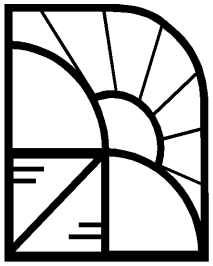

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Phoenix Center Policy Paper Number 55:

A Fresh Look at the Lifeline Program

George S. Ford, PhD.

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Phoenix Center Policy Paper No. 55
A Fresh Look at the Lifeline Program

George S. Ford, PhD*

Abstract: In an effort to expand the use of telecommunications services by low-income Americans, the Federal Communications Commission's Lifeline program offers subsidies to qualifying low-income households. In recent years, the program has undergone significant reform and more modifications have been proposed. Much attention is devoted to improving the administration of the program to reduce waste, fraud and abuse, but some reforms appear motivated by the claim that that nearly all Lifeline subscribers would obtain service even without the subsidy. In this POLICY PAPER, I review the evidence supporting that claim and find it lacking. I then offer new empirical evidence showing that in modern times the relationship between regular paid subscriptions and Lifeline accounts reveal no displacement. Theoretical analysis is offered showing that the "free but limited" service packages offered by resellers may explain this result. This analysis also reveals that many of the Commission's reforms, including proposals to exclude resellers from the program and the scheduled increases in minimum service standards, are counterproductive in that both reforms will increase the alleged displacement of Lifeline for regular accounts and reduce the adoption of advanced telecommunications services by low-income Americans.

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* Chief Economist, Phoenix Center for Advanced Legal & Economic Public Policy Studies. The views expressed in this paper are the authors' alone and do not represent the views of the Phoenix Center or its staff. The author thanks R. Carter Hill and T. Randolph Beard for helpful comments and suggestions on this paper.

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I. Introduction

In an effort to make telecommunications services more affordable to low-income Americans, since 1985 the Federal Communications Commission ("FCC") has offered discounted service to qualifying low-income consumers through its Lifeline subsidy program.[1] Today, almost all the one billion-plus subsidy dollars go to mobile wireless subscriptions, with most of those accounts provided by Mobile Virtual Network Operators ("MVNOs"), colloquially known as wireless resellers. Despite a federal subsidy of approximately $10 per month, beginning in 2009 some MVNOs began offering a basic package of services, including in later years broadband, voice minutes and text messages, at *no charge* to qualifying homes, an unquestionably affordable service plan that is now the

[1] *See, e.g., In the Matter of Lifeline and Link Up Reform and Modernization Telecommunications Carriers Eligible for Universal Service Support Connect America Fund,* FCC 16-38, THIRD REPORT AND ORDER, FURTHER REPORT AND ORDER, AND ORDER ON RECONSIDERATION, 31 FCC Rcd. 3962 (rel. April 27, 2016) (hereinafter "*Lifeline Modernization Order*") at ¶¶ 71, 23 (available at: https://apps.fcc.gov/edocs_public/attachmatch/FCC-16-38A1.pdf).

most popular option for Lifeline consumers.[2] In 2016, there were about twelve million Lifeline accounts, with about 90% being wireless accounts.[3]

Over the past decade, the Lifeline program has undergone substantial reform aimed at administrative improvements in the eligibility process, changes to the treatment of tribal areas, and reductions in the waste, fraud and abuse inherent in nearly all government subsidy programs.[4] At present, the FCC is contemplating additional reforms.[5] While the mission of the Lifeline program is "to target a broad audience of low-income households" with affordable advanced communications services, some economists and policymakers, while not opposed to low-income subsidies for communications service *per se*, argue that Lifeline subsidies must be targeted at households that would not have

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[2] *See, e.g., In the Matter of Bridging the Digital Divide for Low-Income Consumers, Lifeline and Link Up Reform and Modernization, Telecommunications Carriers Eligible for Universal Service Support,* FCC 17-155, FOURTH REPORT AND ORDER, ORDER ON RECONSIDERATION, MEMORANDUM OPINION AND ORDER, NOTICE OF PROPOSED RULEMAKING, AND NOTICE OF INQUIRY, 32 FCC Rcd. 10475 (rel. December 1, 2017) (hereinafter *"2017 NPRM"*) (available at: https://docs.fcc.gov/public/attachments/FCC-17-155A1.pdf) at ¶ 112 ("(In 2016, [] 85 percent of all Lifeline program participants [] subscribed to plans providing free-to-the-end-user Lifeline service."). SafeLink Wireless, one of the nation's largest providers of mobile Lifeline accounts, offers users a bundle of 1 GB of mobile data, 1,000 minutes of voice calls, unlimited text messages, and a free Android-based smartphone. (Site visited on June 13, 2019: www.safelinkwireless.com.)

[3] *Universal Service Monitoring Report*, Federal Communications Commission (2017), at Table 2.1 and 2.4 (available at: https://www.fcc.gov/sites/default/files/2017_universal_service_monitoring_report.pdf).

[4] *See, e.g., In the Matter of Lifeline and Link Up Reform and Modernization Lifeline and Link Up, Federal-State Joint Board on Universal Service, Advancing Broadband Availability Through Digital Literacy Training,* FCC 12-11, REPORT AND ORDER AND FURTHER NOTICE OF PROPOSED RULEMAKING, 27 FCC Rcd. 6656 (rel. February 6, 2012) (hereinafter *"2012 Lifeline Reform Order"*); *Lifeline Modernization Order, supra* n. 1; *see also Additional Action Needed to Address Significant Risk in FCC's Lifeline Program*, GOVERNMENT ACCOUNTABILITY OFFICE, GAO-17-538 (May 30, 2017) (available at: https://www.gao.gov/products/GAO-17-538); *FCC Should Evaluate the Efficiency and Effectiveness of the Lifeline Program*, GOVERNMENT ACCOUNTABILITY OFFICE, GAO-15-335 (Washington, D.C., March 24, 2015) (available at: https://www.gao.gov/products/GAO-15-335); *Improved Management Can Enhance FCC Decision Making for the Universal Service Fund Low-Income Program*, GOVERNMENT ACCOUNTABILITY OFFICE, GAO-11-11 (October 28, 2010) (available at: https://www.gao.gov/products/GAO-11-11); D. Thornton, *FCC Details Plan to Cut Fraud and Abuse in Lifeline Program*, FEDERAL NEWS NETWORK (September 20, 2017) (available at: https://federalnewsnetwork.com/hearings-oversight/2017/09/fcc-details-plans-to-cut-fraud-and-abuse-in-lifeline-program).

[5] *2017 NPRM, supra* n. 2.

telecommunications services *but for* the subsidy. That is, in economic parlance, the subsidy program should target *marginal* and not *inframarginal* low-income households, where inframarginal accounts are those that would exist absent the subsidy. Based on results from a 2015 graduate student research paper, these economists and policymakers make the empirical claim that nearly all Lifeline subscribers—about seven-of-eight—are inframarginal users, indicating a very high displacement rate of regular paid subscriptions for Lifeline accounts.[6]

Lifeline involves a more than billion-dollar subsidy, and concerns over inframarginal users displacing regular paid subscriptions underlie many of the proposals for reforming the program. In this POLICY PAPER, I consider the prudence of some proposed reforms by conducting an economic and empirical analysis of the Lifeline program with particular attention to the displacement of paid subscriptions by subsidized ones. My findings are at odds with both the existing (and now dated) empirical evidence, and many of the proposed reforms, including efforts to reduce reseller access to subsidies, to return to partially-discounted rather than free services, and aggressive increases in prescriptive minimum service standards. Theory indicates that the basic-package of free service offered by resellers reduces the displacement rate while higher minimum service standards and maximum discount policies increase the displacement rate. If reducing displacement is a goal, then returning to a partially-discounted rather than free (or completely discounted) service is among the worst of all possible options. Using the most recent data on Lifeline and regular paid wireless accounts, my straightforward empirical analysis indicates no displacement between regular and Lifeline mobile wireless accounts in the current, reseller-dominant Lifeline environment. Consequently, foreclosing access to reseller services will leave millions of Americans without basic communications services. This result is robust to specification alternatives, different estimators, potential outliers, and sample choices. I conclude that both conventional economic theory, and empirical evidence using the most recent data, provide no support whatsoever for many current reform proposals.

II. Background

Section 254(b)(1) of the Communications Act of 1934 directs the FCC to ensure that "quality services [] be available at just, reasonable, and affordable

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[6] O. Ukhaneva, *Universal Service in a Wireless World*, Unpublished Manuscript, Georgetown University (November 17, 2015) (available at: https://www.aeaweb.org/conference/2016/retrieve.php?pdfid=1011).

rates" for the "preservation and advancement of universal service."[7] This mandate along with other parts of Section 254(b) serve as the basis for the Commission's numerous Universal Service programs, including the Lifeline program created in 1985.[8] The stated mission of the Lifeline program is to "target a broad audience of low-income households in need of improved access to voice and broadband services"[9] by making "voice and broadband services more available and affordable for low-income consumers by providing a discount on these services."[10] To do so, the Lifeline program provides a monthly subsidy to eligible low-income households (those with incomes 135% or less than the federal poverty guidelines) for the purpose of acquiring telecommunications services. Eligibility for the program also may be determined by receipt of some other low-income program such as Supplemental Nutrition Assistance Program ("SNAP" or food stamps), Medicaid, and so forth, all of which are tied to income.[11]

Since 2012, the federal monthly subsidy for the Lifeline program is $9.25 for basic Lifeline subscribers, and the subsidy now applies to an array of services including fixed and mobile voice services and, since 2016, broadband Internet connections.[12] Certified tribal areas receive an additional $25 per month in subsidies (for a total of $34.25).[13] There is some state-level supplementation in

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7 47 U.S.C. § 254(b).

8 General information on the Universal Services program is available at: https://www.fcc.gov/general/universal-service.

9 *Lifeline Modernization Order, supra* n. 1 at ¶¶ 175, 178 (emphasis supplied).

10 *National Lifeline Association, et. al. v. FCC,* 921 F.3d 1102, 1105 (D.C. Cir. 2019).

11 Details on qualifications are discussed at: https://www.lifelinesupport.org/ls/do-i-qualify/default.aspx; https://www.lifelinesupport.org/ls/do-i-qualify/federal-poverty-guidelines.aspx. Detailed data on eligibility by state is provided in: https://www.usac.org/_res/documents/li/xls/stats/Eligible-Lifeline-Population-Statistics.xlsx.

12 *See 2012 Lifeline Reform Order, supra* n. 4; *Lifeline Modernization Order, supra* n. 1; *see also* S. Lichtenberg, *Lifeline and the States: Designating and Monitoring Eligible Telecommunications Carriers*, NATIONAL REGULATORY RESEARCH INITIATIVE REPORT No. 13-12 (November 2013) (available at: http://nrri.org/download/nrri-13-12-lifeline-and-the-states).

13 In 2017, the FCC moved to restrict this additional subsidy to only tribal areas and to limit tribal subsidies to facilities-based carriers. *See 2017 NPRM, supra* n. 2. Both rules were vacated by the D.C. Circuit in *National Lifeline Association, et. al. v. FCC, supra* n. 10, as arbitrary and capricious due to a lack of sufficient evidence to the support the policies. 921 F.3d 1102 (D.C. Cir. 2019).

the Lifeline program with a few states offering additional support and a few imposing additional restrictions on the program.[14]

Practically, mobile wireless services were made broadly eligible for Lifeline in 2005 as a result of the *Tracfone Forbearance Decision*, and then the Program saw significant subscription increases beginning in 2009 after the *Tracfone Petition* allowing resellers to qualify as competitive eligible telecommunications carriers ("CETCs") and thus receive Lifeline subsidies.[15] After this forbearance decision, a number of MVNOs began offering Lifeline services at no charge to the end user, rapidly increasing participation in the Lifeline program and the share of wireless Lifeline accounts. Wireless subscriptions now account for about 92% of Lifeline's budget, and the free (or very low cost) services offered by wireless resellers account for almost all of wireless Lifeline accounts.[16]

Table 1 summarizes Lifeline disbursements over the past sixteen years including the payout share to CETCs, which are mostly wireless resellers. In recent years, the largest wireless providers (e.g., Verizon, AT&T, and T-Mobile) have substantially scaled back participation in the program.[17] The free or "fully discounted" service of MVNOs (and some Sprint affiliates), rather than a

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[14] Lichtenberg, *supra* n. 12; https://www.usac.org/about/tools/fcc/filings/2012/Q4/LI02%20-%20Low%20Income%20Support%20Available%20by%20State%20-%204Q2012.xlsx.

[15] *In the Matter of TracFone Wireless, Inc.'s Petition for Forbearance from 47 U.S.C. § 214(e)(1)(A) and 47 C.F.R. § 54.201(i); Federal-State Joint Board on Universal Service,* DA 05-441, ORDER, 20 FCC Rcd. 3677 (rel. February 17, 2005) (available at: https://www.fcc.gov/document/tracfone-wireless-inc-petition-forbearancefederal-state-joint-board); *In the Matter of Federal-State Joint Board on Universal Service, TracFone Wireless, Inc. Petition for Designation as an Eligible Telecommunications Carrier in the State of New York, et al.,* FCC 08-100, ORDER, 23 FCC Rcd. 6206 (rel. April 11, 2008) (available at: https://docs.fcc.gov/public/attachments/FCC-08-100A1.pdf).

[16] Competitive Eligible Telecommunications Carriers ("CETC") are mostly wireless providers. Prior to 2009, the CETC share of total payments was less than 10%. Between 2008 and 2009, CETC's share of payments doubled from 18% to 36%, increasing in 2010 to 53%. In 2018, CETC's share was 92%. *Total Support ILEC vs. CETC*, Universal Service Administrative Co., (visited November 1, 2018) (available at: https://www.usac.org/li/about/process-overview/stats/total-support.aspx).

[17] A. Pressman, *Why Telecom Carriers Are Resisting a Program for Low-Income Internet Subsidies*, FORTUNE (December 1, 2016) (available at: http://fortune.com/2016/12/01/fcc-att-verizon-lifeline-broadband); J. Engebretson, *CFO: 'Non-sustainable' T-Mobile Lifeline Business to be Phased Out*, TELECOMPETITOR (June 8, 2017) (available at: https://www.telecompetitor.com/cfo-non-sustainable-t-mobile-lifeline-business-to-be-phased-out).

partially discounted service by major carriers, now dominates the provision of Lifeline service and leverages the $9.25 federal subsidy into a highly-affordable service with a market value well in excess of the subsidy.

Table 1. Lifeline Disbursements

Year	Total ($ Bil.)	CETC Share	Year	Total ($ Bil.)	CETC Share
2017	$1.27	91.7%	2009	$1.01	36.6%
2016	$1.52	91.2%	2008	$0.82	17.9%
2015	$1.51	89.0%	2007	$0.82	14.7%
2014	$1.63	86.3%	2006	$0.81	12.4%
2013	$1.79	83.5%	2005	$0.80	8.4%
2012	$2.13	78.9%	2004	$0.76	5.2%
2011	$1.64	68.1%	2003	$0.72	3.2%
2010	$1.22	52.5%	2002	$0.68	1.9%

During the last decade, the Lifeline program has undergone a series of modifications and significant reforms.[18] In recent years the Commission has focused on reducing waste, fraud and abuse, reducing duplicates, and other changes that have reduced expenditures on the program.[19] At present, the FCC is considering a number of additional reforms to the Lifeline program, including a cap on the program's budget, eliminating resellers, a maximum discount, among other reforms.[20] The Agency's recent reforms to the Tribal subsidy program, including limits on the receipt of the $25 tribal subsidy and a facilities-based requirement, were vacated by the Court of Appeals for the D.C. Circuit in *National Lifeline Association v. FCC* (2019) due to a lack of supporting evidence for its rules.[21] As a result, in the future the FCC may look to support any decision on its proposed Lifeline reforms with an appeal to quality economic analysis and empirical evidence.

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18 *Lifeline Modernization Order, supra* n.1.

19 A history of FCC orders is available at: https://www.usac.org/li/tools/rules-orders/default.aspx; *see also 2017 NPRM, supra* n. 2.

20 *2017 NPRM, id.*

21 *Supra* n. 10.

A. *Displacement and Reform*

While not challenging the effectiveness of the program at having at least some effect on the adoption of communications services by low-income Americans, some economists have argued that the Lifeline program is inefficient because it fails to grant subsidies only to low-income households whose adoption depends on the subsidy, implying Lifeline subsidies expand adoption at a high cost per new account. Eisenach (2017), for instance, claims that "there is no compelling evidence that Lifeline causes a significant number of people to get communications services they would not already have in the absence of the subsidy."[22] Among others, Mayo, Ukhaneva and Wallsten (2015) concur with that charge, concluding "subsidies flow to many consumers who would have subscribed even without a subsidy."[23] It is hard to criticize the goal of increasing the efficiency of a subsidy program. Yet, whether this call for targeting marginal households is reasonable or even possible depends on a few things, including the purpose of the Lifeline program, administrative realities, and the magnitude of the displacement effect.

First, if the mission of the program is to improve affordability to a *broad audience* of low-income households, as the Commission has observed, then the fact inframarginal consumers use the subsidy is hardly germane. The subsidy avoids, in part, the need for the inframarginal consumer to trade off food for communications services.[24] Second, targeting marginal users is a difficult

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[22] J. Eisenach, *Testimony: Addressing the Risk of Waste, Fraud, and Abuse in the Federal Communications Commission's Lifeline Program*, Senate Committee on Commerce, Science and Transportation (September 6, 2017) (available at: http://www.aei.org/publication/testimony-addressing-the-risk-of-waste-fraud-and-abuse-in-the-federal-communications-commissions-lifeline-program).

[23] J.W. Mayo, O. Ukhaneva, and S. Wallsten, *Toward a More Efficient and Effective Lifeline Program*, GEORGETOWN UNIVERSITY CENTER FOR BUSINESS & PUBLIC POLICY ECONOMIC POLICY VIGNETTE 2015.8.31 (August 2015) (available at: https://preview.tinyurl.com/yc4ka4bu) at p. 2; J. Mayo, O. Ukhaneva, and S. Wallsten, *Toward a More Efficient Lifeline Program*, Comments Submitted *In the Matter of Lifeline and Link-Up Reform and Modernization*, WC DOCKET NO. 11-42 (August 31, 2015) ("Better targeting those who would not have service without a subsidy") (available at: https://techpolicyinstitute.org/wp-content/uploads/2015/08/comments-filed-with-the-federa-2007730.pdf).

[24] A study by the World Bank suggests that the world's poorest are more likely to have access to a mobile phone than to a toilet or clean water. C. Matthews, *The World's Poorest Are More Likely to Have a Cellphone Than a Toilet*, FORTUNE (January 15, 2016) (available at: http://fortune.com/2016/01/15/cellphone-toilet); *Digital Dividends*, *World Development Report*

(Footnote Continued. . . .)

administrative problem for which a cost-benefit tradeoff may be hostile. Third, the *importance* of displacement depends on the *extent* of displacement. To date, the only evidence available regarding this displacement effect is an unpublished graduate student research paper by Olga Ukhaneva (2015).[25] Ukhaneva concludes that seven-of-eight Lifeline accounts, and nineteen-of-twenty wireless Lifeline accounts, are held by inframarginal households. These are very high displacement rates: each Lifeline account displaces 0.875 regular paid accounts and each wireless Lifeline account displaces 0.95 regular paid accounts.

While economists might be rightly concerned by such high displacement rates, I find that the results of Ukhaneva's work are not credible. Her complex econometric model is poorly documented, measures displacement only indirectly, and contains a number of apparent flaws. Perhaps most limiting to the analysis is that nothing in the data indicates whether a household is a Lifeline subscriber. So, at best, the analysis of the Lifeline program is indirect. It is simply not possible for Ukhaneva to measure accurately the flow of households in-and-out of the Lifeline program. Also, Ukhaneva's data (ending in 2010) precedes a number of material reforms (in 2012, 2014, 2015, and 2016)[26] as well as the dramatic shift from wireline to wireless services. At a minimum, an update to the empirical work, ideally using data measuring actual Lifeline activity, is needed to better describe the current environment.

Even if the displacement effects were found to be large, there is also the question of solutions. In its most recent *Notice of Inquiry*, the Commission inquired about how "to better focus Lifeline funds on those households who need it most" and sought comment regarding "changes to the Lifeline program funding paradigm that will help the Lifeline program more efficiently target funds to areas and households most in need of help to obtain digital opportunity."[27] Presumably, those that "need it most" are those with the lowest

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2016, WORLD BANK (2016) (available at: http://documents.worldbank.org/curated/en/896971468194972881/pdf/102725-PUB-Replacement-PUBLIC.pdf).

[25] Ukhaneva, *supra* n. 6; W. Rinehart, *The High Cost to Add Lifeline Wireless Customers*, AMERICAN ACTION FORUM (November 3, 2015) (available at: https://www.americanactionforum.org/research/the-high-cost-to-add-lifeline-wireless-customers).

[26] *See supra* n. 19.

[27] *2017 NPRM, supra* n. 2 at ¶ 131, 120.

incomes and those that may have a relatively low valuation for service due to a lack of knowledge about the value of broadband and low technological skills (also reducing effective demand).[28] These households are those with the lowest effective demand for telecommunications service, which is to say the *marginal* users.

Reforms to the Lifeline program are numerous but those of primary interest for my analysis are reforms aimed at curtailing reseller access to subsidies, scheduled increases in minimum service standards, and curtailing the availability of fully-discounted service by adopting a maximum discount level for Lifeline subscribers.[29] Such reforms are likely to be counterproductive. As discussed below, economic theory suggests that the displacement by inframarginal users of regular paid subscriptions for Lifeline accounts is most problematic under a minimum charge/maximum discount approach to Lifeline. Perhaps surprisingly, the free but more limited service offered by resellers is an incentive-based mechanism that reduces inframarginal displacement. Proposals to limit reseller access to subsidies and to impose a minimum charge or maximum discount works against the FCC's objectives. Also, in the presence of resellers, significant increases to minimum service standards likewise promotes displacement. It appears the Commission is working against itself with these proposed reforms to the Lifeline program.

B. *Economics of Lifeline Subsidies*

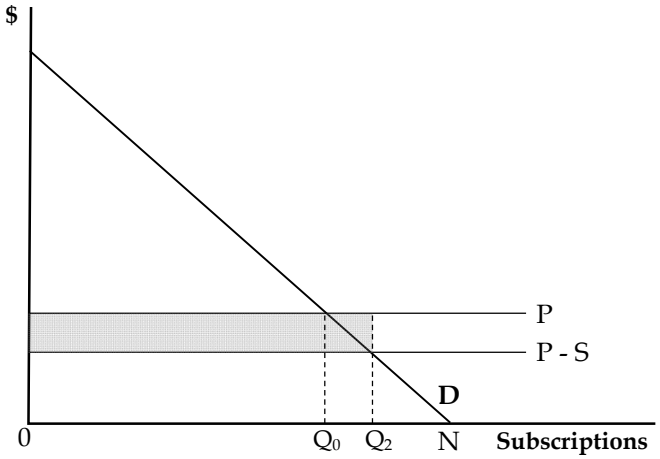
To begin, consider the simple case of a fixed subsidy per Lifeline account that applies broadly to low-income Americans. In Figure 1, low-income users are ranked by their effective willingness-to-pay for mobile wireless services, rendering a demand curve for subscriptions (labeled D). The market price for service is P, so that absent a subsidy the subscription rate of low-income households is Q_0. Now, let there be a subsidy per account equal to S, so the net price faced by low-income households is (P – S). With a subsidy, and assuming all qualifying household receive it, the subscription rate rises to Q_2. The incremental effect on service adoption is $Q_2 - Q_0$, and the total subsidy is the shaded area ($S \cdot Q_2$). The marginal subscribers are those between quantities Q_0 and Q_2; subscriptions up to Q_0 are the inframarginal users that do not require the subsidy to obtain service but receive it. In this setting, most of the subsidy

[28] *Id.* at ¶ 120 ("in particular for low-income Americans who have not adopted broadband").

[29] *Id.* at ¶¶ 30-33.

dollars go to inframarginal subscribers ($S \cdot Q_0$, the shaded area in the figure), which in this figure suggests a very high displacement effect ($= Q_0/Q_2$).

Figure 1. A Simple Subsidy Scheme



Now consider an alternative policy with no subsidy for purchasing the market offering. Define a service with lower quality than the existing market service, where "quality" here refers to a vertical characteristic on which all potential buyers agree (e.g., gigabytes of data).[30] (Here, quantity is defined as the subscription rate to a defined package of communications services. I use the term "lower quality" to reference a package of smaller *quantities* of these services, not a poorer quality or less reliable transmission.) If, for simplicity, a consumer of type θ had a willingness to pay for the market service of θ, and thus obtained surplus of $\theta - P$ from it, then it is natural to imagine that a basic-service subscription, made available at an effective price of zero, would yield surplus $\gamma\theta$, where $1 > \gamma > 0$. w In this simple schema, the parameter γ represents the "quality" of the subsidized service (e.g., the number of minutes, texts, and data allowance), with lower values of γ indicating increasing departures of the basic, free service from the market service.

In this world, willingness to pay by low-income consumers for the subsidized service is given by the schedule labeled *Z* in Figure 2. Low-income households,

[30] The "quality" of the data is the same, but the fully discounted services include limited quantities of services. I use quality so as not to confuse the reader with multiple uses of "quantity."

then, are confronted by a choice: they can buy a market service at a market price P or select a "free" service of quality γ. Which option they select, of course, will reflect a comparison between the net benefits both offer. Hence, a low-income consumer of type θ will opt for the limited free offering if and only if $\theta < P/(1 - \gamma)$; otherwise, the market product is selected.[31]

Figure 2. Limited, Free Service Alternative

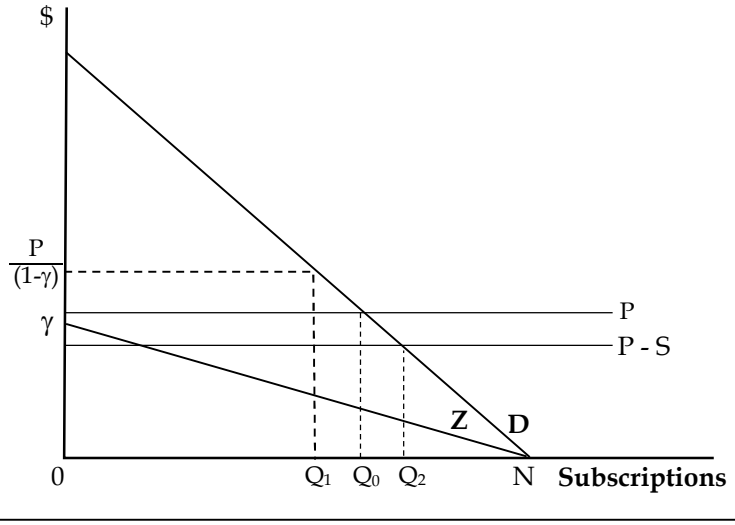


Figure 2 illustrates the separating equilibrium induced by this mechanism.[32] Those consumers with valuations above $P/(1 - \gamma)$ select the market offering. Those with all lower values select the free subscription. The number of subscriptions to the market offering is given as Q_1. Connectivity is complete in this simple example: all poor households have access, although for some that takes the form of the free service (there are $N - Q_1$ such accounts). Also indicated in Figure 2 is a hypothetical uniformly-subsidized market offering which we assume is of identical quality to the market offering corresponding to the

[31] I ignore here the uninteresting complication of a household buying both services, and the generalization that the lower quality service has a low, but positive, price.

[32] On the separating equilibrium *see, e.g.*, M. Mussa and S. Rosen, *Monopoly and Product Quality*, 18 JOURNAL OF ECONOMIC THEORY 301-317 (1978) (available at: http://www.sciencedirect.com/science/article/pii/0022053178900856); T.R. Beard, G.S. Ford, and M. Stern, *Private Solutions to Broadband Adoption: An Economic Analysis*, 69 FEDERAL COMMUNICATIONS LAW JOURNAL 1-23 (2017).

demand schedule D. Thus, as indicated, take-up of service under the uniform subsidy policy is indicated by Q_2.

We are now in a position to analyze the relative merits of a more limited-but-free service program versus a traditional discounted service (the price less the subsidy). To make the comparison meaningful, in both cases we envision a market offering of the same quality. First, the policy of offering a "free" (or fully-discounted) though more limited service is far more likely to achieve high rates of connectivity, as opposed to a subsidized conventional program, unless in the latter case the per subscription subsidy was very large. In contrast, the subsidy implied by the limited free offering is circumscribed by the cost of such a service. This might be relatively low, depending on the service quality offered. For example, numerous MVNOs currently offer free plans which meet FCC standards and include free phones. Evidently, this business plan has been profitable (thus far) where it is offered. This observation highlights the fact that changes in mandated service quality (i.e., minimum service standards), which are a part of some reform proposals, will materially affect the comparison between Lifeline-type policies, as providers alter their offerings based on the subsidy level and minimum service standards. As minimum service standards rise, a fully-discounted offering may no longer be feasible, raising the price of communications services (and/or devices) to low-income Americans participating in the Lifeline program.

Second, the issue of subsidies which end up flowing to households that would buy service even in the absence of assistance is likely to be far more serious under broad, uniform partial subsidies to market offerings, rather than targeted, free-but-limited services. This is because the market separation induced by offering a basic, lower-priced service serves to channel subsidies squarely at the lowest income households who have a lower willingness to pay for quality. In Figure 1, the households receiving a subsidy under the uniform plan (where subscribers receive a grant of S when they buy the market service at P) is composed primarily of consumers who would buy service anyway, even in the absence of subsidy. In fact, Q_0 accounts would buy service even without assistance, yet all of these receive the subsidy. The net increase in subscriptions is $Q_2 - Q_0$. Thus, the proportion of spending under the S subsidy that is "wasted" on inframarginal buyers is (Q_0/Q_2).

The use of separation of consumers by the valuation of service quality, if done correctly, can reduce this transfer to inframarginal households. In Figure 2, Q_1 accounts continue to select the unsubsidized market offering in the presence of the diminished free service. Customers who receive the subsidized service but would buy the service without assistance number $Q_0 - Q_1$. These are households

who have a willingness to pay for the market service near and slightly above its market price.

In fairness, the relevant policy comparison is perhaps between an "optimal" standard subsidy S*, and an optimal free (or reduced price) service of lesser quality γ*, and supply cost. Although Figure 2 is suggestive in this regard, it is also clear that such a calculation requires detailed and reliable information on the distribution of willingness to pay, the nature of market offerings, the relationship between production cost and subscription quality, and so on, and these are not data that are available. But the basic economics of this problem strongly suggest that a "simple" subsidy offered to offset part of the price of a market service is very unlikely to be efficient versus a well-designed alternative that allows for a basic-package service offering at a greatly reduced or zero price, at least when the goals involve expanded connectivity and limiting inframarginal subsidy spending. This is because such schemes fail to make use of an important instrument available to the regulator that induces consumers to sort themselves by income/willingness to pay, while extending connections to even the lowest-income users.

III. Existing Estimates of the Displacement Effect

Whether Lifeline subscriptions displace regular paid accounts is an empirical question, since displacement is certainly a theoretical possibility (as just demonstrated). Also, the analysis above shows that displacement would likely be higher in the era before wireless resellers came to dominate the Lifeline landscape. To date, only one study seeks to quantify displacement—a graduate student paper by Olga Ukhaneva (of Georgetown University) from 2015.[33] This research project employs survey data collected by the National Health Institute that includes questions about household choices of communications services. The data does not, however, provide any indication whether the household has a Lifeline account, forcing an indirect assessment (at best) of the displacement effect. The data span the years 2003 through 2010, and thus precede many of the Commission's reforms (or effects thereof) to the program in recent years and also precede most of the transition to the wireless Lifeline services offered by resellers (see Table 1). Between 2003 and 2008 less than 10% of Lifeline accounts were wireless, and none of these were reseller accounts.[34] The reseller accounts that

[33] Ukhaneva, *supra* n. 6.

[34] Ukhaneva, *id.* at Figure 4.

now dominate the program enters Ukhaneva's data only in years 2009 and 2010, and then in relatively small quantities. Irrespective of the quality of Ukhaneva's empirical analysis (which suffers from a number of problems), theory indicates that an update to the empirics is clearly needed as the reseller service packages and prices are materially different from the Lifeline services of the past. Nothing in Ukhaneva's analysis addresses the introduction of the limited and free services. Before providing such an update to the analysis, I first provide a brief review of the Ukhaneva (2015) study.

A. *The Model*

Ukhaneva attempts to quantify how household decisions about telephone subscriptions are influenced by Lifeline subsidies.[35] Using the estimates from this econometric model, Ukhaneva quantifies the changes in household choices by simulating the elimination of the Lifeline subsidies (or parts thereof). Neither the econometric analysis nor the simulation is properly documented, so a full analysis of Ukhaneva's work is not possible. Nonetheless, my review of the study revealed some rather severe problems with the econometric model and the simulation. Ukhaneva's model is quite complex, so naturally my comments are of a rather technical nature. Consequently, I try to focus on problems of the most severe sort.

Some more technical problems, and those that may simply reflect the study's poor documentation, are not discussed at length. For instance, when employing a two-step estimator, which Ukhaneva does, the covariance matrix of the second stage must be adjusted to account for the inclusion of predicted values from the first stage. The Murphy-Topel Covariance Matrix or Bootstrapping are two such adjustments.[36] Ukhaneva does not indicate how the covariance matrix is computed, though it is common practice to do so as it is consequential. Also, Ukhaneva assumes price has a random coefficient that is distributed normally. This is a poor specification because the normal distribution varies from $-\infty$ to ∞, thus permitting a higher price to increase the probability of subscription. A one-

[35] Ukhaneva, *id.*

[36] A. Petrin and K. Train, *A Control Function Approach to Endogeneity in Consumer Choice Models*, 47 JOURNAL OF MARKETING RESEARCH 3-13 (2010) (draft available at: https://eml.berkeley.edu/~train/petrintrain.pdf).; K. Murphy and R. Topel, *Estimation and Inference in Two Step Econometric Models*, 3 JOURNAL OF BUSINESS AND ECONOMIC STATISTICS 370–379 (1985); P. Karaca-Mandic and K. Train, *Standard Error Correction in Two-Step Estimation with Nested Samples*, 6 ECONOMETRICS JOURNAL 401-407 (2003).

sided distribution might be better. Also, while the price variable has a random coefficient, the subsidy variable does not, but the subsidy is merely a component of price and thus should have a random coefficient as well. Ukhaneva's specification is inconsistent. Moreover, the Lifeline subsidy variable does not vary by alternative, so it should have a coefficient for each outcome, but Ukhaneva's tables show only one coefficient across choices.[37] These specification problems may be fatal to the analysis, but the lack of documentation and explanation precludes any additional commentary.

Ukhaneva employs a dataset on the telecommunications choices of a large sample of U.S. households from which indicators are constructed regarding whether the household has: (1) no service; (2) only wireline service; (3) only wireless service; and (4) both wireless and wireline service. These are simple indicator outcomes and do not include information about the amounts of service used, the plan purchased, or the number of wireline or wireless accounts subscribed to by the household (the latter of which is likely more than a single account). The data includes no indicator for whether a household has a Lifeline account, so it is not possible for Ukhaneva to estimate or simulate the actual flow of households in-and-out of the Lifeline program.

To model the multiple choices, Ukhaneva employs a Mixed Logit Model, a complex but flexible technique for estimating models with multiple dichotomous outcomes.[38] The flexibility comes at the cost of having to make a number of critical decisions regarding which parameters are to be random parameters, what are the distributions of these random parameters, and so forth. Such decisions are not detailed by Ukhaneva. The flexibility and complexity also come at the risk of misuse. As Henser and Greene (2003) observe,

> … the learning curve is steep and the unwary are likely to fall into a chasm if not careful. These chasms are very deep indeed given the complexity of the mixed logit model. Although the theory is relatively clear, estimation and data issues are far from clear. Indeed there is a great deal of potential mis-inference consequent on trying to extract increased behavioural realism from data that

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[37] A.C. Cameron and P.K. Trivedi, MICROECONOMETRICS: METHODS AND APPLICATIONS (2005), at Ch. 15.

[38] *Id.* at p. 500.

> are often not able to comply with the demands of mixed logit models.[39]

Adding to an already complex model, Ukhaneva addresses the potential endogeneity of a few variables using the Control Function method, which differs from the Instrumental Variables ("IV") method by including in the model additional variables intended to break the endogeneity (whereas IV aims to "repair" the endogenous variable).[40] Her treatment of the endogenous variables, as discussed later, is improper.

In the empirical model, household choices are determined by a number of exogenous factors including household income, education level, size, homeownership, population density, the number of cell sites, among other factors. Time fixed effects are also included. The key variables, however, are service prices and subsidy levels. Wireline prices are measured at the county level and based on 2002 data updated by a telephone price index for the additional years.[41] Wireless prices are measured at the state level using data on average revenue per user. This index of price is uniform nationally and varies only by the addition of state and local taxes to the price measure, where the values of these taxes are interpolated for missing years. Ukhaneva does not indicate what price is used for the bundle of wireless and wireline service, though it is likely the sum of the two individual prices (even though consumers are often able to obtain such bundles at discounts).

There are likewise two subsidy variables: (1) the sum of state and federal Lifeline support per line (labeled *LL*) and (2) the *total Lifeline payments* in each state to wireless resellers (labeled *WLI*). These subsidy variables are multiplied by a Lifeline eligibility indicator variable (measured as income below 135% of the poverty-level income), thus the variables only affect the decisions made by eligible households. Note that the two subsidy variables are measured on a different scale since the wireless Lifeline variable equals the per-subscriber subsidy multiplied by the total number of reseller wireless Lifeline accounts (N_{WLI}) in the state (= $LL \cdot N_{WLI}$, a serious problem I'll return to later). In fact, since

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[39] D.A. Hensher and W.H. Greene, *The Mixed Logit Model: The State of Practice*, 30 TRANSPORTATION 133-176 (2003) (draft available at: http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.194.5621).

[40] Petrin and Train, *supra* n. 36.

[41] These updates are likely captured by the year fixed effects also included in the model.

the per-subscriber subsidy is an explanatory variable and is thus held constant, the coefficient on the *WLI* variable is determined by the variability in the count of wireless Lifeline accounts in the state provided by resellers. This specification is ad hoc. What is the theoretical basis for a household's decision depending on the total number of reseller Lifeline accounts in a state? Surely, few if any low-income households are aware of how many Lifeline accounts are in a state, and if so, it is unclear why such knowledge would affect subscription choices. Also, as discussed later, specifying the reseller variable in this way is extremely problematic, since the number of wireless Lifeline accounts is directly related (and a part of) the dependent variable.

B. *Endogeneity*

A Control Function approach is used to account for the potential endogeneity of the prices and the per-line Lifeline subsidy variable *LL* (but not the wireless Lifeline variable, *WLI*). Endogeneity occurs when changes in an unobserved factor alters both the value of the regressor and the value of the dependent variable, thus embedding a correlation between the endogenous regressor and the disturbance term. Solving this problem requires finding a variable that is correlated with the explanatory variable but not the dependent variable (i.e., an instrumental variable), which may be used to purge the disturbance of the correlation. In the case of a Control Function, the residual from a first-stage regression is added to the model to remedy the problem. Finding a good instrument is often a difficult task, and using a poor instrument, as Ukhaneva does, results in low-quality estimates of the relationship of interest, perhaps causing more problems than it may solve.[42]

For the *wireline* price, measured at the county level, Ukhaneva chooses as the instrument the average price in the other counties in the state, a plausibly valid approach that has been used in other research employing the Mixed Logit Model.[43] For the *wireless* price, the instrument is the mobile penetration rate in the state, about which Ukhaneva states, "the mobile penetration rate does not influence telecommunications demand."[44] This is a peculiar statement since the mobile penetration rate is a direct measure of wireless telecommunications

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42 *See*, *e.g.*, W. Greene, ECONOMETRIC ANALYSIS (2018) at pp. 259-261.

43 This approach mimics that of Petrin and Train, *supra* n. 36.

44 Ukhaneva, *supra* n. 6 at p. 13.

demand. In fact, the mobile penetration rate is the sum of the individual demands for telecommunications service, and it is these individual decisions that are the dependent variable of the model. Consequently, this variable is an invalid instrument since the determinants of the mobile penetration rate likewise determine, by definition, a household's choice to obtain wireless service: a choice that appears twice as potential outcome of the model (i.e., wireless only and dual-service households).

The per-line subsidy level is also treated as endogenous. The instrument used for the subsidy level is the percent of families at or below 135% of the poverty level, which Ukhaneva justifies as valid because this "variable does not directly affect the telecommunications demand."[45] Yet, this instrumental variable is based on income and income is plainly a determinant of telecommunications demand. In fact, income is included as an explanatory variable in Ukhaneva's regression, appearing as dummy variables for income levels (including poverty income). Ukhaneva explains that her model shows "the major drivers of telephone demand are found to be price, *income*, age, home ownership, and quality of mobile service" and that the "results indicate that the most price-sensitive groups of consumers are households *below the poverty line*, and with the ratio of the poverty level between one and two."[46] Since income is a determinant of telecommunications demand, the instrument is correlated with the dependent variable rendering it an invalid instrument. This poor specification is especially important since Ukhaneva's simulation on the effects of Lifeline subsidies is (presumably) based on this variable.[47]

Ukhaneva's model also includes as an explanatory variable the total wireless subsidies paid in the state (*WLI*). This variable is equal to subsidy level, itself included as an endogenous variable, multiplied by the number of reseller Lifeline accounts ($WLI = LL \cdot N_{WLI}$). No explanation is provided as to why the per-line subsidy (*LL*) is treated as endogenous but the wireless subsidy variable *WLI*, which includes *LL*, is not. Additionally, the number of wireless reseller Lifeline accounts in the state is a measure of telecommunications demand, and telecommunications demand is the dependent variable of the model. To see this,

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45 Ukhaneva, *id.* at p. 13.

46 Ukhaneva, *id.* at p. 16-7 (emphasis supplied).

47 Again, the analysis is poorly documented making it impossible to ascertain how the simulation is conducted.

let y_R and y_L be the probability the household subscribes, respectively, to a wireless regular paid account and to a wireless Lifeline account. The dependent variable (for the wireless outcome) is $(y_R + y_L)$, which is either 0 or 1. The wireless subsidy variable is the subsidy level multiplied by the sum of the $N_{WLI} = \Sigma y_L$. Consequently, the Lifeline variable is unquestionably endogenous, since y_L appears both in the dependent variable and the regressor. Making matters worse (or perhaps explaining the treatment of the variable as exogenous), there is no possibility of finding a valid instrument for the variable—the dependent variable and the wireless Lifeline variable are inextricably linked through the Lifeline accounts that appear in both.

What are the consequences of ignoring the endogeneity? Since Ukhaneva argues that "states with lower telephone subscriptions might offer higher subsidies,"[48] the covariance between subsidies and telephone subscriptions is negative. Consequently, the coefficient on wireless subsidy payments is biased downward.[49] Ukhaneva's simulation of the effect of the elimination of the wireless subsidy (presumably) sets this variable to zero and recomputes the outcomes, and this downward bias attenuates the effects on the outcomes.[50] That is, the effects of eliminating the wireless subsidies on subscriptions are *too small*, and the subsidy-cost per line and substitution rate, which serve as a focal point of her analysis, *too large*. The simulation results, consequently, are presumably invalid for this reason alone, though the ad hoc inclusion of the *WLI* variable is a serious and likely fatal error.

Though the choice of instruments is critical to the validity of the results, Ukhaneva provides no tests as to the validity of the instruments for either of the endogenous variables.[51] The statistical insignificance of the residual from the first-stage price regression suggests the instruments may be weak. That said, the

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[48] Ukhaneva, *supra* n. 6.

[49] It might be argued that Lifeline accounts are a small share of total accounts and thus the bias is small. This is unhelpful, however, as it is the displacement of regular accounts for Lifeline accounts that the model is attempting to quantify. If there are no (or very few) Lifeline accounts represented in the dependent variable, then the empirical model is fatally defective.

[50] The simulation may also set *LL* equal to zero for the wireless and wireless portion of the dual-service outcomes, but there is so little description of the simulation it is impossible to tell exactly how it was conducted.

[51] The first-stage is a linear model, so all the normal tests for weak instruments and over-identification are available.

instruments for the wireless price and the Lifeline subsidy are plainly invalid on theoretical grounds alone. Not treating the reseller subsidy payments as endogenous is also inconsistent with the treatment of the per-line subsidy and a grave error for that and other reasons. As these subsidy variables are key to the model and the simulation, the poor choice of instruments greatly diminishes the credibility of the model's estimates, the simulations, and, in turn, Ukhaneva's conclusions. Including total reseller subsidy payments as an explanatory variable in a household choice model, whether as an exogenous or endogenous variable, is an ad hoc specification without theoretical justification. In all, I conclude the results Ukhaneva reaches are not credible and are not useful for policymaking.

C. *Simulation Problems*

Citations to the Ukhaneva study focus primarily on the simulated effects on telephone subscriptions from eliminating Lifeline subsidies. These simulations are based on the econometric model, so are no more credible than the model itself. Aside from that issue, there are some troubling features of the simulation.

Consider, for instance, Ukhaneva's attempt to simulate the effect of eliminating subsidies to wireless resellers. Ukhaneva claims that eliminating the wireless reseller subsidy reduces the number of wireless-only households by 1.75 million. However, most of these losses merely shift to the other outcomes: wireline-only households are claimed to increase by 616,658 and households using both wireline and wireless service increase by 988,944 households. In all, eliminating the subsidy program only causes 147,034 households to lose telephone service. At 2010 subsidy levels, Ukhaneva concludes that the average cost per new connected household of $2,835 per year. At a per-household subsidy rate of $136 per year, Ukhaneva states that "only one out of twenty households is a marginal subscriber," implying a displacement rate of 0.95.[52]

In explaining how the simulation is conducted, Ukhaneva states "I conducted a policy experiment."[53] That is all the detail that is provided, which is a problem since it is not obvious how one might go about properly doing such a simulation. I suspect, but could not verify, that for the wireless reseller simulation Ukhaneva

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52 Ukhaneva, *supra* n. 6 at p. 18.

53 Ukhaneva, *id.* at p. 17.

simply sets the *WLI* variable equal to zero and recomputes the outcomes.[54] This approach would not render a valid simulation of the effects of the reseller subsidies. Recall (from above) that the outcomes are functions of both the subsidy level (*LL*) and the total wireless reseller subsidy ($WLI = LL*\Sigma y_L$). Setting *WLI* equal to zero does not eliminate the influence of *LL* on the household choices regarding wireless service. It is not clear how one would restrict the influence of *LL* to wireless reseller accounts since Ukhaneva's data has no information on whether households use Lifeline at all much less obtain Lifeline service from a reseller.[55] Including the *WLI* variable offers no solution. In fact, the wireless subsidy variable (*WLI*) has no legitimate place in the model. Household choices are not sensibly influenced by the total number of wireless reseller Lifeline accounts in a state (and with *LL* held constant, that is what the *WLI* variable is measuring); the estimated coefficients on that variable are almost surely measuring spurious relationships.[56] The per-line subsidy variable (*LL*) is sufficient to capture the effect of the subsidy, at least to the extent such effects can be quantified with the data available.[57] In my opinion, the *WLI* variable is included in the model not for any valid, theoretical reason, but rather to create an opportunity to conduct the simulation to see the effect of reseller subsidies. The arbitrary inclusion of variables in a model to satisfy the objectives of the researcher is not a valid empirical method, since the estimated coefficients are certain to be spurious.

Another problem with the model's specification of wireless subsidies impacting the simulation is that Ukhaneva does not account for the fact that wireless Lifeline resellers often offer a service for free. The price faced by eligible households is not the retail price minus the subsidy (as she models it); the price is zero. A simulation of the elimination of wireless resellers would require the price variable to be zero in the estimation model and then set equal to the market price in the simulation. Yet, nothing in the model accounts for the unique (and

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54 Email inquiries to the author of the study were not returned.

55 Perhaps the *LL* variable could be set to zero for all wireless options, since the bulk of wireless Lifeline is reseller accounts.

56 The mathematical link of the *WLI* variable to the wireless choices may the cause of such spurious relationships.

57 The *LL* variable could be set to zero for the wireless options, but I believe that would require a reformulation of the model where the price variable measures net price (price less the subsidy level multiplied by eligibility).

now dominant) feature of the Lifeline program, though the data used in the study mostly precede the availability of the highly-discounted services.

Irrespective of the validity of the results, it makes sense to contemplate the cost-benefit tradeoff from eliminating the reseller subsidy. First, say that the service offered by resellers is free, which it almost always is (and often includes a device, saving the household maybe hundreds more). When the reseller subsidy is eliminated, say that the households with such accounts shift to a partially-discounted Lifeline account, so the government must continue to incur the average annual cost of $136 per Lifeline household. The reduction in subscriptions save the government about $20 million annually in subsidies [= 147,043 × 136]. According to Ukhaneva's simulation, eliminating the reseller subsidy moves 616,658 of these reseller households to a wireline Lifeline service with a price of $201.72 annually. These low-income households pay $40.5 million more in telecommunications expenses [= 616,658 × (201.72 - 136)]. Also, the simulation predicts that 988,944 households of the reseller households now choose to purchase both wireless and wireline services with a market price of $904 annually. Without the reseller subsidies, low-income households now incur an additional expense of $760 million annually [= 988,944 × (904 – 136)]. So, each year, eliminating the subsidy program saves the government $20 million but costs low-income Americans about $800 million. Plainly, the cost-benefit tradeoff, at least in these respects, is not favorable.

There are other features of the simulation results that are confounding. First, Ukhaneva describes the estimates of her model as indicating that the "wireless Lifeline initiative increases the household propensity to subscribe to landline service," since the wireless subsidy variable has a positive coefficient on the wireline-only outcome.[58] Ukhaneva (2015), apparently puzzled by the result, explains it by claiming "eligible households started enrolling not only in wireless Lifeline, but also in Lifeline for wireline service."[59] Yet, since 2011, the Commission has clarified that its rules allow for only one Lifeline account per household, so this explanation is invalid (at least for modern times). Second, Ukhaneva (2015) states that the "the two services are substitutes," but this assertion seems to contradict the results of her model—in the model the

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[58] Ukhaneva, *supra* n. 6 at p. 17. Note that the interpretation of the coefficients is more complex in the Mixed Logit Model than in more traditional regression methods.

[59] Ukhaneva, *id.* at p. 17.

subsidies, which lower the cost of wireless Lifeline service, increase wireline demand.[60] The two services are thus complements by this model.

Finally, Ukhaneva's simulation implausibly suggests that households respond to a nearly $12 increase in the wireless price (a 20% price increase) by subscribing both to the higher priced wireless service and also incurring the costs of a wireline subscription. (In fact, the price increase faced by consumers from the elimination of wireless resellers is the full price of wireless service since the resellers typically offer service at little-to-no charge to the consumer.) Buying more services at a much higher price is a peculiar result that demands a theoretical explanation. There appears to be something askew with the simulation, though the lack of detail makes determining the exact cause impossible.

IV. Quantifying Lifeline's Displacement Effect

Considering the numerous problems with Ukhaneva's analysis and the time period covered by her data, it is reasonable to conclude the study offers neither credible nor policy-relevant estimates of a displacement rate, especially in the modern Lifeline environment. Displacement may, however, be estimated in a more direct manner. In this section, I offer an answer to the empirical question about the extent to which wireless Lifeline subscriptions displace regular paid mobile wireless accounts. Wireless Lifeline accounts now dominate the subsidy program and wireless service often or typically includes a broadband service, which fixed line telephone services do not, so I limit my attention to wireless accounts. If Lifeline accounts displace regular accounts, then there should be a quantifiable relationship between the two types of accounts over time. With panel data on the two types of accounts, the displacement rate may be estimated directly, a task to which I now turn.

A. *Data*

Data on total mobile subscriptions for each of the 48 contiguous states is obtained from the FCC's *Voice Telephone Services Report*.[61] Biannual, state-level

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[60] Ukhaneva, *id*. Two goods are complements (substitutes) when a price reduction for one good increases (reduces) the demand for the other.

[61] VOICE TELEPHONE SERVICES: STATUS AS OF JUNE 30, 2017, Industry Analysis and Technology Division, Wireline Competition Bureau, Federal Communications Commission, November 2018 (rel. February 2018) (available at: https://www.fcc.gov/voice-telephone-services-report).

data (June, December) for mobile subscriptions span the period June-2012 through June-2017 (the last period available), for a total of eleven periods and 528 total observations, or 480 observations of first-differenced data. State-level wireless Lifeline accounts are obtained for the same periods.[62] Quarterly data on population and per-capita personal income data are obtained from the Bureau of Economic Analysis ("BEA"), and the second and fourth quarters are used to match the subscription data.[63] Since the data includes observations from multiple states of varying sizes, all the data is expressed in per-capita terms.

B. *Estimation Strategy*

If Lifeline accounts displace regular accounts, especially at the very high rates suggested by Ukhaneva, then we would expect to see a relationship over time between changes in Lifeline accounts and changes in regular paid subscriptions. This relationship forms the basis of my empirical strategy.

More formally, let R_t be the number of regular paid subscriptions (per capita) and L_t be the number of Lifeline accounts (per capita) at time t. If Lifeline accounts displace regular accounts, then a larger number of Lifeline accounts should be associated with fewer regular paid accounts. To obtain an estimate of the displacement rate with panel data, I first estimate a two-way fixed effects regression,

$$R_{it} = \lambda L_{it} + \mu_i + \theta_t + u_{it} \quad , \tag{1}$$

where λ is the displacement rate between Lifeline and regular accounts, μ_i is a permanent but unobservable state-specific effect, θ_t is a time effect common to all states (time demeaning), and u_{it} is an econometric disturbance term.[64] In this model, *full displacement* of Lifeline accounts for regular subscriptions is indicated by $\lambda = -1$; that is, a one-unit increase in Lifeline accounts reduces regular paid accounts also by one unit. Ukhaneva's conclusions suggest a displacement rate very close to -1 (i.e., -0.95 for the wireless reseller Lifeline program). In the opposing case of *no displacement*, λ equals zero ($\lambda = 0$). Intermediate values of λ

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62 These data were obtained from CGM Consulting under a non-disclosure agreement.

63 https://www.bea.gov/data/by-place-us.

64 J.M. Wooldridge, ECONOMETRIC ANALYSIS OF CROSS SECTION AND PANEL DATA (2002) at Ch. 10.

($-1 < \lambda < 0$) indicate *partial displacement*. It may also be that $\lambda > 1$, which suggests that Lifeline accounts actually spur additional regular accounts, which may occur through an experience effect within low-income households.[65] Hypothesis tests may be straightforwardly applied to the λ coefficient. Additional time-varying variables may be added to the model and I include in some models per-capita income (with coefficient β), a change that does not alter the interpretation of λ.

An alternative way to approach the panel data is to eliminate the state fixed effects (μ_i) by first differencing the data (where Δ is the first difference operator).[66] The model is now,

$$\Delta R_{it} = \lambda \Delta L_{it} + \dot{\theta}_t + \Delta u_{it} \quad , \tag{2}$$

where the interpretation of λ is the same. The time fixed effects now measure something different, but the model still includes dummy variables for each year. Due to first differencing the data, the first observation is lost. An alternative to first differencing is forward orthogonal differencing, which subtracts from the level in period *t* the average of all remaining future periods.[67] In contrast to first differencing, forward differencing requires the deletion of the final period. I explore both differencing options.

The fixed effects and differenced equations will produce the same estimate of λ only when there are two periods of data. With a longer time-series component, differences in the coefficient estimates between the two approaches depends on a number of factors, but relative efficiency hinges on whether ε_{it} is serially correlated.[68] Wooldridge's test for serial correlation in panel data rejects the null hypothesis of no first-order serial correlation, so the fixed-effects estimates are

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[65] Experiencing the benefits of connectivity may spur other members of the households to obtain a regular paid subscription.

[66] Wooldridge, *supra* n. 64 at p. 279-285.

[67] M. Arellano and O. Bover, *Another Look at the Instrumental Variable Estimation of Error-Components Models*, 68 JOURNAL OF ECONOMETRICS 29-51 (1995) (available at: https://www.cemfi.es/~arellano/arellano-bover-1995.pdf).

[68] Wooldridge, *supra* n. 64 at p. 284.

expected to be more efficient.[69] In any case, clustered standard errors are used in all cases thus accounting for both heteroskedasticity and serial correlation.

It may be that Lifeline and regular paid accounts have some equilibrium relationship over time are thus jointly determined (R and L are endogenous).[70] With panel data, the fixed effects and differenced estimators correct for endogeneity as long as the source of it is time invariant, since endogeneity is basically an omitted variables problem. Endogeneity will present itself as a correlation between L_{it} and u_{it}, and Wooldridge proposes a simple test of strict exogeneity that augments the regression models with a forward lag of L_{it}.[71] The test is conducted and reported, and the null hypothesis of strict exogeneity is never rejected at anywhere near traditional levels.

[69] Based on Equation (1), the F-statistic is 161.03 with a probability level less than 1%.

[70] Estimating a cointegrating relationship requires a lengthy time-series component to the data, which I do not have. As such, I must focus on short-run effects. W. Enders, APPLIED TIME SERIES ECONOMETRICS (2004), at Ch. 6; P. Pedroni, *Panel Cointegration: Asymptotic and Finite Sample Properties of Pooled Time Series Tests with an Application to the PPP Hypothesis*, 20 ECONOMETRIC THEORY 597-625 (2004) (showing poor performance at even 20 time periods).

[71] Wooldridge, *supra* n. 64 at p. 284-5.

Table 2. Results

	Fixed Effects		First Differences		Forward Differences	
	A	B	C	D	E	F
	Coef (t-stat)	Coef (t-stat)	Coef (t-stat)	Coef (t-stat)	Coef (t-stat)	Coef (t-stat)
λ	0.074 (0.77)	0.029 (0.36)	-0.0003 (-0.00)	-0.006 (-0.06)	0.074 (0.79)	0.044 (0.50)
β	…	0.005*** (3.27)	…	0.003*** (2.83)	…	0.005*** (3.10)
Time FE (μ)	Yes	Yes	Yes	Yes	Yes	Yes
State FE (θ)	Yes	Yes	No	No	No	No
Test: λ = -1	123.2***	165.9***	132.5***	127.9***	129.8***	134.0***
Test: λ = 0	0.59	0.13	0.00	0.00	0.62	0.25
Exogeneity (*t*)	-0.35	-0.42	0.72	0.64	-0.58	-0.69
R^2	0.23	0.53	0.19	0.20	0.53	0.57
Obs.	528	528	480	480	480	480

Significance: *** (1%); ** (5%); * (10%)

The estimated displacement rates from Equations (1) and (2) are summarized in Table 2. Equation (1) results are provided in Columns A and B where Columns C through E are the results for the differenced data of Equation (2). State-level clustered t-statistics are likewise provided, addressing both heteroskedasticity and serial correlation. Statistical testing indicates the state and time fixed effects are not redundant in the fixed effects estimates, and the time fixed effects are not redundant for the differenced models. For all models, the null hypothesis of strict exogeneity (a t-test) is not rejected.

Turning to the displacement rate, regardless of specification the displacement coefficient λ is near zero. The t-statistic on the coefficient is a direct test of the null hypothesis that the displacement rate is zero, and the null hypothesis is not rejected for any specification; the t-statistics are all quite small. An additional test of the null hypothesis of *no displacement* (λ = 0) is also provided (a χ^2 test) and cannot be rejected, though this test offers nothing over the traditional t-test. Alternately, the null hypothesis of full displacement (λ = -1) is rejected at better than the 1% level in all specifications. Per-capita income, when included, is statistically different from zero (the β coefficients), suggesting the results from Columns B, D and E are best. The positive sign on the income variable is as expected.

The displacement rate is an estimate and estimates have distributions. From Column D, we see that the point estimate of the displacement rate λ is -0.006, indicating almost no displacement (about 1 regular account lost for 167 Lifeline accounts). The upper and lower limits of the 95% confidence interval of the

displacement rate are -0.17 and 0.17. I can say with 95% confidence, therefore, that at most one regular paid account is lost for every six Lifeline accounts, or, at the other extreme, every additional six Lifeline account results in an increase in regular paid account by one unit (a promotion effect). The displacement effect, to the extent there is one, is very small, and the null hypothesis of no displacement cannot be rejected.

C. *Robustness*

Robustness may be evaluated in a few ways. The model assumes a homogenous relationship between regular and Lifeline accounts across the states, so this reasonableness of this assumption is worth evaluating and I do so in three ways. This analysis also considers the possibility of the undue influence of states and observations on the estimated displacement rates. Furthermore, I consider the possibility that endogeneity arises from time-variant factors and estimate Equation (2) by Instrumental Variables. The results reported in Table 2 are found to be robust to modifications.

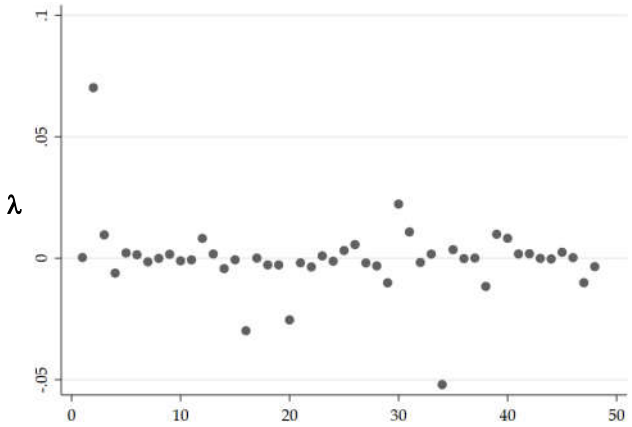
1. *Homogeneity and Influential Observations*

First, I estimate the random-coefficients linear regression model of Swamy (1970).[72] The test statistics under the null hypothesis of homogeneity of the λ coefficients is 87.42 (Prob = 0.67), so the null hypothesis of homogeneous relationships cannot be rejected.

Second, I evaluate whether any one state has an undue influence on the results. Each state is excluded from the sample and Equation (2) is re-estimated in first differences with the income variable included. The estimated displacement rate ranges from -0.052 to 0.070 and has a 95% confidence interval bound by -0.025 and 0.011, indicating no particular state is driving the results. Figure 3 shows the estimated displacement rate. There are two states that have a relatively large influence on the estimated displacement rate: Arkansas and Oklahoma, the latter of which has a large tribal population. Excluding these two states from the sample produces a displacement rate of 0.0036 (t-stat = 0.05), matching closely the full sample.

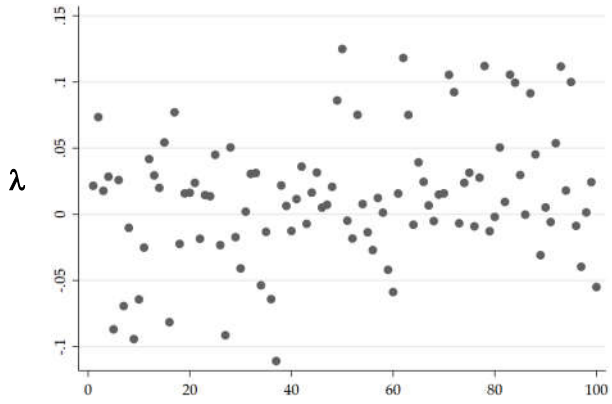
[72] P.A.V. Swamy, *Efficient Inference in a Random Coefficient Regression Model*, 38 ECONOMETRICA 38: 311–323 (1970). The test uses the command -xtrchh- in Stata 15.

Figure 3. State Influence on Displacement Rate



Third, a randomly-selected group of between seven and ten states is excluded from the same and Equation (1) is re-estimated (again without state fixed effects). This procedure is repeated 100 times and the displacement rates are illustrated in Figure 4. The displacement rate (λ) is remarkably stable with a range of -0.118 to 0.119 and a 95% confidence interval bound by -0.080 and 0.103. Again, the results are robust to the inclusion or exclusion of states.

Figure 4. Multiple-State Influence on Displacement



Last, I test for differences in the displacement rate between states where AT&T, Verizon, and T-Mobile offer Lifeline service and those where the three providers do not.[73] The results are summarized in Table 3, Column A. The coefficient δ measures the difference in the displacement rate for the states where the large mobile providers offer service. This coefficient is not statistically different from zero, so the displacement effect is found to be equal across the two groups.[74]

Table 3. Robustness Analysis

	A	B	C
	Coef (t-stat)	Coef (t-stat)	Coef (t-stat)
λ	0.042 (0.73)	0.056 (0.47)	0.047 (0.24)
δ	-0.072 (-0.48)	…	…
Time FE (μ)	Yes	Yes	Yes
Inst. Variables	No	No	Yes
Test: λ = -1	76.62***	63.72***	29.44***
Test: λ = 0	0.12	0.22	0.06
Test: λ+δ = -1	84.64***	…	…
Test: λ+δ = 0	0.08	…	…
K-P rk LM	…	…	12.02***
Hansen J	…	…	1.19
Exog. Test	…	…	2.66
Obs.	480	460	432

Significance: *** (1%); ** (5%); * (10%)

Next, a graphical analysis indicates a few high leverage observations for Arkansas and Louisiana. As shown in Column B of Table 2, excluding those two states from the sample renders a displacement rate of 0.056 (t-stat = 0.47), so these troublesome observations are not very influential.

[73] The states are listed at (visited June 23, 2019): https://www.verizonwireless.com/solutions-and-services/lifeline; https://www.att.com/esupport/article.html#!/wireless/KM1008768; https://www.t-mobile.com/offers/lifeline-program.

[74] The coefficient for non-large provider states is 0.043 (t-statistic = -0.48) and the interaction term is -0.072 with a t-statistic of -0.48.

2. *Endogeneity*

As noted above, it is certainly feasible that R and L are jointly determined. Fixed effects estimation and differencing, both of which I used above, are methods that address endogeneity when the source of the endogeneity is time invariant. In order to evaluate the possibility that endogeneity arises from time-varying factors, I estimate the regression model using IV. Note, however, that the IV estimator is inherently biased and less precise than the OLS estimator, especially in small samples. Thus, I test for endogeneity to determine whether an IV estimator is worth embracing. If statistical testing indicates that ΔL is sensibly treated as exogenous, then the results from Table 2 are presumably valid and better estimates of the displacement rate. Given the time series component of the data, I use lagged values of the Lifeline accounts as instruments and estimate Equation (2) in first differences.[75]

Results are summarized in Table 3, Column C. The point estimate of the displacement rate is 0.047, again quite close to zero. The coefficient is not statistically different from zero at anywhere near traditional levels, so the null hypothesis of a displacement rate of zero cannot be rejected. Alternately, the null hypothesis that the displacement rate equals -1 is rejected at better than the 1% level. The positive coefficient indicates that there is *an additional regular paid account* for every twenty Lifeline accounts, so Lifeline accounts actually enhance the number of regular paid subscriptions (though, statistically, there is no relationship between the two).

A battery of tests is conducted to evaluate the suitability of the instruments.[76] First, using the Lagrange-Multiplier Test, I can reject the null hypothesis that the model is under-identified at better than the 5% level, indicating that the instruments are relevant. Second, the Kleibergen-Paap *rk* Wald test rejects the null hypothesis of a weak instrument even for a 10% size distortion.[77] Third,

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[75] Wooldridge, *supra* n. 64 at pp. 305-307. Instruments include the lag of ΔL and a two-period lag of L (since the one-period lag is a component of ΔL).

[76] These tests are conducted using the -ivreg2- command in Stata 15.

[77] F. Kleibergen and R. Paap, *Generalized Reduced Rank Tests Using the Singular Value Decomposition*, 133 JOURNAL OF ECONOMETRICS 97-126 (2006); J. H. Stock and M. Yogo, *Testing for Weak Instruments in Linear IV Regression*, in IDENTIFICATION AND INFERENCE FOR ECONOMETRIC MODELS: A FESTSCHRIFT IN HONOR OF THOMAS J. ROTHENBERG (D. W. K. Andrews and J. H. Stock, eds.) (2005); C.F. Baum, M.E. Schaffer, and S. Stillman, *Enhanced Routings for Instrumental*

(Footnote Continued. . . .)

even assuming weak instruments, the null hypothesis that the displacement rate equals zero cannot be rejected by the Anderson-Rubin test.[78] Finally, the null hypothesis that ΔL is exogenous is not rejected even at the 10% level. The OLS results from Table 2, therefore, are valid and preferred to an IV estimation approach, though the results are highly comparable.

D. *Panel Vector Autoregression*

With the panel data on subscriptions and the potential for endogeneity of R and L, a sensible alternative estimation approach is Panel Vector Autoregression ("PVAR"), which extends the traditional VAR approach to panel data.[79] With PVAR, both regular and Lifeline accounts are treated as endogenous. I note, however, that while my data includes eleven biannual observations for each state, the data only spans five years. Ideally, more time series observations would be available for estimating the PVAR. After estimating the dynamic relationship between the two types of accounts using PVAR, the relationship between the two account types can be observed by simulating a shock to L (actually, to its disturbance term) to see how R responds over time. Such a response is referred to as the Impulse Response Function ("IRF").

The PVAR model is summarized as:

$$Z_{it} = A(G)Z_{it-1} + \mu_i + \theta_t + \varepsilon_{it} \quad , \tag{3}$$

where Z_{it} is a matrix of endogenous variables, $A(G)$ is a matrix polynomial in the lag operator G, and ε_{it} is the econometric disturbance term. To ensure stationarity and to address the state-level fixed effects, the series are time-demeaned and forward orthogonal differencing is applied (accounting for μ_i and

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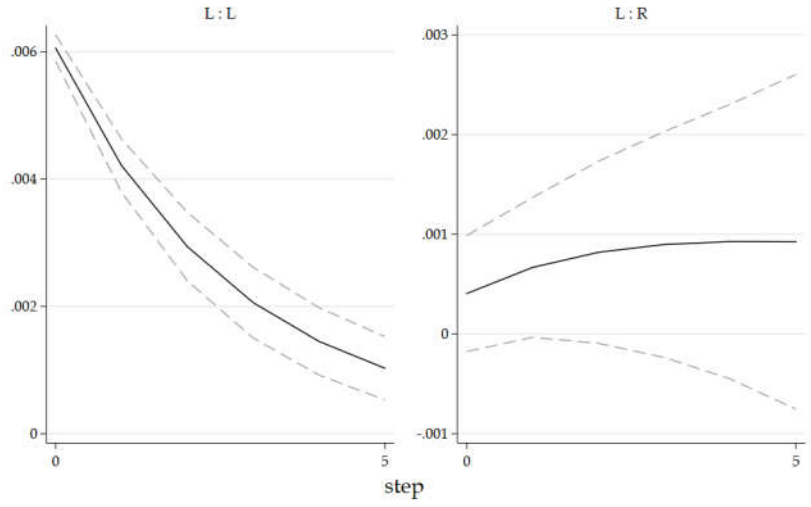
Variables/Generalized Method of Moments Estimation and Testing, 7 STATA JOURNAL 465-506 (2007) (available at: https://ageconsearch.umn.edu/record/119291/files/sjart_st0030_3.pdf).

[78] T.W. Anderson and H. Rubin, *Estimators of the Parameters of a Single Equation in a Complete Set of Stochastic Equations*, 21 ANNALS OF MATHEMATICAL STATISTICS 570-582 (1949).

[79] M. Abrigo and I. Love, *Estimation of Panel Vector Autoregression in Stata*, 16 THE STATA JOURNAL 1-27 (2016); F. Canova and M. Ciccarelli, *Panel Vector Autoregressive Models: A Survey*, EUROPEAN CENTRAL BANK WORKING PAPER SERIES No. 1507 (January 2013) (available at: https://www.ecb.europa.eu/pub/pdf/scpwps/ecbwp1507.pdf).

θ_t).[80] The Bayesian, Akaike and Hannan-Quinn Information Criteria indicates a single lag is best. PVAR is estimated by Generalized Method of Moments ("GMM") and for the instruments three lags are used.[81] Analysis of the model provides encouraging results: Hansen's *J*-Test (an over-identification test for the instruments) has a probability of 0.25 and an analysis of the eigenvalues indicates the PVAR satisfies the stability condition.

Figure 5. Orthogonalized Impulse Response Functions
(One Standard Deviation Confidence Bands)



[80] I. Love and L. Ziccino, *Financial Development and Dynamic Investment Behavior: Evidence from Panel VAR*, 46 QUARTERLY REVIEW OF ECONOMICS AND FINANCE 190-210 (2006). Hayakawa shows that forward differencing tends to work better than first differences in PVAR models. K. Hayakawa, *First Difference or Forward Orthogonal Deviation- Which Transformation Should be Used in Dynamic Panel Data Models?: A Simulation Study*, 29 ECONOMICS BULLETIN 2008-2017 (2009). Applying Pesaran's second-generation panel unit root test that allows for cross-sectional dependence, I am able to reject the null hypothesis that both series are non-stationary in the differences (at the 1% level or better). M.H. Pesaran, *A Simple Panel Unit Root Test in the Presence of Cross-Section Dependence*, 22 JOURNAL OF APPLIED ECONOMETRICS 265-312 (2017) (available at: https://onlinelibrary.wiley.com/doi/full/10.1002/jae.951). I use the -pescadf- command in Stata developed by Piotr Lewandowski. A single lag minimizes the Bayesian, Akaike, and Hannan-Quinn Information Criteria, so that lag structure is used.

[81] D. Holtz-Eakin, W. Newey, and H. S. Rosen, *Estimating Vector Autoregressions with Panel Data*, 56 ECONOMETRICA 1371-1395 (1988).

The Orthogonal Impulse Response Functions ("OIRF") of the response of Lifeline and regular accounts to a shock to Lifeline accounts is provided in Figure 5. Confidence bands are one-standard deviation bands based on 200 simulations. Standard errors are clustered at the state level. As expected, a positive (one standard deviation) shock to Lifeline accounts increases Lifeline accounts—an effect that decays over time. However, the positive shock to Lifeline accounts has no statistically-significant effect on regular paid subscriptions; though the point estimate indicates a slight positive effect, the one standard deviation confidence band includes zero in all periods. Point estimates indicate that there is an *additional* regular paid account for every ten Lifeline accounts (the effect is complementary), a result smaller than found above when accounting for simultaneity. Still, with zero in the confidence interval, the evidence suggests there is no meaningful relationship between Lifeline and regular accounts.

As with the OLS estimation strategy, we find no evidence to support a displacement effect of Lifeline accounts for regular paid subscriptions using PVAR. If anything, the results indicate a mild positive relationship between the two, suggesting Lifeline accounts may actually stimulate the number of regular paid accounts. This result is sensible in the presence of network effects within a household or family; that is, after one member of the household experienced mobile wireless, other members of that household may decide mobile wireless service is worth the market price.

V. Conclusion

Underlying numerous of the proposed reforms to the Lifeline program is the concern that many, if not most, low-income households receiving the Lifeline subsidy would subscribe to wireless service without the subsidy. The Commission has sought comments on way to better target the Lifeline subsidies to households that would not otherwise purchase service at market prices. Attention to the use of Lifeline by inframarginal consumers hinges critically on a graduate student research paper that concludes that only one-in-eight Lifeline households requires the subsidy to obtain service. As detailed here, that study is poorly documented and suffers from a number of serious errors and irregularities.

In this POLICY PAPER, I provide new empirical evidence on the displacement rate of regular for Lifeline accounts. Using data on regular paid subscriptions and Lifeline accounts and a variety of empirical methods, I find no evidence of such displacement. I also demonstrate theoretically why this may be so, showing that the free/fully discounted or very-low-cost/partially discounted offerings of

wireless resellers diminish the displacement rate through a separating equilibrium. This analysis also suggests that some proposed reforms, which are aimed largely at reducing reseller participation in the program and increasing minimum service standards, are counterproductive. The "free but limited" reseller offerings attenuate displacement, not promote it, and increases in minimum service standards encourage displacement, not reduce it. Also, raising minimum service standards is likely to raise the price for Lifeline services, thereby making service unaffordable to precisely those households the Commission seeks to target with its subsidies.

Targeting waste, fraud and abuse is a sensible policy goal, and the Commission is right to focus on doing so. Reforms aimed at reducing reseller involvement, aside from those proven guilty of fraud, is found to have no theoretical or empirical support. Abuse by unscrupulous parties operating as resellers may sensibly be addressed using means that target proven bad actors but otherwise maintain the services offered by the resellers that now account for the bulk of Lifeline subscriptions, especially as the larger facilities-based providers abandon the field.