The Digital Divide and Refinancing Inequality

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Abstract

Low-income households often fail to refinance their mortgages when interest rates fall. Using granular spatial variation from a broadband subsidy program, I find that access to subsidized internet increases refinance originations by 8 percent. For the average low-income household that refinances, the effects imply a 5 percent increase in disposable income and lifetime savings equal to roughly 10 percent of net worth. The effects are strongest in areas with limited bank branch access and among less-educated households, consistent with broadband reducing transaction and information frictions.

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1 Introduction

The home is the largest asset on the balance sheet of U.S. households; however, many Americans fail to refinance their mortgages when interest rates fall due to frictions such as search costs and limited financial sophistication (Campbell, 2006; Keys et al., 2016). This failure is particularly pronounced among low-income households earning about $40,000 per year or less, nearly half of whom are homeowners. At this scale, the refinancing disparity weakens monetary policy transmission and can exacerbate wealth inequality. This paper asks whether access to modern information and communications technology can mitigate mortgage refinancing frictions. I show that access to subsidized broadband increases refinance originations and reduces mortgage costs for low-income borrowers.

While ubiquitous in the modern era, high-speed internet is still inaccessible to millions of American households living without a broadband connection at home. The persistent gap in access to information technology, known as the "digital divide," has become a prominent policy issue in recent decades due to its overall impact on household well-being (White House, 2024). In 2024, nearly a quarter of U.S. households lacked a wireline broadband subscription, with low-income households reporting significantly lower subscription rates (Figure 1). This trend cannot be fully explained by disparities in physical access to a broadband provider: in urban areas where physical coverage is nearly universal, roughly one in five low-income households still lack a home subscription due to high costs. The relevant margin is therefore reliable access within the home, not whether a household can get online at all.

This margin has become more important as financial services move online, and is especially relevant for mortgages, which are high-stakes transactions that require repeated interactions and extensive documentation. Using the internet, an applicant can easily exchange paperwork by e-mail, link financial accounts online to expedite credit verification, and spend less time meeting with a loan officer or visiting a bank branch. Indeed, processing times for mortgage applications at online lenders are estimated to be 15 to 30 percent shorter than at their physical counterparts, with a larger effect for refinance loans (Fuster et al., 2019). To the extent that the internet can provide information on the value of refinancing and connect people through social media, it can also reduce missed refinancing opportunities driven by inattention or limited information. Despite these advantages, broadband may lower the cost of completing a refinance without changing whether households pursue one, making its effect on refinancing an empirical question.

Studying the effects of internet access on refinancing inequality is challenging for several reasons. First, the location choice of broadband internet providers is correlated with subscriber characteristics such as employment and educational attainment. As these characteristics are also linked to refinancing demand, estimates of refinancing outcomes driven by differences in broadband availability will most likely be biased. Second, it is difficult to directly observe exogenous changes in broadband adoption by households, especially for low-income homeowners that exhibit the lowest refinance propensities. As a result, little is known about the extent to which broadband access can reduce refinancing frictions.

To address these empirical challenges, I analyze the Internet Essentials program by Comcast, one of the largest broadband providers in the U.S. Introduced in 2012 to receive regulatory support for a merger, Internet Essentials heavily subsidized broadband subscription fees to qualifying low-income households. The monthly cost of $9.95 was 75 percent lower than that of a comparable regular plan, and all activation fees and recurring equipment charges were waived. The program became highly successful, connecting 750,000 American families (or 3 million individuals) nationwide in the first five years (Comcast Corporation, 2016). Internet Essentials is a suitable setting to study refinancing behavior due to its unique properties. First, it was immediately available in all of Comcast's existing service areas. This method of rollout is important for identification because physical infrastructure expansions not only take time but also can increase local house prices, confounding the estimated impact of broadband access on refinancing (Wall Street Journal, 2015). Second, Internet Essentials was directly aimed at increasing broadband take-up by low-income households making less than around $40,000 per year — the group that fails to refinance most prominently. Third, refinancing requires repeated interactions with lenders and extensive information gathering, activities for which broadband access is especially valuable. Lastly, the program coincides with the prolonged low-interest rate period following the Great Recession, during which refinancing incentives were high.

This paper exploits geographic, temporal, and program eligibility variation to estimate the effect of broadband access on refinancing frictions. Specifically, I compare the outcomes of low-income households that were eligible and ineligible for the program, across census tracts with and without Comcast service, before and after 2012. The identifying assumption is that, absent Internet Essentials, the change in the refinancing gap between the two income groups would have evolved similarly across areas with different Comcast coverage. Indeed, I find no evidence of differential pretrends in this setting. I construct a unique data set that matches Comcast availability at the census tract level to the universe of refinance originations by eligibility group between 2009 and 2015. I also enhance my analysis using prepayment outcomes as well as Census survey microdata on mortgage cost burdens.

I find that the availability of subsidized broadband increases refinance originations by 8 percent among low-income households. The estimate remains similar after absorbing metropolitan area-specific changes in the refinancing gap. Household financial gains arise through an increased likelihood of refinancing rather than lower interest rates conditional on origination. Survey data provide complementary evidence that Internet Essentials reduces mortgage payment burdens by 1.3 to 1.7 percentage points for eligible homeowners. The effects are largest where transaction and information frictions are likely to be most consequential: refinance originations rise most in census tracts with limited bank branch access, while payment burdens fall most among households with lower educational attainment. I find no evidence that these gains spilled over to ineligible households living nearby, consistent with broadband easing each household's own convenience and awareness frictions rather than diffusing information through the community.

The economic magnitudes of these results are significant: the average low-income household that refinanced its mortgage between 2012 and 2015 would have saved up to $100 per month on mortgage payments even after accounting for the nominal cost of subscribing to Internet Essentials. This translates to a 5 percent increase in monthly disposable income and total household wealth gains of up to $18,000 in present value terms, equal to about 10 percent of the average net worth of homeowners in this income bracket. The prepayment estimate is equivalent to approximately 18 percent of the baseline eligible-ineligible prepayment gap.

These empirical findings are robust to several validation and falsification tests. First, I trace the originations effect from the unconditional group means through the full specification with granular fixed effects and controls. I further show that the estimates remain positive and statistically significant with geography-specific trends and in broader samples. Second, I verify that the results hold when using mortgage prepayment as an alternative measure of refinancing. Third, I show that the result is not driven by concurrent federal housing relief programs aimed at underwater borrowers. Fourth, I assign placebo treatment indicators for AT&T and Charter coverage and find no effects on refinancing.

This paper is principally related to the literature on frictions and inequality in household finance (Campbell, 2006). In the mortgage market, these frictions are particularly consequential because the money at stake is large. Most prominently, disadvantaged households participate less in mortgage refinancing when interest rates fall (Agarwal et al., 2013; Keys et al., 2016; Johnson et al., 2019; Andersen et al., 2020; Agarwal et al., 2024a; Gerardi et al., 2023).[1] Several papers identify specific behavioral channels of refinancing inequality, such as financial illiteracy (Agarwal et al., 2017), inattention (Andersen et al., 2020; Berger et al., 2025), distrust of financial institutions (Johnson et al., 2019; Yang, 2025), and peer effects (Maturana and Nickerson, 2019), while another strand

[1]See Goodstein (2014), Agarwal et al. (2016), and DeFusco and Mondragon (2020), who study refinancing trends among homeowners with lower incomes and minority homeowners, the sources of borrower refinancing mistakes, and employment constraints on refinancing during recessions. Bajo and Barbi (2018) document financial illiteracy and McCartney and Shah (2022) peer effects, while Coen et al. (2023) and Tian and Zheng (2025) study additional sources of search frictions and policy-induced lender market power. Badarinza et al. (2016) survey household finance across countries.

documents search frictions from lender market power in mortgage and consumer credit markets (Woodward and Hall, 2012; Allen et al., 2014; Gurun et al., 2016; Agarwal et al., 2024b; Argyle et al., 2023). Consistent with these channels, Bhutta et al. (2026) show that mortgage knowledge and shopping behavior predict substantial overpayment on otherwise comparable loans. Whereas this literature documents the frictions, I show that subsidized broadband measurably relaxes them, narrowing the baseline eligible-ineligible prepayment gap by approximately 18 percent.

This paper also contributes to a literature that models the distributional consequences of refinancing frictions. In equilibrium models of the mortgage market, borrowers who rarely refinance cross-subsidize those who refinance often, and reforms that change refinancing behavior are priced into the rates every borrower faces (e.g., Berger et al., 2024; Fisher et al., 2024).[2] Related work studies alternative mortgage contract designs (Campbell, 2013; Allen and Li, 2025). Amromin et al. (2020) review how refinancing frictions weaken monetary policy transmission, while Beraja et al. (2019) document its geographic heterogeneity. My estimates show that a scalable technology intervention can reach low-income households whose slow refinancing generates regressive cross-subsidies, without redesigning the mortgage contract itself.

The literature on the role of financial technology ("fintech") institutions has highlighted their large impact on mortgage market composition and consumer lending practices (Buchak et al., 2018; Fuster et al., 2022; Bartlett et al., 2022; Di Maggio and Ratnadiwakara, 2026). This paper complements Fuster et al. (2019), who document fintech lenders' shorter mortgage processing times but find no effect of broadband access on outcomes using the Google Fiber rollout as an instrument. Studying a national program without large upfront costs for low-income customers, I find that broadband access does reduce refinancing frictions for marginal households. On the supply side, D'Andrea et al. (2026) show that broadband adoption by bank branches changes credit supply, whereas I study the demand side among low-income borrowers. Related work highlights the continued importance of bank branches (Brown et al., 2019; Célerier and Matray, 2019; Nguyen, 2019; Fonseca and Matray, 2024), the capacity of digital lenders to broaden financial access (Erel and Liebersohn, 2022; Jiang et al., 2026), household income as a determinant of financial participation (Yogo et al., 2025), and racial disparities in credit access (Bhutta et al., 2025; Howell et al., 2024; Frame et al., 2025). Because internet access shapes the applicant pool itself, the household margin I document is a precondition for the documented gains on the lender side, and one that fintech adoption alone cannot reach for unconnected households.

Lastly, this paper adds to the literature on the economic importance of broadband internet — a

[2] Zhang (2024) and Piskorski and Seru (2018) examine closing cost distortions and mortgage market design. A recent literature studies the opposite regime, in which rising rates lock households into existing low rate mortgages (Liebersohn and Rothstein, 2025; Fonseca and Liu, 2024).

key infrastructure in the modern era. Prior work shows that broadband affects labor market outcomes (Dettling, 2017; Akerman et al., 2015; Hjort and Poulsen, 2019) and household education choices (Dettling et al., 2018). Most closely related, Zuo (2021) uses Internet Essentials to document positive employment effects. I extend this literature to mortgage refinancing, using granular geographic variation to study whether broadband reduces costly household financial mistakes. Related work shows that the remaining digital divide increasingly reflects usage and skills gaps (Pereira et al., 2024) and that broadband reduces credit search frictions between small firms and banks (Mazet-Sonilhac, 2025). A related strand studies broadband and financial market participation (Hvide et al., 2024; Gerken and Qiu, 2024). Refinancing operates on the asset low-income households already own rather than one they must be persuaded to buy. For the bottom of the income distribution, where housing is most of net worth and stock market participation is rare, a one-time transaction that permanently lowers the cost of holding that asset is the wealth channel with the widest reach.

The remainder of the paper is structured as follows. Section 2 describes the digital divide and the Internet Essentials program. Section 3 outlines the data and sample construction. Section 4 presents the main results on broadband access and refinancing activity. Section 5 provides robustness and falsification tests. Section 6 analyzes the mechanisms behind these results, including convenience, awareness, and refinancing frictions. Section 7 concludes.

2 Mortgage Refinancing and the Internet Essentials Program

2.1 Mortgage Refinancing Inequality

Households use mortgages to purchase a new property or refinance an existing mortgage on a previously purchased property. Since most mortgages in the U.S. are fixed-rate loans without prepayment penalties, a refinance allows households to reduce their cost of credit when interest rates fall. In essence, the refinance decision is a call option that should be exercised when the original loan is “in the money” after adjusting for interest rate differentials and closing costs. Refinancing constitutes a large segment of residential real estate markets, accounting for more than half of all mortgage originations by volume between 2005 and 2015 (Haughwout et al., 2021).

Homeownership is the primary source of wealth creation among American families, with about 65 percent of the population residing in owner-occupied units as of 2019. Understanding what drives households to refinance their mortgage is important in light of the weight placed on homeownership in their portfolios, representing between 30 and 40 percent of household net worth (Current Population Reports, 2019). As such, refinancing to lower mortgage payments is one of the most consequential decisions a household makes throughout its lifetime. Housing is particularly important for low-income households, whose homes account for over 80 percent of their total wealth. I first document the prevalence of homeownership among low-income households. Homeownership remains common at the bottom of the income distribution: roughly 44 percent of households earning under $35,000 in my urban sample live in owner-occupied units (Table 1). This group's contribution to the housing market is not trivial: households earning less than $35,000 obtained roughly half a trillion dollars in purchase mortgages between 2001 and 2008, with an average loan amount of about $120,000 and monthly payments near $700 over 30 years. Housing cost burdens are also widespread in these urban tracts, where more than 40 percent of homeowners spend at least 30 percent of income on housing. Reducing mortgage payments through refinancing, therefore, is an important way to increase household net worth in the long run.

Prior research has documented that many households fail to refinance their mortgages when it is optimal to do so (Agarwal et al., 2016; Keys et al., 2016; Johnson et al., 2019; Andersen et al., 2020; Agarwal et al., 2024a). The gap is especially large among low-income households. Of the mortgages originated between 2004 and 2008 for households earning less than $35,000, approximately 69 percent were refinanced by 2015, compared with 88 percent for households earning more than $75,000. The positive relationship between income and refinancing is monotonic and also prevalent in metropolitan areas, which tend to have more resilient banking systems (Figure 3). This income gradient persists even after controlling for standard underwriting measures, including debt-to-income ratio (DTI), combined loan-to-value ratio (CLTV), and credit score. This paper shows that the digital divide contributes to these disparities by reinforcing transaction and information frictions.

2.2 Broadband Internet in the United States

Broadband technology, which has grown in prevalence since the early 2000s, allows households to use the internet for all aspects of life including work, education, and entertainment. In this paper, I define broadband as a residential, high-speed, wireline internet service available in a given geographic area. I focus on residential (as opposed to commercial) service as it is relevant to at-home household financial decisions. High-speed status is determined by whether a service meets the standards for broadband set by the Federal Communications Commission (FCC). At the time Internet Essentials was introduced, the FCC defined broadband as service with download speeds of at least 4 megabits per second (Mbps), which was adequate for general web browsing, e-mail communication, and some low-bandwidth video streaming. I exclude legacy dial-up internet, which did not meet broadband speed standards during the study period. Lastly, I consider only wireline broadband and exclude cellular data plans.

The lack of broadband internet at home, particularly in urban areas, can largely be attributed to low affordability. Figure 2 shows a clear negative relationship between census tract poverty rates and broadband subscription rates. This trend is not driven by limited access to a broadband provider. In fact, more than 90 percent of the urban population in the U.S. lived in areas with broadband service by 2015, while only 60 percent of low-income households reported actually having a broadband subscription.[3] Survey results confirm that the price of subscription (59 percent) and cost of computer equipment (45 percent) are the top two reasons for not subscribing to broadband (Pew Research Center, 2015). While the urban-rural disparity in broadband coverage is an important access-driven cause for the digital divide, I focus on cost-driven disparities in subscription conditional on having access to infrastructure. This framework is useful for identification because it is invariant to unobservable differences in broadband service quality and customer demand across urban and rural areas.

2.3 Broadband and Mortgage Transaction Costs

At-home internet access is relevant for refinancing inequality due to the unique properties of a refinance mortgage. First, refinancing is largely standardized and compatible with technological innovation. In most interest rate refinances, the housing asset in question is already determined and the prospective borrower is in good standing on the existing mortgage.[4] Borrower uncertainty is thus low, allowing the refinance process to be streamlined and automated. Recent innovations in online approval and underwriting technology have led to a notable decrease (up to 30 percent from an average of 51 days) in processing time for refinance applications (Fuster et al., 2019). The internet has also enabled both bank and nonbank lenders to reach populations outside their immediate geographic markets, improving access to mortgage credit for underbanked households.

Second, refinancing involves large shadow costs that can be reduced through internet usage. The process requires extensive documentation and repeated interactions with lenders. With a computer and broadband connection, households can access and transmit financial documents online, communicate with loan officers by e-mail, and sign documents electronically, reducing the need for branch visits. The internet can also increase awareness and provide access to more lenders.

In this paper, I show that reducing transaction costs through broadband internet improves refinancing outcomes for low-income households. These households face particularly high refinancing frictions due to limited access to financial services and the time and information required to complete a

[3]Statistics are compiled from the 2015 FCC Broadband Progress Report and the author's calculations using 2017 ACS 5-year estimates. Appendix Figure A.3 also shows that low-income urban households without home broadband do not readily substitute cellular data plans.

[4]Since a refinance requires current homeownership, it is not determined by exogenous motives to move into or out of a dwelling. This is important as it allows the borrower pool to be invariant from significant income shocks or migrational incentives.

refinance. Figure 3 provides motivating evidence that broadband access is associated with disparities in realized refinancing outcomes. Among households with high refinancing likelihood, voluntary prepayment probabilities are generally lower in census tracts with limited broadband access, with the largest gaps concentrated in the bottom two income quintiles.

2.4 The Internet Essentials Program

Internet Essentials by Comcast provides a useful quasi-experimental setting to study the digital divide in mortgage refinancing. Comcast is one of the nation's largest internet service providers (ISPs), operating in 39 states and the District of Columbia and covering 48 million households at the time of the study. Internet Essentials was originally conceived to secure the FCC's support for Comcast's proposed merger with NBC Universal. The FCC ultimately approved the merger and required Comcast to implement the low-income subsidy program to promote the public interest (FCC, 2012). By early 2012, Internet Essentials was available throughout Comcast's national service area and was the first comprehensive program of its kind offered by a major ISP.

Internet Essentials significantly reduced the cost of at-home internet for eligible households. Enrolled households paid a $9.95 monthly fee plus applicable taxes, about 75 percent below the cost of a comparable unsubsidized plan (Hussain et al., 2013). All installation and activation fees (up to $100) and modem and router rental fees (up to $20 per month) were also waived. Fee savings over a three-year period would have exceeded $1,720, which is sizeable for eligible households with an average annual income of $30,000. Internet Essentials also offered subsidized computers for $149.99 and digital literacy training through online resources and a network of over 9,000 community organizations, libraries, and elected officials.

Internet Essentials eligibility depended on three requirements. First, a household must reside in an area served by Comcast at the time of application. Second, a household qualifies if it has a child receiving free or reduced-price lunch under the National School Lunch Program (NSLP). These benefits in turn depend on household size and income. Specifically, eligibility is restricted to households with annual income below 185 percent of the federal poverty level (FPL), which translates to around $35,000 for a three-person family and $42,000 for a four-person family during the study period. Third, an applicant must not have any past-due debt to Comcast and cannot have been a Comcast subscriber in the preceding 90 days, which makes it unlikely that new subscribers already had Comcast broadband service. Indeed, 80 percent of Internet Essentials customers reported not having broadband internet service at some point in the past (Comcast Corporation, 2016). Comcast promoted Internet Essentials through public service announcements and partnerships with thousands of school districts, nonprofit organizations, and city councils. It also streamlined the application process in the early years by automatically approving households with children attending majority low-income schools.

Internet Essentials connected more than 750,000 low-income families (or 3 million individuals) between 2012 and 2016. Adoption was concentrated in urban areas, with 75 percent of subscribers in the first five years coming from 10 of the 40 states served and the top 10 cities accounting for 25 percent of subscriptions (Comcast Corporation, 2016). Among subscribers, 89 percent reported using the internet almost every day. Appendix Table A.11 reports subscriber characteristics and internet usage. A majority of subscribers reported using the internet to find general information (92 percent), access e-mail (80 percent), and connect with others on social media (71 percent). Importantly, 65 percent reported that banks or other financial institutions expected them to have internet access at home. In a subsequent survey, 42 percent reported using the internet to access banking and financial services (Horrigan, 2014, 2019).

3 Data and Sample

3.1 Data Sources

I use several publicly available data sources to measure broadband coverage and refinancing outcomes. I supplement the main analysis with household survey data and measures of local economic conditions.

Comcast Coverage Rates. I compute coverage rates for Comcast and other major ISPs using service availability data obtained from the National Telecommunications and Information Administration (NTIA)'s State Broadband Initiative. Each ISP reports whether it offered internet service in a given census block on a biannual basis. I restrict the provider responses to those classified as broadband (high-speed, wireline, residential service) and aggregate the information up to the census tract.

Mortgage Applications and Originations. Home Mortgage Disclosure Act (HMDA) data provide loan-level information on the near-universe of mortgage applications in the U.S. I restrict the sample to owner-occupied, one- to four-family, conventional refinance mortgages.[5] HMDA reports the census tract of the property and applicant income, which allows me to assign treatment based on Comcast availability and Internet Essentials eligibility. For each year between 2009 and 2015, I count the number of refinance applications and originations for eligible and ineligible households in each census tract. I also compute denial rates for each eligibility group. HMDA additionally reports borrower characteristics such as race and sex.

[5]Conventional mortgages are loans that are not insured or guaranteed by the Federal Housing Administration (FHA), Veterans Administration (VA), Farm Service Agency (FSA) and Rural Housing Service (RHS).

GSE Loan Performance Data. I match loan performance data from Fannie Mae and Freddie Mac to HMDA to obtain mortgage outcomes and characteristics not reported in HMDA. For purchase mortgages originated between 2004 and 2008, I construct a panel of voluntary prepayment outcomes as an alternative measure of refinancing activity. The matched data also contain interest rates, debt-to-income ratios (DTI), combined loan-to-value ratios (CLTV), and credit scores, allowing me to control directly for refinancing incentives and frictions. I separately match refinance mortgages originated between 2009 and 2015 to obtain interest rates conditional on origination.

Mortgage and Rental Costs. I obtain household mortgage and rental payments from the Integrated Public Use Microdata Series (IPUMS) of the American Community Survey (ACS). The ACS also reports detailed information on income and household composition, allowing me to assign Internet Essentials eligibility more precisely. Household location is identified at the Public Use Microdata Area (PUMA) level, which has an average population above 100,000 and is therefore less geographically granular than a census tract. I also collect household characteristics including age, gender, race, and educational attainment.

House Prices and Average Income. I construct time-varying proxies for house prices and income within each census tract and eligibility group using HMDA data. Average originated loan amounts proxy for house prices, while average borrower income captures changes in income levels within each group. For specifications that do not rely on within-tract variation in house prices, I use annual census tract-level house price indices from the Federal Housing Finance Agency (FHFA).

Bank Branch Access. I obtain bank branch locations from the FDIC's Summary of Deposits, which provides geographic coordinates for all FDIC-insured institutions. For each census tract, I compute the number of full-service bank branches within a 1-mile radius of the population centroid as of 2010. Population centroids are obtained from the Census Bureau.

Fintech Lenders. I classify lenders annually as fintech if they allow borrowers to complete the mortgage origination process entirely online, following Fuster et al. (2019), and match the classification to HMDA using lender RSSD identifiers. Because the annual classification begins in 2010, I apply the 2010 classification for lenders in 2009.

Other Demographics. I measure urbanicity using the National Center for Health Statistics (NCHS) 2006 county-level classification. Demographic characteristics including unemployment, broadband usage, and educational attainment are obtained from ACS summary and microdata files.

3.2 Comcast Coverage and Program Eligibility

Assignment to treatment relies on two sources of variation: Comcast availability and income eligibility. To calculate Comcast coverage rates, I restrict the NTIA block-level provider data to connections that qualify as broadband under the definition used in this paper. Since census blocks are nested within census tracts, I aggregate the block data as of December 2012 as follows:

$$Comcast_{c,2012} = \frac{\sum_{b=1}^{c} Population_{b,2010} \times \mathbf{1}(Comcast_{b,2012})}{\sum_{b=1}^{c} Population_{b,2010}}, \tag{1}$$

where $Population_{b,2010}$ is the 2010 population of census block b and $\mathbf{1}(Comcast_{b,2012})$ indicates whether Comcast offers broadband service in block b as of 2012. $Comcast_{c,2012}$ therefore measures the fraction of census tract c's population with access to Comcast broadband. I use this December 2012 measure as $Comcast_c$ throughout the tract-level analysis. Comcast coverage in large central metropolitan census tracts is highly bimodal, providing variation between tracts with near-complete Comcast coverage and those with little or no Comcast presence. For placebo tests, I construct analogous December 2012 coverage rates for AT&T and Charter.

Internet Essentials eligibility also depends on whether a household has a child receiving free or reduced-price school lunch, which in turn depends on household income and size. A key challenge is that HMDA and GSE data do not report household size or the presence and age of children, so I cannot observe program eligibility directly. I classify households with income below 185 percent of the FPL for a three-person household as eligible and households with income between the thresholds for five- and six-person households as ineligible. I exclude households between the three- and five-person thresholds to reduce measurement error around the eligibility cutoff.[6] This classification defines an income-based proxy for program eligibility. Because HMDA does not observe family composition, some households in each income band may be misclassified, so the estimate is a reduced form comparison across income groups rather than a literal eligibility contrast. The resulting annual thresholds are reported in Appendix Table A.12.

For analyses using ACS data, I directly observe household income, family size, and the presence and age of children, allowing for more precise assignment of Internet Essentials eligibility. I classify households with at least one school-aged child and income below 170 percent of the FPL as eligible. The control group consists of households with income between 200 and 270 percent of the FPL, no school-aged child, or both. I also construct an alternative control group with income below 170 percent of the FPL but no school-aged child, allowing me to compare households with similar income but different program eligibility.

[6]Appendix C.1 shows that expanding the control band leaves the main result materially unchanged and that placebo income comparisons above the eligibility threshold yield no effect.

3.3 Final Sample

Internet Essentials initially expanded most rapidly in major cities through Comcast's partnerships with local governments and school districts. I therefore restrict the sample to census tracts in large central metropolitan counties. I also exclude census tracts without any refinance applications between 2009 and 2015.

The final sample consists of 5,256 census tracts covering 57 metropolitan statistical areas (MSAs). Comcast coverage is highly bimodal, with most census tracts near zero or full coverage, and I use the continuous coverage share in all main specifications.[7] Comcast and no Comcast census tracts are broadly distributed across the country. Most urban tracts without Comcast are instead served by AT&T or Charter, so the comparison is generally between areas served by different major providers, with only eligible households in Comcast areas gaining access to Internet Essentials in 2012.

Table 1 presents descriptive statistics for Comcast and no Comcast census tracts. While Comcast census tracts are slightly less populated on average, the two groups are economically similar in income distribution, urbanicity, age, household size, owner occupancy rates, housing cost burdens, employment, and educational attainment. Comcast census tracts have greater bank branch access and higher pre-treatment broadband subscription rates.

I restrict the mortgage sample to conventional, conforming loans to limit the influence of targeted government programs introduced following the Great Recession. This restriction also helps isolate income-related refinancing frictions from other dimensions of borrower distress. I therefore focus on borrowers who are relatively stable in terms of creditworthiness and housing equity but constrained along the income dimension.

Table 2 presents mortgage and borrower characteristics in Comcast and no Comcast areas by eligibility group. Across both areas, ineligible households have higher income and credit scores, purchase more expensive homes, and receive somewhat more favorable interest rates than eligible households. For mortgages originated between 2004 and 2008, the average interest rate differential available for refinancing between 2009 and 2011 is roughly 1.5 percentage points, exceeding standard thresholds for optimal refinancing (Agarwal et al., 2013). Differences between eligible and ineligible households are generally similar across Comcast and no Comcast areas. Although their incomes are lower, sample households have interest rates and credit quality similar to the broader population, consistent with income being their main observable constraint.

[7] For descriptive purposes, Appendix Table A.13 classifies census tracts at the 50 percent threshold and reports the 15 largest Comcast and no Comcast MSAs by population served.

4 Broadband Access and Refinancing Activity

Internet Essentials sorts households into eligible and ineligible groups at a single program date, making a difference-in-differences comparison a natural starting point. A complication is that these groups exhibit different refinancing trends even in the absence of the program. Income is positively correlated with mortgage principal, creditworthiness, and financial literacy, making ineligible households with marginally higher income generally more likely to refinance (Figure 3). Because repeated refinancing is uncommon, the potential refinance pool declines faster among ineligible households when interest rates fall, leading eligible low-income households to exhibit higher refinancing growth later in the cycle.

The geographic structure of Internet Essentials provides a way to account for this differential trend. I compare the eligible-ineligible refinancing gap at full Comcast coverage with the same gap at zero coverage. Table 3 illustrates this logic. The eligible-ineligible gap grows by 43 percent at full Comcast coverage and 33 percent at zero coverage, showing that most of the raw change reflects an economy-wide divergence between income groups. Differencing across coverage levels reduces the estimate to 7 percent. Repeating the exercise on a placebo income band entirely above the eligibility threshold produces a difference of only 1 percent, statistically indistinguishable from zero.

Motivated by these comparisons, I combine variation in continuous Comcast coverage, program eligibility, and time in a difference-in-difference-in-differences ("triple differences") design following Gruber (1994). The design compares changes in the refinancing gap between eligible and ineligible households across census tracts with different Comcast coverage rates. Census tract-level shocks common to both eligibility groups are therefore absorbed. Identification requires that, absent Internet Essentials, differences in refinancing between eligible and ineligible households would have evolved similarly across areas with different Comcast coverage rates.

4.1 Refinance Outcomes

I first study the effect of Internet Essentials on refinance originations using the following specification:

$$\begin{aligned} y_{i,c,t} = \alpha &+ \beta(Eligible_{i,c,t} \times Comcast_c \times Post_t) + X'_{i,c,t}\Phi \\ &+ \rho_1(\lambda_t \times \gamma_c) + \rho_2(Eligible_{i,c,t} \times \lambda_t) + \rho_3(Eligible_{i,c,t} \times \gamma_c) + \epsilon_{i,c,t}, \end{aligned} \tag{2}$$

where $y_{i,c,t}$ is the number of refinance mortgages originated by eligibility group i in census tract c in year t. I also use denial rates to study whether approval outcomes change alongside originations. $Eligible_{i,c,t}$ indicates Internet Essentials eligibility, $Comcast_c$ measures Comcast coverage in census tract c, and $Post_t$ identifies 2012–2015, when the program operated nationwide. $X_{i,c,t}$ includes average income and house prices measured for each eligibility-group-by-census-tract-by-year cell. Census tract by year fixed effects, $\lambda_t \times \gamma_c$, absorb local time-varying shocks common to both eligibility groups. Eligible by year fixed effects absorb aggregate differences in refinancing trends between eligible and ineligible households, while eligible by census tract fixed effects absorb permanent differences between the two groups within each census tract. The coefficient of interest, β, therefore captures the change in the refinancing gap between eligible and ineligible households associated with moving from no Comcast coverage to full coverage after the introduction of Internet Essentials. Identification requires that, absent Internet Essentials, differences in refinancing between eligible and ineligible households would have evolved similarly across areas with different levels of Comcast coverage.

I also test whether broadband access affects interest rates conditional on origination by replacing $y_{i,c,t}$ with loan-level interest rates for refinance mortgages originated between 2009 and 2015. The specification includes loan-level controls for income, loan amount, race, sex, number of applicants, combined LTV, DTI, credit score, and loan term, with the same fixed effects as equation (2).

I estimate count outcomes using Poisson pseudo maximum likelihood (PPML) (Gourieroux et al., 1984; Silva and Tenreyro, 2006; Correia et al., 2020), so β can be interpreted as a percentage change in the expected count. Standard errors are clustered by PUMA, allowing for correlated outcomes across neighboring census tracts. The sample ends in 2015, before comparable broadband subsidy programs emerged in 2016.

Column 1 of Table 4 presents the triple differences estimate for refinance originations. Moving from no Comcast coverage to full coverage increases the number of mortgages originated to eligible households by 8 percent per year, relative to a pre-treatment average of around 6 mortgages per census tract. The estimate is statistically significant at the 1 percent level. Appendix Table A.1 shows that its magnitude remains stable from the unconditional comparison through the full specification. Figure 4 presents the corresponding event study. I find no evidence of differential pretrends, while the treatment effect grows during the early years of the program and becomes statistically significant in 2013 and 2014. This pattern mirrors the growth in Internet Essentials subscribers over the same period (Comcast Corporation, 2016).

Column 2 of Table 4 shows a modest 1.4 percentage point decline in denial rates, which is marginally statistically significant. This pattern is consistent with eligible households submitting applications that were more likely to be approved, although the estimate does not distinguish changes in applicant composition from changes in lender behavior.

Among originated loans, Column 3 of Table 4 shows no detectable difference in interest rates after controlling for a rich set of loan-level characteristics. This may reflect the relative uniformity of terms and requirements for conventional mortgages. Fintech lenders also had limited market penetration during this period, which may have limited opportunities for online rate shopping. Taken together, broadband access primarily operates through the likelihood of refinancing.

4.2 Magnitudes and Household Savings

The 2.8 percentage point prepayment estimate is equivalent to approximately 18 percent of the 15.9 percentage point baseline gap between eligible and ineligible borrowers (Table 7). The savings from refinancing are also economically substantial for low-income households, which hold most of their wealth in home equity. Between 2004 and 2008, the average first-time homebuyer in my sample obtained a fixed-rate mortgage of around $120,000 at an interest rate of 6.2 percent. Applying the prevailing average rate of 4 percent between 2012 and 2015, a marginal household that refinanced would have saved around $110 per month before adjustments, or around $100 after accounting for the cost of Internet Essentials. This corresponds to a 14 percent reduction in mortgage payments and a 5 percent increase in monthly disposable income. Lifetime savings are up to $29,000, or $18,000 after discounting and adjusting for closing costs.[8] These savings represent around 10 percent of the net worth of low-income homeowners (Survey of Consumer Finances, 2013; Wolff, 2016).

An 8 percent increase in refinance originations, from a base of around 13,000 annual originations for the treated group before 2012, implies approximately 1,000 additional refinancings and $120 million in additional origination volume per year. Applying the $18,000 estimate of household savings implies approximately $73 million in aggregate savings between 2012 and 2015. Because the baseline sample includes only urban census tracts, extrapolating to Comcast's broader coverage area yields national savings of up to $135 million.[9]

The 8 percent estimate is an intent-to-treat effect because I do not observe household subscription. Comcast identified around 2.6 million households in its service area as eligible for Internet Essentials, while take-up averaged 10.6 percent during the period after treatment (Zuo, 2021) and reached around 22 percent by the end of 2015.[10] The estimate therefore captures the effect of program availability rather than a per-subscriber effect.

The American Community Survey provides complementary evidence on mortgage payment

[8]To calculate present value, I use a discount rate of 4 percent and reduce savings by an additional 15 percent to account for marginal taxes, closing costs, and the probability of moving. Using more conservative parameters from Agarwal et al. (2013) and Keys et al. (2016) reduces estimated savings to $15,000.

[9]Urban census tracts account for about 54 percent of Comcast's coverage area by population. The upper bound assumes the same treatment effect outside urban areas; assuming an effect half as large yields approximately $104 million in national savings.

[10]Comcast reported 500,000 subscribing households in August 2015 and more than 600,000 by March 2016. I interpolate between these figures to estimate take-up at the end of 2015 (Comcast Corporation, 2016).

burdens. Appendix D shows that eligible homeowners in Comcast areas experience a 1.3 to 1.7 percentage point decline in mortgage payments relative to income after 2012 across two specifications, with no corresponding change for an income-matched placebo group.

5 Robustness and Falsification Tests

5.1 Specification Diagnostics

I begin by testing whether the originations result is sensitive to the metropolitan areas included in the sample. Excluding the five largest MSAs with little or no Comcast coverage weakens the baseline estimate. Once I allow the eligible-ineligible refinancing gap to evolve separately within each MSA, however, the estimate remains positive and statistically significant in both the full and restricted samples (Appendix Table A.3). The result therefore does not depend on those five markets, although accounting for heterogeneous metropolitan trends is important.

The baseline also focuses on large central metropolitan counties. Expanding to all metropolitan counties or all urban census tracts produces smaller but precisely estimated effects (Appendix Table A.4). These exercises show that the finding is not confined to the baseline geographic definition.

5.2 Alternative Outcome Variables

While loan counts are the most direct measure of refinancing activity, they do not capture refinancing relative to the stock of existing mortgages. I therefore analyze prepayment behavior for home purchase mortgages originated between 2004 and 2008. The outcome is whether a loan has prepaid by 2011 and by 2015, and the specification is the counterpart of equation (2) at the loan level. Column 5 of Table 7 shows that Internet Essentials increases the prepayment probability for low-income households by 2.8 percentage points, or 6.7 percent relative to the pre-treatment mean of 42 percent. The estimate is statistically significant at the 5 percent level. The effect is concentrated among loans with the strongest refinancing incentives, where it holds without restricting the sample by credit score. This confirms that the credit score restriction in the baseline prepayment specification is not driving the result.[11]

Internet Essentials also launched while federal programs such as HARP and HAMP operated at scale, raising the possibility that refinancing among low-income households depended on government support. I address this concern in three ways using the matched loan-level sample. First, eligible households in Comcast and no Comcast areas were similar at origination in combined LTV, debt-to-income, credit score, and interest rates (Appendix Table A.7). Second, estimated positive equity

[11] Appendix B reports the estimating equation, sample construction, and optimal refinancing classification.

among eligible borrowers bottoms out in 2011 and 2012 and recovers above 90 percent by 2015 (Appendix Figure A.1). Third, the prepayment effect among positive-equity borrowers is nearly identical to the full-sample estimate (Appendix Table A.8). Together, these results show that the baseline findings are present among households for whom conventional refinancing was a viable option, with federal relief programs operating alongside rather than in place of the transaction cost channel.

5.3 Falsification and Targeting Tests

Internet Essentials was the only broadband subsidy program of its kind during the study period, motivating placebo tests using other major broadband providers. I assign AT&T and Charter as placebo program providers between 2012 and 2015, construct their census tract-level coverage rates, and reestimate equation (2). Columns 3 and 4 of Table 7 show no effect on refinance originations. Appendix Table A.9 reports the same continuous-coverage placebo comparison separately.

I next test whether the effects concentrate in census tracts where Internet Essentials was most likely to reach eligible households. Since I cannot observe program take-up directly, the sharpest test uses the prevalence of owner-occupied households with children under age 18 as a proxy for program eligibility. Columns 2 to 4 of Table 8 show that the treatment effect is concentrated in census tracts with a higher fraction of these households. The effect also increases with housing cost burden: estimates are 2.1, 8.7, and 18.1 percent across the low, middle, and high terciles, respectively. Finally, splitting census tracts within PUMAs by broadband subscription rates yields the largest and most precisely estimated effect in the top tercile, at 10.4 percent. These patterns provide additional evidence that the refinancing effects are concentrated where program exposure or the benefits of refinancing are likely to be greatest.

A final set of falsification tests examines groups and outcomes that should not be affected by Internet Essentials. Assigning placebo treatment and control groups using the income thresholds for families of 5–6 and 7–8 people, respectively, produces no statistically significant refinancing effect (Table 7, column 2). Rental payments also show no meaningful change (Table 7, column 7), consistent with refinancing among homeowners rather than a broader decline in housing costs.

6 Broadband and Refinancing Frictions

I next examine how broadband access reduces refinancing frictions. I study whether broadband substitutes for costly access to lenders, whether its benefits vary with household educational attainment, and whether income gains or information spillovers through communities can explain the increase in refinancing.

6.1 Substituting for Costly Lending Channels

Obtaining access to financial services is particularly challenging for the 20 percent of American households classified as underbanked (Federal Deposit Insurance Corporation, 2013). In the refinancing context, the relevant friction is not access to a first banking relationship, since every borrower in my sample already holds a mortgage, but the cost of reaching and transacting with lenders. Broadband can reduce these costs by helping households recognize valuable refinancing opportunities and making it easier to complete the process with existing financial institutions.

I first test whether refinancing growth is concentrated among online lenders. Columns 1 and 2 of Table 5 report the effects on fintech and non-fintech refinance originations using the annual lender classification from Fuster et al. (2019). Non-fintech originations increase by 8.6 percent, while the fintech estimate is 5.0 percent and imprecise. Fintech lenders had limited penetration in the mortgage market at the time, with online search interest remaining low before 2015 (Figure 5); the result does not imply that low-income households actively avoided them.

I next examine whether broadband is particularly valuable where physical access to lenders is limited. Columns 3 to 5 of Table 5 divide census tracts into terciles based on the number of bank branches within 1 mile of the population center. The treatment effect is 10.2 percent and statistically significant in low branch access tracts, weaker and marginally significant in the middle tercile, and statistically insignificant in the highest tercile. The pattern is consistent with broadband lowering the costs of reaching and transacting with existing local lenders.

6.2 Financial Literacy and Information Frictions

Internet Essentials combines broadband access with free training in internet use, job search, social services, and personal finance (Appendix Figure A.4), which may help households find and act on financial information.[12] Consistent with this possibility, subscribers with stronger digital skills are more likely to use online banking and financial services (Horrigan, 2019).

Because the ACS reports the educational attainment of the household head, it allows me to examine information frictions at the household level. I divide households into those with no college education and those with at least some college education and estimate equation (D.1) using mortgage payments relative to income as the dependent variable. Table 6 shows that Internet Essentials reduces mortgage payment burdens by 2.1 to 2.2 percentage points among households with no college

[12] Bhutta et al. (2023) show that financial literacy predicts households' ability to maintain liquid savings even after accounting for income and other resources.

education, with no corresponding change among households with some college education. The differences between the two groups are statistically significant under both definitions of the control group. These patterns are consistent with broadband lowering information and navigation frictions.

6.3 Income Effects and Information Spillovers

I first examine whether modest income gains mechanically improve households' ability to refinance. Among employed homeowners with a mortgage, income rises by 1.7 percent, statistically significant at the 10 percent level. The magnitude is close to Zuo's (2021) estimate, although that effect primarily reflects employment entry, which my sample excludes. Because the originations estimate remains essentially unchanged after controlling for income and house prices, this channel does not appear to explain the refinancing response.

Second, Internet Essentials may have spread information about refinancing through communities rather than operating through a household's own broadband access. Such spillovers should benefit ineligible households living in areas with a high share of eligible households. I test this prediction by comparing refinancing outcomes of ineligible households across Comcast census tracts with high and low shares of eligible households. Appendix Table A.14 shows no evidence of spillovers: the interaction term for ineligible households is small and statistically insignificant, and eligible households themselves show no differential effect in tracts with high eligible shares. Aggregate refinancing searches also show no differential increase in Comcast media markets after 2012.

Taken together, estimates for refinance originations are largest where physical access to lenders is limited, while mortgage payments relative to income fall most among households with lower educational attainment. Modest income gains or information spillovers through communities do not appear to explain the refinancing response. The education differences are statistically significant under both control definitions, while the branch access pattern remains suggestive.

7 Conclusion

Mortgage refinancing can generate substantial savings, yet many households do not refinance when interest rates fall. This pattern is particularly pronounced among low-income households, for whom the resulting savings can represent a meaningful share of disposable income and household wealth. In this paper, I study whether disparities in broadband access contribute to these differences in refinancing behavior. I exploit the introduction of Internet Essentials, a large broadband subsidy program for low-income households, together with geographic, temporal, and program eligibility variation. I find that broadband access increases refinance originations by 8 percent for eligible households, closing a meaningful portion of the preexisting refinancing gap between high- and low-income households. Household survey data show that Internet Essentials also reduces mortgage payment burdens, and the implied savings from refinancing are substantial. Effects on refinance originations are largest in areas with limited access to bank branches, while payment burdens fall most among households with lower educational attainment. These patterns are consistent with broadband reducing the transaction and information frictions involved in refinancing.

These findings have implications for the transmission of monetary policy. When interest rates fall, households must actively refinance to benefit from lower mortgage rates, and the costs of doing so can differ substantially across households. Limited access to financial services can therefore weaken the pass-through of monetary policy to precisely the households for whom mortgage payment savings are most valuable. This issue is particularly relevant in the U.S. mortgage market, where fixed-rate contracts place the refinancing decision largely in the hands of borrowers. Mortgage designs that reduce the need for costly borrower action may therefore improve the transmission of lower rates to constrained households.

The results also highlight a broader consequence of the digital divide. As financial services increasingly move online, access to financial institutions depends not only on physical proximity but also on whether households can readily search for and transact with lenders. Broadband can help bridge this gap, particularly where traditional financial access is limited. The benefits of policies that expand broadband access may therefore extend beyond internet connectivity itself to household financial decisions with lasting consequences for wealth. Whether similar frictions shape other decisions in household finance remains an important question for future research.

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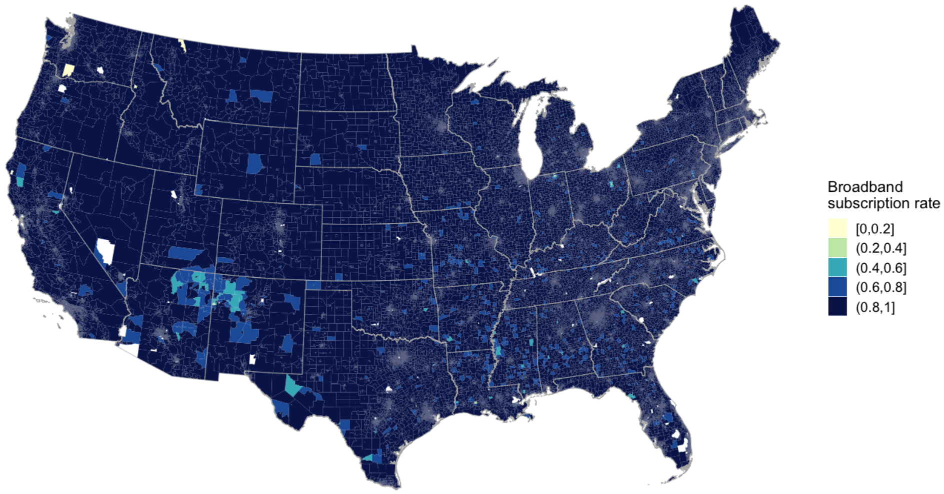
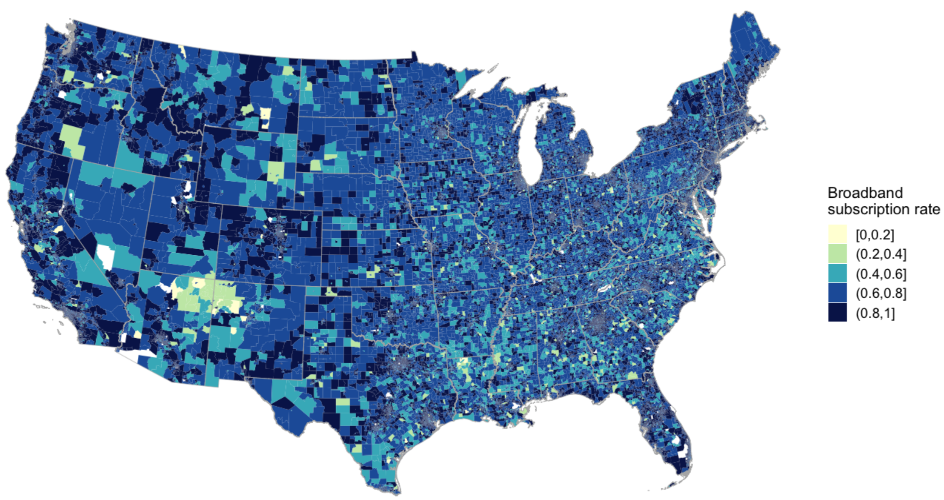
Figures and Tables



(a) Household income above $75,000



(b) Household income below $35,000

Figure 1: Broadband Access in the United States

Notes: This figure maps the fraction of high- and low-income households with a broadband internet subscription at the census tract level. Annual household income is in 2024 inflation-adjusted dollars.
Source: 2024 ACS 5-year estimates.

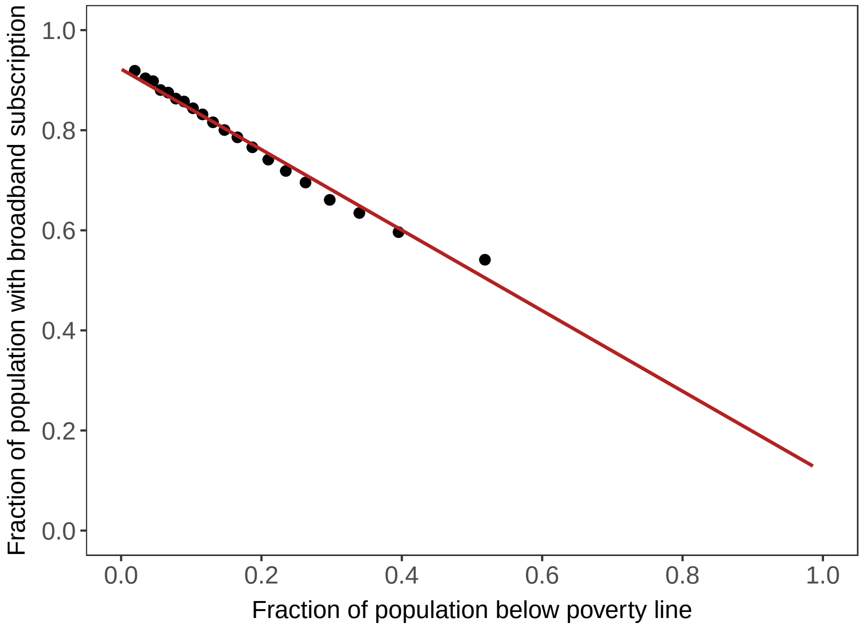


Figure 2: Broadband Affordability and the Digital Divide

Notes: This figure plots census tract poverty rates against broadband subscription rates in large central metropolitan counties with high ISP coverage. The x-axis is the fraction of the population living below the poverty line, and the y-axis is the fraction with a high-speed broadband subscription at home. Each point represents the average broadband subscription rate within one of 20 equal-sized bins for the poverty rate. The fitted line is estimated using the underlying census tract-level observations.
Source: NCHS, 2017 ACS 5-year estimates.

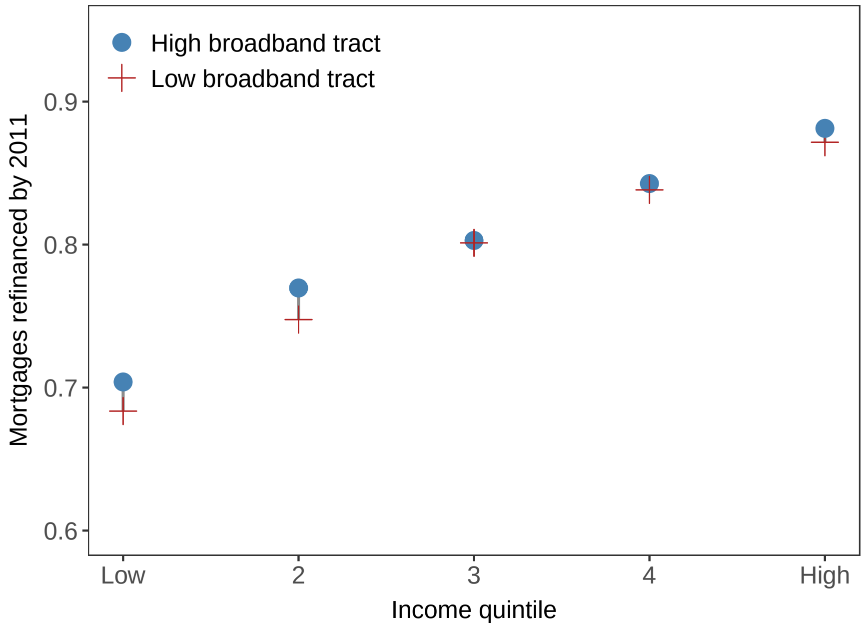


Figure 3: Refinancing Inequality and Access to Broadband

Notes: This figure plots mortgage refinancing rates by household income quintile separately for census tracts above and below approximately 45 percent broadband subscription as of December 2011. The sample consists of conventional mortgages originated between 2004 and 2008 with an interest rate of at least 6.25 percent, a credit score of at least 780, a combined LTV of 80 percent or less, and a DTI of 36 percent or less. Refinancing is proxied by voluntary prepayment by 2011.
Source: HMDA, Fannie Mae and Freddie Mac loan performance files, FCC Form 477, and author's calculations.

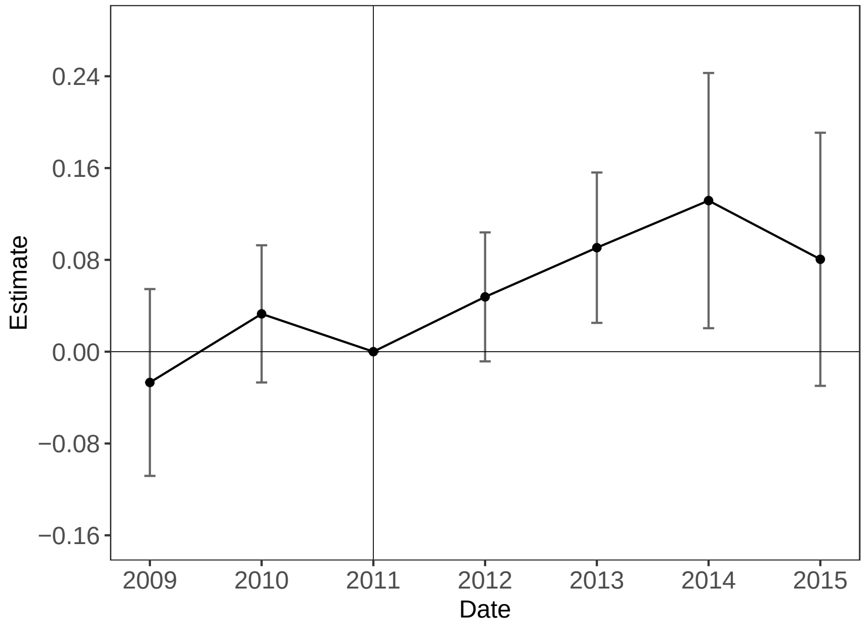


Figure 4: Event Study Estimates for Refinance Originations

Notes: This figure plots dynamic triple difference estimates (β_t) and 95 percent confidence intervals for refinance originations from 2009 through 2015. Each annual coefficient is estimated from $Eligible_{i,c,t} \times Comcast_c \times Year_t$, where $Comcast_c$ is the continuous December 2012 census tract coverage share. The specification includes census tract by year, eligibility by year, and eligibility by census tract fixed effects, along with controls for average income and house prices. The interaction for 2011, the reference year, is omitted. Robust standard errors are clustered at the PUMA level.
Source: HMDA, NTIA State Broadband Initiative.

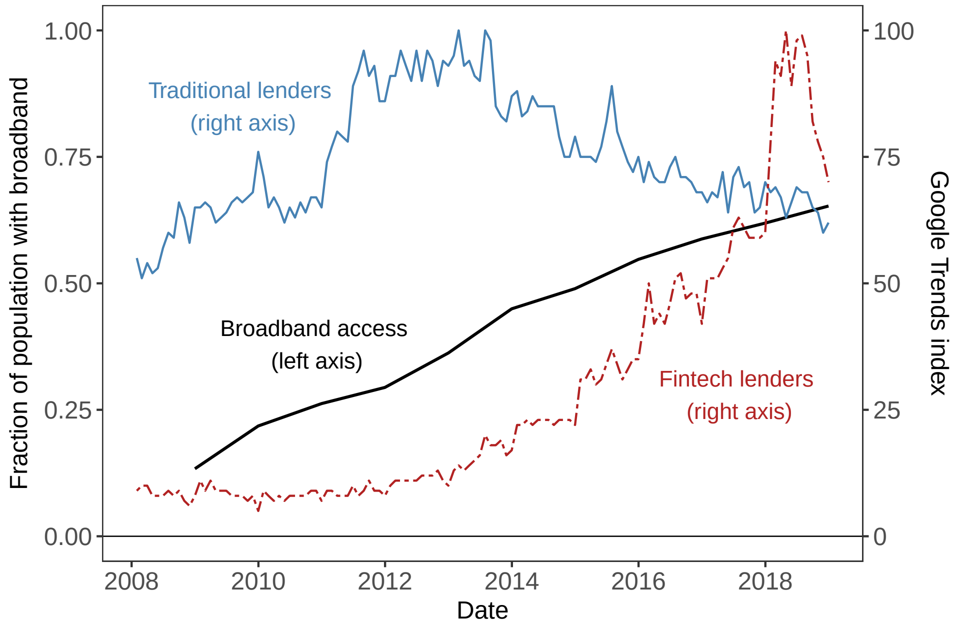


Figure 5: Online Search Trends for Mortgage Lenders

Notes: This figure plots Google Trends search indices for traditional and fintech mortgage lenders together with the national broadband subscription rate. Monthly search indices are constructed for the top 10 traditional lenders and top 10 fintech lenders by origination volume from 2008 to 2018 and normalized to a maximum of 100 over the sample period. Fintech lender classifications follow Buchak et al. (2018) and Fuster et al. (2019). The national broadband subscription rate is constructed from annual county-level subscription estimates and housing unit counts. Broadband is defined as a wireline connection with a minimum download speed of 10 Mbps.
Source: Google, FCC Form 477.

Table 1

Descriptive Statistics

	Comcast ($N = 2,218$)		**No Comcast** ($N = 3,038$)		
	Mean	SD	Mean	SD	Diff.
	(1)	(2)	(3)	(4)	(5)
Population (2010)	9218.22	7606.29	11028.18	9480.71	7.67
Annual income under \$35,000	29.67	12.83	30.11	11.97	1.27
Annual income \$35,000 - \$50,000	13.39	4.28	13.84	4.04	3.82
Living in urban areas (2010)	98.76	6.57	97.51	10.20	-5.37
Median age (2010)	36.68	5.14	36.64	5.87	-0.28
Average household size (2010)	2.69	0.48	2.82	0.60	8.67
Owner-occupancy					
Annual income under \$35,000	44.59	20.08	44.18	19.17	-0.74
Annual income \$35,000 - \$50,000	56.77	20.82	55.29	20.03	-2.60
With school-aged child	33.69	10.53	35.19	10.66	5.09
Cost-burdened homeowners	42.06	11.55	44.60	12.63	7.58
Employment rate	91.03	4.08	91.11	3.54	0.72
High school diploma or higher	90.13	9.23	87.50	12.18	-8.92
Number of bank branches	3.95	6.17	3.34	3.71	-4.10
Broadband connections	50.36	14.55	36.72	19.14	-29.34

Notes: This table reports means and standard deviations for Comcast and no Comcast census tracts in large central metropolitan counties. The two groups use the December 2012 50 percent tract coverage classification described in Section 3.3. Population, urban status, median age, and household size are from the 2010 Decennial Census; remaining demographic characteristics are from the 2007–2011 ACS 5-year estimates. Cost-burdened homeowners spend at least 30 percent of income on housing costs. Bank branches are full-service branches within 1 mile of the tract population centroid as of 2010. Broadband connections are fixed residential connections with download speeds of at least 4 Mbps as of December 2011. Statistics are weighted by 2010 tract population, except tract population itself, which is unweighted, and the household income and ownership rows, which are weighted by ACS household counts. Column 5 reports Welch two-sample t-statistics.

Table 2

Mortgage Characteristics by Comcast Coverage

	Comcast			No Comcast		
	All (1)	Eligible (2)	Ineligible (3)	All (4)	Eligible (5)	Ineligible (6)
HH income ($ thousands)						
'04-'08 purchase	98.91	24.02	45.73	110.40***	23.72***	45.67**
'09-'11 refinance	103.43	25.67	51.31	101.56	25.28***	51.30
Loan count						
'04-'08 purchase	671.25	30.65	50.59	653.58	35.85***	49.24
'09-'11 refinance	280.92	11.66	14.61	218.50***	13.05***	14.20
Loan amount ($ thousands)						
'04-'08 purchase	237.27	115.47	137.30	284.96***	122.98***	141.47***
'09-'11 refinance	194.29	116.89	145.57	214.23***	123.40***	152.65***
Interest rate (percent)						
'04-'08 purchase	6.06	6.18	6.09	6.07	6.18	6.09
'09-'11 refinance	4.64	4.65	4.62	4.64	4.62**	4.63
Debt-to-income						
'04-'08 purchase	36.49	36.43	37.53	37.37***	36.50	37.53
'09-'11 refinance	30.22	30.92	31.05	31.62***	32.12***	31.60**
Combined loan-to-value						
'04-'08 purchase	80.32	77.37	80.67	78.49***	76.46	78.04***
'09-'11 refinance	66.71	59.22	63.21	63.34***	57.88***	62.32*
Credit score						
'04-'08 purchase	734.58	726.79	732.70	735.01	726.75	732.94
'09-'11 refinance	761.35	762.93	764.54	760.08***	761.59	762.68**
Male (percent)						
'04-'08 purchase	59.51	45.52	53.12	60.44***	47.36***	54.53***
'09-'11 refinance	58.86	41.28	51.73	60.28***	44.75***	54.32***
Black (percent)						
'04-'08 purchase	17.69	18.78	19.58	14.92***	15.44***	15.77***
'09-'11 refinance	13.79	11.49	10.78	12.76**	9.83***	9.37***
Hispanic (percent)						
'04-'08 purchase	12.99	14.51	13.38	20.88***	21.03***	19.54***
'09-'11 refinance	8.22	10.06	9.02	15.04***	18.99***	16.31***

Notes: This table reports average mortgage and borrower characteristics by Comcast coverage and program eligibility. Comcast and no Comcast use the December 2012 50 percent tract coverage classification described in Section 3.3. *'04–'08 purchase* refers to purchase mortgages originated from 2004 through 2008; *'09–'11 refinance* refers to refinance mortgages originated from 2009 through 2011. Eligible and ineligible households follow the income bands defined in Section 3.2. HMDA provides income, loan count, loan amount, sex, and race or ethnicity; interest rates, DTI, combined LTV, and credit scores come from the matched Fannie Mae and Freddie Mac loan performance sample. Stars report Welch two-sample tests comparing Comcast and no Comcast means. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 3

Refinancing Gaps Before and After Internet Essentials

	Baseline income band			Shifted income band (placebo)		
	Full Comcast	No Comcast	Difference	Full Comcast	No Comcast	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Eligible × *Post*	0.354***	0.287***	0.067**	0.099***	0.088***	0.011
	(0.018)	(0.024)	(0.030)	(0.009)	(0.008)	(0.012)
Observations	72,422	72,422	72,422	68,068	68,068	68,068
Clusters	255	255	255	253	253	253

Notes: This table reports difference-in-differences estimates for refinance originations using the continuous December 2012 census tract coverage share. Columns 1–3 use the baseline income bands and columns 4–6 shift both groups above program eligibility as a placebo. Each block reports the fitted eligible-ineligible post-period change at full Comcast coverage, the fitted change at zero coverage, and their difference. All estimates use PPML with eligibility group by census tract, year, and coverage by year fixed effects and controls for average income and loan amount. The sample covers large central metropolitan census tracts from 2009 through 2015. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 4

Broadband Access and Refinancing Activity

Dependent variable	Number of originations (1)	Denial rate (2)	Interest rate (3)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.078***	-1.433*	0.012
	(0.026)	(0.776)	(0.014)
Mean of dependent variable (Eligible)	6.48	40.86	4.34
Mean of dependent variable (Ineligible)	6.09	30.95	4.27
Eligibility group controls	✓	✓	
Loan characteristics controls			✓
Fixed effects	✓	✓	✓
Observations	71,660	72,422	92,277
Adjusted R-squared	0.65	0.21	0.73

Notes: This table reports triple differences estimates of the effect of Internet Essentials on refinancing outcomes from 2009 through 2015 in large central metropolitan counties. The treatment interaction in every column is $Eligible_{i,c,t} \times Comcast_c \times Post_t$, where $Comcast_c$ is the continuous December 2012 census tract coverage share. Column 1 uses annual refinance originations by eligibility group, column 2 uses the percentage of refinance applications denied, and column 3 uses interest rates on originated refinance loans matched to GSE loan-level data with nonmissing loan and borrower characteristics. Column 1 is estimated by PPML and columns 2 and 3 by OLS. Eligibility group controls are average income and loan amount; column 3 additionally includes loan and borrower characteristics. All specifications include eligibility-year, eligibility-census tract, and census tract-year fixed effects. Dependent-variable means are reported as of 2011. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 5

Refinance Originations by Lender Type and Bank Branch Access

Dependent variable	Lender type: Fintech (1)	Lender type: Non-fintech (2)	Branch access: Low (3)	Branch access: Mid (4)	Branch access: High (5)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.049	0.082***	0.102***	0.060*	0.070
	(0.112)	(0.027)	(0.033)	(0.035)	(0.051)
Mean branches within 1 mile			0.92	3.94	11.55
Mean of dependent variable (Eligible)	0.10	6.38	6.52	7.03	5.10
Mean of dependent variable (Ineligible)	0.15	5.94	6.14	6.63	4.78
Eligibility group controls	✓	✓	✓	✓	✓
Fixed effects	✓	✓	✓	✓	✓
Observations	25,220	71,546	29,102	29,546	13,012
Pseudo R-squared	0.33	0.64	0.66	0.66	0.54

Notes: This table reports the effect of Internet Essentials on refinance originations by lender type and bank branch access. All columns use $Eligible_{i,c,t} \times Comcast_c \times Post_t$, with the continuous December 2012 census tract coverage share, and are estimated by PPML. Columns 1 and 2 use refinance originations from fintech and non-fintech lenders, respectively. Fintech lenders are classified annually following Fuster et al. (2019); because the annual classification begins in 2010, the 2010 classification is applied to 2009. Columns 3–5 use total refinance originations after dividing census tracts into terciles based on the number of full-service bank branches within 1 mile of the population centroid as of 2010. All specifications include controls for average income and loan amount and eligibility-year, eligibility-census tract, and census tract-year fixed effects. The sample covers 2009 through 2015. Dependent-variable means are reported as of 2011. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6

Mortgage Payment Burden by Educational Attainment

Dependent variable	Mortgage payments relative to income			
	All ineligibles		Low-income ineligibles	
	No college (1)	Some college (2)	No college (3)	Some college (4)
$Eligible_{i,p,t} \times Comcast_p \times Post_t$	-0.022***	-0.003	-0.021***	0.005
	(0.005)	(0.004)	(0.006)	(0.006)
Mean of dep. var: Eligible	36.64	38.88	36.64	38.88
Mean of dep. var: Ineligible	28.04	29.87	36.52	41.82
Household controls	✓	✓	✓	✓
Fixed effects	✓	✓	✓	✓
Observations	290,407	312,176	155,625	130,043
Adjusted R^2	0.57	0.56	0.53	0.52

Notes: This table reports the effects of Internet Essentials on mortgage payment burdens separately for households whose heads have no college education and those whose heads have at least some college education. The treatment interaction is $Eligible_{i,p,t} \times Comcast_p \times Post_t$, where $Comcast_p$ is the continuous December 2012 PUMA coverage share. Columns 1 and 2 use all ineligible households as controls; columns 3 and 4 use low-income ineligible households. The differences between the two education groups are statistically significant in both designs ($p < 0.01$). Coefficients are changes in the ratio of mortgage payments to income, while dependent-variable means are reported in percent as of 2011. All specifications include household controls and the corresponding fixed effects, with standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 7

Alternative Outcomes and Falsification Tests

	Originations						
Dependent variable	Baseline (1)	Placebo cutoff (2)	Placebo ISP (3)	(4)	Prepayment (5)	Log(income) (6)	Log(rent payment) (7)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.078*** (0.026)	0.014 (0.012)			0.028** (0.013)		
$Eligible_{i,c,t} \times AT\&T_c \times Post_t$			0.006 (0.023)				
$Eligible_{i,c,t} \times Charter_c \times Post_t$				-0.010 (0.059)			
$Eligible_{i,p,t} \times Comcast_p \times Post_t$						0.017* (0.010)	-0.001 (0.011)
Mean of dependent variable (Eligible)	6.48	6.46	6.48	6.48	41.99	29.91	0.68
Mean of dependent variable (Ineligible)	6.09	6.79	6.09	6.09	57.93	24.35	0.71
Loan characteristics controls					✓		
Eligibility group controls	✓	✓	✓	✓			
Household controls						✓	✓
Fixed effects	✓	✓	✓	✓	✓	✓	✓
Observations	71,660	67,354	71,660	71,660	214,364	159,622	421,412
Adjusted R-squared	0.65	0.65	0.65	0.65	0.21	0.66	0.43

Notes: This table reports alternative outcome and falsification tests for Internet Essentials. Every treatment uses the continuous December 2012 coverage share at the geography of observation: census tract coverage in columns 1–5 and PUMA coverage in columns 6 and 7. Column 1 reports the baseline refinance origination estimate; column 2 shifts the treatment and control income bands above program eligibility; columns 3 and 4 use AT&T and Charter coverage as placebo treatments; column 5 uses mortgage prepayment; column 6 uses log household income; and column 7 uses log monthly rent payments. Specifications include the corresponding controls and eligibility-year, eligibility-geography, and geography-year fixed effects. Dependent-variable means are reported as of 2011. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 8

Targeting Tests for Internet Essentials

		School-aged children			Cost-burdened homeowners			Broadband subscription rate		
Dependent variable: Originations	Baseline (1)	Low (2)	Mid (3)	High (4)	Low (5)	Mid (6)	High (7)	Low (8)	Mid (9)	High (10)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.078***	0.043	0.079***	0.097**	0.021	0.087**	0.181***	0.061	0.061*	0.104***
	(0.026)	(0.040)	(0.030)	(0.040)	(0.031)	(0.039)	(0.041)	(0.047)	(0.035)	(0.035)
Mean of sorting variable		19.64	30.82	44.55	30.11	45.10	61.07	74.36	85.40	94.05
Mean of dependent variable (Eligible)	6.48	5.42	6.65	7.08	6.71	6.79	5.57	5.56	7.02	5.98
Mean of dependent variable (Ineligible)	6.09	4.68	6.38	6.84	6.84	6.26	4.62	5.35	6.47	5.89
Eligibility group controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	71,660	18,616	29,222	23,822	27,286	27,708	16,666	20,510	31,932	19,218
Adjusted R-squared	0.65	0.59	0.64	0.68	0.66	0.66	0.60	0.62	0.67	0.63

Notes: This table reports refinance origination estimates across groups with different likelihoods of program exposure or refinancing benefits. Every column uses $Eligible_{i,c,t} \times Comcast_c \times Post_t$, with the continuous December 2012 census tract coverage share. Column 1 reports the baseline estimate. Columns 2–4 divide census tracts by the share of owner-occupied households with a child under age 18; columns 5–7 by the share of cost-burdened homeowners; and columns 8–10 by broadband subscription among low-income homeowners with a school-aged child. Low, mid, and high denote the bottom, middle, and top terciles. All specifications use PPML with controls for average income and loan amount and eligibility-year, eligibility-census tract, and census tract-year fixed effects. The sample covers 2009 through 2015. Means of the sorting variables are reported in percent and dependent-variable means as of 2011. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Internet Appendix

"The Digital Divide and Refinancing Inequality"

August 2026

A Constructing the Triple Differences Estimate

A.1 From Unconditional Means to the Validated Specification

Table A.1 traces the triple differences estimate from a saturated model with no fixed effects to the specification used in the main analysis. The estimation sample is held fixed across columns. Census tract and year fixed effects enter first, followed by eligibility-by-tract and eligibility-by-year fixed effects, the tract-by-year fixed effect, and finally the income and house price proxy controls.

The coefficient moves from 0.066 in the saturated model to 0.078 in the full specification, while the standard error falls from 0.031 to 0.026. The estimate is statistically significant at the 5 percent level throughout and at the 1 percent level once tract-by-year fixed effects are included. The point estimate therefore changes little as the specification absorbs increasingly local variation.

A.2 Geographic Comparisons and Metropolitan Heterogeneity

Table A.2 examines the two comparisons that motivate the triple differences design using continuous Comcast coverage. The first row reports the fitted eligible-ineligible post-period change at full coverage from Table 3. The remaining rows compare eligible households across the coverage distribution as geographic controls are added. Panel B repeats the exercise after excluding Los Angeles.

Without geographic controls, the estimate across the coverage distribution is -2.8 percent and statistically insignificant. Metropolitan-by-year and county-by-year fixed effects move the estimate to 2.8 and 2.5 percent, respectively, but both remain imprecise. Excluding Los Angeles changes the uncontrolled comparisons but leaves the specifications with local year fixed effects unchanged. The simple comparison across areas is therefore sensitive to market composition and is not the specification used for the main analysis.

Table A.3 reports four versions of the originations specification. The baseline estimate is 7.8 percent. Excluding the five largest metropolitan areas with little or no Comcast coverage reduces the estimate to 1.3 percent. Allowing the refinancing gap between eligible and ineligible households to follow a separate trend in each metropolitan area yields an estimate of 8.2 percent in the full sample and 8.5 percent after excluding the five metropolitan areas; both are statistically significant at the 5 percent level. Table A.4 provides the complementary exercise of expanding the sample beyond large central metropolitan counties, where the estimates remain positive and precisely estimated.

Table A.1

Construction of the Triple Differences Estimate

	(1)	(2)	(3)	(4)	(5)
A. Refinance originations					
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.066**	0.064**	0.063**	0.074***	0.078***
	(0.031)	(0.030)	(0.030)	(0.025)	(0.026)
Census tract FE		✓	✓		
Year FE		✓	✓		
Eligible × year, eligible × tract FE			✓	✓	✓
Tract × year FE				✓	✓
House price / income proxy controls					✓
Mean of dependent variable (Eligible)	6.40	6.40	6.40	6.40	6.40
Mean of dependent variable (Ineligible)	6.06	6.06	6.06	6.06	6.06
Observations	71,660	71,660	71,660	71,660	71,660
Pseudo R-squared	0.01	0.52	0.57	0.65	0.65

Notes: This table traces the originations estimate from a saturated specification with no fixed effects to the full specification. Every column uses $Eligible_{i,c,t} \times Comcast_c \times Post_t$, with the continuous December 2012 census tract coverage share. Columns 2 through 4 add progressively more granular fixed effects, and column 5 adds controls for average income and loan amount. The estimation sample is held fixed across columns. All specifications use PPML and standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.2

Naive Comparisons Under Geographic Controls

	Coefficient	N	Clusters
Eligible–ineligible change at full coverage	0.354***	72,422	255
	(0.018)		
A. All metros			
Difference-in-differences	-0.028	36,211	255
	(0.068)		
+ unit, year FE	-0.027	36,211	255
	(0.066)		
+ metro × year FE	0.028	36,211	255
	(0.045)		
+ county × year FE	0.025	36,205	255
	(0.045)		
B. Ex-LA			
Difference-in-differences	-0.011	30,324	225
	(0.076)		
+ unit, year FE	-0.011	30,324	225
	(0.073)		
+ metro × year FE	0.028	30,324	225
	(0.045)		
+ county × year FE	0.025	30,318	225
	(0.045)		

Notes: This table reports refinance origination estimates under progressively finer geographic controls. The first row reports the fitted eligible-ineligible post-period change at full coverage from Table 3. The remaining rows compare eligible households across the continuous December 2012 Comcast coverage distribution. Panel B excludes Los Angeles. All specifications use PPML on large central metropolitan census tracts from 2009 through 2015, with standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.3

Metropolitan Heterogeneity and Local Trends

	(1)	(2)	(3)	(4)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.078***	0.013	0.082**	0.085**
	(0.026)	(0.032)	(0.036)	(0.036)
Drop five MSAs		✓		✓
MSA-specific eligibility trends			✓	✓
Observations	71,660	54,292	71,660	54,292
Clusters	255	205	255	205

Notes: This table reports refinance origination estimates under alternative metropolitan-area samples and trend controls. Column 1 reports the baseline specification. Column 2 excludes the five largest metropolitan areas with average Comcast coverage below 10 percent, ranked by refinance origination volume. Column 3 replaces eligibility-by-year fixed effects with eligibility-by-metropolitan-area-by-year fixed effects, and column 4 combines that specification with the column 2 sample restriction. The sample covers large central metropolitan census tracts from 2009 through 2015. All specifications use the continuous December 2012 Comcast coverage share and PPML, with standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.4

Robustness to Geographic Scope

	(1) Large central metropolitan counties	(2) All metropolitan counties	(3) All urban tracts
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.078***	0.044***	0.043***
	(0.026)	(0.013)	(0.013)
Census tracts	5,256	19,045	19,003
Observations	71,660	261,692	259,876

Notes: This table reports refinance origination estimates across three geographic samples. $Comcast_c$ is the continuous share of a census tract's 2010 population living in census blocks where Comcast broadband was available as of December 2012. Column 1 uses large central metropolitan counties, column 2 uses all metropolitan counties, and column 3 uses census tracts classified as urban under the Rural Urban Commuting Area scheme. All specifications use PPML, with standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

B Voluntary Prepayment and Refinancing Feasibility

B.1 Matched Loan Sample and Prepayment Specification

I use the matched GSE-HMDA sample to study voluntary prepayment as a loan-level measure of refinancing. The Fannie Mae and Freddie Mac files provide interest rates, debt-to-income ratios, combined loan-to-value ratios, credit scores, and loan performance. HMDA adds household income and demographic characteristics needed to assign Internet Essentials eligibility.

I match the two data sets using origination year, agency, owner occupancy, loan type, number of applicants, loan amount, and location. Location is matched less precisely because the GSE files report 3-digit ZIP codes rather than census tracts. The resulting sample covers 22 to 25 percent of the full HMDA refinance sample.[1] Table A.5 validates this matching procedure on refinance originations, a larger sample than the purchase mortgage cohort used below, and compares it with the full HMDA refinance universe. Black borrowers are underrepresented in the match, so the loan-level results are most directly interpreted as evidence for the conforming segment.

I estimate the following specification on this matched sample:

$$\begin{aligned} prepay_{i,c,t} = \alpha + \beta(Eligible_{i,c,t} \times Comcast_c \times Post_t) + Y'_{i,c,t}\Phi \\ + \rho_1(\lambda_t \times \gamma_c) + \rho_2(Eligible_{i,c,t} \times \lambda_t) + \rho_3(Eligible_{i,c,t} \times \gamma_c) + \epsilon_{i,c,t}, \end{aligned} \tag{B.1}$$

where $prepay_{i,c,t}$ indicates whether loan i has prepaid by 2011 or 2015. As in the main HMDA specification, $Comcast_c$ is the continuous December 2012 census tract coverage share. Eligibility is measured at origination and held fixed through 2015. I use a control group with higher income beginning above the six-person eligibility threshold and drop households with income above twice that lower bound. The baseline sample also requires an origination credit score of at least 750. $Y'_{i,c,t}$ includes borrower and loan characteristics, and the fixed effects follow the same tract, eligibility, and time structure as the main specification. Standard errors are clustered by PUMA.

B.2 Prepayment Estimates and Dynamics

Column 5 of Table 7 shows that Internet Essentials increases the probability of prepayment by 2.8 percentage points, or 6.7 percent relative to the pre-treatment mean of 42 percent. I use the optimal refinancing rule of Agarwal et al. (2013) to examine whether this result is concentrated among loans

[1] The incomplete match reflects the difference in reported geography and the exclusion of government-insured and several nonstandard mortgage products from the GSE performance files.

with stronger refinancing incentives. By 2012, 94.5 percent of loans in the matched sample are in the money under the closed-form rule, so the binary classification provides little separation. Table A.6 instead focuses on the distance above each loan's refinancing threshold. The estimate is largest in the deepest tercile and remains statistically significant without imposing the credit score restriction.

B.3 The 2012 Housing Market and Refinancing Feasibility

Internet Essentials launched while the housing market was still recovering from the Great Recession and federal programs such as HARP and HAMP were operating at scale. I use the matched loan sample to examine whether differences in borrower condition or negative equity account for the refinancing results.

Eligible borrowers in Comcast and no Comcast areas are similar at origination in combined LTV, debt-to-income, credit score, and interest rates. The normalized differences for these variables are below 0.08 in absolute value in Table A.7. Figure A.1 tracks estimated home equity after origination. The share of eligible borrowers above water declines through 2011 and 2012 and then recovers above 90 percent by 2015.

Table A.8 splits the prepayment sample by estimated home equity in 2012. The estimate for above-water borrowers is 2.8 percentage points, nearly identical in magnitude to the full-sample estimate, although less precise. The estimate among borrowers with high estimated LTV is also positive, a group for which HARP may have facilitated refinancing. The baseline result is therefore not confined to borrowers whose refinancing depended on relief for negative equity.

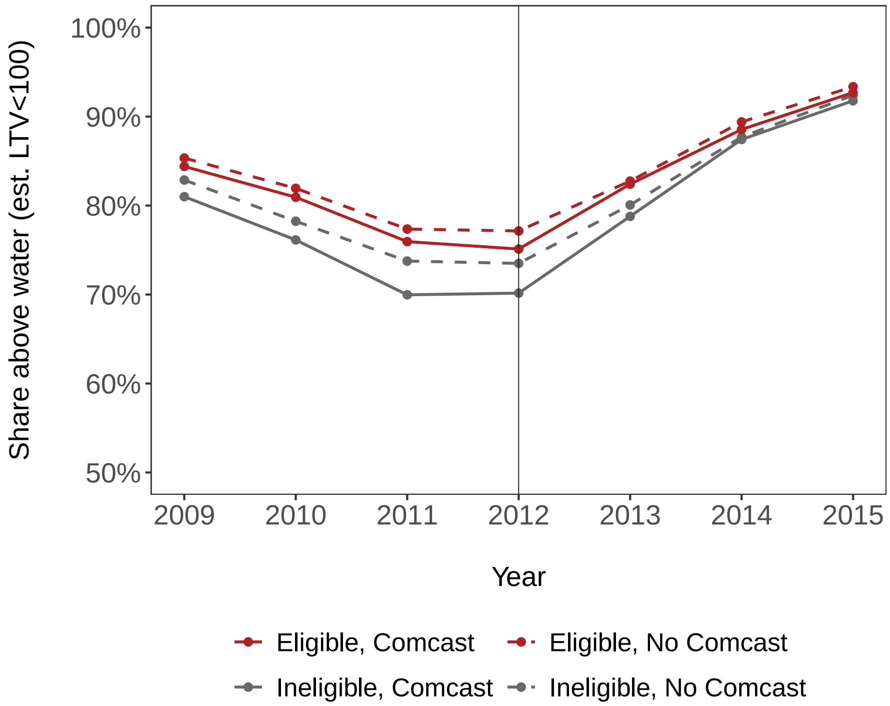


Figure A.1: Estimated Home Equity by Year, Eligibility, and Comcast Coverage

Notes: This figure plots the estimated share of mortgages with positive home equity from 2009 through 2015. Comcast and no Comcast use the December 2012 50 percent census tract coverage classification. Current combined LTV is estimated by updating origination LTV with census tract house price indices. The vertical line marks the introduction of Internet Essentials in 2012. Loan-year observations without a usable current LTV estimate are excluded.
Source: HMDA, Fannie Mae, Freddie Mac, Federal Housing Finance Agency.

Table A.5

Matched Sample versus Full HMDA Refinance Universe

	Eligible		Ineligible	
	Matched	Full HMDA	Matched	Full HMDA
Household income ($000s)	25.59	25.63	51.28	51.30
Loan amount ($000s)	123.48	115.13	160.53	157.60
Black (%)	5.74	6.70	4.15	4.72
Hispanic (%)	13.45	13.28	8.99	9.53
Male primary applicant (%)	45.18	43.67	56.80	55.26
Observations	26,592	119,125	33,419	135,957
Matched share of full HMDA refi (%)	22.32		24.58	

Notes: This table compares the matched GSE-HMDA loan-level sample with the full HMDA refinance universe for the same large central metropolitan census tracts and 2009 through 2011 origination window. GSE-only variables, such as credit score and combined LTV, cannot be compared with raw HMDA. The bottom row reports the matched share of HMDA originations within each eligibility group.

Table A.6

Optimal Refinancing Targeting

	Prepayment			
	Baseline (1)	In the money (2)	Not in the money (3)	Deepest tercile (4)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.0284**	0.0219*	0.0128	0.0468**
	(0.0134)	(0.0133)	(0.0467)	(0.0198)
Loan characteristics controls	✓	✓	✓	✓
Fixed effects	✓	✓	✓	✓
Observations	214,364	200,694	9,596	132,970
Adjusted R-squared	0.21	0.21	0.16	0.25

Notes: This table applies the optimal refinancing rule of Agarwal et al. (2013). All columns use $Eligible_{i,c,t} \times Comcast_c \times Post_t$, with the continuous December 2012 census tract coverage share. Columns 2 and 3 split loans by whether the 2012 rate differential exceeds the loan-specific refinancing threshold. Column 4 restricts the sample to the deepest tercile above that threshold and does not impose the baseline credit score restriction. Loan controls and fixed effects are the same across columns. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.7

Covariate Balance at Origination by Comcast Coverage

	Comcast	No Comcast	Norm. diff.	t-stat
A. Eligible households				
Household income ($000s)	25.00	24.62**	0.079	2.29
Loan amount ($000s)	92.67	85.71*	0.179	1.83
Interest rate (%)	6.20	6.18	0.024	0.46
Combined loan-to-value (%)	78.17	78.54	-0.017	-0.28
Debt-to-income (%)	36.10	36.13	-0.002	-0.16
Credit score	726.90	726.44	0.007	0.19
Black (%)	11.12	8.77	0.078	0.97
Hispanic (%)	16.68	18.61	-0.051	-0.38
Male primary applicant (%)	48.01	49.24	-0.025	-0.68
Multiple applicants (%)	11.89	13.81*	-0.057	-1.77
Loan term (months)	337.97	337.81	0.003	0.14
B. Ineligible households				
Household income ($000s)	46.35	46.40	-0.019	-0.74
Loan amount ($000s)	146.81	132.17**	0.277	2.28
Interest rate (%)	6.07	6.10	-0.049	-1.58
Combined loan-to-value (%)	80.97	80.71	0.014	0.20
Debt-to-income (%)	37.35	36.97	0.032	0.97
Credit score	734.60	732.08	0.045	0.98
Black (%)	7.33	6.53	0.032	0.66
Hispanic (%)	10.59	12.11	-0.048	-0.56
Male primary applicant (%)	55.71	56.51	-0.016	-0.66
Multiple applicants (%)	24.46	27.67*	-0.073	-1.65
Loan term (months)	345.94	344.81	0.023	0.77

Notes: This table reports loan and borrower characteristics at origination in the matched GSE-HMDA panel. Panels A and B report eligible and ineligible households, respectively, split by the December 2012 50 percent census tract coverage classification. Normalized differences follow Imbens and Rubin (2015). The reported t-statistics use PUMA-clustered standard errors. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table A.8

Prepayment Effects by 2012 Home Equity Position

	Prepayment		
	Full sample	Above water (LTV<95)	High LTV (LTV≥95)
	(1)	(2)	(3)
$Eligible_{i,c,t} \times Comcast_c \times Post_t$	0.028**	0.028	0.054*
	(0.013)	(0.018)	(0.030)
Mean of dependent variable (Eligible)	41.99	45.63	34.28
Mean of dependent variable (Ineligible)	57.93	63.27	48.11
Loan characteristics controls	✓	✓	✓
Fixed effects	✓	✓	✓
Observations	214,364	114,572	47,182
Adjusted R-squared	0.21	0.19	0.28

Notes: This table reports prepayment estimates after splitting the loan-level sample by estimated combined LTV in 2012. All columns use $Eligible_{i,c,t} \times Comcast_c \times Post_t$, with the continuous December 2012 census tract coverage share. Current LTV is constructed from origination LTV and census tract house price indices. Column 2 restricts to loans with estimated LTV below 95 and column 3 to loans at or above 95. Loan controls and fixed effects are the same across columns. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

C Additional Falsification Tests

C.1 Alternative Eligibility Thresholds

Program eligibility depends on household income and family composition, while HMDA and the GSE files do not report family size. I therefore test whether similar income comparisons above the eligibility threshold produce the same refinancing pattern. Column 2 of Table 7 assigns a placebo treated group at the five- to six-person income band and a placebo control group at the seven- to eight-person band. The estimate is statistically insignificant. Expanding the baseline control group through the seven-person threshold also leaves the main result materially unchanged.

Figure A.2 traces the treatment effect across fixed income bins. The largest estimates occur among the lowest-income households and decline toward the eligibility threshold. The pooled estimate above the paper's control band is indistinguishable from zero.

C.2 Placebo Internet Service Providers

I also replace Comcast coverage with AT&T and Charter coverage, neither of which was associated with a comparable low-income broadband program during the sample period. Table A.9 uses the continuous provider coverage share, matching the treatment definition in the main specification. The AT&T and Charter estimates are small and statistically indistinguishable from zero.

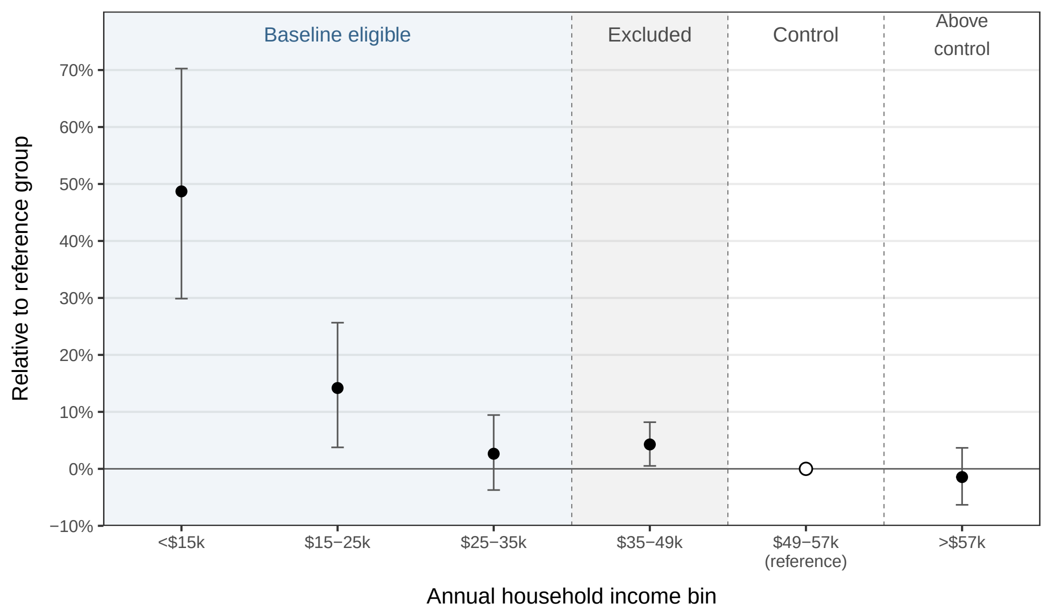


Figure A.2: Treatment Effects Across the Household Income Distribution

Notes: This figure reports exact percentage changes from a PPML regression using six fixed household income bins. The $49,000 to $57,000 control band is the omitted category. The continuous December 2012 census tract coverage share is interacted with the post period and each income bin. The regression includes income-bin-by-tract, income-bin-by-year, and tract-by-year fixed effects. Error bars show 95 percent confidence intervals based on standard errors clustered by PUMA. Shading marks the baseline eligible range and the excluded band.
Source: HMDA, Fannie Mae, Freddie Mac.

Table A.9

Placebo Internet Service Providers

	Refinance originations		
	Comcast	AT&T	Charter
	(1)	(2)	(3)
$Eligible_{i,c,t} \times Provider_c \times Post_t$	0.078***	0.006	-0.010
	(0.026)	(0.023)	(0.059)
Mean of dependent variable (Eligible)	6.48	6.48	6.48
Mean of dependent variable (Ineligible)	6.09	6.09	6.09
Eligibility group controls	✓	✓	✓
Fixed effects	✓	✓	✓
Observations	71,660	71,660	71,660
Adjusted R-squared	0.65	0.65	0.65

Notes: This table compares Comcast with AT&T and Charter coverage in refinance origination regressions. All three columns use the continuous December 2012 provider coverage share. The sample covers large central metropolitan census tracts from 2009 through 2015. All specifications use PPML with the same controls and fixed effects, and standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

D Household Survey Evidence

D.1 Design

The ACS observes both mortgage payments and household income, allowing me to measure mortgage payment burdens directly. It also reports family size and the presence of children, which I use to define program eligibility. Household location is observed at the PUMA level, so this analysis is less geographically granular than the HMDA design.

I estimate

$$\begin{aligned} m_{i,p,t} = \alpha + \beta(Eligible_{i,p,t} \times Comcast_p \times Post_t) + Z'_{i,p,t}\Phi \\ + \rho_1(\lambda_t \times \gamma_p) + \rho_2(Eligible_{i,p,t} \times \lambda_t) + \rho_3(Eligible_{i,p,t} \times \gamma_p) + \epsilon_{i,p,t}, \end{aligned} \tag{D.1}$$

where $m_{i,p,t}$ is the ratio of mortgage payments to income. $Eligible_{i,p,t}$ follows the household-level definition based on income and a school-aged child. $Comcast_p$ is the continuous share of the PUMA population served by Comcast as of December 2012. I restrict the sample to households that have lived in their current residence for more than three years and include household controls. Standard errors are clustered by PUMA.

The ACS also permits a comparison that does not require a third difference. I restrict the sample to households that directly satisfy program eligibility and compare outcomes across the continuous PUMA coverage distribution before and after 2012. I repeat this comparison for low-income households without a school-aged child, which provides a placebo group matched on income.

D.2 Mortgage Payment Burdens

Table A.10 reports both designs. In the triple differences specification, Internet Essentials reduces mortgage payments relative to income by 1.3 percentage points. The eligible-only comparison yields a decline of 1.7 percentage points, while the estimate for the placebo group matched on income is statistically insignificant. The ACS does not identify the refinanced mortgage itself, so I interpret these estimates as changes in household mortgage payment burdens rather than as a separate lifetime savings calculation.

D.3 Renters

Renters have no mortgage refinancing decision, so rental payments provide a falsification outcome. Column 7 of Table 7 shows no meaningful change in rent payments following Internet Essentials. This null is consistent with the mortgage payment results operating through refinancing rather than a broader change in housing costs.

Table A.10

Broadband Access and Mortgage Payment Burden

	All ineligible control	Eligible-only DD
	Payment-to-income (1)	Payment-to-income (2)
$Eligible_{i,p,t} \times Comcast_p \times Post_t$	-0.013***	
	(0.003)	
$Comcast_p \times Post_t$		-0.017***
		(0.004)
Placebo: income-matched control		-0.007
		(0.004)
Mean of dependent variable (Eligible)	37.71	37.71
Mean of dependent variable (Ineligible)	28.96	38.62
Household controls	✓	✓
Fixed effects	✓	✓
Observations	602,817	130,564
Adjusted R-squared	0.56	0.57

Notes: This table reports two estimates of the effect of Internet Essentials on mortgage payment burdens. Column 1 uses the household-level triple differences specification with all ineligible households as controls. Column 2 uses the eligible-only difference-in-differences specification. Both columns use the continuous December 2012 PUMA coverage share. The placebo row repeats the eligible-only comparison for low-income households without a school-aged child. Coefficients are expressed as changes in the ratio; dependent-variable means are reported in percent. The sample covers ACS homeowners from 2008 through 2015 who have a mortgage and have lived in the current home for at least three years. Household controls and the corresponding fixed effects are included. Standard errors are clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

E Supplementary Figures and Tables

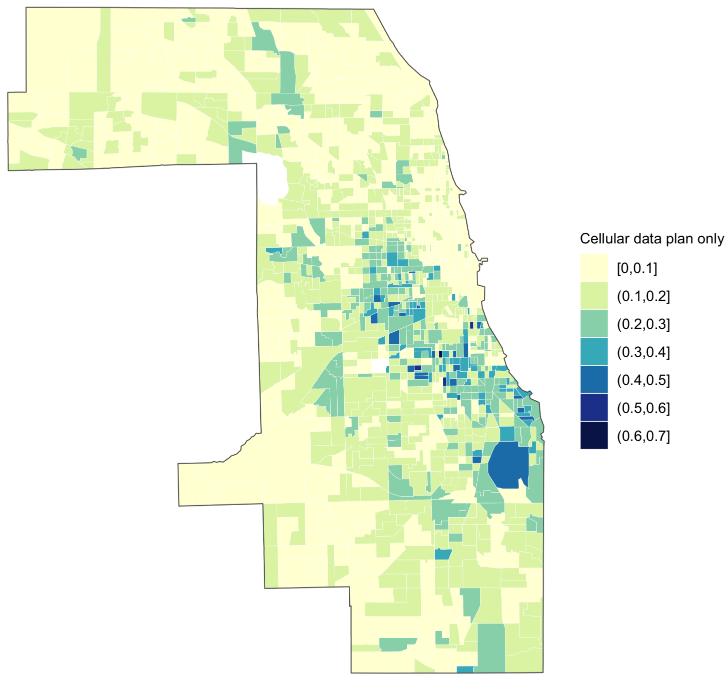


Figure A.3: Cellular Substitution in Cook County, IL

Notes: This figure maps the fraction of households without a home broadband subscription that use a cellular data plan only. Each observation is a census tract in Cook County, Illinois.
Source: 2018–2022 ACS 5-year estimates.

Figure A.4: Internet Essentials Online Learning Center

Notes: This figure reproduces the Internet Essentials Online Learning Center in 2015.
Source: Comcast Corporation (2015). Archived website accessed using the Wayback Machine.

Table A.11

Internet Essentials and Home Internet Use

	Estimate
Subscriber household characteristics	
Average age	39
Average household size	4
Female (%)	74
Married (%)	46
High school diploma or less (%)	51
Income less than $40,000 (%)	78
Race/ethnicity	
White (%)	44
Hispanic (%)	43
Black or African-American (%)	33
Demand factors and usage	
Children's schoolwork (%)	98
Finding general information (%)	92
E-mail (%)	80
Social networking (%)	71
Paying bills (%)	63
Access to banks and financial institutions (%)	65
Access to government services (%)	52
Access to employment/job search (%)	49

Notes: This table reports characteristics and internet use among Internet Essentials survey respondents between 2012 and 2014. Estimates come from surveys administered by the Comcast Technology Research & Development Fund and may not describe the household head.

Table A.12

Income Thresholds for Internet Essentials Eligibility

	Eligible		Ineligible	
Year	Min	Max	Min	Max
2008	0	32.56	45.88	52.54
2009	0	33.87	47.71	54.63
2010	0	33.87	47.71	54.63
2011	0	34.28	48.42	55.48
2012	0	35.32	49.97	57.30
2013	0	36.13	51.01	58.44
2014	0	36.61	51.63	59.15
2015	0	37.17	52.56	60.26
Average	0	34.98	49.36	56.55

Notes: This table reports the annual income thresholds used to define eligible and ineligible households. Eligible households have income below 185 percent of the FPL for a three-person household. The ineligible band runs from the corresponding five-person threshold to the six-person threshold. Dollar amounts are in thousands.

Table A.13

Urban Metropolitan Statistical Areas by Comcast Coverage

		Census tracts	2010 population (millions)
Comcast			
1	Chicago–Naperville–Joliet, IL	337	2.740
2	Houston–Sugar Land–Baytown, TX	337	2.686
3	Seattle–Bellevue–Everett, WA	232	1.851
4	San Jose–Sunnyvale–Santa Clara, CA	129	1.561
5	Minneapolis–St. Paul-Bloomington, MN-WI	246	1.486
6	Oakland-Fremont–Hayward, CA	245	1.423
7	Miami–Miami Beach–Kendall, FL	77	1.289
8	Sacramento–Arden-Arcade–Roseville, CA	94	1.232
9	Philadelphia, PA	195	1.053
10	Pittsburgh, PA	230	0.932
11	Salt Lake City, UT	87	0.923
12	San Francisco–San Mateo–Redwood City, CA	131	0.742
13	Portland–Vancouver–Beaverton, OR–WA	100	0.719
14	Detroit–Livonia–Dearborn, MI	176	0.693
15	Washington–Arlington–Alexandria, DC–VA–MD–WV	107	0.689
No Comcast			
1	Los Angeles–Long Beach–Glendale, CA	1,233	9.200
2	New York–White Plains–Wayne, NY–NJ	939	4.154
3	Santa Ana–Anaheim–Irvine, CA	144	3.007
4	San Diego–Carlsbad–San Marcos, CA	216	2.926
5	Phoenix–Mesa–Scottsdale, AZ	178	2.041
6	Dallas–Plano–Irving, TX	160	1.976
7	Tampa–St. Petersburg–Clearwater, FL	150	1.653
8	Fort Worth–Arlington, TX	87	1.507
9	Chicago–Naperville–Joliet, IL	365	1.465
10	Riverside–San Bernardino–Ontario, CA	83	1.371
11	Las Vegas–Paradise, NV	61	1.170
12	San Antonio, TX	94	1.127
13	Columbus, OH	139	0.980
14	Cleveland–Elyria–Mentor, OH	143	0.933
15	Austin–Round Rock, TX	39	0.875

Notes: This table lists the 15 largest Comcast and no Comcast metropolitan areas by population served. Comcast census tracts have at least 50 percent of their 2010 population in census blocks where Comcast broadband was available as of December 2012; no Comcast census tracts have less than 50 percent. Populations are aggregated from the 2010 Census.

Table A.14

Refinancing Outcomes and Local Eligible Household Density

	Number of originations	
	Ineligible	Eligible
	(1)	(2)
$HighEligShare_c \times Comcast_c \times Post_t$	0.0695	0.0530
	(0.0619)	(0.0640)
Tract-year controls	✓	✓
Tract and year fixed effects	✓	✓
Observations	36,211	36,211

Notes: This table tests for information spillovers using local eligible-household density. High eligible share indicates that a tract's share of refinance applications from eligible households in the period before the program is above the full sample median. The regressions use the continuous December 2012 census tract coverage share and include the Comcast-by-post and high-eligible-share-by-post terms alongside the triple interaction. Column 1 reports the interaction for ineligible households and column 2 for eligible households. Both specifications use refinance originations, tract and year fixed effects, and controls for group income and home value. Estimates use PPML with standard errors clustered by PUMA. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.