Wired and Hired: Employment Effects of Subsidized Broadband Internet for Low-Income Americans*

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Abstract

We present evidence on the relationship between broadband pricing and labor market outcomes for low-income individuals. Specifically, we estimate the effects of a Comcast service providing discounted broadband to qualifying low-income families. We use a triple differences strategy exploiting geographic variation in Comcast coverage, individual variation in eligibility, and temporal variation pre- and post-launch. Program enrollment increases the probability that an eligible low-income individual is employed by 4.4 percentage points (7.8%), driven by greater labor force participation and decreased probability of unemployment. Internet use increased substantially where the program was available, narrowing the income-broadband gap by at least 40 percent.

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1 Introduction

Many Americans live without a wired broadband connection in their homes, with a large number citing price as the limiting factor (Horrigan and Duggan, 2015). Lack of affordability has led to substantial income-based disparities in broadband adoption rates: 56% of families earning less than $40,000 annually have a broadband subscription, compared to 86% of families earning more than $70,000.[1] Without the convenience of home broadband, poor families may be less equipped to navigate the labor market in the digital era. Job seekers without broadband are more than 20 percent less likely to use online resources for job search, and face other obstacles to employment that modern online tools may be suited to address.[2]

This paper studies whether changes in broadband pricing can meaningfully close the income-broadband gap and produce downstream benefits in the labor market for economically disadvantaged Americans. We do so by analyzing Internet Essentials, a commercial broadband discount program launched in 2012 by Comcast, the nation's largest internet service provider by subscriber count. Internet Essentials offers 15 megabits-per-second (Mbps) broadband internet for $9.95 per month to families with children eligible for free and reduced-price lunch through the National School Lunch Program (NSLP). The discounted pricing is $20 to $30 lower than typical, non-promotional prices for equivalent speeds. The program also provides ancillary benefits such as fee waivers and instructional materials to mitigate other financial and psychic costs of connecting online from home. According to its 2018 progress report, Internet Essentials has connected over six million low-income Americans to the internet since launching in 2012, with over 90 percent of customers connecting online from home for the very first time (Comcast Corporation, 2018).

Although the relationship between broadband access and labor market outcomes has been a topic of interest for policymakers and researchers alike (Council of Economic Advisers, 2015), evidence from economics is almost entirely comprised of studies analyzing geographic expansions of broadband infrastructure.[3] This paper's novel focus on broadband affordability builds on the literature in several important ways. First, 97 percent of the U.S. population lives where 10 Mbps broadband speeds are available—enough to comfortably stream Netflix in HD.[4] Despite this, only 56 percent of low-income

[1] Authors' calculation using 1-Year Estimates from the American Community Survey (2013-17).

[2] Authors' calculation using the 2015 Pew Broadband Survey on Gaming, Jobs, and Broadband.

[3] See expansions from Hjort and Poulsen (2019) in Africa, Akerman, Gaarder and Mogstad (2015) and Bhuller, Kostol and Vigtel (2019) in Norway, Briglauer et al. (2019), Gürtzgen et al. (2018), and Denzer and Schank (2018) in Germany, and Dettling (2017) and Kolko (2012) in the United States.

[4] See Figure 3 in Tomer, Kneebone and Shivaram (2017), and Netflix's "Internet Connection Speed Recommendations"

Americans actually own broadband subscriptions. Expanding broadband coverage remains an important policy objective; however, the persistent income-broadband gap is arguably the more pressing and current issue. To the best of our knowledge, this is the first paper in the economics literature on broadband and labor market outcomes to focus specifically on low-income families. Second, studies leveraging geographic broadband expansions typically document greater broadband adoption rates in both households *and firms*, making it difficult to separately identify how much of the observed labor market impact should be attributed to each. The policy variation used in this paper is specific to broadband pricing for low-income families, allowing us to isolate the effects of household broadband take-up. Finally, the policy variation we use is the most recent in the literature. This is important given that it coincides with recent, explosive growth in mobile wireless technologies that may influence the relative importance of wired broadband connections moving forward.

To empirically estimate the employment effects of Internet Essentials, we use the fact that only eligible families living within Comcast's broadband service area after 2012 could enroll in Internet Essentials. This meant that an individual's ability to enroll in Internet Essentials was determined by a confluence of three sources of variation: 1) geographic variation in Comcast availability, 2) temporal variation pre- and post-launch, and 3) individual variation in eligibility. We leverage this variation in a triple differences framework comparing outcomes of eligible and ineligible individuals across locations with varying Comcast broadband coverage rates before and after the launch of Internet Essentials. Identification relies on the assumption that *differences* in labor market outcomes between eligible and ineligible individuals are uncorrelated with Comcast coverage rates before and after 2012, but for the impact of Internet Essentials.

We also exploit the fact that program eligibility based on the NSLP is two-pronged. Individuals with children eligible for free/reduced-price lunches must have 1) family incomes beneath a specific threshold, and 2) at least one child attending K-12. In our main specification, we restrict the control group of ineligible individuals to those who meet the income requirement but do not have any school-aged children. Ineligible individuals in this group are more likely to access similar kinds of labor markets and job opportunities as those who are eligible, decreasing the likelihood that post-2012 differences between the two groups diverge in high- versus low-Comcast areas for reasons other than Internet Essentials. Building on this, we show that the estimated employment effects are reassuringly similar when further refining the control group of ineligibles to low-income individuals who

(https://help.netflix.com/en/node/306)

are parents, but whose children are either too young or too old to attend K-12. The results also hold when restricting the entire sample to low-income parents whose children's ages vary within a narrow bandwidth above and below the cutoff for kindergarten—incorporating elements of regression discontinuity into triple differences.

The data we use come from two sources. We first use data from the National Telecommunications and Information Administration to construct estimates of local Comcast broadband coverage rates. These data include census block-level indicators of where Comcast provides broadband service in the United States. We then link these data to individuals and their outcomes in the American Community Survey (ACS) at the Public-Use Microdata Area (PUMA) level, which is the lowest level of geography identified for all survey respondents. The ACS data also contain information on family income and children, which we use to determine program eligibility.

The results indicate that PUMA-wide availability of Internet Essentials increases the probability that an eligible low-income individual is employed by 0.9 percentage point (off a base of 56.7 percent). This estimate is an intent-to-treat (ITT) estimate of Internet Essentials, since we do not observe actual program enrollment. After adjusting for nationwide program take-up rates, we estimate that the treatment-on-the-treated (TOT) effect of enrolling in Internet Essentials is approximately 4.4 percentage points (7.8%). We find evidence that the effect is driven both by increases in labor force participation and decreases in the probability of being unemployed. Enrolling in Internet Essentials also increases average earnings by 5.3%. Using the earnings effect, we calculate that the average benefit to each household is approximately $1,060. We estimate that this benefit is roughly double the annual cost to provide the service, which includes a monthly broadband subsidy, fee waivers, and other administrative costs. This calculation does not include the value of other potential benefits unrelated to labor market outcomes.

We also conduct a simple placebo test leveraging the fact that Internet Essentials was the only low-income program of its kind among large internet service providers until 2016. This implies that post-2012 employment differences between eligible and ineligible individuals should not be associated with broadband coverage rates of other large internet service providers. We confirm that the significant effects associated with post-2012 Comcast coverage vanish when using broadband coverage rates of the next three largest internet service providers: AT&T, Charter (Time Warner), and Verizon. This test bolsters a causal interpretation of our findings.

Internet use data from the Current Population Survey suggest that state-wide exposure to Internet

Essentials increased home internet use among eligibles by roughly 8 percentage points. This estimate, in conjunction with program take-up rates, implies that the program induced approximately 40 percent of its customers to purchase a broadband subscription. The remaining customers either switched providers, or would have purchased a subscription in the absence of the program. The effect size is commensurate with a reduction in the income-broadband gap of roughly 39 percent. We caveat that the data used for this calculation have important limitations. Other work by Rosston and Wallsten (2019) approximates that 66 percent of Internet Essentials customers were induced to purchase a broadband subscription. Survey data from Comcast also indicate that as many as 90 percent of its customers were first-time broadband subscribers. This suggests that our estimates, though substantial, may represent an underestimate of the program's true impact on the income-broadband gap.

Our findings are generally consistent with previous work in economics documenting positive labor market effects resulting from the geographic expansion of broadband infrastructure. Hjort and Poulsen (2019) leverage the arrival of sub-marine internet cables in Africa and find positive effects on employment and incomes, particularly for high-skilled workers. Akerman, Gaarder and Mogstad (2015) examine a staggered expansion of broadband infrastructure in Norway and also find positive effects on economic outcomes for high-skilled workers. Bhuller, Kostol and Vigtel (2019) use the same Norwegian expansion to document increases in the speed and quality of labor market matching. Denzer and Schank (2018), Briglauer et al. (2019), and Gürtzgen et al. (2018) study expansions of broadband across Germany and find evidence of shortened unemployment durations, but no net effects on job creation. Finally, Dettling (2017) uses state-wide shares of multifamily residences to instrument for the diffusion of internet access across the U.S., and finds increases in labor force participation rates of married women (with no corresponding effect for single women or single/married men).

Broadband has become a near-necessity in the digital era, yet many low-income families remain unwilling or unable to pay for a broadband subscription. The findings in this paper suggest that Internet Essentials, which subsidized broadband subscriptions by $20 to $30 per month, meaningfully increased broadband adoption and increased employment and earnings for low-income individuals. The findings also suggest that there remain non-trivial benefits to having a home broadband subscription even in light of the growing ubiquity of smartphones, public WiFi, and other methods of connecting online.

2 Background

2.1 Internet Terminology

We begin by establishing some terminology conventions. This paper focuses on *in-home wireline broadband*, meaning high-speed internet that is accessed from home and is delivered via cable lines, digital service lines (DSL), satellite, or fiber. This stands in contrast to internet that is delivered via dial-up (not high-speed) or via wireless/mobile data plans (not wireline). In Appendix C, we provide a short description of how broadband is originated and how it ultimately reaches consumers. For brevity, references to broadband throughout this paper refer to in-home wireline broadband, unless specified otherwise. Next, a vendor for internet service (such as Comcast, AT&T, or Google Fiber) is an *internet service provider* ("ISP"). Finally, we refer to broadband *availability* as a location-specific term that reflects whether an ISP supplies broadband service to end users in that location.[5]

2.2 Broadband Affordability and Labor Market Outcomes

Numerous barriers impede broadband adoption for low-income families (Tomer, Kneebone and Shivaram, 2017). Figure 1 depicts a clear negative relationship between local poverty and broadband subscription rates. Price is one of the largest barrier to broadband adoption, with 50% of non-broadband users indicating that cost is the primary reason why they do not have a broadband subscription (Horrigan and Duggan, 2015).[6] The monthly cost of internet can start at $40 for entry-level speeds, and prior work has estimated that eliciting a 10% increase in subscribers would require a price reduction of 15% (Carare et al., 2015). Setting up a connection also requires purchase or rental of various equipment and peripheries, and ISPs frequently charge a one-time activation fee for first-time customers. Not least, a computer (or smartphone/tablet) is required to access to access broadband from home. Beyond the financial costs of accessing internet, lack of digital literacy and trust of technology may impose additional psychic costs.

Access to affordable home broadband can affect labor market outcomes in a variety of ways. First, the internet is an important resource for job seekers in the digital era. In a 2015 survey conducted by

[5]The FCC states: "Fixed broadband connections are "available" in a census block if the provider does, or could, within a service interval that is typical for that type of connection—that is, without an extraordinary commitment of resources—provision two-way data transmission to and from the Internet with advertised speeds exceeding 200 kbps in at least one direction to end-user premises in the census block" (Federal Communications Commision, 2016).

[6]Other cited reasons for non-broadband users include: smartphone does the job (14%), options available outside the home (12%), service not available or not sufficiently useable (6%), and some other reason (19%). We note that numbers provided here differ slightly from Horrigan and Duggan (2015), who include non-responses in their tabulations, which we omit.

the Pew Research Center on broadband and job search, 79% of Americans who looked for jobs from 2013 to 2015 indicated that they used online resources during their job search, with roughly one-third of job seekers indicating that online resources were the single-most effective resource that they used (Smith, 2015). We use the same survey data to characterize job seekers with and without broadband in their homes. In Table A1, we break down how usage rates across six job search resources (connections, online search, employment agencies, print ads, job fairs, and other) differ between respondents with and without broadband. 81% of job seekers with broadband used online resources compared to 67% of those without broadband, a 14 percentage point difference (21%). Respondents also indicated the resource that they found to be most effective for their job search. Those with and without broadband favored online search at similar rates (31% versus 32%). However, conditional on having used online search, 38% of those with broadband indicated that it was the most useful resource (31%/81%), compared to 48% of job seekers without broadband (32%/67%). This suggests that while job seekers without broadband were less likely to use online search, those who managed to connect online through other means found it to be disproportionately effective for their search.

Several factors could counteract the search-enhancing benefits of cheaper broadband, which we describe here and formalize in Appendix A. The existence of substitutes for broadband, such as access through local libraries or public WiFi, could diminish the impact of Internet Essentials if low-income job seekers effectively leverage these alternative options. 12% of non-broadband users cite the availability of such alternatives as the main reason why they do not have broadband (Horrigan and Duggan, 2015). Financially constrained households may also be forced to choose between purchasing a mobile data plan versus purchasing a broadband subscription. 14% of non-broadband users do not have broadband subscriptions because a smartphone delivers sufficient access to the internet, and 21% of households earning less than $20,000 per year have a smartphone but no broadband at home (Horrigan and Duggan, 2015).[7] Individuals may also distort their job search intensity, although the direction of such distortions are theoretically ambiguous and depend on the elasticity of substitution between time and inputs into activities such as leisure and home production (Dettling, 2017; Aguiar and Hurst, 2007). Better leisure options could also increase the option value of remaining unemployed, decreasing the expected net utility gain from job search.

[7] While capable of delivering high speeds to end users with the convenience of a handheld device, mobile internet users frequently report limitations from slow and unstable connections, costly data limits, and reduced functionality for tasks such as word processing, file composition/transfer, and browsing websites not optimized for mobile experiences (Tomer, Kneebone and Shivaram, 2017). Of people who used smartphones to apply for a job, 47% had difficulties accessing content that did not display properly, 38% had difficulties entering in a large amount of text, 37% had difficulties submitting required files and supporting documentation, and 23% had difficulties bookmarking saved job applications for later (Smith, 2015).

Home broadband could also affect those on the extensive margin of labor force participation. The element of convenience conferred by home job search may be crucial for individuals unable to travel to libraries or public WiFi hotspots due to location and time constraints.[8] A home broadband connection also decreases frictions associated with working; Dettling (2017) finds that labor force participation among married women increased with the expansion of home internet access in the U.S., which created opportunities for telework and decreased the cost of home production. The internet can also mitigate work frictions by serving as a source of information for topics such as commuting and child care options.

Home broadband access could also affect the quality of jobs available to job seekers. Bhuller, Kostol and Vigtel (2019) find that the arrival of broadband in Norway led to improvements in the efficiency and quality of labor market matching, resulting in higher starting wages and job matches that were more likely to be in distant locations. Individuals could also use home broadband to increase labor productivity through skill acquisition. The ability to navigate the internet is a skill that can be used productively in many job settings, and can be improved by regularly using the internet at home. If affordable broadband induces the purchase of a desktop or laptop, individuals could also benefit by acquiring computer skills that employers may value.

2.3 Internet Essentials by Comcast

Internet Essentials was conceived during Comcast's proposed merger with NBC Universal in 2010 (Davidson, Santorelli and Kamber, 2012). In response to antitrust concerns raised during the review process, Comcast submitted a letter to the FCC where it committed to deploying a program that would "substantially increase broadband adoption in low-income homes throughout Comcast's service area" (Zachem, 2010). In the letter, executive vice president Kathy Zachem noted that among households located in Comcast's service territory and earning less than \$20,000 in annual income, broadband subscription rates were only 40%. After the approval of the merger, the commitment to implement Internet Essentials became enforceable by the FCC (Davidson, Santorelli and Kamber, 2012). The program first piloted in Chicago and DC in 2011, and launched nationwide in 2012 to all locations within Comcast's service area.

[8] Additionally, while public libraries typically serve as hubs for accessing the internet, not all libraries are equipped with an adequate stock of computers for job seekers, and branches with large computer stocks have even instituted strict time limits for users in response to excessive wait times that can exceed several hours (Hannah-Jones, 2009). In practice, job seekers would need to go to the library every day to check and respond to emails, provide supporting documents, etc. Public WiFi hotspots represent another viable alternative, but come with location-specific restrictions, slower connection speeds, and potential security risks.

The core feature of the program is that households are provided 15/2 Mbps broadband service for $9.95 a month, plus applicable taxes.[9] In addition to the subsidized price, Comcast waives all fees, including a one-time activation fee (typically costing $50), a one-time installation fee ($50 or greater), and a modem rental fee ($10 per month). A wireless router, which enables WiFi access throughout a residence, is also provided free of charge. Families are also given the option to purchase a subsidized low-cost computer for a fixed price of $149.99, and the program has subsidized approximately 85,000 such computers as of 2018 (Comcast Corporation, 2018).[10] Finally, customers are given access to complementary Internet and technology training resources, which can be accessed online, in print, or in person.

Families are eligible to enroll if several conditions are met. First, families must have a child eligible for free/reduced price lunch via the National School Lunch Program (NSLP). Eligibility for free/reduced price lunches, in turn, is based on whether a student's family income falls below 185 percent of the federal poverty level (FPL). The NSLP eligibility requirement stems from Comcast's original commitment to market Internet Essentials as a means to provide low-income students with home broadband access for schoolwork. At launch, the program was only available to families with students eligible for free lunches (130% FPL), but eligibility was quickly expanded to those on reduced price lunches within the first several months of the program. Eligibility is verified through school districts annually. Second, Comcast restricts eligibility to families that do not have outstanding debt owed to Comcast that is less than a year old.[11] Finally, families cannot have subscribed to Comcast internet within the last 90 days. This restriction makes it more likely that program enrollees are first-time internet subscribers. In 2016, Internet Essentials expanded eligibility to individuals receiving public housing assistance (including Section 8 vouchers), low-income veterans, and households receiving various public assistance programs including Medicaid, SNAP, TANF, and SSI.

Beginning in 2016, other ISPs also began rolling out their own broadband subsidy programs for low-income households. AT&T launched its "Access" program, which provides subsidized broadband for households receiving benefits from the Supplemental Nutrition Assistance Program (SNAP). The federal government also reformed its Lifeline program in 2016. The program had traditionally

[9] "Mbps" is an abbreviation for megabits per second, and 15/2 represents download and upload speeds, respectively. 15Mbps download speeds are considered the minimum needed to stream HD content; see: https://broadbandnow.com/guides/how-much-internet-speed-do-i-need.

[10] In comparison to the 6 million customers who have enrolled in Internet Essentials, only 85,000 computer vouchers were used. This seems to suggest that hardware costs are not necessarily the binding constraint to broadband adoption. In fact, program enrollees save $20-$30 per month on broadband, which could quickly exceed the one-time cost of purchasing a computer even in the short-run.

[11] An amnesty policy was introduced in August 2014 for families with past-due debts to Comcast.

provided subsidized phone service to low-income Americans, and would begin allowing recipients to use their subsidy on broadband service from participating ISPs (Federal Communications Commision, 2018). However, over 80 major telecommunications providers including AT&T, Verizon, CenturyLink, and Frontier sought major exceptions or opted out completely from the program (Holsworth, 2016). Numerous small-scale broadband subsidy programs also piloted around this time, including the ConnectHome program run by the US Department of Housing and Urban Development (HUD), which provided free or low-cost internet to roughly 20,000 people in HUD-assisted housing units. To the best of our knowledge, Internet Essentials was the only widely available broadband subsidy program from 2012 through 2015.

3 Methods and Data

3.1 Overview

Internet Essentials launched nationwide in 2012 and was only available to eligible families living within Comcast's broadband service area. This meant that one's ability to enroll in Internet Essentials was determined by geographic variation in Comcast broadband coverage, temporal variation pre- and post-launch, and individual variation in eligibility status. We leverage these three sources of variation using triple differences (Gruber, 1994) to determine whether Internet Essentials improved labor market outcomes for eligible low-income individuals relative to ineligible individuals. Identification relies on the assumption that *differences* in labor market outcomes between eligible and ineligible individuals are uncorrelated with local Comcast broadband coverage before and after 2012, but for the impact of Internet Essentials. Our estimate is an intent-to-treat effect of Internet Essentials availability because we do not directly observe program enrollment.

3.2 Estimating the Intent-to-Treat Effects of Internet Essentials

We briefly establish two conventions. First, we refer to individuals eligible for the program as "eligibles" (treatment group), and those who are not eligible as "ineligibles" (control group). Second, Comcast coverage rates refer to the percentage of the population within a given area living within Comcast's broadband service territory. In triple differences, we compare differences between eligibles and ineligibles across areas with varying degrees of Comcast coverage, before and after the launch of Internet Essentials in 2012.

The triple differences estimating equation is as follows:

$$\begin{aligned} y_{igt} = \alpha &+ \rho(Eligible_{igt} \times Comcast_g \times Post_t) + \delta_1(Eligible_{igt} \times \lambda_t) \\ &+ \delta_2(Eligible_{igt} \times \gamma_g) + \delta_3(\gamma_g \times \lambda_t) + X'_{igt}\beta + \epsilon_{igt} \end{aligned} \tag{1}$$

where y_{igt} represents a labor market outcome for individual i in geographic area g and year t. Outcomes include the probability of being employed, the probability of being in the labor force, the probability of being unemployed, and log wage earnings. Our primary geographic unit of observation is the Public-Use Microdata Area ("PUMA"), as it is the lowest level of geography that is identified for all observations in the outcomes data. PUMAs are geographic boundaries that contain at least 100,000 people; densely populated counties can be composed of many underlying PUMAs, whereas sparsely populated counties will often combine together to form a single PUMA. We explore other levels of geographic aggregation in robustness tests. $Eligible_{igt}$ is a binary indicator equal to one if the individual is eligible for Internet Essentials. $Comcast_g$ is the percentage of PUMA g's population living within Comcast's broadband service area as of 2012. $Post_t$ is an indicator for observations in 2012 and later. The interaction of $Comcast_g$ and $Post_t$ represents the coverage of Internet Essentials in PUMA g, which is equal to zero prior to 2012 and equal to $Comcast_g$ thereafter. Further interacting this term with $Eligible_{igt}$ captures the incremental effect of Internet Essentials availability for eligibles. λ_t and γ_g are time and PUMA fixed effects, and the inclusion of PUMA-by-year fixed effects $\gamma_g \times \lambda_t$ absorb the underlying one-way fixed effects. X_{igt} is a vector of individual-specific covariates, including gender, age and its square, race (indicators for Black and Hispanic), marital status, years of education, and number of children. All standard errors are adjusted for clustering at the PUMA level. We restrict the sample time frame to the post-recession years 2009 through 2015. Although data are available for later years, we exclude them in our core analysis given the launch of other major internet subsidy programs in 2016, such as AT&T's "Access" program and the federal government's expansion of Lifeline subsidies to cover broadband service. 2016 also marks the first year that Internet Essentials began expanding eligibility to individuals on other public assistance programs beyond free/reduced-price lunch. We revisit this data assumption in robustness checks.

The parameter of interest ρ represents the effect of PUMA-wide coverage of Internet Essentials on labor market outcomes y_{igt}. The pairwise interaction terms associated with the parameters δ_1 through δ_3 control for a variety of confounding factors that standard differences-in-differences may miss. The interaction $Eligible_{igt} \times \lambda_t$ controls for nationwide, time-varying differences between eligibles and non-eligibles. $Eligible_{igt} \times \gamma_c$ controls for permanent, PUMA-specific differences between eligibles and non-eligibles. $\gamma_g \times \lambda_t$ non-parametrically absorbs all PUMA-specific trends that are invariant to eligiblity status. The PUMA-by-year two-way fixed effect absorbs a substantial amount of variation and greatly mitigates the influence of confounding regional labor market trends in areas with greater Comcast penetration. The identifying variation in y_{igt} that remains after controlling for the three pairwise interaction terms contains only time-varying, within-PUMA differences between eligibles and ineligibles. ρ is an estimate of how much of this remaining variation is captured by local availability of Internet Essentials.

The identifying assumption is that within-PUMA *differences* in labor market trends between eligibles and ineligibles in PUMAs with higher versus lower Comcast service would have remained the same in the absence of Internet Essentials, conditional on covariates X_{igt}. Taking the difference in trends between eligibles and ineligibles removes the influence of shared unobservable confounders that may bias standard differences-in-differences analyzing eligibles alone. The identifying assumption is violated if, in the absence of Internet Essentials, differences between eligibles and ineligibles would have trended differently in PUMAs with greater Comcast coverage rates. Put another way, any correlation between PUMA Comcast coverage rates and eligibility-based differences in labor outcomes after 2012 (conditional on X_{ict}) must have been due to Internet Essentials.

One way to relax the identifying assumption is to refine the control group of ineligibles so that differences in labor outcomes between eligibles and ineligibles are less likely to diverge for reasons unrelated to Internet Essentials. We do so by exploiting the fact that program eligibility is two-pronged and depends on both family income and having a child in K-12. We use this to construct three versions of the control group composed of: 1) all ineligibles, 2) ineligibles that have a school-aged child but do not meet the low-income threshold, and 3) low-income ineligibles that do not have school-aged children. Restricting the control group to ineligibles with school-aged children eliminates the possibility that diverging outcomes between treatment and control are due to changing labor market conditions for parents with school-aged children. Similar logic applies when restricting the control group to ineligibles with incomes below 185% of the FPL. Restricting the control group to low-income ineligibles is particularly useful given that low-income eligibles and ineligibles are likely to access similar types of labor markets and job opportunities.

We also use the two-pronged eligibility structure to test even more restrictive versions of the control group at the cost of smaller sample sizes. For example, ineligibles can be further restricted to include only low-income individuals with children who are too young (or too old) for K-12. Taking this even further, we compare eligibles whose oldest children are elementary-aged versus low-income ineligibles whose oldest children are pre-K-aged. This approach incorporates elements of regression discontinuity into triple differences by comparing low-income parents whose oldest child's age varies within a narrow bandwidth about the age cutoff for kindergarten. As the treatment and control groups become more observably similar, firms become less able to differentiate between the two (conditional on X_{igt}), and violations of the identifying assumption must occur through more specific and complicated channels.

We highlight several potential threats to identification that could cause within-PUMA differences between eligibles and ineligibles to diverge in ways aligning with Internet Essentials availability. First, post-2012 Comcast exposure could directly benefit eligibles independent of Internet Essentials. This could occur if businesses which tend to differentially hire eligibles were attracted to areas with cost-effective and reliable broadband infrastructure after 2012. For example, Comcast Business is a subsidiary of Comcast which provides cost-effective, tailored broadband solutions for small businesses. However, most prominent ISPs are likely to provide competitively similar services. This motivates a simple placebo test in Section 4.3 testing whether broadband coverage rates of other large ISPs correlate with labor market differences between eligibles and ineligibles after 2012.

Second, we consider the possibility that after the launch of Internet Essentials, Comcast chose to expand in locations where labor conditions were improving differentially for eligibles. This would imply that any expansion in coverage rates after 2012 is endogenous to local labor conditions among eligibles. The threat of endogenously time-varying coverage rates motivates our decision to fix $Comcast_g$ to 2012 levels; however, we show in robustness checks how the results change when allowing $Comcast_g$ to vary over time.

Third, Comcast may have proactively located their broadband networks in markets where employment opportunities for eligibles were predicted to expand relative to ineligibles after 2012. We dismiss this possibility for three reasons. First, Comcast originated as a cable provider and has historically tied its expansion strategy around cable market growth as opposed to labor market growth for low-income families (although the two could be correlated). Second, this violation requires that the location of broadband networks be correlated with diverging outcomes between eligibles and ineligibles *specifically after 2012*. Such foresight is unlikely given that the program's 2012 launch was tied to the approval of Comcast's merger with NBC Universal; the timing of the program's launch is therefore unlikely to be endogenous to underlying labor market trends. Third, prior to Internet Essentials, Comcast would have little strategic reason to proactively expand in areas where outcomes were expected to diverge differentially for eligibles.

Finally, we do not rule out the possibility that Comcast coverage simply correlates with other factors causing differential trends between eligibles and ineligibles after 2012. High Comcast coverage rates in a given area could be symptomatic of a robust economic infrastructure facilitating quicker post-recession recovery, which might have differentially benefited eligibles looking to get back to work sooner. The placebo test in Section 4.3 comparing employment effects associated with Comcast versus other large ISPs can potentially rule out violations related to correlates of broadband density. Additionally, our strategy of refining the control group of ineligibles to more closely resemble eligibles limits the channels through which unobserved correlates of Comcast coverage could differentially benefit eligibles.

3.3 Calculating the Treatment-on-the-Treated Effect of Internet Essentials

The treatment-on-the-treated effect of enrolling in Internet Essentials can be approximated by dividing the estimated intent-to-treat effects by the national program take-up rate (averaged across post-treatment years). We calculate the take-up rate by obtaining annual estimates of the number of households that Internet Essentials served.[12] These estimates are provided in Column (1) of Table 1. Internet Essentials enrolled 150,000 households during its first year of operation in 2012, which increased to 600,000 households by the end of 2015.

We then translate the household estimates to individual estimates. We do this by multiplying the number of households by the average family size among eligibles in the ACS (4.5). Column (2) provides the translated estimates for number of individuals served. Next, we estimate the potential market size of Internet Essentials—the total population of eligible individuals living in locations where Comcast was available. We calculate this by multiplying the total population of eligibles in each PUMA by the percentage of the population in each PUMA living in a Census block where Comcast was available. These totals, which tend to gravitate around 8 million individuals, are presented in Column (3). Column (4) presents the estimated take-up rates, obtained by dividing the number of individuals served by the total market size of potential customers. In 2012, the take-up rate was 8.2%, which increased to 34.2% by 2015. The blended take-up rate across all four years is 20.3%. We use this estimate as the link between the ITT and TOT estimates.

[12]These estimates are provided in Comcast's 5-Year progress report for Internet Essentials.

3.4 Calculating Comcast Coverage Rates

Availability of Internet Essentials is tied to geographic coverage of Comcast service. From 2010 to 2014, data on ISP coverage was collected via the State Broadband Initiative, run by the National Telecommunications and Information Administration (NTIA). The FCC Form 477 continued tracking the data beginning in 2014. The data for any given ISP include a list of Census blocks where broadband service can be provided to at least one location within the block. The data do not detail what *percentage* of a census block's population live within the provider's service area, although blocks are typically small and contain resident populations ranging from zero to several hundred (in the case of a single block containing a large multi-family housing unit).

While the coverage data are available at the Census block level, individuals in our outcomes data can only be identified at higher levels of geographies, such as counties, metros, PUMAs, and states. We aggregate the data to the desired level of geography by computing the percentage of a geography's population living in a Census block covered by Comcast. Census block population counts are obtained from the 2010 Decennial Census. For a given geography with B underlying census blocks, we calculate:

$$\text{Comcast Coverage} = \frac{\sum_{b=1}^{B} \text{Population}_b \times \mathbb{1}\{\text{Covered by Comcast}\}_b}{\sum_{b=1}^{B} \text{Population}_b} \tag{2}$$

where the indicator function resolves to one if Comcast supplies broadband in Census block b. Figure 2 plots the geographic distribution of Comcast availability across counties. Comcast appears to have footholds in all major regions across the US, although coverage appears to be most concentrated along the Northeast Corridor. Figure 3 provides a histogram of Comcast availability at the PUMA level. The distribution is roughly bimodal; approximately one-half of PUMAs do not have any Comcast coverage, whereas one-third of PUMAs have greater than 75 percent coverage.

3.5 Linking Internet Essentials to Eligibility and Outcomes

We link geographic availability of Internet Essentials to individuals by merging Comcast coverage rates to American Community Survey 1-Year Estimates from 2009-2015 obtained via the Integrated Public Use Microdata Series ("IPUMS") (Ruggles et al., 2017). Linking is made possible with the help of cleaned geographic indicators provided by IPUMS. The primary geographic unit we use is the Census PUMA, which is the lowest level of geography identified for all respondents in the ACS. Every

PUMA contains at least 100,000 people and is drawn according to census tract and county boundaries. Densely populated counties are often comprised of multiple PUMAs. The boundaries of these denser PUMAs are derived from underlying census tract boundaries. Less densely populated counties with populations less than 100,000 will typically combine to form a single PUMA. PUMAs are re-drawn with each decennial Census, meaning that individuals from a given PUMA in 2008 could live in a different PUMA in 2012. To account for this, IPUMS produced an algorithm to optimize aggregation of PUMAs into "consistent" PUMAs, which can be compared across time.[13] We use consistent PUMAs as the baseline geographic unit in our analysis. In 2012, there were 2,378 PUMAs and 1,078 consistent PUMAs. For brevity, we refer to IPUMS consistent PUMAs simply as PUMAs, unless stated otherwise.

The ACS also provides individual-level outcomes and eligibility markers. For outcomes, we focus on employment, labor force participation, unemployment, and log wage earnings.[14] We restrict the sample to those who are 18 and older and are non-institutionalized. Because surveys are given throughout the year, employment, labor force participation, and unemployment can all be interpreted as probabilities. To construct a measure of Internet Essentials eligibility, we combine information on reported family income and children to proxy for free/reduced-price lunch eligibility. We deem an individual eligible if reported family income as a percentage of the Federal Poverty Limit is less than or equal to 185 percent, and if the respondent has at least one child between the traditional K-12 schooling ages of 5 and 17.[15]

In Table 2, we present summary statistics for consistent PUMAs with greater and less than 50 percent Comcast coverage. In the left-hand panel, we provide summary statistics on demographics and labor outcomes as of 2011, the year prior to the nationwide launch of Internet Essentials. We also summarize broadband use as of 2013. High-Comcast PUMAs have slightly greater concentrations of African Americans and married individuals, and tend to be more educated and affluent. High-Comcast PUMAs also have higher rates of broadband use. The right-hand panel examines trends leading up to the launch of Internet Essentials by presenting differences between high- and low-Comcast PUMAs from 2009 through 2011. There are small differences in gender and racial composition trends; population in low-Comcast PUMAs also grew slightly faster and incomes fell at a slightly lower rate.

The ACS also collects data on broadband use as of 2013, although these data are not informative of

[13] From IPUMS: "To construct [consistent PUMAs], we applied an aggregation algorithm that groups together 2010 PUMAs iteratively until the total population mismatch between each set of 2010 PUMAs and its closest matching set of 2000 PUMAs falls below 1% for both the 2000 and 2010 populations."

[14] We calculate log wage income as ln(Wage Income + 1).

[15] IPUMS derives family income as a percentage of the Federal Poverty Limit via the POVERTY variable.

internet use prior to the launch of Internet Essentials in 2012. The survey questions ask whether individuals have an internet subscription, as well as whether the subscription is a broadband subscription. In Table A2, we also use the 2013-15 broadband data in the ACS to calculate summary statistics based on broadband use. First, approximately 45 percent of low-income Americans do not have broadband, compared to 27 percent in the total population. As expected, individuals without broadband are more likely to be black or hispanic, single, without children, and less affluent. They are also older, reflecting potential generational differences in propensities to purchase broadband.

4 Results

4.1 Graphical Evidence

Figure 4 provides a visual depiction of the variation captured by triple differences. The graph plots trends in employment differences between eligibles and low-income ineligibles without school-aged children. We divide the data into two series: one representing high-Comcast PUMAs (at least 50 percent coverage) and one representing low-Comcast PUMAs (less than 50 percent coverage). Note that the regression form of triple differences in Equation (1) leverages the full continuum of Comcast coverage rates between 0% and 100%, which has been discretized in this simplified figure. Employment is pre-residualized with respect to control variables in the vector X_{igt}. We also residualize employment with respect to PUMA-by-year fixed effects to remove the influence of potentially confounding regional trends.

In high-Comcast PUMAs (represented by the solid line), the employment gap between eligibles and ineligibles remains relatively stable prior to the launch of Internet Essentials at approximately 3.3 percentage points. The gap begins increasing in 2012 and reaches 5.8 percentage points by 2015. The difference pre- and post-Internet Essentials appears statistically significant. This series represents the variation that would be captured by differences-in-differences comparing the outcomes of eligibles and ineligibles in high-Comcast areas. However, the gap between eligibles and ineligibles may have increased for reasons independent of Internet Essentials. This motivates a third level of differencing, using individuals living in areas that were less likely to receive Internet Essentials.

The dotted line plots the eligible-ineligible gap in PUMAs with less than 50 percent coverage. The data reveal that there was a secular increase in low-Comcast PUMAs that would have been omitted by standard differences-in-differences, resulting in otherwise upward-biased estimates. However, the upward trend is less pronounced in low-Comcast PUMAs; the eligible-ineligible gap in low-Comcast

PUMAs was greater by 0.55 percentage point in 2009 but is surpassed by high-Comcast PUMAs by 2015. The triple differences estimator conceptually captures the net difference between high- and low-Comcast PUMAs after 2012.

The implied effect sizes are also consistent with the trajectory of the program's expansion. The graph plots subscriber counts for each year using estimates from Table 1. While the gap between high- and low-Comcast PUMAs does not noticeably respond within the first year of the program's launch, uptake was relatively low at first (8.2 percent). We show in Section 4.4 that the implied TOT employment effect, which accounts for take-up rates, remains fairly stable across each of the post-treatment years in the sample.

4.2 Main Results

In Table 3, we present intent-to-treat estimates of Internet Essentials availability on the probability of employment, probability of labor force participation, probability of unemployment, and log wage income. Each estimate represents the effect of PUMA-wide program availability for eligible low-income individuals. Panel A presents the baseline triple differences estimate from Equation (1) using the full sample of ineligibles as the control group. Panel B presents the same triple differences estimate after restricting the control group to non-eligibles with school-aged children. Panel C, our main specification, restricts the control group to non-eligibles that are low-income but do not have school-aged children. The baseline treatment group mean at the top of the table provides the average of the dependent variable for the treatment group in 2011, one year prior to the launch of Internet Essentials.[16] We also provide control group-specific means of the dependent variable in each panel.

We find that PUMA-wide availability of Internet Essentials increased employment rates of eligibles by 0.9 percentage point, off a baseline of 56.7 percent (1.6%). The point estimate is similar across all three versions of the control group and is consistently significant at the 1 percent level. The increase in employment rates is accompanied by positive but small and non-significant effects on labor force participation ranging from 0.3 to 0.4 percentage point (SE: 0.3 percentage point). Despite the small and imprecise coefficients, these estimates mask a great deal of variation in effect sizes that grew over time as the program continued to expand, which we show in Section 4.4. Next, we find that the probability of being unemployed decreases by 0.5 to 0.6 percentage points. These estimates are significant at the 5 percent level when the control group consists of all ineligibles or ineligibles with children; the esti-

[16] In the calculation of treatment means, we account for whether how much experimental exposure an individual in the treatment group receives by multiplying each individual's ACS person-level weight by the percent of their PUMA covered by Comcast.

mates are only significant at the 10 percent level when when restricting the control group to ineligibles with low income. Finally, income increases by approximately 0.06 to 0.075 log points, with significance levels mirroring those of the unemployment estimates. The effect on income is potentially driven by both an intensive channel (higher earnings for those already working) and an extensive channel (more likely to earn non-zero income). We show in Section 5 that the extensive channel is more likely to be the driving force between the two.

We calculate the treatment-on-the-treated effect of enrollment by dividing the ITT estimates by the average take-up rate of Internet Essentials from 2012 to 2015 (20.3%). For employment, which we most precisely estimate, the TOT effect size is approximately 4.4 percentage points (0.9/0.203). This implies that Internet Essentials increased the probability of employment by 7.8% for eligibles who enrolled. To contextualize this effect size, Hjort and Poulsen (2019) find that the arrival of fast internet in Africa increased the probability of employment by up to 13.2%. Using the ITT estimates from Panel C, the TOT estimates for unemployment and wage income are -2.5 percentage points (-17%) and 0.30 log points (5.3%).

The estimates appear to be reasonably sized. First, job seekers in Table A1 who do not have broadband are 14 percentage points (21%) less likely to use online search, despite the fact that nearly half of job seekers who use online search find it to be the most effective resource for their search. This is compounded by the fact that as many as 90 percent of Internet Essentials customers were reportedly first-time broadband users. Second, according to surveys from Internet Essentials customers, 62 percent indicated that the program had helped them or someone in their family find employment (Comcast Corporation, 2018). These observations, while ultimately anecdotal, lend additional support for our main findings.

The TOT estimates on earnings can also be used for a rudimentary cost-benefit analysis. The average baseline income for low-income eligibles nationwide is $10,000.[17] The TOT earnings estimate implies that Internet Essentials increases the average enrollee's wage income by approximately $530. With an average of two working-age adults in each eligible household,[18] the average benefit to a household is $1,060. Assuming a monthly subsidy of $25 ($300 annually), forgone one-time fees of $100, and an additional 25 percent added to costs to account for program administration, the $1,060

[17] This is the average among all eligibles in the US, weighing by ACS person level weights. The average is stable over time, only fluctuating within several hundred dollars for any given year.

[18] We approximate the number of adults in each household using the ACS by taking each eligible respondent's family size and subtracting the number of own children living in the household. Many families have working-age children living in the household, so this calculation is an underestimate of the actual number of working-age adults per household.

household benefit doubles the approximate $500 program cost per household.

4.3 Placebo Test: Non-Comcast ISPs

One concern is that Comcast broadband coverage may be correlated with other determinants of economic development. The availability of high-speed broadband infrastructure, irrespective of the provider, may also be conducive towards attracting businesses and promoting economic activity. These factors could produce a causal link between post-2012 Comcast coverage rates and labor market benefits for eligibles that are unrelated to Internet Essentials.

These confounding properties are not unique to Comcast and are likely shared by ISPs that are comparable in scale. However, Internet Essentials *was* the only mainstream low-income broadband discount program implemented at scale nationwide from 2012 to 2015.[19] We use this fact to construct a falsification test of our employment results by estimating whether local exposure to other large ISPs yields significant employment effects. We selected the three largest non-Comcast ISPs by subscriber count as of 2018: Charter/Time Warner Cable (24.6 million), AT&T (15.8 million), and Verizon (7.0 million).[20] Because none of these ISPs widely subsidized broadband for low-income families during the sample time frame, we would not expect exposure to these ISPs to be associated with greater post-2012 employment outcomes for eligibles relative to low-income ineligibles without school-aged children. Figure A2 provides a map indicating which of the four largest ISPs captures the largest share of the population in any given PUMA. Despite Comcast's wide national coverage in Figure 2, we see that it competes heavily with Verizon on the east coast, and with Charter and AT&T elsewhere in the United States.

We begin with the triple differences specification from Equation (1), again using the control group of low-income ineligibles. For the placebo test, we replace $Comcast_g$ with coverage rates of either Charter, AT&T, or Verizon. For example, the Verizon-based placebo test for labor outcomes would be:

$$\begin{aligned} y_{igt} = \alpha &+ \rho(Eligible_{igt} \times Verizon_g \times Post_t) + \delta_1(Eligible_{igt} \times \lambda_t) \\ &+ \delta_2(Eligible_{igt} \times \gamma_g) + \delta_3(\gamma_g \times \lambda_t) + X'_{igt}\beta + \epsilon_{igt} \end{aligned} \tag{3}$$

Panel A in Table 4 presents the placebo test outcomes for employment. As we see, only the coefficient on Comcast is significant. When Comcast exposure is substituted for exposure to Charter, AT&T, or

[19] Other programs were provided through federal and state governments, but these programs were generally limited to specific neighborhoods or small subsets of under-served populations. For example, the ConnectHome program, piloted in 2015 and administered by the U.S. Department of Housing and Urban Development, provided broadband to approximately 20,000 individuals living in HUD-assisted households.

[20] Statistics are reported from using data from Leichtman Research Group (2018); see Table A3.

Verizon, there is no effect on employment. Since the availability of certain ISPs could be correlated with one another, we provide results from a horserace regression in Column (5), which includes the triple interaction terms for all four ISPs in the same regression.[21] Still, effects are only significant for Comcast. Our results lend some assurance that our estimates are in fact driven by Internet Essentials, as opposed to characteristics of robust broadband markets.

4.4 Robustness Checks

The identifying assumption of the triple differences regression in Equation (1) is that absent Internet Essentials, $Comcast_g$ should not be correlated with within-PuMA employment differences between eligibles and ineligibles, conditional on covariates X_{igt}. This is the triple differences analogue of the differences-in-differences parallel trends assumption. We begin this section with two tests of the identifying assumption. We then analyze the sensitivity of our results to alternate specifications and data assumptions.

More restrictive control groups: We begin by testing how the results change after refining the control group of ineligibles to more closely resemble the treated group of eligibles. This reduces the number of channels through which the gap between eligibles and ineligibles could diverge in high-Comcast PUMAs for reasons unrelated to Internet Essentials. However, the more restrictive the control group, the more imprecise the data-intensive triple differences estimates will become.

In our main specification, we compared eligibles against low-income ineligibles without school-aged children. We can also compare eligibles against low-income *parents* whose children are either too old or too young for K-12 schooling.[22] This reduces the overall sample size by approximately 60 percent. Panel B of Table 5 shows that the effect on employment increases ($\beta : 0.014, SE : 0.005$), driven by a greater and marginally significant effect on labor force participation ($\beta : 0.009, SE : 0.005$). The point estimate on unemployment remains unchanged, although standard errors increase enough to eliminate statistical significance ($\beta : -0.005, SE : 0.003$). Effects on wage income are qualitatively similar to our baseline estimates and remain significant at the 10 percent level ($\beta : 0.073, SE : 0.043$).

Next, we further restrict the control group by including only parents whose children are too young to attend school. Violations of the identifying assumption must now arise from differential trends between low-income parents with children in K-12 versus low-income parents whose oldest child is too young for kindergarten. We first observe that the treatment and control means are

[21] Table A4 provides a correlation table between PUMA-level coverage rates of the four largest ISPs.

[22] The ACS only collects data on children living in the same household as the primary respondent.

quite similar between these two groups. The sample size is also nearly 80 percent smaller than in the baseline estimates. Still, the effects on employment ($\beta : 0.011, SE : 0.007$), labor force participation ($\beta : 0.014, SE : 0.007$), and wage income ($\beta : 0.106, SE : 0.061$) in Panel C are positive and significant at the 10 percent level or better. The effect on unemployment changes signs, but is small and remains highly insignificant as standard errors continue increasing ($\beta : 0.003, SE : 0.005$).

Finally, we restrict both eligibles and ineligibles to a sample of parents with children whose ages vary within a specific bandwidth about the age threshold. Specifically, we compare eligible parents whose oldest child is elementary-aged (ages 5-11) versus ineligible parents whose oldest child is roughly pre-kindergarten aged (ages 2-4). The group means are nearly identical by this point, and the sample size has been reduced by nearly 90 percent in total. The estimates on employment ($\beta : 0.015, SE : 0.009$) and labor force participation ($\beta : 0.018, SE : 0.008$) remain significant at the 10 and 5 percent levels, despite the fact that the cumulative sample size reductions continue to increase the standard errors. Interestingly, the large effect on labor force participation drives the employment effect for this particular subsample.

In total, we find that the effect on employment probabilities remain consistently positive and significant throughout. The point estimates are also fairly stable across all four specifications. The effect on labor force participation, which was otherwise masked in our main specification, comes out clearly with each additional restriction. This implies that labor force participation among ineligible low-income *non-parents* increased substantially during this time, which may have biased our main estimates labor force participation estimates downwards.

Analyzing pre-trends and effect size trajectories: Although we cannot observe counterfactual trends in employment differences, we can observe whether trends appear to diverge in pre-treatment years. Differential trends in pre-treatment years are a warning sign that parallel trend violations may be occurring in the counterfactual post-treatment years. In Table 2, we provided evidence that populations in high- versus low-Comcast PUMAs did not display substantial pre-trends leading up to the launch of Internet Essentials. Figure 4 also provides some limited assurance that pre-trends do not appear to be substantial.

One additional way to probe for potential violations of this assumption is with an event study formulation of the triple differences regression in Equation (1). To do so, we replace $Eligible_{igt} \times Comcast_g \times Post_t$ in Equation (1) with separate interaction terms constructed by multiplying $Comcast_g \times Eligible_{igt}$ with dummies for each year in our sample (Angrist and Pischke, 2008). This allows the effect of $Comcast_g \times Eligible_{igt}$ to vary separately for each year. The interaction term on the final pre-treatment year, 2011, is the omitted period. The event study design provides two benefits: first, the coefficients on pre-treatment years provide a falsification test for parallel trends, as significant trends in pre-treatment years may raise concerns about parallel trends holding in the counterfactual. Second, we can use the event study formulation to observe how the treatment effect of Internet Essentials evolves over time. Adoption of Internet Essentials grew over time as marketing for the program developed and the customer base expanded, and we expect to observe increasing treatment effect sizes over time.

In Table 6, we first confirm that none of the effect sizes in pre-treatment years are statistically significant. We also observe that the point estimates in post-treatment years generally increase over time. The employment effect in 2012 appears to be quite small (β: 0.005), becomes large and statistically significant by 2014 ($\beta : 0.015$), and continues to grow through 2015 ($\beta : 0.018$). When combining each annual ITT estimate with its corresponding take-up rate in Table 1, we find that the implied TOT effects for each year are quite stable (6.1, 6.4, 6.4, and 5.3 percentage points).[23] The upward trajectory of the treatment effect coinciding with the launch and subsequent expansion of the program provides additional assurance that our estimated effects are tied to variation in Internet Essentials.

We find that the effects on labor force participation evolve in a similar way, rising to a statistically significant effect size of 0.15 percentage point by 2014. One additional reason why we do not observe a significant effect on labor force participation in Table 3 is that the large effects in later years are concealed by the smaller effects in the first two post-treatment years. Though the point estimates on most post-treatment years for unemployment are negative, the standard errors are large enough that we do not find a significant effect in any given year. This may be due to the fact that unemployment is a comparatively rare event, and allowing the effect to vary non-parametrically across time is highly demanding on the data. Finally, we confirm that the effect on wage income is positive in all post-treatment years and significant in 2014 and 2015.

Alternative Income Limits: Another potential concern is that families who find employment through Internet Essentials will earn more than the 185% FPL income limit and become ineligible for the program after their 12-month enrollment period ends.[24] A related concern is that our measure of poverty

[23]These implied TOT effects are slightly larger than our baseline TOT estimate of 4.4 percentage points. This could be due to the fact that there appears to have been an idiosyncratic dip in the baseline year of 2011; note that the point estimates for 2009 and 2010 are both positive relative to 2011 as well. It is possible that the event study effect sizes in post-treatment years are mechanically inflated due to the idiosyncratic dip in 2011.

[24]Under 2019 guidelines, 185 percent of the federal poverty level for a family of three corresponds to an annual income

is mismeasured, and that the ACS poverty variable does not precisely identify respondents whose children are eligible for the NSLP. To address this, we vary the required income eligibility threshold across 130%, 185%, 250%, and 300% FPL. We present these results for employment in Panel A of Table 7 using the low-income ineligible control group. The effect size generally decreases as the income eligibility threshold increases. At 150% FPL, the effect size is 0.10 percentage point, which decreases to 0.03 percentage point by the 300% FPL threshold (and is not significant).

Alternative Geographies: Another concern is that PUMAs can vary widely in terms of geographic area, despite the fact that each is drawn to ensure that population sizes are at least 100,000. IPUMS provides indicators for counties and metro areas that can be derived from PUMAs. Counties are only identified in the ACS if a county is coterminous with one or more PUMAs. Metro areas are determined based on the metro area in which the majority of each PUMA's population resided. Therefore, counties and metro areas that can be identified in the ACS are a selected sample that tend to be more densely populated than counties and metros that cannot identified in the ACS. We present employment results for these alternative geographic aggregations in Panel B of Table 7, using low-income ineligibles as a control group. All regressions are conducted after recalculating coverage rates via Equation (2) and re-running Equation (1) at the appropriate level of geographic aggregation. The point estimates we observe for counties are still large, but are more imprecisely estimated (β : 0.010, SE : 0.007). We observe similar results when aggregating to the metro level. We also run the analysis using states as the unit of geographic aggregation, and similarly find that the point estimates remain large but imprecise.

PUMA-Level Differences-in-Differences: Although our data are at the individual level, our measure of Comcast coverage is calculated at the PUMA level. One way to verify our results from triple differences is to aggregate all outcomes and covariates to the PUMA level and run a standard differences-in-differences regression. The regression is estimated as follows:

$$y_{gt} = \alpha + \rho(Comcast_g \times Post_t) + X'_{gt}\beta + \lambda_t + \gamma_g + \epsilon_{gt} \tag{4}$$

where X_{gt} is the same vector of individual covariates as in Equation (1), but aggregated at the PUMA level. Because treatment only affects eligibles, all aggregation to the PUMA level occurs using only

of $39,460.50.

the sample of eligibles within each PUMA.[25] We provide the results of this specification in Column (1) of Panel C in Table 7, weighing each PUMA by its population of eligibles. Our results are robust to this specification and remain significant ($\beta : 0.010, SE : 0.003$).

Time-varying Comcast coverage: In Equation (1), we fixed $Comcast_g$ to coverage rates as of 2012. This mitigated the possibility that changes in Comcast coverage rates over time are endogenous to local labor market conditions. We alluded to the fact that strict regulatory conditions make it challenging for large broadband network expansions to occur independent of mergers and acquisitions. Therefore, we expect year-over-year changes in Comcast coverage rates to be small. In Appendix Table $A5$, we show how the distribution of Comcast coverage rates change year to year from 2012 through 2015. 90 percent of PUMAs experienced changes of less than one percent, and the median change in each year is zero. In Column (2) of Panel C in Table 7, we find that the effect remains positive and significant when allowing Comcast coverage rates to vary across years ($\beta : 0.008, SE : 0.003$).

Discretizing Comcast availability: The interpretation of ρ in Equation (1) is the effect of PUMA-wide Internet Essentials availability. We noted in Figure 3 that many PUMAs only have partial Comcast coverage. We can test whether our main result holds when restricting the sample to obervations living in PUMAs at the upper and lower ends of the Comcast coverage distribution and discretizing $Comcast_g$ into a binary indicator for very high versus very low Comcast coverage. We assign this indicator to PUMAs with greater than 90 percent coverage and with 0 percent coverage. Note that this restriction effectively implies that $Comcast_g \approx Comcast_{ig}$, or that PUMA-level availability is equivalent to household-level availability. This is because the restricted sample either lives in a PUMA with no Comcast coverage or near-universal coverage; ρ can then be interpreted as the effect of *individual*-level availability. We show this result for employment in Column (3) of Panel C of Table 7, again using low-income ineligibles as the control group. The effect remains positive and significant ($\beta : 0.013, SE : 0.004$).

Including 2016 and 2017 data: We excluded post-2015 data due to the rise of other large-scale broadband subsidy programs, as well as the expanded eligibility to individuals on other forms of public assistance. Inclusion of these data will bias our results downward if individuals in low-Comcast PUMAs and in the control group are able to enroll in other broadband subsidy programs (or Internet Essentials itself). Column (4) of Panel C in Table 7 shows that the point estimate remains unchanged and

[25] Averages are weighted using ACS person-level weights.

is significant at the 1 percent level ($\beta : 0.009, SE : 0.003$). We note that inclusion of later years also coincides with the continual, rapid increase in smartphone usage. While this trend could theoretically mitigate the impact of more affordable broadband, we still document large and significant effects.

Control-Driven Effects: Conceptually, the DDD estimator is the difference in DD estimates, run separately for the treatment group (eligibles) and control group (ineligibles). This raises the possibility that the estimated effects were not driven by positive changes in the treatment group, but rather negative changes in the control group. In Table A6, we present differences-in-differences estimates run on four separate samples: once on the treatment group and once for each of the three control group formulations used in our main analysis. Column (1) is standard differences-in-differences run on the group of eligibles. We find large and positive effects on employment (β: 0.013, SE: 0.003). In columns (2) through (4), Comcast availability leads to positive but comparatively small effects on employment, with coefficients ranging from 0.003 to 0.006. This supports our decision to use triple differences, given that there were underlying secular employment increases for ineligibles living in high Comcast PUMAs that differences-in-differences would not have accounted for. We also verify that our triple differences estimates were driven by positive effects in the treatment group, as opposed to negative effects in the control group. Finally, these results imply that there was not a major redistribution of jobs from ineligibles to eligibles, which would have manifested in negative differences-in-differences estimates in the control group. This suggests that general equilibrium effects of providing affordable broadband may be small, at least in the short run.

5 Mechanisms

5.1 Effects on Internet Use and the Broadband Gap

We also assess the effect of Internet Essentials on internet adoption. We use a variation of our main analysis to empirically estimate the effects of Internet Essentials availability on internet use. Because internet use data do not exist for the ACS prior to 2013, we turn to the Current Population Survey (CPS) (Flood et al., 2017). The CPS includes a supplement on computer and internet use in certain years; since 2003, internet use data have been collected as a part of the Educational Supplement in 2007, 2009, 2010, and 2012, and were additionally collected as a standalone Computer and Internet Use Supplement in 2011, 2013, and 2015. However, these data have several important limitations. First, questions and sample universes are not the same from year to year. For example, the sample in 2010 is composed of households where respondents used computers, whereas other years do not have this restriction. The inclusion of year fixed effects will only partially mitigate this issue. We provide the exact survey questions and sample universes in each year in Appendix B. Additionally, data on family income are not pre-transformed to reflect poverty status and are instead binned into 16 different categories. For example, households making $32,000 would be labeled as earning between $30,000 and $34,999. We use the upper bound of each interval, which in principle underestimates the number of people who would be eligible for Internet Essentials. We then map family income and family size to U.S. federal poverty tables in order to determine if a family with school-aged children is income-eligible for reduced price lunch.[26]

The sample is also underpowered relative to the ACS. Sample sizes are small, consisting of approximately 130,000 individuals per survey year. Geographically, the CPS does not identify PUMAs. Individual counties are also sparsely identified; in our sample, only 42 percent of respondents live in identifiable counties. We can instead rely on metro areas as the geographic unit of aggregation (identified in 73 percent of the sample) or states (identified for 100 percent of the sample). We use the seven surveys given during the years 2007-2015. The sample size of eligibles with identified metro areas is 21,232 and is 37,976 for states. When relaxing the income eligibility threshold to 250 percent of the federal poverty limit (to account for measurement error in determining poverty status), the sample grows to 29,613 respondents with identified metros and 53,245 for states.

Given sample size and measurement error concerns inherent in the data, we rely on basic differences-in-differences to estimate the effect of Internet Essentials on internet use. We begin by restricting the sample to the treatment group of eligibles to ensure that the estimate is not diluted by respondents who cannot enroll in Internet Essentials. The regression we run is as follows:

$$HasInternet_{igt} = \alpha + \rho(Comcast_g \times Post_t) + \lambda_t + \gamma_g + X'_{igt}\beta + Z'_{gt}\delta + \epsilon_{igt} \quad (5)$$

where $HasInternet_{igt}$ is equal to one if the respondent indicates that he or she accesses the internet from home. The vector of individual-level covariates X_{igt} includes gender, age, age squared, race, marital status, and number of children. We also include a vector of metro/state-level covariates in Z_{gt}, including population and unemployment rates. The identifying assumption in this regression is the standard difference-in-differences parallel trends assumption: in the absence of Internet Essentials,

[26]Poverty tables were obtained from the U.S. Department of Health & Human Services via the following URL: https://aspe.hhs.gov/prior-hhs-poverty-guidelines-and-federal-register-references.

internet usage trends among eligibles would have evolved equally in high-Comcast versus low Comcast CBSAs, conditional on covariates X_{igt} and Z_{gt}. Inclusion of year fixed effects will only partially mitigate the issue of inconsistency in survey methodologies across years.

In Table A7, we estimate the effects of geography-wide exposure to Internet Essentials on whether internet was used at home. At the metro level, metro-wide access to Internet Essentials led to a 4.9 percentage point increase in internet use. When using the less restrictive income threshold, this estimate falls to 3.9 percentage points. Both are significant at the 5 percent level. At the state level, state-wide access to Internet Essentials led to an 8 percentage point increase in internet use, which falls to 6.8 percentage points using the less restrictive income threshold. Both state-level estimates are significant at the 1 percent level. We also re-run the placebo test from Equation (3) using the differences-in-differences regression in Equation (5) with state-level Comcast coverage rates. Panel B of Table 4 verifies that the increase in internet use is only significant when using Comcast exposure.

How substantial are these effects within the broader picture of the digital divide? We use the ACS to regress broadband adoption rates on an indicator for individuals with family income less than 185% of the federal poverty limit. We find that such individuals are 20.6 percentage points less likely to have in-home broadband than their non-poor counterparts.[27] Our estimates indicate that state-wide availability of Internet Essentials induced broadband take-up rates to increase by up to 8 percentage points, which would narrow the digital divide by 39 percent in locations where it was available.

We note that survey results from a recent Internet Essentials progress report suggest that 90 percent of Internet Essentials customers are first-time broadband subscribers Comcast Corporation (2018). While this estimate is not causal (some first-time households may have chosen to adopt broadband regardless of Internet Essentials availability), it suggests that the program's impact on the income-broadband gap may be even greater than our estimates imply. A working paper by Rosston and Wallsten (2019) specifically documenting the effects of Internet Essentials on broadband adoption also finds that 66% of Internet Essentials customers were induced by the program to purchase a broadband subscription. These two data points suggest that our CPS estimates, though substantial, may in fact represent an underestimate of the true impact of Internet Essentials on low-income broadband adoption.

[27] We also condition on year and PUMA fixed effects, and weigh the regression by ACS person-level weights.

5.2 Geographic and Demographic Heterogeneity

Urban and Non-Urban Geographies: The employment effects of subsidized broadband may exhibit spatial differences based on the degree of local urbanization. Marketplaces for online job postings and job seekers may be less "thick" in less urbanized areas, and lack of physical broadband infrastructure in non-urban areas (Ziliak, 2019) could further diminish the employment effects of subsidized broadband. At the same time, broadband subsidies could potentially have a larger effect on broadband adoption rates if broadband use is at a lower baseline in non-urban areas. Employment effects may also differ based on the availability of job openings in urban and non-urban areas.

There are many ways to classify whether a PUMA is urban. Census blocks are typically classified as urban if population density within the block exceeds 1,000 people per square mile (Ratcliffe et al., 2016). A census block that touches an urban block and has a population density over 500 people per square mile is considered part of an "urban cluster". One way to classify the urban status of a PUMA is to determine the percentage of the population living in an urban cluster. Another approach is to simply calculate whether the population density across an entire PUMA exceeds 1,000 people per square mile. This second approach allows large, sparsely-populated blocks to be more influential. In practice, we find that the two methods yield equivalent classifications in 90 percent of cases.

In Table A8, we present triple difference employment effects estimated separately for urban and non-urban PUMAs. We use three different ways of classifying urban PUMAs: 1) at least 95 percent of the population lives in an urban cluster, 2) at least 99 percent of the population lives in an urban cluster, and 3) PUMA population density exceeds 1,000 people per square mile. The control group includes only low-income ineligibles. Employment effects appear significantly greater in urban PUMAs. While the majority of the other comparisons are not significant, we observe that across almost every outcome and urban classification, the point estimates on urban PUMAs are larger in magnitude than their rural counterparts. The overall narrative suggests that the labor market effects of Internet Essentials are generally larger in urban versus non-urban PUMAs. However, we caveat that the analysis is ultimately unable to disentangle whether the heterogeneity is due to differences in employment elasticities or differences in take-up rates.

Differences by Demographic Characteristics: In Table A9, we present triple difference results estimated separately by gender, education (high school degree or less versus more than high school degree), and age (older or younger than 38, the median age for eligibles). We calculate differences between each pair of groups and determine whether the null hypothesis of no difference can be rejected.[28] We do not find statistically significant differences across any of the three demographic categories. The strongest case for a difference is that the beneficial effects on unemployment may be greater for men than for women. Still, this difference is not statistically significant, even at the 10 percent level ($p = 0.14$).

5.3 Job Characteristics

Household broadband access may potentially improve average job quality by 1) increasing the choice set of jobs to apply to, 2) improving match quality between job seekers and firms, or 3) increasing skill accumulation in online- and computer-based tasks. At the same time, decreasing barriers to work may lead to offsetting selection effects from individuals with lower average labor productivity entering the labor market. In equilibrium, high-quality job vacancies are also easier to fill and have low turnover rates, implying that the typical unfilled vacancy may be of lower average quality.

We use the ACS data to analyze the effects of Internet Essentials on three basic job characteristics: part-time status, wage income, and transit time. In Table A10, we show the effects of Internet Essentials availability on these three outcomes conditional on being employed. We run the estimates separately using the three control groups from Table 3. We do not find convincing evidence that Internet Essentials led to changes in any of these three outcomes.

These findings stand slightly in contrast to recent findings from Bhuller, Kostol and Vigtel (2019), who argue that broadband expansion in Norway increased commuting distances and starting wages. The wage effects in Norway were driven by high-wage individuals locating high-paying firms, which likely explains the discrepancy in wage outcomes between the two settings. The commuting time discrepancy is more difficult to reconcile, given that broadband access should theoretically improve access to information about job openings. We provide two potential explanations. First, we argue that individuals induced by Internet Essentials to join the labor force are more likely to favor jobs that are close by. Because these individuals face comparatively greater opportunity costs with respect to job search and work, they are less likely to accept jobs that are further away. Second, broadband adoption in Norway increased by 25 to 30 percent in both households *and firms*. Firms located in densely populated areas may have had less difficulty filling vacancies prior to the arrival of broadband internet, and the arrival of broadband would have benefited these firms primarily through job match quality. On the other hand, firms located further away may have benefited more from improved match

[28] Explicitly, we run a variation of triple differences where every single independent variable, including fixed effects, are interacted with an indicator for male/HS graduate/older. The difference (and corresponding p-value) is captured in the four-way interaction term between the DDD triple interaction and the indicator.

frequencies. The setting we study in this paper abstracts from firm broadband adoption, removing a potential mechanism that could favor greater commute times.

5.4 Online Job Search Behavior

Stevenson (2009) describes three ways in which internet access enables more productive job search. First, the internet has decreased the cost of applying for jobs. Online job boards such as indeed.com and monster.com connect job seekers to openings through an online marketplace, eliminating the need to apply for jobs in person. The same platforms enable users to quickly filter through job postings to find positions that match their skills and preferences. Second, internet access provides information and resources that can make job seekers more knowledgable and prepared during the job search process. Websites such as glassdoor.com and vault.com provide job seekers with deeper insights about firms and jobs, including information on wages, job satisfaction, and even interview questions. Third, internet access expedites communication between prospective employers and applicants. Many employers will contact prospective applicants through email, if not directly through online job boards.

We attempt to attribute changes in online job search behavior to the launch of Internet Essentials. To do so, we require location-specific data about the online job search behavior of low-income job seekers over time. We choose to use Google Trends, which provides metro- and time-specific data on trends in Google searches. If Internet Essentials increased online job search activity among eligibles, then Google searches related to job search should differentially increase after 2012 in locations with greater Comcast coverage. We therefore compare searches in metros with high Comcast availability versus those in metros with low Comcast availability, before and after the launch of Internet Essentials.

The Google Trends data are not without limitations. First, we cannot easily disentangle online search behavior of eligibles from that of the general population because we cannot observe search behavior for specific population groups. Second, as we describe shortly, data from Google Trends are not easily interpretable. The data are transformed in a way that effectively rules out typical regression-based methods. Despite these limitations, the data are publicly available and broadly capture job search behavior that can be descriptively analyzed across geographies and over time.

We attempt to differentiate between the search behavior of eligibles versus other job seekers by interpreting patterns of Google searches for the online job board Snagajob.com.[29] Snagajob (recently re-branded as Snag) is one of the leading marketplaces for hourly and shift work, with over 90 million

[29]One might be concerned that individuals prefer navigating to the website directly via the URL search bar. However, based on a free search on website analytics platform SimilarWeb, we find that as of December 2018, 63 percent of individuals visited the Snagajob website via search engine.

registered hourly workers.[30] "Top jobs" listed on the website's home page include cashier, delivery driver, bartender, server, dishwasher, host/hostess, cook, merchandiser, assistant manager, and team member. "Top companies" on the home page include KFC, Arby's, Red Lobster, Chili's, KMart, Family Dollar, Denny's, Taco Bell, Michaels Arts and Crafts, Wendy's, and McDonald's.

We next discuss the Google Trends data. To access the data, users specify a list of up to five terms to compare, the desired geographic breakdown (national, state, metro, or city), and a range of dates, which Google uses to aggregate search volumes.[31] To make queries of its enormous database more manageable, Google takes a random sample of its database. This leads to slight amounts of variability across equivalent data requests (Choi and Varian, 2012). Queries for search terms that are uncommon within a given geographic area are also restricted in order to protect privacy. We requested a separate dataset aggregated at the metro level for each year from 2009 through 2015. We then link metros to their respective levels of Comcast exposure from the NTIA data.[32]

The Google Trends data do not contain absolute search volumes, but rather relative search indices, which are calculated from absolute search volumes via a two-step transformation which we describe in Appendix D. To summarize this discussion, the relative search indices are not comparable across metros or time due to shifting baseline search volumes. The literature has typically benchmarked Google search terms using trends of unrelated search terms (Nghiem et al., 2016; Ficetola, 2013). This comparison allows us to determine whether a change in relative search volume is driven by an absolute change in search interest, as opposed to a shift in baseline search volumes. Benchmarking search terms in this way produces an absolute index that can be compared across years and metros. As we show in Appendix D, benchmark terms should ideally have relatively stable absolute search volumes over time. Benchmark terms should also be within the same order of magnitude of search interest due to rounding issues when two terms of widely differing interest are compared. Nghiem et al. (2016), for example, use the word "computer" as a benchmark; however, the popularity of this search term dwarfs that of "Snagajob", making it unsuitable as a benchmark term.

In Figure A4, we present three graphs of Google searches for "Snagajob", referenced against three different search terms that satisfy the two criteria listed above.[33] Each graph in Figure A4 is composed

[30] See https://www.talentlyft.com/en/resources/what-is-snagajob for details.

[31] We used the `pytrends` library in Python to extract our data.

[32] According to Google, metros are "geographical areas that generally correspond to metropolitan areas". Upon inspection, the metros provided in Google Trends cannot directly be linked to official delineations of MSAs or CBSAs. To link the Google metro delineations (which are provided as strings) to metros in our NTIA data, we first used a combination of fuzzy string matching to filter out clear non-matches. We then manually inspected potential matches to complete the process.

[33] The three terms we selected were: "Nintendo 64", "Thomas Jefferson" and "Pulp Fiction". Appendix Figure A3 shows

of two series: one for high Comcast metros with at least 75 percent coverage, and the other for metros with no Comcast presence. Each point is a weighted average of our absolute search index, weighted by the number of eligibles residing in each metro (as calculated in the ACS).

In all three of the graphs, Snagajob became more widely searched in high-Comcast metros after the launch of Internet Essentials in 2012. We emphasize that this finding is purely descriptive, but supports the notion that online job search served as a potentially important channel through which Internet Essentials increased employment.

6 Conclusion

The majority of job seekers today use the internet to find and apply for jobs. Those who lack the means to afford an ongoing broadband subscription are less likely to use online resources for job search, and may be less likely to overcome other barriers to labor force participation and work. We investigated how Internet Essentials, a program which has provided discounted broadband access to over six million Americans since its launch in 2012, affected labor outcomes among eligible low-income individuals. Our results indicate that PUMA-wide availability of Internet Essesntials led to a 0.9 percentage point increase (1.6%) in the probability that an eligible low-income individual was employed. After adjusting for take-up rates, we find that enrollees were 4.4 percentage points (7.8%) more likely to be employed. The effects are driven both by increases in labor force participation and decreases in the probability of unemployment. Our findings also suggest that Internet Essentials was responsible for narrowing the income-broadband gap by at least 40 percent. A back-of-the-envelope cost-benefit calculation suggests that the value to consumers (in terms of increased earnings) is double that of the cost to provide the service. The program's cost-effectiveness suggests additional scope for private and public expansions of broadband subsidies for low-income households.

High-speed internet continues to become increasingly centralizing force in modern society. Those who cannot afford monthly broadband subscriptions are inherently handicapped when navigating the labor market in the digital era. They also risk falling behind in ways that extend beyond the labor market. The internet plays a central role in education, access to goods and services, communication, and more. These additional considerations, despite being beyond the scope of this paper, only further compound the need for policy solutions to bridge the income-broadband gap.

that these three terms are likely to have low absolute search growth over time, and search volumes comparable to that of the search term "Snagajob". Although there is seasonal variation for each term, we see decreasing trends in the Google relative search index. This is to be expected if absolute search volumes for these terms stay constant over time, as the baseline total search volume in the US has increased over time.

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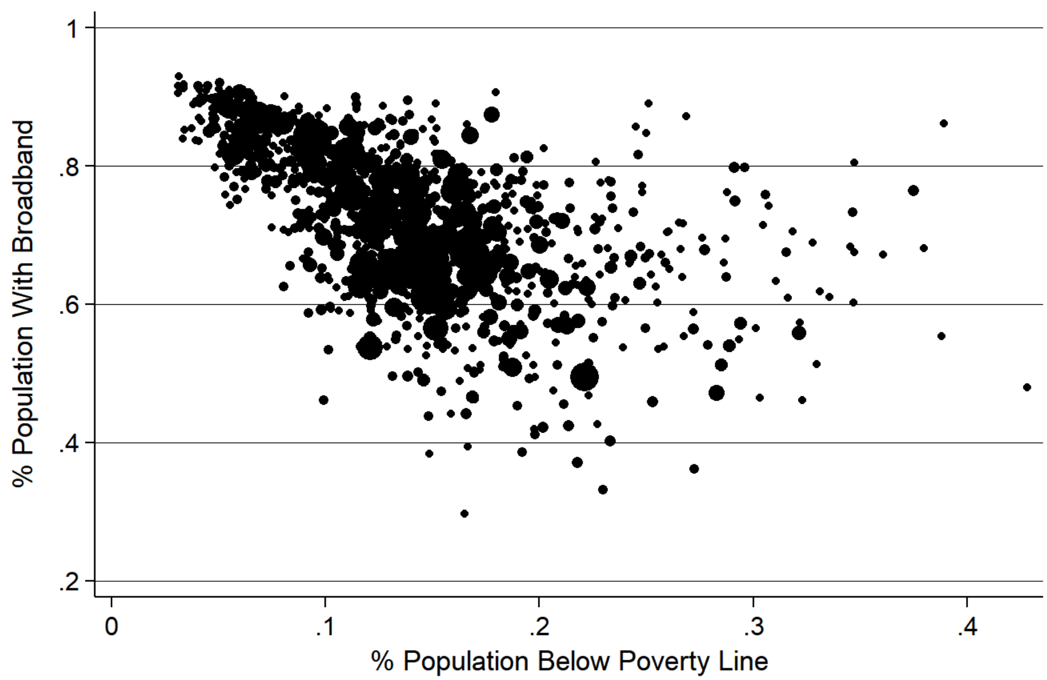
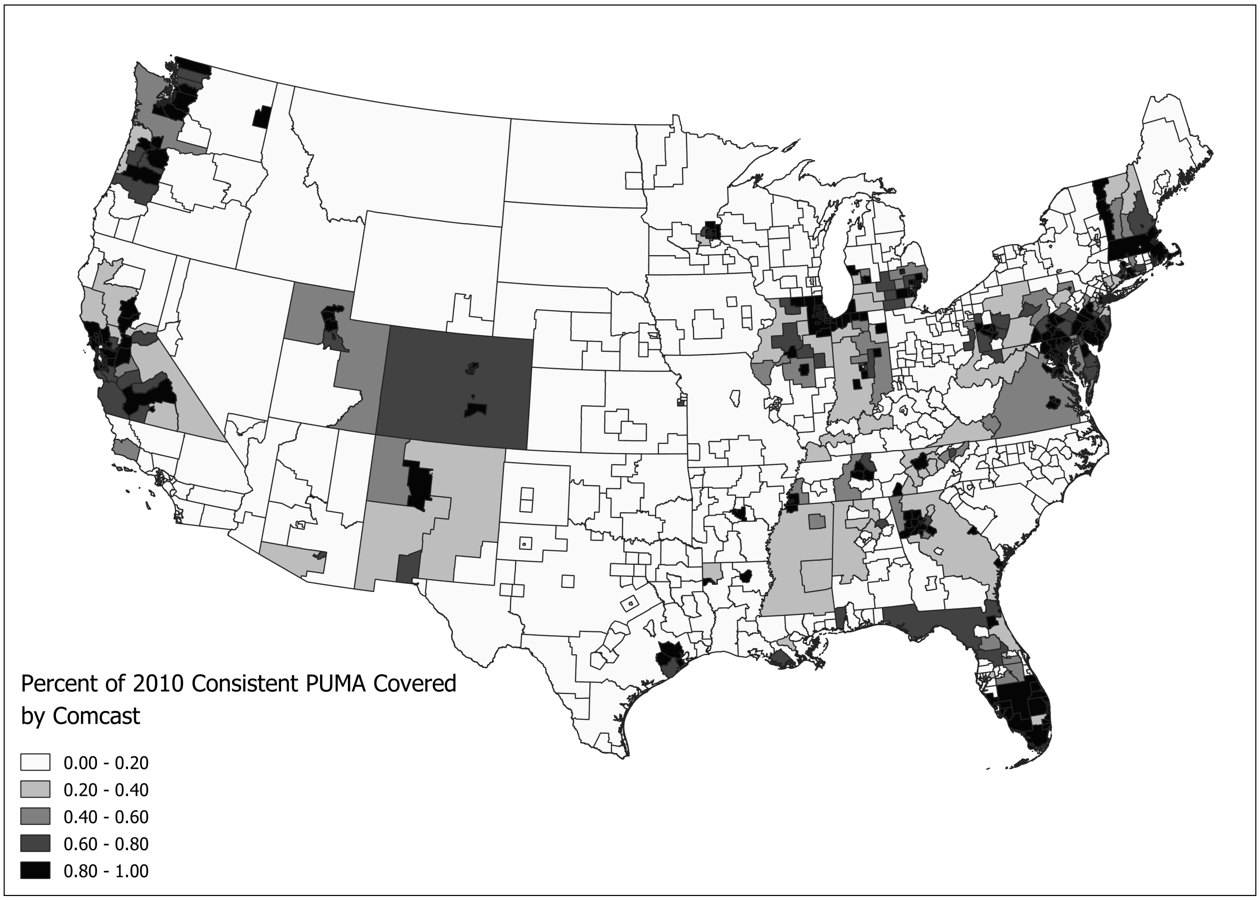
Figure 1: PUMA-Level Poverty and Broadband Adoption Rates



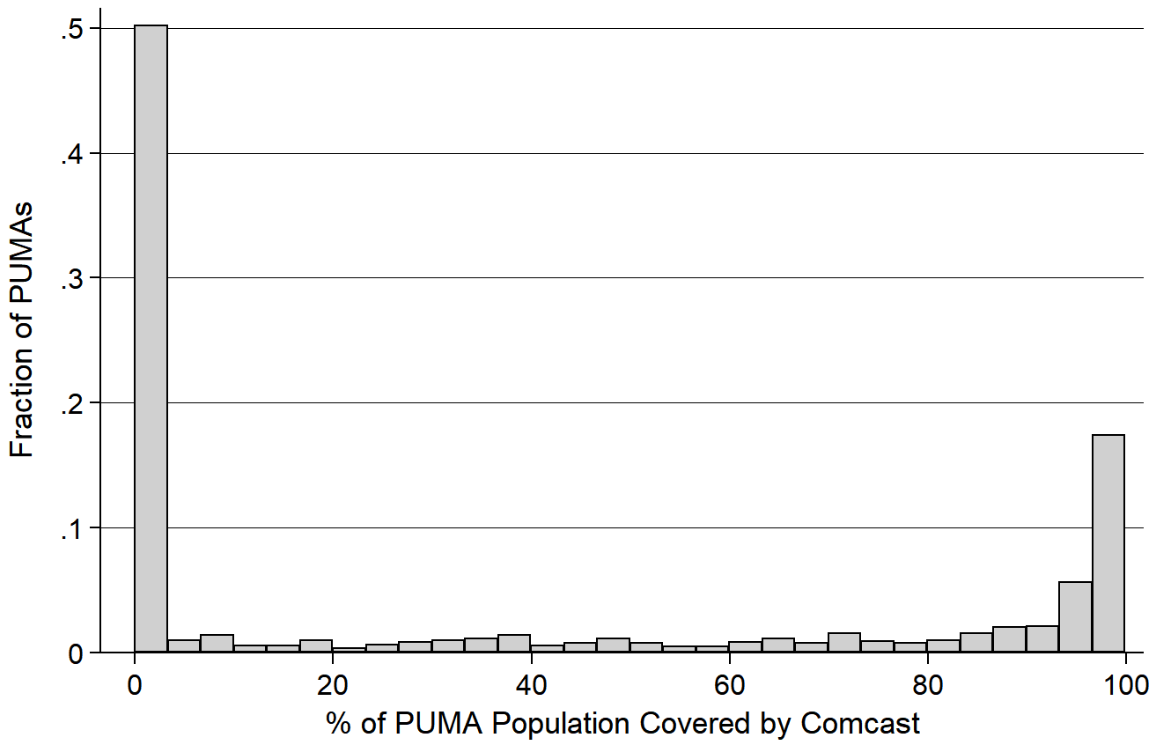
Note: This figure shows the relationship between the percentage of a PUMA's population in poverty and the percentage of the population with home broadband access. Each point on the graph is a separate PUMA in the ACS, and the size of the marker reflects the population of the PUMA. Data are compiled from the 2013-16 ACS One-Year Estimates. Observations are weighted by the sum of person level weights in each PUMA.

Figure 2: National Comcast Coverage, by PUMA



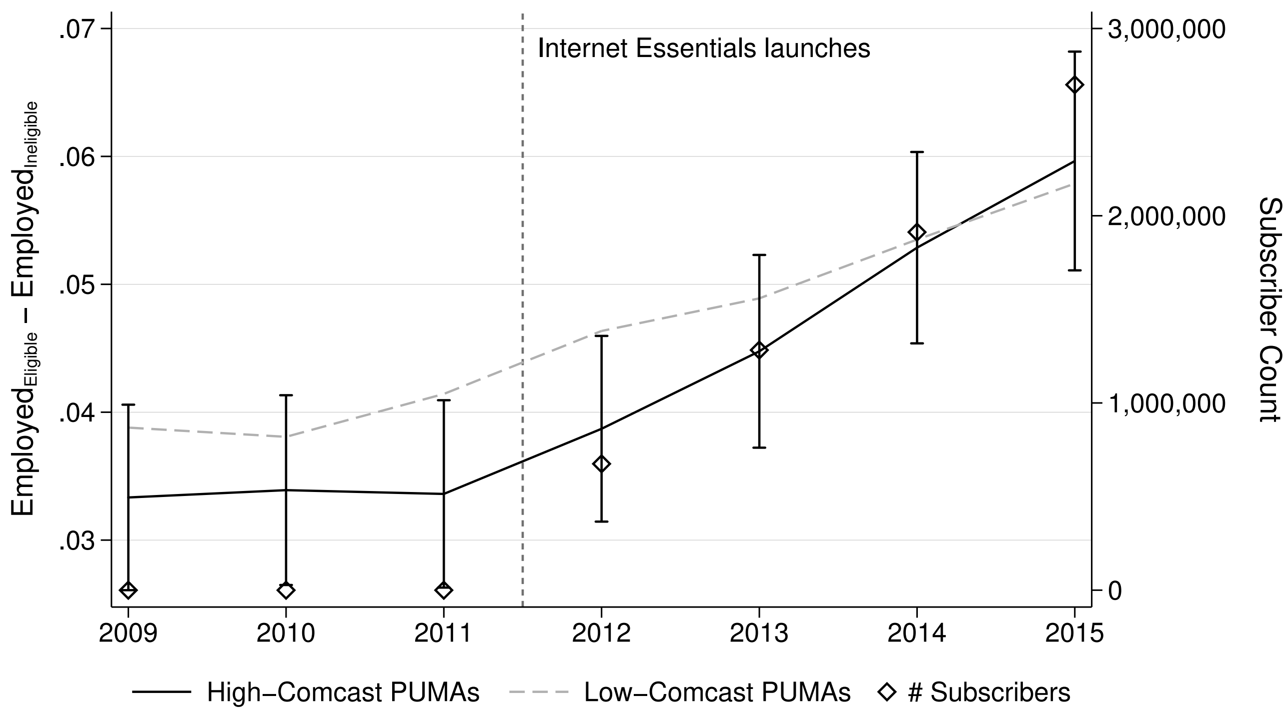
Note: This figure depicts the percent of each consistent PUMA's population that lives in a census block where Comcast provides broadband service. The data are collected at the census block level via the 2012 NTIA Broadband Map, and are aggregated at the PUMA level via Equation (2) to produce the figure above.

Figure 3: PUMA-Level Comcast Coverage Rates



Note: $N = 1,078$. This histogram shows the distribution of Comcast coverage rates calculated in Equation (2), aggregated at the IPUMS consistent-PUMA level. The data are collected at the census block level in 2012 via the State Broadband Initiative, run by the National Telecommunications and Information Administration (NTIA). The data are then aggregated at the PUMA level via Equation (2) to produce the histogram above.

Figure 4: Visualizing Triple Differences and the Employment Effects of Internet Essentials



Note: This figure plots employment differences between eligibles (<185% FPL with a school-aged child) and low-income ineligibles (<185% FPL without a school-aged child) from 2009-2015. Observations are grouped into one of two series based on whether they live in a high-Comcast PUMA (≥50% Comcast coverage) or low-Comcast PUMA (<50% Comcast coverage), as calculated in Equation (2). The triple differences estimator is conceptually derived from the difference in the two series, before and after the launch of Internet Essentials in 2012. Employment is residualized with respect to gender, age and its square, race (Black and Hispanic), marital status, years of education, number of children, and PUMA-by-year fixed effects. All calculations are weighted by person-level ACS weights. Data come from the 2009-15 one-year ACS estimates merged with the 2012 NTIA broadband map, as described in Sections 3.4 and 3.5. Subscriber counts mirror those presented in Table 1.

Table 1: Internet Essentials Take-up Rate Estimates

Year	(1) # Households Served	(2) # Individuals Served (Approx.)	(3) Estimated IE Market Size	(4) Estimated Take-Up
2012	150,000	675,000	8,216,945	8.2%
2013	285,000	1,282,500	8,199,881	15.6%
2014	425,000	1,912,500	8,110,835	23.6%
2015	600,000	2,700,000	7,905,469	34.2%

Note: This table presents take-up rates of Internet Essentials from 2012 to 2015. Data on households served are obtained from Comcast's 5-Year progress report for Internet Essentials (Comcast Corporation, 2016). Individual counts are calculated by using the ACS to determine the average family size of individuals who are eligible for the program (4.5). The estimated market size is determined by multiplying the total number of eligibles living in each PUMA by the percentage of the population living in a Census block with Comcast broadband coverage, then summing across all PUMAs nationwide. Take-up rates are calculated by dividing individuals served by the total market size. The blended take-up rate across 2012-15 is 20.3%.

Table 2: PUMA-Level Summary Statistics, by Comcast Coverage

	2011 Levels			Change between 2009-2011		
Variable	High Comcast	Low Comcast	P-Value	High Comcast	Low Comcast	P-Value
Population	336,037	390,964	0.08	49,341	63,268	0.03
% Male	0.48	0.48	0.10	-0.003	-0.001	0.08
Average Age	46.18	46.61	0.07	0.380	0.456	0.06
% Black	0.14	0.10	0.02	0.005	0.002	0.00
% Hispanic	0.14	0.14	0.82	0.009	0.006	0.00
% Married	0.50	0.52	0.01	-0.011	-0.013	0.24
Average Years Education	13.37	13.01	0.00	0.060	0.052	0.37
Average Children	0.70	0.70	1.00	-0.012	-0.015	0.38
Unemployment Rate	0.10	0.10	0.04	0.003	0.004	0.87
Average Income (% FPL)	314.84	294.61	0.00	-9.965	-8.279	0.01
% with NSLP-eligible child	0.07	0.08	0.00	0.005	0.004	0.14
% Any Internet (2013)	0.81	0.76	0.00	.	.	.
% Broadband Internet (2013)	0.75	0.69	0.00	.	.	.
Number of PUMAs	404	674		404	674	

Note: This table provides summary statistics aggregated to the IPUMS consistent-PUMA level. "High Comcast" refers to any PUMA where Comcast coverage rates equal or exceed 50 percent, where coverage is computed based on Equation (2). "Low Comcast" refers to PUMAs where coverage is less than 50 percent. All calculations are weighed by PUMA-level populations (except for the population outcome), which are calculated by adding individual-level person weights for each PUMA. Trends are weighed by 2009 populations (except for the population outcome). The sample used to construct these summary statistics includes all non-institutionalized respondents ages 18 and older in the ACS. Internet data is only available in the ACS beginning in 2013.

Table 3: Labor Market Effects of PUMA-wide Internet Essentials Availability

Outcome	Employed	In Labor Force	Unemployed	ln(Wage Income)
Treatment group mean (2011)	0.567	0.710	0.143	5.678
A. Control Group = All Ineligibles				
(% Comcast Coverage)$\times$(Year$\geq$2012)	0.009***	0.003	-0.006**	0.075**
$\times$(IE-Eligible)	(0.003)	(0.003)	(0.002)	(0.029)
N	16,557,536	16,557,536	16,557,536	16,557,536
Adjusted R^2	0.240	0.285	0.035	0.271
Control group mean	0.601	0.662	0.061	6.381
B. Control Group = Ineligibles with Children in K-12				
(% Comcast Coverage)$\times$(Year$\geq$2012)	0.009***	0.003	-0.006**	0.069**
$\times$(IE-Eligible)	(0.003)	(0.003)	(0.002)	(0.033)
N	3,453,660	3,453,660	3,453,660	3,453,660
Adjusted R^2	0.152	0.136	0.043	0.190
Control group mean	0.843	0.878	0.035	8.949
C. Control Group = Ineligibles with Low Income				
(% Comcast Coverage)$\times$(Year$\geq$2012)	0.009***	0.004	-0.005*	0.061*
$\times$(IE-Eligible)	(0.003)	(0.003)	(0.002)	(0.031)
N	4,656,835	4,656,835	4,656,835	4,656,835
Adjusted R^2	0.160	0.222	0.038	0.205
Control group mean	0.352	0.464	0.112	3.692

Note: This table shows the effects of PUMA-wide availability of Internet Essentials, estimated using triple differences via Equation (1). Outcomes for employment, labor force participation, and unemployment represent probabilities. "% Comcast Coverage" refers to the percentage of a PUMA's population living within Comcast's broadband service territory, which is calculated following Equation (2). "IE-Eligible" is a binary indicator for whether a respondent is eligible for Internet Essentials, which requires 1) family income less than or equal to 185% FPL, and 2) a child enrolled in K-12. Panel A compares eligibles to all individuals who are ineligible. Panel B compares eligibles to ineligibles who have a school-aged child. Panel C restricts to ineligibles whose family income is below the program threshold. Treatment group means are weighted by person-level weights, multiplied by the Comcast coverage rate of the individual's PUMA. Control means are weighted by person-level weights. All regressions contain controls for gender, age and its square, race (Black and Hispanic), marital status, years of education, and number of children. Regressions also control for pairwise interactions between individual eligibility, year, and PUMA fixed effects. All regressions are weighted by ACS person-level weights; standard errors are clustered at the PUMA level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Placebo Test: Effects of Exposure to Non-Comcast ISPs

	(1)	(2)	(3)	(4)	(5)
A. Employment					
Comcast × Post × Eligible	0.009***				0.009***
	(0.003)				(0.003)
Charter × Post × Eligible		-0.002			0.002
		(0.007)			(0.007)
AT&T × Post × Eligible			-0.003		-0.000
			(0.005)		(0.005)
Verizon × Post × Eligible				0.002	0.002
				(0.004)	(0.004)
N	4,656,835				
B. Accesses Internet at Home					
Comcast × Post	0.080***				0.079***
	(0.017)				(0.017)
Charter × Post		-0.057**			-0.055*
		(0.023)			(0.028)
AT&T × Post			-0.030		0.028
			(0.041)		(0.041)
Verizon × Post				0.027	-0.016
				(0.020)	(0.028)
N	37,976				

Note: This table presents results from a placebo test replacing Comcast coverage rates with coverage rates of the three next largest ISPs: Charter (Time Warner Cable), AT&T, and Verizon. Panel A contains triple differences results from Equation (1), using employment as the outcome variable. The analysis uses the group of low-income ineligibles as the control group (see Panel C of Table 3). These regressions are weighted by ACS person-level sample weights, and standard errors are adjusted for clustering at the PUMA level. Panel B contains a similar falsification test for the internet use differences-in-differences regression from Equation (5), where the sample includes only those eligible for Internet Essentials and treatment is at the state level. These regressions are weighted by CPS supplement weights, and standard errors are adjusted for clustering at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects of Internet Essentials Availability; Triple Differences Treatment/Control Refinements

	Employed	In Labor Force	Unemployed	ln(Wage Income)
A. Control Group: All Low-Income Ineligibles				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.004	-0.005*	0.061*
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(0.031)
N	4,656,835	4,656,835	4,656,835	4,656,835
Treatment group mean (Eligibles)	0.567	0.710	0.143	5.678
Control group mean (Ineligibles)	0.352	0.464	0.112	3.692
B. Control Group: Low-Income Ineligibles w/ Any Children				
(% Comcast Coverage)×(Year≥2012)	0.014***	0.009*	-0.005	0.073*
×(IE-Eligible)	(0.005)	(0.005)	(0.003)	(0.043)
N	1,605,932	1,605,932	1,605,932	1,605,932
Treatment group mean	0.567	0.710	0.143	5.678
Control group mean	0.413	0.528	0.114	4.206
C. Control Group: Low-Income Ineligibles w/ Young Children				
(% Comcast Coverage)×(Year≥2012)	0.011*	0.014**	0.003	0.106*
×(IE-Eligible)	(0.007)	(0.007)	(0.005)	(0.061)
N	1,064,243	1,064,243	1,064,243	1,064,243
Treatment group mean	0.567	0.710	0.143	5.678
Control group mean	0.543	0.695	0.152	5.774
D. Sample: Low-Income Parents, Oldest Child Aged 2-11				
(% Comcast Coverage)×(Year≥2012)	0.015*	0.018**	0.003	0.050
×(IE-Eligible)	(0.009)	(0.008)	(0.007)	(0.077)
N	554,555	554,555	554,555	554,555
Treatment group mean	0.563	0.710	0.146	5.753
Control group mean	0.551	0.701	0.151	5.797

Note: This table is an extension of Table 3, where additional restrictions are placed on control and treatment groups to more convincingly satisfy the triple differences identifying assumption. Panel A contains results from Panel C in Table 3, using low-income ineligibles as the control group. Panel B further restricts this group to low-income ineligibles with children living in the household (but are too old/young to be eligible for the NSLP). Panel C then restricts this group to low-income ineligibles with children four and under. Panel D restricts both the treatment and control groups to low-income individuals whose oldest children are between the ages of 2 and 11, eliciting a comparison between low-income parents with pre-K versus elementary-aged children. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Event Study of Internet Essentials and Labor Market Outcomes

	Employed	In Labor Force	Unemployed	ln(Wage Income)
(% Comcast)×(IE-Eligible)×(Year=2009)	0.003	0.009	0.006	0.093
	(0.006)	(0.005)	(0.005)	(0.058)
(% Comcast)×(IE-Eligible)×(Year=2010)	0.007	0.007	0.000	0.018
	(0.007)	(0.006)	(0.005)	(0.058)
(% Comcast)×(IE-Eligible)×(Year=2012)	0.005	-0.000	-0.005	0.021
	(0.006)	(0.006)	(0.005)	(0.056)
(% Comcast)×(IE-Eligible)×(Year=2013)	0.010	0.006	-0.004	0.060
	(0.006)	(0.006)	(0.004)	(0.059)
(% Comcast)×(IE-Eligible)×(Year=2014)	0.015**	0.015***	0.001	0.176***
	(0.006)	(0.006)	(0.005)	(0.056)
(% Comcast)×(IE-Eligible)×(Year=2015)	0.018***	0.015**	-0.003	0.132**
	(0.006)	(0.006)	(0.004)	(0.057)
N	4,656,835	4,656,835	4,656,835	4,656,835

Note: This table shows the labor effects of PUMA-wide Internet Essentials availability, allowing the triple interaction term in Equation (1) to vary each year. The interaction term on the final pre-treatment year 2011 is omitted. The control group is the set of all low-income non-eligibles. The analysis is otherwise identical to the main analysis presented in Panel C of Table 3. All estimates are weighted using ACS person weights, and standard errors are clustered at the PUMA level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Sensitivity and Robustness of Triple Differences Employment Estimates

	(1)	(2)	(3)	(4)
A. Sensitivity to Income Eligibility Threshold				
DDD Estimate	0.010***	0.009***	0.005**	0.003
	(0.004)	(0.003)	(0.003)	(0.003)
N	3,111,571	4,656,835	6,443,309	7,738,828
Adjusted R^2	0.131	0.160	0.181	0.192
Income Eligibility Threshold:	150% FPL	185% FPL	250% FPL	300% FPL
B. Sensitivity to Geographic Aggregation				
DDD Estimate	0.009***	0.010	0.006	0.009
	(0.003)	(0.007)	(0.005)	(0.006)
N	4,656,835	2,608,046	3,245,268	4,656,835
Adjusted R^2	0.160	0.147	0.150	0.153
Level of Geographic Aggregation:	PUMA	County	CBSA	State
C. Alternative Specifications and Data Assumptions				
DDD (or DD) Estimate	0.010***	0.008**	0.013***	0.009***
	(0.003)	(0.003)	(0.004)	(0.003)
N	7,544	4,656,835	2,819,637	5,935,298
Adjusted R^2	0.469	0.160	0.159	0.164
Specification	DD	DDD	DDD	DDD
Modification	PUMA-Aggregated	Time-varying Comcast	Discretization	Include 2016/17

Note: This table presents several sensitivity and robustness checks. The baseline regression is the triple differences specification in Equation (1) and implemented in Table 3. In Panel A, we allow for the low-income eligibility threshold to vary from its original value of 185%. In Panel B, we change the level of geographic aggregation from PUMAs to counties, metros, and states. In Panel C, we provide four additional tests. Column (1) provides an aggregated, PUMA-level differences-in-differences regression, estimated using Equation (A6). All aggregation is conducted after restricting the sample to those eligible for Internet Essentials. PUMAs are weighted by population, and standard errors are adjusted for clustering at the PUMA level. Column (2) allows Comcast coverage rates in Equation (1) to vary over time. Column (3) changes the definition of $Comcast_c$ in Equation (1) to a binary indicator equal to one if coverage rates exceed 75 percent, and zero if coverage rates are zero. Column (4) includes ACS data from 2016 and 2017, after other large-scale commercial/federal broadband subsidy programs were launched. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix A: Conceptual Model

We formalize a model illustrating the relationship between internet access, job search, and employment for the non-working job seeker. The model has two periods, and the job seeker is endowed with a time-invariant supply of internet $\theta \in [0, \infty)$, which we can interpret as the speed of in-home internet. In period 1, the individual receives a transfer y_1 and allocates his time (normalized to 1) between job search s and leisure ℓ_1 (which we generalize to include all tasks unrelated to job search). For simplicity, earnings and transfers are spent on consumption c, and by assuming that internet is an endowment, we abstract from prices and quantities of internet consumption.[34]

In period 2, the individual is employed with probability $p(s, \theta)$ or remains non-employed with probability $1 - p(s, \theta)$. The function $p(s, \theta)$ represents a mapping of search intensity and internet endowment onto the probability of being employed, and is weakly increasing and concave in both arguments to reflect positive but diminishing returns to search intensity and internet access. If employed, the individual receives earnings wh and can spend the rest of his allocation of time on leisure, $\ell_2 = 1 - h$. Since workers eligible for Internet Essentials are likely to be low-wage workers, we assume for simplicity that there is no bargaining over wages w or hours h; they are fixed and exogenously determined. If the individual is not employed, there are no earnings, and he spends his entire endowment of time on leisure, $\ell_2 = 1$. The individual also receives a transfer of y_2 regardless of employment status.[35]

[34]In practice, an internet subsidy will weakly induce all individuals to increase their consumption of internet; the model would therefore be needlessly complicated by introducing an additional choice variable along the consumption margin.

[35]We abstract from the possibility that the transfer income amount y_2 may be conditional on employment and earnings.

The full maximization problem can therefore be expressed as follows:

$$\max_{s} u(c_1, \ell_1, \theta) + \beta E u(c_2, \ell_2, \theta) \tag{6}$$

$$\text{s.t. } c_1 = y_1$$

$$\ell_1 = 1 - s$$

$$c_2 = \begin{cases} y_2 + wh & \text{w.p. } p(s, \theta), \\ y_2 & \text{w.p. } 1 - p(s, \theta) \end{cases}$$

$$\ell_2 = \begin{cases} 1 - h & \text{w.p. } p(s, \theta) \\ 1 & \text{w.p. } 1 - p(s, \theta) \end{cases}$$

$$0 \leq s, p(s, \theta) \leq 1$$

where $u(c, \ell, \theta)$ is assumed to be increasing and strictly concave in all three arguments. Note that because $s \in [0, 1]$, we will also need to consider corner solutions. An interior solution satisfies the following first order condition:

$$\frac{\partial u(y_1, 1 - s, \theta)}{\partial l_1} = \beta \frac{\partial p(s, \theta)}{\partial s} \left[u(y_2 + wh, 1 - h, \theta) - u(y_2, 1, \theta) \right] \tag{7}$$

Equation (7) implies that the individual optimally chooses search intensity s such that the utility loss from giving up a marginal unit of leisure in period 1 equals the expected discounted utility gain from spending a marginal unit of time on job search, which is equal to the difference between utility from employment versus unemployment in period 2. Because $\frac{\partial p}{\partial s}$>0, a necessary condition for the existence of an interior solution is $u(y_2 + wh, 1 - h, \theta) > u(y_2, 1, \theta)$, or that the individual prefers employment to unemployment in period 2. Otherwise, the individual prefers to be unemployed and will allocate no time to search in period 1.

We now explore how optimal search intensity $s^*(\theta)$ changes as internet θ increases in response to a subsidy. The first order condition in (7) can be differentiated with respect to θ to derive the following expression for $s'(\theta)$, where subscripts represent partial derivatives, u^1 represents utility in period 1, u^{emp} represents utility from being employed in period 2, and u^{unemp} represents utility from being unemployed in period 2:

$$s'(\theta) = \frac{\beta p_{s\theta}(u^{emp} - u^{unemp}) + \beta p_s \frac{\partial}{\partial \theta}[u^{emp} - u^{unemp}] - u_{\ell\theta}}{-u^1_{\ell\ell} - \beta p_{ss}(u^{emp} - u^{unemp})} \tag{8}$$

Since $u_{\ell\ell} < 0$ and $p_{ss} < 0$, and we must have $u^{emp} > u^{unemp}$ in order for the first order condition in (7) to hold, the denominator in (8) is positive. There are therefore three components in the numerator that determine how search intensity changes in response to an increase in the endowment of internet:

1. $p_{s\theta}$, or the change in the the marginal productivity of search in response to receiving more internet. This will be smaller for individuals with higher endowments of θ, e.g., they use high-speed internet through phones, libraries, public WiFi, etc.

2. $u_{\ell\theta}$, or the change in marginal utility from an additional unit of leisure in response to internet access. Recall that "leisure" here represents the sum of all non-job search activities, including home production. For example, while $u_{\ell\theta}$ is likely to be positive for actual leisure activities (internet increases marginal utility of leisure), it is likely negative for home production (internet makes all home production time more productive, so the marginal time spent on home production will give less utility).

3. $\frac{\partial}{\partial \theta}(u^{emp} - u^{unemp})$, or the change in relative attractiveness of employment to unemployment in response to higher θ.

The balance between these factors represents the push-and-pull between expected payoffs from job search, and utility from spending time on other activities. The goal of this paper is to determine how the probability of employment $p(s(\theta), \theta)$ changes with θ, which can be expressed via the chain rule:

$$\frac{d}{d\theta} p(s(\theta), \theta) = \frac{\partial p}{\partial s} s'(\theta) + \frac{\partial p}{\partial \theta} \tag{9}$$

From this equation, we observe that the change in employment can be decomposed into a search intensity and a search productivity component. Since the first partials of $p(s, \theta)$ are both positive, Equation (9) implies that employment will unequivocally increase if search intensity increases ($s'(\theta) > 0$ in Equation (8)); on the other hand, if internet access causes search intensity to decline, then the net effect on employment will depend on the relative magnitude between the gains to search productivity and the distortions to search intensity.

Appendix B: Data and Technical Appendix

B.1. Technical Geographic Appendix

We create a variable that calculates the percentage of the population in a given area that has access to broadband internet. A major challenge in creating this variable is that Internet Essentials launches in 2012, so that data before the program launches is based on the 2000 decennial census geographies and data after the program launches is based around 2010 decennial census geographies. Microdata from the American Community Survey provides very rich data at the individual level, but data on where these individuals live is constrained. When performing studies across census periods, it's typical to use data at the county level, however this significantly limits which geographies are observed to mostly highly populated urban areas.

Census geographies are constructed using very small areas, called census blocks. These areas are "statistical areas bounded by visible features, such as streets, roads, streams, and railroad tracks, and by nonvisible boundaries, such as selected property lines and city, township, school district, and county limits and short line-of-sight extensions of streets and roads."[36] In 2010 census blocks on average contained roughly 27 people. Each census block is based on a 15 digit code. The first through fifth contain the county fips code, and the first through eleventh digit contain the census tracts. Census block and tract boundaries change every decennial census, but counties typically don't. By construction then counties tend to be a time consistent unit of geography.

The smallest unit of geography that fully covers the United States and is publicly available in the American Community Survey (ACS) is the public use micro area (PUMA). These areas nest within states, contain atleast 100,000 people, and are built on census tracts and counties. There are three possible cases that define the relationship between a PUMA and county. First, a PUMA can be coterminous within a county. Second, highly populated counties can be defined by many PUMAs. Third, multiple low population counties can be defined by a single PUMA. An individuals county is only observed in the ACS in the first and second case. This primarily results in observing counties that contain more than 100,000 people, which are highly urban areas.

The primary challenge is that PUMAs boundaries have changed from the decennial census in 2000 to 2010. Since PUMAs are built around census tracts, which have changing boundaries to reflect physical changes such as new roads and highways as well changes in population trends. Tracts with

[36]Source: See 2010 Census Summary File 1 Urban/Rural Update Technical Documentation prepared by the U.S. Census Bureau, 2012 at A-10, http://www.census.gov/prod/cen2010/doc/sf1.pdf.

high population growth are split and tracts with population decline are combined. In order to use data from the ACS from 2009 to 2015 consistent geographies across time need to be constructed.

The Integrated Public Use Microdata Series (IPUMs) has created a geographic entity, Consistent Public Use Microdata Areas (CPUMAs), which are consistent across time. Each CPUMa is "an aggregation of one or more 2010 PUMA that, in combination, align closely with a corresponding set of 2000 PUMAs." This process essentially creates new geographies where PUMAs in 2000 and 2010 are perfectly mapped into one another. This creates larger geographies than the original PUMAs, where the number of PUMAs in 2010 is 2378 but the number of CPUMAs is 1085.

To create an access to internet variable, we use two main sources. For internet availability from 2012-2013 data are collected from the National Telecommunications and Information Administration. The data come from the State Broadband Initiative, which among other things has worked to assist states in collecting data on availability, speed, and location of broadband services within a state. The result is a dataset that contains data on which internet providers and services are available in each census block. Using this data it is possible to distinguish which blocks are served by different internet service providers, for instance which census blocks have access to Comcast internet services. Data is similarly available from 2014-2015 from Federal Communications Commissions Fixed Broadband Deployment Data in the same format.

We create several access to internet variables by different internet service providers; one for Comcast, Verizon, AT&T, Charter, and Time Warner. To create this variable for Comcast, for instance, we first identify which census blocks Comcast internet is available in. We then merge these census blocks to a census block to PUMA concordance file. Since PUMAs are created along census tracts, each census block is entirely contained by a PUMA. This concordance file, which contains the PUMA and census block population from the 2010 census, is created using the Missouri Census Datacenter's Geocorr 2014 utility. Then, within a puma, we aggregate the census block populations that have access to Comcast and divide this number by the total PUMA population. In all years access to internet service providers are based on 2010 census populations since census block populations are only observed during the decennial census. An example is that PUMA 0100100 which corresponds to Lauderdale, Colbert, Franklin, and Marion counties in Alabama has a total population of 148,972, of which 99,283 live in census blocks which have access to Comcast, so 66.6% of the population has access to Comcast.

Once population data are aggregated to the PUMA level, they are then further aggregated into CPUMAs using the IPUMs 2010 PUMA components list, which gives a listing of the 2010 PUMAs that comprise each CPUMA. The result is a variable which indicates the percentage of people who live in a census block based on 2010 population definitions who have access to Comcast. This process is repeated for each internet service provider and each year.

B.2. Construction of ACS Analysis Data

We took the following steps to construct the analysis data set from the ACS

- We obtained ACS 1-year estimates from IPUMS
- We restricted the sample to individuals age 18 and older and dropped anyone with a RELATE code indicating institutional inmate status
- We merged to the NTIA broadband data via the consistent-PUMA code in the ACS
- We merged on CBSA via the 2013 metro FIPS delineation
- We defined "child" status as having at least one child between the ages of 5 and 18. This was done by making sure that the eldest child was at least 5 and that the youngest child 18 years old or younger
- Poverty status is defined via the POVERTY variable in the ACS

See supplemental code for details on coding of outcomes and control variables in the ACS.

B.3. Construction of CPS Analysis Data

We took the following steps to construct the analysis data set from the CPS:

- We obtained Computer and Internet Use supplements for the years 2007, 2009, 2010, 2011, 2012, 2013, and 2015 via IPUMS.
- We obtained federal poverty tables for years 2009-15 from: https://aspe.hhs.gov/poverty-guidelines. The tables were compiled in a Excel file in the "Raw Data" directory named "poverty_tables.xlsx".
- We merged this data to family sizes in the CPS.
- We restricted the sample to individuals age 18 and older
- We merged the NTIA broadband data at the county, CBSA (2013 metro FIPS), and state levels
- We defined "child" status as having at least one child between the ages of 5 and 18. This was done by making sure that the eldest child was at least 5 and that the youngest child 18 years old or younger
- For poverty status, we first use the income brackets provided in the CPS, then took the upper bound of each bracket. We then merged this onto the aforementioned poverty tables.

B.3. CPS Internet Use Survey Questions

The following are the exact questionnaires used to derive the IPUMS internet use indicators in the CPS Computer and Internet Supplements.

July 2015 Computer and Internet Use Supplement

- Universe: all Supplement respondents

- Question Number/Text: INHOME / [Do you/Does anyone in this household] use the Internet at home? *(Yes: 70.0%)*

July 2013 Computer and Internet Use Supplement

- Universe: All respondents
- Question Number/Text: NET3 / Does anyone in this household use the Internet from home? *(Yes: 78.6%)*

October 2012 School Enrollment and Internet Use Supplement

- Universe: All respondents
- Question Number/Text: NET3 / People can connect to the Internet in multiple ways, including using mobile devices such as laptops or smartphones, as well as on desktop computers. Does anyone in this household use the Internet from home? *(Yes: 79.2%)*

July 2011 Computer and Internet Use Supplement

- Universe: Universe: All households where respondent accesses the internet from any location
- Question Number/Text: PUHOME / Does Name1 access the Internet from home? How about Name 2? (Does Name2 access the Internet from home?) Etc. *(Yes: 98.8%)*

October 2010 School Enrollment and Internet Use Supplement

- Universe: All households where respondent uses some sort of computer
- Question Number/Text: NET2a / At home, [do you / do you or any member of this household] access the Internet? *(Yes: 93.5%)*

October 2009 School Enrollment and Internet Use Supplement

- Universe: All households where respondent accesses the internet from any location
- Question Number/Text: NET3 / (Do you/Does anyone in this household) connect to the Internet from home? *(Yes: 91.0%)*

October 2007 School Enrollment and Internet Use Supplement

- Universe: All households where respondent accesses the internet from any location
- Question Number/Text: NET3 / (Do you/Does anyone in this household) connect to the Internet from home? *(Yes: 88.5%)*

Appendix C: Broadband Production and Regulation

This content is directly adapted and summarized from Figure A1 in Tomer, Kneebone and Shivaram (2017), which we highly recommend for further reading on this subject.

How Does Broadband Ultimate Reach Consumers?

Broadly speaking, broadband infrastructure is comprised of the backbone, the middle mile, and the last mile.

- The backbone is the physical stock of large capacity trunks that is capable of transmitting large amounts of data, and is where "broadband" originates.
- Between the backbone and middle mile, internet traffic across ISPs is processed through physical locations called Internet eXchange Points (IXPs), which require mutual peering agreements. At Points of Presence (POPs), long-distance carrier cables transfer into a regional or city network.
- In the last mile, broadband is delivered to homes and end users through telephone/utility poles through cable companies or telephone exchanges.

Federal Regulations

- Right-of-Way Permits: ISPs must secure permits to build and operate on federal lands, buildings, highways, and roadways.
- Franchise Agreements: The Cable Communications Act of 1984 requires ISPs to reach a contract with local governments.
- Pole Permits: ISPs must secure agreements to access telephone/utility poles that are owned by investor-owned utilities in states without pole regulations prior to the Pole Attachment Act.

State Regulations

- Right-of-Way Permits: ISPs must secure permits to build and operate on state lands, buildings, highways, and roadways.
- Franchise Agreements: ISPs must secure state-wide cable and video franchise agreements from the Department of State or Public Utilities Commissions.
- Pole Permits: ISPs must secure agreements to access telephone/utility poles in states that had pole regulations which pre-empted federal regulation.

Local Regulations

- Right-of-Way Permits/Franchise Agreements: The franchise agreements listed above typically include local right-of-way permits, and can also include franchise fees, programming requirements, and customer service standards.
- Pole Permits: ISPs must secure agreements to access telephone/utility poles owned by public electric cooperatives and municipalities in states without any specific pole regulation.
- Last-mile Access: Often, ISPs must contend with exclusive contracts provided by owners/homeowners' associations, who otherwise gate access to customers living in individual/multiple dwelling units.

Appendix D: Calculation and Derivation of Google Relative Search Indices

Suppose we request a data set of n different search terms from the year t. First, search volumes for search term i in metro m are re-calculated as a share of the entire volume of searches that occurred in m during year t, which we denote as s_{im}^{t}. Second, the index is re-scaled to a range of 0 to 100, with 100 being assigned to the maximum value of s_{im}^{t} across all n requested search terms and all M metros in the entire extract for year t. Each remaining s_{im}^{t} is then assigned a corresponding index value proportional to its relationship to the maximal value, rounded to the nearest integer. Explicitly, the index is calculated as follows, before rounding:

$$Index_{im}^{t} = 100 \times \frac{s_{im}^{t}}{\max_{i,m}\left\{s_{im}^{t}\right\}} = 100 \times \frac{Searches_{im}^{t} / \sum_{i=1}^{n} Searches_{im}^{t}}{\max_{i,m}\left\{Searches_{im}^{t} / \sum_{i=1}^{n} Searches_{im}^{t}\right\}} \tag{10}$$

This relative index has several important shortcomings for our uses. First, although within-metro comparisons of search term volumes are possible, search volumes cannot be compared across metros. Second, data from different years are not comparable due to shifting baselines of underlying total search volumes. We therefore propose two counteracting transformations that will facilitate cross-year and cross-metro comparisons. First, we note that for two search terms $i = 1, 2$, the ratio $\frac{Index_{1m}^{t}}{Index_{2m}^{t}}$ is equal to the ratio of absolute search volumes $\frac{Searches_{1m}^{t}}{Searches_{2m}^{t}} \equiv r_{m,1,2}^{t}$. Suppose that the numerator represents our search term of interest, "Snagajob". If we choose a search term for the denominator which does not vary in absolute search volumes over time, then within-metro, cross-time comparisons of Snagajob become possible. We refer to such search terms as *reference* terms. Finally, despite being comparable across time, differences in levels of $r_{m,1,2}^{t}$ will vary across metros. To address this, we implement a within-metro de-meaning of $r_{m,1,2}^{t}$ in order to facilitate comparisons across metros, which we denote as $\tilde{r_{m,1,2}^{t}}$.

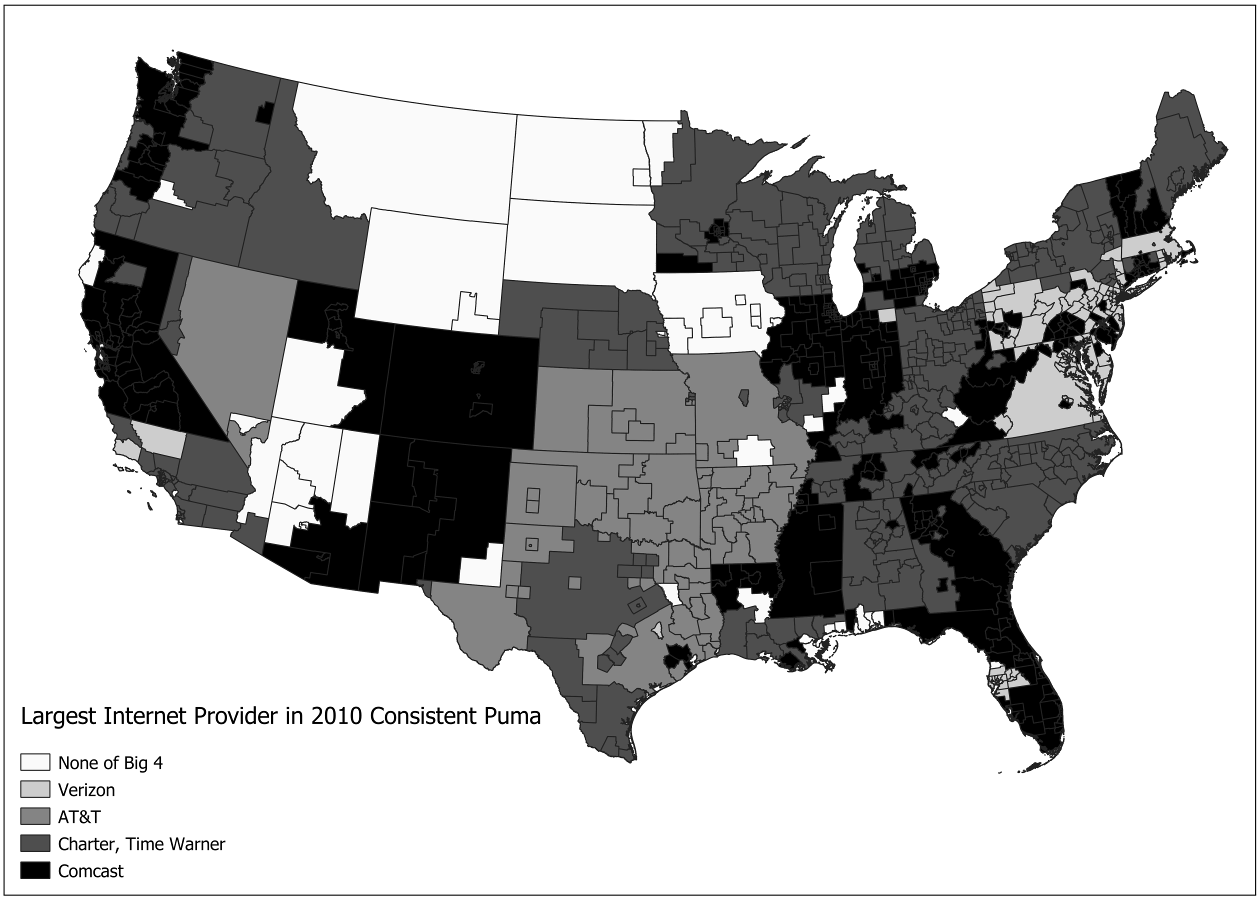
One final difficulty is the fact that all index values are rounded to the nearest integer. Some reference search terms will be sufficiently popular such that the relative frequency of "Snagajob" will register as mostly zeroes by comparison. Conversely, terms with insufficient search volumes will also register as zeroes in comparison to "Snagajob". Therefore, for scaling purposes, it is important to choose reference terms with search volumes of comparable magnitudes to "Snagajob", in order to avoid the issue of zeroes in the calculation of the ratio $r_{m,1,2}^{t}$.

Appendix E: Appendix Tables and Figures

Figure A1: Major Comcast M&A Events: 1990-2018

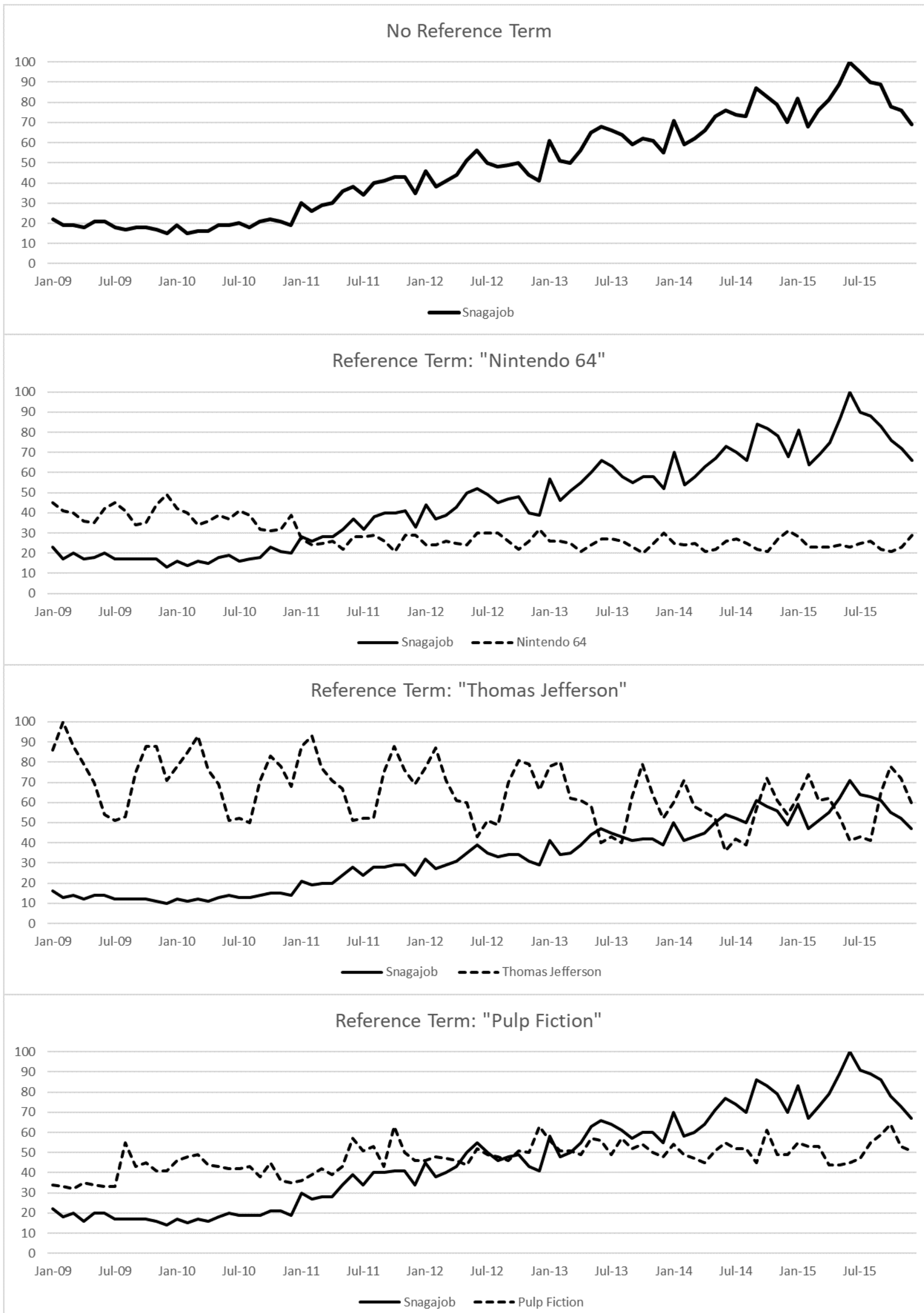
1994	Comcast acquires Canadian based Maclean Hunter's U.S. cable operation based in New Jersey, Michigan, and Florida, adding 550,000 subscribers
1995	Comcast acquires E.W. Scripps cable systems based in California, Tennessee, Georgia, West Virginia, Florida, and Kentucky, adding 800,000 subscribers
1998	Comcast acquires Jones Intercable, Inc in the Mid-Atlantic adding 1 million subscribers
1998	Comcast acquires Prime Communications in Maryland, Virginia, adding 430,000 subscribers
1999	Comcast acquires Greater Philadelphia Cablevision, Inc in Philadelphia, adding 79,000 subscribers
1999	Comcast and AT&T enter agreement to exchange cable communications systems, gaining cable communications systems serving 1.5 million subscribers
2000	Comcast acquires Lenfest Communications in Pennsylvania, Delaware and New Jersey adding 1.3 millions subscribers
2000	Comcast completes cable swaps with Adelphia and AT&T broadband, gaining customers in Florida, Indiana, Michigan, New Jersey, New Mexico, Pennsylvania and Washington D.C.
2001	Comcast acquires select AT&T Broadband cable systems in New Mexico, Maryland, Delaware, New Jersey, Pennsylvania and Tennessee adding 585,000 subscribers
2001	Comcast acquires AT&T Broadband cable systems in Baltimore adding 112,000 subscribers
2001	Comcast and A&T Broadband merge forming the largest cable provider in the US with nearly 22 million subscribers
2005	Comcast and Time Warner jointly acquire Adelphia communications, gaining 1.7 millions video subscribers
2005	Comcast acquires cable systems of Susquehanna Communctions in Pennsylvania, Mississippi, Maine, Illinois, Indiana and New York, gaining 225,000 subscribers
2005	Comcast and Time Warner jointly acquire the assets of Adelphia Communications, with Comcast gaining 1.7 million subscribers
2006	Comcast and Time Warner announce that Comcast will take over holdings in Texas gaining 700,000 subscribers
2007	Comcast acquires Patriot Media in New Jersey, gaining 81,000 subscribers
2008	Comcast complete the acquisition of Insight cable systems in Illinois and Indiana, gaining 696,000 subscribers

Figure A2: Largest ISP in each PUMA



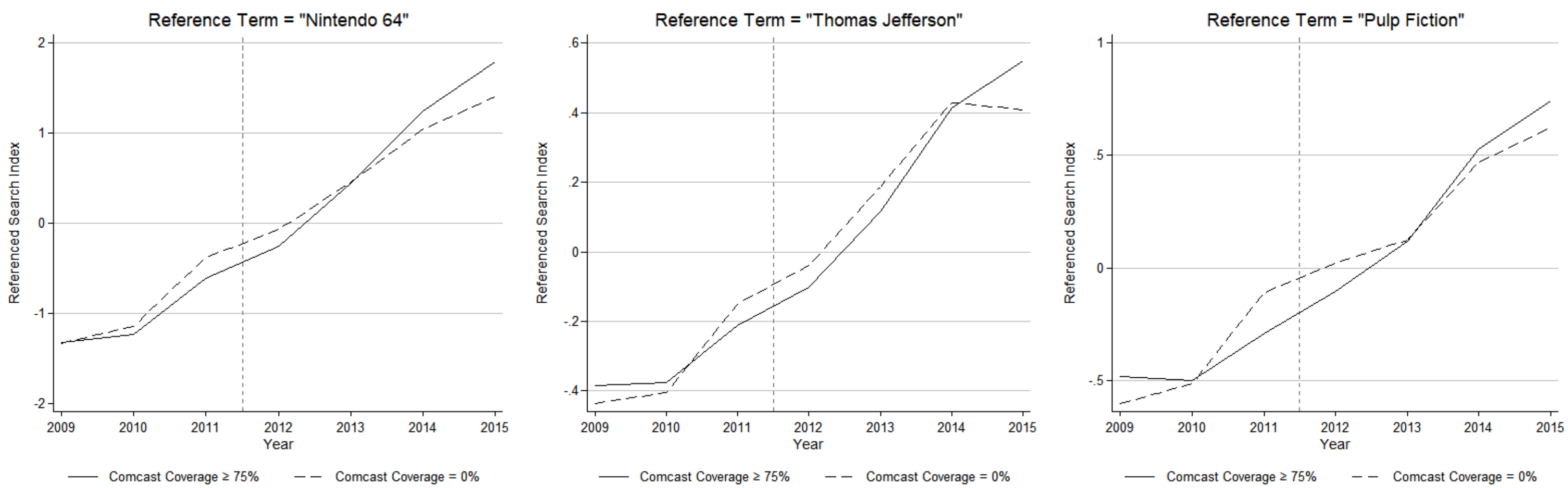
Note: This figure shows which of the four largest ISPs (Comcast, Charter, AT&T, Verizon) serves the greatest percentage of population for each consistent PUMA. PUMAs with no shading were served primarily by ISPs other than AT&T, Charter, Verizon, and Comcast.

Figure A3: National-Level Google Trends



Note: This figure provides Google Trends search indices at a national level. The y-axis is Google's search index, which here represents the number of searches for a given search term in a given month, relative to the total volume of all searches in that month. The search index is then normalized such that across all data points in the series, and the maximal value is set to 100. All four graphs provide trends for the search term "Snagajob"; the latter three compare against reference terms which we assume to be approximately constant in terms of annual absolute search volumes.

Figure A4: Trends in Google Searches for “Snagajob”, by Comcast Availability



Note: This figure shows trends in Google Searches, comparing metros with higher versus lower availability of Comcast. Each series shows the ratio of Google’s relative search indices for the term “Snagajob” and a reference term. As we explain in Appendix D, relative search indices provided by Google are not comparable across time or across metros in their original form. However, the ratio of two relative search indices is equivalent to the ratio of absolute search volumes. Therefore, if the absolute search volume of the reference term is approximately constant over time, the resulting ratio will yield an absolute measure of search volumes which is comparable over time. The ratio is made comparable across metros by subtracting out within-metro means.

Table A1: Frequency and Effectiveness of Job Search Methods

	Has Broadband	No Broadband
Used in job search		
Connections	83.4%	67.1%
Online search	81.3%	67.0%
Employment agency	33.0%	27.9%
Print ads	31.1%	34.8%
Job fairs	26.6%	30.3%
Other	10.1%	11.8%
Most effective resource		
Connections	49.8%	42.3%
Online search	30.9%	32.2%
Employment agency	6.5%	2.4%
Print ads	2.7%	5.2%
Job fairs	5.5%	7.2%
Other	3.6%	4.1%
N	478	116

Note: This table presents mean response rates from a sample of individuals who searched for a job within the last two years. Data comes from the 2015 Pew Research Center survey on gaming, jobs, and broadband. The top panel contains means indicating whether a specific job search method was ever used in the most recent job search. Respondents were allowed to select multiple methods. "Connections" is an aggregation of connections from close friends/family members, connections from acquaintances/friends of friends, and connections from professional or work settings. In the bottom panel, respondents were asked: "Thinking of the resources that you used in your last job search, which of them was the MOST important?" Columns in this panel add to less than 100 percent due to non-response. All means are weighted by Pew survey sample weights.

Table A2: Summary Statistics, by Broadband Usage and Income

	Whole Population		Poor ($\leq$185% FPL)	
	Has Broadband	No Broadband	Has Broadband	No Broadband
Male	0.49	0.47	0.44	0.43
Age	45.90	51.10	41.52	49.37
Black	0.10	0.16	0.15	0.21
Hispanic	0.13	0.20	0.22	0.25
Married	0.56	0.41	0.35	0.29
Years Education	13.75	12.04	12.48	11.18
Number of Children	0.74	0.57	0.88	0.69
Employed	0.66	0.49	0.48	0.35
In Labor Force	0.70	0.55	0.57	0.43
HH Income, % FPL	341.43	241.26	102.69	97.68
N	4,894,814	1,846,335	952,960	768,160

Note: This table presents summary statistics computed separately for individuals with and without in-home broadband. Results are also shown for a subsample of low-income adults with family incomes below 185% FPL. Broadband data in the ACS are only available 2013 and later. The sample used to construct this table includes all non-institutionalized adults living in counties that are identified in the ACS from 2013-2015. Means are weighted by ACS person-level weights.

Table A3: Top Five ISPs, by Subscriber Count

Rank	Broadband Provider	Subscribers (as of 2Q 2018)
1	Comcast	26,509,000
2	Charter	24,622,000
3	AT&T	15,772,000
4	Verizon	6,956,000
5	CenturyLink	5,506,000

Source: Leichtman Research Group, August 2018. https://www.leichtmanresearch.com/455000-added-broadband-in-2q-2018/.

Table A4: Correlation between County Coverage Rates of Top 4 Internet Service Providers

	Comcast	Charter	AT&T	Verizon
Comcast	1.000			
Charter	-0.247	1.000		
AT&T	-0.207	0.153	1.000	
Verizon	0.011	-0.209	-0.173	1.000

Note: This table presents the correlation between county-level coverage rates of the four largest Internet Service Providers in the US.

Table A5: Changes in Comcast Coverage Rates Across Time

	Δ PUMA Comcast Coverage Rates		
	2012-2013	2013-2014	2014-2015
Min	-0.129	-0.231	-0.022
P1	-0.057	-0.083	-0.010
P10	-0.002	-0.018	0.000
P25	0.000	-0.005	0.000
P50	0.000	0.000	0.000
Mean	-0.001	-0.005	0.001
P75	0.001	0.000	0.000
P90	0.003	0.002	0.002
P99	0.022	0.024	0.013
Max	0.069	0.053	0.097

Note: This table shows the distribution of how PUMA-level Comcast coverage rates change from year to year. A value of zero means no change, whereas a value of $+0.100$ implies that coverage increased by 10 percentage points. Coverage rates are calculated via Equation (2), and represent the percentage of a PUMA's population living in a census block where Comcast provides broadband service. Census block population counts are taken from the 2010 decennial census, and indicators for Comcast coverage within a given block come from the NTIA in 2012 and 2013, and from the FCC Form 477 in 2014 and 2015.

Table A6: Testing for Control-Driven Effects in Triple Differences

Outcome	Employed	In Labor Force	Unemployed	ln(Wage Income)
Treatment group mean (2011)	0.567	0.710	0.143	5.678
Panel A: Eligibles				
(% Comcast Coverage)×(Year≥2012)	0.013***	0.004	-0.009***	0.111***
	(0.003)	(0.003)	(0.003)	(0.030)
N	1,014,881	1,014,881	1,014,881	1,014,881
Group Mean	0.581	0.702	0.120	5.705
Panel B: All Ineligibles				
(% Comcast Coverage)×(Year≥2012)	0.004***	0.002*	-0.002***	0.036***
	(0.001)	(0.001)	(0.001)	(0.010)
N	15,542,655	15,542,655	15,542,655	15,542,655
Group Mean	0.603	0.662	0.059	6.362
Panel C: Ineligibles with Children				
(% Comcast Coverage)×(Year≥2012)	0.003*	0.000	-0.002**	0.037**
	(0.002)	(0.001)	(0.001)	(0.018)
N	2,438,779	2,438,779	2,438,779	2,438,779
Group Mean	0.844	0.878	0.035	8.919
Panel D: Ineligibles with Low Income				
(% Comcast Coverage)×(Year≥2012)	0.006***	0.003	-0.003*	0.066***
	(0.002)	(0.002)	(0.002)	(0.019)
N	3,641,954	3,641,954	3,641,954	3,641,954
Group Mean	0.360	0.467	0.107	3.753

Note: This table presents differences-in-differences results for four separate sub-samples: 1) the treatment group (those eligible for the program), 2) the full control group (non-eligibles), 3) the control group restricted to non-eligibles with a child, and 4) the control group restricted to non-eligibles with low-income. The DD treament variable is Comcast coverage rates. Conceptually, the triple differences estimator is the difference between the differences-in-differences estimates of the treatment and control group, so it is important to verify whether significant effects in triple differences are driven by changes in the treatment group, or less desirably, the control group. Control variables include gender, age, age-squared, race, marital status, and number of children. All regressions are weighted by ACS person-level sample weights; standard errors are clustered at the PUMA level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Effects of Metro/Statewide Availability of Internet Essentials on Internet Use

	(1)	(2)	(3)	(4)
Outcome: Uses Internet at Home				
(% Comcast Coverage)×(Yr≥2012)	0.049**	0.039**	0.080***	0.068***
	(0.022)	(0.017)	(0.017)	(0.014)
N	21,232	29,613	37,976	53,245
Untreated mean	0.650	0.681	0.635	0.670
Low-income threshold (% of FPL)	185	250	185	250
Geographic Aggregation	Metro	Metro	State	State

Note: This table provides differences-in-differences estimates from Equation (5) of the effect of metro/state-wide Internet Essentials availability on home internet use. Data on internet use come from an aggregation of internet use supplements from the Current Population Survey in 2007, 2009, 2010, 2011, 2012, 2013, and 2015. The sample is restricted to individuals eligible for Internet Essentials. Approximately 73 percent of the sample lives in a metro area that is identified in the CPS. Results are also presented using state-level aggregation, which is identified for all respondents. "% Comcast Coverage" is calculated using Equation (2) using metro/state as the respective level of geographic aggregation. The table presents results for two different income eligibility thresholds to account for noisy reports of family income in the CPS. All regressions are weighted by CPS supplement weights; standard errors are clustered at the respective metro/state level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Labor Market Effects of Internet Essentials Availability, Urban vs. Non-Urban

	Employed	In Labor Force	Unemployed	ln(Wage Income)
A. Urban: 95% Pop. in Urban Cluster				
DDD Estimate - Urban PUMAs	0.014***	0.008*	-0.006*	0.100**
	(0.005)	(0.005)	(0.003)	(0.045)
N	1,736,027	1,736,027	1,736,027	1,736,027
Treatment group mean (2011)	0.566	0.716	0.150	5.640
DDD Estimate: Non-Urban PUMAs	-0.000	-0.003	-0.002	0.013
	(0.005)	(0.005)	(0.004)	(0.048)
N	2,920,808	2,920,808	2,920,808	2,920,808
Treatment group mean (2011)	0.568	0.704	0.136	5.720
Difference (p-value)	0.035	0.129	0.438	0.201
B. Urban: 99% Pop. in Urban Cluster				
DDD Estimate - Urban PUMAs	0.010*	0.008	-0.002	0.087*
	(0.005)	(0.006)	(0.004)	(0.051)
N	1,145,717	1,145,717	1,145,717	1,145,717
Treatment group mean (2011)	0.558	0.716	0.158	5.597
DDD Estimate: Non-Urban PUMAs	0.006	0.000	-0.006**	0.033
	(0.004)	(0.004)	(0.003)	(0.040)
N	3,511,118	3,511,118	3,511,118	3,511,118
Treatment group mean (2011)	0.572	0.707	0.135	5.724
Difference (p-value)	0.692	0.358	0.473	0.482
C. Urban: ≥1,000 residents/sq. mile				
DDD Estimate - Urban PUMAs	0.014***	0.009*	-0.005	0.104**
	(0.005)	(0.005)	(0.004)	(0.047)
N	1,580,666	1,580,666	1,580,666	1,580,666
Treatment group mean (2011)	0.566	0.715	0.149	5.637
DDD Estimate: Non-Urban PUMAs	0.001	-0.003	-0.004	0.005
	(0.005)	(0.005)	(0.003)	(0.046)
N	3,076,169	3,076,169	3,076,169	3,076,169
Treatment group mean (2011)	0.568	0.706	0.138	5.719
Difference (p-value)	0.081	0.096	0.913	0.158

Note: This table replicates the triple differences analysis in Table 3 separately for urban and non-urban PUMAs. Each analysis is conducted using the group of low-income ineligibles as the control group (Panel C of Table 3). Each panel in this table contains a defines "urban" PUMAs differently. The Census classifies census blocks as "urban" if population density exceeds 1,000 people per square mile (Ratcliffe et al., 2016). A census block that touches an urban block and has a population density over 500 people per square mile is considered to be a part of an "urban cluster". Panel A classifies a PUMA as urban if 95% of the population lives within an urban cluster. Panel B increases this threshold to 99%. Panel C classifies a PUMA as urban if population density throughout the PUMA exceeds 1,000 people per square mile. All regressions are weighted by ACS person-level sample weights; standard errors are clustered at the PUMA level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Labor Market Effects of Internet Essentials Availability, by Demographic Groups

	Employed	In Labor Force	Unemployed	ln(Wage Income)
A. Gender				
DDD Estimate - Male	0.009*	-0.000	-0.009**	0.052
	(0.005)	(0.004)	(0.004)	(0.051)
N	2,016,412	2,016,412	2,016,412	2,016,412
Treatment group mean (2011)	0.691	0.842	0.151	6.758
DDD Estimate - Female	0.009*	0.007	-0.002	0.066
	(0.004)	(0.005)	(0.003)	(0.041)
N	2,640,423	2,640,423	2,640,423	2,640,423
Treatment group mean (2011)	0.494	0.633	0.139	5.045
Difference (p-value)	*0.95*	*0.28*	*0.14*	*0.83*
B. Education				
DDD Estimate - HS Grad or Greater	0.012**	0.005	-0.008*	0.077
	(0.005)	(0.005)	(0.004)	(0.051)
N	1,951,528	1,951,528	1,951,528	1,951,528
Treatment group mean (2011)	0.592	0.739	0.147	5.976
DDD Estimate - Less than HS	0.007	0.004	-0.003	0.054
	(0.005)	(0.004)	(0.003)	(0.043)
N	2,705,307	2,705,307	2,705,307	2,705,307
Treatment group mean (2011)	0.550	0.692	0.141	5.483
Difference (p-value)	*0.48*	*0.91*	*0.39*	*0.72*
C. Age				
DDD Estimate - Age 38 and Older	0.012**	0.006	-0.006*	0.092**
	(0.005)	(0.004)	(0.003)	(0.043)
N	2,748,598	2,748,598	2,748,598	2,748,598
Treatment group mean (2011)	0.569	0.707	0.139	5.500
DDD Estimate - Less than Age 38	0.006	0.002	-0.004	0.029
	(0.005)	(0.005)	(0.004)	(0.048)
N	1,908,237	1,908,237	1,908,237	1,908,237
Treatment group mean (2011)	0.565	0.714	0.148	5.859
Difference (p-value)	*0.38*	*0.46*	*0.81*	*0.31*

Note: This table replicates the triple differences analysis in Table 3 separately for gender, education, and age. Each analysis is conducted using the group of low-income ineligibles as the control group (Panel C of Table 3). All regressions are weighted by ACS person-level sample weights; standard errors are clustered at the PUMA level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Effects of Internet Essentials Availability on Job Characteristics

Outcome	Part-time	ln(Wage Income)	Transit Time (Mins.)
Treatment group mean (2011)	0.276	8.734	23.996
Panel A: Control Group = All Ineligibles			
(% Comcast Coverage)×(Year≥2012)	0.004	0.019	-0.106
×(IE-Eligible)	(0.003)	(0.025)	(0.178)
N	16,557,536	9,679,305	9,679,305
Control group mean	0.207	9.717	24.052
Panel B: Control Group = Ineligibles with Children			
(% Comcast Coverage)×(Year≥2012)	0.006*	0.004	-0.030
×(IE-Eligible)	(0.003)	(0.027)	(0.204)
N	3,453,660	2,655,248	2,655,248
Control group mean	0.177	10.199	25.429
Panel C: Control Group = Ineligibles with Low Income			
(% Comcast Coverage)×(Year≥2012)	0.000	0.017	-0.213
×(IE-Eligible)	(0.003)	(0.030)	(0.226)
N	4,656,835	1,783,238	1,783,238
Control group mean	0.259	8.331	21.126

Note: This table replicates the triple differences analysis in Table 3, using three different outcomes which are all conditional on being employed: the probability of part-time work (defined as working less than 40 hours per week), log wage income (conditional on working), and transit time to work. All regressions are weighted by ACS person-level sample weights; standard errors are clustered at the PUMA level and are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.