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

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I present evidence on the relationship between broadband pricing and labor market outcomes for low-income individuals. Specifically, I estimate the effects of a Comcast service providing discounted broadband to qualifying low-income families. I use a triple differences strategy exploiting geographic variation in Comcast coverage, individual variation in eligibility, and temporal variation pre- and post-launch. Local program availability increased employment rates and earnings of eligible individuals, driven by greater labor force participation and decreased probability of unemployment. Internet use increased substantially where the program was available.
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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 21 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. I 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

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

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

$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 a 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).

The relationship between broadband access and labor market outcomes has been a topic of growing interest for policymakers and researchers alike (Council of Economic Advisers, 2015). However, 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 economic literature in several important ways. First, to the best of my knowledge, this is the first paper that specifically focuses on low-income families. While expanding geographic broadband coverage remains an important policy objective, addressing the persistent income-broadband gap is arguably the more pressing and current issue. 97 percent of the U.S. population lives where 10 Mbps broadband speeds are available—enough to comfortably stream Netflix in HD—yet only 56 percent of low-income Americans actually own broadband subscriptions.[4] Second, studies that leverage geographic broadband expansions typically document greater broadband adoption rates in both households *and firms*, making it difficult to disentangle the labor market impacts of household versus firm broadband take-up. I analyze policy variation in broadband pricing for low-income families, which conceivably isolates the effect of increasing household take-up. Finally, the timing of the policy variation I use is the most up-to-date in the literature. This is important given that it coincides with a recent surge 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, I 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. I 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

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

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.

I 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 my main specification, I restrict the control group of ineligible individuals to those who meet the low-income requirement but do not have school-aged children. Ineligible individuals in this group may be more likely to access similar kinds of labor markets and job opportunities as those who are eligible, mitigating the likelihood that post-2012 differences between the two groups diverge in high- versus low-Comcast areas for reasons besides Internet Essentials. Building on this, I show that the estimated employment effects are reassuringly similar when further restricting the control group of ineligibles to low-income individuals who 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.

The data I use come from two sources. I first use data from the National Telecommunications and Information Administration to construct estimates of local Comcast broadband coverage rates.[5] These data include census block-level indicators of where Comcast provides broadband service in the United States. I 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 I 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 I do not observe actual program enrollment. The effects are robust to a variety of sensitivity tests and alternate specifications, including the addition of various controls for confounding trends that could potentially correlate with program availability. The effect appears driven both by increases in labor force participation and decreases in the probability of being unemployed. After adjusting for nationwide program take-up rates, I calculate that the treatment-on-the-treated (TOT) employment effect of enrolling in Internet Essentials is approximately 8.1 percentage points (14.3%), with a lower bound of 2.1 percentage points (3.7%). While large, the effect size lies within the range of previous estimates on broadband and employment, and aligns with qualitative evidence on the program's outsized role in helping job seekers find work. Availability of Internet Essentials ultimately increases income by

[5]I am especially thankful to Danny Kolliner here for his work on assembling this data set.

$147 (1.3%), an effect which appears driven by earnings gains along the extensive margin of employment. I conduct a back-of-the-envelope calculation and find that the program benefit to enrolled households is approximately $2,202. This benefit is more than four times the estimated cost to provide the service, including the monthly broadband subsidy, fee waivers, and other administrative costs.

I 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. I 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 my findings.

Finally, 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. The effect size is commensurate with a reduction in the income-broadband gap of roughly 40 percent. While the data used for this calculation have important limitations, the results are broadly consistent with other work by Rosston and Wallsten (2019), who approximate that 66 percent of Internet Essentials customers were induced by the program to begin a broadband subscription. Survey data from Internet Essentials customers also indicate that as many as 90 percent may have been first-time broadband subscribers.

The findings in this paper are generally consistent with previous work in economics documenting positive labor market effects 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 statewide 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 home broadband subscription. The findings in this paper provide evidence that Internet Essentials, which subsidized broadband subscriptions by $20 to $30 per month, meaningfully increased broadband adoption, employment, and earnings for program enrollees. Even in light of the growing ubiquity of smartphones, public WiFi, and other methods of connecting online, the benefits of a home broadband subscription remain non-trivial for low-income families navigating the labor market in the digital era.

I. Background

A. Broadband Terminology

I 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). Appendix C provides 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, I refer to broadband *availability* as a location-specific term that reflects whether an ISP supplies broadband service to end users in that location.[6]

B. 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 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).[7] 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 broadband from home. Beyond the financial costs of accessing internet, lack of digital literacy and trust of technology may impose additional psychic costs.

[6]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).

[7]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%). I note that numbers provided here differ slightly from Horrigan and Duggan (2015), who include non-responses in their tabulations, which I omit.

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 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). I use the same survey data (Pew Research Center, 2015) to characterize job seekers with and without broadband in their homes. Table A1 provides a breakdown of 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 diminish or counteract the job search benefits of cheaper broadband. The existence of substitutes for broadband, such as access through local libraries or public WiFi, could decrease the impact of Internet Essentials if low-income job seekers already leverage these alternatives effectively. 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 opt to purchase a mobile data plan instead of 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).[8] Cheaper broadband may also distort 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 also increase the option value of remaining unemployed, decreasing the expected net utility gain from job search.

[8]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 individuals on the extensive margin of labor force participation. The convenience of searching for jobs from home may be crucial for individuals unable to do so at libraries or public WiFi hotspots due to location and time constraints.[9] 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, which created opportunities for telework and decreased the costs 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.

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. The ability to navigate the internet is also 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.

C. *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). The letter 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.

The core feature of the program is that households are provided 15/2 Mbps broadband service for $9.95 a month, plus applicable taxes.[10] In addition to the subsidized price, Comcast waives all fees, including a one-time activation

[9] 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.

[10] "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.

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).[11] Finally, customers have access to 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 or 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.[12] Finally, families cannot have subscribed to Comcast internet within the last 90 days, increasing the likelihood 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 SNAP. The federal government also reformed its Lifeline program in 2016. The program had traditionally 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 my knowledge, Internet Essentials was the only widely available broadband

[11] 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.

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

subsidy program from 2012 through 2015.

II. Methods and Data

A. 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. I 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. The estimate is an intent-to-treat effect of Internet Essentials availability because I do not directly observe program enrollment.

B. Estimating the Intent-to-Treat Effects of Internet Essentials

I briefly establish two conventions. First, I 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. The triple differences strategy compares differences between eligibles and ineligibles across areas with varying degrees of Comcast coverage, before and after the launch of Internet Essentials in 2012.

The estimating equation is as follows:

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

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 income.[13] The 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

[13] Income includes earnings from both wages as well as business/farm income (to capture earnings from self-employment). I top-code the variable at the 95th percentile to remove the influence of outliers, which should not affect my primary specification (where the entire sample is low-income) but will likely affect specifications using other control group constructions.

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. I 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. I restrict the sample time frame to the post-recession years 2009 through 2015. Although data are available for later years, I exclude them in the 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. I 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. 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. $\gamma_g \times \lambda_t$ nonparametrically absorbs all PUMA-specific trends that are invariant to eligiblity status. This final 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 a standard differences-in-differences analysis restricting the sample to eligibles alone. The identifying assumption is violated if in the absence of Internet Essentials, the eligibles-ineligible gap would have trended differently after 2012 in PUMAs with greater Comcast coverage rates. Put another way, the identifying assumption is that 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. I 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. These eligibility requirements give rise to three immediate versions of the control group: 1) all ineligibles, 2) ineligibles who have a school-aged child but do not meet the low-income threshold, and 3) low-income ineligibles who 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. A similar logic applies when restricting the control group to ineligibles with incomes below 185% of the FPL.

I choose to restrict the control group to low-income ineligibles in my primary specification. This comparison is useful because low-income eligibles and ineligibles may be more likely to access similar types of labor markets and job opportunities. Another important consideration is that Internet Essentials may affect earnings, which subsequently affects future program eligibility. Restricting the sample only to individuals beneath the income limit ensures that potentially endogenous income-based eligibility does not enter the identifying variation. In fact, if program enrollment increases an individual's income beyond the 185% FPL limit, then the program's positive impact on such individuals would not be captured and would mechanically attenuate the results.

I 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, low-income ineligibles can be further restricted to include only low-income individuals with children who are too young (or too old) for K-12. A natural extension is to 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 are less able to differentiate between the two (conditional on X_{igt}), and violations of the identifying assumption must occur through more specific channels.

I 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.[14] This motivates a simple placebo test in Section III.C testing whether broadband coverage rates of other large ISPs correlate with labor market differences between eligibles and ineligibles after 2012.

Second, I consider the possibility that after the launch of Internet Essentials, Comcast chose to expand in locations where labor market conditions were improving differentially for eligibles. This would imply that any expansion in coverage rates after 2012 was endogenous to local labor market conditions for eligibles. The threat of endogenously time-varying coverage rates motivates the decision to fix $Comcast_g$ to 2012 levels. I revisit this decision in robustness by assessing how the results change when allowing $Comcast_g$ to vary across time. Comcast could have also preemptively chosen to expand broadband networks in these locations prior to 2012, although such specific foresight seems both unlikely and inconsequential to its core expansion strategy.

Finally, Comcast coverage could simply correlate with other factors that led to differential trends between eligibles and ineligibles after 2012. The trajectory of post-recession expansion differed both geographically and across different segments of the population in ways that could be correlated with the availability of Internet Essentials. I take several approaches to account for specific channels that could produce spurious differential trends. Most directly, I control for eligibility-varying time trends based on census divisions, base-year labor market conditions, and urbanicity. The placebo test in Section III.C can also rule out violations related to correlates of broadband density. Finally, 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.

C. 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). I calculate the take-up rate by obtaining annual estimates of the number of households that Internet Essentials served.[15] 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.

I then translate the household estimates to individual estimates. I do this by multiplying the number of households by the average number of eligibles

[14] One example is Comcast Business, a subsidiary of Comcast which provides discounted, tailored broadband solutions for small businesses. However, most prominent ISPs likely provide competitively similar services.

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

per household (1.59). Column (2) provides the translated estimates for number of individuals served. Next, I estimate the potential market size of Internet Essentials—the total population of eligible individuals living in locations where Comcast was available. I 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 5.5 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 4.2%, which increased to 17.8% by 2015. The blended take-up rate across all four post-treatment years is 10.6%. I use this estimate as the link between the ITT and TOT estimates.

D. 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) (National Telecommunications and Information Administration, 2014). The FCC Form 477 continued tracking the data beginning in 2014 (Federal Communications Commission, 2017). 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 the outcomes data can only be identified at higher levels of geographies, such as counties, metros, PUMAs, and states. I aggregate the data to a specific level of geography by computing the percentage of that 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, I 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.

E. *Linking Internet Essentials to Eligibility and Outcomes*

I 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 I use is the Census PUMA, which is the lowest level of geography identified for all respondents in the ACS. PUMAs are re-drawn with each decennial Census, meaning that a location in a given PUMA in 2008 could be 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.[16] I use consistent PUMAs as the baseline geographic unit in the analysis. For brevity, I refer to IPUMS consistent PUMAs simply as PUMAs, unless stated otherwise.

The ACS also provides individual-level outcomes and measures of eligibility. For outcomes, I focus on employment, labor force participation, unemployment, and earnings. Employment reflects whether a respondent worked in the week prior to being surveyed. A respondent is unemployed if they do not currently have a job, are looking for a job, and have not found one yet. A respondent is in the labor force if they are either employed or unemployed. Income reflects the sum of the respondent's individual wage, business, and farm income earned over the past 12 months.[17] I 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, I combine information on reported family income and children to proxy for free/reduced-price lunch eligibility. I 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.[18]

Table 2 provides summary statistics on demographics and labor market outcomes as of 2011, the year prior to program launch. I calculate the summary

[16]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." In 2012, there were 2,378 PUMAs and 1,078 consistent PUMAs.

[17]I provide additional details on the construction of these outcome variables in Appendix B.

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

statistics separately for consistent PUMAs with greater and less than 50 percent Comcast coverage. I also show how these numbers differ across eligibles, ineligibles, and low-income ineligibles. Across all three groups, high- and low-Comcast PUMAs appear mostly similar, although High-Comcast PUMAs tend to have lower populations and slightly greater concentrations of minorities. The differences between eligibles, ineligibles, and low-income ineligibles appear more pronounced. Compared to eligibles, low-income ineligibles are more likely to be older, unmarried, and without children. They have similar levels of educational attainment and affluence (as a percentage of the FPL) but have lower overall attachment to the labor force. Ineligibles as a whole share demographic similarities with low-income ineligibles but are less likely to be minorities and are substantially more likely to be married. They are also more affluent and exhibit greater rates of labor force attachment.

The ACS also collects data on broadband use as of 2013, although these data are not informative about 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 specifically a home broadband subscription. Table 2 provides summary statistics for these variables. High-Comcast areas tend to have greater rates of internet and broadband use. Eligibles and low-income ineligibles have low rates of broadband use at 66 and 56 percent, respectively. In Table A2, I also use the 2013-15 broadband data to calculate how summary statistics vary based on broadband use. 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.

III. Results

A. *Graphical Evidence*

Figure 4 provides a preliminary visual depiction of the variation captured by triple differences. The graph plots trends in employment differences between eligibles and low-income ineligibles. I divide the data into two series: one representing high-Comcast PUMAs (at least 50 percent coverage) and one representing no-Comcast PUMAs (0 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 for illustration in this simplified figure. Employment is pre-residualized with respect to control variables in the vector X_{igt}.

In high-Comcast PUMAs, the employment gap between eligibles and ineligibles prior to Internet Essentials is approximately 3.1 percentage point and remains relatively constant. The trend begins an upward climb in 2012 and reaches 5.6 percentage points by 2015, an overall change which appears statistically significant.

This series represents the effect that would be captured by standard 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 other than Internet Essentials.

This motivates a third level of differencing using areas without Internet Essentials to remove the influence of common confounders. Indeed, PUMAs without Comcast experienced a slight but gradual increase in the eligible-ineligible gap after 2012, which could be due to any number of reasons that made labor market conditions differentially favorable to low-income individuals with school-aged children.[19] If the reasons underlying this increase also contributed to the increase observed in high-Comcast PUMAs, then failure to difference out these confounders would bias estimates from standard differences-in-differences upward.

While the two series are not parallel in the pre-treatment period, the difference in levels is consistently within half of a percentage point. Given that the pre-treatment period occurs during a turbulent transition following the Great Recession, the small pre-treatment differences provide a source of assurance in light of slightly unstable pre-trends (Kahn-Lang and Lang, 2019).

In 2012, the trajectory in high-Comcast PUMAs becomes clearly positive and quickly outpaces the trajectory in no-Comcast PUMAs, increasing from a -0.5 percentage point gap between the two series in 2011 and culminating in a +1.0 percentage point gap by 2015. The triple differences estimate captures the difference between the two series before and after 2012 in this simplified figure. The implied effect sizes are also consistent with the trajectory of the program's expansion. The graph plots household enrollment counts for each year using tabulations from Table 1. I also show in Section III.D 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.

B. *Main Results*

In Table 3, I present intent-to-treat estimates of Internet Essentials availability on the probability of employment, probability of labor force participation, probability of unemployment, and average income. Each estimate represents the effect of PUMA-wide program availability for eligible low-income individuals. For completeness, I present the estimates for each of the three basic control groups arising from the two-pronged eligibility requirement. Panel A presents the baseline triple differences estimate from Equation (1) using the full sample of ineligibles as the control group. Panel B restricts the control group to ineligibles with school-aged children. Panel C, my main specification, restricts the control group to low-income ineligibles. The baseline treatment group mean at the top of the table

[19] These reasons could include shifts in policies and norms surrounding child care and working parents, or even a general improvement in labor market accessibility for time- and resource-constrained parents (e.g., better technology, rise of gig work, etc.). Triple differences will difference out these confounders so long as they are common to locations irrespective of Comcast coverage rates.

displays the average of the dependent variable for the treatment group in 2011, prior to the launch of Internet Essentials.[20] I also provide control group-specific means of the dependent variable in each panel.

I find that PUMA-wide availability of Internet Essentials increased relative 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 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). The labor force estimate masks a great deal of variation in effect sizes that grew over time as the program continued to expand, which I shed light on in Section III.D. Next, I find that the probability of being unemployed decreases by 0.5 to 0.6 percentage points. The estimate is significant at the 5 percent level when the control group consists of all ineligibles or only ineligibles with school-aged children, and is significant at the 10 percent level when when restricting the control group to low-income ineligibles. Finally, income increases by approximately \$147 in the main specification. 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). I show in Section IV.C that program availability has no discernible impact on income conditional on being employed, suggesting that the extensive channel likely drives the estimate.

I calculate the treatment-on-the-treated effect of enrollment by dividing the main ITT estimates in Panel C by the average take-up rate of Internet Essentials from 2012 to 2015 (10.6%). For employment, the implied TOT effect size is approximately 8.1 percentage points (0.0086/0.106; 14.3%) with a lower confidence interval of 2.1 percentage points.[21] While the point estimate appears large, I argue that its magnitude lies within reasonable ranges. To contextualize this effect size, Hjort and Poulsen (2019) find that the arrival of fast internet in Africa increased the probability of employment by 4.6-7.7 percentage points (6.9-13.2%). This estimate reflects the effect of fast Internet *availability* and corresponds to my ITT estimate of 0.9 percentage point, though it coincides with both a 12% increase in daily internet use as well as an increase in firm entry and productivity. The TOT point estimate for Internet Essentials is also supported by the qualitative observation that 62 percent of surveyed enrollees indicated that the program had helped them or a family member find employment (Comcast Corporation, 2018). This may have been due to large differences in online job search between those with and without broadband (Online Appendix Table A1) that the program helped to bridge, given that as many as 90 percent of Internet Essentials customers are first-time broadband subscribers.

Using the ITT estimates from Panel C, the corresponding TOT estimate for

[20]In the calculation of treatment means, I 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.

[21]The lower confidence interval of the ITT estimate is 0.2 percentage points.

unemployment is -4.2 percentage points (-17%) and is \$1,385 (12.4%) for income. The TOT estimates on earnings can also be used for a rudimentary cost-benefit analysis. With an average of 1.59 eligible adults in each eligible household,[22] the typical benefit to a household is \$2,202. 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 household benefit of \$2,202 is more than four times the approximate \$500 program cost per household.

C. Placebo Test: Non-Comcast ISPs

One concern is that Comcast broadband coverage may be correlated with other determinants of economic development that favored eligibles during the post-recession recovery period. 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 spurious link between post-2012 Comcast coverage rates and better labor market outcomes for eligibles that are unrelated to Internet Essentials.

Many of 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.[23] I use this fact to construct a falsification test of the employment results by estimating whether local exposure to other large ISPs yields significant employment effects. I 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).[24] Because none of these ISPs widely subsidized broadband for low-income families during the sample time frame, exposure to these ISPs should not be associated with greater post-2012 employment outcomes for eligibles relative to low-income ineligibles. Online Appendix 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, it competes heavily with Verizon on the east coast, and with Charter and AT&T elsewhere in the United States.

I begin with the triple differences specification from Equation (1), again using the control group of low-income ineligibles. For the placebo test, I replace $Comcast_g$ with coverage rates of either Charter, AT&T, or Verizon. For example,

[22] I 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.

[23] 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.

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

the Verizon-based placebo test for labor outcomes would be:

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

Panel A in Table 4 presents the placebo test outcomes for employment. Only the coefficient on Comcast is significant; when Comcast exposure is substituted for exposure to Charter, AT&T, or Verizon, there is no effect on employment. Since the availability of certain ISPs could be correlated with one another, I provide results from a horserace regression in Column (5), which includes the triple interaction terms for all four ISPs in the same regression.[25] The estimate is only significant for Comcast, although the test cannot formally reject that the Comcast estimate is equal to the other three. Still, the results lend some assurance that the employment estimates are being driven by Internet Essentials, as opposed to characteristics of robust broadband markets. Results for labor force participation, unemployment, and income are provided in Online Appendix Table A5. The effects generally remain insignificant or opposite-signed for other ISPs, although income is significant for AT&T.

D. *Robustness Checks*

More restrictive control groups: I begin by testing how the results change when refining the control group of ineligibles to more closely resemble the treated group of eligibles. This approach mitigates certain observable and unobservable channels through which the gap between eligibles and ineligibles could diverge in high-Comcast PUMAs for reasons unrelated to Internet Essentials. Note that the more restrictive the control group, the more imprecise the data-intensive triple differences estimates will become.

The main specification compared eligibles against low-income ineligibles without school-aged children. I can also compare eligibles against low-income *parents* whose children are either too old or too young for K-12 schooling.[26] 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 such that the estimates are no longer significant ($\beta : -0.005, SE : 0.003$). Effects on income are qualitatively similar to the baseline estimates and remain significant at the 10 percent level ($\beta : 156.0, SE : 94.6$).

Next, I further restrict the control group to include only parents whose children are too young to attend school. Violations of the identifying assumption must

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

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

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. I first observe that the treatment and control means are 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 ($\beta : 0.011, SE : 0.007$) and labor force participation ($\beta : 0.014, SE : 0.007$) 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 imprecise as standard errors continue to increase ($\beta : 0.003, SE : 0.005$). For income, the point estimate only decreases slightly but the standard errors increase by nearly 50% ($\beta : 130.2, SE : 134.6$).

Finally, I restrict both eligibles and ineligibles to a sample of parents with children whose ages vary within a specific bandwidth about the age threshold for kindergarten. Specifically, I 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 essentially 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, I find that the effect on employment remains 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 the main specification, comes out clearly with each additional restriction. This implies that labor force participation among ineligible low-income *non-parents* may have increased during this time, which would bias the main labor force participation estimates downwards. The effect on income remains large and positive, but quickly loses precision as the sample shrinks.

Controlling for trends that may correlate with Internet Essentials availability: Figure 2 suggests that Comcast availability tends to be geographically clustered. Large pockets of availability appear on the west coast, the northeast corridor, and the south. It is therefore possible that the main effects are simply a product of time-varying geographic differences in the eligible-ineligible gap that are correlated with Comcast coverage. To this end, I test the robustness of the results after controlling for a census division linear trend which varies by eligibility. These results are presented in Panel B of Table 6. The estimates remain similar in direction and significance for all outcomes except the probability of being unemployed, which is still negative but no longer significant at conventional levels.

Beyond geographic trends, I also test the robustness of the results to controlling for a linear trend varying by a PUMA's base-year unemployment rate in 2009, as well as a linear trend varying by a PUMA-specific indicator for urbanicity.[27] These controls absorb trend differences between eligibles and ineligibles that vary by initial labor market conditions and population density. The results are presented in Panels C and D in Table 6. Both estimates closely mirror the direction and magnitude of the original estimates. Finally, Panel E presents estimates that include all three trend controls simultaneously. The results are slightly weaker, with only employment and income remaining significant at the 10 percent threshold or better. This is somewhat expected given that the simultaneous inclusion of these controls may be reducing the amount of identifying variation available for the data-intensive triple differences procedure.

Event study: One additional way to probe for potential violations of the identifying assumption is with an event study formulation of the triple differences regression in Equation (1). To do so, I 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 the 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 pre-treatment effects may raise concerns about parallel trends holding in the counterfactual. Second, the event study formulation sheds light on 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, which should produce treatment effect sizes that increase over time.

In Figure 5, I first confirm that none of the effect sizes in pre-treatment years are statistically significant. I also observe that the point estimates in post-treatment years generally increase over time. Online Appendix Table A7 provides the corresponding coefficients. The employment effect in 2012 appears to be quite small (β: 0.005), becomes large and statistically significant by 2014 (β : 0.015), and continues to grow through 2015 (β : 0.018). When combining each annual ITT estimate with its corresponding take-up rate in Table 1, I find that the implied TOT effects for each year are stable between 10 and 12 percentage points.[28] The upward trajectory of the treatment effect coinciding with the launch and subsequent expansion of the program provides additional assurance that the estimated effects are tied to variation in Internet Essentials.

[27]I define a PUMA as urban if at least 95 percent of its population lives within an urban cluster. 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". Roughly one-third of the sample lives in an urban PUMA, defined as having at least 95 percent of its population living in an urban cluster.

[28]These implied TOT effects are slightly larger than the baseline TOT estimate of 8.1 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.

I find that the effects on labor force participation also become large and significant in later years, rising to a statistically significant effect size of 0.15 percentage point by 2014. One additional reason why I 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 I do not detect 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 demanding on the data. Lastly, I confirm that the effect on income is positive in all post-treatment years and significant in 2014 and 2015. However, the estimate is significant in 2009, and there is a downward trajectory in the pre-treatment estimates leading up to 2012. Online Appendix Figure A3 shows that these issues are partially alleviated when estimating the event study using the more restrictive control group of low-income ineligibles with children, suggesting that the pre-treatment effect I observe is partially driven by temporary differences between low-income parents and non-parents immediately following the recession.

Alternative Income Limits: As previously stated, one 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.[29] A related concern is that the ACS measure of poverty does not precisely identify respondents whose children are eligible for the NSLP. To address this, I vary the required income eligibility threshold across 185% (the reduced-price lunch threshold), 130% (the free lunch threshold), 250%, and 300% FPL. I present these results for employment in Panel A of Table 7. 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 due to 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. I present employment results for these alternative geographic aggregations in Panel B of Table 7. All regressions are conducted after recalculating coverage rates via Equation (2) and

[29]Under 2019 guidelines, 185 percent of the federal poverty level for a family of three corresponds to an annual income of $39,460.50.

re-running Equation (1) at the appropriate level of geographic aggregation. The point estimates for counties are still large, but are more imprecisely estimated ($\beta : 0.010, SE : 0.007$). I find similar results when aggregating to the metro level. I also run the analysis using states as the unit of geographic aggregation, and similarly find that the point estimates remain large but imprecisely estimated.

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

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

where $ShareEligible_g$ represents the share of the population in PUMA g that is eligible and $(Comcast_g \times ShareEligible_g)$ represents the PUMA's overall exposure to Internet Essentials. X_{gt} is the same vector of individual covariates as in Equation (1), but aggregated at the PUMA level. As written, the coefficient ρ represents the effect of launching Internet Essentials in a PUMA where the entire population is eligible and lives within Comcast's service area. To align the estimate's interpretation with that of my main estimates, I scale the estimate by dividing the endogenous differences-in-differences variable by the sample mean of fraction eligible. The resulting coefficient reflects the effect of launching Internet Essentials in a PUMA with full Comcast coverage and an average share of eligibles. I provide the results of this specification in Column (1) of Panel C in Table 7. The results are half as large but become substantially more precise ($\beta : 0.004, SE : 0.001$).

Time-varying Comcast coverage: In Equation (1), I fixed $Comcast_g$ to 2012 coverage rates. This was to mitigate the possibility that changes in Comcast coverage rates over time are endogenous to local labor market conditions. Strict regulatory conditions make it challenging for large broadband network expansions to occur independent of mergers and acquisitions. Therefore, year-over-year changes in Comcast coverage rates tend to be small. In Online Appendix Table A6, I 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. Even so, I show in Column (2) of Panel C in Table 7 that the effect remains positive and significant when allowing Comcast coverage rates to vary over time ($\beta : 0.008, SE : 0.003$).

Discretizing Comcast availability: The interpretation of ρ in Equation (1) is the effect of PUMA-wide Internet Essentials availability. Figure 3 shows that many PUMAs only have partial Comcast coverage. I can test whether my findings hold when restricting the sample to observations 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. Specifically, I include only PUMAs with greater than 90 percent coverage or 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 respondents in the restricted sample either live in a PUMA with no Comcast coverage or near-universal coverage; ρ can then be interpreted as the effect of *individual*-level availability. Column (3) of Panel C in Table 7 shows that the effect remains large and significant ($\beta : 0.013, SE : 0.004$).

Including 2016 and 2017 data: I excluded post-2015 data due to the fact that 2016 marked the rise of other large-scale broadband subsidy programs, as well as an expansion of IE eligibility to individuals on other forms of public assistance. Inclusion of data after 2015 may bias the 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 is significant at the 1 percent level after including 2016 and 2017 ($\beta : 0.009, SE : 0.003$). Note that inclusion of later years also likely coincides with further increases in smartphone usage. While this trend could theoretically mitigate the impact of subsidizing broadband, I find that the effect size remains nearly equivalent.

Control-Driven Effects: Conceptually, the DDD estimator is the difference in DD estimates, run separately for the treatment group (eligibles) and control group (low-income 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 Online Appendix Table A8, I present differences-in-differences estimates run separately on the treatment and control group. Panel A shows that the treatment group experienced large effects on employment (β: 0.013, SE: 0.003), probability of being unemployed, and income. Panel B shows that the control group also experienced changes, but the changes are comparatively small. The result verifies that the triple differences estimates were driven by positive effects in the treatment group, as opposed to negative effects in the control group. This also suggests 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. As a final check, I estimate differences-in-differences using the sample of low-income ineligibles with children. Panel C shows that these estimates are approximately zero. Because both the original and restricted group of ineligibles should have been similarly impacted by spillovers or general equilibrium effects, the difference between the two estimates suggests that such effects are likely not the main factors driving the positive estimate in Panel B.

Alternative Income Specifications: The main estimates for income rely on an untransformed measure of income in levels. I test the robustness of these specific estimates to alternative transformations, which are provided in Online Appendix Table A9. Column (1) provides the baseline result. Columns (2) and (3) show that the ln(Income+1) and inverse hyperbolic sine transformations have little discernible effect on the significance of the estimates. Finally, column (4) shows the estimate when using the natural log transformation and bottom coding income to the 5th percentile of non-zero values. This estimate is lower but still significant, which likely arises from the reduced impact of earnings gains along the external margin of employment.

Age Limits: Finally, Internet Essentials may not have an effect on respondents who are of retirement age. At the same time, roughly one-third of respondents at the full retirement age of 66 are still in the labor force, so the program could potentially have a non-trivial effect on elderly eligibles relative to elderly ineligibles. As a final robustness check, I restrict the sample to respondents 66 and under. The results are presented in Column (5) of Table 7 and remain similarly positive and significant.

IV. Mechanisms

A. *Effects on Internet Use and the Broadband Gap*

I also assess the effect of Internet Essentials on internet adoption. I use a variation of the 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, I 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 with respondents who used computers, whereas other years do not have this restriction. The inclusion of year fixed effects will only partially mitigate this issue. I 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. I use the upper bound of each interval, which underestimates the number of respondents who are income-eligible for Internet Essentials. I then map family income and family size to federal poverty tables to determine each family's eligibility for reduced-price lunch (Office of the Assistant

Secretary for Planning and Evaluation, 2017).[30]

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 the sample, only 42 percent of respondents live in identifiable counties. I 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). I 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 these concerns, I rely on basic differences-in-differences to estimate the effect of Internet Essentials on internet use. I restrict the sample to eligibles to ensure that the estimate is not diluted by respondents who cannot enroll in Internet Essentials. The estimating equation is:

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

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. I also include a vector of metro/state-level covariates in Z_{gt}, including population and unemployment rates. The identifying assumption is now the standard difference-in-differences parallel trends assumption: in the absence of Internet Essentials, internet usage trends among eligibles would have evolved equally in high-Comcast versus low Comcast metros, conditional on covariates X_{igt} and Z_{gt}.

Online Appendix Table A10 presents the effects of geography-wide Internet Essentials exposure 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 among eligibles. 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 7.8 percentage point increase in internet use, which falls to 6.7 percentage points using the less restrictive income threshold. Both state-level estimates are significant at the 1 percent level. I also re-run the placebo test from Equation (3) using the differences-in-differences regression in Equation (5) with state-level ISP coverage rates. Panel B of Table 4 verifies that the increase in internet use is only significant for Comcast exposure.

How substantial are these effects within the broader picture of the digital divide? I first use the ACS to regress broadband adoption rates on an indicator for

[30]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.

individuals with family income less than 185% of the federal poverty limit. Such individuals are 20.6 percentage points less likely to have in-home broadband than their non-poor counterparts.[31] The estimates indicate that state-wide availability of Internet Essentials induced broadband take-up rates to increase by up to 7.8 percentage points, which would narrow the digital divide by nearly 40 percent in locations where it was available. These findings are broadly consistent with results from Carare et al. (2015), who estimate that a 10% increase in subscribers would require a price reduction of 15%; here, Internet Essentials offers an effective 66% to 75% discount relative to non-promotional prices.

B. *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. Broadband infrastructure is less developed in less urbanized areas (Ziliak, 2019) and marketplaces for online job postings may be less "thick", which could diminish the impact of subsidizing broadband. At the same time, broadband subsidies could be more potent in non-urban areas if broadband use is low and has more room to grow. Employment effects may also vary based on differences in the availability of job openings in urban versus non-urban areas.

In Online Appendix Table A11, I present triple difference employment effects estimated separately for urban and non-urban PUMAs. I 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. Employment estimates appear greater in urban PUMAs, although the difference is only significant for one of the three urbanicity measures. While differences for other outcomes are generally not significant, point estimates for labor force participation are also consistently larger in magnitude for urban PUMAs than for non-urban PUMAs. Overall, the findings suggest that the labor market effects of Internet Essentials were somewhat larger in urban versus non-urban PUMAs. Even so, these results cannot ultimately shed light on differences in employment elasticities versus differences in take-up rates.

Differences by Demographic Characteristics: In Online Appendix Table A12, I 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). The results indicate that there are no 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

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

not statistically significant, even at the 10 percent level ($p = 0.14$).

C. *Job Characteristics*

Subsidizing broadband access could also potentially improve job offer quality by 1) increasing the choice set of job postings (Stevenson, 2009), 2) improving match quality between job seekers and firms (Bhuller, Kostol and Vigtel, 2019), or 3) increasing skill accumulation in online- and computer-based tasks. At the same time, decreasing job application frictions could induce individuals with lesser prospects to enter the labor market, which could lead to offsetting effects on job quality. 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.

I use the ACS data to analyze the effects of Internet Essentials on three basic job characteristics: part-time status, income, and transit time. Online Appendix Table A13 shows the effects of Internet Essentials availability on these three outcomes, conditional on being employed. I do not find convincing evidence that Internet Essentials led to changes in any of these outcomes.[32]

These results differ slightly in narrative compared to recent work by Bhuller, Kostol and Vigtel (2019), who find 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 could be driven by two factors. First, individuals induced by Internet Essentials to join the labor force could favor jobs that are close by given the many constraints that they already face when searching for jobs. 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 frequency. The setting I study abstracts from broadband adoption in firms, removing a potential mechanism that could favor longer commute times.

V. 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. I investigate 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. The results indicate that PUMA-wide availability of

[32] I note that the effect on income is slightly different than in Table Table A9. This estimate is conditional on being employed whereas the estimate in Table A9 is conditional on having positive income.

Internet Essentials 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, I calculate that enrollees were 8.1 percentage points (14.3%) more likely to be employed. The effects appear driven both by increases in labor force participation and decreases in the probability of unemployment. The findings also suggest that Internet Essentials was responsible for narrowing the income-broadband gap by as much as 40 percent. A back-of-the-envelope cost-benefit calculation suggests that the value to consumers (in terms of increased earnings) is four times that of the typical 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 an increasingly centralizing force in the digital era. Those who cannot afford monthly broadband subscriptions are inherently restricted when navigating the modern labor market. These individuals 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.

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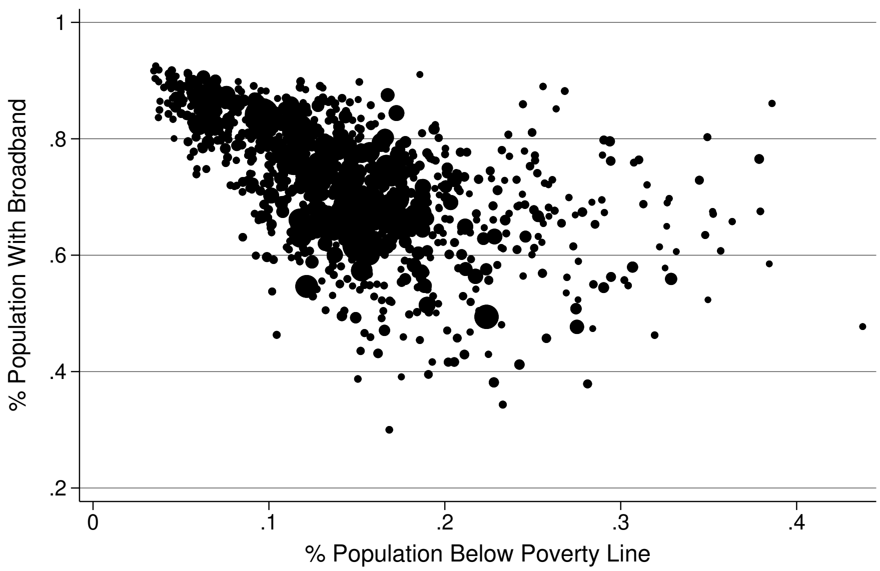

Figure 1. PUMA-Level Poverty and Broadband Adoption Rates

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.

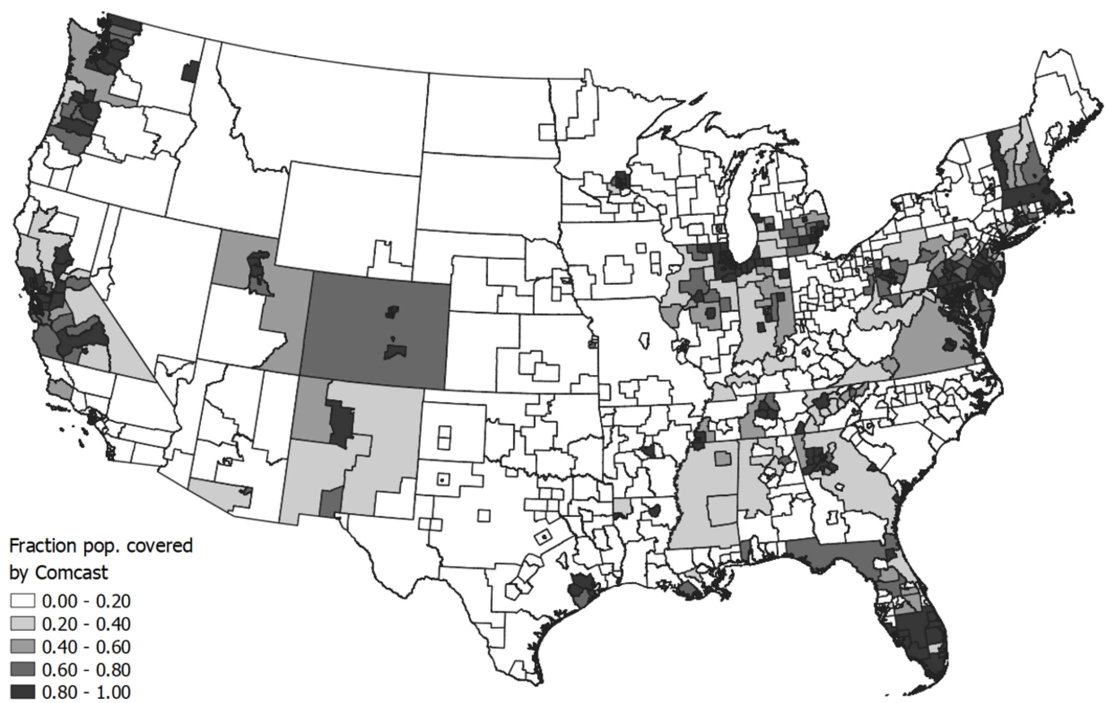

Figure 2. National Comcast Coverage, by PUMA

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.

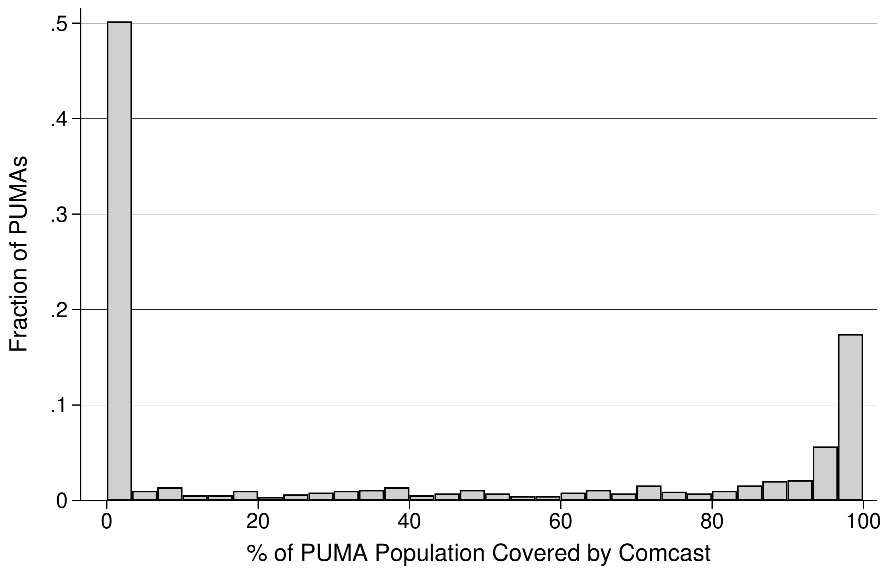

Figure 3. PUMA-Level Comcast Coverage Rates

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.

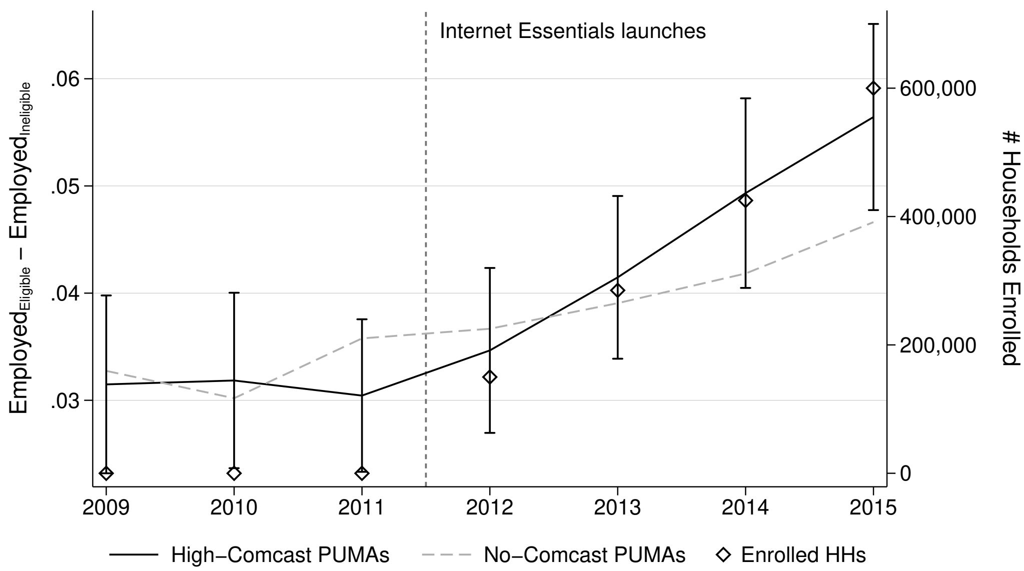

Figure 4. Visualizing Triple Differences and the Employment Effects of Internet Essentials

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 a no-Comcast PUMA (0% 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, and number of children. 95% confidence intervals are plotted for the high-Comcast series only. 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 II.D and II.E. Subscriber counts mirror those presented in Table 1.

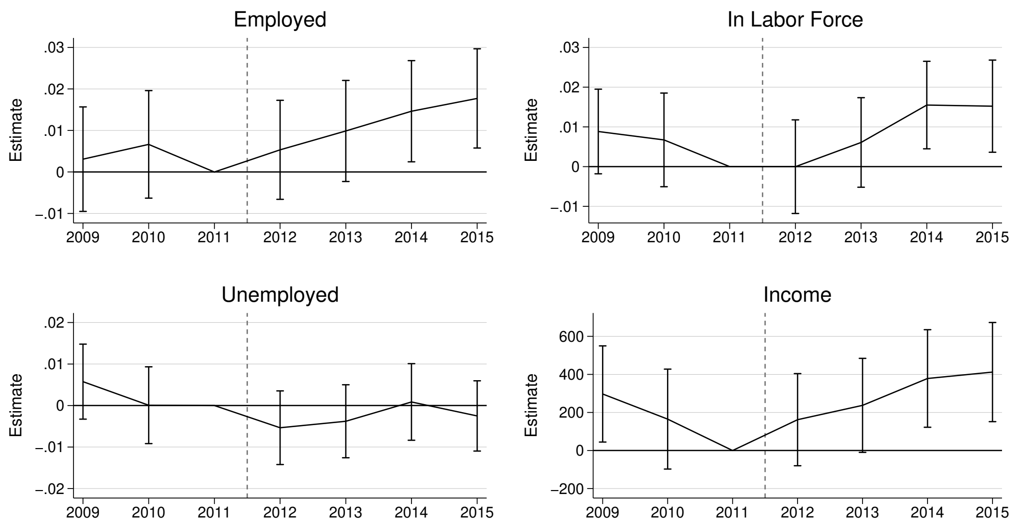

FIGURE 5. EVENT STUDY ESTIMATES

Note: This figure plots time-varying labor market 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 ineligibles. 95% confidence intervals are provided for non-omitted years. Estimates are provided in Table A7. All estimates are weighted using ACS person weights, and standard errors are clustered at the PUMA level.

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	237,842	5,721,983	4.2%
2013	285,000	451,900	5,678,363	8.0%
2014	425,000	673,886	5,478,239	12.3%
2015	600,000	951,368	5,354,097	17.8%

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 number of eligible individuals per eligible household (1.59). 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 10.6%.

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

	Eligible for IE		All Ineligibles		Low-Income Ineligibles	
Variable	High Comcast	Low Comcast	High Comcast	Low Comcast	High Comcast	Low Comcast
Population	21,649	29,355	313,649	360,806	75,390	98,719
% Male	0.37	0.38	0.49	0.49	0.46	0.46
Age	38.26	37.89	46.74	47.33	44.26	45.11
% Black	0.21	0.15	0.13	0.10	0.20	0.16
% Hispanic	0.35	0.31	0.13	0.13	0.19	0.17
% Married	0.56	0.56	0.50	0.52	0.21	0.24
Years of Education	11.69	11.68	13.49	13.12	12.05	11.81
Number of Children	2.51	2.50	0.57	0.55	0.23	0.24
HH Income (% FPL)	100.1	100.4	330.0	310.7	92.9	94.7
Employed	0.57	0.58	0.61	0.59	0.36	0.35
In Labor Force	0.71	0.70	0.68	0.65	0.48	0.46
Unemployed	0.14	0.12	0.06	0.06	0.12	0.11
Earnings	11,257	11,310	27,480	24,103	4,718	4,644
% Internet (2013)	0.73	0.69	0.81	0.77	0.62	0.57
% Broadband (2013)	0.66	0.59	0.76	0.69	0.56	0.50
Number of PUMAs	404	674	404	674	404	674

Note: This table provides summary statistics aggregated to the IPUMS consistent-PUMA level and presented separately for eligibles, ineligibles, and low-income ineligibles (i.e. ineligibles with incomes $\leq$185% of the FPL). "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. 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	Income ($/Yr)
Treatment group mean (2011)	0.567	0.710	0.143	11,173
A. Control Group: All Ineligibles				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.003	-0.006**	96
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(111)
N	16,557,536	16,557,536	16,557,536	16,557,536
Adjusted R^2	0.240	0.285	0.035	0.327
Control group mean	0.601	0.662	0.061	25,458
B. Control Group: Ineligibles w/ Child in K-12				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.003	-0.006**	146
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(145)
N	3,453,660	3,453,660	3,453,660	3,453,660
Adjusted R^2	0.152	0.136	0.043	0.439
Control group mean	0.843	0.878	0.035	45,700
C. Control Group: Ineligibles w/ Low Income				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.004	-0.005*	147**
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(71)
N	4,656,835	4,656,835	4,656,835	4,656,835
Adjusted R^2	0.160	0.222	0.038	0.201
Control group mean	0.352	0.464	0.112	4,668

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	0.078***				0.072***
	(0.016)				(0.014)
Charter × Post		-0.030			0.024
		(0.041)			(0.036)
AT&T × Post			-0.053***		-0.053**
			(0.022)		(0.024)
Verizon × Post				0.026	-0.015
				(0.018)	(0.022)
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	Income
A. Control Group: All Low-Income Ineligibles				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.004	-0.005*	147**
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(71)
N	4,656,835	4,656,835	4,656,835	4,656,835
Treatment group mean (Eligibles)	0.567	0.710	0.143	11,173
Control group mean (Non-Eligibles)	0.352	0.464	0.112	4,668
B. Control Group: Low-Income Ineligibles w/ Children				
(% Comcast Coverage)×(Year≥2012)	0.014***	0.009*	-0.005	156*
×(IE-Eligible)	(0.005)	(0.005)	(0.003)	(95)
N	1,605,932	1,605,932	1,605,932	1,605,932
Treatment group mean	0.567	0.710	0.143	11,173
Control group mean	0.413	0.528	0.114	6,768
C. Control Group: Low-Income Ineligibles w/ Young Children				
(% Comcast Coverage)×(Year≥2012)	0.011*	0.014**	0.003	133
×(IE-Eligible)	(0.007)	(0.007)	(0.005)	(131)
N	1,251,467	1,251,467	1,251,467	1,251,467
Treatment group mean	0.567	0.710	0.143	11,173
Control group mean	0.543	0.695	0.152	8,920
D. Sample: All Low-Income Parents w/ Children Aged 2-11				
(% Comcast Coverage)×(Year≥2012)	0.015*	0.018**	0.003	135
×(IE-Eligible)	(0.009)	(0.008)	(0.007)	(163)
N	554,555	554,555	554,555	554,555
Treatment group mean	0.563	0.710	0.146	10,914
Control group mean	0.551	0.701	0.151	9,379

Note: This table is an extension of Panel C in 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—Sensitivity to Controls for Confounding Trends

	Employed	In Labor Force	Unemployed	Income
A. Original Results				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.004	-0.005*	147**
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(71)
B. Control: Census Division × Eligible × Trend				
(% Comcast Coverage)×(Year≥2012)	0.007**	0.005	-0.003	169**
×(IE-Eligible)	(0.003)	(0.003)	(0.003)	(71)
C. Control: Base Unemployment × Eligible × Trend				
(% Comcast Coverage)×(Year≥2012)	0.009***	0.004	-0.005*	147**
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(71)
D. Control: Urbanicity × Eligible × Trend				
(% Comcast Coverage)×(Year≥2012)	0.008**	0.004	-0.004*	168**
×(IE-Eligible)	(0.003)	(0.003)	(0.002)	(72)
E. All Controls				
(% Comcast Coverage)×(Year≥2012)	0.007*	0.004	-0.002	184**
×(IE-Eligible)	(0.003)	(0.003)	(0.003)	(72)
N	4,656,835	4,656,835	4,656,835	4,656,835
Treatment Mean	0.567	0.710	0.143	11,173

Note: This table presents an extension of the main results, controlling for a variety of trends that may correlate with Internet Essentials availability. Panel A provides the original results from Panel C in Table 3. Panel B controls for geographic trends based on census division. Panel C controls for trends by initial labor market conditions, defined as the unemployment rate in the base year 2009. Panel D controls for trends by urbanicity, defined based on whether 95% of the population lives in an urban cluster. Panel E includes all three sets of controls simultaneously. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 7—SENSITIVITY AND ROBUSTNESS OF TRIPLE DIFFERENCES EMPLOYMENT ESTIMATES

	(1)	(2)	(3)	(4)	(5)
A. Sensitivity to Income Eligibility Threshold					
DDD Estimate	0.009***	0.010***	0.005**	0.003	
	(0.003)	(0.004)	(0.003)	(0.003)	
N	4,656,835	3,111,571	6,443,309	7,738,828	
Income Eligibility Threshold:	185% FPL	130% 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	
Level of Geographic Aggregation:	PUMA	County	CBSA	State	
C. Alternative Specifications and Data Assumptions					
DDD (or DD) Estimate	0.004***	0.008**	0.013***	0.009***	0.007**
	(0.001)	(0.003)	(0.004)	(0.003)	(0.003)
N	7,546	4,656,835	2,819,637	5,935,298	3,778,967
Specification	DD	DDD	DDD	DDD	DDD
Modification	PUMA DD	$Comcast_{gt}$	Discretization	Add 2016/17	Age 66 Limit

Note: This table presents several sensitivity and robustness checks described in the latter half of Section III.D. The baseline regression is the triple differences specification in Equation (1) and implemented in Table 3. In Panel A, I allow for the low-income eligibility threshold to vary from its original value of 185%. In Panel B, I change the level of geographic aggregation from PUMAs to counties, metros, and states. In Panel C, I provide four additional tests. Column (1) provides an aggregated, PUMA-level differences-in-differences regression, estimated using Equation (4). 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. Column (5) restricts the sample to individuals 66 and under. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.