REPORT SERIES

WINTER 2012

The Dynamic Effects of Triple Play Bundling in Telecommunications

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Foreword

By Fernando R. Laguarda, Time Warner Cable

When we launched the Time Warner Cable Research Program on Digital Communications, we hoped to fund scholarship on topics that reflected the pace of innovation in the dynamic communications marketplace. But innovation in digital communications isn't just about technological advancements. Innovative business models, including strategies for packaging and promoting product offerings, have also had a significant impact. The "triple play" bundle from broadband providers such as cable operators and telephone companies, which combines voice, video and high-speed Internet access for a single price, is one prominent example of a successful strategy. In essence, bundles are discount vehicles, and they have proliferated in the digital communications marketplace as a result of increased competition among providers.

First launched by cable operators beginning to offer voice and broadband services, the triple play bundle has become ubiquitous in the marketplace due to competition among multiple platform providers. Bundling helped cable operators enter the voice marketplace and compete with incumbent phone companies, adding millions of new customers and realizing the key telecommunications policy goal of robust, facilities-based competition.

In this research paper, Professor Jeffrey Prince takes a look at the "triple play" and its implications for customers and competitors. The report provides a broad picture of the landscape and recent evolution of triple play bundling and its interaction with demand for telecommunications services. In doing so, he proposes a novel, dynamic effect of bundling on service demand. From an industry perspective, bundling is a logical way to obtain new customers. From his perspective, bundling is more about keeping existing customers (reducing "churn"). Either way, understanding more about how consumers value product bundles will help stakeholders do a better job when it comes to formulating the right sort of policy for this marketplace.

Professor Prince raises interesting policy questions about bundling in technology markets and uses innovative econometric techniques in the process. Consumers clearly benefit from expanded output and more choice in the communications marketplace. At the same time, policy stakeholders question the role of different business strategies and their impact on different dimensions of competition and, ultimately, on consumer welfare. This paper makes a contribution to the debate through its careful use of data to measure consumer behavior in response to the widespread use of bundling in the marketplace. It is just the sort of thoughtful contribution we wanted to make when we launched the Research Program on Digital Communications.

We hope this report stimulates debate and encourages a more thoughtful policy debate. As always, we look forward to your comments and feedback.

I. Introduction

When one firm sells two or more separate products in a package for a single price, it is engaged in "bundling." Bundling is a pervasive practice in many markets, including software, insurance, and telecommunications services. In this research project, we study bundling of landline digital telephone, multichannel video programming and high-speed broadband by cable operators, often called "triple play." Virtually every broadband service provider in the United States offers triple play (including cable operators, telephone companies, and broadband overbuilders), and many households subscribe to such packages.

The emergence of triple play is a sign of technological innovation and convergence[1] in telecommunications. The rapid adoption of triple play by households over the past few years makes it a compelling topic of study for a wide audience, including telecommunications firms, the Federal Communications Commission, and American consumers in general. It also raises several interesting questions, including:

- Who subscribes to triple play?
- Why might a firm offer a discounted triple play bundle?
- What is the competitive impact of triple play bundling?
- Does triple play bundling affect long-run consumer behavior?

The research presented here seeks to address all of the above questions, with particular emphasis on the last. Specifically, we examine whether bundles reduce subscriber churn between 2007 and 2009, when offering triple play became widespread at virtually every large cable multi-system operator ("MSO") in the United States. We define churn as the abandonment of a service or service provider by an existing user, a household. If bundling reduces churn, then it causes households to be less likely to switch services (and service providers) once a bundle is purchased. This effect, if it exists, is likely due to changes in households' payoffs generated from bundling via, e.g., increased convenience from single billing or monetary and/or non-monetary costs of dismantling a bundle.

We measure churn for wired telephone, pay television (cable or satellite), and broadband internet, as well as provision of these services by cable MSOs, employing a rich consumer marketing dataset provided by Forrester Research. While bundling was offered by firms other than cable MSOs, our firm-level churn analysis focuses on this group due to its relatively large presence in our dataset with respect to bundling. This allows for much more precise estimates relative to any other bundle provider in our data.

A recent survey by the Federal Communications Commission (FCC) serves to highlight the likely relationship between bundling and churn. In the survey, nearly 40% of respondents noted that having to change their bundle was a major reason for keeping their service. That statistic suggests bundling reduces churn by altering the payoffs from switching (i.e., state dependence), but it may

This paper was written with financial support from the Time Warner Cable Research Program on Digital Communications. The views expressed are those of the author(s) and not necessarily those of Time Warner Cable or the Time Warner Cable Research Program on Digital Communications

also screen inert consumers (unobserved heterogeneity), identifying consumers who are inherently least likely to switch to another supplier. If this is occurring, bundling does not cause churn reduction, but rather identifies households less likely to churn. This is a key issue for our empirical analysis, which examines a much larger and richer dataset than the FCC survey.

Overall, the contribution of this study is to provide a broad picture of the landscape and recent evolution of triple play bundling, and its interaction with demand for telecommunications services. In doing so, we propose and examine a novel, dynamic effect of bundling on telecommunications service demand — churn reduction — and we employ state-of-the-art econometric techniques to measure such an effect using repeated cross-sectional data. While we are able to identify the effects of bundling, we do not specifically address *why* bundling occurs in this market (e.g., are firms intentionally reducing churn via bundling or simply using bundles as a competitive response?). However, our ability to establish a bundling effect per se is an important step toward improved understanding of this market's competitive environment and consumer behavior.

In Section II, we discuss different types of bundling and classify triple play bundling in this context. We then examine incentives for firms to offer bundles and incentives for consumers to purchase them. Next, we discuss the recent emergence of the triple play bundle in telecommunications. Then, we provide intuition and a formal model of how bundling can impact consumers' service purchasing decisions over time. Lastly, we highlight the implications of bundling having a dynamic effect on service demand.

In Section III, we provide preliminary data analysis. We begin by describing the source and contents of the data we use for this study. We then break down how key household demographics correlate with telecommunication service demand and triple play bundle demand. Finally, we provide a simple empirical analysis of demand over time for triple play services individually (both in general and from cable MSOs), and for the triple play bundle itself.

In Section IV, we present and describe our empirical model. We then discuss the key methods and assumptions we utilize to properly identify dynamic effects of bundling using only repeated cross sections of data. Next, we provide a simple, suggestive test of whether triple play bundling also serves the role of a screen, helping to identify a subset of households less likely to switch services. Finally, we present our main empirical findings concerning the effect of triple play bundling on customer churn for telecommunications services.

II. Bundling in Telecommunication

A. Types of Bundles

As described in Stremersch & Tellis (2002), product and service bundles generally fall into two categories:

- **Pure bundles.** Examples of pure bundling are the bundling of Internet Explorer with Windows (Firefox users still get Internet Explorer on their PCs), exercise equipment in a health club (you get the rowing machine whether you want it or not), as well as pay television channel bundling in tiers of service (*e.g.*, broadcast basic, basic, expanded basic, digital, expanded digital, etc.). For these bundles, consumers are only given the option of buying the bundle, or nothing at all.
- **Mixed bundles.** Mixed bundling gives consumers the option of buying the bundle or any subset of bundle components. For example, combo meals at fast food restaurants are mixed bundles — consumers may buy the bundle (e.g., a hamburger, French fries and soda), or any component of the bundle a la carte (e.g., French fries).

The practice of bundling wired telephone, pay television, and broadband internet is an example of mixed bundling. In addition to purchasing the bundle, consumers have the option of buying any subset of services instead. Further, the observed pricing of the individual services is such that some consumers will make this choice, and cable operators do not require service contract commitments that prevent customers from switching without paying penalties for doing so.

This last feature of triple play bundles is important, as it demonstrates that triple play offerings by cable MSOs are truly mixed bundles, and not pure bundles disguised as mixed bundles. If a la carte prices were so high that no one would choose to purchase a single service, or they came with onerous penalties for switching, then the firms are de facto pure bundling. As noted in Stremersch & Tellis (2002), the practice of mixed bundling is generally not condemned under the antitrust laws. Indeed, there are many reasons why such bundling could be considered pro-competitive and/or output enhancing.

B. Why Bundle as a Firm?

Why would cable MSOs want to offer triple play bundles? There are several standard explanations. First, triple play bundling may be an attempt to extend market power (Whinston, 1990). However, the mixed nature of these bundles mitigates this possibility. Second, triple play bundling could be an attempt at price discrimination. For example, Crawford (2008) shows that bundling of cable channels within tiers rather than "a la carte" is an effective way of second degree price discrimination, which enables the firm to recover its high fixed costs across a customer base with heterogeneous (and hidden) preferences. By creating bundles that consist of dissimilar channels, where virtually every customer will like some of the channels but dislike others, the firm effectively "collapses" the demand curve. Put another way, bundles can serve to homogenize customers' willingness to pay, so that households generally place similar value for the bundles offered. As a simple example, a company may offer a bundle of HGTV and MTV to accomplish this end; households who like HGTV may tend to dislike MTV, and households who like MTV may tend to dislike HGTV. However, both types of households may value the bundle similarly. Consequently, the company is able to price more effectively, cover its fixed costs, and offer a service that cus-

tomers value. It is unlikely that triple play bundling is meant to accomplish this goal though, as it seems intuitive that demand for these technology services will be positively correlated (e.g., because income is a strong determinant of demand for any of them). This precludes the possibility of "collapsing demand" as described above. Other popular reasons to bundle include: the presence of economies of scope in production, and bundling as a means to simplify the choice set for consumers.

Some key features of triple play bundling by telecom firms make it different from other examples that have been studied. In particular, telecom firms offer services, not products. The recurring nature of services allows bundling to play two distinct roles — either to attract new users, or to reduce the willingness of existing users to leave. In a one-time purchase of bundled services, such as a movie, there is no reason to distinguish between these two roles, as the latter role is irrelevant. In telecommunications services, however, users may go for years with the same supplier before reconsidering their arrangement. It is quite natural to focus on the importance of bundling for delaying those moments of reconsideration, which shape outcomes. In Section E below, we discuss this motivation for triple play bundling more extensively.

C. Why Bundle as a Consumer?

There are several reasons why a consumer may choose to purchase a bundle. Of course, bundling may be the only option in the case where sellers choose to offer pure bundles. There, consumers must either purchase a bundle or make no purchase at all. For our purposes, perhaps the most common reason to bundle is cost savings. When firms offer mixed bundles, they typically are priced lower than the sum of the a la carte prices of the bundle components. Consider again a fast food combo meal, where the combo price may be, say, $3.99, but the sum of the prices of each component (e.g., hamburger, drink, and French fries) may be, say, $4.25.

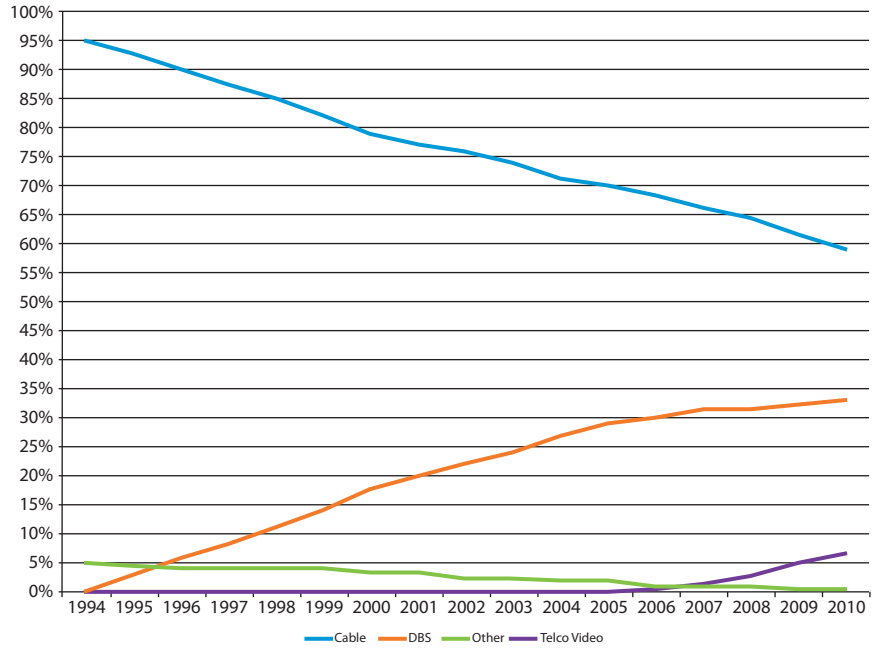
Consumers may also bundle because the bundle, per se, creates value. Triple play bundles allow consumers to pay for wired telephone, pay television, and broadband internet all on one bill at one time. This can simplify record-keeping, payment scheduling, and utilization of customer service. Consequently, consumers may find value in a triple play bundle, even if there is no bundle price discount.

While bundles may appeal to consumers due to cost savings and added value, consumers may also have reason to resist mixed bundles, even if the price seems appealing. Following insights from Farrell & Klemperer (2007), we hypothesize that bundling can alter consumers' payoffs when considering an alternative provider or dropping a service. This could be due to, e.g., the hassle of having to reconsider the pricing and payment timing of one's entire set of telecommunications services when making a switch. The results of the FCC (2011) study seem to indicate the presence of an effect like this for many households. If households foresee such an effect of bundling, they may demand an even better bundle price up front, recognizing they will be especially reluctant to switch later. Regardless, if bundling does alter household payoffs, this can affect consumer demand over time, a point we discuss in more detail in Section E.

D. The Emergence of the Triple Play Bundle

The triple play bundle is a sign of technological innovation and convergence in telecommunications services. Cable MSOs were the first to roll out video competition to incumbent broadcasters, the first to offer residential high-speed broadband Internet access services, and the first to provide facilities-based landline voice services in competition with incumbent telephone companies.

Figure 1: MVPD Market Share



While cable operators have been able to offer components of a triple play since the late 1990s, the telephone companies have been catching up with video and broadband offerings over copper and fiber infrastructure. In addition, the telephone companies have the ability to bundle mobile voice and broadband services in a "quadruple" or "quintuple" play. Cox Cable began marketing bundled voice, video and high-speed broadband access service in the late 1990s, and Cablevision began promoting the "triple play" aggressively in 2004 and probably deserves credit for launching the concept. Our data provider, Forrester Research, first added a question about triple play purchases to its questionnaire in 2006, and at that time well under 15% of households in our data were participating.

E. Can a Triple Play Bundle Have a Dynamic Effect on Service Demand?

Some key features of triple play bundling by cable firms make it different from other examples that have been studied, both in the economics literature and in communications regulatory settings.[2] The novelty motivates a new explanation for why firms may want to bundle their products. In particular, cable MSOs offer services, not products. While much, if not all, of the theoretical work on bundled products can include services, or be easily extended to services, the recurring nature of triple play services allows for bundling to serve a specific purpose. As mentioned in Section C, it may lead to bundling playing two distinct roles — either to attract new users, or to make it less attractive for existing users to leave. In cable services, the latter role is more apparent, as users may go for years with the same supplier before reconsidering their arrangement.

We hypothesize that bundling can alter consumers' payoffs when considering an alternative provider or dropping a service. For this reason, bundling may have a causal effect on service renewal rates, i.e., households who bundle may be less likely to "churn" their services and/or service providers. To anticipate issues we address in Section IV and provide a formal framework to fix ideas, we build a simple model of consumer demand for telecommunications services. The triple play

covers wired telephone, cable TV, and broadband internet, and so these are the three services we consider in our model.

In our model, there exist N households, indexed by $i = \{1,\ldots,N\}$, and three services, indexed by $j = \{1,2,3\}$. For a given household i, the utility it derives from purchasing service j at time t is formulated as $U_j(P_{jt},X_{it})$, where P_{jt} is the price of service j at time t and X_{it} is a vector representing household i's idiosyncratic characteristics that may affect service utility (e.g., education, income, etc. in our data). We assume that, holding everything else constant, utility is decreasing in price, i.e., $\partial U_j/\partial P_{jt} < 0$. In this formulation, we now have a dichotomous variable, D_{ijt}, which equals one if and only if household i purchases service j at time t, represented as:

$$(1)\ D_{ijt} = \begin{cases} 1 \text{ if } U_j\,(P_{jt}, X_{it}) > 0 \\ 0 \text{ otherwise} \end{cases}$$

In our case, we observe many X's (but not price), so we can test how utility changes with each of the X's (i.e., test the signs of $\partial U_j/\partial X_{ikt}$ for each component k of X) if we specify an econometric model for D_{ijt} .

As mentioned above, the primary focus of this paper is to determine whether bundling alters households' payoffs from switching, and consequently reduces churn. Bundling can also affect service adoption patterns. Specifically, bundling enters through a price for the three services, which is less than the sum of prices for each service purchased individually.[3] In a standard model of bundled pricing in a world where bundling does not reduce churn, a bundle contract price trades off two revenue streams. It sacrifices variable revenue — i.e., total revenue less variable cost per customer — from existing customers who would have bought all three services, and gains variable revenue from additional marginal adopters who purchase an additional service(s) they would otherwise not have purchased. The implications for a world where bundling does reduce churn are that the adopter is more reluctant to drop the service, or switch to another supplier. That might lead to a longer collection of additional variable revenue or a different price level or both. We will observe whether there is longer collection.

To account for this possibility, we extend X_{it} to include bundle status at time $t\text{-}1$, B_{it-1}. Here, B_{it-1} is a dichotomous variable equaling one if household i purchased a bundle at time $t\text{-}1$ and zero otherwise. The focus of our empirics will be to identify how utility changes with prior bundling status at the product and firm level, i.e., identify the sign of $\partial U_{jt}/\partial B_{it-1}$. If bundling alters households' payoffs in a way that reduces churn, it will appear as if $\partial U_{jt}/\partial B_{it-1} > 0$, that is, as if utility for each service is higher if a bundle was purchased last period.

To complete this model, we also include D_{ijt-1} and AT_{it-1}. Here, AT_{it-1} is a dichotomous variable equaling one if household i purchased all three services at time $t\text{-}1$. These additional determinants of utility allow for the possibility of a service-level impact of bundling on utility and spillover effects from prior adoption of other services, respectively. Consequently, the choice model we envision is as follows:

$$(2)\ D_{ijt} = \begin{cases} 1 \text{ if } U_{jt}\,(P_{jt}, D_{ijt-1}, B_{it-1}, AT_{it-1}, X_{it}) > 0 \\ 0 \text{ otherwise} \end{cases}$$

We also consider a choice model at the firm level. This is especially appropriate for our study since a single firm — the local cable company — is the predominant provider of triple play bundles during the time period studied. If bundling alters households' payoffs, this could impact households' decisions to switch away from purchasing a service from the local cable company.

There are several potential ways to model this, but we show one as an illustration. Consider one model of a household *i*'s decision to purchase service *j* from the cable company *c* at time *t* as:

$$(3)\ D_{ijct} = \begin{cases} 1 \text{ if } U_{jct}(P_{jct}, D_{ijct-1}, B_{it-1}, AT_{it-1}, X_{it}) > U_{jkt}(P_{jkt}, D_{ijkt-1}, AT_{it-1}, X_{it}) \forall\ k \\ 0 \text{ otherwise} \end{cases}$$

Here, D_{ijct} is a dichotomous variable representing a given household's decision to buy service *j* from the cable company at time *t*. We note here that our variable capturing a purchase of all three services at time *t-1* (AT_{it-1}) allows for purchase from any firm, implicitly assuming any spillovers across services will not be specific to whether the other services were purchased from the cable company or elsewhere[4]. The impact of bundling on payoffs is captured through $\partial U_{jct}/\partial B_{it-1}$. If bundling impacts utility in a way that reduces churn it will appear as if $\partial U_{jct}/\partial B_{it-1} > 0$, that is, as if utility for each service from the cable company is higher if a bundle was purchased last period.

The choice model has several implications for our econometric exercise. The first two are well known. First, it is obvious that a single cross section of data from households cannot discern whether bundles reduce churn. Bundling's effect can only be seen over time. Second, even with ideal household data an observer will be unable to identify the effect of bundling in the presence of stable demand. If the bundle price does not change over time, and the fundamentals behind demand do not change, the marginal adopter/dropper of service will not change. Pushing this point further, we should only be able to observe an effect from bundling when significant numbers of households drop a service or service provider, since bundling (at time *t-1*) is unable to influence the decision of a household that is adding a service or service provider (such households could not have had a Triple Play bundle the prior period).

Consider now the details behind the case where demand for a service is falling. This decline may be due to an exogenous increase in the technical capabilities of a service that substitutes for one of the three services in the bundle, which induces a decreased demand for it. Abusing notation, that means $U_{jt}(P, X) < U_{jt-1}(P, X)$, namely, utility is lower in time *t* in comparison to time *t-1* with the same price and demographics. Bundling will deter dropping of service if it alters payoffs in such a way so as to make switching less attractive. That is, for the same X, households with bundling will act to hold on to the service more. If no such effect is observed, then bundling likely has no effect on churn. As we'll show below, we see this downward shift in demand for both pay television and wired telephone during one of our observed time periods.[5] This allows us to measure the effect of bundling on households' decisions to purchase these services in general, and to purchase them from the cable company.

To get a correct measurement of bundling's effect, it is crucial to observe all relevant determinants of utility (Xs). If an unobserved X correlates with the use of bundling, then observing behavior consistent with utility from purchasing a service increasing with prior bundling ($\partial U_{jt}/\partial B_{it-1} > 0$) or utility from purchasing from the cable company increasing with prior bundling ($\partial U_{jct}/\partial B_{it-1} > 0$) has two interpretations — bundling caused it or an unobserved X did. One obvious concern is that a given service's price (which is unobserved) may be correlated with prior bundling behavior. If bundling in *t-1* implies a lower price at time *t*, then this will make it appear as though bundling is reducing churn when it is really a price effect. We note here that individual service prices and the bundle price generally increased after one year of service (following a low, introductory price). Further, the novelty of bundling during the time period of our data strongly suggests that bundlers experienced the second-year price jump in greater proportion than non-bundlers, implying the price of their services likely increased more than those for comparable households. Consequently, if anything, unobserved price is likely serving to mask (underestimate) any bundling effects in our

analysis. Nevertheless, we give the question attention in our econometric techniques (discussed in Section IV), which are designed to minimize the role of unobserved price in our findings.

Another particular concern would be if bundling serves the role of a screen. That is, households with a low propensity to switch services/providers self-select into purchasing a bundle. In this case, there would be an unobserved X (aversion to switching) that is positively correlated with bundling and with the (re-) purchase of a service, causing bundling to appear to reduce switching when it does not. While this is a plausible theory, the theory of bundling provides an important countervailing force. Households with a bundle selected into a bundled contract because they received a lower price, so the group of bundlers includes some households with lower marginal valuations for the services than households who get the same service without bundle. These lower marginal-value households are more likely to drop the service when demand falls. In this case, there is an unobserved X (low marginal value) that is positively correlated with bundling and negatively correlated with (re-) purchase of a service. This would tend to mask the causal effect of bundling, meaning our results would underestimate the true effect.

In our empirics, we seek some evidence of bundling serving as a screen. The potential presence of such unobserved heterogeneity again highlights the importance of using econometric techniques that separate it out (discussed in Section IV).

For one of our services — broadband — demand is expanding over the time period we observe, due to an exogenous increase in the capability of the service. In the context of our model, this means $U_{jt}(P, X) > U_{jt-1}(P, X)$ for any given P and X. As mentioned above, bundling's effect is primarily identified through service dropping, so we do not expect its effect to be evident in our data for broadband at the service level. Certainly there are some households that drop broadband even while overall demand expands, but this is likely a very small group. However, when we instead consider broadband from the cable company (D_{ijct}), there is likely a significant amount of dropping (churn) occurring. Broadband is still a relatively new service with many providers, meaning many households may still be learning their own preferences across providers.[6] Consequently, we expect any effect of bundling on broadband to be most evident at the provider level, as opposed to the service level.

In the case of an expanding service such as broadband, we again should be concerned about unobserved heterogeneity driving our results due to bundling serving as a screen. However, since we expect the effect to be identified at the provider level, it is no longer clear that households dropping broadband service from the cable company are marginal adopters (they are merely switching providers, not dropping the service necessarily). Consequently, this is no longer an obvious source of unobserved heterogeneity.

Nevertheless, it is possible the expanding demand for broadband can generate an alternative source of unobserved heterogeneity. Specifically, consider the following model. Suppose many households that bundled in period *t-1* were marginal adopters, and also price sensitive. As demand overall increased, these households are no longer marginal but still price sensitive. This makes them especially prone to purchasing the (price discounted) bundle, and hence retaining broadband service. In this case, there is an unobserved X (price sensitive and no longer marginal at time *t*) that is positively correlated with bundling and (re-) purchasing broadband service. Again, the possible presence of such unobserved heterogeneity requires econometric techniques that separate it out (Section IV).

F. Implications of Dynamic Bundling Effects

What are the implications for firm and consumer strategies if bundling alters households' payoffs in ways that reduce churn? The most obvious concern might be that these altered payoffs give sellers market power, allowing them to "raise price above competitors' by an amount almost equal to the switching cost" (Farrell and Shapiro, 1988). That concern generates several related responses. From a policy perspective, any altered payoffs due to bundling are most worrisome with myopic consumers, who do not forecast the change. If bundling creates switching costs, policy has less reason to worry about forward-looking customers, who may ask for price discounts in advance. Empirically, however, these explanations cannot be distinguished in even the most ideal data, since both lead to the same prediction, slower switching at some later time. Interestingly, this last prediction is generally unquestioned in the literature on bundling. Though empirical evidence of switching costs arises in a variety of contexts, there is little empirical evidence of its presence or absence in telecommunications services, or its consequences for bundling.

Beyond this, one might be concerned that altered payoffs due to bundling create a barrier to entry (e.g., Porter, 1980). Specifically, incumbents have a cost advantage over potential entrants, and could exploit this to exclude entrants while still making positive profits. Farrell and Shapiro (1988) note that incumbents may not wish to do this if they are unable to distinguish new buyers from existing buyers. However, in our case, it is standard practice for cable companies (and other firms) in this industry to price discriminate according to customers' tenure with the company (e.g., through introductory, temporary price offers). Consequently, we may not be concerned with bundling as a means to extend market power (since it is in the form of mixed bundling). If it increases switching costs, however, we may be concerned that it harms competition/consumer welfare via increased long-run market power and stunted take-up of substitute services offered by entrants, such as satellite television or a cable over-builder.

Overall, the welfare effects of bundling depend on the balance of its impact on long-run welfare, the prevalence of forward-looking behavior by consumers, and the inherent value bundles create (e.g., through simplified billing) for consumers.

III. Empirical Overview of Triple Play Activity

A. The Data

The data for this project come from Forrester Research, Inc. Each year since 1997, Forrester collects thousands of mail surveys of U.S. households on their technology purchases and preferences. The surveys are known as Technographics, and they are administered in December of each year. The earliest waves consisted of both independent cross sections and panel data, as a significant number of the same households were purposefully surveyed in consecutive years. Such panel data was used in Prince (2008) and potentially would be ideal for our purposes. Questions regarding triple play bundling did not appear on the survey until 2006.[7] Unfortunately, by this time repeated sampling of households had ceased. Hence, our usable data consist of repeated cross sections.

The focus of our analysis is on the three most recent waves with similar survey structure — 2007–2009. The data contain a wide range of demographic information. Though Forrester makes attempts to produce a survey that varies the population in different locations and economic circumstances, they also make no pretense that their sample is precisely representative of the US population. Hence, this demographic information is useful both as controls, and for identifying comparable subgroups across years when constructing a pseudo-panel (as described in section IV). That is, we use the demographics to make sure our inferences are robust to slight variations in year to year composition of the sample of households. The demographic information we utilize includes: location (DMA)[8], education, income, household size, and age.

Beyond demographics, the data contain information on the use of telecom services and their providers. Specifically, we can observe, for each year, whether a household subscribes to the following services: wired telephone, pay television (cable or satellite), and broadband internet (cable, DSL, cable overbuilder, fiber, or satellite). For each of these services, if the household has a subscription, we can observe the provider (e.g., Time Warner Cable, Verizon, DirecTV, etc.).

However, this information is also limited in some important ways. We do not observe the quality of the service, nor its price, nor the menu of choices put in front of each household. We also do not observe the price of the services for the unchosen option(s), such as the prices for the bundle (for those who did not adopt a bundle) or the prices of individual services (for those who did adopt a bundle). Working around these limitations is the principal challenge for our econometric approach.

A key variable for our analysis concerns bundling behavior. For each year, Forrester asks the household whether it receives "A bundle of TV, Internet, and phone service from one company on one bill for a package price." The answer to this question generates our binary "bundle" variable, and we are primarily interested in the effect of bundling behavior in one year on households' service subscription choices the following year.

We provide summary statistics for all variables used in years 2007–2009 in Table 1 on the next page.

Table 1: Summary Statistics

	2007			2008			2009		
Variable	**Mean**	**Std. Dev.**	**# of Obs.**	**Mean**	**Std. Dev.**	**# of Obs.**	**Mean**	**Std. Dev.**	**# of Obs.**
Telephone	0.908	0.289	49847	0.902	0.298	47698	0.855	0.352	36194
Television	0.828	0.377	49847	0.823	0.382	47698	0.774	0.418	36194
Broadband	0.594	0.491	49847	0.619	0.486	47698	0.678	0.467	36194
TelephoneCableCo	0.105	0.307	49847	0.142	0.349	47698	0.165	0.371	36194
TelevisionCableCo	0.538	0.499	49847	0.531	0.499	47698	0.447	0.497	36194
BroadbandCableCo	0.254	0.435	49847	0.291	0.454	47698	0.318	0.466	36194
Bundle	0.225	0.418	49847	0.279	0.448	47698	N/A	N/A	N/A
All Three Services	0.486	0.500	49847	0.508	0.500	47698	N/A	N/A	N/A
Less Than H.S.	0.056	0.229	49847	0.063	0.243	47698	0.062	0.241	36194
H.S. Degree	0.248	0.432	49847	0.260	0.439	47698	0.249	0.433	36194
Some College	0.344	0.475	49847	0.337	0.473	47698	0.330	0.470	36194
College Degree	0.216	0.411	49847	0.209	0.407	47698	0.221	0.415	36194
Graduate Degree	0.136	0.343	49847	0.131	0.337	47698	0.137	0.344	36194
< 25K	0.189	0.391	49847	0.204	0.403	47698	0.197	0.398	36194
25K — 49K	0.299	0.458	49847	0.277	0.448	47698	0.261	0.439	36194
50K — 69K	0.187	0.390	49847	0.179	0.384	47698	0.165	0.371	36194
70K — 99K	0.165	0.371	49847	0.159	0.366	47698	0.175	0.380	36194
100K+	0.160	0.367	49847	0.180	0.384	47698	0.202	0.402	36194
HHSize = 1	0.157	0.364	49847	0.171	0.377	47698	0.162	0.368	36194
HHSize = 2	0.363	0.481	49847	0.369	0.483	47698	0.344	0.475	36194
HHSize = 3	0.209	0.406	49847	0.203	0.403	47698	0.203	0.402	36194
HHSize = 4	0.171	0.377	49847	0.162	0.368	47698	0.177	0.382	36194
HHSize = 5+	0.099	0.299	49847	0.095	0.293	47698	0.115	0.319	36194
Age < 25	0.091	0.288	49847	0.082	0.275	47698	0.094	0.291	36194
Age 25–34	0.163	0.369	49847	0.135	0.342	47698	0.161	0.367	36194
Age 35–44	0.196	0.397	49847	0.179	0.383	47698	0.197	0.397	36194
Age 45–54	0.222	0.415	49847	0.214	0.410	47698	0.196	0.397	36194
Age 55–64	0.170	0.376	49847	0.190	0.392	47698	0.175	0.380	36194
Age 65+	0.158	0.364	49847	0.200	0.400	47698	0.178	0.383	36194

B. Who Uses Triple Play Services and Who Bundles Them?

In this subsection and the next, we provide a simple snapshot of our data as it pertains to triple play bundling behavior. We first analyze how demand for each service and demand for a triple play bundle relate to key demographic measures. A simple, efficient way to perform this analysis is to run some basic regressions, which provide us conditional correlations. For example, a regression of broadband internet demand on income and education can illustrate how demand for this service correlates with income, controlling for education level (and vice versa). In Table 2, we regress our dichotomous variables for service demand on our primary demographic measures (income, education, age, household size) in the most recent year of data for which we cleanly observe triple play bundling behavior, 2008. The results for other years are comparable.

Table 2: Service and bundle regressions[9]

	Wired Telephone		Pay Television		Broadband Internet		Triple Play	
Variable	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
H.S. Degree	0.034**	0.007	0.043**	0.009	0.088**	0.009	0.030**	0.008
Some College	0.023**	0.007	0.029**	0.009	0.167**	0.009	0.047**	0.008
College Degree	0.018**	0.007	0.021*	0.009	0.228**	0.010	0.052**	0.009
Graduate Degree	0.021**	0.007	0.006	0.010	0.246**	0.010	0.058**	0.010
25K — 49K	0.020**	0.004	0.091**	0.006	0.134**	0.007	0.060**	0.006
50K — 69K	0.038**	0.005	0.130**	0.006	0.231**	0.007	0.094**	0.007
70K — 99K	0.055**	0.005	0.160**	0.006	0.296**	0.007	0.140**	0.007
100K+	0.055**	0.005	0.182**	0.006	0.349**	0.007	0.154**	0.007
HHSize = 2	0.031**	0.004	0.066**	0.006	0.075**	0.006	0.048**	0.006
HHSize = 3	0.055**	0.005	0.074**	0.006	0.089**	0.007	0.074**	0.007
HHSize = 4	0.073**	0.005	0.057**	0.007	0.115**	0.008	0.086**	0.008
HHSize = 5+	0.064**	0.006	0.003	0.008	0.102**	0.009	0.076**	0.009
Age 25–34	0.018*	0.009	0.056**	0.008	0.029**	0.009	0.028**	0.009
Age 35–44	0.129**	0.008	0.068**	0.008	-0.010	0.009	0.020*	0.009
Age 45–54	0.171**	0.007	0.063**	0.008	-0.044**	0.009	0.018*	0.008
Age 55–64	0.209**	0.007	0.076**	0.008	-0.082**	0.009	0.036**	0.009
Age 65+	0.248**	0.007	0.051**	0.008	-0.203**	0.009	0.008	0.009
R-squared	0.078		0.040		0.162		0.027	
Observations	47698		47698		47698		47698	

The results indicate that, unsurprisingly, demand for all three services is positively correlated with income. However, such uniformity does not exist for the other demographics. Pay television and broadband internet have opposing correlations with education (negative and positive, respectively), while wired telephone has little correlation with education. Regarding household size, there is generally a positive correlation with wired telephone and broadband internet, but more of a "hill shaped" relationship with pay television, peaking at a household size of three. Finally, we see that wired telephone and broadband internet have opposing correlations with age (positive and negative, respectively), while pay television has little correlation with age.

The last regression in Table 2 measures the relationship between triple play bundling and demographic variables. Here, we use a dichotomous variable indicating whether the household has a triple play bundle as our dependent variable. This regression indicates that triple play bundling is positively correlated with education, income, and household size, but largely uncorrelated with age.

C. The Evolution of Triple Play Service Demand and Triple Play Bundling Behavior

It is also informative to see how demand for triple play services and the triple play bundle evolved over the time period of our data. Below, we present the proportion of households in our data that purchased each service between 2007 and 2009, as well as the proportion that purchased a triple play bundle in 2007 and 2008. As mentioned above, we do not present results for the triple play bundle in 2009 due to a change in question concerning triple play in the Forrester questionnaire, making cross-year comparisons problematic.

Table 3: Service adoption levels by year

Variable	Year	Adoption Rate
Wired Telephone	2007	0.9080
	2008	0.9017
	2009	0.8550
Pay Television	2007	0.8284
	2008	0.8225
	2009	0.7739
Broadband Internet	2007	0.5939
	2008	0.6193
	2009	0.6784
TelephoneCableCo	2007	0.1052
	2008	0.1419
	2009	0.1646
TelevisionCableCo	2007	0.5380
	2008	0.5307
	2009	0.4467
BroadbandCableCo	2007	0.2538
	2008	0.2908
	2009	0.3178
Triple Play Bundle	2007	0.2251
	2008	0.2786

Table 3 shows stable levels of adoption for wired telephone and pay television between 2007 and 2008, but then a precipitous drop in 2009, consistent with the significant recession that occurred in the United States at that time. This drop is also apparent for pay television from the cable company, but not for wired telephone with the cable company. In contrast, broadband internet adoption and broadband internet from the cable company steadily increased between 2007 and 2009. Finally, triple play bundling increased significantly between 2007 and 2008.

IV. Measuring the Dynamic Effects of Triple Play Bundling

A. The Empirical Model

We begin our empirical analysis by constructing a model of product and firm choices at the household level. We employ what's known as a dynamic linear probability panel data model to explain household choices, which we detail below. The choices we are trying to explain consist of: wired telephone service, pay television service, broadband internet service, wired telephone service with the cable company, pay television service with the cable company, and broadband internet service with the cable company. Given a choice variable we want to model, let Y_{it} be a binary variable that equals one if household i chooses to subscribe to that service/provider in time t and zero otherwise. For example, if we are modeling the wired telephone service decision, $Y_{it} = 1$ if household i subscribes to wired telephone service in time t. Our econometric model then looks as follows:

$$(1)\quad Y_{it} = \beta_0 + \beta_1 X_i + \beta_2 Y_{it-1} + \beta_3 B_{it-1} + \beta_4 AT_{it-1} + \varepsilon_{it}$$

Here we assume X_i is a vector of household characteristics that are constant over time[10], B_{it-1} is a binary variable indicating whether the household had a bundle at time $t-1$, AT_{it-1} is a binary variable indicating whether the household had all three services at time $t-1$ (bundled or not), and ε_{it} constitutes unobservables for household i at time t that affect its choice on Y. The inclusion of Y_{it-1} and AT_{it-1} is important, as they (respectively) control for switching costs at the individual service level and spillover effects from purchasing either of the other two services. The primary goal of our empirical analysis is to assess whether $\beta_3 > 0$.

Of course, there is reason to believe Y_{it-1}, AT_{it-1} and B_{it-1} are endogenous. Put another way, we have reason to be concerned that these variables are correlated with unobservables that influence the service choice of household i at time t (Y_{it}). For example, if a household has an inherent, persistent preference for having a wired telephone connection (captured in both ε_{it-1} and ε_{it}), it likely chose to purchase this service at time $t-1$ (i.e., $Y_{it-1} = 1$), and will likely choose to purchase this service at time t as well (i.e., $Y_{it} = 1$). This will make it appear as though the purchase decision at $t-1$ influenced the purchase decision at time t, when in fact it was the household's persistent preference for a wired telephone connection driving both decisions. Beyond this, because we only have repeated cross sectional data, we cannot actually observe Y_{it-1}, AT_{it-1} and B_{it-1} for a given household i. We address both of these concerns below.

B. Estimating the Model using a Pseudo Panel Dataset

If our data from Forrester was a panel, we would execute standard panel data methods to estimate equation (1). However, because we only have repeated cross sectional data, we must construct and utilize a pseudo (or synthetic) panel using these cross sectional data. Such an approach began with Deaton (1985), and has been developed further by several subsequent papers in the econometrics literature (e.g., Moffitt, 1993, Collado, 1997, McKenzie, 2004, Verbeek & Vella, 2005). These techniques have most often been applied in macroeconomics, labor economics and development economics, where available data are often in the form of repeated cross sections (e.g., Cuesta et al., 2011).

In general, constructing a pseudo panel involves identifying a set of time-invariant criteria with which to construct data groupings. In our case, these criteria will consist of observable demographic characteristics that we believe are stable over a short period of time (one year). The basic idea for our application is that information about lagged variables (Y_{it-1}, AT_{it-1}, and B_{it-1}) for household i can be gathered by observing Y_{ht-1}, AT_{ht-1}, and B_{ht-1} for households h in the same group as household i. To this end, we follow the approach described in Moffitt (1993) and further discussed in Verbeek and Vella (2005). Specifically, for each household i in a given group g, we replace Y_{it-1}, AT_{it-1}, and B_{it-1} with $\overline{Y}_{gt-1}$, $\overline{AT}_{gt-1}$, and $\overline{B}_{gt-1}$, respectively. The latter three variables are the averages for Y, AT, and B in group g at time $t-1$.

The above methodology has two key merits. First, it fills in the missing pieces in equation (1). For a given household i that we observe at time t, the averages for Y, AT, and B in the same group as household i at time $t-1$ intuitively provide information about Y_{it-1}, AT_{it-1}, and B_{it-1}. Put another way, knowing information about households that "look like" household i at time $t-1$ can give us information about household i at time $t-1$. Second, the above methodology actually mitigates endogeneity concerns for our lagged variables, described above. Whereas with panel data, we would have a clear concern that unobservables (ε_{it}) are correlated with our lagged variables (Y_{it-1}, AT_{it-1}, and B_{it-1}), this is not the problem we face when replacing lagged variables with group averages at $t-1$. Moffitt (1993) describes the above approach as an application of simple two stage least squares regression, where in the first stage, we regress Y_{ht-1}, AT_{ht-1}, and B_{ht-1} on group dummy variables. Then, in the second stage, we use the predicted values from the first stage (which will simply be group averages) when estimating equation (1).

While Moffitt 1993's justification for using group means as instruments is intuitive, we follow the discussion in Verbeek and Vella (2005) to more clearly pin down the assumptions we employ to get unbiased parameter estimates. We begin by reformulating equation (1) as follows:

$$(2)\ Y_{it} = \beta_0 + \beta_1 X_i + \beta_2 \overline{Y}_{gt-1} + \beta_3 \overline{B}_{gt-1} + \beta_4 \overline{AT}_{gt-1} + (\varepsilon_{it} + \beta_2 (Y_{it-1} - \overline{Y}_{gt-1}) + \beta_3 (B_{it-1} - \overline{B}_{gt-1}) + \beta_4 (AT_{it-1} - \overline{AT}_{gt-1}))$$

Given a choice of groupings, it is this equation we will estimate with our data, where the terms in parentheses constitute our composite "error term." Written this way, the use of group averages essentially introduces measurement error, in the form of $Y_{it-1} - \overline{Y}_{gt-1}$, $B_{it-1} - \overline{B}_{gt-1}$, and $AT_{it-1} - \overline{AT}_{gt-1}$. As Verbeek and Vella (2005) note, this measurement error is uncorrelated with our explanatory variables, in particular $\overline{Y}_{gt-1}$, $\overline{B}_{gt-1}$, and $\overline{AT}_{gt-1}$. Hence, this eliminates concerns about a "classic errors in variables" problem that could induce bias.

Formulating the econometric model as we do in equation (2) allows us to clearly determine the necessary assumptions for our parameters to be identified. On a broad level, we simply need our composite error term to be uncorrelated with our explanatory variables. However, by considering each part of the composite error term, we can more clearly determine the believability of this assumption. To begin, as noted above, the measurement error is not correlated with the group averages by construction. In addition, the measurement error is not correlated with X_i since X_i does not vary over time (see Verbeek and Vella, 2005).

This means identification depends on a key assumption, that the idiosyncratic term (ε_{it}) is uncorrelated with the explanatory variables. Maintaining that ε_{it} and X_i are uncorrelated is standard, as the components of X_i play the role of "exogenous" demographic controls. Further, we note here that our controls help account for price variations (which we cannot observe). Specifically, telecom

service prices generally vary regionally. Hence, our regional (DMA) controls account for a great deal of (unobserved) price variation.

Assuming ε_{it} is uncorrelated with $\overline{Y}_{gt-1}$, $\overline{B}_{gt-1}$, and $\overline{AT}_{gt-1}$, could be more problematic. As noted in Verbeek and Vella (2005), this requires us to believe there are no "group effects" in the unobservables. That is, we must assume there are no persistent, group level unobservables (e.g., exactly those households with mid-level education, high income, aged 45–54, with four members in Chicago consistently get better prices for pay television). The existence of such group effects creates obvious concern for bias in β_2 since they directly imply correlation between $\overline{Y}_{gt-1}$ and ε_{it}; however, their potential for bias in β_3 and β_4 depends on whether we believe these group effects that impact *Y* are also correlated with *B* and/or *AT*. Regardless, even if we believe any group effects would be uncorrelated with *B* and *AT*, a bias in β_2 can generate a bias in β_3 and/or β_4. Hence, to have the greatest faith in our estimates, we want to maintain the assumption of no group effects.

In principle, we can completely eliminate the presence of group effects in our unobservables by including group fixed effects in X. However, with just two waves of usable data, this severely limits remaining variation in our variables. Therefore, we proceed without group fixed effects, and maintain the assumption that there are no group effects beyond those captured in X. This implies that cross-sectional variation in unobservables (having controlled for X) is transient. The believability of this assumption depends on our method for constructing groups (and the persistence of our results across specifications), which we now describe.

We construct our groups using classic demographic measures in our data. These include: designated market area (DMA), income level, education level, household size, and age. The question then is where to draw the boundaries for the groups, and it is here that we face a tradeoff, part of which is summarized in Cuesta et al. (2011). The tradeoff for our analysis is summarized as follows. As we draw tighter boundaries, we have more groups but a smaller number of observations per group. With more groups, we generate more variation in our variables (in particular with regard to our variables measured at the group level), creating more "observations" and hence greater identification power. More groups also allows us to include more "X" controls. With more controls, the existence of "group effects" in the unobservables becomes less likely, thus making our assumption that ε_{it} is uncorrelated with $\overline{Y}_{gt-1}$, $\overline{AT}_{gt-1}$, and $\overline{B}_{gt-1}$ more credible. However, with fewer observations per group, the group means used in the regression will be poor estimates of the true population mean for that group. This essentially will inflate the variance in our composite error term, and hence tend to inflate standard errors.

Recognizing this tradeoff when constructing our groupings, we opt for a larger number of groups. This is because it allows us to include important controls without completely eliminating variation in our averaged variables (bundle, adoption of all three services, and prior adoption). Perhaps the most important controls in our analyses are DMA-level dummy variables. These controls net out persistent price and service quality differences across locations, which certainly exist and we do not directly observe. Consequently, our results group our observations according to: DMA, income, education, household size, and age. Here, the categories for the last four measures are as defined in Table 1. In supplementary analysis, we consider some coarser groupings; however, the importance of including DMA-level dummies precludes us from coarsening our groupings too much. For example, we do not conduct analysis for observations grouped only at the DMA level. In this case, DMA-level dummies are impossible to include; and without them, our results would be highly suspect, since we'd have inadequate controls for unobservable price variation and service quality variation.

C. Testing for Screening and Selection

As noted above, households with lower marginal valuation may be induced to make a purchase by the price savings in a bundle. A household's inherent preference for inertia also may induce selection into a bundle, though it is something unobservable to both service providers and the econometrician.

Since we do not observe individual households' bundle statuses, we cannot test for a correlation with the unobservables in the error term (ε_{it}) via, e.g., a Hausman test. Despite such limitations, we still have several options. We can at least test whether, along the demographic measures we observe, bundlers significantly differ from non-bundlers who also purchased all three services. Any differences we find would be suggestive of bundling playing a screening role — picking off households that are less inclined to churn services and/or service providers. We note that any implied screening effect that we find would be in addition to causal effects we identify, since, by design, our econometric model above abstracts away from any correlation between unobserved household characteristics and bundling status.

D. Empirical Findings and Interpretation

In Tables 4a-4d, we present our results[11] for our six choice variables (landline, television, broadband, landline with the cable company, television with the cable company, and broadband with the cable company) for 2008 and 2009. For ease of presentation, these tables present our key estimates[12], while full results (including estimates for all of our controls) are in Prince and Greenstein (2011).

Table 4a: 2007–2008

Covariates	Dependent Variable					
	Wired Telephone		Pay Television		Broadband Internet	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
$DepVarAvg_{t-1}$	0.184743**	0.012256	0.169289**	0.009366	0.161881**	0.012196
$BundleAvg_{t-1}$	-0.00271	0.005634	0.005715	0.006959	0.015415+	0.008615
$AllThreeAvg_{t-1}$	-0.00083	0.004787	-0.00572	0.006514	-0.00442	0.011733
Covariates for DMA, income, education, household size, age	Yes		Yes		Yes	
DMA-level Dummies	Yes		Yes		Yes	
R-squared	0.104		0.068		0.198	
Observations	34070		34070		34070	

Table 4b: 2007–2008

Covariates	Dependent Variable					
	TelephoneCableCo		TelevisionCableCo		BroadbandCableCo	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
$DepVarAvg_{t-1}$	0.149843**	0.012478	0.184701**	0.0079	0.156389**	0.009327
$BundleAvg_{t-1}$	0.013511+	0.008183	-0.0111	0.009751	0.022765*	0.009355
$AllThreeAvg_{t-1}$	0.00414	0.005727	-0.00444	0.008365	0.011664	0.007726
Covariates for DMA, income, education, household size, age	Yes		Yes		Yes	
DMA-level Dummies	Yes		Yes		Yes	
R-squared	0.056		0.065		0.093	
Observations	34070		34070		34070	

Table 4c: 2008–2009

Covariates	Dependent Variable					
	Wired Telephone		Pay Television		Broadband Internet	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
$DepVarAvg_{t-1}$	0.256198**	0.01344	0.201352**	0.011716	0.22366**	0.012296
$BundleAvg_{t-1}$	0.009231	0.006851	0.022288**	0.007904	0.007229	0.00844
$AllThreeAvg_{t-1}$	-0.00521	0.006628	0.015384+	0.008463	-0.02083+	0.011121
Covariates for DMA, income, education, household size, age	Yes		Yes		Yes	
DMA-level Dummies	Yes		Yes		Yes	
R-squared	0.140		0.083		0.239	
Observations	25668		25668		25668	

Table 4d: 2008-2009

Covariates	Dependent Variable					
	TelephoneCableCo		TelevisionCableCo		BroadbandCableCo	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
$DepVarAvg_{t-1}$	0.183547**	0.012094	0.203363**	0.008644	0.201266**	0.00975
$BundleAvg_{t-1}$	0.035604**	0.00851	0.022013*	0.010114	0.005973	0.009715
$AllThreeAvg_{t-1}$	0.005201	0.006747	0.00017	0.009462	0.007187	0.00869
Covariates for DMA, income, education, household size, age	Yes		Yes		Yes	
DMA-level Dummies	Yes		Yes		Yes	
R-squared	0.073		0.075		0.115	
Observations	25668		25668		25668	

Before looking at the results for bundling we examined all the estimates for symptoms that the model performs reasonably well. This appears to be the case, as our coefficient estimates for our controls largely mirror those in Table 2. For example, as expected, broadband demand is monotonic in income (increasing), education (increasing), size of household (increasing except at the highest level), and age of head of household (decreasing, especially after 65). Further, as we might expect, we find evidence of switching costs for each service individually (captured by positive coefficients for Y_{it-1}). For example, subscription to pay television at time *t-1* increases the likelihood of subscribing to pay television at time *t*. Given we control for this effect, any effect we find for bundling is above and beyond any switching costs at the individual service level. Lastly, it is interesting to note that a purchase of the other two services at time *t-1* appears to generate very little, if any, spillover effects for any of the services. This is captured by the fact that our coefficient estimates for $AllThreeAvg_{t-1}$.

Moving to our variable of interest, the results indicate an effect of households' bundle status, but it appears to differ across services/providers and across years. In particular, in 2008 bundling appears to reduce churn with respect to: broadband, broadband with the cable company, and wired telephone with the cable company. In 2009, bundling appears to reduce churn with respect to: pay television, wired telephone with the cable company, and pay television with the cable company. This pattern of results holds up well to different groupings we tried where the number of groups remained large.

The only instances where we observe some differences involve broadband. Specifically, we do find a significant effect of bundling for broadband and broadband with the cable company for some of our alternative groupings (e.g., grouping along DMA, income, education, and household size – no longer grouping on age).[13] We discuss this peculiarity further below.

Overall, these results suggest that bundling does reduce churn in many instances, but the variance in its effect is curious. We believe there are two fundamental drivers behind the pattern we find. The first is very straightforward – effects from bundling are more visible in "turbulent" markets. Put another way, we only expect to observe an effect from bundling in markets where there is a significant amount of turnover in services and/or service providers. At the time of our data, wired telephone and pay television are widely diffused, while broadband internet is still in the midst of diffusing. Hence, we may only expect to see an effect on broadband (and particularly broadband with the cable company) since many consumers are still relatively new to the market and may still be learning about the service and service providers (as discussed above).

However, the U.S. suffered a deep recession during the time period of our data, whose nadir was sometime in 2009. Consequently, there was a great deal of turmoil in the wired telephone and pay television markets in the form of service dropping between 2008 and 2009. In Table 3 above, where we present simple summary statistics for overall adoption rates for our six choice variables, the effects of the recession are apparent. In it, we can clearly see the wired telephone and pay television markets were very stable between 2007 and 2008, but both took major downturns between 2008 and 2009. Concurrently, we see broadband continue a steady diffusion, moving from 59% to 62% to 68% of our sample between 2007 and 2009.

If the effects of bundling are generally only visible in turbulent markets, our results suggest that bundling does reduce churn for all services and service providers, and we simply observed it during times that their markets experienced turbulence. While this is our general conclusion from what we find, there still remains the aforementioned variance in the measured effect of bundling on broadband and broadband from the cable company. Specifically, we measure an effect in 2008,

and in 2009, we don't see an effect using our primary groupings, but do in some alternative groupings (e.g., grouping by DMA, income, education, and household size only).

We put forth that this is likely due to a feedback effect from bundling's effect on television, which sometimes masks its effect on broadband. Specifically, as subscription to pay television dramatically declined in 2009, we saw the emergence of a new phenomenon. During this time, there was a significant shift by households toward subscribing to broadband internet and *not* subscribing to television.

In Table 5, we illustrate this shift. Here, we see the proportions of households for all four combinations of pay television and broadband subscription statuses across our three years of data. In 2009, the proportion of households with broadband and no television dramatically increased compared to 2008, while all other proportions remained relatively stable or declined. The shift is of comparable magnitude to the shift in pay television subscription over the same period, suggesting it represents a significant number of households moving from subscribing to pay television and broadband (or possibly television and no broadband) to only subscribing to broadband[14].

Table 5: Proportions of households with all possible broadband and (pay) television service combinations

Year	TV & Broadband	TV & No Broadband	Broadband & No TV	No TV & No Broadband
2007	0.4945	0.3084	0.0664	0.1307
2008	0.5399	0.2785	0.0650	0.1165
2009	0.5325	0.2167	0.1175	0.1334

Our findings in Table 5 indicate that, for a subset of the population, broadband became a substitute for pay television. To corroborate this claim, we note that the most likely group to choose to substitute broadband for television in the face of a recession would be those who are most capable of using broadband to view programming content, and have the greatest need to save money. In the context of our demographic measures, this group likely consists of younger, educated households with less income. In Table 6, we present and compare across our three years of data the averages of our demographic measures for the group of households with broadband service but no pay television service. Here, we see that this group become younger, more educated, and had lower income in 2009, as we'd expect.

If broadband became a substitute for pay television for a significant number of households in 2009, this could reduce our measured effect of bundling on broadband and broadband from the cable company in 2009, via a feedback effect. This is because, for households dropping television service, broadband service is more likely. However, this group is also less likely to have bundled, since bundling reduces churn. Hence, the measured effect of bundling on broadband will be less than its "true" effect, since the measured effect captures both the effect of altered payoffs for broadband and the effect of less churn in television.

Taken as a whole, our results indicate the following. Bundling does reduce churn, and its effect is most visible when markets are turbulent. In addition, broadband appears to have emerged as a substitute for pay television for a significant number of households in 2009. Bundling's churn-reducing effect on pay television likely prevented some households from dropping pay television to go "broadband-only," thus creating a smaller net effect of bundling on broadband.

Table 6: Relationship between having broadband and no pay television and key demographics

Dep. Var.: Broadband & No TV	2007		2008		2009	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Age 25–34	-0.02684**	0.004272	-0.02571**	0.004739	-0.0537**	0.006529
Age 35–44	-0.04743**	0.004139	-0.04394**	0.00452	-0.07195**	0.006301
Age 45–54	-0.0486**	0.004088	-0.04731**	0.004444	-0.08718**	0.006312
Age 55–64	-0.05936**	0.004384	-0.06029**	0.004667	-0.10691**	0.006669
Age 65+	-0.06793**	0.004431	-0.07181**	0.004719	-0.1241**	0.006766
25K – 49K	-0.00669*	0.003027	-0.00139	0.003176	0.003888	0.004705
50K – 69K	-0.01142**	0.003472	-0.00766*	0.003625	-0.00539	0.005428
70K – 99K	-0.0173**	0.00369	-0.01276**	0.003835	-0.02348**	0.005525
100K+	-0.03333**	0.003929	-0.02235**	0.003958	-0.0318**	0.005636
H.S. Degree	0.009667*	0.004398	0.001303	0.004546	0.019587**	0.006688
Some College	0.029969**	0.004353	0.027016**	0.004509	0.048667**	0.006634
College Degree	0.053895**	0.004725	0.047302**	0.004908	0.072536**	0.007191
Graduate Degree	0.068672**	0.005073	0.057847**	0.005334	0.085964**	0.007773
HHSize = 2	-0.00168	0.003096	-0.00735*	0.003186	-0.01151*	0.004752
HHSize = 3	-0.00212	0.003589	-0.01064**	0.003748	-0.01954**	0.005495
HHSize = 4	0.01185**	0.003851	0.002673	0.004061	-0.00612	0.00584
HHSize = 5+	0.037211**	0.0044	0.027081**	0.004643	0.019868**	0.006482
Constant	0.087547**	0.005805	0.095941**	0.006195	0.167391**	0.00892
R-squared	0.017		0.016		0.021	
Observations	59368		52765		41580	

The effect also appears to be economically significant. For example, we see that, in 2009, bundling the prior year increases the likelihood of subscribing for pay television and wired telephone services from the cable company by 2.2 and 3.5 percentage points, respectively. This not only represents a substantial revenue boost for 2009, but to the extent that bundling persists (and household bundling is increasing over the time period we observe), it could mean increased revenues for multiple years. This long-term impact of bundling on revenue should factor in to a firm's consideration of tradeoffs when deciding if and how to bundle (as discussed in Section I). It is also worth noting that this substantial effect may also be capturing added value to consumers, if altered payoffs from bundling are, at least in part, due to consumers finding it beneficial to have all services on one bill at one time.

We conclude this subsection by again considering the possible role of bundling as a screen, as discussed in subsection C above, we can only provide suggestive evidence of bundling playing this role, in addition to its causal effect on churn. To examine whether it may be serving as a screen, we conduct simple comparisons along our demographic measures between households who bundled and households who purchased all three services but did not bundle[15]. We present these results in Table 7.

Table 7: Demographics for bundlers and non-bundlers (with all 3 services) by year[16]

Year	Variable	Mean for Bundlers	Mean for Non-Bundlers with all 3 Services	Different at 5% level?
2007	**Income**	3.034	3.197	Yes
	Education	3.199	3.297	Yes
	Age	3.542	3.654	Yes
	Household Size	2.821	2.785	Yes
2008	**Income**	3.106	3.207	Yes
	Education	3.177	3.254	Yes
	Age	3.835	3.847	No
	Household Size	2.745	2.725	No

Here, we see that bundlers generally have lower income and lower education levels, as compared to non-bundling households who also purchased all three services. Further, there is some mild evidence that they live in larger households and are younger. These findings illustrate non-trivial differences between bundlers and non-bundlers of all three services along basic demographics, suggesting bundlers are a somewhat selected group. To the extent that selection along these dimensions is related to selection according to propensity to switch, bundling may be playing the role of a screen, in addition to its direct impact on payoffs from switching. However, the importance of income suggests that bundling probably also picks up users who are sensitive to price, namely, marginal demanders. Overall, bundling probably combines both.

V. Conclusions

In this paper, we analyze triple play bundling behavior by households, i.e., the bundling of wired telephone, pay television, and broadband internet on one bill from one company. We find that triple play bundling is positively correlated with income, education, and household size, but generally uncorrelated with age.

Beyond these basic results, we present a novel explanation for firms to bundle that is particularly pertinent in recurrent service industries – reduction of churn. We test whether bundling appears to alter households' payoffs from switching by analyzing household-level choices for the telecommunications services that make up the triple play bundle. We find that bundling does reduce churn for all three services. The effect is most pronounced for adoption of these services from the cable company, and the effect is only evident in our data when services experienced "turmoil." More households appear to view broadband as a substitute for television, which implies a smaller net effect of bundling on broadband churn. Finally, we test for screening in supplemental analysis. While not conclusive, these differences suggest that bundlers are a selected sample of households, and therefore may differ on other relevant dimensions, including propensity to switch.

Our findings have several implications. First, they imply that households' utility from purchasing a given service in a given period notably differs if it purchased this service as part of a bundle in the prior period. To the extent that this effect reduces incentives to switch, this could lead to bundling firms earning higher margins on bundling customers than they otherwise would. However, to the extent that bundling creates value for households over time, the net welfare effects are ambiguous. Beyond this, bundling's reduction of churn may dissuade entry in markets with high proportions of bundlers. In such a case, potential entrants may find it challenging to lure households away from their current bundle packages; however, entrants that also offer bundles (e.g., Verizon and AT&T) may not experience such a challenge. Finally, to the extent that bundling serves as a screen, it can be useful for bundling firms in optimizing efforts to retain customers, recognizing that such efforts are best used on non-bundlers.

Our findings also motivate further work on the role of bundling in other contexts. We see a variety of areas where users choose a supplier for services and then periodically consider switching between vendors. Our work motivates further investigation of markets where such user behavior dominates economic conduct. For example, many software markets have increasingly moved away from packaged formats, and toward service models, and bundling practices are quite common in that market. We look forward to further empirical evidence on the relevance of such practices to competitive outcomes.

Endnotes

1. Technological convergence is the tendency for different technological systems to evolve toward performing similar tasks.
2. For example, there has been a long-standing controversy about whether some local telephone firms did or did not offer a full array of mixed bundles in broadband and voice telephone markets and whether consumers would benefit from it if they did. This controversy focused on whether consumers could get DSL service without also subscribing to phone service, so-called "naked DSL." See e.g., http://www.cybertelecom.org/broadband/dslnaked.htm. Our empirical exercise has little to say about the prevalence or relevance of this particular strategy for bundling.
3. If bundling per se lowers households' payoffs from switching, this makes it theoretically possible for the bundled price to exceed the sum of each individual service price. However, in practice we are not aware of any cable firm that prices bundles this way. This is almost certainly due to a lack of market power and the presence of firms offering subsets of services, forcing those that can offer all three to offer mixed bundles, where the bundled price is no higher than the sum of individual prices. It may also be due to forward-looking consumers.
4. For simplicity, we allow bundling to affect utility only for purchases from the cable company. This allows us to capture any switching costs via an increase in the utility for the cable company rather than a decrease in utility for all other providers, and thus provides a more natural link to our econometric model below.
5. In this study we do not investigate the causes behind the decline in demand in much detail. We also follow considerable contemporary commentary and proceed under the assumption that the rise of online entertainment largely causes the decline in demand for pay television, while the rise of cell phones and smart phones largely causes the decline in demand for wired telephone. During this time, there is also a significant shift in demand due to the onset of a major recession. This shift in demand likely exacerbated the above effects.
6. For example, Prince (2011) finds some suggestive evidence about households learning their preferred PC brand over time.
7. This is consistent with other survey data about bundling. In their study of prices for US broadband services, Greenstein and McDevitt (2011) do not begin their price index for bundled services until 2006, the first year when there is sufficient data from which to construct a price series.
8. A DMA is a designated market area. These generally coincide with sizeable cities in the United States.
9. * is significant at 5% level, ** is significant at 1% level.
10. The importance of X being fixed over time will be made explicit below.
11. Since we are using a linear probability model, there is heteroskedasticity by construction. All of our results contain robust standard errors.
12. + is significant at 10% level, * is significant at 5% level, ** is significant at 1% level.
13. These results are available from the authors upon request. We did not include them since in involves a lot of output in order to generate one small observation.
14. It may appear from Table 5 that the rise in the proportion of households with broadband and no TV mirrors the drop in households with TV and no broadband. However, as broadband continued to diffuse during this time, the households newly adopting broadband are likely from the group with TV and no broadband, thus moving them into the group with TV and broadband. The size of the latter group remained constant because pre-existing members concurrently moved to the group with broadband and no TV.
15. Note that Table 7 differs from our analysis in Table 2, since Table 7 is limited to only the subset of households who purchased all three services. In Table 2, the analysis is for all households in our dataset.
16. Note that these are averages when we use ordered rankings for each demographic measure. Specifically, the lowest level is coded as 1 (e.g., income < $25K), the next level as 2 (e.g., $25K < income < $50K), etc.

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