Crowd-Out or Affordability? The Lifeline Expansion's Effect on Wireless Service Spending*

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Abstract

Public subsidization of private goods often leads to crowd-out, reducing private spending. This effect is intended for a policy like the 2008 Lifeline phone subsidy expansion, which aimed to increase *affordable* access to services. I examine the effects of this policy on households' self-reported wireless phone service spending in the Consumer Expenditure Survey. Using state-level variation in policy implementation and triple-differences event study methods, I estimate that the expansion reduced households' wireless service spending by more than 100 percent of subsidy payments. I document that the expansion led to a separate, competitive market for providers catering to low-income households. Consequently, higher-quality subsidized services crowded out lower-quality unsubsidized options, saving households more than an equivalent cash transfer. This highlights how market segmentation and competition can magnify a targeted subsidy's impact.

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1 INTRODUCTION

Prior to 2008, the Federal Lifeline Program was a stable and relatively uncontroversial piece of U.S. telecommunications policy, subsidizing the telephone bills of 6.9 million low-income households. By 2012, enrollments rose to 17.1 million households, almost entirely through the efforts of newly-approved private providers offering free cell phone service. Annual program costs grew from $700 million to over $2 billion, accompanied by concern over costs and waste, attracting significant media and legislative attention.[1] The Federal Communications Commission (FCC) responded with reforms, but questions about the program's effects remain. In particular, how did the expansion to free cell phone services affect those households receiving the benefits?

The traditional goal of universal service programs like Lifeline is to ensure all households have access to telephone services. More recent legislation broadened this goal to ensuring *affordable* access to phone services.[2] Success on the first measure would be seen in higher phone ownership rates among eligible households. A previous study of the Lifeline wireless expansion, Ukhaneva (2015), found a very small effect on phone ownership rates, estimating that only one in twenty enrollments went to households that would not have otherwise had phone service.[3] This is understandable given the very high phone ownership rates of even the lowest-income U.S. households. The aim of this paper is to evaluate the expansion's success in accomplishing the second goal of affordable access, as measured by households'

[1] "Millions Improperly Claimed U.S. Phone Subsidies," The Wall Street Journal, February 11th, 2013; "Who Gets Rich Off 'Free' Government Phones?" CNN Money, October 26th, 2012; "'Obama Phones' Subsidy Program Draws New Scrutiny On The Hill," The Washington Post, April 9th, 2013; H.R. 176 - Stop Taxpayer Funded Cell Phone Act of 2011, introduced in 113th Congress, January 4th, 2013.

[2] Universal Service has been part of the FCC's mandate since its formation under the Communications Act of 1934. The updated language explicitly stating that "quality services should be available at just, reasonable, and affordable rates" was implemented in the Telecommunications Act of 1996.

[3] This is also consistent with the small access gains from landline Lifeline subsidies found by Ackerberg et al. (2014). Additional pre-expansion Lifeline studies have focused on explaining low take-up rates (Burton, Macher and Mayo, 2007; Hauge, Jamison and Jewell, 2007, 2008; Hauge, Chiang and Jamison, 2009), or estimating landline-wireless substitution patterns (Rodini, Ward and Woroch, 2003; Ward and Woroch, 2010; Macher et al., 2012).

out-of-pocket spending.[4]

I exploit geographic differences in the timing of provider entry to estimate the effect of the expansion on households' wireless service spending, based on self-reported data from the Consumer Expenditure Survey (CEX). Using difference-in-differences and triple-differences event study methods, I find that eligible households in states with free Lifeline have significantly lower wireless spending, with the per-household reduction exceeding 100 percent of the per-household subsidy spending. This could be interpreted as total crowd-out; in the absence of the program, the enrolled households would have purchased unsubsidized service anyway. It also suggests that the expansion accomplished the broader goal of the program, making phone service more affordable.

The size of the estimated out-of-pocket spending reduction suggests that households may value free Lifeline service above the cost of the subsidy. Several factors make this result possible, including price discrimination based on income, quality competition between Lifeline providers, and reduced transaction and hassle costs. Like many other industries, wireless service markets typically feature a form of price discrimination in which consumers buying higher quality service plans — measured in units of minutes, texts, or data — pay a lower price per unit.[5] The intuition is that these markets feature a mix of high- and low-income households, to whom firms market high quality (expensive) and low quality (cheaper) plans, respectively. To ensure that high-income households purchase the more expensive plans, firms lower the relative quality of their cheaper plans. As a result, households buying cheaper wireless service plans (in total dollar terms) pay a higher price-per-minute than households buying more expensive plans.

[4]The FCC has cited the goal of affordability as an unaccounted for metric in previous studies of the Lifeline program (GAO, 2015).

[5]The same logic applies for the discounts on larger quantities seen in many markets, another example of second-degree price discrimination. See Maskin and Riley (1984) for a discussion of these incentives in a monopoly context and Stole (2007) for a literature review of price discrimination models in competitive markets. Armstrong and Vickers (2001) and Rochet and Stole (2002) provide theoretical results showing that the monopoly predictions of downward quality (or upward price) distortions for low willingness-to-pay customers also hold in imperfectly competitive markets. For empirical evidence, see Busse and Rysman (2005); McManus (2007); Asplund, Eriksson and Strand (2008), and the literature survey by Lambrecht et al. (2012).

By making household income verifiable through proof-of-eligibility rules, the design of the Lifeline program allows a transition toward more explicit market segmentation in which high-income households can be legally barred from purchasing the low-cost plans.[6] Competing firms can then raise the quality of services offered to low-income households, without the risk of high-income customers substituting to these cheaper plans. I document service offerings consistent with this theory, showing that the quality of free Lifeline services paid for by the $9.25 monthly subsidy exceeds that of similarly priced unsubsidized plans. A typical free Lifeline plan offered 250 minutes of service during this period, compared to the 100 minutes or fewer that could be purchased at equivalent cost.

The entry of new providers offering free wireless service changed the provision of Lifeline benefits in other important ways. By reducing households' out-of-pocket costs to zero, the expansion lowered transaction and hassle costs (no monthly billing), and likely reduced turnover by replacing the less predictable stream of payments from households with steady government subsidies. These changes were accompanied by aggressive marketing campaigns as providers competed to enroll households. Transaction costs and awareness have important effects on take-up across many benefit programs (Currie, 2004), and Lifeline saw huge growth in enrollments during this period. While these changes in isolation may have translated to increased take-up and profits, the resulting competition on service quality, no longer constrained by the incentives to dissuade high-income customers, led to higher service quality and large household savings. The number of providers, program awareness, and service quality all increased over time, which helps explain the pattern of enrollment growth, as state-level enrollments (and estimated treatment effects) continued to increase for several years after the first provider enters. In total, these changes suggest a substantial impact of the Lifeline expansion on wireless service affordability.

In addition to measuring the Lifeline expansion's effect on household spending, this paper adds to the broader empirical literature on the public provision of private goods, particularly

[6] This is a form of third-degree price discrimination, commonly seen in pricing which provides discounts to specific populations like students or the elderly.

regarding how the provision mechanism affects crowd-out and consumer benefits. Numerous examples of in-kind provision, in which the government directly provides the good or service, have been studied in markets for health insurance (Cutler and Gruber, 1996; Card and Shore-Sheppard, 2004; Gruber and Simon, 2008; Dague et al., 2011; Koch, 2013), lotteries (Kearney, 2005), radio (Berry and Waldfogel, 1999), higher education (Cellini, 2009; Cohodes and Goodman, 2014), and substance abuse treatment (Cohen, Freeborn and McManus, 2013).[7] More similar to the Lifeline provision mechanism, crowd-out effects have also been found in programs where subsidies are given to competing private providers based on the number of households enrolled. Eriksen and Rosenthal (2010) find that the increased construction of tax-subsidized rental housing units was entirely offset by a reduction in unsubsidized construction. In a study of state preschool provision, Bassok, Fitzpatrick and Loeb (2014) find that a private provider subsidy increases total consumption more than an in-kind program, though there is still substantial crowd-out of consumption in the unsubsidized market. All of these studies document the limits of government programs to increase private goods consumption due to crowd-out. Both Eriksen and Rosenthal (2010) and Bassok, Fitzpatrick and Loeb (2014) note the possible benefit of increased quality due to subsidies, but are unable to observe quality directly. By studying a provider subsidy in the wireless service market, which features observable quality measures and market segmentation, this paper further explores this added dimension of potential program impacts.

The wireless Lifeline expansion created a set of subsidized goods that is not available in the unsubsidized market. The free wireless service offered by competing private providers did crowd out spending, which on its own could suggest that Lifeline is operating simply as a transfer program, with potential waste and inefficiency as described in Conkling (2015). However, the expansion also reduced the transaction costs of wireless phone ownership and resulted in a separate, competitive market for providers catering to low-income households; a

[7] See Payne (2009) for a literature review. Kearney (2005) and Hong (2013), which estimates the effect of Napster's introduction on music spending, are most empirically similar to this paper. Both use difference-in-differences style models on CEX data.

market in which firms previously had incentives to reduce service quality. In a sense, higher quality free Lifeline plans are crowding out lower quality unsubsidized plans. Based on a dollar-for-dollar measure then, it is conceivable for the crowd-out rate to exceed 100 percent, with the program saving households more than an equivalent cash transfer.

Calculating the overall welfare implications of the Lifeline expansion would require additional data on the prices and market shares of paid wireless plans, administrative and legal costs, and the efficiency cost of raising funds. The contributions here are to document the large reduction in household wireless service spending due to the Lifeline expansion, and to highlight the potential for market segmentation, competition, and reduced transaction costs to magnify the impact of a targeted subsidy program.

Section 2 provides background on the Lifeline wireless expansion, evidence on service quality, and a discussion of the crowd-out and price discrimination mechanisms. The data is described in Section 3. Section 4 provides summary statistics and basic difference-in-differences comparisons. Section 5 outlines the empirical regression model. Section 6 describes the results, and Section 7 discusses some potential confounding factors and robustness. Section 8 concludes.

2 BACKGROUND AND MECHANISMS

2.1 The Lifeline Wireless Expansion

The Lifeline program was created in 1984 with the goal of achieving universal access to telephone service in the United States. Lifeline provided a subsidy of $9.25 per month to landline service providers for each low-income household they enrolled, with the requirement that the full value of the subsidy be passed through to the household in the form of a discounted monthly bill.[8] In practice, this requirement meant that providers typically offered

[8]There was some small variation in the size of the subsidy across states prior to 2012, at which point $9.25 became the national standard.

Lifeline customers one of their standard unsubsidized service plans, with the \$9.25 subsidy subtracted from the price. By rule, only one enrollment is allowed per household. The program's basic structure has remained constant, but the set of firms allowed to participate has expanded.

Traditionally, participation was restricted to landline and facilities-based wireless carriers – those who owned their own towers or other infrastructure. By the early 2000s, Lifeline plans were available from the major national wireless carriers (AT&T, Verizon, etc.) but not from providers in the growing low-cost prepaid wireless market. These prepaid carriers (TracFone, Virgin Mobile, etc.) were almost exclusively wireless resellers, meaning that they leased minutes from the facilities-based carriers and resold them under their own brands. These carriers often catered to the low-income segment of the market, and TracFone in particular actively petitioned the FCC to participate in the Lifeline program. In 2005, the FCC granted TracFone forbearance from the facilities-based requirement, though they did not approve TracFone as a carrier until the middle of 2008.[9] Throughout 2009, numerous states (though importantly not all) began approving wireless resellers' requests to participate in Lifeline.[10] The FCC's approval of TracFone covered eleven states which had deferred their carrier designation authority, while all other states' public utilities commissions maintained the authority to approve or deny carrier applications.[11]

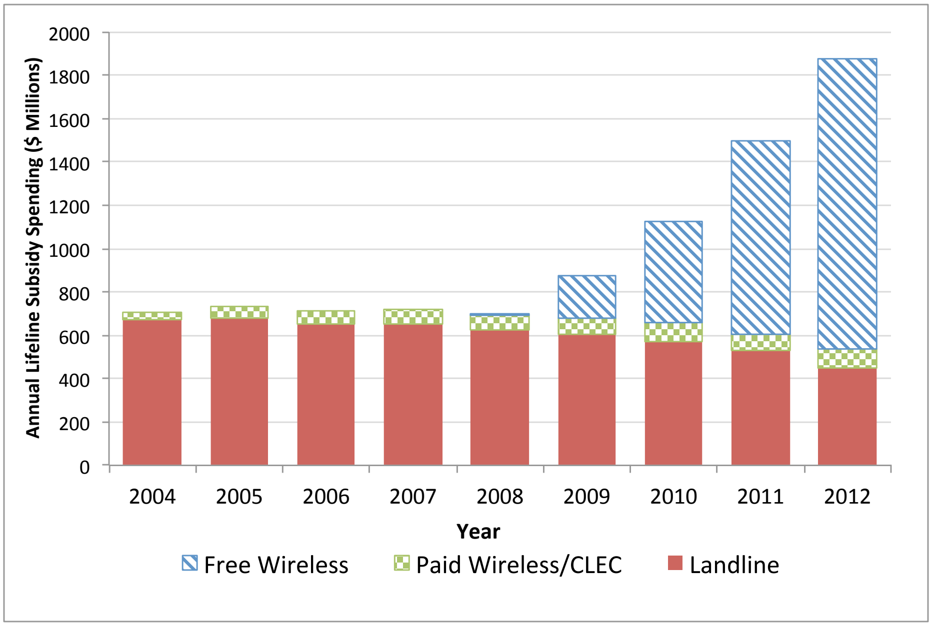
The entry of wireless resellers greatly increased Lifeline enrollment. As shown in Figure 2.1, annual program spending had been fairly flat at around \$700 million prior to 2009, but rose to over \$2 billion by 2012, driven only by the enrollments of new entrants offering free

[9]TracFone Forbearance Order, September 6th, 2005, and TracFone ETC Designation, April 9th, 2008 in FCC Docket No. 96-45.

[10]In addition to federal Lifeline subsidies, some states offered their own supplemental subsidies. However, the large prepaid wireless carriers did not seek or receive funding from these state subsidies, perhaps in an attempt to expedite their regulatory approval (Ex. Comments of TracFone Wireless, Inc., April 21st, 2011 in FCC Docket No. 96-45 stating that it "has never received support from any state fund," and California's Resolution T-17284, May 5th, 2011, stating that Virgin Mobile "is not seeking ... California State Universal Service support."

[11]The eleven states deferring to the FCC were AL, CT, DC, DE, FL, MA, NC, NH, NY, TN, VA. Throughout this paper I refer to DC as a state for brevity.

Figure 2.1: Annual Lifeline Program Spending



Note: Annual subsidy claims to the Universal Service Administrative Company. Excludes Tribal Lifeline claims, and all claims from Oklahoma and Alaska. Free Wireless carriers are defined by author as those carriers offering a wireless service plan at no monthly cost.

monthly service.[12] In violation of program rules, some households signed up for multiple enrollments Conkling (2015), which will be considered when interpreting the estimation results.

2.2 Mechanisms: Service Quality, Crowd-Out, and Price Discrimination

The entry of the wireless resellers created a new wireless service option for low-income households which had previously not existed. These new Lifeline plans were only available to eligible households. Over the period studied in this paper, these new Lifeline plans exceeded the quality – as measured in minutes of service included – of unsubsidized plans available for purchase in the market.

[12]The spending totals in Figure 2.1 and throughout the paper do not include Alaska and Oklahoma, due to their high participation rate in the Tribal Lifeline program, which provides more generous subsidies.

The free business model favored by wireless resellers offered customers a free handset and an allotment of minutes and text messages each month at no out-of-pocket cost. The earliest free Lifeline entrants typically offered 68 minutes of service per month, with no text messages included. This was roughly in line with similarly priced unsubsidized plans at the time.[13] By 2010, additional providers entered the free Lifeline market offering 200 minutes per month, and incumbent firms responded by offering 250 minutes per month. By 2012, all major free Lifeline providers were offering 250 minutes per month, and began to compete on the number of included text messages.[14] Over time, providers continued to increase the quality of their subsidized Lifeline plans relative to their equivalently priced unsubsidized plans. As of April 2015, the largest Lifeline carriers, TracFone and Virgin Mobile, offered subscribers a free handset plus 250 minutes per month and unlimited free text messages. The lowest-cost unsubsidized plans from the companies offered 50 minutes for $10 and 400 minutes for $20, respectively, with no handset or text messages included.[15]

This difference in quality between Lifeline plans and unsubsidized plans is the mechanism I propose for how crowd-out could exceed 100 percent, allowing households to reduce spending by more than the size of the $9.25 monthly subsidy. A key point is that free Lifeline plans are an additional option available to eligible consumers, alongside any unsubsidized plans in the market.

Consider a simplified hypothetical market where consumers have just two unsubsidized plan options: a Low quality plan offering 50 minutes for $10 and a High quality plan offering 1000 minutes for $40. In addition to these two options, eligible consumers have a subsidized Lifeline option, offering 250 minutes for $0.

[13] For a discussion comparing similar subsidized and unsubsidized plans at the time, which notes that "the TracFone Lifeline plan would be equivalent to TracFone's retail Value Plan which cost $9.99 per month for 50 minutes of airtime," see Comments of the National Association of State Utility Consumer Advocates on TracFone's Petition for Waiver, July 6th, 2009 in FCC Docket No. 03-109.

[14] Details on individual providers' offerings can be found in the Compliance Plans available in FCC Docket Nos. 09-197, 11-42.

[15] See TracFone's www.safelinkwireless.com and Virgin Mobile's www.assurancewireless.com for current Lifeline rates; and https://www.tracfone.com/direct/ValuePlans and www.virginmobileusa.com/cell-phone-plans/paylo-plans/overview for unsubsidized plans.

Consumers always prefer having more minutes, but the additional benefit diminishes as the number of minutes increases. Consumers choose some combination of the available plans to suit their needs, though a maximum of one Lifeline option per eligible consumer is allowed. Before the introduction of Lifeline, a consumer who demands a relatively small number of minutes may have purchased two Low quality plans, receiving 100 minutes for $20. Given the plan prices, a second consumer who demands a larger number of minutes (anything beyond 200) would be best off purchasing a High quality plan, receiving 1000 minutes for $40. After Lifeline is introduced, the first consumer, if eligible, can instead enroll in a single Lifeline plan, reducing spending by $20. Depending on her preferences, the second consumer may find it sufficient to enroll in one Lifeline plan and purchase one Low quality plan, reducing spending by $30 while still receiving 300 minutes.

In this example, both consumers reduce spending by more than the value of the Lifeline subsidy. The total consumption of minutes increases for the first customer and decreases for the second, stemming from the tiered pricing of the plan options. I am unable to observe the number of minutes consumed by households, so this paper focuses on out-of-pocket spending on wireless service.

While the gap in service quality between Lifeline and unsubsidized plans helps explain the large spending reductions found in this paper, it raises the question of why the gap exists. Some of the gap is likely explained by reduced billing costs and customer turnover, saving firms money which can be passed on to consumers. However, while the generosity of Lifeline plans has increased over time, the lowest-cost unsubsidized plans have been largely static. Competition appears to have passed through falling costs of wireless service in the free Lifeline market, but not in the lowest-cost unsubsidized market. This pattern is consistent with reduced quality for the low end of the unsubsidized market due to second-degree price discrimination incentives.

Returning to the simple example above, based on costs alone, firms could likely offer a Low quality plan with more than 50 minutes for $10, as evidenced by the better rates on their

Lifeline and High quality plans. While raising quality would help a firm win more consumers who demand small numbers of minutes, it risks inducing the firm's own higher demand consumers to switch to the cheaper plan, potentially reducing total profits. However, if most consumers with higher demand also have high incomes, the enforcement of eligibility rules prevents them from enrolling in more generous Lifeline plans, mitigating firms' incentive to reduce quality offered to households with smaller demand for minutes (and likely lower incomes).[16]

Similar empirical patterns have been observed across a range of imperfectly competitive markets (Busse and Rysman, 2005; McManus, 2007; Asplund, Eriksson and Strand, 2008; Lambrecht et al., 2012). The added element here is the expansion of a means-tested benefit program, allowing firms to market directly to low-income households, without making the product available to households with higher incomes.

The remainder of this paper is devoted to estimating the effect of the program on the intended recipients, in terms of their out-of-pocket expenditures on wireless phone services.

3 DATA

3.1 Household Data on Wireless Spending and Eligibility

The primary data on household characteristics and spending come from the Consumer Expenditure Survey (CEX) public-use microdata for the years 2005 through 2012. The dependent variable of interest is a household's self-reported monthly spending on wireless phone service. New households enter the survey each month throughout the year, and are asked how much they spent on their cell phone bill in each of the previous three months. Households are told to "include any bills you receive or pay online or have automatically deducted." Be-

[16] The exact trade-off for the firm depends on how many high demand customers they serve, and what share of them are Lifeline eligible. These considerations, along with brand reputation concerns, may help explain why the major national wireless carriers did not offer free Lifeline plans directly, but rather participated indirectly by leasing use of their service towers to prepaid providers.

ginning in 2007, households are then asked for the amount spent on prepaid cellular minutes "not already reported" over the full three month period. The format of the questionnaire should ensure that wireless expenditures are not double-counted, but it is not clear that the division between reported billed and prepaid spending will match industry conventions. Prepaid wireless service providers often offer automatic monthly renewal options charged to a saved payment method, which households may reasonably report under the first spending category. As a result, I calculate a variable for average monthly spending, adding up all of the reported expenses (billed and prepaid) and dividing by three. Spending is constant across the three months for the vast majority of households. I also generate an indicator variable based on whether the household reports any positive wireless spending. The survey does not record whether a household has a cell phone, so a household reporting zero spending could have no cell phone service, or could have only a free Lifeline subscription. Both the average monthly spending and an indicator for any spending will be used as dependent variables.[17]

Although there is a small panel aspect to the surveys, I keep only the first observation for each household. This avoids selection issues on which households respond to subsequent surveys, and sacrifices little identifying variation due to very high persistence of spending levels within households over time.

The use of self-reported expenditure and income data could lead to concerns about possible reporting errors or biases. Bee, Meyer and Sullivan (2013) surveys the literature on the validity of CEX data, noting that for the Interview portion of the survey (used in this paper), spending on communication and telephone services are close in level and trend to outside expenditure sources such as the Personal Consumption Expenditure (PCE) data from the National Income and Product Accounts.[18] The distribution of household demographics is

[17] Additional details on variable construction and sample restrictions are available in Appendix A.4.

[18] While there is a long tail of high wireless service spending, the primary results do not trim or winsorize the dependent variables due to the potential bias induced by doing so without evidence of over-reporting (Bollinger and Chandra, 2005). Estimates when winsorizing the within-quarter highest one percent of spending households provide very similar results, shown in panel (a) of Appendix Figure A.3.

also found to be close to that seen in the Current Population Survey (CPS).

The quality of reported income is of particular interest for this study, as it a key household characteristic used to calculate Lifeline eligibility based on state-level requirements. States are allowed to set their own income and program participation requirements for Lifeline eligibility, and these were collected from state regulatory websites, program applications, and FCC documents.[19] In practice, most states had the same requirements: households must either be enrolled in a federal benefit program like SNAP, Medicaid, SSI, etc., or have income below 135 percent of the Federal Poverty Level.[20] Participation is only observed for SNAP, Medicaid, and SSI, which are the most common programs used by households to certify eligibility (FCC, 2012). The CEX does not provide any information on whether a household is actually enrolled in Lifeline, so eligible households constitute the intended treatment population.

Previous studies have documented under-reporting of income for households with both the lowest (Meyer, Mok and Sullivan, 2015) and highest (Sabelhaus et al., 2015) incomes. Meyer, Mok and Sullivan (2015) also find that low income households under-report transfers from the other benefit programs that households can use to certify Lifeline eligibility directly. When income under-reporting occurs independently of the under-reporting in transfers, it will lead me to assign some households as eligible who in fact are not. Under-reporting that occurs because transfer income from a program like SNAP or SSI is not being included should be less problematic, as the households should be assigned as eligible due to the program enrollment. At the high end of the income range, Sabelhaus et al. (2015) find that while the number of households reporting incomes over $100,000 in the CEX matches other sources, these households report lower income levels. The assignment of Lifeline eligibility should be unaffected by this type of under-reporting, as these household incomes are well above income-eligibility thresholds. However, since income is a strong predictor of wireless

[19]See Appendix A.1 for a list of sources, and Appendix Table A.1 for state-specific requirements.

[20]Other programs recommended by federal guidelines for eligibility include Low Income Home Energy Assistance Program, National School Lunch Program's free lunch program, Federal Public Housing Assistance, and Temporary Assistance for Needy Families (GAO, 2010).

service spending, all models include cubic terms in income, plus an indicator for reported incomes over \$100,000.[21]

Finally, for confidentiality reasons the CEX suppresses state identifiers for between 14 and 17 percent of observations in each survey year, and re-codes state identifiers for approximately four percent of observations. Given that eligibility rules and Lifeline availability are both determined at the state level, I drop all observations for which state identifiers are suppressed, or from states (Minnesota and Delaware) for which "either all observations from this state have been re-coded or all strata of observations from this state include 're-codes' from other states" based on CEX documentation definitions.[22]

3.2 Total Lifeline Enrollments

Whether eligible households could actually enroll in free Lifeline service depends on whether a free carrier was operating in their state of residence. The dates of Lifeline carrier entry and overall enrollment levels are obtained from the public filings of the Universal Service Administration Company, which handles the subsidy disbursements for the FCC. Carriers report their monthly subsidy claims by state, which is combined with data on per-subscriber subsidy amounts to calculate subscriber counts. In the data, carriers often claim a negligible number of subscribers for the first few months they appear in the filings. If this is indicative of a slow rollout, or a delay in marketing of the program, any effects on spending should not be seen until the service is more broadly available. To account for this, I define free Lifeline as being available in a state once the number of subscribers has exceeded one percent of the eligible population (described below) for three consecutive months. The three month threshold ensures that Lifeline was available for the full reporting period for interviewed

[21]In addition, the small number of households with reported incomes below \$0 are dropped. Panel (d) of Appendix Figure A.3 shows results for a specification estimating the main model with binned income values rather than a polynomial.

[22]I keep observations from states with more limited re-coding in my main sample, but the results are similar when dropping all states with any re-coding (see panel (b) of Appendix Figure A.3).

CEX households.[23]

These subscriber counts are available for all Lifeline carriers, but this paper focuses on the wireless resellers who were able to enter the market after the Forbearance Order was issued by the FCC. The bulk of program growth came from those carriers offering free monthly service, whose identities were determined from their application and compliance filings with state regulatory agencies.

3.3 Eligible Population and Take-Up Rates

A key statistic for interpreting my estimation results and calculating crowd-out is the Lifeline take-up rate. I use the American Community Survey (ACS), which is an annual survey representative at the state and national level, to estimate the number of Lifeline eligible households in each state, each year. Using the same state-level eligibility criteria as with the CEX data, I again define household eligibility based on income relative to the Federal Poverty Level or enrollment in SNAP, Medicaid, or SSI.

Once the number of eligible households has been estimated for a given state and year, I calculate the free Lifeline take-up rate as the number of Lifeline subscribers divided by this count of eligible households. When calculating take-up across multiple states or months, I divide the sum of Lifeline subscribers by the sum of eligible households. To the extent that the ACS has similar income under-reporting as the CEX, my estimates would overstate the number of eligible households, and hence understate the true take-up rate.

3.4 Additional State-Level Controls

Finally, one potential confounding factor with the Lifeline expansion is the timing of the Great Recession. Negative economic shocks that affect households directly, say through their income level or take-up of unemployment benefits, can be accounted for with household-level

[23] Definitions using alternative percentages of the eligible population, fixed numbers of subscribers, or the removal of the three month threshold yield similar results (See Appendix Figure A.4).

characteristics. To allow for the possibility that indirect negative shocks could change wireless spending behavior, I include two additional state-level control variables in the regression specifications: monthly unemployment rates published by the Bureau of Labor Statistics, and a quarterly house price index published by the Federal Housing Finance Agency.

4 SUMMARY STATISTICS AND DIFFERENCE-IN-DIFFERENCES

The basic estimation approach is a difference-in-differences style comparison over a repeated cross-section data set. Eligible households in states with free Lifeline service available are compared to eligible households in states without. The program's effect is estimated from variation in when Lifeline first becomes available; some states had their first free Lifeline provider by the first quarter of 2009, while others had no Lifeline providers during the entire sample period. For each state, Table 1 lists the month that free Lifeline became available, Lifeline eligibility and take-up rates, along with the number of households in my estimation sample.

Of course, the availability of Lifeline in a given state market is not a randomly assigned event, but rather an outcome of the decisions made by carriers and state-level regulators. Carriers may find it more profitable to enter states with high poverty rates or strong preferences for wireless phones. State regulators may be more willing to approve carriers if the perceived benefit to their residents is high. Section 5.1 details how the regulatory approval process introduces some plausibly exogenous variation to treatment timing, as well as how the household characteristics plus state and time fixed effects in the full model help control for many observable and unobservable differences across states.

To construct a basic comparison, I divide the sample into early and late Lifeline availability states. I define early availability states as those with their first free Lifeline provider by the end of 2009, and late availability states as those with no free Lifeline provider until

Table 1: Free Lifeline Availability, Eligibility, and Take-Up by State

State	**Month Lifeline initially available**	**Share of households eligible (2012)**	**Take-up among eligibles (2012)**	**Observations in CEX estimation sample**
FL	February, 2009	34%	34%	4,260
TN	February, 2009	33%	43%	1,058
VA	March, 2009	22%	35%	2,361
MA	May, 2009	28%	34%	1,588
NY	June, 2009	31%	43%	4,461
GA	June, 2009	32%	74%	2,711
MI	July, 2009	33%	45%	2,028
PA	July, 2009	27%	35%	3,766
NC	August, 2009	31%	41%	0
DE	September, 2009	27%	39%	0
AL	October, 2009	34%	39%	1,074
WV	November, 2009	33%	43%	44
LA	November, 2009	35%	84%	1,514
CT	November, 2009	26%	28%	961
DC	November, 2009	31%	51%	197
WI	November, 2009	27%	23%	1,735
NJ	November, 2009	25%	37%	2,249
NH	November, 2009	21%	19%	263
OH	December, 2009	31%	51%	1,914
IL	January, 2010	28%	48%	3,346
AR	February, 2010	34%	78%	0
MD	March, 2010	22%	94%	1,377
MO	May, 2010	20%	34%	1,200
KS	May, 2010	27%	18%	286
ME	August, 2010	32%	23%	441
TX	October, 2010	32%	13%	4,983
NV	November, 2010	30%	33%	770
MS	February, 2011	39%	39%	0
WA	April, 2011	27%	18%	1,364
SC	May, 2011	32%	36%	1,704
IN	May, 2011	29%	26%	1,032
IA	July, 2011	27%	18%	0
RI	July, 2011	31%	49%	102
UT	August, 2011	24%	7%	929
AZ	November, 2011	31%	12%	1,750
KY	December, 2011	34%	32%	1,087
CA	February, 2012	32%	3%	8,015
MN	July, 2012	23%	9%	0
CO	December, 2012	2%	13%	970
OR	December, 2012	24%	2%	1,302
NM	December, 2012	36%	3%	0

Note: States are defined as having free Lifeline available once enrollments have exceeded one percent of eligible households for three months. States without free Lifeline by December 2012 are unlisted. The percentage of households eligible is estimated from ACS data on household enrollment in SNAP, Medicaid, and SSI, as well as income relative to the Federal Poverty Limit. The number of free Lifeline enrollments (at the end of 2012) is divided by the number of eligible households to calculate Lifeline take-up.

Table 2: Sample Means

	Full sample	**Eligible sample**	**Eligible, early avail.**	**Eligible, late avail.**
Income ($)	62,611	20,853	19,654	21,580
	(63,339)	(23,232)	(22,853)	(22,929)
Age	47.6	46.1	46.8	45.8
	(17.5)	(19.7)	(20.1)	(20.0)
Family Size	2.50	2.57	2.41	2.68
	(1.50)	(1.77)	(1.68)	(1.89)
Hispanic	.142	.225	.149	.341
Black	.135	.213	.270	.078
High School Degree	.885	.753	.768	.743
Bachelors Degree	.353	.137	.136	.157
SNAP Benefits ($)	111	390	382	347
	(671)	(1,217)	(1,171)	(1,208)
Medicaid	.109	.382	.382	.316
Avg Cell Bill ($)	53.90	38.54	35.89	36.64
	(65.91)	(59.05)	(57.70)	(56.47)
Has Cell Bill	.588	.456	.429	.461
Observations	64,202	18,218	9,002	4,703

Note: Consumer Expenditure Survey sample means. Standard deviations in parentheses (not included for indicator variables). Households are grouped based on their state of residence. States with free Lifeline available by the end of 2009 are defined as Early Availability. States without free Lifeline until at least July 2011 are defined as Late Availability. Households in states whose first carrier entered between these dates are not included in either subsample.

at least June 2011. States which had their first provider enter between these dates are not included in either group, but will be included when estimating the full model with more continuous treatment timing measures. These group definitions generally divide states between those that approved a provider within a year of the FCC's Forbearance Order, and those that waited two or three years.

Table 2 shows summary statistics for the full estimation sample, the sample of eligible households, and eligible households in early and late Lifeline availability states. These statistics are pooled across all years from 2005 to 2012. The early availability states have slightly lower income and slightly higher enrollment in other targeted government programs like SNAP and Medicaid. Geographically, Lifeline tended to become available earlier in eastern states than in western states. This contributes to the larger difference between groups in the portions of black and Hispanic heads of household.

Table 3 shows sample means for the main dependent variable of interest — average wireless spending — with subsamples split by household eligibility, early or late Lifeline availability, as well as between pre-2009 and post-2009 time periods. Spending is increasing over time for all groups, but the increase was \$4.96 less for eligible households in states with early Lifeline availability than in late availability states, as reflected in the difference-in-differences estimate. Conversely, among ineligible households, spending increased by \$3.72 more in early relative to late availability states. Thus, if the trend in ineligible household spending is used as the baseline, the triple-differences estimate of Lifeline's effect is an \$8.68 reduction in spending for treated eligible households. The corresponding statistics based on an indicator for any wireless spending are in Appendix Table A.2, and show that eligible households in early availability states were 4.4 percentage points less likely to report any spending, or 6.4 percentage points less likely if using ineligible households as the baseline.

To assess the level of crowd-out the average spending estimates imply, they need to be interpreted in the context of Lifeline take-up rates. Over the post-2009 period the average free Lifeline take-up rate among eligible households was 33.6 percent in early availability states, and 1.9 percent in late availability states. Thus, if enrolled households reduced wireless spending by the exact \$9.25 value of the subsidy, implying 100 percent crowd-out, one would expect to see an average effect of (0.336) times -\$9.25, equal to -\$3.11, across all eligible households in early availability states.[24] A smaller reduction in spending, implying less than full crowd-out, should result in a weaker (less negative) average treatment effect. Spending reductions stronger than -\$3.11 imply more than full crowd-out. Both the difference-in-

[24]This calculation assumes away both the small number of enrollments in late availability states (which would slightly reduce the expected average effect), as well as any spillover effects on the spending of ineligible households.

Table 3: Mean Wireless Spending, Pre- and Post- Expansion, Early and Late Availability States

	Lifeline availability	**Pre-2009**	**Post-2009**	**Difference**	**Difference-in-Differences**	**Triple-Differences**
Eligible households	Early	\$26.94	\$45.45	\$18.50		
		(0.73)	(1.10)	(1.27)		
	Late	\$26.62	\$50.07	\$23.46	-\$4.96	
		(0.99)	(1.55)	(1.75)	(2.62)	
	Observations	7,058	5,036	12,094	12,094	
Ineligible households	Early	\$47.12	\$73.47	\$26.36		
		(0.52)	(0.84)	(0.94)		
	Late	\$50.62	\$73.26	\$22.63	\$3.72	-\$8.68
		(0.80)	(1.20)	(1.39)	(2.12)	(2.36)
	Observations	18,761	11,308	30,069	30,069	42,163

Note: Standard errors in parentheses, clustered at state level for difference-in-differences and triple-differences estimates. Households are grouped based on their state of residence. States with free Lifeline available by the end of 2009 are defined as Early Availability. States without free Lifeline until at least July 2011 are defined as Late Availability. Households in states whose first carrier entered between these dates are not included in either subsample. Households interviewed in 2009 are excluded.

differences and triple-differences point estimates are consistent with spending reductions, and hence crowd-out, exceeding the full subsidy value. To test the statistical significance of the results, I conduct one-sided t-tests under the null hypothesis that each estimate is greater (less negative) than -$3.11. The resulting p-values are 0.24 for the differences-in-differences estimate, and 0.01 for the triple-differences estimate. The latter test rejects crowd-out below 100 percent of the subsidy value at standard significance levels.

There are a few of potential concerns which motivate the use of the full event-study model below. First, these results do not make use of (or control for) the available data on households characteristics. This may be particularly important given that the CEX is *not* representative at the state-level, as a particular demographic group may be over-sampled in a handful of states, with these households intended to represent the spending of the group nationally. Second, Lifeline availability is determined at the state level, and states may differ in persistent ways that affect both the level and trend of wireless spending. The event-study framework helps assess these differences using pretreatment trends, after controlling for state, time, or state-by-time fixed effects, depending on the specification. Finally, these results rely on a fairly rough split between early and late availability states and pre- and post-treatment periods. The full model uses a more continuous measure of treatment timing, making use of the data from all states, and allowing for the possibility that treatment effects may increase over time, due to increasing take-up and increasing plan quality.

5 MODEL

The models used are event study generalized difference-in-differences (DD) and triple-differences (DDD) specifications (Jacobson, LaLonde and Sullivan, 1993). The DD specification will be limited to only eligible households, comparing the spending trends of those in states with and without free Lifeline available. The DDD specification will utilize the full sample of households, comparing the difference between eligible and ineligible spending trends in states with free Lifeline to the difference in states without. Note that because the sample is a repeated cross-section, each household is only observed in a single state and time period. The DD specification has the form

$$Y_{ist} = \sum_{j \in K} \theta_j T_{st}^j + \gamma_s + \gamma_t + \mathbf{X}_i \beta + \boldsymbol{U}_{st} \lambda + \epsilon_{ist} \tag{1}$$

$$\text{where } K = \{\leq -6, -5, -4, -3, -2, 0, 1, \geq 2\}.$$

Y_{ist} is the reported wireless spending outcome for household i residing in state s and observed in time period t, θ_j are the treatment effect parameters of interest, and γ_s and γ_t are state and time fixed effects. $\mathbf{X}_i$ is a vector of household characteristics, $\boldsymbol{U}_{st}$ is a vector of state-level economic variables, specifically an unemployment rate and a house price index.[25]

The lag and lead indicators for treatment status T_{st}^j are defined relative to the year free Lifeline first became available, where $j = 0$ represents the initial year of availability. T_{st}^j is set equal to one if free Lifeline has been available for j years in state s during time period t, and zero otherwise. The estimated treatment effects are measured relative to the omitted category of one year prior to adoption.[26] I group treatment indicators for more than five years prior to availability or more than two years after, due to the limited number of observations and states appearing beyond these categories.[27]

In the data, free Lifeline enrollment increases over time after the service first becomes available. There are a number of explanations for this pattern, including entry by additional carriers, improved plan quality, increased marketing, and higher awareness of the program from word-of-mouth information. Since the model is designed to pick up average treatment effects on the intended treatment population, the program's estimated impact should be

[25] The full set of household characteristics includes family size, cubic terms in income, quadratic terms in age, as well as categorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100,000.

[26] T_{st}^j equals zero in all periods for states where free Lifeline was never available during the sample period.

[27] Appendix Figure A.2 shows the results removing this grouping, which feature imprecise estimates for the endpoints but otherwise very similar results.

increasing in the number of enrollees. This should hold even for additional enrollments that go to duplicate households, violating the one-per-household rule. If a household's first and second enrollments have differing marginal effects on spending, the model estimates will represent an average across these.

One weakness of the DD model is that it does not allow for state-by-time indicators, as this type of non-parametric state-specific time trend would absorb the treatment effect indicator variables. The difference-in-differences estimates from Table 3 suggest that spending among ineligible households grew faster in early Lifeline availability states than late availability states. The DDD model uses the spending of ineligible households, who remain untreated in all periods, to estimate non-parametric state time trends by including state-by-time fixed effects. The DDD model also includes eligible-by-time fixed effects to account for any differential trend between eligible and ineligible spending at the national level. The full DDD specification has the form

$$Y_{ist} = \sum_{j \in K} \theta_j T_{st}^j E_i + \gamma_{st} + \gamma_{et} + \gamma_{es} + \mathbf{X}_i \beta + \boldsymbol{U}_{st} \lambda + \epsilon_{ist} \quad (2)$$

$$\text{where } K = \{\leq -6, -5, -4, -3, -2, 0, 1, \geq 2\}$$

and E_i equals one if household i is Lifeline-eligible in its state of residence; γ_{st}, γ_{et}, and γ_{es} are state-by-time, eligibility-by-time, and eligibility-by-state fixed effects respectively.[28]

The two primary outcomes of interest are the dollar level of household wireless spending, and an indicator for any positive spending. The models are estimated by OLS for both outcomes, with the latter representing a linear probability model. A large number of households report zero spending, and the linear probability model is useful for examining extensive margin effects. Although the availability of free Lifeline plans could be expected to increase the portion of households subscribing to wireless service, if these households subscribe to

[28]In practice, the state-by-time fixed effects identify both the intercept and trend for one of the eligibility groups at the state level, so the corresponding eligibility-by-time and eligibility-by-state fixed effects for that particular group are omitted due to collinearity.

a free Lifeline plan rather than a paid plan, they may be less likely to report any positive spending.

In Section 7, the same models are estimated on a variety of other household spending variables as placebo tests, to evaluate whether the included fixed effects are controlling for unobserved trends in a satisfactory manner.

5.1 Causal Interpretation

In order to interpret the Model (1) coefficients θ_j as causal average treatment effects, the standard parallel trends assumption of the difference-in-differences model must hold for households in states with and without free Lifeline carriers. For Model (2), the required assumption is that in the absence of treatment, the trend in the difference between eligible and ineligible household spending would have been the same in treated and untreated states. In other words, I assume the gap between eligible and ineligible households would have grown (or shrunk) by the same amount, in levels. If the availability of Lifeline is correlated with some unobserved factor at the state level, and that same unobserved factor independently causes wireless spending to follow a different trend, this effect will be absorbed as part of the treatment effect.

The date free Lifeline becomes available in a state depends on the decisions of two parties: potential entering carriers and state public utilities commissions. Potential entrants must apply and be approved to operate by these commissions, and in Appendix A.3 I use the observed application dates of carriers to show that nearly all states had at least one free Lifeline carrier apply soon after the FCC issued its forbearance order. Additionally, after controlling for observable differences across states, the application dates are only weakly correlated with eventual demand (enrollment). It thus appears that state level variation in the timing of Lifeline availability is mostly driven by the approval processes of state utilities commissions.

While state commissions' timing of approval decisions could be driven by unobserved demand trends, the anecdotal evidence suggests that the speed of approval for a given carrier application depends on a variety of other factors. These include the number of public comments put forth by consumer groups and competing phone companies, requests for additional information by the commission itself, the resources (or lack thereof) allocated to the commission, as well as disagreements over tangential legal and technical issues.

In one stark example, California had drafted a resolution in October 2008 to approve the carrier TracFone's application, but after a late-filed comment from a group of small local landline carriers, eventually denied the application on the grounds that TracFone had not collected and remitted Universal Service Fees to the state.[29] California did not approve its first free Lifeline carrier until the third quarter of 2011.

Another important case comes from the eleven states which deferred their approval authority to the FCC. These states claimed that because TracFone was only applying to be a Lifeline provider — but not a provider of other universal service programs — the states did not have the jurisdiction to approve the applications.[30] As a result, the FCC eventually approved TracFone as a provider in all these states simultaneously, establishing a large share of the earliest availability states.

For identification purposes, the argument here is that the workings of state regulators are guided by a wide variety of institutional characteristics, but that these are time-invariant, at least over the period applicable here. As discussed in Besley and Case (2000), if the differences in state policies are influenced by fixed unobservable characteristics, then the state fixed effects in the DD model will account for this endogeneity. Further, if state policies are influenced by state trends affecting spending by all (eligible and ineligible) households, the DDD model will account for these with the included state-by-time fixed effects.

[29] Resolution to Deny the Request of TracFone Wireless, Inc. (U-4231-C) to be Designated as an Eligible Telecommunications Carrier (ETC) in California, Public Utilities Commission of the State of California, Dec. 17th, 2009.

[30] TracFone ETC Designation, April 9th, 2008 in FCC Docket No. 96-45.

5.2 Crowd-Out Estimates

To estimate the amount of crowd-out caused by the free Lifeline expansion, I compare the estimated average treatment effects, which represent the per-household spending reductions, to the per-household subsidy expenditures. As described in Section 3.3, for the set of states and months included in each treatment group ($T^0_{st} = 1$, $T^1_{st} = 1$, $T^2_{st} = 1$), I divide the sum of free Lifeline enrollments by the sum of eligible households. These average take-up rates are then multiplied by the per-enrollment subsidy of $9.25 to get average per-household subsidy expenditures.

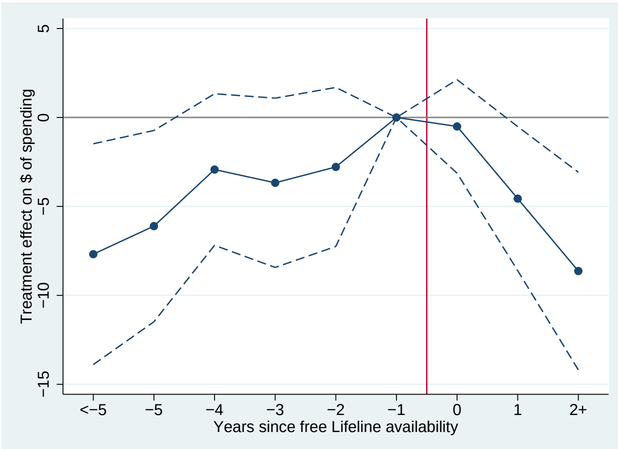
The validity of calling this crowd-out depends on there being no spillover effects on unenrolled households' wireless spending. If the entry of free Lifeline carriers lowered prices for unsubsidized wireless plans and reduced spending by unenrolled (but eligible) households, this would be part of the average treatment effect, but would not fit the description of crowd-out.

6 RESULTS

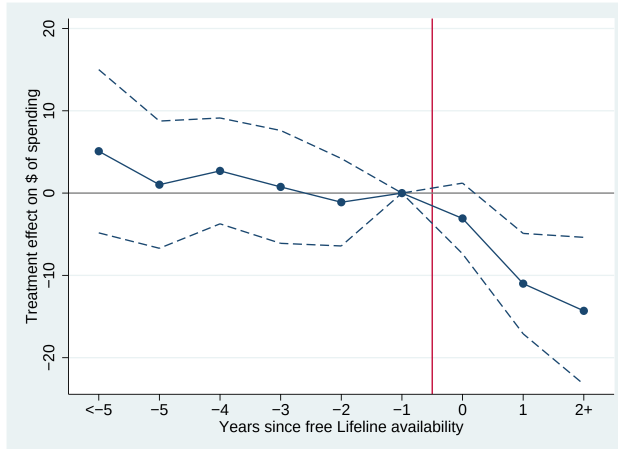
Table 4 presents the results for the DD and DDD models, with treatment indicators based on how long free Lifeline has been available in a state. The first two columns' dependent variable is a household's average monthly wireless spending in dollars. The earliest pretreatment effects estimated for the DD model are negative and statistically significant, suggesting a faster growth trend in wireless spending in early Lifeline treatment states. The DDD model controls for these differential state trends through the inclusion of state-by-time fixed effects. Both models are plotted in Figure 6.1, with the DDD estimates showing no statistically significant pre-trend and stronger post-treatment effects. In both models, as a state's time with free Lifeline available increases, the estimated effects become more negative. This is to be expected, given that the average free Lifeline take-up rate is 13.0 percent in the initial year of availability, 25.4 percent in the second full year, and 43.3 percent beyond the second

Figure 6.1: Lifeline Treatment Effects on Household Wireless Service Spending

(a) Event Study Difference-in-Differences (DD)



(b) Event Study Triple-Differences (DDD)



Note: All plots show estimated event study treatment effects on Lifeline-eligible households. Plots (a) and (b) shows generalized difference-in-differences and triple differences estimates, respectively, corresponding columns (i) and (ii) in Table 4. Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variable is the household's self-reported average monthly wireless service spending spending measured in dollars. The treatment indicator variable for the year immediately prior to treatment is omitted. All specifications include the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

year. Focusing on the DDD model, eligible households in markets where free Lifeline had been available for two or more years spent $14.31 less per month than observably identical households in markets where free Lifeline plans were not available. The estimated effects are measuring an average across all intended treatment households, including those with zero enrollments, one enrollment, and multiple enrollments. While no effect would be expected for unenrolled households, those with multiple enrollments (in violation of program rules) could potentially forgo large amounts of spending.

The treatment estimates for the years beyond the first in both the DD and DDD model are negative and statistically significant, indicating that the availability of free Lifeline did crowd out wireless spending by eligible households. For each of these estimates, following the

Table 4: Effect of Free Lifeline Availability on Households' Wireless Spending

	Average Spending				**Pr(Any Spending)**		
	(DD) (i)	100% crowd-out p-value	(DDD) (ii)	100% crowd-out p-value	(DD) (iii)	(DDD) (iv)	Lifeline take-up
Treatment, < -5 years (T^{-6})	-7.68**	-	5.09	-	-.123**	-.037	-
	(3.06)		(4.88)		(.050)	(.042)	
Treatment, -5 to -4 years (T^{-5})	-6.11**	-	1.02	-	-.086**	-.041	-
	(2.65)		(3.81)		(.038)	(.036)	
Treatment, -4 to -3 years (T^{-4})	-2.93	-	2.69	-	-.042	-.013	-
	(2.10)		(3.17)		(.032)	(.030)	
Treatment, -3 to -2 years (T^{-3})	-3.67	-	0.75	-	-.040	-.014	-
	(2.34)		(3.38)		(.027)	(.026)	
Treatment, -2 to -1 years (T^{-2})	-2.78	-	-1.11	-	-.019	-.016	-
	(2.20)		(2.62)		(.017)	(.016)	
Treatment, 0 to 1 years (T^{0})	-0.50	.296	-3.08	.191	-.012	-.026	13.0%
	(1.30)		(2.11)		(.015)	(.018)	
Treatment, 1 to 2 years (T^{1})	-4.56**	.137	-11.00***	.003***	-.013	-.033	25.4%
	(1.99)		(3.00)		(.029)	(.025)	
Treatment, > 2 years (T^{2})	-8.63***	.050**	-14.31***	.012**	-.043	-.070**	43.3%
	(2.74)		(4.40)		(.037)	(.031)	
Eligible households only	Y				Y		
Full sample			Y			Y	
Time F.E.	Y				Y		
State F.E.	Y				Y		
State X Time F.E.			Y			Y	
Eligible X Time F.E.			Y			Y	
Eligible X State F.E.			Y			Y	
Outcome Variable Mean	38.54		53.90		.456	.588	
Observations	18,218		64,202		18,218	64,202	

*** p<0.01, ** p<0.05, * p<0.1

Note: Robust standard errors (in parentheses) are clustered at the state level. Dependent variables are the household's self-reported average monthly wireless service spending in dollars, and an indicator equal to 1 if the household reported any positive spending. The omitted treatment category includes treated states 0 to 1 years prior to treatment, as well as states never treated during the sample period. All models estimated with OLS. All specifications include the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as categorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Time fixed effects are quarter-by-year. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

approach laid out in Section 4, I conduct one-sided t-tests to assess whether the hypothesis of crowd-out below 100 percent can be rejected. For example, the estimated treatment effects beyond the second year are compared to the product of the corresponding take-up rate for that period (43.3 percent) and the full size of the per-enrollment subsidy ($9.25), equaling $4.01 of subsidy spending per eligible household. The p-values for these tests are reported in the columns to the right of the estimates. The results indicate that the hypothesis of less than full crowd-out can be rejected with 95 percent confidence for both the DD and DDD models beyond two years of free Lifeline availability, and with 99 percent confidence in the DDD model for the second year of free Lifeline availability. Phrased another way, given the estimated DDD treatment effect of -$14.31 beyond the second year of free Lifeline availability, there is only 0.012 probability that the true treatment effect is greater than or equal to -$4.01. Thus, while the standard errors for the treatment effect point estimates are fairly large, the evidence suggests that households taking up free Lifeline benefits are able to reduce their out-of-pocket wireless spending by at least the amount of the $9.25 monthly subsidy.

Enrollment in a free Lifeline plan may reduce wireless spending in two ways. For households already subscribing to paid wireless service, a free Lifeline plan could provide supplemental minutes of service, or a second line for another family member. This could be expected to reduce the number of minutes or phone lines purchased. Alternatively, a household may sign up for free Lifeline instead of subscribing to any paid wireless service. This second scenario would be observed in the data through a lower proportion of households reporting any positive spending. These are the results presented in columns (iii) and (iv), the treatment effects from the linear probability model. As with average spending, the DD model shows differential pre-trends for the earlier treatment states, which are controlled for in the DDD model. Statistically significant treatment effects are only found for the DDD model beyond two years of free Lifeline availability. Households in these states are 7 percentage points less likely to report any positive spending. The pattern of increasingly negative effects with treatment time is consistent whether looking at average spending or any spending.

To get a sense of the scale of the extensive margin effects, I multiply these average treatment effects by the size of the intended treatment population. Based on first quarter 2012 data for early availability states as defined in Section 4 (which is the set of states where $T^2_{s,\text{Q1 2012}} = 1$), the total number of eligible households is 16.2 million. The estimates then imply that seven percent of these 16.2 million households, equal to 1.13 million, reported zero wireless spending due to the availability of free Lifeline. While this represents a large number of affected households, the total number of free Lifeline subscriptions in these states was 8.5 million for this period. The extensive margin results then suggest that only about 13 percent of free Lifeline subscribers substituted from some positive spending to zero spending because of the program.[31] This means that the majority of subscribers either (a) never would have enrolled in paid wireless service anyway, or (b) used their free Lifeline subscription to supplement other paid wireless service. Given the large effects on average spending, along with the previous literature's findings of small ownership gains, the use of free Lifeline as a supplemental service is probably the more common situation.

Taken together, these results suggest that the free Lifeline expansion has reduced the out-of-pocket wireless spending by eligible consumers on both the intensive and extensive margins. The question of whether this indicates success for the program depends on which stated goal is taken as the reference.

Based on a goal of obtaining universal access to service, the program would aim to provide phones to those who would not have otherwise had access. This type of result would lead to zero (or even positive) coefficients for the treatment effects, as new customers are brought into the market, with the price effects inducing first-time phone ownership that would otherwise have not occurred. From this perspective, the results suggest substantial crowd-out, as households receiving free Lifeline service would have paid for service in the

[31] This calculation assumes one phone per household. To the extent that households received multiple phones, this would decrease the denominator of subscribed households, and hence understate the percentage of subscribed households reporting zero spending due to the program.

absence of the program. This is consistent with the findings of Ukhaneva (2015), which estimated that only one out of twenty free Lifeline subscriptions went to households that would not have otherwise owned a phone.

The evidence is more favorable towards the Lifeline program's goal of ensuring affordable access to phone services. These large estimates suggest that enrollment in free Lifeline reduces household spending by more than the size of the subsidy. This is plausible due to the relative generosity of free Lifeline plans as compared to unsubsidized phone services, as described in Section 2. If free Lifeline service crowded out an equivalent amount of paid service, as measured in minutes and texts, this could result in household savings of well over the $9.25 subsidy.

7 POTENTIAL CONFOUNDING FACTORS AND ROBUSTNESS

The results in Section 6 suggest that the Lifeline wireless expansion substantially reduced households' out-of-pocket spending on wireless service. This section addresses some of the potential concerns raised earlier about the validity of the difference-in-differences modeling assumptions. In particular, I conduct placebo tests of the model on other spending categories, discuss the coincident timing of the Great Recession and other policy changes, and assess whether the extensive margin crowd-out estimates are consistent with state-level subscribership data.[32]

[32] Additionally, the supplemental figures in Appendix A.4 test the robustness of the main results to certain modeling choices, including the grouping of event study endpoints, winsorized dependent variables, the parameterization of income levels, alternative availability timing definitions, and differential trends by year of treatment.

7.1 Placebo Tests on Other Spending Categories

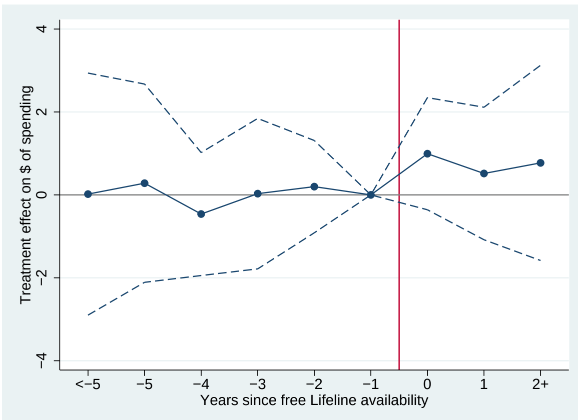
The estimated treatment effects in this paper show a larger wireless spending gap between eligible and ineligible households in treated states as compared to untreated states. One possible interpretation could be that the introduction of free Lifeline service was coincident with some other change in household spending patterns more generally. If this were the case, The Lifeline treatment effect should be detectable in other spending categories as well. Figure 7.2 shows estimates of the triple-differences model using a selection of other spending categories as dependent variables. Panels (a) and (b) show estimated treatment effects for home internet and television services. These categories have comparable spending levels compared to wireless phone service and similar consistent monthly renewal. In the case of internet service, there is also an upward trend in spending and adoption over the period of study. The estimated treatment effects are not statistically different from zero, and feature relatively level trends.

Panels (c) and (d) show estimated treatment effects on the larger categories of gasoline and food. These categories may be more likely to pick up the effects of households' general financial well-being, as well as larger aggregate fluctuations that are not captured by the households-level characteristics, or the included non-parametric time trends. Though one of the sixteen point estimates (the initial post-treatment year for gasoline) is statistically significant, the effect is not persistent and does not appear to reflect a systematic trend across spending categories. Beyond those shown in Figure 7.1, I estimate the model on each of the 13 other major spending categories (in addition to food) which collectively make up total spending in the interview portion of the CEX survey. I find a total of only five out of 104 estimated treatment coefficients are statistically significant at the 95 percent level, in line with the number to be expected from random sampling variation given true treatment coefficients of zero.

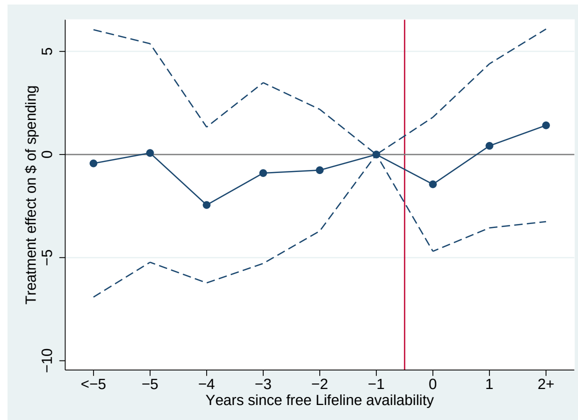
In combination, these tests support the notion that the estimated model is picking up a causal Lifeline treatment effect that is specific to wireless service spending.

Figure 7.1: Placebo Tests of Lifeline Treatment Effect on Household Spending Categories

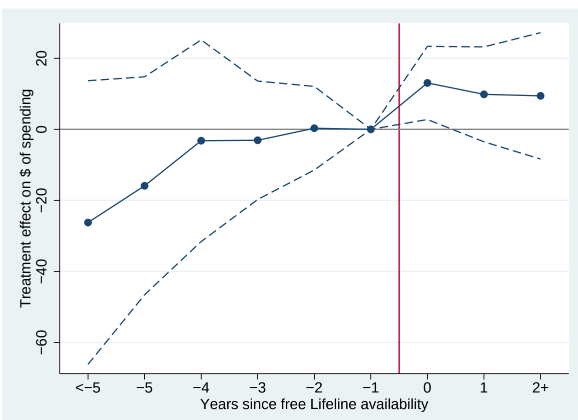
(a) Home Internet Service



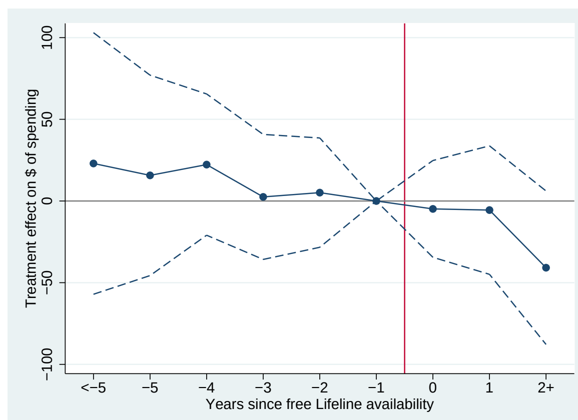
(b) Cable and Satellite TV Service



(c) Gasoline



(d) Food



Note: All figures show estimated event study treatment effects on Lifeline-eligible households from triple-differences regression (DDD Model). Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variables are the household's self-reported average monthly household spending in the given category, measured in dollars. The indicator variable for the year immediately prior to treatment is omitted. All specifications include the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

7.2 Coincident Timing of the Great Recession

One possible confounding factor is the coincident timing of the wireless expansion with the Great Recession. Reductions in employment and wages would increase the number of eligible households, and decrease the earnings of those already below the qualifying income threshold. Lower incomes are likely to influence both household spending and take-up rates for benefit programs generally. In addition, a number of states expanded eligibility criteria for SNAP and Medicaid during my study period. I discuss the potential direct effects from the recession first, followed by the interactions with other program changes in the next subsection.

In a study of the SNAP program, Ganong and Liebman (2013) showed that take-up rates rose from 69 percent of eligible households in 2007 to 87 percent in 2011, and estimated that two thirds of this enrollment growth was due to changes in local unemployment. For the CEX sample used in this paper, 26 percent of households were Lifeline eligible from 2005 to 2008. This rises to 30 percent for the years 2010 to 2012. The growth in eligibility seems to occur earlier in late availability states relative to early availability states, using the group definitions described in Section 4.

If the recession affects wireless spending and Lifeline enrollment only through the household's own current income, then the inclusion of income as a dependent variable in all regression specifications would be sufficient. If there is a fundamental difference between households that have suffered a negative income shock and those with stable incomes, then household factors beyond income should be considered. Additionally, if state or local economic shocks indirectly affect households' wireless spending, they could affect the estimated results as well.

To address the first possibility, all models include indicator variables for whether a household is claiming unemployment benefits. Households receiving unemployment insurance have likely suffered a recent negative income shock, and given the persistence of wireless service spending over time, may have a higher average spending level than other households at the same current income level.

To allow for the possibility that the health of the broader state economy could affect household wireless spending and take-up, the models also include variables for the state-level monthly unemployment rate and quarterly house price index. A less healthy local economy may be expected to decrease wireless spending, and increase the take-up of benefit programs like Lifeline. While the state-by-quarter fixed effects in the DDD model will already be controlling or these local economic trends, they help control for local variation in the DD model.

The results reported above are robust to the inclusion (or exclusion) of these variables. A household's own unemployment status is positively correlated with wireless spending, and the state-level unemployment rate is negatively correlated with wireless spending. This suggests that household- and state-level unemployment shocks are important factors in wireless spending, but are not the source of the estimated free Lifeline treatment effects.

7.3 SNAP and Medicaid Eligibility Expansions

In addition to changing economic conditions, a number of states expanded eligibility for programs like SNAP and Medicaid during this period. Lifeline eligibility is measured by enrollment in SNAP, Medicaid, or SSI, as well as household income relative to the Federal Poverty Level (FPL). Take-up of these three benefit programs is certainly endogenous, and they have their own eligibility requirements based on income. Throughout the period studied in this paper, maximum income thresholds for enrollment in SSI ($1010 monthly in 2012) were more strict than the income threshold for Lifeline in most states (135 percent of FPL). Both SNAP and Medicaid had higher income thresholds and policy changes for some households in some states, which I discuss in more detail below.

Beyond eligibility, SNAP, Medicaid, and SSI provide substantially larger benefits than Lifeline, often by more than an order of magnitude. For this reason, I am less concerned about households that would not have otherwise enrolled in SNAP, for example, doing so in order to become eligible for Lifeline. A more likely interaction between programs would come from Lifeline being marketed to consumers at the time they were enrolling in other benefits. This could make new SNAP enrollees more likely to enroll in Lifeline, through increased program awareness and convenience. As a result, I would expect two potential channels for take-up of other programs to influence the wireless spending of an individual consumer: First, a consumer's enrollment in SNAP could be a signal of economic hardship, which might be correlated with a decrease in spending on wireless services, biasing my estimates downward. Second, enrollment in SNAP provides a wealth effect from the benefits received, potentially leading to higher spending on wireless services, biasing my estimates upward. To account for these signaling and wealth effects, I include an indicator for enrollment in SNAP, Medicaid, or SSI in the set of control variables.

While the addition of a program enrollment indicator should help control for endogenous take-up under constant eligibility rules, changes to the SNAP and Medicaid programs are a separate issue to address. For SNAP, about half of states have income thresholds above those of Lifeline, and 14 of these states are in my sample and raised their thresholds during my period of study (Falk and Aussenberg, 2014). For Medicaid, two states in my sample expanded eligibility to additional households beyond those already eligible for Lifeline during the period studied. In all of these cases, income thresholds were raised to levels between 160 and 200 percent of FPL.[33] For the purposes of estimating Lifeline's effects, the issue stems from those affected households with incomes above the Lifeline threshold but below these raised SNAP and Medicaid thresholds, as they gain the ability to select into Lifeline eligibility in the middle of the study period. These affected households represent about four percent of my sample.

Panel (c) of Appendix Figure A.3 shows results for the DDD model excluding those households affected by the expansions. The results remain largely unchanged, suggesting that the estimated Lifeline treatment effect cannot be attributed to coincident changes in the program rules of qualifying benefit programs.

[33]"States Getting a Jump Start on Health Reform's Medicaid Expansion," Kaiser Family Foundation, http://kff.org/health-reform/issue-brief/states-getting-a-jump-start-on-health/, accessed May 1st, 2017.

7.4 Differential Trends in Wireless Prices or Quality

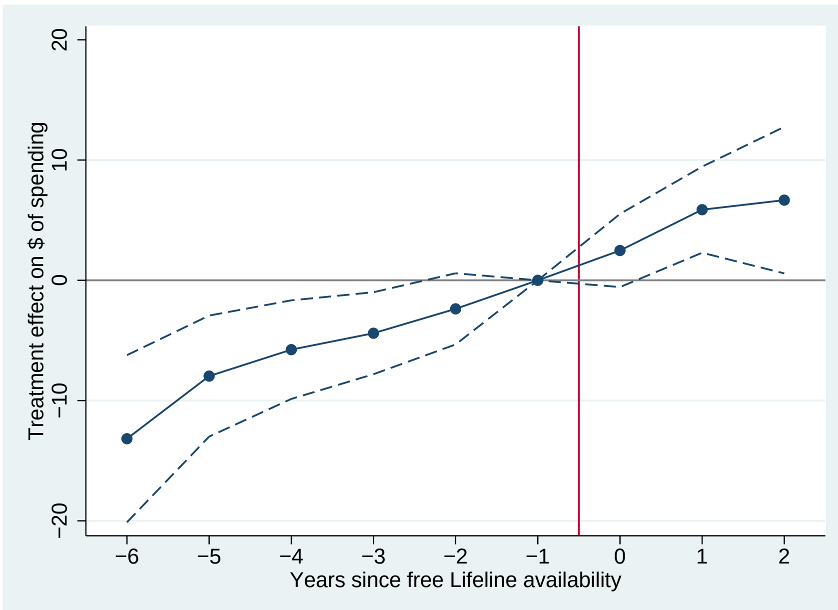
Wireless phone service prices and quality are largely set at the national level, particularly for the major carriers (AT&T, Verizon, Sprint, T-Mobile). However, it is possible that there is some price or quality variation across states due to differences in the set of smaller, regional carriers operating. If these differ only in levels, the effect should be absorbed in the state fixed effects. If, however, these smaller carriers had differing trends in their plan offerings that coincided with free Lifeline entry, it could bias the estimated treatment effects. One expected result of such trends would be that spending levels should respond for all households, not merely those that are eligible for Lifeline. To test this possibility, I estimate the DD model on a placebo sample of only Lifeline *ineligible* households, who have higher incomes and are not enrolled in qualifying benefit programs. Note that this also provides a test of whether states with earlier free Lifeline availability were on a faster upward spending trend, as suggested by the pre-trends in the DD model from panel (a) of Figure 6.1.

Figure 7.2 shows the estimated treatment effects for these ineligible households using the DD model. The results confirm that early eligibility states were on a faster upward trend prior to Lifeline's introduction, with pre-trends mirroring those seen for eligible households. After Lifeline's introduction, there is no evident shift in the steady upward trend for ineligible households. This is in stark contrast to the strong spending reductions seen for eligible households in Figure 6.1. This suggests that early treatment states were on a faster upward spending trend for all households, but that the Lifeline's introduction shifted only spending among eligible households, explaining the stronger estimated treatment effects from the DDD model relative to the DD model.

7.5 Consistency with State-Level Subscriber Data

The results from the linear probability model suggest that crowd-out on the extensive margin, in terms of free Lifeline subscriptions replacing paid subscriptions, is fairly small. If this effect is aggregated up to the state or national level, it would mean that the growth of free Lifeline

Figure 7.2: Placebo Test - Effect of Free Lifeline Availability on Ineligible Households' Wireless Spending - DD Model



Note: Estimated event study treatment effects on Lifeline-ineligible households. Generalized difference-in-differences estimates. Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variable is the household's self-reported average monthly wireless service spending spending measured in dollars. The treatment indicator variable for the year immediately prior to treatment is omitted. Specification includes the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rateand house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

subscriptions is occurring on top of the underlying growth in paid wireless subscriptions. To test this hypothesis, I estimate a simple difference-in-differences model with state-level wireless subscribership data.

The idea is that if 100 Lifeline enrollments increase the number of total (Lifeline-inclusive) wireless subscribers by 100, it supports the finding that free Lifeline does not crowd out paid wireless subscriptions. Using biannual state-level data, I regress total wireless subscribers per person on free Lifeline subscribers per person, including state (s) and time (t) fixed effects:

$$\frac{\text{Total Subs}_{st}}{\text{Population}_{st}} = \beta \frac{\text{Free Lifeline Subs}_{st}}{\text{Population}_{st}} + \gamma_s + \gamma_t + \varepsilon_{st}.$$

The regression yields an estimate of $\hat{\beta} = 1.16$ with 95% confidence interval of $[.86, 1.46]$.[34] This is consistent with free Lifeline enrollments occurring in addition to the underlying growth of the paid wireless industry. Based on the 13 percent extensive margin crowd-out estimates from the household-level data, an estimate of $\hat{\beta}$ in the range of .85 to .9 would be expected. The higher estimate here could be a product of measurement error, or the steeper trend among ineligible households in treated states. In either case, it supports the notion that free Lifeline subscriptions are largely used to supplement, rather than replace, paid wireless subscriptions.

8 CONCLUSION

The expansion to free wireless service brought Lifeline benefits to many previously unenrolled households. This paper suggests how households have utilized these phone services. While the reduction in households reporting any spending represents only 10 to 15 percent of total enrollments, the reduction in wireless service spending may exceed 100 percent of the subsidy payments. The previous literature has found that few households received their first phone due to the wireless expansion. In total, these results suggest that free Lifeline subscriptions

[34] The total subscribership data (2005 to 2012) are from the FCC's Mobile Competition Reports. Robust standard errors are clustered at the state level.

are primarily used to supplement other paid wireless service, allowing households to reduce the quantity of service purchased.

Did the Lifeline wireless expansion help achieve the program's goals? From the standpoint of the phone ownership rate, the expansion seems to have had very small effects. However, the expansion had a large impact on households' out-of-pocket spending and service quality. The new free wireless segment of the market offers higher quality than an equivalent cash transfer could buy. This market segment has crowded out substantial consumer spending on unsubsidized service, and in turn increased the affordability of a given quality of wireless service.

Compared to the other markets and programs studied in the crowd-out literature, wireless phone service offers consumers a higher degree of flexibility and substitution. Consumers can adjust their consumption level by purchasing larger or smaller quantities of minutes, texts, and other calling features. The evidence on spending suggests that they are much more likely to participate in both the subsidized and unsubsidized markets simultaneously. It thus may not be totally surprising that these aggressively marketed, relatively generous plans are able to displace a great deal of unsubsidized spending, without crowding out many enrollments.

Based only on a goal of providing affordable services, this model of allowing private competitors to market close substitutes to their unsubsidized products appears very effective. The most intriguing aspect of the provision mechanism is the resulting shift to market segmentation based on eligibility, resulting in higher quality services for enrollees. Further research is needed to determine whether similar benefits could be achieved in other private goods markets.

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A APPENDIX

A.1 Sources for State Eligibility Requirements

Carrier Websites:

- SafeLink Wireless (TracFone) - https://www.safelinkwireless.com/safelink/program_info/faq/eligibility
- Assurance Wireless (Virgin Mobile) - http://www.assurancewireless.com/Public/HowToQualify.aspx

State Public Utilities Databases:

- Alabama - http://www.psc.state.al.us/SearchOrders2.asp
- Arizona - http://www.azcc.gov/Divisions/Utilities/Tariff/util-tarrifs-telecom.asp#T
- Arkansas - http://www.apscservices.info/
- California - http://www.cpuc.ca.gov/PUC/Telco/
- Florida - http://www.floridapsc.com/dockets/cms/docketFilings3.aspx?docket=110000
- Georgia - http://www.psc.state.ga.us/factsv2/Default.aspx
- Illinois - http://www.icc.illinois.gov/docket/reports/cases.aspx
- Kansas - http://www.kcc.state.ks.us/pi/lifeline.htm
- Kentucky - http://psc.ky.gov/
- Louisiana - http://lpscstar.louisiana.gov/star/portal.aspx
- Maine - https://mpuc-cms.maine.gov/CQM.Public.WebUI/Common/AdvanceSearch.aspx
- Maryland - http://webapp.psc.state.md.us/Intranet/maillog/maillogitems_new1.cfm?
- Michigan - http://efile.mpsc.state.mi.us/efile/
- Minnesota - https://www.edockets.state.mn.us/EFiling/
- Mississippi - http://www.psc.state.ms.us/trinityview/mspsc.html
- Missouri - http://psc.mo.gov/General/Look%20Up%20Docket%20Files
- Nevada - http://pucweb1.state.nv.us/puc2/DktInfo.aspx
- New Jersey - http://search.state.nj.us/
- North Carolina - http://starw1.ncuc.net/NCUC/portal/ncuc/page/Dockets/portal.aspx
- Ohio - http://dis.puc.state.oh.us/AdvS.aspx

- Oregon - http://apps.puc.state.or.us/edockets/docket.asp?DocketID=15747
- Rhode Island - http://www.ripuc.org/eventsactions/docket.html
- South Carolina - http://dms.psc.sc.gov/dockets/
- South Dakota - http://puc.sd.gov/Dockets/Telecom/default.aspx
- Texas - http://interchange.puc.texas.gov/WebApp/Interchange/application/dbapps/login/pgLogin.asp
- West Virginia - http://www.psc.state.wv.us/Orders/default.htm
- Wisconsin - http://psc.wi.gov/utilityinfo/tele/newsInfo/eligibleCarriers.htm
- Wyoming - http://psc.state.wy.us/htdocs/asp/docketmain.asp

A.2 Variable Construction and Sample Restriction Details

Lifeline Eligibility

The treatment effects in this paper are estimated for Lifeline eligible households in the Consumer Expenditure Survey (CEX), and interpreted in the context of free Lifeline take-up rates based on the population of eligible households as measured in the American Community Survey (ACS). I specifically use the Interview portion of the CEX public-use microdata, and the ACS data maintained by IPUMS USA.[35]

Lifeline eligibility is determined by a household's income and family size, or by enrollment in a qualifying benefit program. I define households' eligibility relative to their state-specific requirements as shown in Table A.1. All but one state (California) uses income as a percentage of the Federal Poverty Limit (FPL) to determine Lifeline eligibility. The CEX records the relevant FPL (accounting for state of residence and family size) for each household as *POVLEVCY*, which I compare to the household income variable *FINCBTXM*. In the ACS, the variable *POVERTY* directly records household income as a percentage of the FPL. For California, the calculation is directly based on income and family size, for which I use *FINCBTXM* and *FAM_SIZE* in the CEX and *HHINCOME* and *NUMPREC* in the ACS.

To create comparable measures for Lifeline eligibility obtained through other benefit programs, I restrict measurement of eligibility to enrollment in SNAP, Medicaid or SSI, which are available in both surveys. In cases where a dollar value for benefits received is reported, I take any non-missing reported value above zero as demonstrating enrollment. The respective variables are *FOODSMPX, MDCDENR,* and *FSSIXM* in the CEX, and *FOODSTMP*, *HINSCAID,* and *INCSUPP* in the ACS. Data for Medicaid enrollment is unavailable in the ACS prior to 2007; however I only calculate take-up rates beginning when free Lifeline becomes available in 2009, so this will not affect the take-up estimates.

Once all households have been assigned as Lifeline eligible or ineligible, I calculate the

[35] Steven Ruggles, J. Trent Alexander, Katie Genadek, Ronald Goeken, Matthew B. Schroeder, and Matthew Sobek. Integrated Public Use Microdata Series: Version 5.0 [Machine-readable database]. Minneapolis, MN: Minnesota Population Center [producer and distributor], 2010.

population of Lifeline eligible households for each state and year combination by taking the sum of the ACS defined household weights, *HHWT*, for all eligible households. I perform the same sum across all households, not restricting by eligibility, to obtain total populations for each state and year combination.

Sample Restrictions

In both my CEX and ACS samples, I exclude all households from Alaska or Oklahoma, due to the states' high take-up and eligibility for Tribal Lifeline benefits, which provide more generous subsidies for those living on recognized American Indian Tribal Lands.[36] For households in all other states, I exclude those whose self-reported race is "Native American" or "Pacific Islander" in the CEX (variable *REF_RACE*) or "Native American or Alaska Native" in the ACS (variable *RACE*).

For the CEX estimation sample, I make a few additional restrictions referenced in Section 3. Households with negative reported incomes are excluded. I also exclude any households for which the state identifier is suppressed, or from states (MN and DE) for which "either all observations from this state have been re-coded or all strata of observations from this state include 're-codes' from other states." Panel (b) of Figure A.3 shows the estimated treatment effects when excluding states with any amount of re-coding, which additionally drops AL, GA, MD, and WI in all survey years, and a further twelve states in 2005 (CA, IN, KY, MI, NV, NC, OH, OR, TN, TX, VA).

Average Monthly Wireless Service Spending

A household's monthly wireless service spending is constructed from several variables in the CEX. First, households are asked to report their "total expense for mobile/cellular service for three months ago, adjusted for business," "... two months ago ...," and "... one month ago ...." Households are told to "include any bills you receive or pay online or have automat-

[36]For more details, see https://www.fcc.gov/consumers/guides/lifeline-support-affordable-communications.

ically deducted." The responses are recorded in the variables *TELCEL1X*, *TELCEL2X*, and *TELCEL3X* respectively. Prior to the 2007 survey year, each prior month was a separate observation indexed by the variables *TELCELLX* and *TELMO*.

Beginning in the 2007 survey year, households are then asked for the amount spent on prepaid cellular minutes "not already reported" over the full three month period. This is recorded as the variable *QPRP3MCX*, described as "reference period total for prepaid cellular minutes minus the current month."

For each household, I sum all expenses reported for these variables, and divide by three to obtain average monthly spending over the three month reporting period.

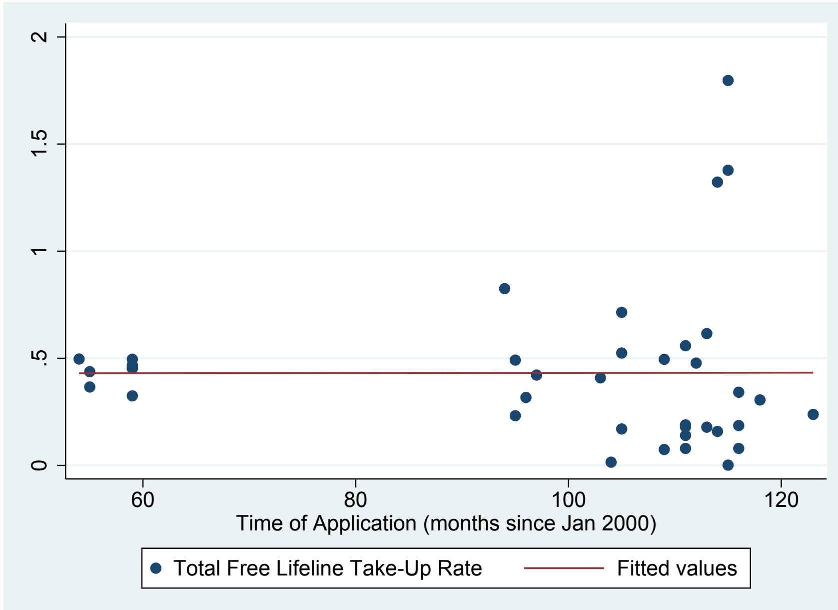
A.3 Firm Application Timing and Unobservable Demand

The treatment definition used in this paper is based on the date when Lifeline becomes available in a state, but firm application dates are also observable. If a state receives Lifeline later because firms choose not to apply for entry until later, then the treatment effect could be picking up some unobservable demand trend which is known by the firm. The goal here is to assess whether (a) the timing of firm applications is driven by observable or unobservable characteristics, and whether (b) the timing of firm applications is predictive of eventual enrollment.

For the application timing data, I focus on the application dates of TracFone, the largest and earliest entrant into the free Lifeline market.[37] TracFone was the first entrant into 30 of the 41 states with free Lifeline by the end of 2012. I am able to observe and verify all firm application dates for eight of the remaining 11 states where TracFone was not the first entrant. In all eight, TracFone either applied before, or within two months of, the eventual first entrant. In market share terms, Tracfone's subscribers made up 94% of all free Lifeline enrollments at the end of 2009. As a result, most states enter the treatment group when TracFone is approved, and its application dates are strong proxy for desired entry by Lifeline

[37] Conkling (2015) suggests that smaller firms select into markets based on the level of regulatory oversight.

Figure A.1: Total Free Lifeline Take-Up Rate vs. Timing of Initial TracFone Application



Note: Each data point represents a state market. On the vertical axis, total Lifeline take-up rates are calculated from Q1 2012 enrollments across all free carriers, divided by number of eligible households. Time of Application is the date of Tracfone's submission of relevant application documents to a given state's public regulatory commission. Earliest applications are on the left side of the figure, latest on the right. Fitted values based on least squares.

carriers.

If TracFone targeted its applications at the most desirable markets first, it should imply some testable implications. First, it should be anticipated that states with earlier applications would also have the higher total enrollments (or take-up) in free Lifeline across all carriers. Given the pattern of increasing enrollments over time, a certain amount of this relationship should be mechanical. Second, if this pattern does exist and is being driven by unobservable demand factors, then it should be robust to the inclusion of observable control variables.

Figure A.1 shows a scatter plot of TracFone's time of application on the horizontal axis, and the portion of eligible households enrolled with any Lifeline carrier on the vertical axis. The enrollment data are for the first quarter of 2012. Each data point is a state. The linear best fit line is plotted in red. The timing of TracFone's initial application turns out to be a poor predictor of the eventual Lifeline take-up rate in a state, though the flat trend line is driven somewhat by the three states with take-up rates exceeding one. Using enrollment data from the first quarter of 2013, after the Lifeline Reform Order had removed ineligible and duplicate enrollments, the trend line is downward sloping, though still with a very small implied effect.

A simple regression analysis can help evaluate the relative predictive power of application timing and other observable state characteristics. I regress total take-up from the first quarter of 2012 on the application timing, eligible population, land area, the portion of households enrolled in SNAP benefits, the highest unemployment rate reached during the recession, and an indicator for states that deferred carrier approval to the FCC. The results indicate that total enrollments are higher in states with higher population and SNAP enrollment, and lower in states with higher unemployment, larger land area, and FCC approval. The coefficient on month of applications has a coefficient of -.0035 with standard error of .0052. This suggests that states that TracFone applied to one year later had total take-up only 4.2 percentage points lower. The estimate is even closer to zero when using first quarter 2013 enrollments as the dependent variable.[38]

These patterns suggest that even if TracFone's application timing were driven by some unobservable state demand trends, total enrollments are not well predicted by these dates. One possible explanation is that the variation in the regulatory approval processes across states is adding enough noise to overwhelm any selection on unobservables in application timing. Of the states that eventually approved free Lifeline carriers, nearly all had received firm applications before the end of 2009. Thus, the lack of early free Lifeline in some states does not appear to be due to a lack of interest from prospective carriers, but rather to differences in regulatory processes and decisions.

A.4 Supplemental Tables and Figures

[38]This effect comes from some of the latest treated states building up their enrollments from 2012 to 2013.

Table A.1: Lifeline Eligibility Criteria by State

State	**Income threshold**	**Medicaid**	**SNAP**	**SSI**
AZ	150	1	1	1
CA	*	1	1	1
CO	0	0	0	1
FL	150	1	1	1
KS	150	1	1	1
MI	150	1	1	1
MO	0	1	1	1
NV	150	1	1	1
NJ	**	1	1	1
NM	150	1	1	1
OH	150	1	1	1
OR	0	1	1	1
RI	150	1	1	1
TX	150	1	1	1
WY	0	1	1	1
Other	135	1	1	1

Note: State Lifeline eligibilty criteria, as gathered from sources listed in Appendix A.1, prior to Lifeline Reform Order. Income Thresholds are as a percentage of the Federal Poverty Level (FPL). The SNAP, Medicaid, and SSI columns show whether a household could qualify through enrollment in one of these three federal programs. California income thresholds are not based on FPL, and in 2012 were $24,700 for a family of two, $28,800 for a family of three, plus $6,000 for each famiy member beyond the third. New Jersey has an income threshold of 150 percent of FPL for those aged 65 and older, and 135 percent otherwise. States that use the federal standard of 135 percent of the Federal Poverty Level and categorical eligibility through SNAP, Medicaid, and SSI are not shown individually. The federal standard is also assumed for states where pre-reform criteria could not be gathered.

Table A.2: Indicator for Any Wireless Spending, Pre- and Post- Expansion, Early and Late Availability States

	Lifeline availability	**Pre-2009**	**Post-2009**	**Difference**	**Difference-in-Differences**	**Triple-Differences**
Eligible households	Early	.345	.522	.177		
		(.007)	(.009)	(.011)		
	Late	.365	.587	.221	-.044	
		(.010)	(.012)	(.015)	(.026)	
	Observations	7,058	5,036	12,094	12,094	
Ineligible households	Early	.570	.704	.134		
		(.004)	(.005)	(.007)		
	Late	.603	.717	.114	.020	-.064
		(.006)	(.007)	(.010)	(.018)	(.021)
	Observations	18,761	11,308	30,069	30,069	42,163

Note: Standard errors in parentheses, clustered at state level for difference-in-differences and triple-differences estimates. Households are grouped based on their state of residence. States with free Lifeline available by the end of 2009 are defined as Early Availability. States without free Lifeline until at least July 2011 are defined as Late Availability. Households in states whose first carrier entered between these dates are not included in either subsample. Households interviewed in 2009 are excluded.

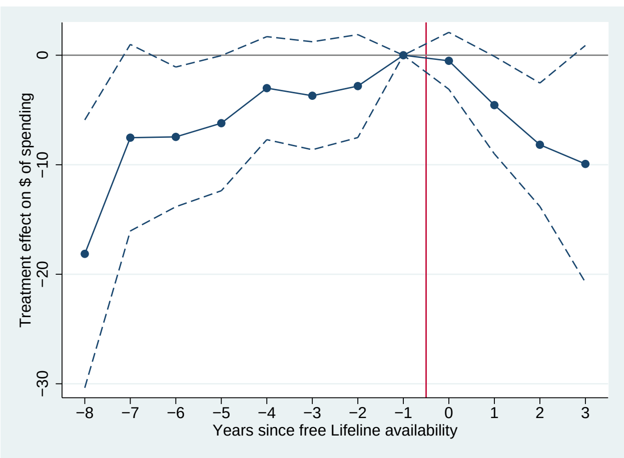
Table A.3: State Characteristics by Availability Timing

	Early availability states	**Late availability states**
SNAP participation rate (2012)	89.4%	82.6%
Poverty rate (2012)	13.4%	13.5%
Unemployment (2007-2012)		
Maximum rate	9.7%	9.8%
Maximum change in rate	5.4%	5.9%

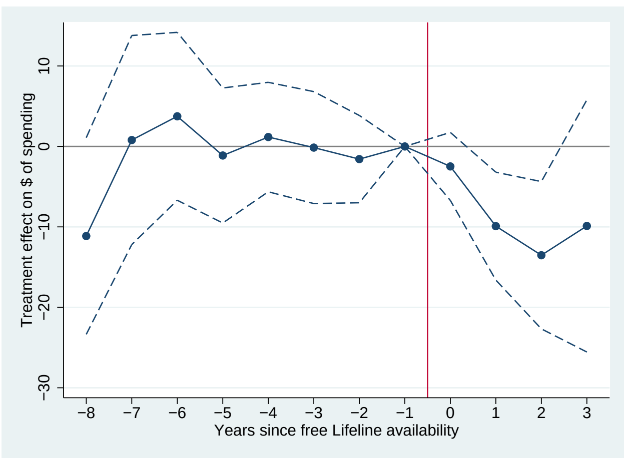
Note: Aggregate characteristics of states with early and late Lifeline availability, as defined in Section 4. Rates shown are simple (unweighted) averages across states satisfying each availability definition. SNAP participation rates come from "Reaching Those in Need: Estimates of State Supplemental Nutrition Assistance Program Participation Rates in 2012," published by the U.S. Dept. of Agriculture. Poverty Rates are estimated from ACS data. Unemployment data is from the Bureau of Labor Statistics.

Figure A.2: Lifeline Treatment Effects, No Endpoint Grouping

(a) Event Study Difference-in-Differences (DD)



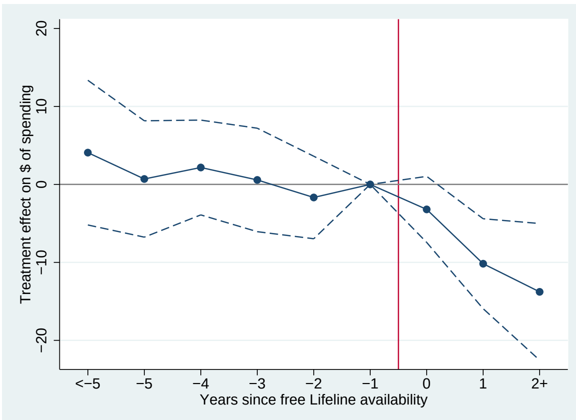
(b) Event Study Triple-Differences (DDD)



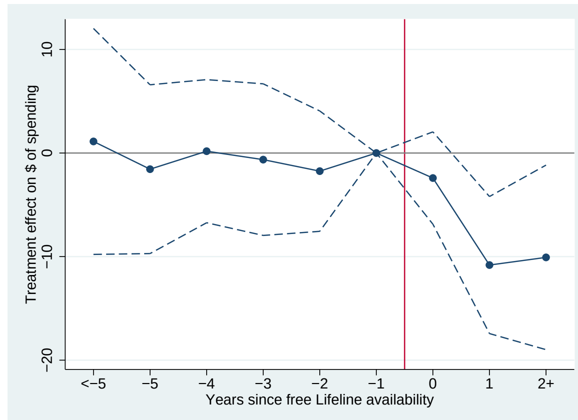
Note: All plots show estimated event study treatment effects on Lifeline-eligible households. Plots (a) and (b) shows generalized difference-in-differences (DD Model) and triple differences (DDD Model) estimates, respectively. Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variable is the household's self-reported average monthly wireless service spending spending measured in dollars. The treatment indicator variable for the year immediately prior to treatment is omitted. All specifications include the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

Figure A.3: Lifeline Treatment Effect, Alternative Specifications

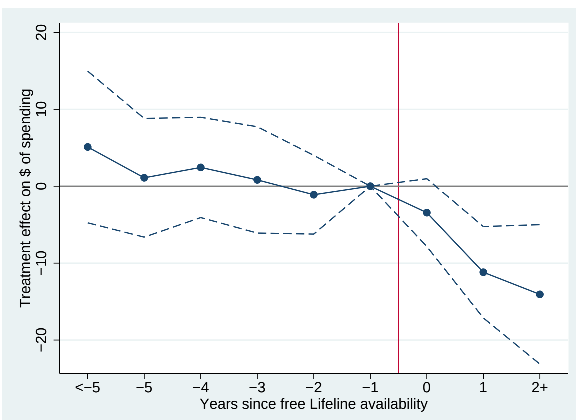
(a) Winsorized Top 1% of Outcomes



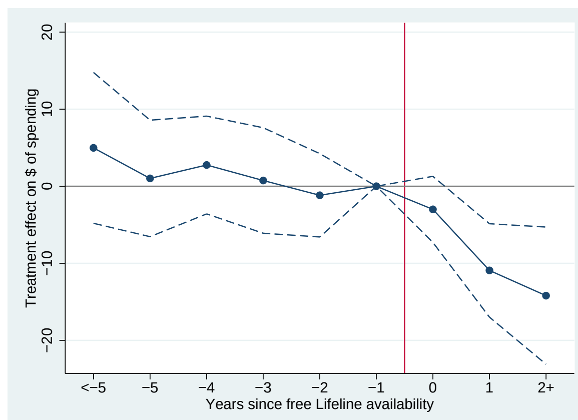
(b) Drop States with Any Recoding



(c) Drop SNAP/Medicaid Expansion HH's



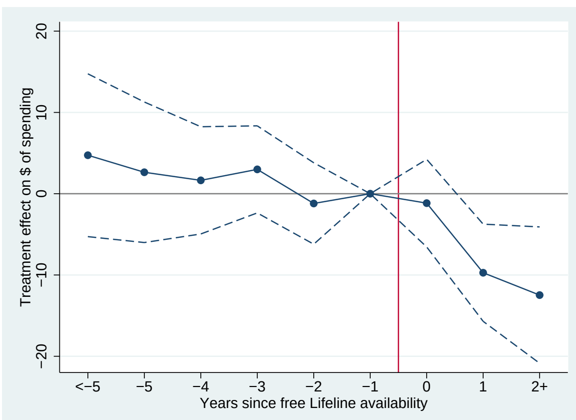
(d) Binned Income Categories



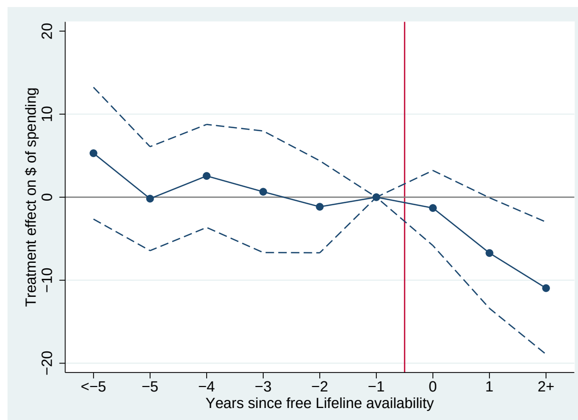
Note: All figures show estimated event study treatment effects on Lifeline-eligible households from triple-differences regression (DDD Model). Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variables is the household's self-reported average monthly wireless spending, measured in dollars. The indicator variable for the year immediately prior to treatment is omitted. All specifications include the following covariates: family size, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Panels (a) through (c) also include cubic terms in income, while panel (d) includes indicators for income bins of width $10k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

Figure A.4: Lifeline Treatment Effect, Alternative Availability Timing Definitions

(a) First Month Take-Up Above 1%



(b) First Month Take-Up Above 5%



Note: All figures show estimated event study treatment effects on Lifeline-eligible households from triple-differences regression (DDD Model). Figure subtitles describe threshold used for defining Lifeline availability. Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variables is the household's self-reported average monthly wireless spending, measured in dollars. The indicator variable for the year immediately prior to treatment is omitted. All specifications include the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.

Figure A.5: Lifeline Treatment Effect, Separate Trends by Treatment Year

Note: Estimated treatment effects on Lifeline-eligible households from a single triple-differences regression (DDD Model), with separate event study treatment trends interacted with year of treatment. Dashed lines represent 95% confidence intervals based on robust standard errors clustered at the state level. Dependent variable is the household's self-reported average monthly wireless spending, measured in dollars. The indicator variable for the year immediately prior to treatment is omitted. Specification includes the following covariates: family size, cubic terms in income, quadratic terms in age, state unemployment rate and house price index, as well as catetogorical indicator variables for family structure, marital status, education, population of area of residence, race, receipt of unemployment benefits, enrollment in other benefit programs, and income over $100k. Observations are weighted using Consumer Expenditure Survey inverse sampling weights.