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Effects of Lifeline and Linkup

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Abstract

A comprehensive data set on local telephone service prices is used to evaluate the effect of Lifeline and Linkup programs on the telephone penetration rates of low-income households in the United States. Lifeline and Linkup programs respectively subsidize the monthly subscription and initial installation charges of eligible low-income households. This is the first study to use specific rates for telephone service faced by low-income households to explain the telephone penetration rates of low income populations at different locations. Telephone penetration rates are explained by an estimated nonlinear function of local service characteristics (including subsidized prices) and the demographic composition of low-income populations. This empirical specification is based on an underlying discrete choice model of household demand for telephone service and an exact aggregation across demographic groups. A generalized method of moments estimator corrects for endogeneity and clustered heteroskedastic residuals. The resulting estimated median price elasticity of demand for telephone service is -0.027 for the monthly charge and -0.008 for the connection charge, and a policy simulation predicts that low-income telephone penetration rates would be 6.24% lower without Lifeline and Linkup. The analysis also suggests that Linkup is more cost-effective than Lifeline, and that low-income penetration would increase significantly if all states were to automatically enroll eligible households in Lifeline and Linkup programs.

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Introduction

"Universal service" has at least nominally been a concern of telecommunications regulators for quite a while. Usually this concern is directed at two different (but somewhat overlapping) groups: rural and low-income households. Our focus is on low-income households and the economic factors affecting their decisions to subscribe to telephone service. Our goal is to evaluate the effectiveness of Lifeline and Linkup subsidy programs that have been adopted to increase the telephone penetration of low-income households. These issues are important for current telephone subsidy programs, and perhaps is a starting point for the gathering debate on subsidy programs for Internet access.

Overall telephone penetration in the U.S. is high – almost 95% according to the Federal Communications Commission (FCC) "Penetration Report." (Belinfante, 2007) This same report shows that penetration rates are significantly lower for low-income households. Less than 92.3% of households with income less than $20,000 had a working telephone in their households according to the Penetration Report. Disaggregated data are not available in the most recent report, but the 2003 Penetration Report shows that low-income penetration rates differ substantially across states, ranging from 96.5% in Maine (compared to 98.2% of all households) to under 80% in Mississippi (compared to 90.9% of all households).

The FCC has two programs targeted to increase the telephone penetration rates of low-income 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. Both federal programs work in concert with state-level low-income programs. The FCC, in its implementation of the universal service portions of the Telecommunications Act of 1996 ("Act"), dramatically increased the size of its Lifeline subsidy. Prior to the Act, the federal Lifeline program waived the federal subscriber line charge (SLC), which was equal to $3.50 in most jurisdictions, as long as states matched this with a state-funded subsidy to low-income households. The Lifeline program provided all low-income customers in all states a baseline support equal to the federal SLC plus $1.75, for a total of $5.25 in all jurisdictions in 2000 with the exception of the District of Columbia where the federal SLC was less than $3.50.[1] Lifeline customers received additional federal support equal to one half of any support provided by an intrastate program, up to $7.00 in total federal support. In states that took full advantage of the matching federal program Lifeline customers receive a subsidy of $10.50.[2] The federal Linkup program reduces low-income subscribers' initial connection charge by 50 percent of the customary charge, or $30, whichever is less.[3] The level of federal Linkup support was unaffected by the FCC's implementation of the 1996 Act.

Several studies have examined the effect Lifeline and Linkup programs have on penetration rates.[4] The majority of studies have used state-level data that include

[1] The size of the baseline support has risen in recent years because the cap of the federal SLC for residential customers has increased.

[2] Again, the size of federal support has increased since 2000 with increases in residential SLCs. States have the ability to provide additional unmatched subsidy as well.

[3] 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).

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

variables on 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) use state-level data from the 1970, 1980, 1990 and 2000 decennial censuses to estimate penetration rates. Erikson, Kaserman and Mayo (1998) use state-level data from the current population survey ("CPS"), which is available more frequently than the census, to conduct their study. Both of these studies find that Lifeline and Linkup programs have a statistically significant small impact on overall penetration rates. Garbacz and Thompson (2003) find that the demand for local service is highly inelastic (-0.006 to –0.011 in 2000) and that Lifeline and Linkup programs have 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) find 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, 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) use location-specific price data obtained from incumbent local exchange carriers (ILECs) and data from the 1990 census for 1,897 different towns or places. They use both the price of local service and the Lifeline rates as explanatory variables in different regressions, as well as a dummy variable if the state has a Lifeline or Linkup plan interacted with the poverty rate 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 find no significant effect for Lifeline programs, which is consistent with their finding that there is little price elasticity of demand for telephone service overall.

Crandall and Waverman find that a higher charge for connecting a new subscribers reduces penetration rates, estimating an elasticity of penetration with respect to the connection charge ranging from -0.025 to –0.030. Surprisingly, they find that states with a Linkup program have lower penetration rates. 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. Crandall and Waverman also suggest that the Linkup result may be due to a reverse-causation problem because states that have high penetration rates may choose not to participate in federal low-income programs. Perl (1984) simulates the effect on penetration of a $10 per month increase in flat rate charges and finds that low income households were much more sensitive to price increases than the average income household. Cain and McDonald (1991) also find that the elasticity of demand is higher for low-income households.

Our study differs from previous work in at least three important ways. First, we estimate the distinct demand for telephone service of poor households, using geographically disaggregated data on Lifeline and Linkup service prices, a dummy variable indicating whether the state program automatically enrolls eligible households in these plans, local characteristics of the telephone service, and demographic characteristics of households. One virtue of restricting attention to poor households is that implicitly we allow the price elasticity of demand for low-income populations to differ from the rest of the population. Another is that we can directly exploit price variation resulting from new Lifeline subsidies introduced in wake of the 1996 Telecommunications Act. Second, our preferred specification controls for the possible endogeneity of Lifeline prices. The endogeneity of Lifeline prices is particularly 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. Finally, in addition to using local prices, we also use the size of the local calling area as an explanatory variable. 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.

Our empirical analysis uses data on connection and monthly subscription prices for households eligible for Lifeline and Linkup programs, and on the characteristics of relevant service plans. Data on prices and service characteristics, obtained from Bell Operating Company (BOC) tariffs, are matched to more than 8,000 census "places." The rich dataset and our exclusive focus on poor populations allow us to estimate elasticities with respect to Lifeline and Linkup prices. These elasticity estimates are employed to evaluate how effective were 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 6.24%, with a 95% confidence interval between 3.36% and 8.64%. We also conclude that Linkup is more cost effective than

Lifeline because it is targeted at low-income households who do not have telephone service, and that the automatic enrollment programs in three states 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 to a representative household is assumed to be multiplicatively separable in the characteristics of the household and the characteristics of the service. That is, a representative household is willing to pay $\phi(e^t V)$ for telephone service, where t describes the household, V describes the service, and $\phi(\cdot)$ is a strictly increasing function. If the price of telephone service is R, then the household elects service if $e^t V \geq \phi^{-1}(R)$, or equivalently, if $t \geq \ln \phi^{-1}(R) - \ln V \equiv \psi(R) - \ln V$, where $\psi(\cdot)$ is an 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

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

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 $S_g = 1 - F_g(\psi(R) - \ln V)$, and the penetration rate of the whole population is

$$S = \sum_{g=1}^{M} X_g S_g \tag{2}$$

Finally, assume the distribution of household types within 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. Assuming $\mu_g \leq \psi(R) - \ln V$ for all g, it follows that $S_g = e^{-\lambda(\psi(R)-\ln V))+\lambda\mu_g}$ and $S = e^{-\lambda(\psi(R)-\ln V))}\sum_{g=1}^{M} X_g e^{\lambda\mu_g}$.[5] Alternatively, the penetration of the entire population is explained by the logarithmic equation

$$\ln S = -\lambda\psi(R) + \lambda \ln V + \ln\left(\sum_{g=1}^{M} e^{\lambda\mu_g} X_g\right) \tag{3}$$

This model of community demand for telephone service is the basis of our empirical specification. A noteworthy simplifying assumption is that all population groups have the same scale parameter λ. Group differences in demand are captured purely by the group-specific location parameters $\left(\mu_1, \ldots \mu_M\right)$

A basic unit of observation is a population of consumers at location *l*. A vector of group shares $\left(X_{1l}, \ldots X_{Ml}\right)$ describes each population, and the penetration rate at location *l* is

$$\ln S_l = -\lambda\psi(R_l) + \lambda \ln V_l + \ln\left(\sum_{g=1}^{M} \beta_g X_{g_l}\right) \tag{4}$$

where $\beta_g \equiv e^{\lambda\mu_g}$. Both the price ($R_l$) and nature of service (V_l) vary across locations. Thus our empirical model allows for locational variation in population and service characteristics.

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[5] We do not explicitly enforce this constraint in estimation, as we found that it significantly hampered the reliability of our optimization algorithm. As a result, violation of this constraint for some group could cause specification bias. If the constraint is violated in a location for some group, say group 1, then $S_1 = 1$ and the correct specification for that location becomes $S - X_1 = e^{-\lambda(\psi(R)-\ln V))}\sum_{g=2}^{M} X_g e^{\lambda\mu_g}$. Our estimates indicate that this specification issue is most likely to arise for Asians, who generally exhibit a higher demand for telephone service. Fortunately, Monte Carlo simulations (available from the authors) suggest that ignoring the possible specification error only generates negligible biases on estimates of the parameters of interest.

Our empirical model must deal with the fact that telephone service typically requires a monthly subscription price (P_l), and a one-time installation charge (I_l). If the household monthly "discount rate" is α, then $R_l = P_l + \alpha I_l$ and

$$\ln S_l = -\lambda \psi(P_l + \alpha I_l) + \lambda \ln V_l + \ln\left(\sum_{g=1}^{M} \beta_g X_{g_l}\right). \tag{5}$$

The discount rate converts the one-time installation charge into a monthly household expense. We assume that this discount rate, like the scale parameter λ, is constant within the whole population.

Our empirical analysis considers two alternative specifications of the function $\psi(\cdot)$: a linear model with $\psi(R) = R$, and a logarithmic model with $\psi(R) = \ln R$. In the logarithmic model, the parameter λ is the price elasticity of demand with respect to the full price R. In the linear model, this price elasticity is equal to λR. We focus on the linear model, and treat the logarithmic model as a robustness check.

It remains to control for differences in the nature of service at different locations such as the number of people within the local calling area. Let Z_l denote a vector of observable characteristics of the service at location *l*. Then $\lambda \ln V_l = \Gamma(Z_l) + \varepsilon_l$, and

$$\ln S_l = -\lambda \psi(P_l + \alpha I_l) + \ln\left(\sum_{g=1}^{M} \beta_g X_{g_l}\right) + \Gamma(Z_l) + \varepsilon_l \tag{6}$$

Unobservable characteristics of the service are summarized by the random variable ε_l. We specify a functional form for $\Gamma(\cdot)$ after we discuss the data and define Z_l.

An alternative way to write the model is

$$\ln S_l = \ln \beta_1 - \psi(P_l + \alpha I_l) + \ln\left(X_1 + \sum_{g=2}^{M} \tfrac{\beta_g}{\beta_1} X_{g_l}\right) + \Gamma(Z_l) + \varepsilon_l \tag{7}$$

Note that the above model implicitly assumes a continuum of consumers. Since we only have a finite sample of consumers at each location *l*, observed S_l will differ from the S_l predicted by the model due to sampling error. Unfortunately, this sampling error cannot be easily dealt with econometrically since it enters the equation non-linearly through the natural log. Hence we simply assume a continuum in our estimation procedure. We have used Monte-Carlo experiments (available from the authors) to evaluate the biases generated by ignoring this sampling error. Even with the fairly small sample sizes in our data (the median number of households per location is 119), we find extremely small biases on the coefficient estimates.[6]

Data

Data for the analysis come from various sources: the 2000 decennial census, state telephone tariffs, the FCC, and the Local Exchange Routing Guide (LERG). The data include telephone penetration rates, demographics, and prices of basic local telephone service including connection charges, Lifeline and Linkup discounts, and other tariff information. In addition, estimates of the cost of providing local service and several other variables relevant for state regulation are used as instruments to control for possible price endogeneity. The basic data set includes 9,060 census places located in 43 states and the District of Columbia in the original Bell Operating Company (BOC) regions,[7]

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[6] These Monte-Carlo experiments involve 1) assuming that our estimates are the "true" parameters, 2) regenerating data according to these parameters (with sampling error), 3) reestimating (incorrectly assuming no sampling error), and 4) comparing the parameter estimates to the "true" parameters. As mentioned in the text, this procedure indicates negligible biases.

[7] 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

representing approximately 50 million residential access lines. FCC (2000a, 2000b) data indicate that the Lifeline program subsidized approximately 5 percent of the lines in our data in 2000.

The dependent variable is the natural log of a census place'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 collected data on local prices and other data from state tariffs at the level of what the LERG calls "localities", and used the LERG to match prices to wire centers.[8] We then matched wire centers to census places with a cross reference provided by Claritas (2003). Some wire centers contain multiple localities with different rates, and some places served by multiple wire centers lack uniform rates. We dropped these from the sample, reducing the sample size to 8,499 places.

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 for estimation. Thus we implicitly assume that eligible households who have telephone service but do not have Lifeline service are not marginal consumers. As discussed earlier, this assumption makes sense because restructured Lifeline subsidies substantially lowered the price of telephone service to low income households in the wake of the 1996 Telecommunications Act. In the majority of states, Lifeline customers can choose from a variety of local-service

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1996. Census places are either "incorporated areas" (like cities) or "census designated places" that are a separately identified concentration of population.

[8] A wire center includes all customers connected to a particular local switch. In metropolitan areas several wire centers serve a single census place, while in rural areas, a single switch may serve multiple census places.

offerings. Customers may subscribe to a usage-based plan, where they pay for each call or minute of local use in addition to a monthly charge. Customers subscribing to a flat-rate plan pay a monthly charge and are allowed to make an unlimited number of local calls. The majority of the states in the sample offer subscribers both flat-rate and usage-based plans. Only Wisconsin and portions of New York (NYC) and Illinois require that consumers subscribe to a usage-based plan, while Kansas, Kentucky, North Carolina and Maine do not offer usage-based options.[9] The empirical analysis uses the variable *Lifeline50*, which is the minimum monthly expenditure of Lifeline customers making 50 local calls. As robustness checks, we also consider *Lifeline0* and *Lifeline100*, which respectively are the minimum monthly expense for zero or 100 calls.[10] The other 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 black and native American populations generally are lower than average while those for Asians are higher (Belinfante, 1997). To control for possible ethnic differences in the demand for telephone service we include the explanatory variables *White, Black, Native* (Native Americans)*, Asians* and *Other* (other non-white populations); these census variables are equal to the percentage of low-income populations belonging to the respective group.

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[9] Vermont also requires a usage-based plan, but is not included in our dataset. Washington requires Lifeline customers subscribe to a flat-rated plan, while Maryland, Arkansas and West Virginia require Lifeline customers subscribe to a usage-based plan. In each of these states non-Lifeline customers may subscribe to either flat-rate or usage-based plans.

[10] *Lifeline*X is the minimum basic monthly charge plus usage charges across all available plans assuming 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.

An important characteristic of the 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.[11] We expect a positive relationship between *LCA* and *Penetration* holding other factors constant.[12]

We study several alternative specifications corresponding to different endogeneity assumptions about the price variables *Lifeline50* and *Linkup*. Endogeneity might arise if prices were set in response to either unobserved (to the econometrician) demand shocks in the low-income population, or unobserved service characteristics that affect demand. Each of the price variables can be decomposed into a regular price and a subsidy. For example, the *Linkup* price a low-income customer must pay to initiate service is equal to the regular connection charge that a resident of the area would pay to initiate service less whatever subsidy is in effect for low-income individuals in that area. Formally,

$$Linkup_l = Hookup_l - SubsidyLU_l$$

where $Hookup_l$ is the regular connection charge in location l and $SubsidyLU_l$ is the subsidy for low-income individuals in that location. Similarly,

$$Lifeline50_l = Monthly50_l - Subsidy50_l$$

[11] In places served by more than one wire center, the household-weighted average LCA is used for the place. *LCA* is constructed from tariffs, census data, Telecordia (2000), and Claritas (2003).

[12] Many states use value-of-service pricing, where local rates are directly related to the size of a customer's LCA.

where *Monthly50* is the regular minimum monthly expenditure for 50 calls, and *Subsidy50* is the subsidy to that rate for low-income individuals.

Given this setup, we investigate several possible endogeneity assumptions regarding *Lifeline50* and *Linkup*. At one extreme, our ALL EXOGENOUS specification assumes that both *Lifeline50* and *Linkup* are uncorrelated with unobservable variables determining low-income penetration rates. While this is a strong a-priori assumption, it is conceivable that historical and political influences on telephone rates are sufficiently influential in determining prices and sufficiently removed from demand conditions to justify it. The ALL EXOGENOUS specification can be estimated using non-linear least squares.

At the other extreme, our PRICES ENDOGENOUS specification admits correlation between any component of prices and the unobservable demand variables. Such correlation might arise for a number of reasons. Perhaps foremost, regulators or politicians who are determining the subsidies *Subsidy50* and *SubsidyLU* might respond to demand conditions. For example, there may be political pressure for larger subsidies in locations with lower low-income penetration rates. Similarly, regulators or firms might take low-income demand conditions into account when determining the normal rates *Monthly50* and *Hookup*. Another possibility is that either the normal prices or the subsidies are set in response to unobservable service characteristics.

Standard NLLS cannot be used to estimate the PRICES ENDOGENOUS specification. We instead use a generalized method of moments (GMM) procedure that instruments for the possible endogeneity of *Lifeline50* and *Linkup*. We use the variables *StateCost, Competition, Business/Residential, ElectPUC,* and *DemocratPUC* as instruments. Thus we assume these variables exogenously shift prices, and, conditional on explanatory variables, do not directly shift demand and are uncorrelated with demand residuals.

The instrument *StateCost* measures the telephone company's average cost of service in the state. This cost measure is constructed from the FCC (2000a) Hybrid Cost Proxy Model (HCPM) which estimates the forward-looking cost of service in all BOC service territories; these estimates depend on factors such as the population density and length of lines needed to supply service. *StateCost* is the sum of the telephone company's forward-looking costs for all wire centers divided by the number of lines served by the company in the state. We measure cost at the state level rather than the local level because rates are regulated at the state level. Higher cost is expected to increase prices because state regulators are required to set rates that recover carriers' costs of service.[13,14]

The instrument *Business/Residential* (FCC 2000b) measures the proportion of business lines to residential lines in a state in 1999. State regulators set higher telephone rates for businesses in order to subsidize lower rates for households (Palmer 1992). If there are more businesses in a state, then cross-subsidization may be easier politically. Thus a higher ratio of business to residential lines is expected to result in lower residential telephone rates.

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[13] State regulation of rates is generally based on historical rather than forward-looking cost. The two, however, are highly correlated.

[14] Perhaps the most likely violation of our IV assumptions occurs when one interprets the demand residual as an unobserved service characteristic (rather than a demand shock in the poor population). In this case, we need to assume that the instruments do not affect choices of these unobserved service characteristics (if they did, they would likely be correlated with them). While this is a fairly strong assumption, it is a very common one in the differentiated products demand literature (e.g. Berry, Levinsohn, and Pakes (1995)).

The instrument *Competition* from FCC (1996) measures whether competitors providing local switched services had begun operations in a state by March 1996, a date immediately following passage of the 1996 Telecommunications Act. Increased competition for business customers is thought to erode the aforementioned "cross subsidy" from business rates to residential rates.

Finally, the instruments *DemocratPUC* and *ElectPUC* describe the state's Public Utility Commission (PUC) and come from National Association of Regulatory Utility Commissioners (2002). *DemocratPUC* is a dummy variable classifying whether the majority of the PUC commissioners are affiliated with the Democratic party. Given their political base, Democrats are expected to be more inclined to provide larger subsidies for the poor. *ElectPUC* is also a dummy variable, measuring whether the state public utility commissioner is an elected position, or whether it is filled by political appointment. One hypothesis is that elected officials may be more sensitive to the contributions of regulated utilities and set higher residential rates. Rosston, Savage and Wimmer (2008) found this variable to positively affect rates.

We also consider specifications with exogeneity assumptions in between the ALL EXOGENOUS and PRICES ENDOGENOUS specifications. For example, we consider specifications where some components of the rates are assumed to be exogenous while others are assumed to be endogenous. One a-priori attractive specification assumes that the normal rates *Monthly50* and *Hookup* are exogenous, but allows the subsidies *Subsidy50* and *SubsidyLU* to be endogenous. This makes some intuitive sense because the residual measures unobserved demand components for only the poor population. Subsidies are presumably decided with primarily the poor population in mind, while base rates are likely set with the entire population in mind. Hence, the subsidies seem more likely to be correlated with the demand unobservable than the base rates. In these specifications, we can use the base rates, *Monthly50* and *Hookup,* as additional instruments for the overall rates *Lifeline50* and *Linkup*. These are expected to be highly predictive instruments since *Monthly50* and *Hookup* are actual components of *Lifeline50* and *Linkup*.[15]

In specifications where only the discount components of rates are considered endogenous, we drop *StateCost* and *Competition* as instruments because we hypothesize that these two instruments affect prices primarily through the undiscounted rates. Since we are already using the undiscounted rates themselves as instruments, *StateCost* and *Competition* become theoretically redundant. In contrast, we think that the other instruments (*Business/Residential, ElectPUC,* and *DemocratPUC*) potentially do influence rates through the discounts *Subsidy50* and *SubsidyLU*, and continue to use these as instruments in addition to *Monthly50* and *Linkup*.

We also consider alternative specifications where one of the overall rates is endogenous, but the other is exogenous. The Lifeline and Linkup programs differ in the extent to which the federal government provides matching incentives. As discussed in the introduction, in the Lifeline program the federal subsidy increases with the amount of state subsidy. In contrast, in the Linkup program the federal subsidy is fixed at 50% of the customary rate (up to $30). Thus state subsidization of the Linkup rate is not matched by the federal government. Thirty-six (81.8%) of the 44 states in our sample provide additional Lifeline subsidies, while only 12 (27.3%) provide additional Linkup subsidies.

[15] Though note that they are not necessary good instruments – for example if subsidies are highly negatively correlated with the base rates, base rates might not be a good predictor of the discounted rates.

Since Lifeline appears to be the margin on which most states are making their subsidization decisions, whereas the Linkup discount is generally just a simple function of the base rate, a reasonable a-priori specification might treat *Linkup* as exogenous while allowing *Lifeline50* to be endogenous.[16]

In later specifications, we allow two additional service characteristic variables to impact demand in our specifications. A few states have programs that automatically enroll eligible households for Lifeline and Linkup.[17] The dummy variable *Autoenroll* is equal to one if the state has such an automatic enrollment program. Given that some low-income households may not be aware of the lower Lifeline rates, this program would be expected to increase low income penetration. The second additional service characteristic we consider is a measure of intrastate access charges. Intrastate access charges are the fees that local exchange carriers charge long-distance companies for completing non-local intrastate calls. Thus they capture the effect that the prices of calls outside a customer's LCA have on penetration.[18] The variable *Access*, which is equal to access charge for a four-minute intrastate long-distance call, is expected to have a

[16] Lifeline rates may also be more responsive to unobserved demand shocks than Linkup charges because Lifeline rates affect more households. Changes in Linkup charges affect only low-income households not connected to the network, while Lifeline rates, which are paid on a monthly basis, also affect already connected low-income households.

[17] The FCC (2003) reports that three states – MA, NY and ND, have automatic enrollment programs. In Massachusetts, households that qualify for the low-income heating assistance program (LIHEAP) are 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 enrolls a household in the Lifeline and Linkup program when it enrolls in a NYDFA program. The North Dakota Department of Human Services sends certificates to households that allow them to enroll in Lifeline and Linkup programs when they are determined eligible for a program that qualifies 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.

[18] Because customers make both local and long-distance calls, the price of long-distance calls affects subscriber decisions. *See* Hausman, Tardiff, and Belinfante (1993).

negative coefficient.[19] We include only intrastate access charges because interstate long-distance prices in states do not reflect the each state's interstate access charge. Section 254(g) of the Telecommunications Act forbids long distance carriers from charging different rates in different states, even if the states have different costs. As a result, there would be little basis for inclusion of interstate access charges, and we do not have the requisite information to determine if there is a systematic difference in actual retail rates paid by consumers in different places.

Since the variables *Autoenroll* and *Access* are chosen by states, there is again a possible endogeneity issue. As with the pricing variables, we alternatively treat these variables as endogenous or exogenous in different specifications. In the cases where they are treated as endogenous, we use the same set of excluded instruments (*StateCost, Competition, Business/Residential, ElectPUC,* and *DemocratPUC*) that are used to instrument for prices. These variables might affect states' *Autoenroll* and *Access* decisions through the same mechanisms that they affect *Lifeline50* and *Linkup*. Note that in contrast to *Autoenroll* and *Access*, we always treat the service characteristic *LCA* (local calling area - detailed above) as exogenous. We think it is reasonable to assume that *LCA* is uncorrelated with the demand residual because there is a large historical component of local calling area determination.

Table 1 provides summary statistics on the variables used in the analysis.

[19] The access charge used includes charges for originating and terminating minutes for carrier common lines charges (CCLC), switched access, transitional and call-set up charges, along with any charges for state universal service programs. In New Jersey, Maryland, Virginia and West Virginia, the CCLC is determined by a long-distance carrier's share of total intra-state long-distance minutes. In these states, the state commission determines the total amount of money to be recovered through the CCLC and charges carriers on a retroactive basis. We estimate the CCLC in these states using ARMIS (FCC, 2000b)and tariff data.

Table 1

	Mean	Median	Std Deviation	Min	Max
Number of Households	614.50206	119.00000	6916.10395	1.00000	601183.00000
Penetration	0.92961	0.95595	0.08854	0.25676	1.00000
Linkup	11.59864	11.00000	7.65731	0.00000	22.95000
Lifeline0	3.15727	2.78000	2.11459	0.00000	10.88000
Lifeline50	5.02723	5.15000	2.49352	0.00000	14.75000
Lifeline100	7.30878	7.35000	3.14053	0.55000	15.32000
Hookup	36.09155	36.50000	11.34616	12.00000	65.00000
SubsidyLU	24.49291	21.00000	12.21298	6.00000	55.00000
Monthly0	11.11440	10.62000	2.30610	7.30000	21.05000
Monthly50	13.56311	13.50000	2.51918	8.94000	21.75000
Monthly100	15.78323	15.67000	2.80285	10.25000	23.95000
Subsidy0	7.95712	7.50000	2.17190	4.67000	13.10000
Subsidy50	8.53588	9.00000	2.10674	5.25000	13.65000
Subsidy100	8.47445	8.56288	2.20992	3.55000	15.70000
Access	0.14478	0.13650	0.09174	0.02551	0.47294
Autoenroll	0.09942	0.00000	0.29925	0.00000	1.00000
*LCA**	206.06935	70.93789	314.12746	0.20278	1810.22190
White	0.74623	0.83881	0.26679	0.00000	1.00000
Black	0.13598	0.01411	0.23933	0.00000	1.00000
Native	0.01587	0.00000	0.07165	0.00000	1.00000
Asian	0.02082	0.00000	0.05945	0.00000	0.78397
Other	0.08109	0.02990	0.12701	0.00000	1.00000
StateCost	22.71768	21.21457	4.51382	16.08888	38.95610
Competition	0.29027	0.00000	0.45391	0.00000	1.00000
Business/Residential	0.49562	0.51010	0.07774	0.34773	1.96536
DemocratPUC	35.88501	33.33333	27.84690	0.00000	100.00000
PUCElect	0.17108	0.00000	0.37660	0.00000	1.00000

*In thousands

Estimation

Following equation (6), our basic econometric model is:

$$\ln Penetration_l = \theta_0 + \theta_1 Lifeline50_l + \theta_2 Linkup_l$$
$$+ \ln(White_l + \theta_3 Black_l + \theta_4 Native_l + \theta_5 Asian_l + \theta_6 Other_l) + \theta_7 \ln LCA_l + \varepsilon_l$$

This equation is at the census place level, e.g. $Penetration_l$ measures the penetration of low-income households rate at census place l. Also the demographic variables measure the percentage of each group at census-place l. Given the non-linearity of the model and the possible endogeneity of particular explanatory variables, we use the generalized method of moments (GMM) for estimation. Our basic moment assumption for estimation is:

$$E\left[Z_l \otimes \varepsilon_l\right] = 0 \tag{8}$$

i.e. the residuals ε_l are uncorrelated with instruments Z_l. The composition of Z_l varies across different specifications of the model depending on exogeneity assumptions. As discussed above, the demographic variables and *LCA* are always treated as exogenous, so they always enter Z_l. *Lifeline50* and *Linkup* enter Z_l when they are treated as exogenous; when they are treated as endogenous, they are replaced in Z_l by the instruments *StateCost, Competition Business/Residential, ElectPUC,* and *DemocratPUC*. When only the subsidy components of *Lifeline50* and *Linkup* are treated as endogenous, *Lifeline50* and *Linkup* are replaced in Z_l by the instruments *Monthly50, Hookup, Business/Residential, ElectPUC,* and *DemocratPUC*.

Note that, given any arbitrary parameter vector, the implied residuals, $\varepsilon_l(\theta)$, can be computed using (7). At the true parameter vector θ_0, the implied $\varepsilon_l(\theta_0)$ will be the true residuals; at other parameter vectors this is not the case. Thus estimation proceeds by considering:

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

Given the orthogonality assumption on the true residuals given by (8), $G(\theta)$ (and, asymptotically, $G_N(\theta)$) will equal 0 when evaluated at θ_0. At other parameter vectors, this will generally not 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.

$$G_N(\theta)' A G_N(\theta)$$

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 $A=\text{Var}(G_N(\theta))^{-1}$. As is fairly standard, we use a two-step procedure to approximate an optimal weight matrix A.

There are two additional, less standard, econometric considerations we address in our estimation procedure. The first concerns the fact that our observations represent geographic areas. One might be concerned that there are shocks or unobservables that are common or correlated across nearby census places. 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 census places in the same state) in computing these standard errors. This makes sense, for example, if some unobservable service characteristics are determined at the state level.

Formally, the variance of the GMM estimator that minimizes (8) is given by

$$Var(\theta) = \left(\Gamma' A \Gamma\right)^{-1}\left(\Gamma' A V A \Gamma\right)\left(\Gamma' A \Gamma\right)^{-1}$$

where

$$V = Var(G_N(\theta)) \qquad \text{and} \qquad \Gamma = \frac{\partial G(\theta)}{\partial \theta'} \approx \frac{\partial G_N(\theta)}{\partial \theta'}$$

With independent observations, one could consistently estimate V with

$$\hat{V} = \frac{1}{N^2}\sum_l g_l(\theta) g_l(\theta)'$$

evaluated at the estimated θ. However, this is not a consistent estimate of V if there are correlations between the ε_l's.

To consistently estimate V with such clustering, note that

$$V = Var\left(\frac{1}{N}\sum_{s}\sum_{l\in s} g_l\left(\theta\right)\right) = Var\left(\frac{1}{N}\sum_{s}\Phi_s\left(\theta\right)\right)$$

where s indexes states, $l \in s$ are the census places l in state s, and $\Phi_s\left(\theta\right) = \sum_{l\in s} g_l\left(\theta\right)$.

Therefore, assuming that residuals are independent *across* states, the Φ_s's are independent across s and V can be consistently estimated with

$$\hat{V} = \frac{1}{N^2}\sum_{s}\Phi_s\left(\theta\right)\Phi_s\left(\theta\right)'$$

evaluated at the estimated θ. Note that this variance estimate is consistent regardless of the patterns of the residual correlation within a state. For example, one might expect there to be higher levels of correlation between residuals the closer are two census places in the same state. In addition, the patterns of the correlations can vary across states (i.e. in some states, residuals may be highly correlated, in others, perhaps they are less so).[20] It is also a consistent estimate of V whether or not there is heteroskedasticity in the residuals. We use this estimate of V to compute standard errors.[21]

The second non-standard issue concerns the fact that our census-place level data on penetration rates is aggregated. Since the size of the population being aggregated over, n_l, differs across these census-places, we might expect the aggregation to generate heteroskedasticity in our residuals. While this potential heteroskedasticity does not affect the consistency of our estimates (or the consistency of our standard errors given that they are computed as described above), one can gain efficiency by appropriately addressing the heteroskedasticity. This is done by introducing weights into the estimation procedure.

[20] On the other hand, this rules out correlation between census places that are nearby geographically, but in different states. This assumption is more reasonable if the residuals represent unobserved service characteristics that are set by states.
[21] Within state correlation in residuals does not affect estimation of the derivative matrix.

To construct these weights optimally, we first estimate the model ignoring the heteroskedasticity. Then we regress the squared estimated residuals, $\hat{\varepsilon}_l^2$, on functions of n_l to estimate how the variance of the residuals depends on n_l. We use the results of this regression to construct weights w_l which are the inverse of the square root of the predicted variance for census place l (as a function of n_l). We then re-estimate the model, using weighted residuals $w_l\varepsilon_l$ instead of the unweighted residuals ε_l. These weights are optimal, as by construction they equalize the variance of the weighted residuals across observations.

<u>Results</u>

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. Column 2 is the PRICES ENDOGENOUS specification that treats both *Lifeline50* and *Linkup* as endogenous. Column 3 treats only *Lifeline50* as endogenous, and Column 4 treats only *Linkup* as endogenous. Finally, Column 5 treats only *Subsidy50* as endogenous, and Column 6 treats only *Monthly50* as endogenous.

The coefficients on the demographic variables and ln*LCA* 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. Being less than 1, the coefficients on *Black*, *Native*, and *Other* indicate that, all else equal, these groups have lower penetration rates than whites. In contrast, the coefficients on *Asian* indicate that Asian households have higher penetration rates. Note that all of these demographic coefficients are significantly different than 1. 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 about 1.6 (1.8%).

Moving to the price coefficients, in addition to the estimated coefficients θ_1 and θ_2, we also report some interesting functions of the coefficients. First, we report the price elasticities implied by the respective coefficients (*Elasticity50* and *ElasticityLU*). These elasticities are evaluated at the sample median of all the other explanatory variables. Second, note that while both variables are measured in dollars, *Linkup* is a one-time connection fee while *Lifeline50* is a recurring monthly fee. Hence we can use the relation of the two coefficients to approximate the rate at which these households discount the future. Formally, the monthly discount rate implied by the coefficients of the penetration equation is computed as $Discount = \theta_2 / \theta_1$.

Examining the price coefficients and the implied elasticities in Column 1, a first observation is that while negative, they are very small – the elasticities with respect to *Lifeline50* and *Linkup* are -0.0061 and -0.01031. That said, demand for telephone service is known to be very inelastic, and these numbers are consistent with prior research. While the *Linkup* coefficient (and elasticity) is significantly different from zero, the *Lifeline50* coefficient (and elasticity) is not.

Moving to the discount rate implied by these parameters, it is extremely large – 79%. This is coming from the fact that the estimated *Lifeline50* coefficient (on the recurring payment) is only slightly higher than the *Linkup* coefficient (on the one-time fee). This comparison suggests that consumers are very myopic and care about the total current payment for service. One would expect fairly high discount rates in the poor population because of credit constraints. Moreover, if there is any heterogeneity in these discount rates across the population, we are probably measuring the high end. This is because penetration rates are very high (over 90%), so the "marginal" households whose decisions are determining our estimated coefficients are likely the poorest of the poor. That said, 79% still seems unreasonably high.

In contrast to Column 1, Column 2 treats both prices *Lifeline50* and *Linkup* as endogenous. A first observation concerns the F-statistics from the respective first stage regressions, reported at the bottom of the table.[22] While both are statistically significant at conventional levels, they are in a range generally considered to be indicative of weak instruments, particularly in the case of *Linkup*. This will tend to show up as high standard errors on the estimated coefficients on the endogenous variables.

Moving to the coefficients themselves, note that both are considerably larger than their analogues in Column 1. The *Lifeline50* coefficient more than triples, while the *Linkup* coefficient more than doubles. The same is true for the implied elasticities. This is an intuitive result. If *Lifeline50* and/or *Linkup* are in fact 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).[23] This endogeneity would cause the *Lifeline50* and *Linkup* coefficients in Column 1 to be biased towards zero relative to those in Column 2.

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[22] We investigate these first stage regressions in more detail in the next section. Note that these F-statistics were computed in STATA and are robust to clustering by state.

[23] Interpreting the residual as unobserved product characteristics also suggests a positive correlation, becaue service with "better" unobserved characteristics seems likely to be more expensive.

We can use the estimates in Columns 1 and 2 to formally test whether *Lifeline50* and *Linkup* are endogenous. The Hausman test statistic for this comparison is 17.98[24], which is significant at the 95% confidence level. This can be interpreted as evidence that at least one of the two price variables is endogenous. It also suggests that Column 2 estimates are more reliable than those in Column 1. However, perhaps because of the weak instruments both of the coefficients are insignificant at the 90% level.

Since we have more instruments (5) than endogenous variables, the model in Column 2 is formally overidentified. The J-test of these overidentifying restrictions does not reject the null that the moment conditions are valid. While this provides no evidence against our modeling assumptions, this test may be weak given the limited strength of the instruments. A last observation is that the estimated discount rate in Column 2 decreases somewhat from Column 1.

Given that our instruments do not seem to be strong enough to well identify coefficients on two endogenous variables, we next investigate the possibility that one of the two price variables can be treated as exogenous. As explained in the data section, we think *Lifeline50* is more likely to be endogenous than *Linkup*. Column 3 is a specification where *Linkup* is assumed exogenous and *Lifeline50* is allowed to be endogeneous, while Column 4 does the reverse. A Hausman test comparing Column 3 to Column 1 is statistically significant (test statistic is 20.50, significant at 99% confidence), providing relatively strong evidence that *Lifeline50* is in fact endogenous. In contrast, a

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[24] 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.

Hausman test comparing Column 4 to Column 1 is not statistically significant (test statistic is 7.79), thus not providing evidence that only *Linkup* is endogenous. Given that these test results coincide with our a-priori hypothesis regarding possible differences in endogeneity between *Lifeline50* and *Linkup*, we focus on Column 3 as our preferred specification.[25] Also note that the J-statistic does not reject the null hypothesis in Column 3 providing additional support for this specification**.**

Examining the coefficients in Column 3, there are a couple of notable differences from the prior results. First, note that the *Lifeline50* coefficient is now even larger relative to Column 1. In addition, this coefficient is highly statistically significant, suggesting that the instruments are strong enough to generate significant coefficients if only *Lifeline50* is endogenous. As might be expected given it is again being assumed exogenous, the *Linkup* coefficient is now similar to its value in Column 1 and also returns to being statistically significant. Another observation that is supportive of this specification is the smaller implied discount rate. With the large increase in the *Lifeline50* coefficient and the decrease in the *Linkup* coefficient, we get a more reasonable (yet still high) monthly discount rate of about 18%.

The last two columns of Table 2 examine whether we can further refine our endogeneity assumptions. Starting from the specification in Column 3 (i.e. where *Linkup* is assumed exogenous), Column 5 makes the additional assumption that *Monthly50* of *Lifeline50* is exogenous, while Column 6 makes the alternative assumption that *Subsidy50* is exogenous. Recalling our prior discussion, Column 5 would be a-priori

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[25] It would have been preferable to test Column 3 vs. Column 2, and Column 4 vs. Column 2, perhaps rejecting Column 4, but not rejecting Column 3. This would be a more robust test than those that we report. However, the problem is that the standard errors in Column 2 are very high (due to the weakness of the instruments), so these tests so not seem to have enough power to reject anything.

preferred to Column 6, as we think it is more likely that subsidies are endogenous rather than regular rates. However, the Hausman test of Column 6 vs. Column 1 (test statistic is 24.23) provides evidence that even the regular price component of *Lifeline50* is endogenous. While the Hausman test comparing Column 5 vs. Column 1 does not reject that the subsidy component of *Lifeline50* is endogenous, our priors that the subsidy should be more endogenous than the regular rate make us reluctant to take this as solid evidence that *Subsidy50* is exogenous. Hence we adopt the "safer" Column 3 specification which treates the entire *Lifeline50* as endogenous as our preferred specification.

Table 3 considers a variety of simple robustness checks on the model. All are perturbations of our preferred specification which treats the monthly Lifeline rate as endogenous, i.e. Column 3 of Table 2. Columns 1 and 2, respectively, use *Lifeline0* and *Lifeline100* as the monthly price instead of *Lifeline50*. As described earlier, these variables respectively measure the minimum monthly expenditure of Lifeline customers making 0 and 100, rather than 50, local calls. Column 3 drops California from the sample. Unlike other states, California does not require formal verification of eligibility for Lifeline and Linkup programs. As such, California has an extremely high take-up rate.[26] Given the size of the state, one might be concerned that this peculiarity may be driving some the results. Column 4 is another robustness check that estimates a model dropping 10% of census places with the smallest populations of poor households. Finally, column 5 estimates the logarithmic model described earlier where $\psi(R) = \ln R$. This leads to the econometric model:

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

$$\ln Penetration_l = \theta_0 + \theta_1 \ln(\theta_2 Lifeline50_l + Linkup_l)$$
$$+ \ln(White_l + \theta_3 Black_l + \theta_4 Native_l + \theta_5 Asian_l + \theta_6 Other_l) + \theta_7 \ln LCA_l + \varepsilon_l$$

where

$$Price = \theta_2 Life50 + Linkup$$

is the capitalized price of service and

$$Discount = \frac{1}{\theta_2}$$

is the implied monthly discount rate. Given the alternative specification, it is hard to compare the estimated price coefficients in Column 5 to the other models. However, the implied elasticities are directly comparable. Overall, Table 3 shows that our results are robust. In particular, the Lifeline and Linkup elasticities do not move by more than about 30%, and generally stay significant. The only coefficient that loses significance is the *Linkup* coefficient in the *Lifeline0* specification, and this is due mainly to the higher standard error.

Table 4 adds two additional service characteristic variables to our specifications, *Autoenroll* and *Access*. As previously discussed, these potentially are endogenous variables. Column 1 starts by adding *Autoenroll* where all variables including prices are assumed exogenous. As we are already reasonably convinced that *Lifeline50* is not exogenous, this column is mainly for descriptive and comparison purposes. Column 2 takes our preferred specification from Column 3 of Table 2 and adds *Autoenroll*, treating it as exogenous. Column 3 does the same, except that it treats *Autoenroll* as endogenous. Both of these specifications generate price coefficients very similar to Column 3 of Table 2 - the *Lifeline50* coefficient increases slightly and the *Linkup* coefficient decreases slightly. These coefficients remain statistically significant, except for the *Linkup* coefficient in Column 3, which becomes borderline insignificant.

The coefficients on *Autoenroll* also are similar across the Column 2 and Column 3. Both coefficients are statistically significant, even though the instruments for *Autoenroll* are quite weak.[27] The magnitude of the coefficients are also quite large – *Autoenroll* is a dummy variable that enters linearly, so the coefficient of 0.0346 in Column 2 indicates that *Autoenroll* =1 increases ln*Penetration* by 3.46 percent. Given that prices enter the penetration equation linearly, the coefficient on *Autoenroll* divided by θ_2 can be interpreted as the reduction in the fixed transaction cost of initiating Lifeline service resulting from an automatic enrollment policy. This is equal to almost $45, suggesting that an automatic enrollment policy has substantial value to consumers.

Given that penetration rates are on average around 92%, a 3.46 percent increase in ln*Penetration* translates to more than a 3 percentage point increase in *Penetration*, implying that an automatic enrollment policy reduces the number of household without service by more than 30%. While this is a large number, it makes sense given the low take-up of these programs in states without automatic enrollment policies.[28] A Hausman test comparing Column 2 to Column 3 does not reject the null (test statistic 0.508) – so there is no evidence that *Autoenroll* is endogenous. As such, we consider Column 2 as our preferred specification, with the caveat that the weakness of the instruments for *Autoenroll* means that this test may have very little power. Fortunately, since the results are so similar across the two specifications, it does not really matter which specification

[27] However, the F-statistic is so low here that we are somewhat suspicious of the validity of the t-test.
[28] The FCC (2003) estimates that about 33 percent of the low-income households that were eligible for the Lifeline and Linkup programs in 2000 participated in the programs. The Center for Media Education/Center for Policy Alternatives (1999) also reports low participation rates.

one believes more. We prefer the *Autoenroll* exogenous specification because it is simpler.

Column 4 adds *Access* to the specification, again starting by treating all variables as exogenous. Then, starting from the Column 2 specification (i.e. *Autoenroll* and *Linkup* exogenous, and *Lifeline50* endogenous), Columns 5 and 6 add *Access* and treat it as exogenous and endogenous, respectively. While the *Access* coefficients are all the anticipated sign, none are significant and the magnitudes are very small. Given the similar coefficients, it is not surprising that a Hausman test does not reject the null that *Access* is exogenous (test statistic – 1.21). In addition, note that the price and *Autoenroll* coefficients in both specifications are extremely similar to those without *Access*. Thus the inclusion of *Access* as an explanatory variable does add much explanatory power or change our other estimates. We drop this variable from our preferred specification for simplicity.

In summary, we adopt Column 2 in Table 4 as our preferred specification for the penetration equation. This specification controls for the possible endogeneity of *Lifeline50* and treats 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 for which controlling for endogeneity clearly 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. Finally, there are significant demographic differences in the demand for service.[29]

Table 2

	ALL EXOGENOUS (1)	PRICES ENDOGENOUS (2)	*Lifeline50* ENDOGENOUS (3)	*Linkup* ENDOGENOUS (4)	*Subsidy50* ENDOGENOUS (5)	*Monthly50* ENDOGENOUS (6)
Lifeline50	-0.00119	-0.00374	-0.00481 ***	-0.00075	-0.00358 **	-0.00351 **
	(0.00159)	(0.00274)	(0.00182)	(0.00221)	(0.00162)	(0.00143)
Linkup	-0.00094 *	-0.00226	-0.00087 **	-0.00379 *	-0.00063	-0.00091 **
	(0.00054)	(0.00168)	(0.00043)	(0.00198)	(0.00046)	(0.00045)
Black	0.90448 ***	0.91308 ***	0.90798 ***	0.92177 ***	0.90454 ***	0.90740 ***
	(0.00936)	(0.01214)	(0.00903)	(0.01308)	(0.00931)	(0.00899)
Native	0.74732 ***	0.76050 ***	0.76138 ***	0.75621 ***	0.76517 ***	0.75940 ***
	(0.02559)	(0.02612)	(0.02574)	(0.02720)	(0.02811)	(0.02607)
Asian	1.08493 ***	1.06478 ***	1.07545 ***	1.05933 ***	1.07522 ***	1.08199 ***
	(0.02801)	(0.02539)	(0.02573)	(0.02658)	(0.02519)	(0.02457)
Other	0.92850 ***	0.92181 ***	0.91518 ***	0.93043 ***	0.92206 ***	0.91718 ***
	(0.01774)	(0.01744)	(0.01580)	(0.01614)	(0.01502)	(0.01606)
lnLCA	0.01848 ***	0.01697 ***	0.01708 ***	0.01756 ***	0.01830 ***	0.01736 ***
	(0.00171)	(0.00160)	(0.00168)	(0.00161)	(0.00177)	(0.00165)
constant	-0.24429 ***	-0.20067 ***	-0.21323 ***	-0.20429 ***	-0.23622 ***	-0.22214 ***
	(0.02522)	(0.02797)	(0.02722)	(0.02970)	(0.02681)	(0.02571)
Elasticity50	-0.00613	-0.01926	-0.02476 ***	-0.00384	-0.01842 **	-0.01808 **
	(0.00811)	(0.01413)	(0.00938)	(0.01140)	(0.00836)	(0.00735)
ElasticityLU	-0.01031 *	-0.02484	-0.00956 **	-0.04165 *	-0.00692	-0.00999 **
	(0.00590)	(0.01847)	(0.00473)	(0.02178)	(0.00510)	(0.00499)
Discount	0.78720	0.60395	0.18065	5.07837	0.17580	0.25865
	(1.25463)	(0.81742)	(0.11723)	(16.55482)	(0.15989)	(0.17166)
Var(yhat)	0.00236	0.00220	0.00227	0.00189	0.00231	0.00232
R2	0.19475	0.18169	0.18720	0.15574	0.19065	0.19117
degrees of freedom		3	4	4	3	5
Hausman Test^		17.98 **	20.50 ***	7.79	11.78	24.23 ***
J-statistic		4.33	4.41	5.74	4.07	5.97
F-stat(Lifeline50)		3.23 **	3.33 **		12.24 ***	
F-stat(Linkup)		2.78 **		3.22 **		

^Each Hausman test compares the all exogenous specification against the alternative specified in the column heading.

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

[29] The estimated coefficient on *Asian* suggests that the maintained assumption $\mu_g \le \psi(R) - \ln V$ may be violated for this group in some locations, in which case the alternative specification described in footnote 5 would be appropriate for these locations. Monte Carlo simulations (available from the authors) demonstrate that neither this possible specification error for Asians or other groups nor sampling error cause any significant estimation bias. Furthermore, we re-estimated the model under the assumption that all Asians have telephone service and obtained essentially the same estimates for other coefficients.

Table 3

	Lifeline0	*Lifeline100*	DROP CALIFORNIA	DROP SMALLEST 10%		LOGARITHMIC MODEL
	(1)	(2)	(3)	(4)		(5)
Lifeline	-0.01011 *	-0.00543 **	-0.00446 **	-0.00329 **	ln *Price*	-0.03530 ***
	(0.00568)	(0.00216)	(0.00186)	(0.00167)		(0.00994)
Linkup	-0.00063	-0.00102 **	-0.00094 **	-0.00099 **	*Lifeline50*	8.17458 *
	(0.00070)	(0.00041)	(0.00043)	(0.00043)		(4.90757)
Black	0.90481 ***	0.90618 ***	0.90870 ***	0.90287 ***	*Black*	0.90720 ***
	(0.01175)	(0.00973)	(0.00944)	(0.00839)		(0.00879)
Native	0.76220 ***	0.75877 ***	0.76108 ***	0.76145 ***	*Native*	0.76144 ***
	(0.03626)	(0.02622)	(0.02590)	(0.02750)		(0.02529)
Asian	1.08045 ***	1.07547 ***	1.10768 ***	1.09774 ***	*Asian*	1.07617 ***
	(0.02412)	(0.02847)	(0.02107)	(0.03114)		(0.02625)
Other	0.90717 ***	0.90474 ***	0.90006 ***	0.91060 ***	*Other*	0.91213 ***
	(0.02168)	(0.01912)	(0.01374)	(0.01860)		(0.01616)
lnLCA	0.01655 ***	0.01794 ***	0.01686 ***	0.01653 ***	*lnLCA*	0.01719 ***
	(0.00197)	(0.00166)	(0.00168)	(0.00143)		(0.00165)
constant	-0.19935 ***	-0.20598 ***	-0.21156 ***	-0.21124 ***	*constant*	-0.11143 **
	(0.03313)	(0.02914)	(0.02702)	(0.02490)		(0.05628)
Elasticity50	-0.02810 *	-0.03991 **	-0.02353 **	-0.01613 **	*Elasticity50*	-0.02747 ***
	(0.01580)	(0.01586)	(0.00981)	(0.00818)		(0.00924)
ElasticityLU	-0.00698	-0.01122 **	-0.01031 **	-0.01093 **	*ElasticityLU*	-0.00718 **
	(0.00768)	(0.00454)	(0.00471)	(0.00472)		(0.00352)
Discount	0.06276	0.18788 *	0.21035	0.30175	*Discount*	0.12233 *
	(0.09399)	(0.10528)	(0.13648)	(0.20467)		(0.07344)
$Var(y_{hat})$	0.00193	0.00205	0.00234	0.00239	$Var(y_{hat})$	0.00222
R2	0.16716	0.16890	0.18644	0.23296	R2	0.18315
J-statistic	3.83418	2.73789	4.45853	4.08963	J-statistic	4.01961
F-stat(Lifeline)	1.22	3.46 **	3.33 **	3.95 ***		3.33 **

Table 4

	Autoenroll Model			Autoenroll + Access Model		
	ALL EXOGENOUS	*Life50* ENDOGENOUS	*Life50 + Auto* ENDOGENOUS	ALL EXOGENOUS	*Life50* ENDOGENOUS	*Life50 + Access.* ENDOGENOUS
	(1)	(2)	(3)	(4)	(5)	(6)
Lifeline50	-0.00179	-0.00526 ***	-0.00546 ***	-0.00183	-0.00556 ***	-0.00541 ***
	(0.00150)	(0.00182)	(0.00190)	(0.00147)	(0.00192)	(0.00195)
Linkup	-0.00083	-0.00077 *	-0.00071	-0.00084	-0.00076 *	-0.00080 *
	(0.00052)	(0.00042)	(0.00044)	(0.00052)	(0.00042)	(0.00043)
Autoenroll	0.02774 ***	0.03461 ***	0.04292 ***	0.02790 ***	0.03520 ***	0.03524 ***
	(0.00564)	(0.00371)	(0.01547)	(0.00575)	(0.00422)	(0.00426)
Access				-0.00843	-0.01476	-0.02460
				(0.04811)	(0.04738)	(0.07400)
Black	0.90808 ***	0.91231 ***	0.91363 ***	0.90782 ***	0.91219 ***	0.91211 ***
	(0.00946)	(0.00905)	(0.00922)	(0.00899)	(0.00873)	(0.00910)
Native	0.75229 ***	0.76429 ***	0.76775 ***	0.75315 ***	0.76637 ***	0.76796 ***
	(0.02550)	(0.02508)	(0.02669)	(0.02614)	(0.02618)	(0.02801)
Asian	1.08280 ***	1.07282 ***	1.07097 ***	1.08128 ***	1.06945 ***	1.06827 ***
	(0.02561)	(0.02421)	(0.02389)	(0.02612)	(0.02516)	(0.02856)
Other	0.93117 ***	0.91973 ***	0.92076 ***	0.93180 ***	0.92101 ***	0.92344 ***
	(0.01851)	(0.01565)	(0.01577)	(0.01843)	(0.01566)	(0.01879)
lnLCA	0.01788 ***	0.01681 ***	0.01685 ***	0.01784 ***	0.01667 ***	0.01664 ***
	(0.00170)	(0.00174)	(0.00175)	(0.00170)	(0.00175)	(0.00176)
constant	-0.23942 ***	-0.21181 ***	-0.21299 ***	-0.23763 ***	-0.20674 ***	-0.20571 ***
	(0.02554)	(0.02802)	(0.02826)	(0.02614)	(0.02967)	(0.03266)
Elasticity50	-0.00921	-0.02707 ***	-0.02811 ***	-0.00940	-0.02864 ***	-0.02787 ***
	(0.00769)	(0.00939)	(0.00976)	(0.00758)	(0.00991)	(0.01005)
ElasticityLU	-0.00917	-0.00848 *	-0.00784	-0.00919	-0.00840 *	-0.00876 *
	(0.00567)	(0.00467)	(0.00483)	(0.00570)	(0.00457)	(0.00475)
ElasticityAccess				-0.00084	-0.00201	-0.00336
				(0.00637)	(0.00647)	(0.01010)
Discount	0.46623	0.14662	0.13060	0.45751	0.13736	0.14713
	(0.52856)	(0.09790)	(0.09654)	(0.51040)	(0.09098)	(0.09640)
$Var(y_{hat})$	0.00243	0.00235	0.00234	0.00243	0.00234	0.00235
R2	0.20032	0.19439	0.19318	0.20024	0.19356	0.19399
degrees of freedom		4	3		4	3
Hausman Tests^		0.50805			1.21009	
J-statistic		2.41388	2.07227		2.25196	2.27297
F-stat(Lifeline50)		3.06 **	3.33 **		3.09 **	3.06 **
F-stat(Linkup)						
F-stat(Autoenroll)			1.13			
F-stat(Access)						5.10 ***

^One Hausman test compares Column 2 against Column 3; the other compares Column 5 against Column 6.

Policy Experiment

Using the estimates from our preferred specification, we evaluate the impact of the Lifeline and Linkup plans on low-income penetration. We use the estimated penetration equation to see how the penetration rates for low-income customers would change if the Lifeline and Linkup programs were discontinued. The actual penetration rate for low income households in our sample is 93.95%. Table 5 below presents two different sets of estimates for the effect of Lifeline and Linkup because different states have different Autoenroll policies. The first column presents the results of simply eliminating Lifeline and Linkup subsidies while allowing *Autoenroll* still to have the same positive impact on predicted penetration even though the programs to which the automatic enrollment policies apply no longer exist. The policy experiment is consistent with the interpretation that automatic enrollment policies reduce the transaction cost of subscribing to subsidized telephone service. The second column shows the results of eliminating Lifeline and Linkup when *Autoenroll* = 0 in all states both before and after the elimination of the low-income support programs. This adjustment results in slightly lower estimated impacts from each of the two programs. Finally, the third column shows the total effect 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.

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

Baseline auto-enrollment policy	Actual	Zero in all states	Actual
Auto-enrollment after policy change	Actual	Zero in all states	Zero in all states
Policy change	*Resulting decrease in penetration*		
Eliminate Lifeline and Linkup		5.72%	6.24%
		[2.89%, 8.16%]	[3.36%, 8.64%]
Eliminate Lifeline	4.00%	3.97%	
	[1.45%, 6.14%]	[1.44%, 6.05%]	
Eliminate Linkup	1.85%	1.83%	
	[0.19%, 3.55%]	[0.19%, 3.48%]	

Actual penetration in sample = 93.95% [95% Confidence Interval]

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 6.24% (5.72%). Most of this increase is explained by the incremental effect of Lifeline, i.e. if Lifeline were discontinued then penetration would be 4.00% (3.97%) lower. Removing Linkup would reduce predicted penetration of telephone service for poor households by 1.85% (1.83%).[30]

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 in the appendix. We calculate that the annual federal funding for Lifeline and Linkup in our sample was about $196 million in 2000. In addition, we calculate that states spent another $57.5 million on these two programs. There are about 5.2 million low-income households in our sample. A 6.24% (5.72%) change in penetration among low-income households, means that these

[30] 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.

programs encourage 324,000 (300,000) more low-income households to subscribe to the telephone network. This works out to a cost of $782 ($846) per household per year. Furthermore, there may be additional costs associated with automatic enrollment polices, as well as other implementation costs.

Linkup appears to be much more cost effective than Lifeline. Linkup costs less than 8% of the Lifeline program, yet has about half 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. One of the reasons Linkup is more cost effective is that by definition it is targeted at poor households who do not have telephone service.

Conclusions

Using data from 8,499 places, we conclude that low-income subsidy programs have increased low-income telephone penetration by 6.24%. The conclusion is based on estimated price elasticities of demand with respect to subscription and connection charges for poor households of -0.027 and -0.008 respectively. These estimated elasticities are low but nevertheless significantly 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.

Because of the high discount rate that low-income households have, 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 the typical 50% discount for Linkup charges could be a large amount to put up for telephone service if the expected tenure in the residence is short. 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 6.24% of poor households in our sample at an expense of $782 each.

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Appendix – Estimating Lifeline and Linkup expense in our sample

Because the sample of 8,499 census places employed in the study does not cover the whole country, it is necessary to estimate the total cost of the Lifeline and Linkup programs for the places included in the sample. Our data do not include all of the households in a state for 3 reasons: 1) RBOCs do not typically serve the entire state; 2) census places do not cover the entire state; primarily rural areas are not in census places; and 3) we eliminated places for which we could not identify a unique price.

We employ three main sources of data to estimate the cost of the Lifeline and Linkup programs for the Census Places included in this study. These data include: 1) FCC's ARMIS database, which contains data on the number of Lifeline lines in each study area (each company in each state); 2) FCC "Monitoring Report" information on the Federal Lifeline and Link-Up subsidies to each study area each state; and 3) FCC (2003) estimates of the number of households eligible for the Lifeline and Linkup programs in each state in 2000.

Because households above the poverty level are eligible to receive Lifeline and Linkup subsidies in several states (e.g., California households with incomes below 150% of the poverty line are eligible for Lifeline and Linkup subsidies), the actual number of Lifeline subscribers may overestimate the number of households receiving Lifeline and Linkup subsidies in our sample of households below the poverty level. To estimate the number of households below the poverty level receiving Lifeline and Linkup subsidies, we compared the FCC's estimate of the number of households eligible for the Lifeline subsidy with the actual number of Lifeline recipients and the number of households below the poverty level in each state. In cases where the number of households that were eligible or receiving the Lifeline subsidies (*Eligible HH*) exceeded the number of households below the poverty level (*Pov HH*), we deflated the number of Lifeline lines in a study area using the following weight:

$$w = Pov\,HH \,/\left(max(\,Eligible\,HH\,, Lifelines\,)\right).$$

In cases where the number of households below the poverty line in a study area exceeds both the number of eligible households and the number of Lifeline lines, we assume that all households receiving the Lifeline subsidy had incomes below the poverty level (w=1). Weighted Lifeline lines equals the product of Lifeline lines in a study area and w. The same methodology is used to determine weighted Linkup dollars.

We allocate weighted Lifeline and Linkup dollars (Monitoring Report) to census places based on a place's share of poor households in a state. We use the census variable P92 (Poverty Status in 1999 of households by household type) to determine the number of poor households in each place and study area. Federal and state subsidies for Lifeline are calculated for each state as follows:

Lifeline program

$$Subsidy50 = Monthly50 - Lifeline50$$

$$Federal50 = Min\left[\left(\$1.75 + SLC + \frac{Monthly50 - Lifeline50 - (SLC + \$1.75)}{3}\right), \$7\right]$$

$$State50 = Monthly50 - Lifeline50 - Federal50,$$

where *SLC* equals the federal subscriber line charge.[31] The Lifeline subsidy in each census place (and the amount corresponding to the Federal and State governments) equals the product of the number Lifeline lines allocated to a place and the per-line subsidy. The total cost of the Lifeline program in our sample equals the sum of the subsidies in each place.

Linkup program

We allocate Federal Linkup dollars to each census place using the product of the share of state poor households corresponding to each census place and the annual Federal Linkup in the state.

To determine the Federal and state components per line:

$$SubsidyLU = Hookup - Linkup$$

$$FederalLU = Min(.50 * Hookup, 30)$$

$$StateLU = Linkup - FederalLU$$

We estimate number of Linkup households in our data as the ratio of Federal dollars allocated to our data (from above) to Federal per line subsidy. The Federal and state Linkup subsidies per place equal the product of the number of estimated Linkups in each census place and the per-line subsidies. The estimated total cost of the Linkup program in our sample equals the sum of the estimated census-place subsidies.

[31] With the exception of the District of Columbia, the federal residential SLC equaled \$3.50 in all states on January 1, 2000. The SLC equaled \$3.32 in the District of Columbia.