Low-Income Demand for Local Telephone Service: Effects of Lifeline and Linkup[*]

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Abstract

This policy study evaluates the effects of the "Lifeline" and "Linkup" subsidy programs on telephone penetration rates of low-income households, and provides a framework for evaluating similar policies for Internet access. These two programs, respectively, subsidize the monthly subscription and initial connection prices for low-income households. Our demand specifications use location-specific subsidized prices and a discrete choice model aggregated across demographic groups. GMM estimators correct for endogeneity of these prices. Our policy simulation suggests that penetration rates would be 4.7 percentage points lower without the policies, that Linkup is more cost-effective than Lifeline, and that automatic enrollment policies are important.

JEL Codes: I39 Welfare and poverty: other, L51 Economics of Regulation, L96 Telecommunications, L98 Government Policy

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Introduction

Universal service for telephony has at least nominally been a public policy concern for quite a while. Universal service policies for ordinary telephone service were expanded significantly in the wake of the 1996 Telecommunications Act, subsequently were expanded to encompass wireless service, and currently are under debate for Internet access service, potentially expanding subsidies by billions of dollars per year (*e.g.* Office of Congresswoman Doris Matsui, 2009). Universal service concerns usually are directed at two different, but somewhat overlapping, groups: rural and low-income households. Our focus is to develop a model for the demand of low-income households and the economic factors affecting their decisions to subscribe to telephone service. Our model evaluates the effectiveness of the post-1996 Lifeline and Linkup subsidy programs at increasing the telephone penetration of low-income households. More generally, our study develops an appropriate methodology and collects appropriate data for evaluating the effectiveness of low-income subsidy programs. Such a framework, and an understanding of its data requirements, is important for evaluating current telephone subsidy programs, and potentially for the gathering debate on Internet access subsidies.

Overall telephone penetration in the U.S. is high – over 95% according to the Federal Communications Commission (FCC) "Penetration Report." (FCC, 2010) Penetration rates are lower for low-income households – less than 93% of households with income less than $20,000 had a working telephone in their households.

The Lifeline program, started in 1985, provides a subsidy that reduces monthly charges for eligible low-income subscribers. The Linkup program reduces the initial connection fee that low-income households pay to establish telephone service. In 1996, the FCC dramatically increased the size of its Lifeline subsidy.[1] The FCC's implementation of the 1996 Act did not change the federal Linkup subsidy.[2]

[1] For detail on the specifics of the Lifeline program, see a prior version of this paper, Ackerberg et al (2009).

[2] The federal Linkup program reduces low-income subscribers' initial connection charge by 50 percent of the customary charge, or $30, whichever is less. Both Lifeline and Linkup are funded by taxes on telecommunications services. To the extent that low-income households are heavy users of the services taxed (e.g. long distance), the overall price reduction is less. We recognize that marginal subscribers are not likely to be heavy users of the taxed services, so low-income telecommunications users presumably experience a price decrease. See Hausman, Tardiff and Belinfante (1993).

Several studies have examined the effect that Lifeline and Linkup programs have on penetration rates.[3] The majority of studies used state-level data that include measures of the size of Lifeline and Linkup programs as explanatory variables in regressions that estimate the overall penetration rate in a state. For example, Garbacz and Thompson (2002, 2003) used state-level data from the 1970, 1980, 1990 and 2000 decennial censuses to estimate penetration rates, while Erikson, Kaserman and Mayo (1998) used state-level data from the current population survey ("CPS"). Both studies found that Lifeline and Linkup programs have a statistically significant but small impact on overall penetration rates. Garbacz and Thompson (2003) found that the demand for local service is highly inelastic (-0.006 to –0.011 in 2000) and that Lifeline and Linkup programs had little effect on penetration, estimating that a 10 percent increase in Lifeline and Linkup expenditures would have added only about 20,000 households to the network in 2000. Erikson, Kaserman and Mayo (1998) found that targeted low-income subsidies affect state-level penetration rates positively, while untargeted subsidies do not have a statistically significant impact on penetration rates.

Studies that rely on statewide data use statewide-average residential prices as an independent variable. Because residential service prices can vary substantially within states, the use of statewide data masks substantial information. For example, in California in 2000, monthly rates for 100 calls a month for Lifeline customers vary from $5.01 to $6.90 and for non-Lifeline customers vary from $11.62 to $15.51.

Crandall and Waverman (2000) used location-specific local service prices and Lifeline rates in 1990 to try to measure whether poor communities in states with Lifeline and Linkup programs have higher penetration rates than poor communities in other states. They found no significant effect of Lifeline programs, which is consistent with their finding of little price elasticity of demand for telephone service overall. Crandall and Waverman found that a higher charge for connecting a new subscriber reduces penetration rates, estimating an elasticity of penetration with respect to the unsubsidized connection charge ranging from -0.025 to –0.030.[4]

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[3] See Riordan (2002) for a more complete background on the economics of universal service.

[4] Surprisingly, a Linkup program was associated with lower penetration rates. Crandall and Waverman explain that the counterintuitive Linkup effect may be due to their use of a dummy variable for Linkup and the fact that only two states did not have a Linkup program in 1990. They also suggest that the Linkup result may be due to a reverse-causation problem. Specifically, states that have high penetration rates may choose not to participate in federal low-income programs.

Earlier studies had estimated higher price elasticities of demand for low-income households than the average income household, thus providing some empirical justification for the first versions of the Lifeline and Linkup programs that were implemented in the 1980's (Perl, 1984; Taylor and Kridel, 1990; Cain and McDonald, 1991).

Our study differs from previous work in at least three important ways. First, using various data sources, we have constructed a dataset that is more extensive than other datasets used to study low-income telephone penetration. We use prices at a disaggregated level, consider penetration specifically for poor households (rather than overall penetration), and use specific Linkup prices rather than a Linkup dummy. By restricting attention to poor households, implicitly we allow price sensitivity for low-income populations to differ from the rest of the population. We can also directly exploit price variation resulting from new Lifeline subsidies introduced in wake of the Act.

Second, our preferred specification controls for the possible endogeneity of Lifeline prices.[5] Lifeline price endogeneity is a concern because states responded to post-1996 changes in federal Lifeline policy differently. Ignoring this endogeneity potentially biases downward the estimated elasticity of demand with respect to Lifeline prices. In addition, we also use the size of the local calling area as an explanatory variable.[6] The inclusion of this value-of-service variable in the demand specification by itself limits price endogeneity because states typically set higher prices in places with larger local calling areas.

Third, our specifications control for automatic enrollment policies. Certain states automatically certify household eligibility in Lifeline/Linkup programs, while other states put the burden on the household to establish eligibility. We interpret automatic enrollment policies as reducing the transaction cost of securing subsidized service.

Our empirical analysis uses connection and monthly subscription prices for households eligible for Lifeline and Linkup programs, and the characteristics of relevant service plans. Data on prices and service characteristics obtained from Bell Operating Company (BOC) tariffs, and Census data on telephone penetration and demographics, are matched to more than 7,000 wire centers.[7] Since the Census data only reports the aggregate low-income penetration rate at each

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[5] Crandall and Waverman (2000) acknowledge the endogeneity issue. They attempted to estimate equations with Lifeline and Linkup as endogenous variables, but were unsuccessful.

[6] Perl (1984) and Taylor and Kridel (1991) use the size of the local calling area as an explanatory variable.

[7] A wire center is a geographic area that includes all customers connected to a particular local switch.

location, our empirical specifications are based on an underlying discrete choice model of household demand for telephone service that is aggregated across demographic groups using information on the demographic composition of each location. The rich dataset and our exclusive focus on poor populations allow us to estimate elasticities with respect to Lifeline and Linkup prices. We find elasticities of demand with respect to median Lifeline price of -0.016 to -0.0219 for low-income households, which, while low, is substantially higher than the Garbacz and Thompson (2003) estimates for the population as a whole, and elasticities of demand with respect to the Linkup price of -0.011 to -0.012. These elasticity estimates are employed to evaluate the effectiveness of Lifeline and Linkup programs at increasing low-income telephone penetration.

Our main conclusion is that Lifeline and Linkup subsidies increased the telephone penetration of poor households in our sample by 4.7 percentage points, with a 95% confidence interval between 2.3 and 7.0 percentage points. We also find that Linkup is more cost effective than Lifeline because it is targeted at low-income households that do not have telephone service, and that the automatic enrollment programs are effective at increasing telephone penetration.

Theory and Empirical Specification of Household Telephone Demand

Telephone service enables a household to place and receive calls. The value of telephone service to a representative household is assumed to be multiplicatively separable in the characteristics of the household and the characteristics of the service. Specifically, we assume a household is willing to pay $\phi(e^{t+V})$ for telephone service, where t describes the household, V describes the service, and $\phi(\bullet)$ is a strictly increasing function. If the price of telephone service is R, then the household elects service if $\phi(e^{t+V}) \geq R$, or equivalently, if $t \geq \ln\phi^{-1}(R) - V \equiv \psi(R) - V$, where $\psi(\bullet)$ is a strictly increasing function.

Consider a population of households described by a cumulative distribution function $F(t)$. The share of households who demand the service (penetration rate) at price R is

$$\bar{S} = 1 - F(\psi(R) - V)$$

Next, partition the population into M demographic groups, indexed $g = 1,...M$. Let X_g denote the population share of group g, and $F_g(t)$ the distribution of t for group g. Then telephone penetration of group g is

$$\bar{S}_g = 1 - F_g(\psi(R) - V)$$

and the penetration rate of the whole population is

$$\bar{S} = \sum_{g=1}^{M} X_g \bar{S}_g$$

Now suppose there is a finite population of households of size N with group shares $(X_1,..X_M)$. Interpret $\bar{S}_g$ as the probability that a randomly chosen member of group g adopts, and $\bar{S}$ as the probability that a randomly chosen member of the whole population adopts. Thus the realized number of households in group g with telephone service is a draw from a binomial distribution $\mathrm{B}(\bar{S}_g, X_g N)$, and the total number of households with service is a draw from the convolution of the distribution functions for the M groups.

Finally, assume the distribution of household types for any group is exponential, with $F_g(t) = 1 - e^{-\lambda(t-\mu_g)}$ for $t \geq \mu_g$, where μ_g is a group-specific location parameter and λ is a common scale parameter. Assume that no group is certain to have 100% penetration, i.e. $\bar{S}_g < 1$ for all g (this is the case if $\mu_g \leq \psi(R) - V$ for all g). It follows that $\bar{S}_g = e^{-\lambda(\psi(R)-V))+\lambda\mu_g}$ and $\bar{S} = e^{-\lambda(\psi(R)-V))}\sum_{g=1}^{M} X_g e^{\lambda\mu_g}$. Alternatively, the expected penetration of the entire population is explained by the logarithmic equation

$$\ln \bar{S} = -\lambda\psi(R) + \lambda V + \ln\left(\sum_{g=1}^{M} e^{\lambda\mu_g} X_g\right) \qquad (1)$$

This equation provides the basis of estimation.

A basic unit of observation is a population of consumers at location l. A vector of group shares $(X_{1l},...X_{Ml})$ describes each population. Both the price (R_l) and other service characteristics (V_l) vary across locations. Our empirical model allows for variation in population and service characteristics across locations. We include in V_l any location specific variables that shift the distribution of tastes, and treat $(\mu_1,...\mu_M)$ as a fixed parameter vector. We make the simplifying assumption that all population groups have the same scale parameter λ. We further simplify to a linear model where $\psi(R) = R$. In the linear model, consumer willingness to pay is $t + V$, and the price elasticity of demand is λR.

The empirical model must deal with the fact that telephone service typically requires a monthly subscription price (P_l), and a one-time connection charge (C_l). There also may be an implicit fixed transaction cost involved in starting (or changing) service. Denote this transaction cost by φ (in monetary units). If the household monthly "discount rate" is α , then $R_l = \frac{1}{\alpha} P_l + C_l + \varphi$ is the present discounted cost of subscribing to phone service.

It also is important to control for differences in the nature of service or distribution of tastes at different locations, such as the number of people within the local calling area. Let

$$\lambda V_l = \Gamma(Y_l) + \xi_l$$

where Y_l is a vector of observed characteristics at location l and unobserved characteristics are summarized by ξ_l . We assume that $\Gamma(\cdot)$ is linear in appropriately defined variables. The unobservable ξ_l can also be interpreted to include a location-specific demand shock for telephone service. As presented, the above is a static model of a one-time adoption decision. However, under some additional assumptions it can also be interpreted as a dynamic model where households can adopt (or drop) service in any time period. Interpret the willingness-to-pay $\phi(e^{t+V})$ as the present discounted value of the utility from having telephone service into the distant future Note that the price of service $R_l = \frac{1}{\alpha} P_l + C_l + \varphi$ is already expressed as a present discounted value. Assume that the household idiosyncratic taste t is constant over time, that V_l is weakly increasing over time, that both P_l and C_l are weakly decreasing over time, and that consumers have rational expectations. Under these assumptions, no household who adopts in any time period would ever want to drop service in a future period. Hence, adoption occurs when $\phi\left(e^{t+V}\right) \geq R$, exactly like in the static version of the model. As V increases and R decreases through time, households with lower and lower t's adopt, essentially moving down the demand curve.

Data

We composed our dataset using various sources: the 2000 decennial Census (United States Department of Commerce, 2000), BOC state telephone tariffs, the FCC, Telcordia (2000) (the Local Exchange Routing Guide, "LERG"), and Claritas (2003). Our unit of observation is the wire center. For each wire center, our data includes telephone penetration rates, demographics, and prices of basic local telephone service including connection charges, Lifeline and Linkup discounts, and other tariff information. In addition, we have variables that proxy the cost of providing local service and several other variables relevant for state regulation. These variables are used as instruments to control for possible price endogeneity. The basic dataset includes 7,938 wire centers located in 43 states and the District of Columbia in the original BOC regions,[8] representing over 80 million residential access lines. The Lifeline program subsidized approximately 5 percent of the residential lines in our data in 2000.[9]

We collected data on local prices and other data from state tariffs. The dependent variable is a wire center's penetration rate for low-income households (*Penetration*); this variable equals the number of low-income households with telephone service divided by the total number of low-income households. For our purposes, a low-income household is one below the poverty line. We dropped some wirecenters for which appropriate price data was unavailable, reducing the remaining number to 7,116. [10]

The independent variables of primary interest are the monthly charge for local service and the connection charge for initiating service. Because low-income households are the focus of this study, we use Lifeline and Linkup rates in our estimation. There is a potential conceptual problem here because takeup of these programs is far less than universal and, in some states, not

[8] Excluded states are Alaska, Hawaii and Connecticut, which were not served by BOCs, Delaware, which is not included in the FCC (2000a) cost model, and Montana, Wyoming and Vermont, which set different prices for households served by each switch depending on the distance from the switch so that it was impossible to accurately determine the prices faced by low-income households. Southern New England Telephone, which serves Connecticut, was purchased by SBC following passage of the Telecom Act of 1996.

[9] See Ackerberg et al. (2009) for details of the estimate.

[10] In metropolitan areas several wire centers serve a single locality, while in rural areas, a single switch may serve multiple localities. We dropped wire centers serving multiple localities with different prices from the sample, reducing the sample to 7,117 wire centers. In 2000, the FCC reported that only 47 percent of Indian tribal households on reservations and other tribal lands have a telephone. It provided additional Lifeline and Linkup monies to tribal areas and changed various other rules to promote subscribership in these areas. FCC (2000c). As a result, we drop one wire center in New Mexico because it contained a large proportion of Native Americans.

all low-income households are eligible for the subsidies.[11] However, as long as these non-takeup households are not marginal consumers, our model is internally consistent. This is possible under the previously discussed dynamic interpretation of our model in which households incur a fixed transaction cost φ to either adopt or switch telephone service, e.g. the transaction cost of contacting the phone company. Specifically, suppose that households who already incurred φ to adopt telephone service prior to the broadening of subsidies in 1996 must incur φ again to access subsidized service post-1996, whereas potential new adopters post-1996 only incur φ once to start subsidized service. In this case, marginal adopters in our 2000 data face the subsidized prices, but some adopters prior to 1996 decline to incur the second switching cost to obtain the subsidized rates, thus generating the low take-up rates. In exploratory regressions, this assumption appears consistent with the data.[12]

Depending on the state, a household is eligible for the program if its income falls below a state-determined cut-off level, or the household participates in one of the means-tested programs the state uses to define eligibility for the Lifeline and Linkup programs.[13] In our data, low-income households in Alabama, Colorado, Maryland and Virginia faced relatively restrictive eligibility criteria. We are concerned that marginal households in these states may not have been eligible for the Lifeline and Linkup subsidies in 2000, and we drop these states from most of our analysis.[14] The remaining data set includes 6,425 wire centers from 39 states.

The empirical analysis uses the variable *Lifeline50*, which is the minimum monthly expenditure of Lifeline customers making 50 local calls.[15] As a robustness check, we also

[11] The FCC (2003) estimates that in 2000 only 37.5 percent of the Lifeline-eligible households participated in the program. Burton et al. (2007) provide evidence that the transactions costs households must undergo to enroll in the program, the level of benefits, and any restrictions that states may impose on Lifeline customers, such as no Caller ID, significantly affect Lifeline participation rates. Our empirical work examines whether transaction costs may be mitigated by state programs that automatically enroll eligible households for Lifeline and Linkup.

[12] Specifically, we considered models in which some proportion of the population face subsidized rates, and some face the unsubsidized rates. We attempted to estimate this proportion, and fairly consistently obtained the result that 100% of the population face the subsidized rates, suggesting that all marginal households faced subsidized rates.

[13] For example, households in California whose income falls below 135% of the poverty level are eligible. Means-tested program include, but are not limited to, Medicaid, Food Stamps Supplement Security Income (SSI), Federal Public Housing Assistance (Section 8), or the Low Income Home Energy Assistance Program.

[14] In Alabama only households receiving Medicaid are eligible for the Lifeline program; in Colorado households must receive SSI benefits; Maryland requires participation in Temporary Aid to Needy Families; and Virginia requires participation in either Food Stamps or Medicaid. All other states either use an income standard under which all low-income households are eligible, or have less stringent program requirements. Inclusion of the four dropped states in the empirical work, as shown in the last column of Table 4, does not affect our general findings.

[15] Customers subscribing to a flat-rate plan pay a monthly charge and are allowed to make an unlimited number of local calls. *Lifeline*X is the minimum basic monthly charge plus usage charges across all available plans assuming

consider *Lifeline100*, which is the minimum monthly expense for making 100 calls. The other price variable of primary interest is *Linkup*, which is equal to the connection charge paid by customers eligible for the Linkup subsidy.

The FCC reports that penetration rates for Blacks and Hispanics generally are lower than average (Belinfante, 2009). To control for possible ethnic differences in the demand for telephone service we consider the variables *White, Black, Native* (Native Americans), *Asian* and *Other* (other non-white populations); these census variables are equal to the percentage of low-income populations belonging to the respective group. These are the $(X_{1l},...X_{Ml})$'s from the prior section.

An important characteristic of telephone service is the number of people within a customer's local calling area (LCA). Customers with flat-rate service can make an unlimited number of calls to customers located within their LCA. When subscribing to a usage-based plan, the rates for local calls are lower than charges for calls outside the customer's LCA. The independent variable *LCA* is equal to the number of households within a customer's local calling area.[16] We expect a positive relationship between *LCA* and *Penetration* holding other factors constant.

We also consider two additional service characteristic variables. Three states had programs that automatically enroll eligible households for Lifeline and Linkup.[17] Such programs lower the transaction cost of obtaining subsidized service. The dummy variable *Autoenroll* is equal to one if the state had such an automatic enrollment program. As a robustness check, we also consider the variable *Autoenroll2,* which includes three additional states that adopted

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the customer completes X three-minute local calls. The monthly charge component equals the non-Lifeline monthly charge, including the federal subscriber line charge (SLC), less the total Lifeline discount; *Lifeline*X includes extended area of service surcharges when such surcharges are non-optional.

[16] In wire centers that serve more than one locality, the household-weighted average LCA is used for the wire center. *LCA* is constructed from tariffs, census data, Telecordia (2000), and Claritas (2003).

[17] The FCC (2003) reports that three states – MA, NY and ND –had automatic enrollment programs. In Massachusetts, households that qualify for the low-income heating assistance program (LIHEAP) were allowed to have the LIHEAP-administrating office contact Verizon and enroll them in the Lifeline program. The New York Department of Family Assistance (NYDFA) automatically enrolled a household in the Lifeline and Linkup program when it enrolled in a NYDFA program. The North Dakota Department of Human Services sent certificates to households that allowed them to enroll in Lifeline and Linkup programs when they were determined eligible for a program that qualified them as eligible for Lifeline and Linkup. Information from Center for Media Education/Center for Policy Alternatives (1999) and local tariffs were used to verify that these programs were in place on January 1, 2000.

programs that reduce the transaction costs associated with enrolling in the Lifeline and Linkup programs.[18]

The second additional service characteristic is the level of intrastate toll rates. Because customers make both local and long-distance calls, the price of long-distance calls affects subscriber decisions, as emphasized by Hausman, Tardiff, and Belinfante (1993). Intrastate access charges are the fees that local exchange carriers charge long-distance companies for non-local intrastate calls. Thus they capture the effect that the prices of calls outside a customer's LCA have on penetration. The variable *Access*, which is equal to the access charge for a four-minute intrastate long-distance call, is expected to have a negative coefficient. We control for for this variable as a robustness check only, dropping it from our main specifications because its coefficient is insignificant and has the wrong sign and does not matter significantly for other estimated parameters, We only consider intrastate access charges because interstate long-distance prices in states do not vary with each state's interstate access charge.

We also control for the median income and population density, thus allowing that telephone service is more (or less) valuable in higher income and less rural/more urban communities. *Median Income* is equal to the median income (in $1000s) of households served by a the wire center, the variable *Rural* is the percent of wire-center households living in rural areas, and the variable *MSA* is the percent of wire-center households living in a metropolitan statistical area.

Endogenous Variables and Instruments

We consider the possible endogeneity of three of the explanatory variables: *Lifeline50*, *Linkup*, and *Autoenroll*. As explained earlier, we are particularly concerned about the possible endogeneity of Lifeline rates because the magnitude of the increases in Lifeline subsidies after 1996 varied significantly across states. Endogeneity could arise if state regulators set these subsidies based on ξ_l, i.e. unobserved (to the econometrician) service characteristics or characteristics of the low-income population.

[18] *Autoenroll2* is set equal to one for California, Maine and Minnesota, in addition to the *Autoenroll* states. California allowed customers to self-certify that they meet the eligibility standards. According to the Maine PUC (2000), the Maine Telephone Education Fund, sent a mailing to people eligible for the Lifeline and Linkup programs. Jackson, Baker and Wilden (2002) report that the Minnesota Department of Human Resources certifies eligibility for the program and informs local telephone companies when their subscribers are found to be eligible for the program.

In considering the possible endogeneity of *Lifeline50*, it is useful to suppose that regulators choose an appropriate subsidy for low-income households, and then subtract this from the normal monthly price:

$$Lifeline50_l = Monthly50_l - Subsidy50_l$$

where *Monthly50* is the normal minimum monthly expenditure for 50 calls, and *Subsidy50* is the discount offered to Lifeline-eligible low-income households. Since the *Subsidy50* component is directed specifically at low-income households, and since these subsidies were significantly increased in 1996, it seems quite plausible that $Subsidy50_l$ is correlated with ξ_l, the unobserved component of demand for low-income households in 2000. For example, correlation might arise from political pressure for higher Lifeline subsidies in areas with lower low-income penetration rates. On the other hand, the *Monthly50* component of *Lifeline50* is a price paid by all subscribing households in an area. Presumably, regulators primarily take *non-low-income* households into account when setting *Monthly50*, since *Subsidy50* can always be adjusted to generate a desired price for low-income households. In addition, *Monthly50* prices tend to change fairly slowly over time, so there may be an important historical component to these prices. Hence we believe a-priori that it is more likely *Subsidy50* is correlated with ξ_l than *Monthly50*. Our empirical analysis, however, considers both possibilities.[19]

Our other two potential endogenous variables are *Linkup* and *Autoenroll.* Our a-priori view is that, even though these variables are also targeted towards low-income households, they are less likely to be endogenous than *Lifeline50*. The Federal-State Joint Board on Universal Service (FCC, 2003) recognizes that implementing automatic enrollment procedures imposes additional administrative burdens and costs, suggesting that this policy decision was determined primarily by infrastructure considerations, i.e. whether state computer systems were up to the task. Regarding the possible endogeneity of *Linkup*, an important issue is the way in which the Federal Government funds low-income subsidy programs, and the resulting incentives for states. As discussed in the introduction, the Lifeline and Linkup programs differ in the extent to which the Federal Government provides matching incentives.

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[19] We emphasize that just because *Monthly50* is set for non-low-income households does not guarantee that it is uncorrelated with ξ_l for low-income households. For example, demand shocks may be correlated across low-income and non-low-income households, and *Monthly50* might be set in response to non-low-income demand shocks. Or, *Monthly50* might be set in response to unobserved product characteristics that affect both non-low-income and low-income demand.

In the Lifeline program the federal subsidy increases with the amount of state subsidy, i.e. the state subsidy is "matched." In contrast, in the Linkup program the federal subsidy is fixed at 50% of the customary rate (up to $30). Thus any state subsidization of the Linkup rate is not matched by the federal government. Presumably in response to these strong economic incentives, 36 (81.8%) of the 44 states in our sample provide additional Lifeline subsidies, while only 12 (27.3%) provide additional Linkup subsidies.[20] Summing up, it appears that *Linkup* is determined for the most part by customary rates and a fixed federal subsidy percentage.[21] Consequently, it seems plausible that *Linkup* is exogenous with respect to unobserved state-level variation in low-income demand conditions. Furthermore, *Autoenroll* appears to be determined primarily by plausibly exogenous technological constraints. Therefore, we are less concerned with the possible endogeneity of *Linkup* and/or *Autoenroll* than with *Lifeline50*. Our empirical work, however, considers possible endogeneity of all three variables.

When allowing for the possible endogeneity of *Lifeline50*, *Linkup*, and *Autoenroll*, we need valid instruments for identification. The instruments must be variables that exogenously shift the relevant endogenous variable, but do not directly shift low-income demand and are uncorrelated with demand residuals.[22] More intuitively, we want "cost-shifters" that affect the subsidized rates (and/or *Autoenroll*) but are unrelated to low-income demand. Our primary instruments are *State Rural, Competition, Elect PUC,* and *Democrat PUC*. *State Rural* is the percent of rural households in the state (whereas *Rural* discussed above pertains the percentage of rural households in the wire center). *State Rural* is interpreted as a proxy for the telephone company's average cost of service in the state because the average cost of service generally decreases with population density.[23] Higher statewide cost is expected to increase prices because

[20] There may be political reasons why states primarily subsidized Lifeline. While Linkup subsidies target a small group of eligible households who have not adopted yet or have just moved, Lifeline subsidies benefit all low-income households. Hence Lifeline subsidies may be politically more feasible.
[21] Like *Monthly50*, the customary connection charge is set for all households, not specifically for low-income households. Also like *Monthly50*, this customary rate typically does not vary much over time.
[22] In particular, we maintain that the instruments are uncorrelated with unobserved service characteristics. While this may be a strong assumption, it is very common in the differentiated products demand literature (e.g. Berry, Levinsohn, and Pakes (1995)).
[23] Like Rosston, Savage and Wimmer (2008), we also considered the BOCs' average forward-looking cost of service constructed from the FCC (2000a) Hybrid Cost Proxy Model (HCPM) as an alternative proxy, but discovered that *State Rural* had more explanatory power. State regulation of rates is generally based on historical rather than forward-looking cost.

state regulators are required to set rates that recover carriers' overall costs of service in the state.[24]

The variable *Competition* (FCC, 1996) measures whether a state had allowed competitive entry in 1995, before passage of the Act, and whether competitors had begun providing local switched services in a state by 1995. Knittel (2004) finds that the introduction of competition before the Act reduced the amount of cross-subsidization present in local telephone markets. Specifically, he shows that residential prices were higher and business prices were lower in states with active competition.

Democrat PUC and *Elect PUC* describe the state's Public Utility Commission (PUC) and come from NARUC (2000). These commissions played a major role in the determination of their state's Lifeline and Linkup subsidies. *Democrat PUC* equals the percentage of a state's PUC commissioners affiliated with the Democratic party, and *Elect PUC* is a dummy variable indicating if state public utility commissioners are elected rather than appointed. Democrats might be more inclined to provide larger subsidies for the poor and elected officials may be more sensitive to the contributions of regulated utilities and set higher residential rates (Rosston, Savage and Wimmer, 2008).

Finally, recalling our decomposition of *Lifeline50, Monthly50* is an additional potential instrument in specifications for which only *Subsidy50* is endogenous. Intuitively, using *Monthly50* as an instrument exploits the part of the variation in *Lifeline50* that is not directed at low-income households as exogenous variation. This should be a particularly strong instrument, since *Monthly50* is mechanically related to *Lifeline50*. In cases where we use *Monthly50* as an additional instrument, we drop the instrument *State Rural* because our arguments above hypothesize that *State Rural* affects prices primarily through the normal rate. Therefore, if we use *Monthly50* as an instrument, *State Rural* is theoretically redundant.

Table 1 provides summary statistics for the full sample of 6,425 wire centers as well as for a restricted sample of 5,944 observations that drops the 481 wire centers with 50 or fewer poor households. We use this restricted sample in our estimation, because, as explained further below, our empirical approach is not strictly valid when there is sampling error, i.e. differences

[24] Recall that we include the percent of households living in rural areas in the wire center as an explanatory variable (*Rural*). Hence, we do allow the level of ruralness in a location to affect telephone demand in that location. The instrument *State Rural* capitalizes on the fact that subsidies are primarily set at the state level. This generates across-state variation in the subsidy conditional on the level of ruralness in a location. We also considered the state poverty rate as an instrument, but found that it did not change our estimates very much.

between the expected penetration rate in a location and the actual penetration rate in a location. These differences will tend to be smaller in locations with larger populations.

Estimation

Following equation (1), our estimation uses the econometric model:

$$\begin{aligned}&\ln Penetration_l = \theta_0 + \theta_1 Lifeline50_l + \theta_2 Linkup_l + \theta_3 Autoeroll_l \\ &+\theta_4 \ln LCA_l + \theta_5 Median\ Income_l + \theta_6 Rural_l + \theta_7 MSA_l + \\ &\ln(White_l + e^{\theta_8} Black_l + e^{\theta_9} Native_l + e^{\theta_{10}} Asian_l + e^{\theta_{11}} Other_l) + \xi_l\end{aligned} \tag{3}$$

This assumes that ψ is linear (i.e. $-\lambda\psi(R) = \theta_1 Lifeline50_l + \theta_2 Linkup_l$) and that $\lambda V = \theta_0 + \theta_3 Autoeroll_l + \theta_4 \ln LCA_l + \theta_5 Median\ Income_l + \theta_6 Rural_l + \theta_7 MSA_l + \xi_l$, where ξ_l is the econometric residual, i.e. unobserved service characteristics or demand shocks. θ_8 through θ_{11} correspond to the $\lambda\mu_g$ for the various demographic groups, and $\lambda\mu_{White}$ has been normalized to 1 since it is not separately identified from the constant term θ_0.

Note that we have replaced the expected penetration rate $\bar{S}$ in equation (1) with the observed penetration rate, *Penetration*. This ignores sampling error, i.e. it ignores the fact that the observed penetration rate will vary around $\bar{S}_l$ because the number of poor households is finite. While one can loosely interpret this sampling error being subsumed into the residual ξ_l, this is not a strictly valid interpretation.[25]

Because of the non-linearity of the model and the possible endogeneity of explanatory variables, we estimate the model using generalized method of moments (GMM). Our basic moment assumption for estimation is:

$$E[\xi_l \otimes Z_l] = 0 \tag{4}$$

i.e. the residuals ξ_l are uncorrelated with instruments Z_l. The composition of Z_l varies across specifications of the model depending on the specific exogeneity assumptions. As discussed

[25] This is one limitation of our estimation, and is the reason why we use the restricted sample that drops wire centers with small poor populations. Another limitation is that the estimating equation is only strictly valid if no demographic group in any location adopts with probability one. This amounts to assuming an upper bound on the support of the distribution of ξ_l. On the other hand, the virtue of this approach is that it enables estimation by simple nonlinear IV/GMM techniques, which require neither a full distributional assumption on ξ_l nor a precise specification of the data generating process for potentially endogenous prices. See Ackerberg et. al. (2009) for a derivation and estimation of a second approach that allows for probability one adoption, but has other limitations that cause us to focus on this approach. The results from the second approach lead to very similar results.

above, *LCA*, *Median Income*, *State Rural*, *MSA* and the demographic variables are always treated as exogenous, so they always enter Z_l. *Lifeline50*, *Linkup*, and *Autoenroll* enter Z_l when they are treated as exogenous. When any of these variables are treated as endogenous, they are removed from Z_l and replaced by the instruments *State Rural*, *Competition*, *PUC Elect*, and *Democrat PUC*. In specifications where only the subsidy component of *Lifeline50* is treated as endogenous (i.e. the *Monthly50* component of *Lifeline50* is assumed exogenous), the instrument *Monthly50* replaces *State Rural*.

Given any arbitrary parameter vector, the implied residuals, $\xi_l(\theta)$, can be computed using (3). At the true parameter vector θ *, the implied $\xi_l(\theta^*)$'s equal the true residuals; at other parameter vectors this is assumed not to be the case. Thus, estimation proceeds by considering:

$$G(\theta) \equiv E\left[\xi_l(\theta) \otimes Z_l\right] \approx \frac{1}{N}\sum_l \xi_l(\theta) \otimes Z_l \equiv \frac{1}{N}\sum_l g_l(\theta) \equiv G_N(\theta)$$

where N is the number of locations. Given the orthogonality assumption on the true residuals given by (4), $G(\theta)$ (and, asymptotically, $G_N(\theta)$) equals 0 when evaluated at θ *; at other parameter vectors, this is assumed not to be the case. Hence, a consistent estimator is obtained by searching for the θ that makes $G_N(\theta)$ "as close as possible" to zero. Formally, this is done by minimizing a quadratic form in $G_N(\theta)$, i.e.

$$G_N(\theta)' A G_N(\theta) \tag{5}$$

where A is a full rank weight matrix that only affects efficiency, not consistency. The weight matrix A that minimizes the variance of the resulting estimate is A=Var$(G_N(\theta))^{-1}$. We use a standard two-step procedure to approximate this optimal weight matrix A.

There are two additional econometric considerations we address in our estimation procedure. The first concerns the fact that our observations represent geographic areas. Thus, one might expect the unobservables ξ_l to be correlated across nearby wire centers, e.g. if some aspects of unobservable service characteristics ξ_l are determined at the state level. While this does not affect the consistency of our GMM estimators, it does impact their standard errors. To address this, we allow for geographic clustering at the state level (i.e. we allow for correlations in the residuals across wire centers in the same state) in computing these standard errors. To allow for such "clustering," we use a robust variance estimator (e.g. Moulton, 1990) to compute standard errors.

The second non-standard issue concerns the fact that wire center penetration data are aggregated. Since the number of households, N_l, varies across wire centers, we expect the aggregation to generate heteroskedastic residuals. While such heteroskedasticity does not affect the consistency of our estimates, it is possible to gain efficiency by addressing the heteroskedasticity appropriately. We do so by introducing weights into the estimation procedure. We first estimate the model ignoring heteroskedasticity, and then linearly regress the squared estimated residuals, $\hat{\xi}_\ell^2$, on functions of N_l to estimate how the variance of the residuals depends on population size. We use these regression results to construct weights w_l equal to the inverse of the square root of the predicted variance for each location. We then re-estimate the model, using weighted residuals $w_l \xi_l$. These weights equalize the variance of the weighted residuals across observations, and therefore are optimal by construction.

Table 2 presents our first set of estimates. The different columns correspond to the different endogeneity specifications discussed earlier. Column 1 is the ALL EXOGENOUS specification. While perhaps unrealistic, this provides a point of comparison for the less restrictive estimators. Columns 2 through 9 allow for increasingly less restrictive exogeneity assumptions, starting with Column 2, in which only the subsidy component (*Subsidy50*) of *Lifeline50* is treated as endogenous, and ending with column 9, where *Lifeline50, Linkup,* and *Auto* are all treated as endogenous.

First, note that the coefficients on the demographic groups, *Rural, ln(LCA), Median Income*, and *MSA* change very little across the various specifications. Given the normalization of the coefficient on *White*, the coefficients on the other demographics measure the strength of demand of these demographics relative to the white population, i.e. $\theta_g = 0$ means that group g has the same demand as white households. Thus significant negative coefficients on *Black*, *Native*, and *Other* indicate that, all else equal, these groups have lower penetration rates than whites. In contrast, the positive coefficients on *Asian* indicate that Asian households have higher penetration rates, although the effect is not statistically significant. As expected, the coefficient on *ln*(*LCA*) is positive and significant across all the specifications. The magnitude of the effect seems reasonable – a doubling of the local calling area increases penetration rates by almost 1%. Also as expected, the coefficient on *Median Income* is positive and statistically significant. The coefficient on *Rural* is negative and generally significant, and the coefficient on *MSA* is significant and positive – suggesting unobserved service characteristics are better (or that telephone demand is stronger) in more urban areas.

We are concerned primarily with the estimated coefficients on *Lifeline50*, *Linkup*, and *Autoenroll*. In addition to reporting the coefficients themselves, we compute some interesting functions of the coefficients. First, we report the price elasticities implied by the coefficients on *Lifeline50* and *Linkup* (*Elasticity-Lifeline* and *Elasticity-Linkup*). These elasticities are evaluated at the sample median.[26] Second, while both price variables are measured in dollars, *Linkup* is a one-time connection fee while *Lifeline50* is a recurring monthly fee. Hence we can use the relationship between the two coefficients to approximate the rate at which these households discount the future. Formally, the monthly discount rate implied by the price coefficients is computed as $Discount = \theta_2 / \theta_1$.

The ALL EXOGENOUS specification in Column 1 establishes a baseline. The price coefficients and elasticities are negative but very small, as expected from previous research on telephone demand. The elasticities with respect to *Lifeline50* and *Linkup* are -0.01017 and -0.00731 respectively, and only the former is statistically significant. The coefficient on *Autoenroll* in Column 1 is a statistically significant 0.022, suggesting that automatic enrollment policy increases low-income penetration rates by 2.2%. Since penetration rates are already quite high (averaging about 92% in our population), an automatic enrollment policy reduces the number of households without service by almost 30%. Given that prices enter the penetration equation linearly, the coefficient on *Autoenroll* divided by θ_2 (the coefficient on Linkup) can be interpreted as the reduction in the fixed transaction cost of initiating Lifeline service resulting from an automatic enrollment policy. The implicit reduction is equal to about $37, suggesting that an automatic enrollment policy has substantial value to consumers. In our preferred specifications (columns 2 and 3) the effect falls to around $26, because θ_2 is higher in these specifications. Finally, the discount rate is over 30% per month, although it is estimated poorly. The high discount rate is not unreasonable for poor households facing credit constraints.[27]

[26] Given our log-linear penetration equation, the reported price elasticity is equal to the estimated coefficient times the median price in the sample.
[27] See, for example, Adams, Einav, and Levin (2009), who study low-income populations in the context of used car loans. If there is heterogeneity in these discount rates across the population, we are most likely identifying the high end of the range, because the "marginal" households determining our estimated coefficients are most likely the poorest of the poor.

Column 2 treats the subsidy portion of *Lifeline50* (i.e. *Subsidy50*) as endogenous, using *Competition, Elect PUC, Democrat PUC*, and *Monthly50* as instruments. As argued in the prior section, our a-priori belief is that *Subsidy50* is the price component that is most likely to be endogenous. A first observation concerns the F-statistic from the corresponding first stage regressions, reported at the bottom of the table.[28] At 12.70, the F-statistic rejects the null hypothesis that the instruments are not predictive of the endogeneous variable, suggesting that the instruments are strong enough for meaningful inference. Moving to the coefficient estimates, the coefficient on *Autoenroll* is similar to that in Column 1, while the price coefficients (and implied elasticities) are considerably larger in absolute value. The *Lifeline50* coefficient/elasticity increases by more than 60%, and the *Linkup* coefficient/elasticity increases by about 55%. In addition, the *Linkup* coefficient becomes significant at the 95% level. The increase in the *Lifeline50* coefficient between Columns 1 and 2 is intuitive. If the *Subsidy50* component of *Lifeline50* is endogenous and correlated with the demand residual, we would expect the correlation to be positive, i.e. we would expect states with lower penetration rates to more heavily subsidize (i.e. set lower prices).[29] This endogeneity would generally cause the *Lifeline50* coefficient in Column 1 to be biased towards zero.[30] The estimated discount rate in Column 2 decreases slightly from Column 1, and becomes significant at the 90% level . Since there are more excluded instruments (4) than endogenous variables (1) in Column 2, the model is overidentified and we can also run a specification test. A J-test of these over-identifying restrictions (test statistic = 1.12) does not reject the validity of the moment conditions. Column 3 relaxes our exogeneity assumptions a bit further, allowing the entirety of *Lifeline50* to be endogenous. Relative to the estimates in Column 2, we drop *Monthly50* as an instrument (since it is now being considered endogenous), and add *State Rural* as an instrument (since it is assumed to be an exogenous determinant of *Monthly50*). Interestingly, the results do not change much from Column 2, although magnitude of the *Lifeline50* coefficient increases by about a third. Again, the instruments appear strong enough (F-stat = 13.62), the specification passes the overidentifying test (J-test statistic = 1.08).

[28] These F-statistics were computed in STATA and are robust to clustering by state.
[29] There are other reasons for a positive correlation, for example, service with "better" unobserved characteristics might be more costly to provide, and hence be priced higher.
[30] As discussed above, prior studies have found low elasticities of demand. While these studies are not directly comparable due to differences in the time period of the data and the techniques, endogeneity may have played a part in the low estimates.

We can use the estimates in Columns 1-3 to formally test whether either the *Subsidy50* or *Monthly50* components of *Lifeline50* is endogenous. The Hausman test statistics[31] for testing Columns 2 and 3, respectively, vs. Column 1, are 16.21 and 21.27. While the first of these Hausman statistics is inconclusive, the second rejects the ALL EXOGENOUS specification at the 95% confidence level. This suggests that some component of *Lifeline50* is in fact correlated with the demand unobservable, and that the estimates in Column 1 are biased. We also perform a Hausman test comparing Column 2 to Column 3 to test the more specific null hypothesis that *Monthly50* is exogenous (recall from the Data section that a-priori, we expect that *Monthly50* is more likely to be exogenous than *Subsidy50*) . We are unable to reject the null, though this might be because the test lacks power.

Columns 4 through 9 further relax our exogeneity assumptions. Columns 4-7 start from the specifications in Columns 2 and 3 and alternatively allow either *Linkup* or *Autoenroll* to be endogenous. Columns 8 and 9 start from the specifications in Columns 2 and 3 and allow both *Linkup* and *Autoenroll* to be endogenous. An unfortunate result in all these specifications is that our excluded instruments are very weak for these additional potentially endogenous variables. The F-stats for *Linkup* and *Autoenroll* (when they are treated as endogenous) are not significant by conventional standards, let alone the standards of the weak instrument literature. The weakness of the instruments manifests itself in the large standard errors of the *Linkup* and *Autoenroll* coefficients in these specifications.

While there is no statistical evidence suggesting that *Linkup* and *Autoenroll* are endogenous, this may be due to the weakness of the instruments and resulting high standard errors.[32] However, several arguments give credence to our choice of Columns 2 and 3 as preferred estimates: 1) our arguments from the prior section that *Lifeline50* (or its component *Subsidy50*) is the most likely of the policy variables to be endogenous; and 2) the fact that the coefficient on *Lifeline50* is relatively stable across specifications.

[31] Since we are in a nonlinear framework, our ALL EXOGENOUS specification does not necessarily yield an efficient estimator. Hence, we cannot use the standard Hausman formula to derive the covariance between each set of estimates (e.g. the correlations between the estimates in column 1 and the estimates in column 2). To derive these covariances, we estimate both specifications simultaneously (using moments from both specifications with two sets of parameters - one entering each set of moments). As a result, the standard GMM variance formula gives us the covariances between the parameters of the two specifications that we need for the Hausman test.

[32] We do not report the results in the paper, but no Hausman test comparing Columns 4-9 to their restricted analogues in Column 2 or 3 rejects the null hypothesis that either *Linkup* or *Autoenroll* is exogenous. Of course, given the weakness of the instruments for *Linkup* and *Autoenroll*, these tests are probably not very powerful.

Table 3.a considers a first set of simple robustness checks on the model. All are perturbations of Column 2 in Table 2, i.e. the model where only *Subsidy50* is considered endogenous.[33] Column 1 uses *Lifeline100* as the monthly price instead of *Lifeline50*. Column 2 uses our alternative definition of automatic enrollment, *Autoenroll2*. Column 3 drops the *Autoenroll* variable altogether, and Column 4 adds the *Access* variable.

The estimates in Table 3.a suggest that our results are quite robust. The Lifeline and Linkup elasticities do not move by more than about 35%, and generally stay significant (the only exception being the *Linkup* coefficient, which becomes insignificant in Columns 2 and 3). Similarly, the *Autoenroll* coefficient is very stable across the specifications, and remains highly significant. Interestingly, the coefficient on *Access* is not close to being significant (and the wrong sign). The failure to find a significant negative effect of intrastate long distance prices on penetration is contrary to Hausman, Tardiff, and Belinfante (1993), but may be explained by the fact that intrastate access prices were lower and had limited variation in our 2000 cross section.[34] The high standard error of our estimate certainly does not rule out a quantitatively significant negative effect of intrastate long distance prices on penetration.

Table 4.a presents five additional robustness checks, all having to do with how we restrict our sample.[35] Unlike other states, California did not require formal verification of eligibility for Lifeline and Linkup programs. As such, California has an extremely high take-up rate.[36] Given the size of the state, one might be concerned that this peculiarity may be driving some of our results. Column 1 drops California from the sample. Columns 2-4 consider alternative ways to address locations with small poor populations. Recall that because our empirical approach to estimation does not explicitly consider sampling error, our results in Tables 2-3 restrict the sample to locations with more than 50 poor households. In Column 2 of Table 4.a, we alternatively include all locations in the sample, and in Column 3, we restrict the sample to locations with more than 100 poor households. Lastly, Column 4 additionally drops locations

[33] Similar robustness checks for the Lifeline50 ENDOGENOUS specification are reported in Table 3.b. The results are qualitatively similar.

[34] Hausman, Tardiff, and Belinfante (1993) used panel data covering 1984-1988. The panel dataset allowed them to exploit variation in interstate as well as intrastate long distance prices. Interstate prices fell substantially during this period due to reduced access prices.

[35] Similar robustness checks for the Lifeline50 Endogenous specification are reported in Table 4.b. Again, results are qualitatively similar.

[36] FCC (2003, Appendix F) estimates that more than 100% of California households eligible for Lifeline received subsidies in 2000.

where the observed adoption rate of poor households is 100%. Even though this last selection criteria is not econometrically valid (since the selection criteria is based on the dependent variable), it still seems like a reasonable check on the robustness of our results to 1) not formally modeling sampling error, and 2) assuming away 100% adoption probabilities. Finally, Column 5 includes the four states we had dropped because of their restrictive eligibility criteria. The estimates in Table 4.a suggest that our results are quite robust. The implied elasticities move very little, and the *Lifeline50, Linkup* and *Autoenroll* coefficients remain significant.

In summary, preferred specifications are Columns 2 and 3 in Table 2. These two specifications control for the possible endogeneity of *Lifeline50*, but treat all other explanatory variables as exogenous. The estimated price elasticities are small but higher than previous studies have found for the entire population, a conclusion that shows controlling for endogeneity matters. The estimated model also shows consumers value larger local calling areas, and that an automatic enrollment policy for Lifeline and Linkup substantially boosts the telephone penetration of low-income households. There are significant demographic differences in the demand for service. These results appear robust to a number of modeling perturbations.

Policy Experiment

Using the estimates from our most preferred specification (Table 2, Column 2, *Subsidy50 ENDOGENOUS*), we evaluate the impact of the Lifeline and Linkup plans on low-income penetration. We use the estimated penetration equation to see what penetration rates for low-income households would have been without Lifeline and/or Linkup programs.[37] The actual penetration rate for low-income households in our sample is 92.1%. Table 5.a presents two different sets of estimates for the effect of Lifeline and Linkup because different states have different automatic enrollment policies. The first column presents the results of simply eliminating Lifeline or Linkup subsidies while allowing *Autoenroll* to have the same positive impact on predicted penetration even though one of the two programs to which the automatic enrollment policies apply is absent. The policy experiment is consistent with the interpretation that automatic enrollment policies reduce the transaction cost of subscribing to subsidized telephone service.

[37] In these policy experiments we assume that normal rates remain unchanged. This is a reasonable approximation since 1) low income households are a small component of overall telephone demand, and 2) normal rates are subject to considerable amounts of regulation.

The second column shows the results of eliminating Lifeline and Linkup when *Autoenroll* equals 0 in all states both before and after the elimination of the low-income support programs. This adjustment results in almost the same estimated impacts from each of the two programs. Finally, the third column shows the total effect of eliminating automatic enrollment, Lifeline, and Linkup policies. The discussion below focuses on the first and third columns; the results from the second column are in parentheses where they differ. The bottom part of Table 5.a shows the very similar effects for the full sample estimates. Table 5.b shows slightly larger effects from the programs when we use *Lifeline50* ENDOGENOUS estimates (column 3 of Table 2.a) instead of *Subsidy50* ENDOGENOUS.

The predicted penetration rates for low-income households with Lifeline and Linkup rates are significantly and substantially higher than the predicted penetration rates without these reduced rates. The estimated difference in the penetration rates of poor households is 4.7 (4.3) percentage points. The incremental effect of Lifeline accounts for more than half of this increase, i.e. if Lifeline did not exist then penetration would be lower by 2.3 percentage points. Removing Linkup would reduce predicted penetration of telephone service for poor households by 2.1 percentage points.[38]

To get an idea of the effectiveness of Lifeline and Linkup relative to the costs of the programs, we estimated crudely the amount of federal and state funding for Lifeline and Linkup in our sample. A description of the methodology is described in Ackerberg et al (2009). We calculate that the annual federal funding for Lifeline and Linkup in our sample was about $183 million in 1999. In addition, we calculate that states spent another $57 million on these two programs. There are about 7.96 million low-income households in our sample. A 4.7 (4.3) percentage point increase in penetration among low-income households, means that these programs encourage about 374,000 (342,000) more low-income households to subscribe to the telephone network. This works out to a cost of about $640 ($700) per poor household per year.[39] Furthermore, there may be additional costs associated with automatic enrollment polices, as well as other implementation costs.

[38] These predictions ignore possibly offsetting factors (Hausman, Tardiff, and Belinfante, 1993). Federal low income subsidy programs are funded by taxes on interstate revenues. To the extent that such extra charges are also borne by low-income households, their bills would decrease somewhat, partially offsetting the increase in hookup and monthly charges.

[39] Our estimate of the cost per household does not account for eligible households whose income exceeds the poverty level. As discussed above, we estimate that over 3 million households with incomes above the poverty level are eligible for the Lifeline and Linkup programs.

Linkup appears to be much more cost effective than Lifeline. Linkup costs less than 8% of the Lifeline program annually, yet has about 90% of Lifeline's incremental effect on predicted penetration. Our estimates suggest that regulators might get the same effect on penetration with substantially less money by increasing the Linkup program and reducing the Lifeline program. The Universal Service Administrative Company (2007) reports that in 2006 the Federal government spent $778 million on Lifeline and only about $33 million on Linkup; so there is room to undertake this policy adjustment. There seem to be at least two reasons why Linkup is more cost effective than Lifeline. First, recall that our estimates suggest that low-income households have very high discount rates. Hence, it will be more cost effective for a policymaker (with a more standard discount rate) to subsidize the one-time Linkup price rather than the recurrent monthly Lifeline price. Another reason Linkup is more cost effective is that by definition it is targeted at poor households who do not have telephone service. We estimate that less than 9% of Lifeline expenses in our sample go to households who would not otherwise subscribe to service.[40] These results suggest that in thinking about how to most cost effectively subsidize broadband adoption, it might be optimal primarily to subsidize up-front costs.

Conclusions

Using data from 5,944 wire centers, we conclude that low-income subsidy programs have increased low-income telephone penetration by 4.7 percentage points. The conclusion is based on estimated price elasticities of demand with respect to subscription and connection charges for poor households of -0.016 and -0.011 respectively. These estimated elasticities are low but nevertheless somewhat higher than previous estimates for all households. The higher estimates are due substantially to bias corrections that account for the possible endogeneity of Lifeline rates in different locations due to different implementations by state regulators. Even with a relatively low price elasticity of demand, the magnitude of Lifeline and Linkup programs are sufficient to reduce substantially the effective prices faced by low-income households so that telephone penetration increases significantly as result of these programs.

[40] We calculate this by adding up the additional households that subscribe due to Lifeline in each wire center times the total lifeline subsidy in that wire center and then dividing the total dollars nationwide by the total cost of the Lifeline program. Calculating the corresponding percentage for Linkup is more difficult because the Linkup subsidy is non-recurring unless a household stops and then renews service. A poor household that moves to a new location might take advantage of Linkup and/or Lifeline when renewing telephone service, and some percentage of these households would subscribe to telephone service without the Linkup and Lifeline subsidies. We are not able to estimate this percentage.

Because of low-income households' high discount rates, the Linkup program has a much higher effect on penetration per dollar spent than the Lifeline program. One possible explanation for this is that low-income households may be credit constrained and even with the typical 50% discount initial hookup charges could be daunting. Furthermore, Linkup subsidies are targeted at households who do not have telephone service.

The bottom line is that Lifeline and Linkup programs in 2000 connected to the telephone network an additional 374,000 of poor households in our sample at an expense of $640 each.

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Table 1

	Full Sample (6425 obs.)					Drop 50 Sample (5944 obs.)*				
	Mean	Median	St.D.	Min	Max	Mean	Median	St.D.	Min	Max
Poor households	1193	419	2026	3	25740	1287	488	2079	50	25740
Penetration	.92	.94	.06	.59	1	.92	.93	.06	.59	1
Lifeline50	5.28	5.50	2.43	0	14.75	5.25	5.34	2.45	0	14.75
Lifeline100	7.35	7.35	3.21	.55	15.32	7.31	7.35	3.23	.55	15.32
Linkup	12.68	12.50	7.47	0	22.95	12.74	12.50	7.49	0	22.95
Hookup	37.00	37.07	11.29	12	65.00	37.04	39.00	11.22	12	65.00
Subsidy Linkup	24.32	21.00	12.21	6	55.00	24.30	21.00	12.13	6	55.00
Autoenroll	.12	.00	.32	0	1	.11	.00	.32	0	1
Autoenroll2	.23	.00	.42	0	1	.23	.00	.42	0	1
Access Charge	.15	.14	.10	.03	.47	.15	.14	.10	.03	.47
White	.73	.81	.25	0	1	.72	.79	.25	0	1
Black	.15	.03	.23	0	.98	.16	.04	.23	0	.98
Native	.02	.00	.05	0	.99	.02	.00	.05	0	.99
Asian	.02	.00	.05	0	.88	.02	.00	.05	0	.73
Other	.09	.04	.11	0	.81	.09	.05	.11	0	.75
LCA	211671	63127	407046	203	3067673	222954	69133	419771	259	3067673
Median Income	43176	38969	16992	12869	175762	42795	38465	16882	12869	160223
Rural	.39	.21	.41	0	1	.35	.17	.39	0	1
MSA	.63	1.00	.48	0	1	.63	1.00	.48	0	1
State Rural	.25	.23	.14	0	.60	.25	.23	.14	0	.60
Competition	.17	.00	.38	0	1	.17	.00	.37	0	1
Elect PUC	.16	.00	.37	0	1	.17	.00	.38	0	1
Democrat PUC	32.85	33.33	26.09	0	100	32.86	33.33	26.33	0	100
Monthly50	13.59	13.58	2.58	8.55	21.75	13.57	13.58	2.60	8.55	21.75
Monthly100	15.79	15.76	2.81	10.25	23.95	15.76	15.76	2.81	10.25	23.95
Subsidy50	8.31	8.25	2.14	5.25	13.65	8.32	8.25	2.13	5.25	13.65
Subsidy100	8.44	8.55	2.32	3.55	15.70	8.45	8.55	2.32	3.55	15.70

*The Drop 50 sample excludes 481 wire centers which have 50 or fewer poor households.

Table 2

	ALL EXOGENOUS	*Subsidy50* ENDOGENOUS	*Lifeline50* ENDOGENOUS	*Subsidy50, Linkup* ENDOGENOUS	*Subsidy50, Autoenroll* ENDOGENOUS
	(1)	(2)	(3)	(4)	(5)
Estimated Coefficients					
Lifeline50	-0.0019 *	-0.00308 ***	-0.0041 ***	-0.00412 *	-0.00267 *
	(0.00113)	(0.00117)	(0.00145)	(0.00212)	(0.00152)
Linkup	-0.00058	-0.0009 **	-0.00095 **	0.00112	-0.00076
	(0.00056)	(0.00040)	(0.00037)	(0.00234)	(0.00053)
Autoenroll	0.02162 ***	0.02372 ***	0.02414 ***	0.0243 ***	0.01382
	(0.00432)	(0.00362)	(0.00341)	(0.00602)	(0.02302)
Black	-0.08911 ***	-0.08436 ***	-0.08907 ***	-0.09779 ***	-0.08646 ***
	(0.01155)	(0.00890)	(0.00941)	(0.01816)	(0.00978)
Native	-0.1602 ***	-0.15101 ***	-0.1467 ***	-0.17822 **	-0.15314 ***
	(0.05515)	(0.04994)	(0.05039)	(0.06967)	(0.05181)
Asian	0.01979	0.00713	0.00576	0.01872	0.00987
	(0.02105)	(0.02018)	(0.02103)	(0.02956)	(0.02281)
Other	-0.09266 ***	-0.09259 ***	-0.10201 ***	-0.08672 **	-0.09201 ***
	(0.03499)	(0.03002)	(0.03124)	(0.03687)	(0.03100)
ln(LCA)	0.00942 ***	0.00967 ***	0.00961 ***	0.00919 ***	0.0097 ***
	(0.00132)	(0.00135)	(0.00137)	(0.00169)	(0.00131)
Median Income	0.00096 ***	0.00094 ***	0.0009 ***	0.00088 ***	0.00097 ***
	(0.00014)	(0.00014)	(0.00014)	(0.00018)	(0.00017)
Rural	-0.02623 ***	-0.02602 ***	-0.02491 ***	-0.02811 ***	-0.02522 ***
	(0.00540)	(0.00465)	(0.00470)	(0.00676)	(0.00496)
MSA	0.01112 **	0.0112 **	0.01044 **	0.01283 ***	0.01194 ***
	(0.00440)	(0.00448)	(0.00449)	(0.00405)	(0.00454)
constant	-0.18937 ***	-0.18083 ***	-0.17053 ***	-0.19367 ***	-0.18631 ***
	(0.02137)	(0.02119)	(0.02161)	(0.03231)	(0.02543)
Functions of Estimated Coefficients					
Elasticity-Lifeline	-0.01017 *	-0.01644 ***	-0.02189 ***	-0.02202 *	-0.01425 *
	(0.00604)	(0.00623)	(0.00773)	(0.01130)	(0.00812)
Elasticity-Linkup	-0.00731	-0.01125 **	-0.01193 **	0.01394	-0.00949
	(0.00697)	(0.00498)	(0.00468)	(0.02929)	(0.00660)
Discount	0.3072	0.2924 *	0.2328 *	-0.27039	0.28456
	(0.34520)	(0.17508)	(0.12867)	(0.55950)	(0.19870)
Diagnostic Statistics					
R2	0.46	0.45	0.45	0.42	0.45
J-statistic (d.f.)		1.12 (3)	1.08 (3)	1.59 (2)	0.89 (2)
F-stat Lifeline50 (d.f.)		12.70 (4) ***	13.62 (4) ***	13.03 (4) ***	12.51 (4) ***
F-stat Linkup (d.f.)				0.40 (4)	
F-stat Auto (d.f.)					1.48 (4)
Hausman (Each) vs. (1)		16.21 (12)	21.27 (12) **	9.89 (12)	25.06 (12) **
Hausman (Each) vs. (2)			5.60 (12)	6.36 (12)	12.96 (12)
N	5,944	5,944	5,944	5,944	5,944

Table 2 (cont.)

	Lifeline50, Linkup ENDOGENOUS (6)		*Lifeline50, Autoenroll* ENDOGENOUS (7)		*Subsidy50 Linkup,Autoenroll* ENDOGENOUS (8)		*ALL* ENDOGENOUS (9)	
Estimated Coefficients								
Lifeline50	-0.00516	**	-0.00409	***	-0.00294		-0.00414	**
	(0.00246)		(0.00150)		(0.00215)		(0.00181)	
Linkup	0.00128		-0.00071		-0.00017		-0.00054	
	(0.00246)		(0.00051)		(0.00267)		(0.00291)	
Autoenroll	0.02433	***	0.00342		0.01418		0.00426	
	(0.00742)		(0.03220)		(0.02436)		(0.03470)	
Black	-0.10375	***	-0.09689	***	-0.09029	***	-0.09767	***
	(0.01976)		(0.01383)		(0.01811)		(0.01882)	
Native	-0.17855	**	-0.15498	***	-0.16094	**	-0.15711	**
	(0.07259)		(0.05323)		(0.06388)		(0.06398)	
Asian	0.02007		0.01186		0.01287		0.01307	
	(0.03133)		(0.02587)		(0.02877)		(0.03259)	
Other	-0.09822	**	-0.1079	***	-0.08947	***	-0.10758	***
	(0.03832)		(0.03220)		(0.03236)		(0.03297)	
ln(LCA)	0.00905	***	0.00955	***	0.00951	***	0.00949	***
	(0.00182)		(0.00136)		(0.00153)		(0.00165)	
Median Income	0.00086	***	0.00094	***	0.00095	***	0.00094	***
	(0.00018)		(0.00017)		(0.00018)		(0.00019)	
Rural	-0.02761	***	-0.02297	***	-0.02581	***	-0.02331	***
	(0.00697)		(0.00534)		(0.00679)		(0.00793)	
MSA	0.01184	***	0.01127	**	0.01278	***	0.01131	**
	(0.00417)		(0.00442)		(0.00425)		(0.00440)	
constant	-0.18469	***	-0.17232	***	-0.1899	***	-0.17339	***
	(0.03092)		(0.02061)		(0.02705)		(0.02686)	
Functions of Estimated Coefficients								
Elasticity-Lifeline	-0.02757	**	-0.02183	***	-0.01571		-0.02212	**
	(0.01312)		(0.00803)		(0.01147)		(0.00966)	
Elasticity-Linkup	0.01606		-0.00889		-0.00208		-0.00674	
	(0.03073)		(0.00634)		(0.03343)		(0.03643)	
Discount	-0.24878		0.17406		0.0567		0.13014	
	(0.45637)		(0.14836)		(0.93447)		(0.73610)	
Diagnostic Statistics								
R2	0.41		0.44		0.45		0.44	
J-statistic (d.f.)	1.11 (2)		0.44 (2)		1.02 (1)		0.45 (1)	
F-stat Lifeline50 (d.f.)	11.42 (4)	***	11.54 (4)	***	12.17 (4)	***	10.23 (4)	***
F-stat Linkup (d.f.)	0.57 (4)				0.41 (4)		0.53 (4)	
F-stat Auto (d.f.)			1.11 (4)		1.47 (4)		1.12 (4)	
Hausman (Each) vs. (1)	9.86 (12)		18.16 (12)		19.33 (12)	*	22.60 (12)	**
Hausman (Each) vs. (2)	10.01 (12)		14.95 (12)		6.98 (12)		14.60 (12)	
N	5,944		5,944		5,944		5,944	

Table 3.a - Robustness to Price Specification Changes (*Subsidy50* ENDOGENOUS)

	Lifeline100		*Autoenroll2*		*NO Autoenroll*		*Access*	
	(1)		(2)		(3)		(4)	
Estimated Coefficients								
Lifeline	-0.00203	**	-0.00196	**	-0.00222	*	-0.00297	**
	(0.00103)		(0.00098)		(0.00122)		(0.00117)	
Linkup	-0.00098	**	-0.00066		-0.00063		-0.00086	**
	(0.00045)		(0.00047)		(0.00047)		(0.00041)	
Autoenroll	0.02815	***	0.02709	***			0.02311	***
	(0.00443)		(0.00449)				(0.00390)	
Access							0.02422	
							(0.04953)	
Black	-0.0798	***	-0.08295	***	-0.08867	***	-0.08487	***
	(0.00813)		(0.00939)		(0.01005)		(0.00888)	
Native	-0.15154	***	-0.16328	***	-0.15537	***	-0.15514	***
	(0.05206)		(0.04738)		(0.05167)		(0.04932)	
Asian	0.0148		-0.02252		0.01655		0.01472	
	(0.01928)		(0.01932)		(0.02203)		(0.02490)	
Other	-0.09372	***	-0.1127	***	-0.09088	***	-0.09573	***
	(0.03421)		(0.02219)		(0.03129)		(0.03443)	
ln(LCA)	0.00956	***	0.00966	***	0.00991	***	0.00966	***
	(0.00129)		(0.00133)		(0.00128)		(0.00133)	
Median Income	0.00098	***	0.0009	***	0.00101	***	0.00096	***
	(0.00014)		(0.00014)		(0.00014)		(0.00014)	
Rural	-0.02643	***	-0.02769	***	-0.02475	***	-0.02571	***
	(0.00481)		(0.00439)		(0.00536)		(0.00460)	
MSA	0.01275	***	0.01132	***	0.01193	***	0.01051	**
	(0.00441)		(0.00413)		(0.00443)		(0.00422)	
constant	-0.18468	***	-0.18864	***	-0.19418	***	-0.18566	***
	(0.02200)		(0.02066)		(0.02066)		(0.02360)	
Functions of Estimated Coefficients								
Elasticity-Lifeline	-0.01491	**	-0.01049	**	-0.01185	*	-0.01585	**
	(0.00757)		(0.00522)		(0.00650)		-0.00622	
Elasticity-Linkup	-0.01231	**	-0.00826		-0.00782		-0.01069	**
	(0.00557)		(0.00583)		(0.00587)		-0.00509	
Discount	0.48545	*	0.33637		0.2819		0.28824	*
	(0.29318)		(0.25609)		(0.23878)		-0.17206	
Diagnostic Statistics								
R2	0.45		0.47		0.44		0.46	
J-statistic (d.f.)	2.27 (3)		1.11 (3)		1.02 (3)		1.10 (3)	
F-stat Lifeline(d.f.)	13.71 (4)	***	12.54 (4)	***	12.51 (4)	***	12.68 (4)	***
Hausman (Each) vs. (1)	19.85 (12)	*	9.92 (12)		27.55 (11)	***	21.71 (13)	*
N	5918		5944		5944		5944	

* 90% confidence ** 95% confidence *** 99% confidence

Table 3.b - Robustness to Price Specification Changes (*Lifeline* ENDOGENOUS)

	Lifeline100	*Autoenroll2*	*NO Autoenroll*	*Access*
	(1)	(2)	(3)	(4)
Estimated Coefficients				
Lifeline	-0.00377 ***	-0.00304 **	-0.00404 ***	-0.00412 ***
	(0.00139)	(0.00134)	(0.00152)	(0.00145)
Linkup	-0.0011 ***	-0.00068	-0.00068 *	-0.00089 **
	(0.00039)	(0.00044)	(0.00041)	(0.00038)
Autoenroll	0.03267 ***	0.02811 ***		0.02375 ***
	(0.00449)	(0.00448)		(0.00366)
Access				0.03061
				(0.04940)
Black	-0.0853 ***	-0.08722 ***	-0.0976 ***	-0.08997 ***
	(0.00791)	(0.01027)	(0.01141)	(0.00932)
Native	-0.15814 ***	-0.16223 ***	-0.15678 ***	-0.14857 ***
	(0.05173)	(0.04703)	(0.05163)	(0.04975)
Asian	0.00952	-0.03194 *	0.01297	0.01493
	(0.02068)	(0.01884)	(0.02376)	(0.02514)
Other	-0.11024 ***	-0.11764 ***	-0.10816 ***	-0.10612 ***
	(0.03589)	(0.02133)	(0.03165)	(0.03638)
ln(LCA)	0.00965 ***	0.00948 ***	0.00956 ***	0.00948 ***
	(0.00129)	(0.00132)	(0.00137)	(0.00134)
Median Income	0.00091 ***	0.00084 ***	0.00095 ***	0.00093 ***
	(0.00014)	(0.00014)	(0.00014)	(0.00015)
Rural	-0.02531 ***	-0.02661 ***	-0.02287 ***	-0.02507 ***
	(0.00497)	(0.00438)	(0.00537)	(0.00463)
MSA	0.01269 ***	0.01158 ***	0.01131 **	0.01035 **
	(0.00437)	(0.00417)	(0.00441)	(0.00426)
constant	-0.16637 ***	-0.17757 ***	-0.17312 ***	-0.17503 ***
	(0.02196)	(0.02076)	(0.02037)	(0.02410)
Functions of Estimated Coefficients				
Elasticity-Lifeline	-0.02768 ***	-0.01624 **	-0.02156 ***	-0.02201 ***
	(0.01020)	(0.00713)	(0.00814)	(0.00773)
Elasticity-Linkup	-0.01371 ***	-0.00848	-0.00854 *	-0.01108 **
	(0.00488)	(0.00546)	(0.00510)	(0.00473)
Discount	0.29120 **	0.22314	0.16928	0.21495 *
	(0.14328)	(0.16619)	(0.12969)	(0.11737)
Diagnostic Statistics				
R2	0.44	0.47	0.44	0.45
J-statistic (d.f.)	0.86 (3)	1.01 (3)	0.45 (3)	1.18 (3)
F-stat Lifeline(d.f.)	12.71 (4) ***	12.46 (4) ***	11.54 (4) ***	13.86 (4) ***
Hausman (Each) vs. (1)	14.81 (12)	18.61 (12) *	26.51 (11) ***	25.83 (13) **
N	5918	5944	5944	5944

* 90% confidence ** 95% confidence *** 99% confidence

Table 4.a - Robustness to Sample Changes (*Subsidy50* Endogenous)

	DROP CALIFORNIA (1)	DROP N <=100 (2)	ALL LOCATION (3)	DROP 100% LOCATION (4)	ALL STATES (5)
Estimated Coefficients					
Lifeline50	-0.00237 **	-0.00276 **	-0.0032 **	-0.0029 ***	-0.00303 ***
	(0.00115)	(0.00107)	(0.00127)	(0.00110)	(0.00113)
Linkup	-0.0008 *	-0.00092 **	-0.00083 **	-0.00092 **	-0.00068 *
	(0.00042)	(0.00039)	(0.00041)	(0.00039)	(0.00039)
Autoenroll	0.02376 ***	0.02024 ***	0.02497 ***	0.02119 ***	0.02476 ***
	(0.00348)	(0.00415)	(0.00389)	(0.00319)	(0.00311)
Black	-0.08423 ***	-0.08028 ***	-0.09004 ***	-0.07845 ***	0.92080 ***
	(0.00941)	(0.00859)	(0.00932)	(0.00871)	(0.00809)
Native	-0.15321 ***	-0.13749 **	-0.16031 ***	-0.14014 ***	0.85871 ***
	(0.04895)	(0.05510)	(0.04520)	(0.04966)	(0.04377)
Asian	0.02252	0.00632	0.01108	0.01928	1.00963 ***
	(0.03086)	(0.01867)	(0.02066)	(0.02252)	(0.01790)
Other	-0.13529 ***	-0.09473 ***	-0.09416 ***	-0.08926 ***	0.91778 ***
	(0.02021)	(0.03046)	(0.03113)	(0.02867)	(0.02608)
ln(LCA)	0.01015 ***	0.0097 ***	0.00957 ***	0.0097 ***	0.00895 ***
	(0.00135)	(0.00138)	(0.00130)	(0.00131)	(0.00127)
Median Income	0.00099 ***	0.00097 ***	0.00092 ***	0.00093 ***	0.00091 ***
	(0.00016)	(0.00014)	(0.00015)	(0.00015)	(0.00013)
Rural	-0.02683 ***	-0.03464 ***	-0.0196 ***	-0.02873 ***	-0.02786 ***
	(0.00465)	(0.00514)	(0.00396)	(0.00460)	(0.00428)
MSA	0.00844 **	0.00998 **	0.01134 **	0.00955 **	0.01142 ***
	(0.00427)	(0.00421)	(0.00479)	(0.00434)	(0.00409)
constant	-0.18954 ***	-0.18199 ***	-0.17966 ***	-0.18392 ***	-0.17549 ***
	(0.02090)	(0.02180)	(0.02001)	(0.02141)	(0.02060)
Functions of Estimated Coefficients					
Elasticity-Lifeline	-0.01301 **	-0.01455 **	-0.01761 **	-0.01596 ***	-0.01564 ***
	(0.00632)	(0.00566)	(0.00698)	(0.00607)	(0.00583)
Elasticity-Linkup	-0.01125 *	-0.01146 **	-0.01039 **	-0.01239 **	-0.00848 *
	(0.00598)	(0.00485)	(0.00513)	(0.00525)	(0.00482)
Discount	0.33677	0.3328 *	0.25973	0.3164 *	0.22427
	(0.23611)	(0.19015)	(0.17275)	(0.18073)	(0.15809)
Diagnostic Statistics					
R2	0.46	0.48	0.43	0.43	0.45
J-statistic (d.f.)	1.49 (3)	1.17 (3)	1.23 (3)	0.93 (3)	1.36 (3)
F-stat Lifeline(d.f.)	10.80 (4) ***	13.12 (4) ***	12.64 (4) ***	12.12 (4) ***	14.16 (4) ***
Hausman (Each) vs. (1)	11.23 (12)	12.78 (12)	5.81 (12)	17.15 (12)	20.05 (12) *
N	5486	5295	6425	5426	6596

Table 4.b - Robustness to Sample Changes (*Lifeline50* ENDOGENOUS)

	DROP CALIFORNIA (1)		DROP N <=100 (2)		ALL LOCATION (3)		DROP 100% LOCATION (4)		ALL STATES (5)	
Estimated Coefficients										
Lifeline50	-0.00346	**	-0.00388	***	-0.00410	***	-0.00396	***	-0.00383	***
	(0.00150)		(0.00134)		(0.00151)		(0.00142)		(0.00137)	
Linkup	-0.00088	**	-0.00097	***	-0.00088	**	-0.00097	***	-0.00070	*
	(0.00039)		(0.00036)		(0.00039)		(0.00036)		(0.00038)	
Autoenroll	0.02404	***	0.02104	***	0.02508	***	0.02197	***	0.02520	***
	(0.00333)		(0.00373)		(0.00366)		(0.00317)		(0.00292)	
Black	-0.08856	***	-0.08552	***	-0.09394	***	-0.08294	***	-0.08643	***
	(0.01015)		(0.00904)		(0.00973)		(0.00922)		(0.00917)	
Native	-0.14675	***	-0.13411	**	-0.15803	***	-0.13451	***	-0.14934	***
	(0.04983)		(0.05534)		(0.04537)		(0.05012)		(0.05120)	
Asian	0.02208		0.00528		0.00864		0.01839		0.00972	
	(0.03038)		(0.01955)		(0.02124)		(0.02362)		(0.01830)	
Other	-0.14443	***	-0.10482	***	-0.10220	***	-0.09783	***	-0.09462	***
	(0.02098)		(0.03148)		(0.03208)		(0.02997)		(0.02991)	
ln(LCA)	0.01002	***	0.00958	***	0.00958	***	0.00970	***	0.00880	***
	(0.00136)		(0.00140)		(0.00131)		(0.00132)		(0.00128)	
Median Income	0.00095	***	0.00094	***	0.00089	***	0.00090	***	0.00087	***
	(0.00016)		(0.00014)		(0.00015)		(0.00015)		(0.00013)	
Rural	-0.02539	***	-0.03285	***	-0.01886	***	-0.02762	***	-0.02724	***
	(0.00469)		(0.00519)		(0.00398)		(0.00459)		(0.00433)	
MSA	0.00802	*	0.00918	**	0.01062	**	0.00848	*	0.01089	***
	(0.00431)		(0.00426)		(0.00469)		(0.00438)		(0.00406)	
constant	-0.17763	***	-0.17049	***	-0.17104	***	-0.17436	***	-0.16623	***
	(0.02144)		(0.02213)		(0.02061)		(0.02164)		-0.0204	
Functions of Estimated Coefficients										
Elasticity-Lifeline	-0.01902	**	-0.02050	***	-0.02256	***	-0.02180	***	-0.01980	***
	(0.00824)		(0.00708)		(0.00832)		(0.00783)		(0.00707)	
Elasticity-Linkup	-0.01247	**	-0.01213	***	-0.01096	**	-0.01307	***	-0.00872	*
	(0.00553)		(0.00448)		(0.00493)		(0.00486)		(0.00472)	
Discount	0.25526		0.25006	*	0.21369		0.24432	*	0.18224	
	(0.15934)		(0.13050)		(0.13140)		(0.13124)		(0.12408)	
Diagnostic Statistics										
R2	0.46		0.48		0.45		0.43		0.45	
J-statistic (d.f.)	1.22 (3)		1.28 (3)		1.01 (3)		1.11 (3)		1.26 (3)	
F-stat Lifeline(d.f.)	10.28 (4)	***	13.67 (4)	***	14.16 (4)	***	12.24 (4)	***	16.80 (4)	***
Hausman (Each) vs. (1)	16.69 (12)		20.10 (12)	*	6.91 (12)		18.74 (12)	*	22.86 (12)	**
N	5486		5295		6425		5426		6596	

Table 5a - Estimated Impact of Lifeline and Linkup on Low-Income Penetration

DROP 50 SAMPLE (*Subsidy50* ENDOGENOUS)

Baseline autoenrollment policy	Actual	Zero in all states	Actual
Autoenrollment after policy change	Actual	Zero in all states	Zero in all states
Policy change	*Resulting decrease in penetration*		
Eliminate Lifeline and Linkup		4.3%	4.7%
		[2.0%, 6.7%]	[2.3%, 7.0%]
Eliminate Lifeline	2.3%	2.3%	
	[0.5%, 4.1%]	[0.5%, 4.0%]	
Eliminate Linkup	2.1%	2.1%	
	[0.3%, 3.9%]	[0.3%, 3.8%]	
Actual penetration in sample = 92.1%	[95% Confidence Interval]		

FULL SAMPLE (*Subsidy50* ENDOGENOUS)

Policy change	*Resulting decrease in penetration*		
Eliminate Lifeline and Linkup		4.3%	4.6%
		[1.8%, 6.6%]	[2.2%, 7.0%]
Eliminate Lifeline	2.4%	2.4%	
	[0.5%, 4.2%]	[0.5%, 4.2%]	
Eliminate Linkup	2.0%	1.9%	
	[0.1%, 3.8%]	[0.1%, 3.8%]	
Actual penetration in sample = 92.2%	[95% Confidence Interval]		

Table 5b - Estimated Impact of Lifeline and Linkup on Low-Income Penetration

DROP 50 SAMPLE (*Lifeline50* ENDOGENOUS)

Baseline autoenrollment policy	Actual	Zero in all states	Actual
Autoenrollment after policy change	Actual	Zero in all states	Zero in all states
Policy change	*Resulting decrease in penetration*		
Eliminate Lifeline and Linkup		5.2%	5.5%
		[3.0%, 7.3%]	[3.3%, 7.7%]
Eliminate Lifeline	3.1%	3.1%	
	[1.3%, 4.8%]	[1.3%, 4.8%]	
Eliminate Linkup	2.2%	2.2%	
	[0.6%, 3.9%]	[0.6%, 3.9%]	
Actual penetration in sample = 92.2%	[95% Confidence Interval]		

FULL SAMPLE (*Lifeline50* ENDOGENOUS)

Policy change	*Resulting decrease in penetration*		
Eliminate Lifeline and Linkup		5.0%	5.4%
		[2.8%, 7.3%]	[3.1%, 7.6%]
Eliminate Lifeline	3.1%	3.1%	
	[1.2%, 4.9%]	[1.2%, 4.9%]	
Eliminate Linkup	2.1%	2.0%	
	[0.3%, 3.8%]	[0.3%, 3.8%]	
Actual penetration in sample = 92.2%	[95% Confidence Interval]		