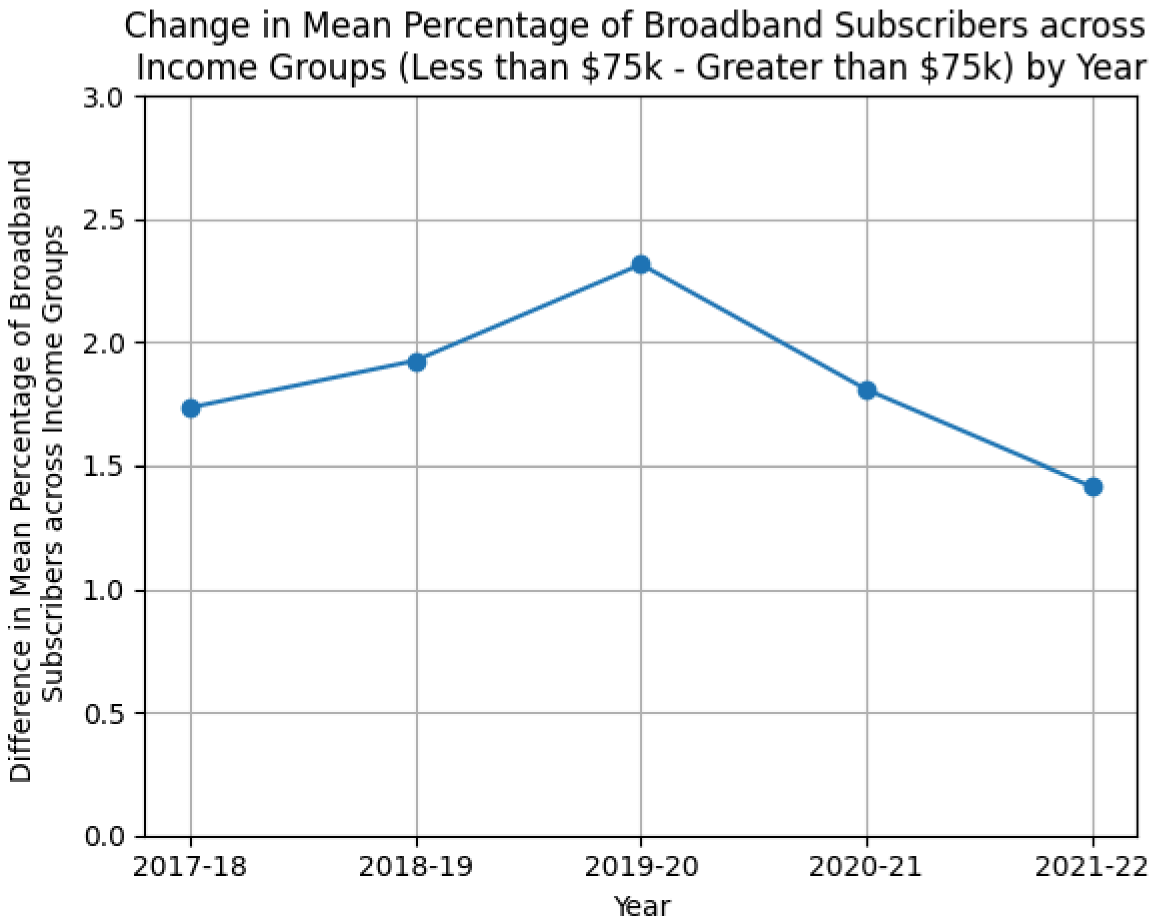

Figure 3: Comparison in low-income and high-income broadband uptake by year.

Because of the issues discussed above, as well as the obvious lack of discontinuity, this methodology was not pursued further.

For continuous treatment, initial filtering and cleaning of ACS and ACP data reduced the number of zip codes from 30309 to 28384. After coding zip codes for treatment as described above, we observe a mean net treatment value of 1.46%.

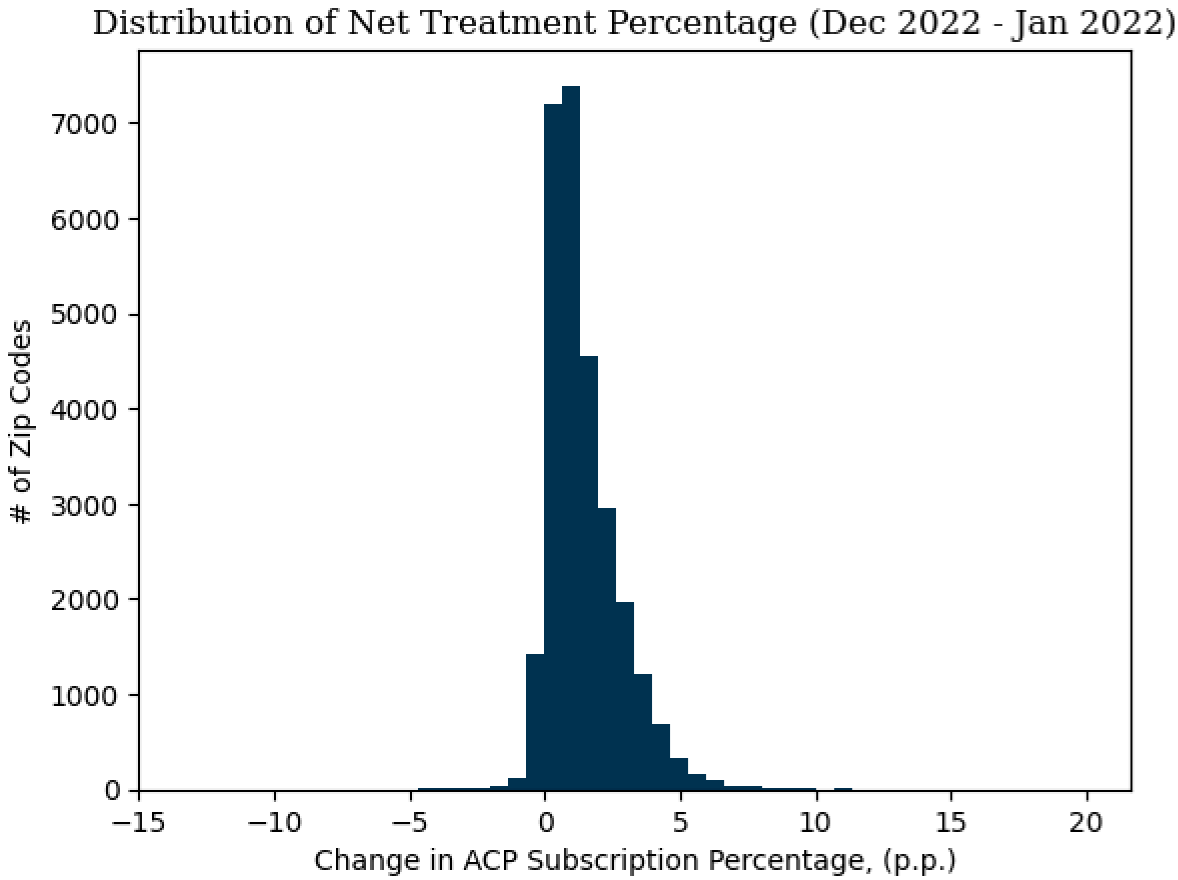

Figure 4: Distribution of treatment levels across zip codes.

First-difference estimation (Specification 1) yielded non-significant results, with a coefficient on treatment of 6.8 percentage points (95% confidence interval: [-1.7, 15.3]). Because this model does not account for any covariates, we expect this result to be a naive estimate of ACP effects heavily influenced by environmental factors. Most importantly, this regression specification ignores both rurality and broadband availability, so it is unlikely to be representative.

Next, regression with all covariates included was performed (Specification 3). This produced non-significant, but similar, results, with a coefficient on treatment of 6.27 percentage points (95% confidence interval: [-3.6, 16.1]).

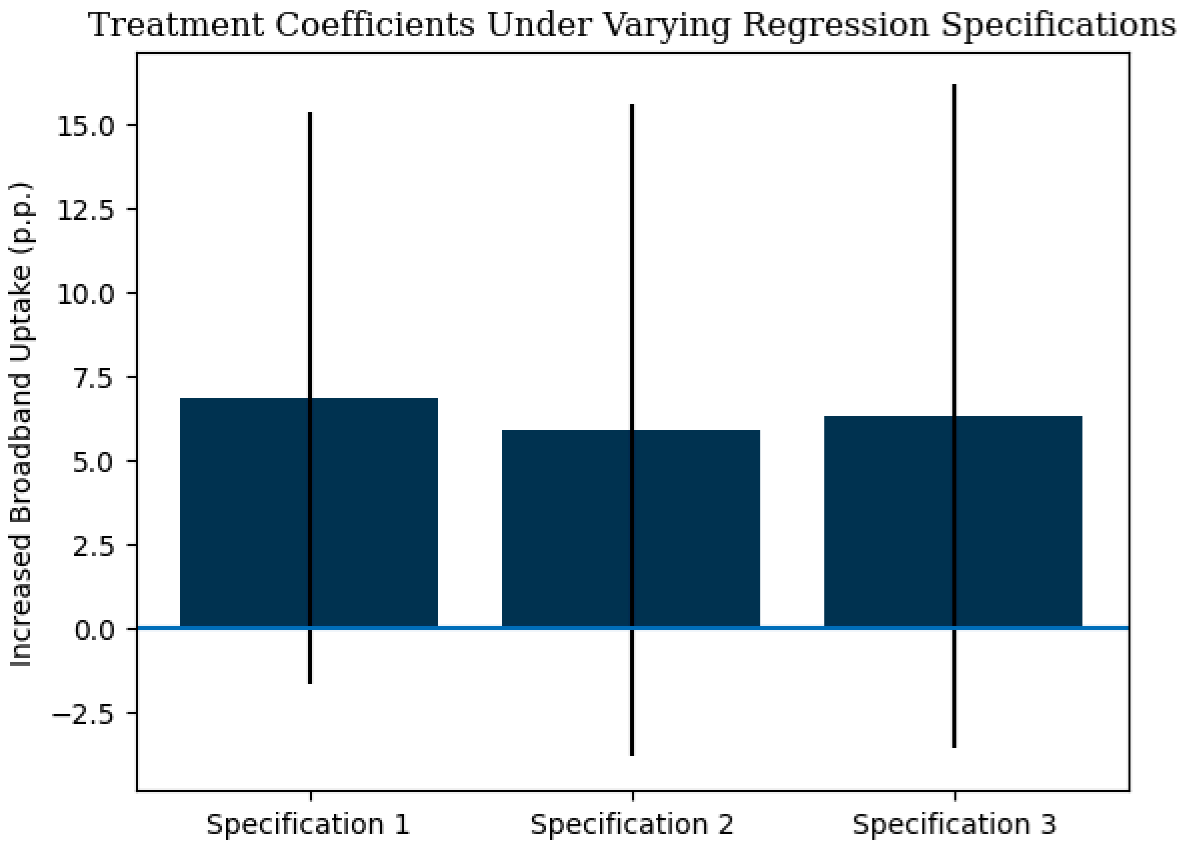

Figure 5: Regression results, in order of increasing covariates

However, including all of median household income ("MHI"), county population, and proxy-eligibility through income relative to the poverty line creates multicollinearity issues. This is unsurprising, as the number of households under 200% of the poverty line is close to a simple function of the median household income of the zipcode and the size of the zip code. We hypothesize two distinct causal effects from income and eligibility, but to check the robustness of our results, we repeat the experiment with population and average monthly claim removed as covariates (Specification 2). This produces similar results, with a coefficient on treatment of 5.88 percentage points (95% confidence interval: [-3.8, 15.8].

	Specification 2	**Specification 3**
Median Household Income	**1.17**	**1.59**
Rurality	**1.52**	**1.43**
Broadband Availability	**1.10**	**1.13**
Number of Eligible Households	**1.23**	**5.95**

Average Monthly Claim Amount per Subscription		**1.06**
Population		**6.33**

Figure 6: Covariate VHI for regression specifications.

Discussion

All regression designs yielded non-significant results. Therefore, we cannot conclude that the ACP had any strong effect on broadband uptake. However, we do find a consistently nonzero and positive correlation between ACP treatment and broadband uptake. Particularly because this signal matches the direction of the effect indicated by the continuous treatment, analyzing this effect offers a potentially interesting insight into the magnitude of the ACP effect, although any such analysis must be taken with a grain of salt because of the significance issue.

Interpretation of the above results is somewhat difficult. The binary-treatment experiments support a positive effect of the ACP on broadband uptake but do not offer a clear signal on the magnitude of this effect. The continuous-treatment effects allow for this: because the treatment variable is taken as a fraction of full-treatment within a zip code, the regression coefficient represents the linear effect on broadband uptake of full treatment (every household in the zip code receiving the ACP subsidy) relative to a control model without the ACP. This helps to contextualize the magnitude of the effect. Our main regression specification suggests that full ACP application to a zip code would result in a 6 percentage point increase in broadband subscriptions.

Further study with additional covariates is clearly necessary. Because of the complicated relationship between broadband uptake and elective ACP subsidies, the covariates included above almost certainly do not fully capture shared causal factors of ACP involvement and increased broadband. For instance, both broadband and ACP participation are likely correlated with government trust, which likely decomposes along demographic lines in ways not captured above. Horrigan, Whitacre, & Galperin find that ACP participation is heavily influenced by community factors, including the availability of public libraries, age, and minority status. Including these variables would likely increase the accuracy of this model.

Moreover, the data issues we encountered deserve additional exploration and attention. Counties with impossibly high ACP participation are likely to be non-representative along multiple broadband-related axes, so excluding them from the scope of the study could significantly alter the results. Determining the exact mechanism behind these data issues—or determining if they reflect the reality of ACP fund disbursement—would represent both an independently interesting research project and the resolution of a significant obstacle to a more in-depth analysis of the ACP proper.

Finally, because of data limitations, this study was only able to analyze the first year of the ACP. Further analysis using 2023 ACS data on the second year of the ACP is necessary. In order to increase the power of the study, looking backward into EBB data is also a possibility, although pandemic-era data irregularities may make that more difficult. Finally, including pre-pandemic years to build a more robust control could shed additional light on the effect of the ACP.

Conclusion

The ACP was not intended as a temporary measure but has currently ceased operations due to a lack of congressionally approved funding. Further analysis is necessary to determine whether re-funding the ACP would achieve its policy goals. The ACP inarguably subsidized low-income households receiving broadband. It remains an open question whether it also resulted in higher broadband participation nationwide. This question is crucial: if the ACP succeeded in this goal, broadband subsidies as a means of closing the digital divide would be justified. Otherwise, the ACP acted as an unusually-targeted welfare program, with minimal effects on the digital divide. Because our analysis does not achieve significance, we cannot conclude that the ACP was successful. However, our study helps contextualize the magnitude of any potential beneficial effect of the ACP.

Our regression analysis results in a point estimate of the treatment coefficient on broadband uptake of approximately 6 percentage points. Because the ACP peaked at 19 million subscribers in August of 2023, we estimate that 1.14 million additional broadband subscriptions were attributable to the ACP at that point. Because our study only looks at 1 data year, this estimate does not account for any durable effects of temporary subsidies: for instance, it's entirely possible that homes that acquire a broadband subscription through the ACP tend to retain that subscription after losing the ACP subsidy. The ACP enrolled a total of 23.2 million households, giving an upper-bound estimate of 1.39 million additional broadband subscriptions.

Particularly our results are not significant, these estimates are overly precise. Taking the estimate from the upper end of our confidence interval, we can bound the likely effect of the ACP at 3.74 million additional broadband subscriptions over the life of the program. As expected, this maximum effect size contradicts FCC survey data on the number of ACP subscribers who received broadband subscriptions because of the ACP. Our lower-bound estimate is negative, which because the ACP almost certainly did not reduce broadband subscriptions, should be interpreted as a failure to conclude that the ACP had a positive effect.

Overall, further research is necessary to characterize the ACP's effects. However, our research finds that the maximum likely effect of the ACP on additional broadband subscriptions is lower than previously reported. These estimates, taken as ranges, likely represent a more robust way to understand the uptake effect of the ACP for cost-benefit policy analysis than existing survey data.

Appendix (Full Regression Tables)

Specification 1:

	coef	std err	z	P>|z|	[0.025	0.975]
Intercept	1.6010	0.083	19.355	0.000	1.439	1.763
net_treatment_percent	0.0681	0.044	1.564	0.118	-0.017	0.153

Specification 2:

	coef	std err	z	P>|z|	[0.025	0.975]
Intercept	2.4489	0.370	6.611	0.000	1.723	3.175
net_treatment_percent	0.0588	0.049	1.190	0.234	-0.038	0.156
RUCA1	-0.0452	0.019	-2.376	0.017	-0.082	-0.008
Q("25d3u")	-0.0029	0.003	-0.862	0.389	-0.009	0.004
Q("Median household income")	-3.678e-06	2.12e-06	-1.731	0.083	-7.84e-06	4.86e-07
Q("eligible")	-3.459e-05	1.84e-05	-1.877	0.060	-7.07e-05	1.52e-06

Specification 3:

	coef	std err	z	P>|z|	[0.025	0.975]
Intercept	2.9842	0.491	6.080	0.000	2.022	3.946
net_treatment_percent	0.0627	0.050	1.250	0.211	-0.036	0.161
RUCA1	-0.0429	0.019	-2.309	0.021	-0.079	-0.006
Q("25d3u")	-0.0037	0.003	-1.105	0.269	-0.010	0.003
Q("Median household income")	-4.203e-06	2.52e-06	-1.666	0.096	-9.15e-06	7.4e-07
Q("population_2020_5y")	3.462e-06	1.27e-05	0.273	0.785	-2.14e-05	2.83e-05
Q("eligible")	-4.35e-05	4.65e-05	-0.934	0.350	-0.000	4.77e-05
Q("Total Claim Amount Per Sub")	-0.0136	0.009	-1.444	0.149	-0.032	0.005

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