# Choosing the Wrong Calling Plan? Ignorance, Learning, and Risk Aversion

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**Abstract**

It is commonly believed that consumers behave irrationally when subscribing optional telephone tariffs. The fact that they show a biased taste for flat rate options has commonly been interpreted as evidence of irrational behavior since such a choice is frequently not cost minimizing *ex–post*. It is also commonly believed that by offering these calling options telephone carriers increase their revenues immensely mostly due to the wrong choices of consumers. The present paper makes use of excellent data from the 1986 Kentucky tariff experiment to address these issues. Results provide strong evidence in favor of the rationality consumers' choices and indicate that measured tariffs are actually the major source of revenue for telephone carriers. It is found that expectation about future consumption play a major role in the choice of tariff options but also that future consumption forecast errors are more related to the volume of local telephone usage than to any particular demographic profile. JEL: D42, D82, L96.

*Keywords:* Expectation Bias, Tariff Choice, Risk Aversion, Random Effects Probit Model.

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# 1 Introduction

Local telephone service in the US has traditionally been priced by means of a single monthly fee regardless of usage. Contrary to the common practice in Europe and many other countries, in the US calls of any duration and/or distance within the local access transport area (LATA) only generated extra revenues very recently despite the existence of a small but positive marginal costs associated to access, call duration, and distance.

After the break up of AT&T, the Regional Bell Operating Companies (RBOCs) asked for permission to introduce optional local measured service from the corresponding state regulators. RBOCs faced the problem of covering the cost of access and transmission within the local loop fulfilling simultaneously the universal service requirement. Optional calling plans (OCPs) were envisioned both as a way of inducing efficient pricing –bringing marginal charges closer to marginal costs–, and also as a valid instrument to ensure universal service as it provides telephone customers with a menu of nonlinear tariffs from which they could self–select depending on their consumption profile. Thus, low income customers could choose among calling plans that required very low monthly fees, ensuring them to have access to emergency numbers and fulfill their most important communication needs, though at an admitedly high marginal rate.

Since the middle of the eighties the use of optional tariffs have generalized to many other industries including long distance and mobile telephony, internet access, banking, rental cars, and even fitness centers. The problem faced by local telephone regulators at that time (and now in the agenda of European regulators) remains a valid question for all those other industries:[1] How can the introduction of OCPs possibly enhance welfare when customers consistently show such a biased taste for tariff options with zero or very low marginal charges? Telephone business economists and practitioners have tried to explain this phenomenon over the years. Broadly speaking, they agreed on two explanations.

The first explanation is that consumers make frequent mistakes in their choice of tariffs, and by remaining in the same tariff option over time they seem to behave in an irrational manner as they end up paying larger amounts with their chosen tariff than they had paid with many other existing options. Since most consumers choose the optional flat tariff whenever it was available, these practitioners and many telecommunications economists concluded that local telephone carriers make most of their profits out of those irrational consumers that wrongly choose the flat tariff option regardless of their telephone

[1] It should be mentioned that these regulatory concerns are not something of the past. Concerns about consumers being fooled by the number and complexity of tariff options and other pricing practices led the Federal Communications Commission (FCC) and Federal Trade Commission (FTC) to issue a joint Policy Statement on Truth–in–Advertising last March 1st, 2000. These new guidelines followed several complains on *Dial–Around* services (10–10 numbers), and *Casual Billing*. That very same day, the FCC and MCI Worldcom signed a consent decree (order DA 00–446) in which MCI Worldcom agreed to make its advertising policy on *Dial–Around* and rates more transparent plus a voluntari donation to the US Treasure Department of $100,000 while the FCC explicitly refused to prosecute the past practices under the Communications Act of 1934. The market for *Dial–Around* services grew from $m96 in 1993 to $bn3 in 1999.

usage profile.[2] Although it is true that most customers prefer the flat rate option, it is not obvious that this empirical regularity leads to the conclusion that consumers are irrational. This paper shows that in fact most of those who coose the flat tariff option were intensive users and thus their overwhelming preference for flat tariffs responds to a consistent and rational behavior.

There is an additional related issue relevant for public policy and regulation debates: Are those mistakes evenly distributed across the population? If customers with certain characteristics were more likely to make mistakes, then the regulatory approval of OCPs could be further critiziced for not considering that all customers do not have the same ability to make "right" choices, and thus the regulated firm would make money out of those less able to predict their future consumption. The empirical results reported in this paper show that this is not the case and that the ability to predict future consumption is more related its volume than to any particular socio–economic or demographic characteristic.

The second explanation of the overwhelming preference bias in favor of the flat tariff option is that it could only be understood as consumers pursuing a way to minimize risks related to bill variation over time. This argument is however commonly rejected by economists on the basis of the small share of personal income that these potential bill savings represents. It is not difficult to rule out any risk aversion argument when savings from switching to the *ex–post* right tariff option average about \$4.88 out of a representative \$1,600 *per capita* monthly income. The present paper estimates a reduced form and structural model to finally test whether this interpretation can be sustained in any way by actual data.

The rationality and risk aversion hypotheses of consumers' tariff choice behaviour have never been tested before. Miravete (1997, 2000) develops and estimate models of second degree price discrimination when consumers are characterized by having stochastic individual demands. These models reconcile the observed tariff choice with the rationality assumption by distinguishing between different individual consumer type realizations at the tariff choice and purchasing stages. These models also make use of the simplifying risk–neutrality assumption. The rich evidence reported here sustains not only that the preference for the flat tariff option responds to a rational behavior of consumers with different ability to compute their future usage expectations, but also that those consumers who wrongly choose the optional measured service were the ones who generated most of the additional revenues to the local telephone carrier of this study.

The goal of this paper is to study in depth the process of tariff choice. I use the excellent individual data comming from the 1986 local telephone tariff experiment carried out in two cities of Kentucky to address the following specific issues:

[2] See Hobson and Spady (1988), Kridel, Lehman, and Weisman (1993), and Srinagesh (1992). Train, Ben–Akiva, and Atherton (1989) use the same argument to explain the choice of tariff service to pay for domestic electricity consumption while Train, McFadden, and Ben–Akiva (1987) report that telephone customers switch options less frequently than expected from a pure cost minimization perspective.

1 ) What are the determinants of consumers' usage expectation mistakes? Is there any systematic pattern that we could explain with socio–economic information commonly available to econometricians?

2 ) Are consumers rational? What is the role of consumers' own usage expectations on the later choice of tariff options? Could we conclude that local telephone customers behave irrationally when choosing tariffs?

3 ) Do consumers learn from past mistakes? Are those who switch tariffs moving in the right direction? How numerous are they?

4 ) Does consumer choice behavior show any evidence of risk aversion? How is the degree of risk aversion related to commonly observable socio–economic characteristics?

The paper is organized as follows. Section 2 briefly describes the tariff experiment carried out in Louisville and Bowling Green, highlights the underlying type varying model, and studies the relationship between expected and actual local telephone usage. Section 3 tests whether and how individual expectation bias conditions the choice among optimal tariffs, and later focuses on those customers who switch tariff options to determine whether they learn from their past usage profile and tariff choice. Section 4 addresses the issue of risk aversion in the choice of tariffs, first by means of reduced form estimates and later through the use of structural methods. Section 5 concludes.

## 2 Data: Expected and Actual Consumption

In 1984, right after the break up of AT&T and the creation of the RBOCs, South Central Bell (SCB), one of the "Baby Bells," requested permission from the Kentucky Public Service Commission (KPSC) to introduce optional measured service rates. By that time there were serious concerns about the impact of such service on the expenses of local telephone customers. Although most people agreed that optional measured service would probably increase economic efficiency by bringing marginal rates closer to marginal cost, the net effect was however unknown and difficult to approach as local telephone service had never been measured before.

In order to help deciding whether the introduction of optional measured service was welfare enhancing, the KPSC asked SCB to conduct a tariff experiment in two cities of Kentucky during the second half of 1986. The tariff experiment affected the whole population of these two local exchanges, but in addition, during the Spring of 1986, SCB conducted a telephone survey collecting socio–economic and demographic data of about 5,000 households among the customers of Bowling Green and Louisville. Simultaneously, SCB recorded the local monthly telephone usage information of those 5,000 households for March, April, and May. This data collection was very detailed and included total number of calls and minutes of conversation by time of the day, day of the week, and distance

bands within the local exchange as defined by the tariffs that will later be introduced at the beginning of July.

By the time of this data collection all consumers were under a mandatory flat rate regime. They paid \$14.34 in Bowling Green and \$18.70 in Louisville for monthly access, having all local calls a zero marginal charge. During the second half of the year, two differentiated regimes were in place, thus making it possible to later compare their relative performance. After a period of adjustment of three months, SCB collected individual monthly telephone usage information again in October, November, and December.

In Bowling Green, all customers were placed under mandatory measured service. This is equivalent to a single, standard nonlinear tariff that offered some discounts to different consumer profiles. The tariff included a monthly fee of \$8.00, and calls were priced through two–part tariffs. Every call included a constant 1 cent per minute plus 1 cent for setup between 8 a.m. and 8 p.m. on weekdays. Any other time was off–peak and both setup and duration charges were 50% less expensive.

Louisville had a more complicated tariff. First, consumers had the choice to remain on the \$18.70 flat rate option or to switch to a more complicated optional measured service. The measured option included a \$14.02 monthly fee and distinguished between peak, shoulder, and off–peak time bands, as well as two distance bands (from the caller's location). Discounts were not similar for duration and setup although pricing was still a two–part tariff within each time–distance combination. In distance band A, setup and duration had the same price for each time band: 2 cents during peak, 1.3 cents during shoulder, and 0.8 cents during off–peak time. Setup charges where the same for distance band B, but duration charges were doubled. Peak time included weakdays from 8 p.m. to 5 p.m., shoulder between 5 p.m. and 11 p.m. on weekdays and Sundays, while off–peak time was any other time.

Although the experiment was intended only for six months, KPSC was concerned about the impact of the measured rates (mandatory or optional) on customers' telephone bills, and thus decided to impose an upper bill cap of \$21.50 in Bowling Green. In Louisville, where I will focus most of my study, the KPSC decided to return to each customer any money paid in excess for having chosen the wrong tariff given the realized consumption pattern of the household. The return of this money was made after the experiment had ended but Louisville consumers were not aware that this money would be returned to them at the time of the experiment.

An interesting feature of the Louisville tariff, very useful when estimating the model, is that it included a \$5.00 allowance under the measured option. Thus, all customers who, according to the measured option consumed a value of \$5.00 or less were charged nothing in addition to the \$14.02 monthly fee. For at least a range of telephone usage (very much determined by individual habits) consumers faced an effective zero marginal charge. A critical additional second of communication could however costs them \$5.00 extra.

For the case of Louisville there is also evidence of a choiced biased sample that need to be adjusted at the empirical stage. There is a clear disproportion between the percentage of customers that chose the measured service in the sample, 30%, and that of the population, only 10%. Since the selection of the sample was made during the Spring of 1986, before the introduction of tariff options, this divergence can only be explained by SCB having targeted a particular group of customers that, based on its previous knowledge of their calling profiles, were more likely to later choose the measured option, as they actually did. This sampling strategy served the interest of SCB, who could argue in favor of the optional measured rate on the basis of its widespread acceptance.

Observations of non–active customers (those who did not make any phone call in none of the spring or fall months), and of households that did not report all the relevant information were excluded. There is no evidence that this deletion produces any biased results as exclusions for different criteria affect few households and overall deleted observations are balanced conditional on demographics. There is however one exception because around 14% of the households did not report their income. In this case I have recoded these observations at the estimated average annual income level of $19,851 and included a dummy variable to index these cases.[3]

The Appendix describes in detail all variables used in this study and Table 1 presents basic descriptive statistics of the sample stratified by location. These two cities have quite a different demographic structure. Residents in Bowling Green make a significantly higher income and households are larger, including the proportion of teenagers. Households with married couples and college graduates are also more common in Bowling Green than in Louisville. In this city, on the contrary, it is more common to find retired people, people who receives some kind of Federal or State benefits to support their income, and a smaller percentage of households that have moved in the last five years. Racial composition of these cities is also different. Only 6% of the population in Bowling Green, but about 12% of the population in Louisville is black.

There is also a significant difference in usage and expected usage of local telephone service between these two local exchanges. While consumption (measured as weekly number of calls) is higher in Louisville than in Bowling Green, the expected consumption is much more accurate in the latter exchange. On average, Bowling Green residents underestimate telephone usage by 2%, while Louisville residents underestimate their usage by 29%. The difference in magnitude of the bias (type shock of the model discussed below) is remarkable. Perhaps it could be explained by positive network effects of the size of the local exchanges [Taylor (1994, §9)]. While Bowling Green barely reached 50,000 inhabitants by the end of the 1980's Louisville had a population that exceeded 250,000.

## 2.1 The Model

[3] The recoding of the reported income categories into a continuous income indicator is based on a parametric density estimation of a displaced gamma distribution for income. See Appendix 2 of Miravete (1997) for more information about this estimation procedure.

I present now a brief discussion of the underlying consumer choice model. I assume not only that consumer are heterogeneous in their preferences, but also that their demands are stochastic. The main consequence is that when consumers sign up for a particular OCP, they do not commit to a particular level of telephone usage. Thus, consumers may face unforeseen needs of communication for which a different *ex–ante* choice of OCP could have been *ex–post* optimal. The actual choice of OCP is however *ex–ante* efficient because it is made conditional on the information available at that stage, *i.e.*, contingent on consumers' own expectation of their future usage. Without loss of generality, consumers' preferences could be represented by the following indirect utility function:[4]

$$V(y - A, p, \theta), \tag{1}$$

where $y$ is the household monthly income, $A$ is the monthly fixed fee, $p$ the average duration charge, and $\theta$ is a single dimensional index that captures the heterogeneity of consumers' preferences. As for now, this indirect utility function is assumed to be quasilinear in income. Thus income effects are not present and Roy's identity implies that:

$$x(p, \theta) = -V_p(y - A, p, \theta). \tag{2}$$

In order to capture the stochastic nature of individual demands, consumers' types are divided in two components:

$$\theta = \theta_1 + \theta_2, \tag{3}$$

where $\theta_1$ is the *ex–ante* type and $\theta_2$ the type shock. Thus, $\theta_1$ captures the average, steady–state, monthly, local telephone usage while $\theta_2$ represents any unforeseen consumption (positive or negative) at the time of subscribing the tariff option. The fact that $\theta_2$ is unknown to consumers when they subscribe the service make such a choice efficient *ex–ante*, but it also makes possible that the chosen tariff plan is not the least expensive one *ex–post*.

Consumers' types, regardless of whether they are realized or expected, intrinsically represent different satiation levels of local telephone usage. Thus, the last assumption of the model is that demands are bounded. This assumption is particularly important in Louisville because consumers face in most cases a zero marginal charge. Therefore:

$$x(0, \theta) = \theta. \tag{4}$$

Thus, $\theta$, $\theta_1$ and $\theta_2$ can be identified through the actual and expected individual consumption during the spring months of 1986, which is an information readily available in the data set.

### 2.2 Descriptive Statistics

[4] See Miravete (2000) for an in detail description of the model and general characterization of optimal *ex–ante* and *ex–post* nonlinear tariffs when consumers' demands are stochastic.

This section discusses the empirical relationship between expected and actual local telephone usage. The analysis is essentially descriptive and aimed to characterize the nature and source of any consumption expectation bias.

Local telephone usage is multidimensional and defines consumers' profiles by the number, duration of the calls, and its distribution over time, but most of the empirical work of this paper ignores such dimensionality to focus on a single index of telephone usage which is available both *ex–ante* and *ex–post*. In addition to demographics SCB also collected information on customers' own usage expectations. SCB explicitly requested customers' estimates of the average number of weekly calls. This information, available for most households of the sample can be later compared with the actual number of weekly phone calls for every month in the study. When restricted to the spring sample, these two measures are free of any price or selection effect and thus they provide excellent instruments of the actual and expected satiation levels $\theta$ and $\theta_1$ as defined in equation (4).

Figure 1 shows the empirical frequency and distribution functions of the expected and actual number of calls in these two cities of Kentucky during the spring months of 1986. Observe that in both cities there is evidence (stronger in Louisville) in favor of a mean increasing spread of the distribution of $\theta$ relative to that of $\theta_1$. It is evident that the distribution of expected weekly calls is characterized by the accumulation of frequencies on a few "focal points" of the usage range. While the concentration of probability around these focal points could partially explain that telephone customers underestimate their future consumption, there is no reason why these "focal points" could not be shifted upwards some few units (particularly in the case of Louisville) to provide a less biased estimator.[5]

Table 2 presents a stratified analysis not only of the magnitude of the average error of prediction but also of the correlation between expected and actual telephone usage by demographic strata. Thus I can assess whether there is a common pattern of "mistakes" across population groups, and whether there is any characteristic that explains a higher or lower correlation between expected and actual usage.

First, observe that the average bias (actual minus expected number of weekly calls) is positive for customers of these two local exchanges, but it is about seventeen times larger in Louisville than in Bowling Green. A more detailed analysis by demographic strata shows further differences between residents of these two exchanges. While in Louisville the average bias is always positive and large, independently of the demographic characteristic considered, in Bowling Green it is more balanced and in several occasions it takes negative values. In both cities consumers tend to underestimate their future usage, but in Louisville they do this by more than an order of magnitude. The smallest average bias in Louisville (single and male household) is still more than seven times larger than the average bias in Bowling Green. The magnitude of this bias in Louisville indicates that it will probably

[5] See Miravete (2000,§5) for an explicit analysis of first and second order stochastic dominance of actual over expected weekly calls that validate the proposed type–varying model.)

play an important role in the choice of tariff that was later offered in the second half of 1986, and perhaps could explain the commonly observed bias towards the flat tariff option.

Second, local telephone usage expectation bias is significantly heterogeneous by strata although there are also common patterns across cities (always with a significant difference in favor of Louisville residents). Forecast errors tend to be higher for very large households and families with at least two teenagers. This evidence favors the interpretation of monitoring costs in the evaluation of future consumption by households with many members. There is however no significant difference between single households or married couples. Monitoring effects appear to be very strong in households with at least four members. Households with high income (above average in their cities) and who are not retired, do not receive social benefits, or have a college degree tend to make more accurate predictions of their future local telephone usage. The most surprising case for its magnitude is that of black households. In Louisville, where 11% of the population is black, their average bias is almost three times larger than the already high average bias in Louisville. But in Bowling Green, where only 6% of the population is black, their average bias is more than eighteen times larger than the city average. This heterogeneity by strata suggests that individual effects might be important and thus a random effects panel will be estimated in the next section. The magnitude of the bias for black households will be controlled by products of demographics with this indicator so as to identify the source of prediction errors.

Another issue is to determine how informative is the expectation of future telephone usage about the actual future usage level. Table 2 also presents a stratified analysis of the correlations between expected and actual number of weekly calls during the spring months in these two local exchanges. While the difference in average expected usage bias is significant between Louisville and Bowling Green, correlations between expected and actual usage, $\rho(\theta, \theta_1)$, are very much alike: a low 34%. There are not significant correlation differences across demographic strata and/or cities within strata. In general household with higher income and only one member achieve higher correlations, around 40%, while black households get the lowest correlations, 20%, which is the only clear outlier. Finally, while the differences in correlations are small, there is evidence of heterogeneity in predictive power within groups. High income, smaller size, and non–black households predict future consumption significantly better in both cities. Married couples and/or households with a college degree perform better in Louisville, while younger and non–retired households, or those who have not moved in the last five years perform better in Bowling Green.

Thus, there is no clear pattern to link demographics to the ability to predict future consumption with the exception of black households. Besides that, only income and the number of members of the household have a significant effect, positive and negative respectively, on the predictive power of future usage expectations, although much smaller in magnitude. But this stratified analysis is conditional on all other characteristics having no significant effect and ignores the possibility of interactions among them. Furthermore, these preliminary results indicate that prediction ability is most likely explained by unobserved

characteristics. The next section makes use of the three months panel for the Spring of 1986 to control for the effect of such unobservable characteristics and interactions of demographics.

### 2.3 Telephone Usage and Expectation Bias

I now study whether there is in the data any source that explains the difference in ability to predict future consumption across households. I conduct a regression analysis that addresses issues such as nonobservable heterogeneity and/or the critical role of some demographics that appeared to be relevant in the previous descriptive analysis.

Absent any source of risk aversion, Economic Theory has little to add in order to explain the observed pattern of usage expectation bias. It could be argued that if the survey had been repeated several times, consumers had eventually learned their average usage and in the limit rational expectations had been fulfilled, *i.e.*, $E[\theta_2] = \mu_2 = 0$. The three months of data is obviously insufficient to answer this question. However, there is not much variation of type shocks over time within the sample and all consumption expectation bias and correlation results also hold on a monthly base. Furthermore, a repeated tariff experiment could not explain the difference of prior across cities.

A model which includes the number of potential calling parties within the local exchange could perhaps explain the different magnitude of the type shock in Louisville (larger city) and Bowling Green. Given a number of residents within a local exchange limits, potential interactions among them are quadratic on the size of the population. Therefore, we should expect a larger average number of calls in larger cities, where in addition computing expectations becomes more complex. Thus the probability of making prediction mistakes becomes also higher. Some of the results reported in this paper are consistent with this view as intensive consumers coincide with those who make important forecast errors.[6]

Building a model to explain how usage expectations are formed falls beyond the goals of this paper. In this section I am going to limit my analysis to study the effect of different demographic characteristics on the magnitude of the usage expectation bias. In later sections of the paper I will analyze whether the ability to accurately compute future consumption has any effect on the choice of optional tariffs in the second stage of the tariff experiment.

Tables 3a–3b present the results of the regressions of the absolute expectation bias of weekly local telephone calls for each month on a set of demographics as well as on the average number of calls computed for the whole spring sample. The introduction

[6] High income levels and small household sizes characterized those who made small forecast errors, but they are common characteristics of customers who do not make frequent local calls. See Miravete (1997) for demand estimates using the present data set. It is commonly acknowledged that income has a positive effect on demand for long distance calls while its influence on demand for local calls is systematically negative.

of this variable allows to control for the effect of traffic related idiosincracies that are not captured by the available demographics.[7] I have also included cross–products of the three main variables in any telecommunications demand estimation –income, size of the household, and number of teenagers–, to account for possible nonlinearities. Also, given the apparently very different behavior of black households, I have included cross–products of demographics based on this characteristics to break down the specific effects of this variable.

I estimated two models for each city. The first is based on a pooled sample for all spring observations (Table 3a) while the second takes advantage of the panel structure of the data by estimating a one factor random effects linear model to account for the existence of non–observable heterogeneity across individuals (Table 3b).[8] The existence of individual heterogeneity favors the panel estimation over the pool estimation which is always rejected. I will therefore focus on the efficient estimates of the random effects linear model.

Two specifications of the model were estimated for each city, one including cross–products of demographics and racial composition of the household, and another ignoring these racial based dummies. The model without cross–product of demographics and race is always rejected (regardless of whether we consider the pool or random effects panel model). I will therefore focus on the second and fourth column of Table 3b since the effect of race on the calling pattern and accuracy of prediction behavior is not homogeneous across households of the same race; it rather varies depending on other socio–economic and demographic characteristic of the households. Still, black households shows a markedly different behavior and demographics appear to have a differentiated effect than for non–black households, especially in Bowling Green.

Very few variables remain significant once I account for the effect of unobservable heterogeneity. In general, an additional member of the household induces that future consumption is underestimated by an extra 22 weekly phone calls in Bowling Green but only 7 phone calls in Louisville (weakly significant in the latter case). The result is reasonable as in Louisville consumers underestimate future consumption by a much larger magnitude than those in Bowling Green. This relationship is however not linear: income appear to reduce the magnitude of the bias in both cases by 2.8 calls in Bowling Green and 3.4 in Louisville as income doubles. In addition, married couples tend to overestimate their telephone usage consumption (4 calls, weakly significant), and consumption in Louisville appears to show a minor but significant decline in May.

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[7] The use of a constant usage measure of consumption for each individual across the three spring months as explanatory variable allows me to estimate the consumption level effects while preserving the effects of non–observable characteristics not necessarily related to usage intensity.

[8] Estimation of a random effects instead of the fixed effects model is conditioned by the time invariant demographics contained in the data (with the exception of the month indicator). I computed several Chow tests of structural stability, and while there were very significant differences between cities, in general there were not significant differences across months within samples of the same city. Consistently with these results, the two factor (time and individuals) random effects linear model did not improve the estimation.

As of interactions with the racial composition variable, black households show important differences in response to other demographic variables. For instance, in addition to nonlinear effects of the size of the household, number of teenagers, and income, in Bowling Green black households holding a college degree, those who use the telephone for charity, or those who moved recently show a much stronger tendency to overestimate their consumption than non–black households. The pattern is however not repeated in Louisville, a much larger exchange where black households are more numerous, and more importantly, where there is a stronger tendency to overestimate consumption. There, black households underestimate future consumption even more than non–black households when they hold a college degree, are married, and/or receive any kind of social benefits (very low income households).

Since demographics appear not to be very helpfull in explaining the magnitude of the usage expectation bias, I conclude that reported expectations basically capture individuals idiosincratic ability to predict future consumption, so that the constructed usage expectation bias can be used as an indicator of individual computation ability to account for the later choice of tariff option. The results of this section show that there appears not to be a common pattern that explains the accuracy of prediction of future consumption as a function of demographics. Actually they only account for about 25% to 45% of the total explained variance of the bias. The rest is explained by one single variable: the actual usage of local telephone service. Any increase in the average number of phone calls leads to a larger underestimation of future consumption of about 50% of that magnitude in Bowling Green and 75% in Louisville (whose average individual usage, measured by the number of phone calls, is 13% higher than in Bowling Green). This is a critical result for two reasons. First, there appears to exist many variables other than the available demographics that better explain the individual ability to predict future local telephone usage. Second, the magnitude of the prediction error is increasing with the usage of local telephone service. Hence, if those consumers who make important mistakes in predicting their future consumption are also those who make an intensive use of the telephone and they end up choosing the optional flat tariff, we should conclude that they behave rationally since they pick up the tariff option that, on average, is better suited for their consumption profile. This is the main issue studied in the following section.

# 3 Choosing Tariffs: Expectations and Consumption Profile

Some important questions remain unanswered. Has the different ability to predict future consumption any effect on the choice of tariff options? Could it explain why consumers make "wrong" tariff choices given their posterior consumption? Do they really make "wrong" choices? Do they learn their type? Do they switch service towards the "right" option? In the first half of this section I discuss whether the ability to predict future consumption play any central role in the choice of tariff option. In the second half I analyze whether consumers tend to remain on the chosen tariff regardless of their consumption

pattern, or whether they instead switch options to minimize the cost of local telephone usage.

### 3.1 Consumption Misscalculation and Tariff Choice

In this section I study the determinants of the tariff choice in Louisville during the second half of 1986. Besides demographics, I also include as explanatory variable an individual indicator of the average weekly forecast error computed over the three spring months were data was collected. Its square should account for the existence of possible nonlinearities. This indicator has been found to be a good dummy to control for idiosincratic individual ability to predict future telephone usage. It is intuitively appealing that individual expectations of future usage should play a role in the choice of tariffs. Furthermore, this indicator is free of any price effect as all consumers were on mandatory flat rate during spring.[9]

Table 4 reports the effects of demographics on the choice of tariffs plans during the fall months of 1986 in Louisville. The table includes the descriptive statistics of the fall sample and the estimation of two probit models.[10] The estimation of the standard probit model using the pooled sample implicitly assumes that some unobservable characteristics (normally distributed) is driving the choice of tariff. Thus, the choice is made depending on the value of the consumer type $\theta$ conditional on observable demographics. But this approach does not capture the idea that individual demands are stochastic. Consumer types may actually change from one period to the next. There is some evidence that supports this interpretation because about 6% of the fall sample switches tariff plans during the three months of collected data. The estimation of a random effects probit model in the last column shows that the effect of this switching (and therefore of the validity of the type–varying model) is very significant. Thus, to be consistent with the suggested type–varying model, I have also allowed for the existence of non–observable individual effect at the estimation of determinants of the tariff choice. To ease the estimation of the random effects probit model I assume the following error structure:

$$\epsilon_{it} = \nu_{it} + u_i \tag{5}$$

$$\mathrm{Var}[\epsilon_{it}] = 1 + \sigma_u^2 \tag{6}$$

$$\mathrm{Corr}[\epsilon_{it}, \epsilon_{is}] = \varrho = \frac{\sigma_u^2}{1 + \sigma_u^2} \tag{7}$$

[9] It is not be correct to include the contemporaneous usage forecast error during the fall months because telephone consumption would then conditioned by the particular choice of tariff (selection effect) and/or the particular marginal rate that individual consumers face given their accumulated consumption (supression effect). Overall we would encounter serious endogeneity problems in estimating such a model.

[10] I am using only the balanced sample for these months. Only 263 observations are ignored and the estimation of the random effects probit model is simplified greatly. Furthermore, there are not significant differences among the demographic characteristics of these samples and in addition the assumption of different estimates for the balanced and unbalanced pooled sample is clearly rejected with a likelihood ratio test of 2.60, far below the critical value $\chi^2_{0.95}(25) = 37.65$.

so that by distinguishing an individual specific error component, $u_i$, total error terms are equally correlated across time for each individual. The advantage of this approach over a model with a more general correlation pattern is that the likelihood function can be factorized as the product of univariate normal distributions and therefore being numerically integrated by Gaussian quadrature as suggested by Buttler and Moffitt (1982).[11] The estimate of $\varrho$ is significant and indicates the existence of a very strong time correlation of the type shock. Thus, if a customer subscribes the optional measured service and she receives a large positive shock we should expect that on average she keeps receiving those high shocks over time. A rational consumer should therefore switch to the optional flat rate option if the shock is large enough. The effects of the estimates for the average spring bias variable in the random effects probit model are consistent with this interpretation. There are nonlinearities in this variable, and while small forecast errors do not have any effect on the probability of choosing the measured option, as they become larger (perhaps because the volume of calls is also higher) the probability of choosing the measured option declines. This a first strong evidence in favor of the rationality of local telephone customers regarding the choice of tariff plans.

Accounting for unobservable heterogeneity that influences the tariff plan choice improves significantly the efficiency and fitness of the estimation and thus the pool specification is rejected in favor of the random effects probit model. Although the sign of the significant parameters parameters is the same for the two specifications, the pool probit model appears to produce downwards biased estimates. For instance, the existence of teenagers has a clear negative effect on the probability of choosing a measured option while its effect was not significant in the pool estimation. The same negative effect is found for young households (weak), the older age group, black households, and those who receive some type of benefits.[12] The only variable that has a clear positive effect is income. Households with higher income tend to subscribe the optional measured service, a relationship that is actually increasing in income (which appears to be the right choice as they consume less than the average). In addition only the time dummies show a significant

[11] The reported maximum likelihood estimates have been obtained using the six–point Gauss–Hermite quadrature and BHHH algorithm. Nodes and weights for the quadrature can be found in Stroud and Secrest (1966, Table V). To check for robustness of the results I have also estimated the model with 2, 4, and 8 quadrature nodes. Parameters are stable and the hypothesis testing results are basically the same, regardless of the number of quadrature nodes used.

[12] As I explained before, the sample is choice based. Thus, the likelihood function has to be modified to correct the proportions of consumers choosing each option so that results could be representative of the population. The t–statistics of Table 4 are obtained from a sample–weight corrected covariance matrix as suggested by Manski and Lerman (1977). The estimation of the random effects probit model poses an additional difficulty because of the existence of switching, so that the proportions of consumers who choose measured service in the sample to those who choose it in the population is changing over time. However, maximum likelihood estimation requires a single value of the weighting variable associated to each individual. Since I only have information about the proportions of the population that chose each option and not of the transition probabilities associated to the switching between options, I arbitrarily decided to estimate this model with the weights of October for every consumer regardless of whether they later switched options or not. There is not much variation of the ratio between sample and population proportions of consumers that chose each option, and results are robust to this assumption as compared to using the weights of November or December.

positive effect. This result may indicate that switching from flat towards measured service is more significant than movements in the opposite direction. This is a second piece of evidence that consumers are rational, that they evaluate cost differences of each plan, and that contrary to the traditional readings of the evidence in this industry, that consumers do not particularly remain under the flat tariff option independently of their usage profile. The section on risk aversion addresses the issue of whether those who switch are most of those who should switch given their consumption.

The previous section showed that there is little correlation between demographics and the ability to predict future consumption. However, the lack of accuracy in computing future consumption does not have the perverse effect of inducing general mistakes at the tariff choice level. This is because those customers who are less able to compute their future usage are those with high usage demand, for whom the flat tariff option is also the least expensive one. This evidence is additional to traditional explanations of the bias towards flat rate options based on inertia from previous mandatory flat rate pricing, ignorance of the introduction of new tariff options, or price responsiveness [Kling and Van Der Ploeg (1990, §6)].

So are consumers irrational? Do the monopolist make huge profits out of those telephone customers who wrongly chose the flat tariff option? Depending on the actual volume of telephone usage, consumers can be clasified *ex–post* as having chosen correctly or incorrectly each tariff option. This classification is made contingent on the same usage pattern independent of price responses, which, although perhaps not totally correct, provides with an upper bound of the gains of switching to a different tariff option. Table 5 reports the demographics of those four groups of customers ordered by average usage (weekly calls during the fall months). The comparison with the descriptive statistics of Table 4 not only confirms the important heterogeneity of effects of each variable, but also quantifies the magnitude in which each demographic characteristic is over or underepresented within each tariff–consumption category. Thus, for instance, those customers that wrongly choose the measured option make a higher income while those who correctly choose the flat tariff option belong to households with many members, including teenagers.

Table 5 also includes the average maximum potential savings (positive or negative) that will be accounted for if consumers had chosen the other option and made the same use of local telephone (ignoring price effects). This indicator gives a first approximation to the potential money transfers between customers and the local monopolist due to wrong tariff choices. Focusing on the customers who chose the right tariff, those on the flat rate option were saving almost four times as much as those on measured service. On the contrary, among those who made the wrong choice, those choosing the measured service paid 50% more than who chose the flat tariff option.

Those consumers who chose the flat option (90% of the population in the Louisville exchange) did not pay attention to pricing in their calling behavior, and telephone usage reached its household specific satiation level (different at each month due to the existence of individually stochastic demand). Surprisingly, they most likely made the right choice.

Out of the 3,410 Louisville customers who chose the optional flat option, only 394 (11.6%) made the wrong choice and would had paid less for their local telephone service if they had consumed the same, ignoring price effects, on the alternative option. Their maximum potential savings is exactly \$4.60, the difference between the monthly fee of the optional flat rate, \$18.70, and that of the optional measured service, \$14.02.[13] On the contrary, the other 3,106 (88.4%) customers who correctly chose the flat option would had paid an average of \$16.77 extra if they had chosen the measured option and had kept their calling pattern unchanged. On the other hand, out of 1,479 customers of this sample who chose the measured option, 630 (42.6%) chose correctly that alternative. They were all, low demand customers that otherwise would had paid \$4.68 extra if they had chosen the flat option. However, the other 849 (47.4%) measured service customers would had saved on average \$6.68 if they had switched to flat tariff.

Therefore, the evidence from the tariff experiment in Kentucky is opposed to the idea that a large proportion of telephone customers systematically prefer the optional flat rate option, independently of their telephone usage. For instance the percentage of customers who wrongly chose the flat tariff option from an expenditure minimization point of view is relatively low –about 12%–, while those who mistakenly chose the measured option represent a much larger proportion: 47% of those who chose the measured service. After correcting for choiced biased sample, the number of customers that wrongly chose the flat option was less than double the number of customers who made the mistake of choosing the measured tariff. Overall, only 16% made the wrong choice, and while it is true that 64% of them (after correcting for choice sampling), chose the flat tariff, it is also true that maximum loss is also bounded in that case, *i.e.*, \$4.86. Thus, the expected gain from a customer that chooses a plan incorrecly is not that different for each tariff: \$2.4 for those choosing measured service and \$3.11 for those who prefer the flat option.

### 3.2 Learning the Right Tariff Choice

Around 6% of the sample switched tariff options during the three months of data collection in the fall of 1986 in Louisville. This is obviously not a long enough period to test whether there is important learning. Furthermore, any estimates of the learning effects could seriously be biased downwards as the data collection took place after three months of adjustment period in which most of the learning could have taken place. It is however the best data available, and can be used to give an idea of whether this option switching followed any consideration regarding potential savings that could be accomplished by signing up for a different option.

In Table 6 I present the results of three probit models. In the first column, the endogenous variable takes a value equal to one when the customer switches options between October and December. In addition to demographics, the maximum potential savings that

[13] Savings are exactly \$4.60 because all these 394 customers had very low demands and none of them would had exhausted the \$5.00 allowance of the optional measured service.

could have been attained by choosing the other tariff option in October is also included. A significant positive estimate for this variable would indicate that consumers react optimally, and that on average they end up choosing the right tariff in the long run. The other two columns explore the posibility that this learning process is asymmetric, *i.e.*, in the second colmumn it is analyzed why customers that started with the flat tariff option in October ended up choosing the measured option in December. In the third column the opposite case is analyzed. In principle we can expect different effects because while consumers under the optional measured service learn how much they pay and can thus compare with the alternative flat rate option, those on this latter tariff plan only have an expectation of how much would they have to pay for their usage, but they have first to experiment switching options to confirm those expectations and be able to compare how expensive each tariff is for them.

# 4 Conclusions

This paper has addressed whether a monopolist can screen consumers even if their individual demands are stochastic. For tractability considerations, I restrict my attention to the case where where consumer taste parameters remain single–dimensional in order to study the conditions under which optional nonlinear tariffs would be characterized by quantity discounts when consumers buy several units of the same product.

This paper shows that the characterization of optional nonlinear tariffs is a straightforward sequential application of the "Revelation Principle." But furthermore, it shows that demand conditions do not play a major role in obtaining the desired quantity discounts results. The main result of this paper is to prove that by assuming that the distribution of all type components is IHR, we ensure that the optimal menu of optional nonlinear tariffs will be characterized by quantity discounts. The paper also shows that in the case of nonlinear tariff options the distribution of the shock has to be sufficiently enough IHR to ensure the concavity of each nonlinear tariff option offered by the monopolist.

The implications of the model have been tested using a unique data set that includes direct observations of consumer types. The hypothesis of individual type variations is strongly supported by the data (SOSD). However, only in one of the subsamples future consumption is systematically underestimated (FOSD). The paper shows that under these conditions, it is optimal for the monopolist to charge a higher mark–up for every purchase level, and thus expected monopoly revenues will be higher with the *ex–post* optional nonlinear tariff than with the *ex–ante* standard nonlinear pricing ("pay–as–you–go").

As mentioned in previous sections, optional nonlinear pricing has not attracted much attention among economists, who have incorrectly extended the application of results of the standard nonlinear pricing theory to situations where consumption and tariff choice are not simultaneous. The early treatment of Clay, Sibley, and Srinagesh (1992) studied the design of optimal two–part tariffs, but restricting the attention to the discrete type

case and drastically limiting the range of variation of $\theta_2$ to ensure that the same SCP held both *ex–ante* and *ex–post* so that the ordering of individual consumer preferences remain unaltered after the realization of the shock. Miravete (1996) extended this model to the case of a continuum of two–part tariff options with a continuum of types, independently of whether the ordering of consumer tastes changed or not after the realization of the shock, and Miravete (1997) used a particular closed form solution of this model to analyze the estimation bias of not dealing with asymmetric information and self–selection issues in a cross–section framework. Finally, Courty and Li (1998) analyze a general model of sequential screening with a continuum of types but limiting the analysis to consumers with unit demands and biased type shocks in the sense of FOSD.

Relative to all these works, the present paper contributes by characterizing a fully nonlinear tariff when consumers buy more than one unit, and by making explicit the role of the statistical assumptions on the existence of quantity discounts (IHR of the distribution of type components), and welfare effects (FOSD and SOSD of $\theta$ over $\theta_1$). This paper also compares different optimal nonlinear tariffs depending on whether they are designed *ex–ante* or *ex–post*, through the preservation of the IHR property of the distribution of type components through convolution. Finally, the paper also contributes to this literature by providing very strong evidence in favor of the suggested type–varying model based on direct observation of consumer types.

The results of this paper help explaining the widespread use of tariff discounts embodied into tariff options in several monopolistic and competitive markets. But the suggested solution also opens the possibility of theoretical extensions to other agency problems where individual stochastic components of moral hazard or adverse selection parameters could also be present. Restricting my attention to common nonlinear pricing issues, there are no significant difficulties in extending the present model to address for example the case of firms that compete through the design of optional nonlinear tariffs. This could be done along the line of the papers by Armstrong and Vickers (1998) or Stole (1995) for the case of differentiated products, or Rochet and Stole (1998) when the competing firms sell an homogeneous good.

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# Appendix

- **Description of Variables**

The data set includes the following variables. Most of them are dummies that take value equal to 1 for the indicated case:

| | |
|---|---|
| AGE1 | The head of the household is between 15 and 34 years old. |
| AGE2 | The head of the household is between 35 and 54 years old. |
| AGE3 | The head of the household is at least 54 years old. |
| APRIL | Observation for the month of April 1986. |
| BENEFITS | The household receives some benefits such as Food Stamps, Social Security, Federal Rent Assistance, Aid to Families with Dependent Children,... |
| BIAS | *Calls* – *Expcalls*. |
| BILL | Total monthly expenditure in local telephone service. |
| BLACK | The head of the household belongs to the black ethnic group. |
| CALLS | Monthly average of weekly number of actual calls. |
| CHURCH | Some member of the household uses the telephone for charity or church work. |
| COLLEGE | The head of the household is at least a college graduate. |
| DECEMBER | Observation for the month of December 1986. |
| DINCOME | The household did not report its annual income. |
| EXPCALLS | Expected number of weekly calls. |
| HHSIZE | Number of people who regularly live in the household. |
| INCOME | Estimated total monthly income of the household. |
| LVOMT | Value of the Spring consumption according to the Fall optional measured tariff in Louisville (regardless of the actual choice). |
| MARCH | Observation for the month of March 1986. |
| MARRIED | The head of the household is married. |
| MAY | Observation for the month of May 1986. |
| MEASURED | The household is on local measured service in one particular month. |
| MOVED | The household moved at least once in the last five years. |
| MWCALLS | Spring average of weekly number of actual calls. |
| NOVEMBER | Observation for the month of November 1986. |
| OCTOBER | Observation for the month of October 1986. |
| ONLYMALE | The head of the household is single and male. |
| RETIRED | The head of the household is retired. |
| SAVINGS | Potential change in monthly telephone bill if the household had switched to the other tariff option (ignoring price effects). |
| TEENS | Number of teenagers (between 13 and 19 years old) living in the household. |

## Table 1. Descriptive Statistics

| | Bowling Green (S) | | Louisville (S) | | Louisville (F) | | TEST |
|---|---|---|---|---|---|---|---|
| CALLS | 32.0489 | (26.902) | 36.6112 | (38.197) | 36.1107 | (38.178) | -6.63 |
| EXPCALLS | 31.4137 | (36.123) | 25.9329 | (30.827) | 26.2311 | (33.176) | 8.02 |
| BIAS | 0.6352 | (37.179) | 10.6783 | (39.966) | 9.8796 | (41.799) | -12.64 |
| INCOME | 7.3097 | (0.798) | 7.0847 | (0.819) | 7.0505 | (0.839) | 13.55 |
| HHSIZE | 2.7960 | (1.266) | 2.5381 | (1.493) | 2.5075 | (1.471) | 9.02 |
| TEENS | 0.3711 | (0.713) | 0.2309 | (0.619) | 0.2399 | (0.624) | 10.31 |
| DINCOME | 0.1328 | (0.339) | 0.1603 | (0.367) | 0.1530 | (0.360) | -3.78 |
| AGE1 | 0.0614 | (0.240) | 0.0625 | (0.242) | 0.0927 | (0.290) | -0.22 |
| AGE2 | 0.2524 | (0.434) | 0.2644 | (0.441) | 0.2767 | (0.447) | -1.33 |
| AGE3 | 0.6861 | (0.464) | 0.6730 | (0.469) | 0.6306 | (0.483) | 1.37 |
| COLLEGE | 0.2803 | (0.449) | 0.2244 | (0.417) | 0.2260 | (0.418) | 6.30 |
| MARRIED | 0.6926 | (0.462) | 0.5059 | (0.500) | 0.4899 | (0.500) | 18.85 |
| RETIRED | 0.1525 | (0.360) | 0.2550 | (0.436) | 0.2293 | (0.420) | -12.40 |
| BLACK | 0.0622 | (0.242) | 0.1168 | (0.321) | 0.1268 | (0.333) | -9.25 |
| CHURCH | 0.2082 | (0.406) | 0.1692 | (0.375) | 0.1608 | (0.367) | 4.87 |
| BENEF | 0.2063 | (0.405) | 0.3152 | (0.465) | 0.2964 | (0.457) | -12.12 |
| MOVED | 0.4820 | (0.500) | 0.4074 | (0.491) | 0.4543 | (0.498) | 7.34 |
| ONLYMALE | 0.0452 | (0.208) | 0.1053 | (0.307) | 0.1141 | (0.318) | -10.99 |
| MARCH | 0.3288 | (0.470) | 0.3325 | (0.471) | | | -0.39 |
| APRIL | 0.3318 | (0.471) | 0.3318 | (0.471) | | | 0.00 |
| MAY | 0.3394 | (0.474) | 0.3357 | (0.472) | | | 0.38 |
| OCTOBER | | | | | 0.3324 | (0.471) | |
| NOVEMBER | | | | | 0.3334 | (0.472) | |
| DECEMBER | | | | | 0.3342 | (0.472) | |
| Observations | 5241 | | 4349 | | 4889 | | |

Mean and standard deviations (between parentheses) of demographics for the spring (S) and fall (F) samples. The column “TEST” shows the test of differences of means for each variable in these two cities during the spring months.

## Table 2. Correlation Between Expected and Acual Consumption

| Bowling Green | | | | | Louisville | | | |
|---|---|---|---|---|---|---|---|---|
| Obs. | $\rho$ t–Stat. | TEST | Avg.Bias Std.Dev. | STRATA | Avg.Bias Std.Dev. | $\rho$ t–Stat. | TEST | Obs. |
| 5241 | 0.3325 (25.52) | | 0.6352 (37.179) | ALL | 10.6783 (39.966) | 0.3448 (24.22) | | 4249 |
| 1723 | 0.3430 (15.15) | 0.62 | 0.9765 (37.076) | MARCH | 11.6001 (43.581) | 0.3110 (12.44) | 5.04 | 1446 |
| 1739 | 0.3352 (14.83) | | 0.6571 (37.014) | APRIL | 10.5580 (39.119) | 0.3482 (14.10) | | 1443 |
| 1779 | 0.3198 (14.23) | | 0.2834 (37.457) | MAY | 9.8842 (36.946) | 0.3843 (15.89) | | 1460 |
| 1967 | 0.2859 (13.23) | 10.05 ** | 2.9062 (39.662) | LOW INCOME | 15.9668 (50.592) | 0.3031 (12.89) | 16.81 ** | 1645 |
| 3274 | 0.3667 (22.55) | | -0.7291 (35.541) | HIGH INCOME | 7.4610 (31.388) | 0.4147 (23.69) | | 2704 |
| 714 | 0.4037 (11.77) | 22.08 ** | 0.0920 (18.198) | HHSIZE=1 | 6.2131 (34.470) | 0.3669 (13.04) | 13.02 ** | 1095 |
| 1774 | 0.3267 (14.55) | | -1.1249 (30.470) | HHSIZE=2 | 6.4538 (27.637) | 0.3285 (13.47) | | 1502 |
| 1290 | 0.2885 (10.81) | | 2.9518 (33.353) | HHSIZE=3 | 13.8281 (38.995) | 0.3210 (9.43) | | 776 |
| 980 | 0.3025 (9.92) | | -0.0021 (47.312) | HHSIZE=4 | 14.3265 (43.909) | 0.2317 (5.74) | | 582 |
| 483 | 0.1580 (3.51) | | 3.0087 (59.734) | HHSIZE $\geq$ 5 | 27.6001 (71.748) | 0.2192 (4.45) | | 394 |
| 3798 | 0.2983 (19.26) | 3.38 | -0.3655 (29.838) | TEENS=0 | 7.5578 (35.786) | 0.3125 (19.87) | 5.02 | 3653 |
| 1029 | 0.2628 (8.73) | | 0.9405 (54.873) | TEENS=1 | 23.4185 (47.131) | 0.2179 (4.78) | | 460 |
| 414 | 0.3587 (7.80) | | 9.0571 (42.156) | TEENS $\geq$ 2 | 34.1479 (65.503) | 0.2459 (3.88) | | 236 |
| 322 | 0.4244 (8.38) | 14.77 ** | -4.7589 (26.910) | AGE1=1 | 8.4026 (32.578) | 0.4074 (7.33) | 1.82 | 272 |
| 1323 | 0.2638 (9.94) | | -2.7377 (42.171) | AGE2=1 | 9.0469 (38.949) | 0.3544 (12.84) | | 1150 |
| 3596 | 0.3626 (23.33) | | 2.3592 (35.866) | AGE3=1 | 11.5307 (40.955) | 0.3365 (19.33) | | 2927 |
| 1469 | 0.3642 (14.98) | 1.73 | -3.4543 (37.277) | COLLEGE=1 | 4.6580 (28.899) | 0.4766 (16.92) | 24.65 ** | 976 |
| 3772 | 0.3285 (21.36) | | 2.2279 (37.024) | COLLEGE=0 | 12.4203 (42.480) | 0.3256 (19.99) | | 3373 |
| 3630 | 0.3483 (22.38) | 3.53 | 0.5463 (36.427) | MARRIED=1 | 10.6344 (32.603) | 0.3824 (19.40) | 5.00 * | 2200 |
| 1611 | 0.2978 (12.52) | | 0.8355 (12.52) | MARRIED=0 | 10.7232 (46.315) | 0.3230 (15.81) | | 2149 |
| 799 | 0.1971 (5.68) | 14.78 ** | 1.3146 (28.672) | RETIRED=1 | 9.6512 (35.496) | 0.3717 (13.32) | 1.46 | 1109 |
| 4442 | 0.3343 (23.64) | | 0.5130 (38.512) | RETIRED=0 | 11.0299 (41.384) | 0.3349 (20.23) | | 3240 |
| 326 | 0.2064 (3.80) | 8.57 ** | 11.6811 (71.411) | BLACK=1 | 29.3614 (66.110) | 0.1968 (4.51) | 22.15 ** | 508 |
| 4915 | 0.3606 (27.10) | | -0.0974 (33.587) | BLACK=0 | 8.2073 (34.340) | 0.3987 (26.94) | | 3841 |
| 1091 | 0.2882 (9.93) | 4.02 * | -1.8867 (45.088) | CHURCH=1 | 7.8696 (52.922) | 0.3477 (10.05) | 0.02 | 736 |
| 4150 | 0.3495 (24.03) | | 1.2982 (34.779) | CHURCH=0 | 11.2505 (36.754) | 0.3424 (21.90) | | 3613 |
| 1081 | 0.2711 (9.25) | 5.30 * | 2.2926 (35.188) | BENEFITS=1 | 13.8292 (42.011) | 0.3804 (15.22) | 0.99 | 1371 |
| 4160 | 0.3423 (23.49) | | 0.2046 (37.671) | BENEFITS=0 | 9.2277 (38.910) | 0.3522 (20.53) | | 2978 |
| 2526 | 0.3088 (16.31) | 4.76 * | 0.0820 (40.646) | MOVED=1 | 10.7220 (39.305) | 0.3411 (15.27) | 0.03 | 1772 |
| 2715 | 0.3624 (20.25) | | 1.1500 (33.634) | MOVED=0 | 10.6482 (40.422) | 0.3461 (18.72) | | 2577 |
| 237 | 0.4275 (7.25) | 2.91 | -3.5797 (23.912) | ONLYMALE=1 | 4.6319 (27.237) | 0.3954 (9.19) | 1.65 | 458 |
| 5004 | 0.3300 (24.72) | | 0.8349 (37.682) | ONLYMALE=0 | 11.3900 (41.151) | 0.3404 (22.58) | | 3891 |

Correlation between expected and actual number of telephone calls. The t–statistics of the correlation coefficients $\rho(\theta, \theta_1)$ have been computed using Fisher's $z$–transformation. Column "TEST" presents a $\chi^2(k-1)$ test of equality of all correlations for each group of variables where $k$ is the number of categories within each group [Hays (1994, p. 651)]. All statistics with p–values less than 0.05 are marked (*), and those with p–values less than 0.01 with (**). Average Bias is measured as difference between weekly actual and expected number of calls during the spring months of 1986.

## Table 3a. Consumption Expectation Bias. Pool

| | Bowling Green | | | | Louisville | | | |
|---|---|---|---|---|---|---|---|---|
| Constant | -23.9737 | (1.17) | -29.0073 | (1.38) | 11.7211 | (0.71) | 28.4967 | (1.57) |
| INCOME | 0.8671 | (0.14) | 2.0034 | (0.32) | -4.6490 | (0.93) | -9.2241 | (1.69) |
| HHSIZE | 19.3946 | (5.57) | 22.3421 | (6.11) | 7.2715 | (3.44) | 6.9686 | (2.69) |
| TEENS | -0.7145 | (0.10) | -3.3248 | (0.43) | -7.5778 | (1.34) | -24.1941 | (3.30) |
| DINCOME | 0.1279 | (0.11) | -0.4245 | (0.35) | 0.2824 | (0.31) | 0.4966 | (0.53) |
| INCOME2 | 0.2949 | (0.64) | 0.3108 | (0.65) | 0.3749 | (0.98) | 0.6869 | (1.65) |
| HHSIZE2 | -0.3307 | (1.37) | 0.0380 | (0.15) | -0.1109 | (2.02) | -0.0737 | (1.15) |
| TEENS2 | -0.1925 | (0.77) | -0.2319 | (0.91) | 0.1236 | (0.53) | -0.1878 | (0.77) |
| INCOME*HHSIZE | -2.1533 | (4.72) | -2.8799 | (5.89) | -0.8440 | (2.83) | -0.9235 | (2.61) |
| INCOME*TEENS | 0.2348 | (0.24) | 0.7079 | (0.68) | 1.2309 | (1.62) | 3.5670 | (3.61) |
| HHSIZE*TEENS | -0.1930 | (0.38) | -0.5425 | (1.03) | -1.1646 | (3.56) | -1.0293 | (3.00) |
| AGE1 | 1.4293 | (1.41) | 2.4394 | (2.34) | -3.0006 | (3.43) | -1.9740 | (2.10) |
| AGE3 | -1.1901 | (0.89) | -0.9791 | (0.73) | 0.1670 | (0.16) | -0.0294 | (0.03) |
| COLLEGE | 0.2965 | (0.32) | -0.6663 | (0.72) | -1.6023 | (1.99) | -0.8374 | (0.99) |
| MARRIED | -4.2271 | (3.65) | -3.7305 | (3.15) | -3.4490 | (4.12) | -2.1388 | (2.40) |
| RETIRED | 2.6935 | (1.75) | 1.8026 | (1.17) | 0.1591 | (0.15) | -0.0784 | (0.07) |
| BLACK | 6.5427 | (3.99) | 213.6374 | (2.65) | 3.1147 | (3.02) | -81.5453 | (1.76) |
| CHURCH | 2.4425 | (2.51) | 0.6997 | (0.71) | 1.5051 | (1.79) | 2.1685 | (2.40) |
| BENEFITS | 1.1269 | (0.83) | 1.4371 | (1.04) | -1.8279 | (1.96) | -0.9172 | (0.91) |
| MOVED | 0.2106 | (0.25) | -0.6445 | (0.74) | -0.1742 | (0.23) | 0.2276 | (0.28) |
| ONLYMALE | -2.2481 | (1.12) | -1.8513 | (0.89) | -0.4488 | (0.39) | -0.8194 | (0.67) |
| APRIL | 0.1655 | (0.18) | 0.1886 | (0.20) | -0.0549 | (0.07) | -0.0125 | (0.02) |
| MAY | 0.2067 | (0.22) | 0.1930 | (0.21) | -0.7246 | (0.95) | -0.7007 | (0.93) |
| MWCALLS | 0.5008 | (31.03) | 0.4965 | (30.99) | 0.7332 | (77.52) | 0.7322 | (77.07) |
| BLACK*INCOME | | | -62.9189 | (2.68) | | | 19.7547 | (1.36) |
| BLACK**HHSIZE | | | 0.2841 | (0.02) | | | 15.3033 | (2.81) |
| BLACK*TEENS | | | -38.9588 | (1.70) | | | 29.0197 | (2.08) |
| BLACK*DINCOME | | | 17.8332 | (3.20) | | | -0.5650 | (0.18) |
| BLACK*INCOME2 | | | 3.8437 | (2.17) | | | -1.0021 | (0.87) |
| BLACK*HHSIZE2 | | | -2.6868 | (3.50) | | | 0.2217 | (1.30) |
| BLACK*TEENS2 | | | -12.5968 | (3.51) | | | 5.4690 | (2.96) |
| BLACK*INCOME*HHSIZE | | | 2.6181 | (1.81) | | | -1.8238 | (2.16) |
| BLACK*INCOME*TEENS | | | 6.2941 | (2.09) | | | -3.5480 | (1.96) |
| BLACK*HHSIZE*TEENS | | | 6.0575 | (2.61) | | | -4.1293 | (2.95) |
| BLACK*AGE1 | | | -13.8920 | (3.66) | | | -6.3948 | (2.46) |
| BLACK*AGE3 | | | -19.7358 | (2.40) | | | -2.5278 | (0.73) |
| BLACK*COLLEGE | | | 31.3188 | (5.73) | | | -8.8625 | (3.24) |
| BLACK*MARRIED | | | -0.1062 | (0.02) | | | -8.9380 | (3.37) |
| BLACK*RETIRED | | | 24.4551 | (2.74) | | | 6.5164 | (1.64) |
| BLACK*CHURCH | | | 26.0327 | (6.46) | | | -1.9024 | (0.78) |
| BLACK*BENEFITS | | | -12.3658 | (2.11) | | | -8.6736 | (3.23) |
| BLACK*MOVED | | | 12.9761 | (3.70) | | | -3.5661 | (1.61) |
| BLACK*ONLYMALE | | | 0.4123 | (0.06) | | | 2.7253 | (0.79) |
| Observations | 5241 | | 5241 | | 4349 | | 4349 | |
| $R^2$ | 0.223 | | 0.254 | | 0.650 | | 0.657 | |

The endogenous variable is the absolute difference between the actual and expected number of weekly calls during Spring of 1986. Income is measured in logarithm of thousand dollars. Estimation method is OLS. Absolute t–statistics are shown between parentheses.

## Table 3b. Consumption Expectation Bias. R.E. Panel

| | Bowling Green | | Louisville | |
|---|---|---|---|---|
| Constant | -23.7434 (0.70) | -26.8940 (0.77) | 10.3624 (0.39) | 27.1186 (0.94) |
| INCOME | 1.4499 (0.15) | 1.9666 (0.19) | -4.1950 (0.53) | -8.7067 (1.00) |
| HHSIZE | 18.8811 (3.33) | 22.1552 (3.67) | 7.2084 (2.16) | 6.6800 (1.63) |
| TEENS | -0.2656 (0.02) | -3.2395 (0.26) | -7.9531 (0.89) | -23.2183 (2.02) |
| DINCOME | -0.0832 (0.04) | -0.6156 (0.30) | 0.1209 (0.08) | 0.1733 (0.11) |
| INCOME2 | 0.2164 (0.29) | 0.2801 (0.35) | 0.3427 (0.56) | 0.6431 (0.97) |
| HHSIZE2 | -0.3866 (0.97) | 0.0173 (0.04) | -0.0963 (1.08) | -0.0693 (0.66) |
| TEENS2 | -0.0837 (0.20) | -0.2112 (0.49) | 0.0830 (0.22) | -0.1474 (0.37) |
| INCOME*HHSIZE | -2.0438 (2.75) | -2.8391 (3.50) | -0.8725 (1.85) | -0.9054 (1.62) |
| INCOME*TEENS | -0.0146 (0.01) | 0.6644 (0.39) | 1.3507 (1.12) | 3.3993 (2.19) |
| HHSIZE*TEENS | 0.1263 (0.15) | -0.5074 (0.57) | -1.2491 (2.38) | -1.0210 (1.84) |
| AGE1 | 1.3212 (0.78) | 2.4346 (1.40) | -2.7214 (1.95) | -1.7166 (1.14) |
| AGE3 | -1.5512 (0.69) | -1.2391 (0.55) | 0.4701 (0.28) | 0.4050 (0.22) |
| COLLEGE | 0.4619 (0.30) | -0.4770 (0.31) | -1.3711 (1.07) | -0.6040 (0.45) |
| MARRIED | -4.5047 (2.34) | -3.7961 (1.92) | -3.2496 (2.44) | -1.9986 (1.40) |
| RETIRED | 2.5527 (0.99) | 1.8016 (0.69) | -0.0911 (0.05) | -0.3042 (0.17) |
| BLACK | 6.2817 (2.32) | 184.4732 (1.38) | 2.6052 (1.61) | -79.3341 (1.06) |
| CHURCH | 2.3609 (1.44) | 0.5396 (0.32) | 1.4875 (1.10) | 2.1649 (1.48) |
| BENEFITS | 1.3652 (0.60) | 1.4295 (0.62) | -2.1030 (1.41) | -1.2923 (0.80) |
| MOVED | 0.3123 (0.22) | -0.5942 (0.41) | 0.2625 (0.22) | 0.6834 (0.53) |
| ONLYMALE | -1.4911 (0.45) | -0.9738 (0.28) | -0.2906 (0.16) | -0.6387 (0.33) |
| APRIL | 0.1675 (0.70) | 0.1676 (0.78) | -0.0260 (0.09) | -0.0219 (0.08) |
| MAY | -0.1625 (0.68) | -0.1681 (0.78) | -0.7659 (2.67) | -0.7640 (2.78) |
| MWCALLS | 0.5028 (41.93) | 0.5019 (45.43) | 0.7445 (82.43) | 0.7450 (84.84) |
| BLACK*INCOME | | -54.5670 (1.40) | | 18.3844 (0.79) |
| BLACK**HHSIZE | | 2.5013 (0.13) | | 16.2143 (1.88) |
| BLACK*TEENS | | -53.7900 (1.43) | | 29.8446 (1.33) |
| BLACK*DINCOME | | 17.8372 (1.89) | | 0.7387 (0.15) |
| BLACK*INCOME2 | | 3.3475 (1.14) | | -0.8640 (0.47) |
| BLACK*HHSIZE2 | | -2.4265 (1.93) | | 0.2884 (1.07) |
| BLACK*TEENS2 | | -11.8541 (1.98) | | 5.3815 (1.79) |
| BLACK*INCOME*HHSIZE | | 1.9645 (0.86) | | -2.0065 (1.51) |
| BLACK*INCOME*TEENS | | 7.6274 (1.55) | | -3.1131 (1.08) |
| BLACK*HHSIZE*TEENS | | 7.3170 (1.94) | | -4.9122 (2.28) |
| BLACK*AGE1 | | -14.1161 (2.22) | | -6.3507 (1.55) |
| BLACK*AGE3 | | -20.5445 (1.48) | | -2.9702 (0.53) |
| BLACK*COLLEGE | | 30.1137 (3.40) | | -8.7580 (2.04) |
| BLACK*MARRIED | | -1.3372 (0.18) | | -8.4800 (1.99) |
| BLACK*RETIRED | | 23.9338 (1.58) | | 6.8949 (1.07) |
| BLACK*CHURCH | | 27.2188 (4.02) | | -2.1477 (0.55) |
| BLACK*BENEFITS | | -11.4768 (1.18) | | -8.4958 (1.96) |
| BLACK*MOVED | | 12.4445 (2.11) | | -3.8608 (1.09) |
| BLACK*ONLYMALE | | -1.5799 (0.14) | | 3.3400 (0.62) |
| Observations | 5241 | 5241 | 4349 | 4349 |
| $R^2$ | 0.223 | 0.254 | 0.649 | 0.657 |
| LM–Test | 4571.08 | 4545.40 | 3339.35 | 3320.87 |

The endogenous variable is the absolute difference between the actual and expected number of weekly calls during Spring of 1986. Income is measured in logarithm of thousand dollars. Estimation method is FGLS. Standard t–statistics are shown between parentheses. LM is Breusch and Pagan's (1980) Lagrange Multiplier test of random effects panel *vs.* the corresponding pool specification. This test is distributed as a $\chi^2(1)$. The critical values are 3.84 and 6.63 at 5% and 1% respectively.

## Table 4. Tariff Choice

| | Descriptive Statistics | | Pool | | R.E. Panel | |
|---|---|---|---|---|---|---|
| | Mean | Std.Dev. | Estimate | t–Stat. | Estimate | t–Stat. |
| Const. | | | -6.1286 | (5.86) | -26.0848 | (3.95) |
| INCOME | 7.0596 | (0.837) | 1.7040 | (5.19) | 7.9617 | (3.78) |
| HHSIZE | 2.5603 | (1.483) | -0.3145 | (1.25) | -0.3179 | (0.85) |
| TEENS | 0.2503 | (0.636) | -0.2534 | (0.68) | -5.8091 | (3.08) |
| DINCOME | 0.1511 | (0.358) | -0.4695 | (7.49) | -1.8647 | (6.15) |
| INCOME2/10 | 5.0538 | (1.106) | -1.2624 | (4.80) | -5.8020 | (3.48) |
| HHSIZE2 | 8.7549 | (13.774) | 0.0181 | (6.79) | 0.1178 | (6.46) |
| TEENS2 | 0.4669 | (2.467) | 0.0335 | (2.68) | -0.0047 | (0.11) |
| INCOME*HHSIZE/10 | 1.8228 | (1.098) | -0.0101 | (0.03) | -2.4274 | (3.43) |
| INCOME*TEENS | 1.7629 | (4.542) | -0.0328 | (0.57) | 0.6149 | (2.44) |
| HHSIZE*TEENS | 1.0402 | (2.970) | 0.0787 | (3.49) | 0.3834 | (4.81) |
| AGE1 | 0.0934 | (0.291) | -0.2588 | (3.21) | -1.4233 | (1.75) |
| AGE3 | 0.6271 | (0.484) | -0.0543 | (0.99) | -1.0388 | (3.59) |
| COLLEGE | 0.2244 | (0.417) | 0.3261 | (6.63) | 0.6043 | (1.54) |
| MARRIED | 0.5019 | (0.500) | 0.1649 | (2.94) | 0.4519 | (1.40) |
| RETIRED | 0.2205 | (0.415) | 0.0139 | (0.22) | -0.1003 | (0.26) |
| BLACK | 0.1265 | (0.332) | -0.0699 | (0.96) | -1.4056 | (3.46) |
| CHURCH | 0.1608 | (0.367) | -0.0812 | (1.38) | 0.2816 | (1.03) |
| BENEFITS | 0.2938 | (0.456) | -0.2118 | (3.48) | -1.9938 | (4.71) |
| MOVED | 0.4527 | (0.498) | -0.0871 | (1.78) | -0.3202 | (1.00) |
| ONLYMALE | 0.1102 | (0.313) | -0.0394 | (0.56) | -0.8580 | (1.41) |
| NOVEMBER | 0.3333 | (0.472) | 0.1364 | (2.91) | 0.6752 | (5.09) |
| DECEMBER | 0.3333 | (0.472) | 0.1352 | (2.86) | 0.6655 | (4.74) |
| SWBIAS | 6.5246 | (42.194) | -0.0016 | (1.86) | 0.0018 | (0.40) |
| SWBIAS2/1000 | 1.8225 | (113.281) | -0.0562 | (2.88) | -0.2844 | (3.09) |
| $\rho$ | | | | | 0.9370 | (142.33) |
| Log-likelihood | | | -1532.756 | | -858.357 | |

First two columns show the mean and standard deviations (between parentheses) of the balanced sample for Louisville customers during the fall of 1986. There is a total of 1,542 households and 4,626 observations. Income is measured in logarithm. The other columns present the maximum likelihood estimates for the pooled sample and a one–factor random effects probit model. In both cases, absolute, choice–biased sampling, consistent, t–statistics are reported between parentheses.

**Table 5. Demographics by Tariff Choice**

| | M-Right | | F-Wrong | | M-Wrong | | F-Right | |
|---|---|---|---|---|---|---|---|---|
| MEASURED | 1.0000 | (0.000) | 0.0000 | (0.000) | 1.0000 | (0.000) | 0.0000 | (0.000) |
| LV | 1.0000 | (0.000) | 1.0000 | (0.000) | 1.0000 | (0.000) | 1.0000 | (0.000) |
| BILL | 14.0200 | (0.000) | 18.7000 | (0.000) | 25.3761 | (7.025) | 18.7000 | (0.000) |
| MONTH | 10.9810 | (0.793) | 10.8376 | (0.781) | 11.0236 | (0.830) | 11.0216 | (0.820) |
| SAVINGS | -4.6800 | (0.000) | 4.6800 | (0.000) | 6.6761 | (7.025) | -16.7745 | (15.789) |
| LVOMT | 16.8118 | (1.271) | 17.1923 | (1.210) | 25.3761 | (7.025) | 35.4745 | (15.789) |
| CALLS | 8.6467 | (4.501) | 9.4682 | (4.530) | 28.0414 | (18.565) | 47.5995 | (43.110) |
| EXPCALLS | 12.5587 | (12.760) | 13.6168 | (12.079) | 22.9505 | (19.801) | 31.6585 | (39.100) |
| BIAS | -3.9120 | (12.272) | -4.1485 | (11.867) | 5.0908 | (20.943) | 15.9411 | (50.505) |
| INCOME | 7.0667 | (0.754) | 7.0598 | (0.838) | 7.1568 | (0.748) | 7.0159 | (0.877) |
| HHSIZE | 1.7238 | (0.900) | 1.9467 | (1.039) | 2.3698 | (1.402) | 2.7832 | (1.547) |
| TEENS | 0.0603 | (0.313) | 0.0990 | (0.339) | 0.1625 | (0.529) | 0.3176 | (0.707) |
| DINCOME | 0.1206 | (0.326) | 0.1726 | (0.378) | 0.0883 | (0.284) | 0.1754 | (0.380) |
| AGE1 | 0.0794 | (0.271) | 0.1701 | (0.376) | 0.0777 | (0.268) | 0.0895 | (0.286) |
| AGE2 | 0.2905 | (0.454) | 0.1827 | (0.387) | 0.3192 | (0.466) | 0.2742 | (0.446) |
| AGE3 | 0.6302 | (0.483) | 0.6472 | (0.478) | 0.6031 | (0.490) | 0.6363 | (0.481) |
| COLLEGE | 0.3540 | (0.479) | 0.2284 | (0.420) | 0.3027 | (0.460) | 0.1774 | (0.382) |
| MARRIED | 0.3873 | (0.488) | 0.3680 | (0.483) | 0.5112 | (0.500) | 0.5212 | (0.500) |
| RETIRED | 0.2873 | (0.453) | 0.2335 | (0.424) | 0.2049 | (0.404) | 0.2235 | (0.417) |
| BLACK | 0.0587 | (0.235) | 0.0761 | (0.266) | 0.1190 | (0.324) | 0.1499 | (0.357) |
| CHURCH | 0.1206 | (0.326) | 0.1497 | (0.357) | 0.1531 | (0.360) | 0.1727 | (0.378) |
| BENEF | 0.2794 | (0.449) | 0.3046 | (0.461) | 0.2403 | (0.428) | 0.3147 | (0.465) |
| MOVED | 0.4794 | (0.500) | 0.5076 | (0.501) | 0.4723 | (0.500) | 0.4370 | (0.496) |
| ONLYMALE | 0.1905 | (0.393) | 0.1878 | (0.391) | 0.1154 | (0.320) | 0.0882 | (0.284) |
| Observations | 630 | | 394 | | 849 | | 3016 | |

Mean and standard deviations (between parentheses) of demographics for Louisville customers during the fall months of 1986 by *ex–post* tariff regime and consumption level. The sample is non–balanced and includes a total of 4,889 observations.

**Table 6. Learning and Tariff Switching**

| | Switching | | Measured to Flat | | Flat to Measured | |
|---|---|---|---|---|---|---|
| | Estimate | t–Stat. | Estimate | t–Stat. | Estimate | t–Stat. |
| Const. | -2.6516 | (3.82) | -0.3300 | (0.17) | -3.5303 | (4.25) |
| INCOME | 0.5326 | (2.45) | -0.2420 | (0.38) | 0.7386 | (2.85) |
| HHSIZE | 0.0782 | (0.50) | 0.1898 | (0.41) | 0.3283 | (1.67) |
| TEENS | 0.0280 | (0.13) | 1.1459 | (0.91) | 1.3071 | (2.23) |
| DINCOME | -0.1099 | (2.48) | 0.1115 | (0.69) | -0.1585 | (3.12) |
| INCOME2/10 | -0.3332 | (1.88) | 0.2658 | (0.51) | -0.4278 | (2.01) |
| HHSIZE2 | 0.0063 | (2.28) | 0.0060 | (0.49) | 0.0083 | (3.57) |
| TEENS2 | -0.0693 | (2.43) | -0.1795 | (1.03) | -1.6954 | (3.74) |
| INCOME*HHSIZE/10 | -0.2372 | (1.11) | -0.2866 | (0.43) | -0.6446 | (2.31) |
| INCOME*TEENS | -0.0035 | (0.11) | -0.1298 | (0.75) | -0.0091 | (0.18) |
| HHSIZE*TEENS | 0.0112 | (0.63) | 0.0405 | (0.31) | 0.1077 | (3.25) |
| AGE1 | -0.0690 | (1.13) | 0.0951 | (0.52) | -0.1221 | (1.75) |
| AGE3 | -0.0407 | (0.89) | -0.0747 | (0.72) | -0.0526 | (0.97) |
| COLLEGE | 0.0138 | (0.30) | -0.1308 | (1.57) | 0.0450 | (0.79) |
| MARRIED | 0.0474 | (1.09) | -0.0143 | (0.12) | 0.0584 | (1.14) |
| RETIRED | -0.0218 | (0.47) | -0.0282 | (0.21) | -0.0074 | (0.14) |
| BLACK | 0.0690 | (1.20) | 0.0661 | (0.41) | 0.0503 | (0.73) |
| CHURCH | -0.0085 | (0.18) | -0.0545 | (0.44) | 0.0002 | (0.00) |
| BENEF | -0.0150 | (0.36) | 0.1429 | (0.94) | -0.0713 | (1.45) |
| MOVED | -0.0367 | (0.97) | 0.0136 | (0.14) | -0.0648 | (1.44) |
| ONLYMALE | -0.0608 | (0.96) | -0.0195 | (0.18) | -0.0886 | (1.15) |
| SWBIAS | 0.0003 | (0.63) | 0.0005 | (0.19) | 0.0004 | (0.62) |
| SWBIAS2/1000 | -0.0184 | (6.80) | 0.0091 | (0.39) | -0.0382 | (6.18) |
| Observations | 1542 | | 445 | | 1097 | |
| Log-likelihood | -117.266 | | -23.391 | | -89.829 | |

The endogenous variable equals one if the household switches tariffs between October and December of 1986. Income is measured in logarithm. The estimation method is weighted ML. Absolute, choice–biased sampling, consistent, t–statistics are reported between parentheses.

## Table 7. Risk Aversion and Tariff Choice

| | Choice | | Risk Averse | | Risk Lovers | |
|---|---|---|---|---|---|---|
| | Estimate | t–Stat. | Estimate | t–Stat. | Estimate | t–Stat. |
| Const. | -1.7119 | (1.86) | 15.6595 | (0.91) | -1.6486 | (2.28) |
| INCOME | 0.5707 | (2.07) | -4.2389 | (0.87) | 0.3353 | (1.49) |
| HHSIZE | -0.2183 | (2.08) | -0.3455 | (0.24) | -0.0719 | (2.96) |
| TEENS | -0.0361 | (0.19) | 12.9847 | (0.00) | -0.0184 | (0.56) |
| DINCOME | -0.1502 | (3.60) | 1.0355 | (1.17) | -0.1463 | (4.25) |
| INCOME2/10 | -0.4524 | (2.15) | 2.7711 | (0.78) | -0.2502 | (1.44) |
| HHSIZE2 | 0.0030 | (1.61) | 0.1690 | (0.67) | 0.0041 | (2.22) |
| TEENS2 | 0.0075 | (0.96) | -6.8885 | (0.00) | 0.0013 | (0.27) |
| INCOME*HHSIZE/10 | 0.2128 | (1.44) | | | | |
| INCOME*TEENS | -0.0101 | (0.40) | | | | |
| HHSIZE*TEENS | 0.0304 | (2.02) | | | | |
| AGE1 | -0.0414 | (0.53) | 7.2522 | (0.00) | -0.0102 | (0.14) |
| AGE3 | -0.0212 | (0.44) | 0.2317 | (0.39) | 0.0079 | (0.19) |
| COLLEGE | 0.1252 | (2.35) | -0.5560 | (1.03) | 0.0702 | (1.57) |
| MARRIED | 0.0431 | (0.99) | -0.4076 | (0.47) | 0.0254 | (0.68) |
| RETIRED | 0.0669 | (1.10) | 1.5724 | (1.30) | 0.0243 | (0.54) |
| BLACK | 0.0079 | (0.16) | -1.0254 | (1.08) | -0.0382 | (0.88) |
| CHURCH | -0.0201 | (0.46) | 1.2485 | (1.44) | -0.0109 | (0.28) |
| BENEF | -0.0882 | (1.75) | -1.7919 | (1.52) | -0.0828 | (2.12) |
| MOVED | -0.0187 | (0.44) | 0.8203 | (1.13) | 0.0120 | (0.33) |
| ONLYMALE | -0.0187 | (0.24) | -0.0235 | (0.03) | -0.0303 | (0.44) |
| SWBIAS | 0.0017 | (4.38) | 0.0978 | (1.03) | -0.0005 | (1.35) |
| SWBIAS2/1000 | -0.0019 | (2.32) | 3.5068 | (0.86) | -0.0134 | (6.41) |
| L-MEAN | -0.0277 | (2.76) | | | | |
| L-STD | -1.2666 | (0.18) | | | | |
| L-MEAN*STD | 0.0815 | (0.22) | | | | |
| H-MEAN | -0.0200 | (13.10) | | | | |
| H-STD | -0.0153 | (2.07) | | | | |
| H-MEAN*STD | 0.0005 | (4.48) | | | | |
| Observations | 1329 | | 60 | | 1163 | |
| Log-likelihood | -188.345 | | -16.206 | | -153.193 | |

The endogenous variable equals one if the household chooses the optional measured tariff in the first two columns, and the optional flat tariff in the last one. Income is measured in logarithm. The estimation method is weighted ML. Absolute, choice–biased sampling, consistent, t–statistics are reported between parentheses with the exception of the middle column for which ML estimation did not converge after correcting for choice–biased sampling.

# Figure 1. Empirical Distributions