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ABSTRACT

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

It is commonly believed that consumers behave irrationally when subscribing optional telephone tariffs. The fact that they show a strong preference for flat rate options has commonly been interpreted as evidence of irrational behaviour since such a choice is believed not to be cost-minimizing *ex post* in most cases. My results, obtained using the data from the 1986 Kentucky tariff experiment, contradict these views and provide strong evidence in favour of the rationality of consumers' choices. I found that expectations on future consumption play a major role in the choice of tariffs but also that consumption forecast errors are more related to the volume of local telephone usage than to any particular demographic profile. More importantly, the evidence shows that there exist important learning effects that induce tariff switching in order to minimize the magnitude of monthly bills even in the short term and responding to very small cost differences. Finally, risk aversion is ruled out as a possible source of consumers' biased taste for flat tariffs.

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Keywords: expectation bias, learning, risk aversion, service switching and tariff choice

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NON-TECHNICAL SUMMARY

It is commonly believed that consumers make frequent mistakes when subscribing to optional tariffs. Proposed explanations include ignorance of the cost of the alternatives, persistent misperception of the actual consumption, monthly variation in consumption, existence of risk aversion towards bill variation and consumer preference for flat tariffs. These explanations insist frequently on the apparently irrational behaviour of consumers that repeatedly fail to choose the least expensive tariff option for his or her consumption profile. The economic impact of the failure of the rationality assumption is however minimized with the argument that the difference in cost of service under alternative tariff options is very small.

However, all these conclusions are reached only using very limited information. The present Paper makes use of the excellent individual data of the tariff experiment carried out in two cities of Kentucky in 1986. The experiment was aimed to convince the regulator that the introduction of optional tariffs was welfare enhancing and consumers did not suffer from extremely high bills.

Local telephone service in the US has traditionally never been measured. Residents can make as many calls of any duration within the local area at no extra cost. They just pay the monthly service charge. However, regulated local monopolists (Baby Bells) introduced optional tariffs after the break-up of AT&T. Until the middle of 1986, that was the situation in Bowling Green and Louisville. South Central Bell collected information on the demographics and consumption pattern for about 5,000 households in these two local exchanges. Usage was not affected by price, as the marginal cost of an additional call was zero. But most importantly, the expected usage that consumers also reported was neither affected by price and/or strategic considerations, as they were unaware of the tariff experiment that would be conducted during the second half of the year.

In a first stage, the Paper studies how informative the expectations are on future telephone usage against the actual consumption. The average correlation between expected and actual number of weekly calls is 34%, although there is important differences among households with different demographic characteristics. Later, the Paper studies the role of consumers' expectations on the tariff choices that they make. The first important result is that consumers do not appear to overwhelmingly make the wrong tariff choice. The magnitude of the prediction error increases with the usage of the telephone and those consumers that make significant mistakes in predicting their future usage tend to subscribe to the flat tariff option, which in turn, ends up being the least expensive one for them.

The data also includes the subscription decisions made during the three months of the fall of 1986 in Louisville. Thus, tariff switching can be studied. The results prove that consumers learn at different speeds depending on whether they start this period subscribing to the measured service (already knowing the cost of their usage and the one of the alternative) or to the flat tariff option. But the potential savings mostly explain the significant switching that consumers may achieve under the alternative option.

This evidence contradicts many previous studies that concluded that consumers were irrational on the basis of the strong preference for flat tariff only. Here, I show that consumers actively search for the least expensive option within a limited period of time, right after the introduction of the optional tariffs and responding to an average potential savings of $5 over a monthly *per capita* income of $1,600. Thus, rationality of consumers appears to be a sound economic assumption even for the small amounts involved in signing up for different optional calling plans for the local telephone service.

1 Introduction

Economic Theory predicts that consumers who compare two alternatives choose the one that provides them the maximum utility given the cost of each option. Random utility models incorporate this helpful structural interpretation where the agents who are making choices implicitly maximize some given utility function [McFadden (1974)]. Accordingly, economists interpret the actual choices as the optimal consumer's actions that solve an implicit constrained maximization problem. However, many economists have noticed anomalies in the behavior of consumers regarding the choice among optional tariffs, something that million of households do regularly in relation with their local phone, long distance, wireless, electricity, cable, internet and other services . And surprisingly, most economists argue that there are important particularities in these cases to justify an exception to the applicability of the rationality assumption to explain the realized choices.

The most studied case is the choice among Optional Calling Plans (OCPs). This paper intends to show that the choice of OCP is not an exception to the economic principle of rationality, and that contrary to common beliefs, customers of local telephone service make, on average, the right choice of tariff conditional on their actual realized consumption. Furthermore, I show that consumers often switch tariff options with the explicit goal of minimizing the cost of their service. Thus, I conclude that the traditional interpretation of the partial evidence previously available in this industry is misleading and incomplete.

While it is commonly argued that consumers make frequent mistakes in their choice of tariff options, analysts this conclusion is reached only from the fact that most customers prefer the flat tariff option whenever it is available. Consumers are accused of being irrational by repeatedly not choosing the least expensive option available for their consumption profile.[1] 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. Proposed explanations include ignorance of the cost of the alternatives, persistent misperception of the actual consumption, monthly variation in consumption, and consumer preference for flat tariffs.[2] The data used in this paper makes possible to explicitly test these hypotheses for the first time.

Since most consumers choose the flat option, analysts conclude 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 usage profile. This argument, among others, triggered that the Federal Communications Commission (FCC) issued the "OCP Guidelines Order" after AT&T introduced the first nationwide calling plan.[3] The Guide-

[1] See Dansby (1983); Hobson and Spady (1988); Mac–Kie Mason and Lawson (1993); and Kridel, Lehman, and Weisman (1993). 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.

[2] See for instance Mitchell and Vogelsang (1991, §8.2.2) and Taylor (1994, §7.1).

[3] Memorandum Opinion and Order, OCP Guidelines Order, CC Docket 84–1235, October 17, 1985.

lines tightened any change of the average price of the options within a 5% boundary of the productivity growth, but most importantly, it still requires that any "*carrier offering an OCP demonstrates, with periodic reports, that the introduction of the OCP does not burden other services or subscribers.*" Thus, the Regulator justifies its intervention in the design and/or approval of the tariff options of local monopolies because otherwise they will make money out of those who make the wrong choices.[4]

The topic of OCPs is also interesting from another policy perspective, and rises some side issues that can be at least partially addressed with my data. If there are numerous and significant mistakes in the choice of tariffs, 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 criticized for not considering that all customers do not have the same ability to make "right" choices. 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 the magnitude of individual demand than to any particular socioeconomic or demographic characteristic.

But do consumers really make systematic mistakes? Are they unable to predict their future consumption and choose the tariff option consistently? Are they ignorant of the cost of their usage with other tariffs? Don't they show any sign of rational behavior by switching tariffs? Although it is true that most customers prefer the flat rate option, it is not so obvious that this empirical regularity leads to the conclusion that consumers are irrational. This paper shows that in fact most of those who chose the flat tariff option were intensive users and thus their overwhelming preference for flat tariffs responds to a consistent and rational behavior. Furthermore, the paper reports that there is significant service switching that is consistent with a cost minimizing strategy of consumers.

It may appear so far that the study of OCPs is only interesting in its relation to telecommunications pricing. Within this literature, it is also commonly argued that the small difference in cost of each alternative justifies a careless behavior of consumers regarding the choice of optional tariff.[5] However, this question opens a much broader discussion that exceeds the limits of telecommunication pricing: Should we just forget our rational choice theory when the price difference among the cost of the alternatives is small? How small should this price difference be?

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[4] 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 FCC and Federal Trade Commission 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. MCI–Worldcom also agreed on a voluntary 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.

[5] See for instance Clay, Sibley, and Srinagesh (1992), and Srinagesh (1992).

An framework that could explain why consumers make systematic choice mistakes is one that accounts for computation costs. Even rational consumers facing numerous and complex tariffs might find optimal not to evaluate all options in detail. This rational behavior could lead to the result that consumers do not choose the cost minimizing option. Fortunately such a framework can be ruled out in the present study as consumers must choose between only two options, one of which is a flat tariff. Thus, if rational consumers systematically fail to choose the least expensive option, we should conclude that either marginal utility of income is negative (which violates the assumption of non–satiation), or that consumers show some kind of risk aversion behavior in their choice of tariffs. It is tempting to rule out any risk aversion argument on the basis that savings from switching to the *ex–post* right tariff option average about $4.88 out of a representative $1,600 *per capita* monthly income.[6] The magnitude of the difference in cost of each alternative may appear insignificant, but finding, as this paper does, that consumers respond in the very short term to this small savings by switching options provides very strong evidence in favor of the rationality of telephone customers, and therefore, by extension it will support the applicability of the the rational theory of choice even to cases where differences in the cost of alternatives are small.

I use the excellent individual data of the 1986 local telephone tariff experiment carried out in two cities of Kentucky. The rich evidence reported in this paper contradicts most common interpretations of the tariff choice puzzle in telecommunications, and more importantly, stop presenting the choice among OCPs as an exception of the theory of rational choice. The evidence 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 those who generated most of the additional revenues to the local telephone carrier of this study. The paper also documents for the first time that consumers respond to small cost differences between options and as result switch services to minimize their monthly bill. Finally, results appear to support the idea that the local telephone customers of this tariff experiment behave as risk lovers rather than risk averse.

The paper is organized as follows. Section 2 briefly describes the tariff experiment carried out in Louisville and Bowling Green 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, later focuses on those customers who switch tariff options to determine whether they learn from their past usage profile and tariff choice, and finally addresses the issue of risk aversion in the choice of tariffs. Section 4 concludes.

[6] The present value of those savings could however represent a substantial amount. The distinction is important because many of these optional tariffs become long term contracts.

2 Data: Expected and Actual Consumption

In 1984, right after the break up of AT&T and the creation of the Regional Bell Operating Companies, 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 evaluate as local telephone service had never been measured before.

In order to help deciding whether the introduction of optional measured service should be approved, 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 five thousand households among the customers of Bowling Green and Louisville. Simultaneously, SCB recorded the local monthly telephone usage information of those 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.

At 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 monthly, individual, telephone usage information again in October, November, and December. In Bowling Green, all customers were placed under mandatory measured service, while in Louisville 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 within the local exchange area. 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 were the same for distance band B, but duration charges were doubled. Peak time included weekdays 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.

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.

There is also evidence of a choice biased sample that need to be adjusted at the empirical stage in the case of Louisville. 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%. However, the selection of the sample was made during the spring of 1986, before the introduction of tariff options. Thus, the 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. This deletion does not produce 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 assigned to these observations the estimated average annual income level of $19,851 and included a dummy variable to index these cases.[7]

The Appendix describes in detail all variables used in this study, while 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, defined as the average number of weekly calls during the spring months, 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

[7] The transformation 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.

[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

I present now a brief discussion of the underlying consumer choice model. I assume not only that consumers have heterogeneous preferences, but also that their demands are stochastic to allow for monthly variation of telephone usage. The main consequence is that consumers may face unforeseen needs of communication for which a different *ex–ante* choice of OCP could have been *ex–post* optimal. Without loss of generality, consumers' preferences can be represented by the following indirect utility function:[8]

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

where y is the household monthly income, A the monthly fixed fee, p the average duration charge, and θ is a single dimensional index that captures the heterogeneity of consumers' preferences. 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 θ_1 is the *ex–ante* type and θ_2 the type shock. Thus, θ_1 captures the average, steady–state, monthly, local telephone usage while θ_2 represents any unforeseen consumption (positive or negative) at the time of subscribing the tariff option. The fact that θ_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*. Obviously, both type components might be related to demographics, some of which are available to us. These 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:

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

$$E_{\theta_2}[x(0, \theta)] = \theta_1. \tag{4b}$$

[8] 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.

2.2 Descriptive Statistics

This section discusses the empirical relationship between expected and actual local telephone usage. Most of the analysis is 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. If we ignore these dimensions and focus on the demand for calls, θ, θ_1 and θ_2 can be identified up to some extent by the actual and expected individual consumption during the spring months of 1986. I focus on a single index of telephone usage –weekly number of calls–, which is available both *ex–ante* and *ex–post*. It is remarkable that in addition to demographics SCB also collected information on customers' own usage expectations. SCB explicitly requested customers' own 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 for the actual and expected satiation levels θ and θ_1 as defined in equation (4).

Table 2 presents a stratified analysis not only of the magnitude of the average error of prediction, $\theta - \theta_1$, 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 demographic characteristic that explains a higher or lower correlation between expected and actual usage.

First, observe that the average expectation error 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 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 preference for flat tariff options.

Second, local telephone usage's 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 those 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, due to 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 also controlled by products of demographics with the indicator of racial composition of the household in order to identify the source of prediction errors.

The immediate next step is to evaluate how informative is the expectation of future telephone usage about the actual future usage level. Table 2 also presents a stratified analysis of 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%. Finally, while the differences in correlations are small, there is evidence of heterogeneity in predictive power within groups. Higher 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. This evidence opens the possibility that this social group gets particularly hurt by the introduction of optional tariffs, but further results presented later in the paper clearly rules out that possibility. However, the present 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 accounting for observed and unobserved heterogeneity. Economic Theory does not provide much guidance 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.*, $E[\theta_2] = 0$. The three months of data is obviously insufficient to answer this question in a definitive manner, although the results of the next section support this view. 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 basis.

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.

The first two columns of Table 3 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 of this variable allows to control for the effect of traffic related idiosyncrasies that are not captured by the available demographics.[9] I have also included cross–products of the three main variables –income, size of the household, and number of teenagers–, to account for possible nonlinearities. Also, given the apparently different behavior of black households, I have included cross–products of demographics based on racial composition of households to break down the specific effects of this variable.

I estimated two models for each city. One was based on a pooled sample for all spring observations while the other, reported here, 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.[10] I computed several 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. This result rejects the monthly variation of consumption as a valid explanation of the potential mistakes that consumers may make in choosing the tariff option.

The existence of individual heterogeneity favors the panel estimation over the pool estimation that is always rejected. Two specifications of the panel data 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

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

[10] 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).

without cross–product of demographics and race, whose results are not reported either, is always rejected. Still, even after accounting for the effect of unobservable heterogeneity, black households show a markedly different behavior and demographics appear to have a differentiated effect than for non–black households, especially in Bowling Green. Therefore, 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. I interpret the reported expectations as essentially capturing individual idiosyncratic ability to predict future consumption. This variable will be latter used as an individual indicator to account for its effect on the choice of tariff option. Regarding the rest of the variance of the bias, it is exclusively 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 appear 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. Therefore, 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

Many 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 part of this section I discuss whether the ability to predict future consumption play any central role in the choice of tariff options using the individual prediction error, an information rarely available, as an explanatory variable. In the second part 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. Finally, I explore whether risk aversion may explain the overwhelming preference for flat rate tariffs.

3.1 Consumption Misscalculation and Tariff Choice

In this subsection 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 of collected data. Its square should account for the existence of possible nonlinearities. This indicator has been found to be a good dummy to control for idiosyncratic 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.[11]

The second half of Table 3 reports the effects of demographics on the choice of tariff plans during the fall months of 1986 in Louisville. The last two columns present the estimates of two probit models: a standard probit using the pooled data, and a random effects probit that makes use of the panel structure of the data also in the fall of 1986.[12] The estimation of the standard probit model implicitly assumes that some unobservable characteristic is driving the choice of tariff. Thus, the choice is made depending on the value of the consumer type θ 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 is very significant. Thus, I have also allowed for the existence of unobservable 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:

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

$$\mathrm{Var}[\epsilon_{it}] = 1 + \sigma_u^2, \tag{5b}$$

$$\mathrm{Corr}[\epsilon_{it}, \epsilon_{is}] = \frac{\sigma_u^2}{1 + \sigma_u^2} = \varrho, \tag{5c}$$

so that by identifying an individual specific error component, u_i, total error terms are equally correlated across time for each individual.[13] The estimate of ϱ is significant and

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

[12] 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 $\chi^2_{0.95}(25) = 37.65$.

[13] 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). 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 hypotheses testing results are the same regardless of the number of quadrature nodes used.

indicates the existence of a very strong time correlation of the type shock due to the existence of important individual heterogeneity. Together with this heterogeneity, the effects of the average spring prediction error variable, SWBIAS, in the random effects probit model can be interpreted as evidence of time persistence of of the consumption pattern on the choice of tariff. 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 –the volume of calls is also higher–, the probability of choosing the measured option declines.

Accounting for unobservable heterogeneity that influence 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 is the same for the two specifications, the pool probit model appears to produce downwards biased estimates. For instance, the number 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.[14] 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.[15] In addition to this variable only the time dummies show a significant 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.

In the previous section I showed that there was little correlation between demographics and the ability to predict future consumption. The finding was troubling because the group of black households appeared to make less accurate predictions of their telephone usage, and thus questioned whether they made more mistakes than average in choosing their optional tariff. 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 make more mistakes computing their future usage are mostly

[14] As I explained before, the sample is choice biased. The likelihood function needs 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 the last two columns of Table 3 are obtained from a sample–weighted 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. Thus, 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 ignore the transition probabilities associated to 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.

[15] This is the right choice as high income households generally consume less than the average customer. High income levels and small household sizes characterize those customers with low demand for local telephone service. See the results of Hobson and Spady (1988), Kling and Van Der Ploeg (1990), and Miravete (1997) who makes use of the same data set of the present study.

those with high usage demand, for whom the flat tariff option is also the least expensive one.[16] Consistently with this argument, black households tend to choose more often the flat tariff, which happens to be the optimal choice for them given their consumption profile. This evidence is additional to traditional explanations of the bias towards flat rate options based on inertia from previous mandatory flat rate pricing as indicated by Kling and Van Der Ploeg (1990, §6).

So do consumers behave irrationally? The evidence presented so far in this section refutes some of the common justifications of irrational behavior mentioned before. Consumers make persistent mistakes in predicting their usage level, but since the magnitude of the error is more related to the volume of traffic than to any other demographic characteristic and most consumers end up subscribing the flat tariff option, I should conclude that the persistent misperception of the actual consumption must be rejected as a potential explanation of the potential mistakes that consumers may make in choosing the tariff option. The sign and significance of SWBIAS in the probit equations of Table 3 support this view. Similarly, the positive effects of the month indicators on the probability of subscribing the measured option refutes the idea that consumers remain ignorant of the cost of the alternative tariff options. Perhaps they were immediately after the introduction of OCPs, but these estimates show that, contrary to the traditional readings of the evidence in this industry, they do not particularly remain under the flat tariff option independently of their usage profile. On the contrary, they appear to experiment switching services to learn the cost of their usage profile under the alternative option. This point is analyzed more in the following subsection.

So, it is worth introducing OCP? Do the monopolist make huge profits out of those telephone customers who wrongly choose the flat tariff option? The data supports the idea that monopoly profits increase after the introduction of OCPs. As the following analysis shows, the expected benefit per customer who chooses the tariff option wrongly is higher for those customers that choose the flat tariff option. However, since there are far more customers making mistakes when subscribing the measured service than the flat tariff option, the bulk of profits due to mistaken choices comes from the measured option.

Depending on the actual volume of telephone usage, consumers can be classified *ex–post* as having chosen correctly or incorrectly each tariff option. This classification is made contingent on keeping the same usage pattern independent of price responses, which provides with an approximate upper bound of the gains of switching to a different tariff option. This amount gives an idea of 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, customers choosing the measured service paid 50% more than those choosing the flat tariff.

[16] Usage of local telephone service is always higher for black households than for non–black households according to the three studies of the previous footnote.

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 subscribed the measured service. 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.[17] On the contrary, the other 3,106 (88.4%) customers who correctly chose the flat option would had paid on average an additional $16.77 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 prefers 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 choice biased sampling, 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% of customers of local telephone service 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 the maximum loss is also bounded in that case, *i.e.*, $4.86. Thus, the monopolist's expected gain from a customer that chooses a plan incorrectly 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 analyze how consumers learn which is the tariff option that better suits their consumption profiles. But given that in the present tariff experiment there were only two options, the choice pattern followed by consumers during the three fall months may reveal important information

[17] 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. In this case, the assumption of equal consumption under the two options is quite accurate because these consumers face the same marginal tariff in both cases.

on whether there is important learning. We should keep in mind that any estimates of the learning effects could be seriously biased downwards as the data collection took place after three months of adjustment period in which most of the learning could have taken place. This is however the best data available, and can be used to give an idea of whether this tariff switching followed any consideration regarding potential savings that could be accomplished by signing up for a different option.

Table 4 presents the results of several probit models. In the first column, the endogenous variable takes a value equal to one when the customer switches options between October and December. The other two columns explore the possibility that this learning process is asymmetric, *i.e.*, the second column analyzes why customers that started with the measured tariff option in October ended up choosing the flat 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 should they 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. About 10% of the 445 customers that started October under the measured option switched back to the flat tariff between November and December. Only 4% of those 1,097 customers that started under the flat tariff experimented with switching to the optional measured service.

Demographics are far more successful in explaining the switch from flat to measured service than the other way around. Income has a positive influence in inducing this switching. This is in accordance with the previous results on the determinants of the choice of tariffs. Households with higher income levels tend to subscribe the optional measured service, and thus, we also observe that those consumers of this group that did not signed up for the optional measured service at the beginning of the tariff experiment kept doing it during later months. In addition to the cross-products of this variable with the size of the household and number of teenagers, the only other significant variable is the expectation bias of future usage. Consumers that underestimate only a little their actual future usage, tend to switch towards measured service. Those with very intensive consumption profiles, and thus very large consumption expectation bias are more likely to remain under the flat tariff option, thus minimizing their monthly payment.

I also include as explanatory variable the maximum potential savings that could have been attained by choosing the other tariff option in October. A significant positive estimate for this variable indicates that consumers react optimally, and that on average they end up choosing the right tariff in the long run. This is exactly the result that is obtained in the three models estimated in the first half of Table 4. All customers that switch tariffs, regardless of whether they go from measured service to flat tariff option or from flat to measured end up reducing their monthly local telephone bill. This result contradicts the common belief that consumers remain under the flat tariff option regardless of their consumption pattern and that they do not respond to potential savings

associated to tariff switching. The reported results show that they do, although the effect of potential savings is three times larger for those who switch from measured option to flat service. The asymmetry in monitoring the cost of the current consumption under the different tariff options suffices to explain the difference magnitude of potential savings in each case. Nevertheless, this result represents the most important piece of evidence that consumers show a rational behavior in evaluating the cost of each alternative for their given consumption profiles regardless of whether the difference in cost between alternatives is small and the time period to learn is constrained to three months.

3.3 Risk Aversion and Massive Flat Tariff Choice

We expect that consumers learn what is their best tariff option in the long run, as shown in the previous subsection, but it is difficult not to reject, even without further empirical analysis, the idea that consumers are risk neutral when the difference in payments represents such a small fraction of monthly income as in the case of local telephone service. However, some economists –Kridel, Lehman, and Weisman (1993) in particular–, have made use of the risk aversion argument to justify why so many consumers prefer the flat tariff option whenever they are given such a choice. This section makes use of the features of the tariff options offered in Louisville to suggest a simple way of testing this risk aversion hypothesis.

Observe that the measured option includes a tariff allowance of $5.00. Therefore, anybody whose consumption profile (number of calls, duration, and hourly distribution of communications) lead to a total bill below $19.02 with the optional measured service should choose this option. Obviously, each consumer does not make the exact same number of calls every month, and thus each one of them know her distribution of expected usage when choosing their optional calling plan. A consumer with a usage distribution characterized by a mean usage of $15.00 and a standard deviation of $2.00 should never subscribe the optional flat service unless she is risk averse. The same happens for a customer with an expected usage of $18.50 and a standard deviation of $0.25. The higher the expected usage the more likely that a risk neutral consumer would choose the flat tariff option. Similarly, for every given mean expected usage, the more disperse is the consumption pattern, the more likely that a risk neutral customer end up choosing the flat tariff option. The same analysis is valid for usage levels above $19.02.

The fact that the data set includes usage information for each household in both the spring and fall of 1986 provides with sufficient information to test this risk aversion hypothesis in a simple way. I proceed as follows. I consider only those customers for whom I have three monthly observations in spring and fall of 1986. This is done to characterize the mean and standard deviation of usage with the best information that is available in this data set. I also restrict my attention to those customers that do not switch tariffs during the fall. This avoids ambiguous result by focusing on those customers that show a marked preference for one of the two tariff options. I then compute how much the cost of local telephone usage would be for each customers in each of the three spring months.

The mean and standard deviation of this individual usage distribution is then used as the best prediction available for consumers in choosing their calling plan.[18] The mean expected usage, its standard deviation, and the product of the two are then included in the probit model presented in the fourth column of Table 4. The effect of these three variables (MEAN, STD, MEAN*STD) are differentiated depending on whether the actual usage is below (L) or above (H) the cutoff usage of $19.02.

The results of the fourth column of Table 4 are in favor of risk neutral agents. Starting with the last three variables we can see that consumers are less likely to choose the measured option the higher is their expected usage and its dispersion. The product of these two variables has however a positive sign for the high demand customers. The result is particularly important for customers whose usage is slightly above the cutoff level. The more spread is the distribution of a customer with a mean expected consumption of, for instance $19.10, the higher the expected benefits of subscribing the measured service option. If this customer unexpectedly increase her usage by $0.20, her total bill will increase in just 20 cents. But if it turns out to be an unexpected decrease in her usage of $0.20, her bill will decrease more than $5.00 due to the tariff allowance. The more spread is the distribution of expected usage, the more likely that a low realization puts the consumer in the allowance region of the measured option. However, the higher is the mean and standard deviation, the lower is the net gain, as high usage realizations become more costly. This is consistent with the negative signs of these two variables. Finally, as for the effect of these variables for those customers whose usage is below the $19.02 level, only the mean expected usage is significant.

There is however important individual heterogeneity that is not accounted by the observable demographics. The remaining two columns of Table 4 isolate clear risk averse and risk lover customers, and try to analyze whether any demographic characteristic is closely related to such profile.[19]

Potential risk averse customers are those whose maximum expected usage is always below $19.02. The expected usage of these 60 customers ranges from $16.02 to $18.96, and almost half of them choose the flat rate option. Only these 30 customers are clear risk averse agents and showed a significantly biased preference for flat tariffs. Demographics have no power in explaining why these customers with such a low usage profile prefer the more expensive flat rate option.

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[18] If I had historical data available, I could control for seasonal effects. I assume that consumers are implicitly accounting for past experience beyond the spring months of observation that I have. However, I decided to control for seasonal effects assuming that the average total increase or decrease of consumption of the fourth quarter relative to the second quarter is the same for all customers, regardless of their tariff choice in the fall. Thus, I can compare the usage level of those customers on optional flat tariff in the fall (90% of the population) and mandatory flat rate in the spring. Usage increases by 11% in the fall relative to spring. This correction factor is then applied to the spring usage of all customers to predict their usage in the fall.

[19] Cross–products are excluded from this regressions because in the risk aversion case, due to the low number of observations some variables were collinear.

On the other hand, potential risk lovers are defined as those customers whose minimum expected usage always exceeds \$19.02. The expected usage for these 1,163 customers (out of 1,329) ranges from \$19.03 to \$117.07, and only one fourth of them choose the potentially more expensive measured service. Here demographics help explaining this choice pattern. Large households with teenagers and those who receive some kind of social benefits are far less likely to love risk and choose the measured option when their usage profile is always above the cutoff level.

Therefore, and comparing the number of risk averse and risk lovers in this sample, we can conclude that there is no evidence that the choice of flat tariffs responds to risk aversion. On the contrary, it appears that some of those who choose the measured option are in fact risk lovers.

4 Conclusions

This paper has presented abundant evidence in favor of the rationality of consumers regarding the choice of optional tariff plans. Using an underlying model of consumer choice with individual stochastic demands, I have made use of the rich information contained in the 1986 Kentucky's local telephone tariff experiment. Thus, I have been able to control for unobserved individual heterogeneity at the expectation formation and tariff choice stage and compute the effects of commonly available demographics on these control variables.

The results reported in this study reconcile the commonly observed tariff choice patterns with the critical axiom of rationality of consumer behavior in two ways: static and dynamic. From a static point of view, when signing up for a particular tariff option, consumers are guided by their expectations of future telephone usage rather than by some sort of pathology. The available indicator of future consumption expectation and its distribution is conditioned by the set of socioeconomic and demographic characteristics of the households, although its influence in explaining the formation of those expectations is quite limited. The most powerful variable in explaining the consumer expectation bias is the actual average telephone usage. Thus intensive users of local telephone service tend to make larger mistakes in predicting their future consumption, but such mistakes have no further economic implications because most of them also choose the flat tariff option, which is the least expensive one for their common range of telephone usage. In addition, from a dynamic perspective we do not only care that consumers behave rationally when choosing tariff plans, but also also expect that they learn after making an initial mistake in the tariff choice and switch tariff to minimize their monthly payments for local telephone service.

The results show that tariff choice is mainly driven by the individual expectation about future telephone usage. This result does not imply that telephone customers make systematic mistakes in the choice of tariff because the usage expectation bias is highly correlated with the usage level of local telephony, and flat tariffs are always better suited for

usage intensive customers. This evidence contradicts numerous previous interpretations of the tariff choice pattern commonly observed in telecommunication services. In particular, it questions the supposed irrationality of consumers while signing up for optional calling plans.

The paper has also confirmed the existence of important learning effects and significant responses to minor potential savings in the short term so that we should conclude that in the long run most customers will pick up the least expensive optional tariff for their telephone usage profile. The reported evidence has also ruled out the commonly used risk aversion argument as a reasonable explanation of the observed tariff choice pattern.

There remains one last issue to be addressed in the future through the structural analysis of this data: the vast majority of consumers predict a minimum usage that will place them above the allowance of the measured service option. We have seen that most of them (76%) always choose the flat rate option. This opens the question of whether SCB was offering the optimal options. Keeping the optional measured service tariff unchanged, SCB could increase its expected revenues by increasing the magnitude of the flat rate option. SCB could have charged \$19.02 for the flat tariff option. The majority of customers that currently prefer the flat tariff (except perhaps those with a distribution of usage narrowly defined around \$19.02) would still prefer the flat tariff option but the monopolist could make an additional \$0.32 from each of them. The higher is the mean expected consumption level of those currently choosing the flat tariff option the easier would be to increase revenues by further increasing the price of the flat tariff option.

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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 during the spring months.
HHSIZE	Number of people who regularly live in the household.
INCOME	Estimated total monthly income of the household.
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
BENEFITS	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.	ρ t–Stat.	TEST	Avg.Bias	Std.Dev.	STRATA	Avg.Bias	Std.Dev.	ρ 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 3. Consumption Expectation Bias and Tariff Choice

	Expectation Bias		Tariff Choice in Louisville	
	Bowling Green	Louisville	Pool	R.E. Panel
Constant	-26.8940 (0.77)	27.1186 (0.94)	-6.1286 (5.86)	-26.0848 (3.95)
INCOME	1.9666 (0.19)	-8.7067 (1.00)	1.7040 (5.19)	7.9617 (3.78)
HHSIZE	22.1552 (3.67)	6.6800 (1.63)	-0.3145 (1.25)	-0.3179 (0.85)
TEENS	-3.2395 (0.26)	-23.2183 (2.02)	-0.2534 (0.68)	-5.8091 (3.08)
DINCOME	-0.6156 (0.30)	0.1733 (0.11)	-0.4695 (7.49)	-1.8647 (6.15)
INCOME2/10	2.8010 (0.35)	6.4310 (0.97)	-1.2624 (4.80)	-5.8020 (3.48)
HHSIZE2	0.0173 (0.04)	-0.0693 (0.66)	0.0181 (6.79)	0.1178 (6.46)
TEENS2	-0.2112 (0.49)	-0.1474 (0.37)	0.0335 (2.68)	-0.0047 (0.11)
INCOME*HHSIZE/10	-28.3910 (3.50)	-9.0540 (1.62)	-0.0101 (0.03)	-2.4274 (3.43)
INCOME*TEENS	0.6644 (0.39)	3.3993 (2.19)	-0.0328 (0.57)	0.6149 (2.44)
HHSIZE*TEENS	-0.5074 (0.57)	-1.0210 (1.84)	0.0787 (3.49)	0.3834 (4.81)
AGE1	2.4346 (1.40)	-1.7166 (1.14)	-0.2588 (3.21)	-1.4233 (1.75)
AGE3	-1.2391 (0.55)	0.4050 (0.22)	-0.0543 (0.99)	-1.0388 (3.59)
COLLEGE	-0.4770 (0.31)	-0.6040 (0.45)	0.3261 (6.63)	0.6043 (1.54)
MARRIED	-3.7961 (1.92)	-1.9986 (1.40)	0.1649 (2.94)	0.4519 (1.40)
RETIRED	1.8016 (0.69)	-0.3042 (0.17)	0.0139 (0.22)	-0.1003 (0.26)
BLACK	184.4732 (1.38)	-79.3341 (1.06)	-0.0699 (0.96)	-1.4056 (3.46)
CHURCH	0.5396 (0.32)	2.1649 (1.48)	-0.0812 (1.38)	0.2816 (1.03)
BENEFITS	1.4295 (0.62)	-1.2923 (0.80)	-0.2118 (3.48)	-1.9938 (4.71)
MOVED	-0.5942 (0.41)	0.6834 (0.53)	-0.0871 (1.78)	-0.3202 (1.00)
ONLYMALE	-0.9738 (0.28)	-0.6387 (0.33)	-0.0394 (0.56)	-0.8580 (1.41)
APRIL	0.1676 (0.78)	-0.0219 (0.08)		
MAY	-0.1681 (0.78)	-0.7640 (2.78)		
NOVEMBER			0.1364 (2.91)	0.6752 (5.09)
DECEMBER			0.1352 (2.86)	0.6655 (4.74)
SWBIAS			-0.0016 (1.86)	0.0018 (0.40)
SWBIAS/1000			-0.0562 (2.88)	-0.2844 (3.09)
ϱ				0.9370 (142.33)
MWCALLS	0.5019 (45.43)	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	4349	1542	1542
R^2 / Log–likelihood	0.254	0.657	-1532.756	-858.357
LM–Test	4545.40	3320.87		

The endogenous variable in the first two columns is the absolute difference between the actual and expected number of weekly calls during Spring of 1986. Income is always measured in logarithm of thousand dollars. Random Effects Panel 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. In the third and fourth columns the endogenous variable equals one whenever the household chose the measured option. The sample is balanced, with a total of 4,626 observations. The last two columns present the maximum likelihood estimates for the pooled sample and the estimates of the one–factor random effects probit model respectively. In both cases, absolute, choice–biased sampling, consistent, t–statistics are reported between parentheses.

Table 4. Tariff Choice and Switching

	Learning			Risk Aversion		
	Switching	Meas. to Flat	Flat to Meas.	Always Meas.	Risk Averse	Risk Lovers
Const.	-2.9860 (3.70)	-6.2301 (1.74)	-3.5631 (4.03)	-1.7119 (1.86)	15.6595 (0.91)	-1.6486 (2.28)
INCOME	0.6097 (2.47)	1.4020 (1.21)	0.7810 (2.86)	0.5707 (2.07)	-4.2389 (0.87)	0.3353 (1.49)
HHSIZE	0.2037 (1.24)	-0.1234 (0.18)	0.3194 (1.57)	-0.2183 (2.08)	-0.3455 (0.24)	-0.0719 (2.96)
TEENS	0.3867 (1.53)	4.3588 (2.52)	1.1644 (2.55)	-0.0361 (0.19)	12.9847 (0.00)	-0.0184 (0.56)
DINCOME	-0.1119 (2.34)	0.2496 (1.43)	-0.1724 (3.27)	-0.1502 (3.60)	1.0355 (1.17)	-0.1463 (4.25)
INCOME2/10	-0.3644 (1.85)	-0.9034 (0.99)	-0.4693 (2.10)	-0.4524 (2.15)	2.7711 (0.78)	-0.2502 (1.44)
HHSIZE2	0.0063 (2.82)	0.0146 (1.18)	0.0075 (3.19)	0.0030 (1.61)	0.1690 (0.67)	0.0041 (2.22)
TEENS2	-0.1716 (5.49)	-0.2950 (1.40)	-1.4603 (4.75)	0.0075 (0.96)	-6.8885 (0.00)	0.0013 (0.27)
INCOME*HHSIZE/10	-0.4039 (1.82)	-0.0267 (0.03)	-0.6091 (2.14)	0.2128 (1.44)		
INCOME*TEENS	-0.0585 (1.54)	-0.6009 (2.69)	-0.0332 (0.64)	-0.0101 (0.40)		
HHSIZE*TEENS	0.0578 (3.22)	0.0565 (0.33)	0.1345 (4.56)	0.0304 (2.02)		
AGE1	-0.1226 (1.85)	0.0587 (0.22)	-0.1855 (2.47)	-0.0414 (0.53)	7.2522 (0.00)	-0.0102 (0.14)
AGE3	-0.0784 (1.55)	-0.1335 (0.87)	-0.0879 (1.55)	-0.0212 (0.44)	0.2317 (0.39)	0.0079 (0.19)
COLLEGE	0.0119 (0.24)	0.0595 (0.73)	0.0542 (0.90)	0.1252 (2.35)	-0.5560 (1.03)	0.0702 (1.57)
MARRIED	0.0667 (1.39)	0.1874 (1.35)	0.0642 (1.21)	0.0431 (0.99)	-0.4076 (0.47)	0.0254 (0.68)
RETIRED	0.0196 (0.40)	-0.0666 (0.35)	0.0313 (0.58)	0.0669 (1.10)	1.5724 (1.30)	0.0243 (0.54)
BLACK	0.0960 (1.56)	-0.0040 (0.02)	0.0789 (1.14)	0.0079 (0.16)	-1.0254 (1.08)	-0.0382 (0.88)
CHURCH	-0.0011 (0.02)	-0.2634 (1.51)	-0.0005 (0.01)	-0.0201 (0.46)	1.2485 (1.44)	-0.0109 (0.28)
BENEFITS	-0.0173 (0.37)	0.3008 (1.42)	-0.0745 (1.43)	-0.0882 (1.75)	-1.7919 (1.52)	-0.0828 (2.12)
MOVED	-0.0509 (1.21)	-0.0973 (0.82)	-0.0722 (1.52)	-0.0187 (0.44)	0.8203 (1.13)	0.0120 (0.33)
ONLYMALE	-0.0638 (0.91)	0.1403 (1.04)	-0.1038 (1.26)	-0.0187 (0.24)	-0.0235 (0.03)	-0.0303 (0.44)
SWBIAS	0.0009 (2.03)	-0.0009 (0.31)	0.0013 (2.30)	0.0017 (4.38)	0.0978 (1.03)	-0.0005 (1.35)
SWBIAS2/1000	-0.0028 (3.11)	0.0063 (0.31)	-0.0073 (1.84)	-0.0019 (2.32)	3.5068 (0.86)	-0.0134 (6.41)
SAVINGS	0.0193 (13.54)	0.0476 (3.98)	0.0153 (10.90)			
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	1542	445	1097	1329	60	1163
Log-likelihood	-108.318	-17.982	-86.264	-188.345	-16.206	-153.193

The endogenous variable equals one if the household switches tariffs between October and December of 1986 in the first three columns; it equals one if the household chooses the optional measured tariff in the fourth and sixth columns, and the optional flat tariff in the fifth 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 fifth column for which the covariance matrix was singular after correcting for choice–biased sampling.