The Effect of Prices on Fixed and Mobile Telephone Penetration:

Using Price Subsidies as Natural Experiments*

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Abstract

A natural experiment, unintentionally conducted by the price subsidy program Lifeline Assistance, underpins our innovative strategy to estimate consumer demand for communications services. Using a national household panel containing demographic and billing information, we estimate own and cross elasticities of demand for fixed and mobile services and find moderate substitution between the two. We control for the role of income effects by showing the subsidy has little effect on consumption of similar products and services. To account for potential bias due to endogeneity of program participation, a difference-in-differences analysis of re-sampled households results in estimates similar to the cross-sectional analysis. An asymmetry in the response to Lifeline participation arose depending on whether a household added or dropped Lifeline between samplings. We use the estimated demand parameters to simulate the impact of making the Lifeline program universal, or eliminating it altogether. In either case we find the net impact on combined fixed and mobile penetration is small.

Keywords: Universal Service, access demand, fixed-mobile substitution, natural experiment  
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I. INTRODUCTION

The goal of ensuring that basic telephone service is available to the greatest number of citizens is a cornerstone of traditional communications policy. Just as this goal of Universal Service is being realized in many parts of the developed world, new technologies challenge the assumption of what it means to be “connected.” In some cases new technologies are a substitute for or provide enhancement to conventional telephone access, while in other cases they create entirely new capabilities. Wireless telephony is an illustration of both possibilities.

Mobile phones not only provide connection to the public network, they also give users unparalleled mobility. Those capabilities help explain why mobile has eclipsed fixed-line service in most markets even though landlines provide better voice quality and greater data bandwidth. Although the popular press has documented cases of individuals who have “cut the cord,” most households in the US continue to subscribe to both services as if they were economic complements.

The spread of mobile telephone service is posing several questions regarding the design of policies aimed at promoting Universal Service. Would policy goals be more effectively pursued by extending subsidies beyond fixed-line access to include mobile services? Should progress toward Universal Service be measured by rates of penetration for fixed and mobile lines combined? And should Universal Service Obligations on fixed-line carriers be removed once mobile penetration reaches a critical threshold? Answers to these questions depend crucially on the willingness of users to substitute between fixed and mobile services as a means of access.

Empirical studies have investigated the impact of Universal Services programs on telephone penetration, and have estimated the degree of fixed-mobile substitution. Our paper bridges these two literatures.\(^1\) We leverage a natural experiment, “Lifeline Assistance,” conducted as part of the federal Universal Service policy, as an innovative identification strategy for two purposes: estimation of fixed-mobile substitution, and simulation of the effects of policy reform on telephone penetration.

The Lifeline program offers low-income households a fixed discount off the standard monthly rates for basic local service. Because individual states can supplement the federal subsidy, and because they retain some discretion over the implementation of the program, the

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\(^1\) Vogelsang (2010) surveys the empirical literature on fixed-mobile substitution, emphasizing the demand-side factors.
amount of the discount varies across states as do participation rates among eligible households. We are able to investigate household response to Lifeline using a large, national panel. That dataset contains detailed information on service subscriptions and monthly billings, along with demographic characteristics. Although we have fine grained data, we do not estimate a structural model of consumer demand; rather, we take a non-parametric approach that compares fixed-line prices and mobile subscription rates for households that participated in the Lifeline program with those for households that did not.

While we know many of the details of each state’s Lifeline program, we cannot observe directly whether a particular household is eligible, or the discount they would receive were they to participate. However, we can determine which households participate in the program, and in that case estimate the Lifeline discount by the average difference between their fixed-line bills and the bills of similar households that do not participate. We fit a binary choice model of household mobile subscription based on Lifeline participation to estimate households’ responses to lower fixed-line prices. Combining this effect with the estimated Lifeline discount, we find fixed-mobile cross-price elasticities ranging from +0.25 to +0.31, confirming modest substitutability of the two access services. Applying basic consumer demand theory to these estimates, other own and cross-price elasticities that we derive compare favorably to published results in previous work.

Since there is reason to suspect that households may treat the Lifeline discount as if it were an income subsidy, and not a price subsidy, we want to remove any portion of our price elasticity estimates that represent income effects. To isolate the income effect, we estimate choice model of household consumption of other technology products and services which includes an indicator for Lifeline participation. We find that Lifeline participation either has no effect on Pay TV and internet service subscription and ownership of a personal computer, or it reduces consumption of these goods and services—just the opposite that would be predicted if Lifeline was treated as an income subsidy. This result allows us to identify the pure price effect of the Lifeline subsidy on mobile subscription.

The wide variation in observed participation rates forces us to examine the possibility that Lifeline participants are not representative of the general population. We check for sample bias by isolating two states—California and Maine—which, for various reasons, have almost complete Lifeline participation among eligible households. While we are unable to reject the
hypothesis that the fixed-line price effect is different in these two states compared to the full sample, the coefficient of this variable is not different from zero in either of those states. This result suggests that near-mandatory participation results in very different mobile subscription decisions compared to when households are free to choose, and signals a possible self selection bias.

Given our elasticity estimates, we simulate the consequences of two hypothesized changes in Universal Service policy. First we consider the impact on fixed and mobile subscriptions were all households eligible under their state plan to participate in Lifeline Assistance. Second, we simulate the impact on penetration if the Lifeline program were to be eliminated entirely. Using our demand estimates, we simulate the responses of the relevant population. In particular we find that universal participation in Lifeline (among all eligible households) results in a negligible net change in telephone penetration when fixed and mobile services are combined.

A hazard that often arises with policy experiments such as this price-subsidy program stems from non-randomness of participation. We are able to avoid the corresponding bias by exploiting the re-sampling that occurs in our dataset. Re-sampling controls for some unobservables that affect households’ choices of telephone access, but which vary little over the sample period. Applying the standard difference-in-differences specification to the mobile subscription probit, we identify the treatment effect associated with Lifeline participation. The use of re-sampled households results in Lifeline effects that are very close to the estimates for the broader sample, and again confirm the robustness of our estimates.

Section II of the paper collects together the Lifeline program details needed to motivate our model specification. We provide a brief survey of studies that estimate consumer responses to Lifeline, as well as the relevant empirical literature on fixed-mobile substitution in Section III. Before turning to our contribution to this literature, Section IV constructs a simple theoretical framework of household choice among access technologies to motivate our econometric specifications. After describing the household panel dataset used in our estimations, we present our estimation strategy along with checks on its robustness and validity. Section VI uses the sub-panel of re-sampled households to correct for suspected endogeneity of Lifeline participation. The concluding section summarizes the conclusions regarding fixed-mobile substitution and
possible reforms of the Lifeline program, as well as the role of such programs as strategies for identification strategies.

II. THE LIFELINE ASSISTANCE PROGRAM

Universal Service programs encourage demand for and supply of telecommunications access among households that are not connected to the public network. Demand-side programs typically disperse funds directly to low-income, under-served households so as to make phone service more affordable. In total, in 2005 low-income subsidies in the U.S. amounted to about $820 million of the $7.3 billion disbursed by the Universal Service Fund.

Lifeline Assistance is one of three low-income programs introduced by the Federal Communications Commission in 1984 that is overseen by the Universal Service Administrative Company (USAC). Initially, its implementation was at the discretion of individual states but the 1996 Telecom Act made state implementation mandatory. States exercise considerable discretion over the setting of consumer and carrier eligibility requirements and the maximum subsidy levels. Many states supplement federal subsidy levels with their own contributions. Table 1 lists the combined state and federal Lifeline discounts at the beginning and end of our sample period. It is clear that the discount varies not only across states but also over time. The variability in Lifeline discounts is a source of cross-sectional variation that helps to identify the price effects. This same variability, however, may reflect underlying endogeneity related to unobservable traits of the states and their implementation of the Lifeline program.

[Table 1 here]

Eligible households can receive a discount on the monthly charge for basic local telephone service provided by an “Eligible Telecommunications Carrier” (“ETC”). Service

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2 Two companion means-tested programs were created with the same purpose as Lifeline Assistance. The “Link-Up America” (LUA) program, which defrays a certain amount off the installation charges for eligible households, and “toll limitation,” which provides subsidies to households that agree to restrict their long distance calling. The discounts available under LUA are small compared to the recurring discounts under the Lifeline program. Both programs are financed by charges on interexchange calling. Telecommunications Act of 1996, Pub. L. No. 104-104, 110 Stat. 56.

3 As of Sept. 2001, 40 (of 50 states and D.C.) had their own state lifeline program but many fewer had a program to subsidize connection charges beyond the LUA terms. See GAO (2002), Appendix III. At present, just 8 states do not supplement in which case they implement to federal default program (Delaware, Hawaii, Indiana, Iowa, Louisiana, New Hampshire, North Dakota, and South Dakota).

4 The increase of $1.50 in the combined federal and state subsidy is the result of $0.75 increase in the FCC’s Subscriber Line Charge in July 2000 and July 2001. The SLC establishes the minimum federal Lifeline subsidy.
providers are free to apply to become an ETC and must meet certain conditions to be certified. An ETC must offer customers all of the covered services—a voice grade, single party line with local usage and touch tone service, plus access to long distance and 911 services. They must also satisfy requirements for disseminating information on individual eligibility and amount of the subsidy. An ETC may be an incumbent or a competitive carrier (a “CETC”). While all major fixed-line carriers are ETCs, relatively few mobile carriers are certified. Indeed, during our sample period, virtually no cellular service provider was active in dispersing Lifeline funds.

The income test for Lifeline eligibility varies by state (and sometimes by county), by the ETC, and by household characteristics (e.g., household size). The test is expressed as a percentage of the “Federal Poverty Guideline,” which is the same nationwide with the exception of Hawaii and Alaska. Households with incomes lower than 135% of FPG are eligible if their state does not supplement the federal discount. If a state chooses add to the discount, then it can choose a higher FPG eligibility threshold (e.g., 150%). The FPG thresholds for selected states are shown in Table 1. Some states have carve-outs for special groups such as Senior Citizens. Alternatively, eligibility is linked to participation in one or more means-tested state or federal assistance programs. The state of California, for instance, coordinates with local phone companies to automatically enroll a household in Lifeline when it participates in a public assistance program. More commonly, consumers “self certify” their eligibility in the Lifeline program and increasingly ETCs are auditing their customer bases to confirm they meet program requirements. Renewal typically requires annual re-application, with the responsibility on consumers to notify carriers when they are no longer eligible.

Despite such efforts, Lifeline participation is quite low, though not out of line with other means-tested public assistance programs. In 2009, only some 6 million out of the roughly 19

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6 See, op.cit., Title 47, sect. 54.405.
7 In 2001, wireless providers disbursed $150,818 of the $548,421,038 in non-tribal Lifeline support, just 0.0275% of the total. See “LI05 - Annual Low Income Support Amounts by Sate and Company through 2Q2005,” Universal Service Administration Company. Nevertheless, a significant portion of the Lifeline discount funding derived from charges applied to mobile bills. In 2005 about 34% of the USF amounts were generated by mobile carriers.
8 FPGs are issued annually by the U.S. Department of Health and Human Services and published in the Federal Register. They are based on the closely related Federal Poverty Thresholds measure issued annually by the Bureau of the Census.
9 These include Supplemental Security Income (SSI), Medicaid, Federal Public Housing Assistance (FPHA), Low Income Home Energy Assistance Program (LIHEAP), Temporary Assistance for Needy Families (TANF) as well as food stamp and school lunch programs.
million eligible U.S. households receive some income-based telephone access subsidy. Many factors contribute to reducing the number of Lifeline participants. Burton, Macher and Mayo (2007) find that state-level restrictions imposed on the use of Lifeline subsidies plus the burden of the sign-up procedure often cancel out the monetary incentive. The wide variation in participation rates signals potential unobservable factors that could bias our sample of Lifeline households.

The Lifeline discount applies to monthly charges for one line per individual. The amount varies across states as can be seen from Table 1. As of early 2002, the subsidy ranges from $6.75 (the federal minimum) to $14.78 (D.C.) with an average of about $11.00. Discounts can vary within states because they depend on household income and the ETC (ETCs charged different fixed monthly Subscriber Line Charges). This is one reason why we do not attempt to discern households’ actual subscription prices but rather estimate the Lifeline discount using household bill information.

III. LITERATURE REVIEW

Two strands in the literature address the issues of concern here: empirical studies of consumers’ responses to Universal Service programs and econometric demand models of fixed-mobile substitution. We believe our paper is the first to connect these two literatures by leveraging the natural experiment of the Lifeline program to estimate consumer substitution behavior.

A number of studies examine the effectiveness of various telephone tax and subsidy programs using aggregate data. Using the 1990 decennial Census data, Garbacz and Thompson (1997) estimate a grouped logit model of state-level telephone penetration. They find price elasticity for access of -0.0078, which is very much smaller than typical estimates (and not statistically significantly different from zero when lagged penetration is included). Our results

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10 Hauge et al. (2007) estimate a grouped logit model of Lifeline participation in Florida counties, 2003-2005. They find participation elasticity with respect to the average monthly discount on a single residential line of about +0.80. They find also that cellular penetration significantly reduces Lifeline participation, a result consistent with fixed-mobile substitution.

11 In principle, the discount can be applied to a fixed or mobile line but since cellular ETCs were virtually nonexistent during the sample period, it effectively can be applied only to fixed-lines.

12 The discount is composed of four tiers: (1) amount of the Subscriber Line Charge which currently ranges from $3.50 to $6.00 per month; (2) a mandatory federal match which equals $1.75 per month; (3) an
show, however, that amendments to the Lifeline and Link-Up programs increase penetration by a small but statistically-significant amount.

Eriksson, Kaserman and Mayo (1998) measure the combined impact of the Lifeline and Link Up programs on state telephone penetration rates. Estimating a linear probability model using a panel of 48 states over the period 1985-1995, they find average household expenditure on the two programs has a positive and significant impact on fixed-line subscriptions. Further, they find that the size of the impact increases with the incidence of poverty in the state.

Stroup (2004) estimates the impact on telephone penetration of raising income eligibility in all states (from the default minimum of 135% of FPG to 150%). He predicts 2002 Lifeline participation under more inclusive income thresholds, and combines these results with a logit model of household fixed-line subscription. The second estimation implies an income elasticity for telephone subscription, conditional on eligibility, equal to +0.0525 which again is close to traditional estimates for fixed-line subscription.

Ackerberg et al. (2008) regress telephone penetration rates for 9,000 “Census locations” against monthly subscription fees and amortized hookup prices after deducting the combined federal and state Lifeline and Link-Up subsidies. They estimate a price elasticity for fixed service of -0.035 which is at the lower end of the range of own-price elasticities for fixed access. It is important to note that Census data do not distinguish whether households subscribe to fixed or mobile services.

Using annual fixed and mobile subscriptions for 1991-1998 for six Korean provinces, Sung and Lee (2002) estimate non-structural models for new fixed-line connections, and for fixed-line disconnections. They control for the stock of mobile lines but not for mobile prices. They conclude that a 1% increase in mobile lines results in a reduction of between 0.10% and 0.18% for fixed-lines, and an increase of between a 0.14% and 0.22% for fixed disconnection. The own-price elasticity is never significantly different from zero. In line with their study, we examine household disconnections, in our case termination of mobile service in response to the Lifeline discount.

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optional state contribution with a federal match of 50% up to a maximum of $1.75; (4) an additional amount for tribal lands up to $25.

13 Solving for the reduced form of Sung and Lee’s two log-log models, one case show that OLS estimates generate an own-price elasticity of new connections of -0.289 and of -0.684 for disconnections. The elasticity of net fixed-lines is a share-weighted difference of the two elasticities, but only the disconnections elasticity is statistically significant.
Rodini, Ward and Woroch (2003) estimate access substitution between *supplemental* fixed-lines and mobile subscription using the same dataset as the current paper. They find a range of cross-price elasticities of +0.13 and +0.18 for cell subscription relative to fixed-line price, and +0.22 and +0.26 for (second) fixed-line subscription in response to an increase in the mobile price. In this paper we look at mobile subscriptions in response to charges for the *first* fixed-line, although we do not directly estimate the cross-price effect of first fixed-line subscription with respect to mobile price.

IV. A THEORETICAL FRAMEWORK

Our empirical work is based on a simple model of household behavior. We take a standard approach assuming that households maximize utility when making Lifeline participation decisions and deciding on subscriptions. Figure 1 depicts the decision problem facing households starting with the decision by an eligible household to participate in the Lifeline program and followed by choice of fixed and/or mobile services. Each decision is assumed to maximize household utility, conditional on its information at the time.\(^{14}\) For instance, a household decides to participate in the Lifeline program after it has weighed the benefits and costs to the household.

[Figure 1 here]

Starting with the last of these decisions, households decide whether to subscribe to fixed service \((F)\), mobile service \((M)\) and some other consumer service \((O)\) (e.g., cable TV or internet access). Each service is offered under a two-part tariff with access fees \(f_{st} = (f_{st}^F, f_{st}^M, f_{st}^O)\) and per-unit usage prices \(p_{st} = (p_{st}^F, p_{st}^M, p_{st}^O)\) in each state \(s\) at each time \(t\). Household \(h\) decides whether to subscribe to each service \((\sigma_{ht}^i = 1)\) or not \((\sigma_{ht}^i = 0)\), and then how much of each service to use \((q_{ht}^F, q_{ht}^M, q_{ht}^O \geq 0)\). While demand for access to a service depends on its subsequent use, we do not take explicit account of usage in our empirical specification. This is not an egregious oversight since the Lifeline program discounts monthly fixed fees and leaves usage fees unaffected.\(^{15}\) Additionally, a composite commodity captures all other

\(^{14}\) In fact, households may make subscription decisions *before* they become aware of the Lifeline program. Once that happens, however, they can reconsider their earlier access portfolio in light of discounted prices.

\(^{15}\) An even more general model would allow a household’s decision to participate in the Lifeline program to depend on which other households are connected to the network. This would be especially true for friends.
consumption, having price \( p^C_{st} \) and quantity \( q^C_{st} \). A household purchases a consumption bundle using its current income, \( Y_{ht} \), so as to maximize its (expected) utility \( U_h \) which depends on current household characteristics, \( X_{ht} \). Indirect utility depends on prices, income and household characteristics, as well as the attributes of the service:\(^{16}\)

\[
V_{ht}(f^F_{st}, p^M_{st}, X_{ht}, Y_{ht}) \equiv \max_{\sigma_{ht}} \left\{ U_h(q^F_{ht}, q^M_{ht}, q^O_{ht}, q^C_{ht}; X_{ht}) : \sum_{i=1}^{F,M,O} f^i_{ht} \sigma^i_{ht} + \sum_{i=F,M,O} p^i_{ht} q^i_{ht} \leq Y_{ht} \right\}
\]  

We are interested particularly in the optimal subscription choices, \( \hat{\sigma}_{ht} = (\hat{\sigma}^F_{ht}, \hat{\sigma}^M_{ht}, \hat{\sigma}^O_{ht}) \), which solve this expression. The Lifeline discount affects this decision by reducing the monthly fee for fixed service from \( f^F_{st} \) to \( f^F_{st} - d_{st} \), where \( d_{st} \) is the Lifeline discount applied to fixed-line service at time \( t \) in state \( s \).

Subscription to mobile service, in particular, will depend on the incremental utility it generates above the maximum available from other consumption. Decomposing indirect utility into a systematic component and an idiosyncratic disturbance:

\[
v_{ht}(\hat{\sigma}^F_{ht}, \hat{\sigma}^M_{ht}, \hat{\sigma}^O_{ht} \mid f^F_{st}, p^M_{st}, X_{ht}, Y_{ht}) + \sum_{i=F,M,O} \sigma^i_{ht} \epsilon^i_{ht}
\]  

where \( \epsilon^i_{ht} \) are random variables specific to the household and the service alternative and where \( v_{ht} \) denotes maximized utility given subscription choices. Then the probability that a household would choose to subscribe to a mobile service is given by:

\[
\pi^M_{ht} = \Pr\left\{ v_{ht}(\hat{\sigma}^F_{ht}, 1, \hat{\sigma}^O_{ht}) + \epsilon^M_{ht} > v_{ht}(\hat{\sigma}^F_{ht}, 0, \hat{\sigma}^O_{ht}) \mid f^F_{st}, p^M_{st}, X_{ht}, Y_{ht} \right\}
\]  

This expression motivates the use of our choice model for mobile subscription. When \( \epsilon^i_{ht} \) are distributed as \( i.i.d. \) normal, we get the usual binary probit expression:

\[
\pi^M_{ht} = \Pr\left\{ \epsilon^M_{ht} > v_{ht}(\hat{\sigma}^F_{ht}, 0, \hat{\sigma}^O_{ht}) - v_{ht}(\hat{\sigma}^F_{ht}, 1, \hat{\sigma}^O_{ht}) \mid f^F_{st}, p^M_{st}, X_{ht}, Y_{ht} \right\} = 1 - \Phi\left( v_{ht}(\hat{\sigma}^F_{ht}, 1, \hat{\sigma}^O_{ht}) - v_{ht}(\hat{\sigma}^F_{ht}, 0, \hat{\sigma}^O_{ht}) \mid f^F_{st}, p^M_{st}, X_{ht}, Y_{ht} \right)
\]  

where \( \Phi(\bullet) \) is \( c.d.f. \) of standard normal random variable. We follow convention by specifying indirect utility as linear in market prices and household characteristics: \( \pi^M_{ht} = \Phi(\beta Z_{ht}) \) where

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\(^{16}\) Utility is derived only from usage in this formulation; subscription does not convey a stand-alone “option value.”
\[ Z_{ht} = (f_{st}, p_{st}, X_{ht}, Y_{ht}) \] is a vector of explanatory variables and \( \beta \) is a vector of linear coefficients.

We estimate this probit model for households with and without the Lifeline discount. We can then compute the (uncompensated) cross-price elasticity of demand for mobile service:

\[
\eta_{ht} = \frac{d \ln \hat{\pi}_{ht}}{d \ln p_{st}} = \hat{\beta}^F \frac{\phi(\hat{\beta}Z_{ht})}{\Phi(\hat{\beta}Z_{ht})}
\]

where \( \hat{\beta}^F \) is the estimated coefficient of the fixed-line subscription fee. An estimate of market cross-price elasticity is then formed by taking the unweighted average of household elasticities:

\[
\hat{\eta}_t = \sum_h \hat{\eta}_{ht} \quad 17
\]

While this expression gives Marshallian elasticities, compensated elasticities of demand are preferable but are not directly measurable. Nevertheless, since income shares and income elasticities tend to be small for local telephone access, \(^{18}\) the two elasticities should not deviate widely from one another.

Finally we can use the above framework to formalize the household’s decision to participate in the Lifeline program:

\[
\begin{align*}
V_{ht}(f_{st}^F - d_{st}, f_{st}^M, f_{st}^O, p, X_{ht}, Y_{ht}) - c_{ht} & \geq V_{ht}(f_{st}, p, X_{ht}, Y_{ht}) \\
\end{align*}
\]

where \( c_{ht} \) is the utility cost of registering with the program. This cost may be incurred only once, or at annual re-certification, in which case the cost needs to be amortized. We do not observe \( c_{ht} \) yet it is likely to be correlated with other factors governing household subscription choices. Some of these will appear in \( X_{ht} \) which will only be partially observed in the data.

We do not estimate the household participation decision, but use the formal expression to think through potential estimation problems. For instance, households balance the savings from the Lifeline discounts against the costs of participating in the program. Those costs, including the

\(^{17}\) We also compute cross-price elasticity using another method that computes the discrete effect of a $1 increase in fixed-line monthly charge on each household’s likelihood of mobile subscription. The fitted probit model gives the percentage change in mobile subscription from a fixed-line price increase as:

\[
\Delta \hat{\pi}_{ht} / \hat{\pi}_{ht} = [\Phi(\hat{\beta}Z_{ht} + \hat{\beta}^F d_{st}) - \Phi(\hat{\beta}^F Z_{ht})] / \Phi(\hat{\beta}Z_{ht}) \]

We can then compute the arc cross elasticity for household \( h \) in period \( t \) as:

\[
(\Delta \hat{\pi}_{ht} / \hat{\pi}_{ht})(1 / p_{st}^F) = p_{st}^F \times (\Delta \hat{\pi}_{ht} / \hat{\pi}_{ht})
\]

Again, we would take the unweighted average of these values over the estimation sample. We found little difference between the two kinds of elasticity.

\(^{18}\) Taylor (1994) lists 7 early estimates of income elasticities of local access demand which range from +0.15 up to +0.61. No alternative to fixed access was available during the sample periods of the data used to generate these estimates.
hassle of initial application and re-certification and any stigma associated with government assistance, may be correlated with factors affecting subscription decisions. For example, a household’s awareness of the Lifeline program may be related indirectly to the determinants of fixed and mobile subscription. It is likely that such awareness depends on the language spoken in the household, and the level of educational attainment in the household, which, in turn, would determine subscription to fixed and mobile services.

V. HOUSEHOLD DATASET

We use data from a unique national panel that surveys households on their use of consumer communications and electronic products and services. The panel, TNS Telecoms’ ReQuest® Market Monitor, samples about 30,000 households every quarter from the 48 continental states and D.C. We use 10 quarters of the panel running from third quarter of 1999 through the fourth quarter of 2001. The dataset contains responses to a mail survey that includes a long list of household demographic measures.

In parallel, TNS Telecoms conducts a “bill harvesting” of its survey respondents, each of whom is invited to submit a month’s worth of original bills for fixed, mobile, cable/satellite TV and internet services in exchange for a small monetary incentive. Detailed information, including calling and itemized charges, is captured from the paper bills. The bill harvesting offer has a response rate of about 25%, i.e., about 8,500 respondents per quarter. We use these bills to estimate fixed and mobile prices with and without Lifeline discount. We also use the name given by the local phone company to the household’s fixed service to identify Lifeline participants. Over 78,000 households submitted telephone bill information of which slightly more than 6,000, or about 8%, participated in Lifeline. We determine whether a survey respondent is eligible for Lifeline by comparing their household income and size against Federal Poverty Guidelines and the Lifeline FPG threshold set by their states. Of course, actual eligibility may differ, but we lack information on whether individual households qualify for their state Lifeline program.

This dataset is extraordinary in its micro-economic detail and it is based on sound sampling design and procedure. Nevertheless, for our purposes, the dataset has some limitations.

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19 We classify a line as receiving Lifeline Assistance based on our interpretation of the company-specific service description. We selected three flags: (1) "lifeline" appears in service description; (2) reference to "telephone assistance program" (TAP) applying to monthly subscription charges; and (3) "credit" deriving from lifeline or TAP.
First, the unit of observation is the “household” not the “individual.” This contrasts with implementation of the Lifeline program which that extends discounts to individuals. On the other hand, the Lifeline and subscription decisions are likely decisions made jointly by household members. Second, submission of bills is voluntary, introducing another potential source of sample bias. Third, the bills that are submitted sometimes mask useful information. While the database is not a strictly longitudinal panel, some re-sampling does occur: about a quarter of the bill submitters were sampled two or more times over the course of the 10 quarters. In our analysis we take advantage of the re-sampling present in the household panel.

Figure 2 plots Lifeline participation and eligibility rates over ten quarters using the household survey data. It also plots penetration rates for mobile subscriptions for the whole sample and for households that participate in Lifeline. As expected, mobile adoption climbs steadily over time, and while Lifeline eligibility is flat during this period, Lifeline participation jumps over the final few quarters. This increase occurs in parallel with increased mobile subscription among Lifeline households, and during a period of growing state certification of wireless ETCs.

[Figure 2 here]

Figure 3 plots Lifeline participation and eligibility rates against household income, and shows that participation declines steadily with income but stabilizes at about 2% even at the very highest income levels. About 20% of households with incomes of less than $15,000 per year receive the Lifeline discount while only 4% of households with income over $15,000 per year participate in the program. About 60% of all Lifeline households have income under $15,000 per year. These data highlight two anomalies. First, over three-quarters of households with incomes of less than $15,000 per year do not receive Lifeline assistance. Second, some households participate in the Lifeline program in each of the income groups, including the higher one. Clearly, the program does not reach all of the poor, but does attract many who certainly are not poor. Perhaps households continue to participate in Lifeline after an increase in income because they fail to properly re-certify their eligibility. On the other hand, a participating household might simply overstate its actual income.

\[\text{To avoid response bias, we identify households with mobile phones by their survey responses and not by submission of mobile bills.}\]
Cross tabulations show that Lifeline participation is more common in our dataset among those living alone or with relatives, and especially households headed by a female. The same is true for the smallest and largest household sizes.\textsuperscript{22} Other patterns for Lifeline participants revealed by cross tabulating demographic indicators such as presence of children, marital status, education and race of head of household. These differences underpin our concern that self selection by Lifeline households is present.

VI. NON-PARAMETRIC ESTIMATION OF CROSS-PRICE ELASTICITY

A. A Differences Approach

We first measure fixed-mobile substitution by computing the ratio of the percentage difference in mobile subscription to the percentage difference in fixed-line bills where the differences are between Lifeline participants and non-participants. This ratio approximates the textbook cross-price elasticity formula. We estimate the numerator and the denominator separately and then calculate their ratio to generate an estimate of cross-price elasticity.

The denominator—the difference in local bills between the two groups—is derived by regressing actual fixed-line bills $f_{h}^F$ on a dummy for Lifeline participation $D_{h}^{LL}$ along with state and quarter fixed effects:\textsuperscript{23}

$$f_{h}^F = \alpha_0 + \alpha_1 D_{h}^{LL} + \alpha_2 s + \alpha_3 t + \epsilon_{hst},$$

(6)

The denominator is estimated by the least-squares coefficient on a Lifeline dummy, $\hat{\alpha}_1$. Table 2 presents the results of these regressions. All Lifeline dummies are negative (indicating lower fixed bills for participants) and highly statistically significant when we compute robust standard errors clustering for each state. Lifeline participation reduces local fixed bills by an amount of $8.32-10.62$ depending on which fixed effects are included.\textsuperscript{24} These estimates are consistent with the discounts for the selected states that appear in Table 1. Compared to average fixed-line

\textsuperscript{21} Note that a redistribution of Lifeline Assistance away from households with incomes over $15,000 per year and toward households with incomes under $15,000 per year would increase the fraction of these poorer households with assistance only by about one-third.

\textsuperscript{22} For each of the cross tabs, a Chi-Square test rejects the hypothesis of equal treatment across cells.

\textsuperscript{23} The dependent variable and the Lifeline indicator are not distinguished explicitly by state and time because we treated the data as if they were from a single cross section of households.
bills (given as the regression constant), the fixed-line bills of Lifeline participants are lower than those of non-participants by 29.7%-30.8%.

[Table 2 here]

Regressions were run taking the logarithm of fixed-line bills as the dependent variable. Table 2 reports the estimated coefficients ranging from -0.53 to -0.44 which, when translated into percentage changes, implies that local bills are 35.5% to 40.8% lower for Lifeline participants, or about 5% higher than those implied by the absolute level regression.\(^{25}\)

Turning to the numerator—the change in mobile subscription rates—we fit a probit model of mobile subscription with a Lifeline dummy indicator and other controls.\(^{26}\) We include demographic determinants \(X\) in some of the probit specifications:

\[
\Pr\{\text{Mobile}_h\} = \Phi(\beta_0 + \beta_1 D_{LL} + \beta_2 X_1 + \beta_3 X_2 + \beta_4 X_3)
\]  

(7)

The coefficient \(\hat{\beta}_1\) gives an estimate of the change in the probability of subscribing to mobile service from participating in the Lifeline program. The maximum likelihood estimates of the Lifeline dummies are consistently negative (confirming substitution away from mobile when there is a fixed-line subsidy) and highly statistically significant (again computing robust standard errors clustering on states).

The upper panel in Table 3 presents the results of several probit estimations. The coefficients of the Lifeline indicator are the marginal effects of the probability of subscribing to mobile. We suppress the coefficient estimates for the many categorical control variables, and instead indicate whether the variables in a demographic measure are jointly significant. Because mobile subscription rates have increased dramatically over time, we include quarter dummies, and because Lifeline participation rates differ considerably across states, we include state dummies. The main conclusion is that we estimate that Lifeline customers are 19.1% less likely to subscribe to mobile service. Once income and demographic variables are taken into account, however, the Lifeline effect falls to 3.6%. This residual Lifeline effect is still statistically significantly different from zero.

\(^{24}\) We drop from the estimation sample those households that report their local bill is a business line. We also drop a small percentage of observations with unrealistically large fixed-line bills, \(i.e.,\) in excess of $100 per month.

\(^{25}\) We also estimated regressions in which Lifeline was interacted with dummies for the various states; the results were qualitatively the same.
Given the two estimates we can then calculate the ratio $\frac{\hat{\alpha}}{\hat{\beta}}$. For instance, using estimates from the specifications having just state and quarter fixed effects, we have an estimate of the cross-price elasticity: $\frac{-49.7\%}{-29.7\%} = 1.673$ indicating a reasonably large substitution from fixed to mobile. This elasticity is very precisely estimated with a standard error of 0.122.\(^{27}\)

We cannot be certain, however, that a portion of this cross elasticity reflects an income effect as in the case where Lifeline households treat the discount as an income supplement even though it applies solely to (fixed) phone service. To address this concern, we examine the effect of Lifeline assistance on household consumption of other products and services that appear to share same characteristics as the phone subscription decisions, including Pay TV and Internet services and personal computer ownership. There is no reason to suspect a non-negligible cross-elastic effect from lower telephone prices to these other demand decisions (e.g., no income effect). However, since Lifeline households tend to be poorer, and these are considered normal goods, we would expect Lifeline customers to choose these products and services less often for purely income related reasons. If Lifeline results in an income effect, then we would expect it to show up positive and significant for household purchases of other services similar to telephone service.

We estimated probit models of household demand for Pay TV and ISP services, and for PC ownership included in household survey responses. Probit results are presented in Table 4. The sample used for these estimations is limited to households that report less than $30,000 annual income. This is the upper limit for eligibility in the Lifeline program across all states given the federal restrictions and the various state income thresholds. The simple specification of each probit includes only state and time fixed effects alongside the Lifeline dummy. The second specification includes many demographic variables and importantly household income. As we look across the three demand models we find that, except for Pay TV, either Lifeline

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\(^{26}\) About 15% of households in the dataset report having one or more prepaid cell phones. We do not treat these households differently from those that subscribe to a post-paid mobile service.

\(^{27}\) This standard error is estimated using the Delta method in which the moment of a nonlinear function of the estimates is approximated by a first-order Taylor expansion. See Oehlert (1992). Specifically, the standard error of the fraction that estimates the cross-price elasticity is given by:
participation does not have a statistically significant effect on consumption, or it has a significant and negative effect. The former outcome supports the notion that Lifeline does not have an income effect on these consumer decisions. In contrast, a significant negative coefficient on the Lifeline dummy points to a factor other than income. In all of the estimated models the coefficients on income were positive and significant at the 1% level, confirming strong positive income effects. Apparently, Lifeline participation is motivated by other factors that are negatively correlated with these consumption decisions, but are not related to income level.

We infer from these results that, for these three choices, the income and demographic variables adequately capture all, or nearly all, of the income effect expected from Lifeline households, suggesting these same income and demographic factors adequately capture the income effect expected from Lifeline households’ mobile subscription decisions. Accordingly, the 19.1% lower probability of subscribing to a mobile service by Lifeline households can be decomposed into a 15.5% income effect and a 3.6% cross price effect. This implies that our best estimate of the compensated cross-price elasticity is then: $-9.21\%/ -29.7\% = +0.310$. While not extremely large, these estimates confirm non-negligible substitution between fixed and mobile telephone services, at least for households receiving Lifeline assistance.\(^{28}\)

### B. Implications for Other Price Elasticities

Our data do not capture a parallel natural experiment that would allow estimation of the effect of mobile price changes on fixed-line subscription. However, demand theory provides a way to derive other demand elasticities.\(^{29}\) The Slutsky symmetry condition implies that the cross-elasticity of an increase in the fixed-line price on mobile subscription will take on the same sign as the effect of a mobile price increase on fixed telephone service take-up. Specifically, for small changes in real income, budget-share weighted cross-price elasticities should be nearly equal:

$$w_{FM}\eta_{FM} \approx w_{MF}\eta_{MF},$$

where \( \eta_{ij} = \frac{d\ln O_i}{d\ln P_j} \) is the cross-price elasticity and \( w_i = p_iO_i \) is budget-share

\[
\text{Var} \left( \frac{aX}{bY} \right) = \left( \frac{a^2}{b^2} \right) \left( \frac{1}{(EY)^2} \right) \left[ \text{Var} X + \frac{(EX)^2}{(EY)^2} \text{Var} Y \right] = \left( \frac{28.01^2}{0.3884^2} \right) \left( \frac{1}{(8.32)^2} \right) \left[ (0.013)^2 + \frac{(0.191)^2}{(8.32)^2} - (0.22)^2 \right] = (0.122)^2.
\]

\(^{28}\) Using the regression results for logarithmic changes in the fixed bill, the estimated cross elasticity becomes: $-9.21\% / -36.4\% = +0.253$.

\(^{29}\) See Deaton and Muelbauer (1980, p. 43).
weight. Among households submitting bills, collective expenditure on mobile is about one-third of their spend on fixed-line. Adjusting this ratio to reflect the disproportionately low frequency of submission of mobile bills, we can conclude that the ratio of the budget weights for mobile to fixed-line is about one-half. Accordingly, applying this ratio to the Slutsky symmetry condition generates cross-price elasticity from mobile prices to fixed-line demand of $+0.126$ to $+0.155$.32

We can also infer own-price elasticities by applying the homogeneity condition:33

\[ \eta_{FF} = -\eta_{FM} + \eta_F^Y \quad \text{and} \quad \eta_{MM} = -\eta_{MF} + \eta_M^Y. \]

Typical estimates of income elasticities for fixed and mobile access are about $+0.1$ and $+0.5$, respectively. If the net effect of all other cross-elasticities is negligible, these estimates imply own-price elasticities of about $-0.23$ to $-0.26$ for fixed service and about $-0.75$ to $-0.81$ for mobile. These own-price elasticities for mobile subscription are quite close to estimates from previous studies in the literature.34 The fixed-line own-price elasticities are more elastic than is typically found in the literature. However, previous studies rarely take account of the prices of substitutes as we do here, and they generally rely on geographic variation for identification, and in that case the cost differences that arise across markets likely affect all forms of access.35

C. Implications of Lifeline Effect for Mobile Penetration

We next examine whether the results for Lifeline participants are valid for the wider population. There are several reasons why this may not be the case. First, the treatment group in the quasi-experiment is drawn disproportionately from low-income households. Second, the

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30 This expression ignores the complications of the non-linear pricing schemes used for these services.
31 If all other components of fixed-line service were added into the bill, including long distance calling, vertical features and taxes, the average fixed bill would increase to $52.30.
32 This is slightly more elastic than was found for second phone lines using the same data but a different estimator. See Rodini, Ward and Woroch (2003).
33 Deaton and Muelbauer (1980), op. cit., p. 16. In general, the homogeneity condition implies that the sum of own- and cross-price elasticities and the income elasticity equals zero: \( \sum_{j \neq i} \eta_{ij} + \eta_i^Y + \eta_i^Y = 0 \) where the \( \eta_{ij} \)'s are own and cross price elasticities and the \( \eta_i^Y \)'s are income elasticities.
34 E.g., Hausman (1997) estimates a market own-price elasticity of about -0.51 using aggregate data on the 30 largest U.S. metro areas. His estimate is based on data from the 1989-93 period, a time when prices were significantly higher than in our sample period. Using more recent data, Hausman (2000) estimates own-price elasticity to be -0.71, but concludes it is not statistically different from his earlier estimate.
35 Note that, with the above estimates, a 1% increase in the prices of both fixed and mobile services results in a 0.1% and 0.5% reduction in fixed and mobile subscriptions, respectively, which approaches the expected estimates.
same household could be eligible for a Lifeline discount in one state but not in another due to state-specific eligibility rules. Relatedly, a household that participates in the Lifeline program in one state might receive a different discount if it participated in the program in another state. Finally, due to some unobserved attributes of state programs, a household may choose to participate in one state but not in another when it is eligible for both.

To investigate the first of the above concerns, we again divide the sample into households with incomes below $30,000 per year and those with incomes above that level, and repeat the regressions for fixed-line bills and the probit for mobile subscriptions, for each of the two income groups. Table 5 contains the results. As expected, the decrease in the average fixed bill attributable to Lifeline is considerably larger for the low-income group. A cross-equation test strongly rejects equality of the Lifeline dummy for the two groups with Chi-squared statistics of 89.51 and 173.18 for the absolute level and the logarithmic regressions, respectively. It is not surprising that the low-income group is more price elastic than the high-income group—though both groups display a fair degree of substitutability between fixed and mobile. For the mobile subscription probits we find little difference for the effect of Lifeline participation. Indeed, we cannot reject the null that the coefficients on Lifeline dummy are the same in the two income groups with a p-value of 0.4258.

[Table 5 here]

Setting aside the various threats to the validity of our results, we simulate the consequences of two innovations in Universal Service policy in terms of fixed and mobile penetration. First, we ask what mobile subscription rates would be if all individuals who were eligible for Lifeline in fact received the subsidy. Second, we measure the effect of totally eliminating the Lifeline subsidy. To characterize these counterfactuals, we use the estimated probit model, and then switch the Lifeline dummy on or off for certain households in the estimation sample. For instance, to examine mobile penetration if all eligible households participated in Lifeline, we set their Lifeline dummy to 1 if it had a value of 0 in the estimation. Setting all Lifeline dummies to 0 generates a prediction for mobile subscription as a result of eliminating Lifeline. The results are summarized in Table 6. The impacts are reported for eligible households only, and the entire sample. For completeness, we re-estimate the probit model using only eligible household to remove the influence of high-income households on parameter estimates.
First, we note the greater impact of changes in Lifeline participation on eligible households compared to the complete sample. This price sensitivity is confirmed if we compare the probit estimates for the two groups using only the eligible population. Second, comparison of the impact of the two policy innovations shows that the move to universal Lifeline participation reduces mobile penetration by more than it would be increased were Lifeline to be eliminated entirely. In other words, elimination of Lifeline would be recommended were mobile penetration the only metric used to judge this Universal Service program. Finally, taking account of the impact on both fixed and mobile penetration, we find that extending Lifeline to the entire eligible population has little effect. Based on the fact that Lifeline participation reduces fixed bills by about 30% on average, and applying a typical own-price elasticity of fixed access of -0.1, we would expect about a 3% increase in fixed subscriptions among eligible non-participants. Since our simulations show a mobile subscription decrease of about 2.5% among eligible households, net combined fixed and mobile penetration increases little over from universal participation in the Lifeline program.

VII. PANEL DATA ANALYSIS OF RE-SAMPLED HOUSEHOLDS

A. Endogeneity of Lifeline Participation and Re-Sampling

While the Lifeline program provides us with a creative identification strategy, it also poses some risks that are common to quasi-experiments. As emphasized above, application of the “treatment” in this experiment, i.e., participation in the Lifeline program, is not random but is correlated to mobile subscription behavior. Given that Lifeline participation varies so much across states, we can expect unobserved heterogeneity among the two groups even for states with similar eligibility requirements and price subsidies. More formally, our suspicions are partly confirmed by comparing Lifeline participants and non-participants. Simple cross tabulations of Lifeline participation show an uneven pattern. On the one hand, \( t \)-tests show no statistical difference between the two groups in terms of mobile subscription rates and cellular bill amounts. On the other hand, the equality of the means of the two groups is rejected in the case of age of head of household, presence and ages of children, and whether the household has moved recently.
As expected, there are substantial differences between the two groups in terms of annual income and household size—the main criteria used to screen households for Lifeline eligibility. Nevertheless statistical differences in income and household size persist when t-tests are performed on the subsample of eligible households.

Taking the analysis a step further, we observe that two states, California and Maine, have near complete participation by eligible households in their Lifeline programs (see Table 1). Is it possible that, compared to states with much lower participation rates, the models estimated using these high-participation states differ? To answer this question, we fit the mobile subscription probit and then test whether the Lifeline coefficients from the two sub-samples are equal (see Table 7). We reject equality when just state and quarter dummies are included, but not if demographics are also included.

[Table 7 here]

Ultimately, there is sufficient reason to look for ways to correct for endogeneity in Lifeline participation. Two conventional methods are available: instrumental variable estimation; and sample selection correction. Instrumental variables estimation is not so well suited to our case since, at least for mobile subscription, we want to estimate a nonlinear choice model. Sample selection methods offer an approach that would require estimating a separate model for Lifeline participation. While these methods are principally designed to correct for sample censoring/truncation, sample selectivity techniques have been developed to deal with non-randomness of treatment. We note that the eligibility criteria (i.e., income, household size, age of head of household) likely belong in the mobile subscription specification, and excluded variables from the mobile subscription probit will not cause problems.

We next look at the properties of the panel dataset to find a way to correct for the non-randomness of Lifeline treatment. As noted previously, a portion of households in the dataset were re-sampled at least once during the 10 quarters. In fact, 46% of survey respondents were sampled more than once. About a third of all households were sampled twice, with the remainder sampled either 3 or 4 times. In addition, re-sampled households were half again more likely to submit their bills.

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36 Sample truncation is a more appropriate description for the subset of households that submit their bills. In fact, when compared to the rest of the sample, bill submitters have significantly lower rates of mobile, Pay TV and internet service subscription, and lower rates of PC ownership; those households tend to skew older and smaller.
Re-sampling of households offers a means to correct for the omitted variable bias that arises as a result of endogeneity of the participation decision. First, it controls for time-invariant factors that affect subscription behavior which are not observed. On the downside, some factors may change between samplings. In fact we find surprisingly little movement in key household variables. For instance, less than one-half of one percent of the re-sampled households changed their income group between samplings. A little more than 1% of re-sampled households changed states between successive samplings, and since this meant that those households would be subject to different Lifeline programs, they were dropped from the estimation.

Second, re-sampling allows us to observe how households respond to the loss as well as the gain of the Lifeline subsidy. Some households that did not receive Lifeline when first sampled did so at a second sampling. They represent the treatment group in the standard before-after quasi-experiment; we are interested in whether those households signed up for fixed-line service in response to the discount. The second scenario is when households lose their Lifeline discount between samplings, in which case we might expect disconnection of fixed service. The two scenarios allow us to detect any asymmetry in the elasticity of mobile activation and de-activation.

The before-after comparison enabled by looking at re-sampled households has some limitations. We classify the first sampling as “before” and the second as “after,” which ignores the fact that the re-sampling pairs can occur anywhere over the 10 quarters, with conditions facing households varying accordingly. This aspect is likely to give rise to autocorrelation in panel structure, leading to underestimation of standard errors. We note that Bertrand et al. (2004) find that bias is greatly reduced if the two observations are grouped as before and after regardless of timing—an estimation strategy we adopt here.

B. A Difference-in-Differences Approach

Given these cautions, we perform a rather standard difference-in-differences (DiD) analysis on households that were sampled exactly twice during the sample period. Because

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37 This situation is rare (less than 20% of the number that signed up for Lifeline between samplings). Few studies have modeled service disconnection; an exception is Sung and Lee (2002) who estimate separate models for disconnections (of fixed-line service) and new connections.

38 Of the possible 1 to 9 quarters between samplings, the average is 3.75 quarters.

39 Other of the usual caveats regarding DiD also apply here. Time between samplings may be too short to register the full effects of the Lifeline program, or so long that the effect dissipates before the household is re-sampled.
these households submitted bills we can observe whether they participate in Lifeline. We then estimate a household-level version of DiD analysis using a probit specification:

\[ \Pr(Mobile_{hts}) = \Phi(\beta_0 + \beta_L D_{LL}^{ht} + \beta_{ss} + \beta_{hx} X_{hx}) \]  

(8)

whereas before, \( h \) identified the household and \( s \) the state, now \( t = 1,2 \) represents the respective “before” and “after” time periods. The dummy variable \( D_{LL}^{ht} \) indicates whether the household participates in Lifeline at the time of the \( t \)-th sampling. The differences-in-differences estimate \( \beta_L \) is estimated using Maximum Likelihood.

Estimation results are presented in Table 8. The above probit model was estimated for three samples. The first includes all households that were sampled exactly twice and which submitted bills. The second and third samples differ in terms of which households should be considered the “control group.” The first is the more usual case where households either never participate in Lifeline or begin participating between the two samplings. The second is the case in which the control group has Lifeline at both times, and the treatment group drops Lifeline before the second sampling.

[Table 8 here]

Estimates of the coefficient of the Lifeline dummy are surprisingly close to those for the full sample where re-sampling is ignored (compare with results in Table 3). This suggests that endogeneity in Lifeline participation does not greatly impact on our earlier estimates. The coefficient of the before-after dummy confirms that mobile penetration increases quite dramatically over the sample period (i.e., about 9% on average between samplings for all households sampled twice). The second subsample has the usual form of a before-after experiment, in which no household initially has Lifeline, and then some receive treatment (Lifeline) before they are observed again. Relative to the control group that does participate in the Lifeline program either before or after, the treatment group shows a 3.9% reduction for mobile, for the more complete specification. This is very close to the 3.7% reduction estimated earlier. The last sample includes households that received the Lifeline discount when first sampled. Some of them lose the subsidy before the second sampling. We find that the treatment effect is 7.7% though not significantly different from zero. The interpretation is that, among those households that lose Lifeline, mobile subscription increases by 7.7%. Consequently, there is some evidence of asymmetry in response to extending Lifeline to additional households, but a
symmetric response to withdrawing discount from current participants. Such a conclusion is not possible from a purely cross-sectional model.

VIII. CONCLUSIONS

In this paper, we treat a Universal Service program, Lifeline Assistance, as a natural experiment that generates data on consumer responses to price differences. Household and bill level data allow us to estimate fixed-mobile substitution as a response to the price subsidy. The estimated cross-price elasticities are modest but in line with other published estimates, and are also quite robust to different sub-samples and endogeneity risks. Coming full circle, our estimates offer some predictions regarding reforms to this particular Universal Service program. For instance, we simulated universal participation in the Lifeline program and found a negligible impact on telephone penetration when measured by fixed and mobile access combined.

We believe that our identification strategy could be usefully applied by other researchers conducting empirical work on telecommunications. The techniques in the rapidly growing field of “program evaluation” hold great promise for telecommunications economics given its success in other areas, notably labor economics. Critical to application of these methods is the ability to deal with the presence of endogeneity which typically arises.

In our study, endogeneity arises because participation in any Universal Service program, whatever its objective, is necessarily voluntary, and hence, likely to be endogenous to the effect of the policy itself. Another source of endogeneity—not explored in this paper—is a policy design that is conditional on historical records. This circumstance would arise in our setting if the size of the Lifeline discount and its eligibility thresholds were set by regulators and lawmakers in response to the state’s past progress toward Universal Service. Such inter-dependence should be taken into account in order to isolate the marginal effects of the actual policy.40

Future research could be based on quasi-experiments to shed light on current issues confronting telecommunications policy makers. One topical issue in the area of Universal Service is whether and how to promote adoption of broadband. Our analysis of fixed-mobile substitution raises the possibility that wireless broadband offers a potential substitute for the fixed-line alternatives of DSL and cable modem services. Although our cross-price elasticity

40 Besley and Case (2000) is an example of how to use political variables correlated with the terms of the program to serve as instruments.
estimates are unlikely to be transferable to this setting and our dataset is not sufficiently up to
date to address voice-data substitution directly we believe, nevertheless, that the program
evaluation strategy in this paper would provide a fruitful way to gain insights into these and
similar policy questions.
REFERENCES


Figure 1: Lifeline Decision Tree and Fees

Eligible?
  | Register?
  | yes
    | mobile only: $f^M_{st}$
    | fixed only: $f^F_{st}$
  no
    | no service: 0
    | fixed only: $f^F_{st}$
    | mobile only: $f^M_{st}$
    | fixed and mobile: $f^F_{st} + f^M_{st}$
Figure 2: Mobile Penetration & Lifeline Participation Over Time

- Mobile Penetration
- Lifeline Participation
- Mobile Penetration among Lifeline HHs
- Eligibility Rate

Source: TNS Telecoms ReQuest Survey

Figure 3: Lifeline Participation and Eligibility by Income

- Lifeline Participation
- Eligibility Rate

Source: TNS Telecoms ReQuest Survey, State Eligibility Requirements.
Table 1: Lifeline Subsidies, Eligibility and Participation Rates For Selected States

<table>
<thead>
<tr>
<th>State</th>
<th>State + federal Lifeline discount</th>
<th>Income eligibility as % of FPG</th>
<th>Actual number of participants, 2000</th>
<th>Participation rate among eligible households, 2000</th>
<th>Statewide % of households participating</th>
<th>% of TNST households with Lifeline</th>
</tr>
</thead>
<tbody>
<tr>
<td>California</td>
<td>$10.50 $12.00</td>
<td>150%</td>
<td>3,196,657</td>
<td>119.2%</td>
<td>27.79%</td>
<td>32.88%</td>
</tr>
<tr>
<td>Connecticut</td>
<td>7.00 8.50</td>
<td>150%</td>
<td>64,745</td>
<td>32.3%</td>
<td>4.97%</td>
<td>14.18%</td>
</tr>
<tr>
<td>Florida</td>
<td>10.50 12.00</td>
<td>125%</td>
<td>134,281</td>
<td>13.5%</td>
<td>2.12%</td>
<td>5.4%</td>
</tr>
<tr>
<td>Illinois</td>
<td>7.50 9.00</td>
<td>125%</td>
<td>57,816</td>
<td>9.0%</td>
<td>1.26%</td>
<td>3.8%</td>
</tr>
<tr>
<td>Maine</td>
<td>10.50 12.00</td>
<td>130%</td>
<td>76,367</td>
<td>99.2%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Massachusetts</td>
<td>13.00 14.50</td>
<td>175%</td>
<td>165,519</td>
<td>28.4%</td>
<td>6.77%</td>
<td>15.11%</td>
</tr>
<tr>
<td>Michigan</td>
<td>8.25 9.75</td>
<td>150%</td>
<td>141,541</td>
<td>20.1%</td>
<td>3.74%</td>
<td>3.62%</td>
</tr>
<tr>
<td>New Jersey</td>
<td>5.25 6.75</td>
<td>150%</td>
<td>29,095</td>
<td>5.9%</td>
<td>0.95%</td>
<td>4.26%</td>
</tr>
<tr>
<td>New York</td>
<td>10.50 12.00</td>
<td>150%</td>
<td>586,660</td>
<td>34.6%</td>
<td>8.31%</td>
<td>14.48%</td>
</tr>
<tr>
<td>Ohio</td>
<td>5.25 6.75</td>
<td>150%</td>
<td>167,213</td>
<td>19.9%</td>
<td>3.76%</td>
<td>5.15%</td>
</tr>
<tr>
<td>Pennsylvania</td>
<td>9.00 10.50</td>
<td>150%</td>
<td>48,975</td>
<td>5.5%</td>
<td>1.03%</td>
<td>2.48%</td>
</tr>
<tr>
<td>Rhode Island</td>
<td>10.50 12.00</td>
<td>175%</td>
<td>47,412</td>
<td>48.1%</td>
<td>11.61%</td>
<td>18.76%</td>
</tr>
<tr>
<td>Texas</td>
<td>10.50 12.00</td>
<td>125%</td>
<td>258,812</td>
<td>18.6%</td>
<td>3.5%</td>
<td>1.68%</td>
</tr>
<tr>
<td>U.S. Total/Average</td>
<td>5,845,934</td>
<td></td>
<td>5,845,934</td>
<td>30.7%</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Table 2: Regression Results of Effect of Lifeline on Local Fixed Bill

<table>
<thead>
<tr>
<th></th>
<th>Dependent Variable: Local Bill</th>
<th>Dependent Variable: Log of Local Bill</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Constant</strong></td>
<td>33.38 (0.06)***</td>
<td>3.40 (0.00)***</td>
</tr>
<tr>
<td><strong>Lifeline Dummy</strong></td>
<td>-10.27 (0.21)***</td>
<td>-0.51 (0.01)***</td>
</tr>
<tr>
<td><strong>Quarter dummies</strong></td>
<td><strong>NO</strong></td>
<td><strong>NO</strong></td>
</tr>
<tr>
<td><strong>State dummies</strong></td>
<td><strong>NO</strong></td>
<td><strong>NO</strong></td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>79,918</td>
<td>79,916</td>
</tr>
<tr>
<td><strong>R-squared</strong></td>
<td>0.030</td>
<td>0.073</td>
</tr>
<tr>
<td><strong>Implied % Difference</strong></td>
<td><strong>-30.8%</strong></td>
<td><strong>-40.1%</strong></td>
</tr>
</tbody>
</table>

Note: standard errors in parentheses; significance levels denoted by: * p<0.10, ** p<0.05, *** p<0.01

Table 3: Probit Estimation of Mobile Subscription

<table>
<thead>
<tr>
<th>Probit Estimation Results</th>
<th>Dependent Variable: Mobile subscription indicator</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td><strong>Full Sample</strong></td>
</tr>
<tr>
<td>Lifeline dummy</td>
<td>-0.191*** (0.013)</td>
</tr>
<tr>
<td>Quarter dummies</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>State dummies</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>Income dummies</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>Age dummies</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>Household composition</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>Household size</td>
<td>Sig* (0.007)</td>
</tr>
<tr>
<td>Kids indicators</td>
<td>Sig*** (0.007)</td>
</tr>
<tr>
<td>Moved recently?</td>
<td>Sig* (0.007)</td>
</tr>
<tr>
<td></td>
<td><strong>N</strong> 79,350</td>
</tr>
<tr>
<td></td>
<td><strong>Pseudo R-squared</strong></td>
</tr>
<tr>
<td></td>
<td><strong>Log Likelihood</strong></td>
</tr>
</tbody>
</table>

Mobile Penetration

Computing Lifeline effect on mobile penetration:

- Mobile penetration in estimation sample: 38.44% 39.10% 15.72% 16.64%
- Probit marginal effect of Lifeline: -19.10% -3.60% -3.70% -19.20%
- Lifeline effect in percentage terms: -49.68% -9.21% -23.54% -19.23%

Simulating Lifeline policy change:

- Eligible sample penetration: 34.83% 16.85% 15.72% 16.64%
- Expand participation to all eligible: 20.56% 15.14% 12.92% 14.19%
- Eliminate Lifeline: 39.21% 17.37% 16.62% 17.42%
- Effect of expanding participation: -14.27% -1.71% -2.80% -2.45%
- Effect of eliminating Lifeline: 4.38% 0.52% 0.90% 0.78%

Note: State-clustered robust standard errors in parentheses; significance: * p<0.10, ** p<0.05, *** p<0.01
### Table 4: Effect of Lifeline Assistance on Demand for Pay TV & Internet Services, PC Ownership

<table>
<thead>
<tr>
<th></th>
<th>Pay TV Subscription</th>
<th>ISP Subscription</th>
<th>PC Ownership</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lifeline marginal effect</td>
<td>-0.007</td>
<td>-0.059</td>
<td>-0.069***</td>
</tr>
<tr>
<td></td>
<td>0.033*</td>
<td>-0.023**</td>
<td>-0.034**</td>
</tr>
<tr>
<td>Quarter dummies</td>
<td>Sig***</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>State dummies</td>
<td>Sig***</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Income dummies</td>
<td>Sig***</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Age dummies</td>
<td>NotSig</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household composition</td>
<td>Sig***</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household size</td>
<td>NotSig</td>
<td>NotSig</td>
<td>NotSig</td>
</tr>
<tr>
<td>Number of children</td>
<td>NotSig</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Moved recently?</td>
<td>Sig***</td>
<td>Sig*</td>
<td>Sig**</td>
</tr>
<tr>
<td>Marital status</td>
<td>NotSig</td>
<td>NotSig</td>
<td>NotSig</td>
</tr>
<tr>
<td>Race dummies</td>
<td>Sig***</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Hispanic origin?</td>
<td>NotSig</td>
<td>Sig**</td>
<td>Sig**</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>38,087</td>
<td>35,887</td>
<td>38,087</td>
</tr>
<tr>
<td><strong>Psuedo-R2</strong></td>
<td>0.016</td>
<td>0.027</td>
<td>0.015</td>
</tr>
<tr>
<td><strong>Log Likelihood</strong></td>
<td>-23,490</td>
<td>-21,830</td>
<td>-23,219</td>
</tr>
<tr>
<td><strong>Mean choice</strong></td>
<td>0.680</td>
<td>0.682</td>
<td>0.309</td>
</tr>
</tbody>
</table>

Note: "Sig" indicates the joint test of all dummies in a category equaling zero rejected at significance level: * p<0.10, ** p< 0.05, *** p<0.01

### Table 5: Effect of Lifeline Assistance For Low and High Income Households

<table>
<thead>
<tr>
<th></th>
<th>Low Income</th>
<th>High Income</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Fixed bill</td>
<td>Log of fixed bill</td>
</tr>
<tr>
<td>Lifeline marginal effect</td>
<td>-7.751***</td>
<td>-0.431***</td>
</tr>
<tr>
<td></td>
<td>(0.241)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Quarter dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>State dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Income dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Age dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household composition</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household size</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Number of children</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Moved recently?</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Marital status</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Race dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Hispanic origin?</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td><strong>Constant</strong></td>
<td>30.196***</td>
<td>3.063***</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>38,087</td>
<td>35,887</td>
</tr>
<tr>
<td><strong>R-squared</strong></td>
<td>0.111</td>
<td>0.075</td>
</tr>
</tbody>
</table>

Note: "Sig" indicates the joint test of all dummies in a category equaling zero rejected at significance level: * p<0.10, ** p< 0.05, *** p<0.01
### Table 6: Simulating the Change in Lifeline Participation on Mobile Penetration

<table>
<thead>
<tr>
<th></th>
<th>X</th>
<th>X</th>
<th>X</th>
<th>X</th>
</tr>
</thead>
<tbody>
<tr>
<td>Quarter fixed effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>State fixed effects</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Demographic variables</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Estimation sample</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Estimation sample size</td>
<td>78,647</td>
<td>78,647</td>
<td>74,825</td>
<td>15,291</td>
</tr>
<tr>
<td>Simulation population</td>
<td>Estimation</td>
<td>Eligible</td>
<td>Estimation</td>
<td>Eligible</td>
</tr>
<tr>
<td>Simulation subsample size</td>
<td>78,647</td>
<td>78,647</td>
<td>78,647</td>
<td>78,647</td>
</tr>
<tr>
<td>Mobile penetration in simulation subsample</td>
<td>38.8%</td>
<td>36.3%</td>
<td>38.8%</td>
<td>35.0%</td>
</tr>
<tr>
<td>Mobile penetration if all eligible households participate</td>
<td>36.5%</td>
<td>24.0%</td>
<td>36.1%</td>
<td>20.8%</td>
</tr>
<tr>
<td>Mobile penetration if Lifeline eliminated</td>
<td>40.1%</td>
<td>40.1%</td>
<td>40.4%</td>
<td>39.6%</td>
</tr>
<tr>
<td>Change in mobile subscription of complete Lifeline participation</td>
<td>-2.3%</td>
<td>-12.3%</td>
<td>-2.6%</td>
<td>-14.3%</td>
</tr>
<tr>
<td>Change in mobile subscription of eliminating Lifeline</td>
<td>1.3%</td>
<td>3.7%</td>
<td>1.6%</td>
<td>4.6%</td>
</tr>
</tbody>
</table>

### Table 7: Comparing High Participation States with Rest of Country

<table>
<thead>
<tr>
<th></th>
<th>Probit Estimation Results</th>
<th>Dependent Variable: Mobile subscription indicator</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>CA and ME</td>
<td>All Other States</td>
</tr>
<tr>
<td>Lifeline dummy</td>
<td>-0.229***</td>
<td>-0.180***</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Quarter dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>State dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Income dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Age dummies</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household composition</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Household size</td>
<td>Not Sig</td>
<td>Not Sig</td>
</tr>
<tr>
<td>Kids indicators</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>Moved recently?</td>
<td>Not Sig</td>
<td>Not Sig</td>
</tr>
<tr>
<td>Race</td>
<td>Sig**</td>
<td>Sig**</td>
</tr>
<tr>
<td>Hispanic origin</td>
<td>Sig***</td>
<td>Sig***</td>
</tr>
<tr>
<td>N</td>
<td>7,462</td>
<td>71,888</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.047</td>
<td>0.020</td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>-4,813</td>
<td>-46,824</td>
</tr>
<tr>
<td>Test of equality of</td>
<td>Chi-squared = 5.15</td>
<td>p = 0.0232</td>
</tr>
<tr>
<td>Lifeline coefficients</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 8: Before-After Analysis of Lifeline Assistance on Re-Sampled Households

<table>
<thead>
<tr>
<th></th>
<th>All Resampled</th>
<th>No Lifeline</th>
<th>Lifeline Before</th>
<th>Lifeline Before</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lifeline dummy</td>
<td>-0.185***</td>
<td>-0.037*</td>
<td>-0.121***</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(0.022)</td>
<td>(0.021)</td>
<td>(0.030)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>Before-after dummy</td>
<td>0.091***</td>
<td>0.086***</td>
<td>0.090***</td>
<td>0.087***</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>State dummies</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Demographic variables</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>N</td>
<td>19,980</td>
<td>18,753</td>
<td>18,916</td>
<td>17,765</td>
</tr>
<tr>
<td>Pseudo R-squared</td>
<td>0.023</td>
<td>0.162</td>
<td>0.019</td>
<td>0.159</td>
</tr>
<tr>
<td>Log Likelihood</td>
<td>-13,009</td>
<td>-10,540</td>
<td>-12,443</td>
<td>-10,075</td>
</tr>
</tbody>
</table>

Note: The sample includes households sampled twice; coefficients are marginal effects; significance levels: * p<0.10, ** p<0.05, *** p<0.01