Attention vs Inattention: Evidence from the Kentucky Tariff Experiment*

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Abstract

Theoretical and experimental studies have shown that understanding when and why people are attentive and when and why people reason accurately or make systematic mistakes are central questions of the economics of human behavior. We study these questions empirically using a micro-panel data set with repeated observations from more than 2,500 subjects when optional measured tariffs for local telephone calls were introduced in Kentucky in an unanticipated manner. We find that consumers are attentive, tend to underestimate their demand for telephone services and initially make mistakes in their choice of tariffs. Yet, they actively engage in tariff switching in order to reduce the monthly cost of local telephone services despite savings of low magnitude. Households' reactions are not symmetric: those who face a more complex and cognitively more expensive tariff problem learn more slowly and are more likely to make mistakes than households that face a simpler tariff choice problem. Appropriately accounting for the effects of dynamic learning, state dependence and unobserved heterogeneity is critical for determining the underlying model of behavior supported by the data.

Keywords: Deliberation Costs, Inertia, State Dependence, Dynamic Discrete Choice Panel Data Models.

JEL Codes: D42, D82, L96.

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"It is evident that the rational thing to do is to be irrational where deliberation and estimation cost more than they are worth."

Frank Knight (1921), *Risk, Uncertainty and Profit.*

1 Introduction

Deliberation about economic decisions is a costly activity. Economists, psychologists and other social scientists have often expressed the idea that decision makers try to achieve a balance between the benefits of better decision making and the effort costs of decisions. According, for instance, to Stigler and Becker (1977):

> "The making of decisions is costly, and not simply because it is an activity which some people find unpleasant. In order to make a decision one requires information, and the information must be analyzed. The costs of searching for information and of applying the information to a new situation may be such that habit [and inertia] are sometimes a more efficient way to deal with moderate or temporary changes in the environment than would be a full, apparently utility-maximizing decision."

Similarly, in his Nobel lecture, Tobin (1982) wrote:

> "Some decisions by economic agents are reconsidered daily or hourly, while others are reviewed at intervals a year or longer except when extraordinary events compel revisions. It would be desirable in principle to allow for differences among variables in frequencies of change and even to make these frequencies endogenous. But at present, models of such realism seem beyond the power of our analytical tools."

The principles established in these statements seem so fundamental for our understanding of human behavior that we would expect they occupy a central role in the theory and empirics of

individual and aggregate decision making. Ideally, various decades of research would have produced detailed empirical evidence on the type of decision problems where consumers behave irrationally and the type of problems where they are rational. Further, we would know how the different types of behaviors depend on the size of information costs, time costs and perhaps even cognitive costs, as well as on the benefits of better decision making. And for the problems that consumers are attentive to, we would also have detailed information on whether subjects are able to reason accurately or tend to make systematic errors.

The fact, however, is that we are quite far from this ideal. There is an important recent theoretical literature modelling inattention and its implications in macroeconomics, and there is also an important theoretical literature on bounded rationality, but to the best of our knowledge there is no empirical evidence from real life (as opposed to experimental) settings that could be considered to belong to the ideal just described. A number of empirical problems that are typically insurmountable justify the existing situation. In natural settings there are often great difficulties:

1. in finding *individual* decision making situations, as opposed to market-level or other aggregate settings;[1]

2. in determining individuals' choice and strategy sets;

3. in observing and characterizing individuals' choices precisely;

4. in measuring the exact incentive structures that individuals face and all the relevant features of the environment;

5. in understanding the determinants of the endogenous frequency of choices;

[1] At the market or other aggregate levels downward-slopping demand functions can be derived *even* as consequences of agents' *random* choices subject to a budget constraint (*e.g.*, Becker (1962) and Gode and Sunder (1993)). As a result, it is generally not possible to distinguish rational from irrational behavior at *any* level of aggregation.

6. in carefully addressing selection problems in settings where preferences are endogenous to the environment or to the behavior of others.

In addition to these difficulties, sufficiently rich detailed data on individual choices are rarely available to allow researches to distinguish the dynamic effects of learning, attention and information from the effects of unobserved heterogeneity.

In this paper we take advantage of a unique opportunity to overcome these obstacles by studying a natural setting where none of these difficulties are present. South Central Bell (SCB) implemented a detailed tariff experiment for the Kentucky Public Service Commission in 1986. SCB collected demographic and economic information for about 2,500 households in Louisville. In the Spring of 1986, *all* households in Kentucky were on mandatory flat rates, paying \$18.70 per month with unlimited local telephone calls. This was the only tariff available. In July 1986, optional measured services were introduced for the first time in a way that was *unanticipated* by consumers. This alternative tariff included a \$14.02 monthly fixed fee, a \$5.00 allowance, and a tariff per call that depended on its duration, distance, and period (time of the day and day of the week). The basic problem that households faced each month was to determine whether their expected demand for local phone calls next month would be above or below \$19.02, as they would not be billed for the \$5.00 allowance unless their usage level exceeded this limit. That is, an attentive household would have to think at time t about the expected consumption level at $t+1$ and the tariff rate to be applied to that consumption level; consumption choices will then take place at time $t+1$. These choices were repeated every month and switching tariffs simply required a free phone call. A rich panel dataset on all the variables and characteristics of interest is available during the months of April-June and October-December.

The setting includes a number of desirable characteristics for evaluating whether or not the costs of attention and learning about their own demand were sufficiently high relative to the expected payoffs so as to induce rational inattention, and whether when subjects pay attention

to their consumption-tariff problem they are able to reason accurately or instead tend to make systematic mistakes. From an empirical perspective there are no market-level data, no endogenous preferences, and no learning from others. More precisely:

1. We have an individual decision making situation;
2. It is trivial to determine strategy sets;
3. It is straighforward to observe and characterize individuals' choices;
4. It is easy to measure the incentives and rewards that subjects face;
5. The frequency of choices (monthly) is exogenously given; that is, there is no endogenous timing of decisions;
6. Individuals are as close as possible to a *tabula rasa* since they could not possibly know their demand prior to the introduction of the alternative tariff because phone calls were not priced at the margin in the flat rate tariff regime;
7. There are no self-selection problems since the penetration of telephone service is basically 100 percent of the population; in this sense, we have a truly representative sample;
8. Data on many economic and demographic variables, including data on consumers' expectations about their own consumption, are available for about 2,500 consumers. Importantly, the data are rich enough to allow the dynamic effects of learning and incentives to be distinguished from the effects of unobserved heterogeneity.

We estimate a dynamic discrete choice panel data model and find that:

1. Consumers are attentive. No rational inattention is observed, that is consumers' behavior is not characterized by habit or inertia. Instead, they actively engage in tariff switching in order to reduce the monthly cost of local telephone services;

2. While consumers facing the new consumption option do make mistakes initially, these mistakes are not systematic;

3. Households' reactions are not symmetric: those who face a more complex and cognitively more expensive tariff problem learn more slowly and are more likely to make mistakes than households that face a simpler tariff choice problem.

4. The magnitude of the differences between the alternative tariff schemes is typically quite small (about \$5-\$6 dollars per month), and yet we find that subjects' responses reacting to these potential savings are significant and sizeable;

5. We find no evidence of impulsiveness or time-inconsistent choices;

6. Lastly, the analysis yields an empirical methodological contribution that may be important for future research. Despite all of the desirable characteristics, the empirical analysis is far from trivial or straightforward since we need to estimate a binary choice panel data model with lagged endogenous variables and unobserved heterogeneity. As is well known, parameter estimates from short panels jointly estimated with individual fixed effects can be seriously biased and inconsistent when the explanatory variables are only predetermined as opposed to strictly exogenous (see Arellano and Honoré (2001) for an overview). In our case, the distinction between the sources of persistence turns out to be critical for determining the underlying model of behavior. Indeed, failing to control appropriately for the effect of state dependence and unobserved heterogeneity in our setting yields radically different results: the evidence would support models of inattentiveness and systematic mistakes when in fact no such support exists.

The remainder of our study proceeds as follows: Section 2 provides a brief literature review. Section 3 describes in detail the Kentucky tariff experiment including the data set and some

descriptive evidence. Section 4 presents our dynamic discrete choice panel data model, Section 5 the empirical results, and Section 6 concludes.

2 Literature Review

An important literature has recently explored the potential of modelling rational inattention in consumers and producers. Reis (2006a) studies the consumption decisions of agents who face costs of acquiring, absorving and processing information. His model predicts that aggregate consumption adjusts slowly to shocks, and is able to explain the excess sensitivity and excess smoothness puzzles.[2] Reis (2006b) studies the same problem for producers and applies the results to a model of inflation. The resulting model fits remarkably well a number of quantitative facts on post-war inflation. Mankiw and Reis (2002) and Ball, Mankiw, and Reis (2005) study inattentiveness on the part of price-setting firms and find that the resulting model matches well the dynamics of inflation and output observed in the data. In the finance literature, Gabaix and Laibson (2002) assume that investors update their portfolio decisions infrequently, and show that this can help explain the equity premium puzzle. In the large and growing literature on bounded rationality, the importance of deliberation and processing costs is relevant for most theories that postulate deviations from the assumption of rational, computationally unconstrained agents.[3]

[2] Sims (2003) and Moscarini (2004) develop alternative models focusing on the information problem that agents face.

[3] These include the behavioral economics literature (*e.g.*, Sims (1955) and Sims (1987)), the learning and robustness literatures in macroeconomics (*i.e.*, Sargent (1993), Hansen and Sargent (2008)), the game theory literature (Rubinstein (1998)), the study of the demand for information in Bayesian decision theory (Moscarini and Smith (2001) and Moscarini and Smith (2002)), the determinants of the adoption of rules of thumb in individual and social learning contexts (Ellison and Fudenberg (1993)), the study of cognitive dissonance and near-rational theories (Akerlof and Dickens (1982) and, Akerlof and Yellen (1982), the study of business decisions and long-term contracting situations (Bolton and Fauré-Grimaud (2005b) and Bolton and Fauré-Grimaud (2005a)) and others. On the infinite regress problem, see Savage (1954) and Lipman (1991).

With respect to microeconomic empirical evidence, it mainly comes from survey and experimental studies. Lusardi (1999), Lusardi (2003) and Americks, Caplin, and Leahy (2003), for instance, find that a significant fraction of survey respondents make financial plans infrequently and that their behavior has a significant impact on the amount of wealth that they accumulate. In a survey two months after the announcement by President George H. Bush of a reduction in the withholding rates for income taxes, Shapiro and Slemrod (1995) find that about half of the respondents were simply not aware of any change in those rates. Carroll (2003) uses survey data on inflation expectations to study how news disseminates throughout the population.

In the experimental literature, Gabaix, Laibson, Moloche, and Weinberg (2006) study a cognition model which successfully predicts the aggregate empirical regularities of information acquisition both within and across experimental games. Costa-Gomes, Crawford, and Broseta (2006), and Costa-Gomes and Crawford (2006) also study cognition and behavior in different experimental games. When subjects are attentive, it is then important to know whether they get it right or wrong. Conslik (1996) reviews the literature on bounded rationality, including experimental studies where subjects make errors in updating probabilities, display overconfidence, and violate several assumptions of unbounded rationality, as well as others where subjects reason accurately, especially after practice.[4] Ultimately, he concludes, the important question is "when and why people get it right or wrong."

Thus, these studies show that modeling attention and experimentally studying the predictions of limited rationality models offer a great deal of promise for improving our understanding of human decision making. Relative to the existing theoretical, survey and experimental literature this paper differs by providing what, to the best of our knowledge, is the first empirical microeconometric study of attentiveness and reasoning in a real world setting using a large panel dataset of a fully representative sample.

[4] Arrow (1987) and Lucas (1987) discuss some limitations of experiments to study bounded rationality.

3 Description of the Tariff Experiment

In the second half of 1986, South Central Bell (SCB) carried out a detailed tariff experiment aimed at providing the Kentucky Public Service Commission (KPSC) with evidence in favor of authorizing the introduction of optional measured tariffs for local telephone service. Prior to this tariff experiment, in the Spring of 1986, all households in Kentucky were on mandatory flat rates and SCB collected demographic and economic information for about 2,500 households in the local exchange of Louisville. In July of 1986, the tariff was modified in this city. Customers were given the choice to remain in the previous flat tariff regime—paying $18.70 per month with unlimited calls—or switch to the new measured service option. The measured service included a $14.02 monthly fixed fee, a $5.00 allowance,[5] and distinguished among setup, duration, peak periods, and distance.[6] Choices could be made every month and, unless a household indicated to SCB otherwise, its current choice of tariff would serve as default choice for the following month.[7] The regulated monopolist also collected monthly information on usage (number and duration of calls classified by time of the day, day of the week, and distance within the local loop), and payments during two periods of three months, one right before (March-May) and the other (October-December) three months after the measured tariff option was introduced.[8]

[5] Consumers on the measured option were not billed for the first $5.00 unless their usage exceeded that limit. Thus, depending on the accumulated telephone usage over a month, a marginal second of communication could cost $5.00.

[6] The tariff differentiated among three periods: peak was from 8 a.m. to 5 p.m. on weekdays; shoulder was between 5 p.m. to 11 p.m. on weekdays and Sunday; and off-peak was any other time. For distance band A, measured charges were 2, 1.3, and 0.8 cents for setup and price per minute during the peak, shoulder, and off-peak period, respectively. For distance band B, setup charges were the same but duration was fixed at 4, 2.6, and 1.6 cents, respectively.

[7] Switching tariffs simply required a free phone call to request the change of service.

[8] This data set has been used in the past. Miravete (2002) identifies the distributions of *ex-ante* and *ex-post* telephone usage to evaluate the profit and welfare performance of sequential pricing mechanisms consisting of optimal two-part tariffs. Miravete (2003) evaluates the effect of expectations of future consumption as stated by consumers and the role of potential savings in driving household tariff switching behavior. Miravete (2005) uses the empirical distribution of future expected consumption of this data to evaluate the profit and welfare performance of sequential

The data set has a number of valuable features. First, local telephony is a basic service and its market penetration is close to 100% in the U.S. Thus, there are no potential self-selection problems or conspicuous consumption considerations that may lead to biased estimates because of selection into this market. Second, it is safe to rule out any risk aversion argument that could otherwise explain systematic mistakes regarding the choice of tariff options because of the low magnitude of the cost differences between the alternative tariff choices relative to the average household income. Third, it is valuable for the purpose of the analysis that in addition to demographic and economic variables, SCB also collected information on customers' own telephone usage expectations in the Spring of 1986, which is a good approximation of consumers' own expected satiation levels as marginal tariffs were nil. Fourth, given that the flat tariff regime means that local calls were not priced at the margin, households might not be aware, at least not perfectly, of their own actual demand for local phone calls at the time of the experiment.[9]

Households receive every month the bill of their consumption. In this sense, the costs of searching for information are minimal, and thus the costs of deliberation and cognition, relative to the expected payoffs, would likely be the main, and perhaps only, determinant of their behavior. Moreover, there is an important asymmetry in the cognitive costs associated with the problem that households face in the different tariff options. Households in the measured tariff simply need to compare their actual bill with the $18.70 cost of the alternative flat tariff in order to ascertain whether or not they made a mistake. Households in the flat tariff option face a much more complex problem: they would need to monitor every phone call and compute whether the total cost of all of their calls in the month would have been above or below $19.02 had they subscribed the measured

pricing mechanisms where options are fully nonlinear tariffs. Finally, Narayanan, Chintagunta, and Miravete (2007) estimate a structural discrete/continuous model of plan choice and demand of local telephone service where consumers update of future usage expectation is conditioned by the choice of tariff made. Relative to these papers, the present one addresses the difficult issue of unobserved heterogeneity due to state dependence which translates into testing whether inertia rather than experimentation through tariff switching better explains the behavior of households.

[9] Measured tariffs were rarely offered in the U.S. before the breakup of AT&T, and local telephone services typically consisted of just a flat monthly fee as in Louisville Mitchell and Vogelsang (1991).

Table 1: Variable Definitions and Descriptive Statistics

Variables	Description	ALL		FLAT		MEASURED	
MEASURED	Optional measured service chosen this month	0.2971	(0.46)	0.0000	(0.00)	1.0000	(0.00)
EXPCALLS	Household own estimate of weekly calls	26.8884	(31.34)	30.1341	(35.05)	19.2104	(17.78)
CALLS	Current weekly number of calls	37.6093	(38.48)	44.4898	(42.62)	21.3326	(17.64)
BIAS	*CALLS — EXPCALLS*	10.7209	(39.92)	14.3558	(45.67)	2.1223	(18.04)
SWCALLS	Household average calls during Spring	37.9434	(37.16)	44.0499	(40.80)	23.4980	(20.32)
SWBIAS	*SWCALLS — EXPCALLS*	11.0550	(39.37)	13.9158	(44.55)	4.2876	(21.39)
BILL	Monthly expenditure in local telephone service	19.4303	(4.41)	18.7000	(0.00)	21.1578	(7.82)
SAVINGS	Potential savings of switching tariff options	−9.9223	(16.53)	−15.1557	(16.45)	2.4578	(7.82)
SAVINGS-SPR	Pot. sav. of subscribing the measured option	−15.4206	(15.27)	−18.7859	(16.21)	−7.4596	(8.56)
SAVINGS-OCT	Potential savings in October	−9.4898	(16.99)	−14.2444	(17.61)	1.7578	(7.60)
SAVINGS-NOV	Potential savings in November	−9.2864	(15.03)	−13.6444	(15.30)	1.0230	(7.47)
SAVINGS-DEC	Potential savings in December	−10.9908	(17.41)	−16.4967	(17.22)	2.0340	(8.83)
INCOME	Monthly income of the household	7.0999	(0.81)	7.0767	(0.84)	7.1547	(0.74)
HHSIZE	Number of people who live in the household	2.6168	(1.51)	2.7858	(1.56)	2.2170	(1.28)
TEENS	Number of teenagers (13–19 years)	0.2440	(0.63)	0.2908	(0.68)	0.1336	(0.49)
DINCOME	Household did not provide income information	0.1577	(0.36)	0.1831	(0.39)	0.0977	(0.30)
AGE = 1	Household head between 15 and 34 years old	0.0632	(0.24)	0.0614	(0.24)	0.0676	(0.25)
AGE = 2	Household head between 35 and 54 years old	0.2686	(0.44)	0.2604	(0.44)	0.2880	(0.45)
AGE = 3	Household head above 54 years old	0.6682	(0.47)	0.6782	(0.47)	0.6444	(0.48)
COLLEGE	Household head is a college graduate	0.2240	(0.42)	0.1821	(0.39)	0.3230	(0.47)
MARRIED	Household head is married	0.5253	(0.50)	0.5342	(0.50)	0.5042	(0.50)
RETIRED	Household head is retired	0.2433	(0.43)	0.2417	(0.43)	0.2471	(0.43)
BLACK	Household head is black	0.1161	(0.32)	0.1295	(0.34)	0.0843	(0.28)
CHURCH	Telephone used for charity and church matters	0.1711	(0.38)	0.1785	(0.38)	0.1536	(0.36)
BENEFITS	Household receives federal or state benefits	0.3095	(0.46)	0.3282	(0.47)	0.2654	(0.44)
MOVED	Household head moved in the past five years	0.4025	(0.49)	0.3899	(0.49)	0.4324	(0.50)
Observations		1,344		949		395	

Mean and standard deviation of demographics and usage variables. This balanced sample contains 1,344 household observations. Income is measured in logarithms of thousands of 1986 dollars.

service, where each call is metered differently depending on their duration, distance, and periods. Clearly, this task is much more complex and requires a great deal of monitoring effort. Empirically, we would expect that these asymmetric cognitive costs are an important driving force of observed behavior.

Table 1 defines the different variables and presents basic descriptive statistics for the whole sample and for two groups of consumers split according to their choice of tariff in October. Only active consumers were considered and a number of observations with missing values for some

variables were excluded.[10] These descriptive statistics initially suggest that individual heterogeneity in consumption and tariff subscription is important. Consumers who subscribe to the FLAT and MEASURED tariffs are in fact quite different. Households subscribing to the optional FLAT service, for instance, are on average larger, with teenagers, and with a lower level of education than those subscribing to the and MEASURED tariff. Further, they not only differ in their level of local telephone usage —as captured by CALLS— but also in their expectations regarding future telephone usage.

Despite all the remarkable features of the data, there are two issues that need to be addressed econometrically. First, about 10% of consumers switched to the optional measured option when given that possibility. Our sample, however, includes 30% of those customers. Choice-based sampling bias can easily be dealt with using well known methods (see Amemiya (1985, §9.5)). All estimates reported in tha analysis take into account this choice-based sampling as we use the weighting procedure of Lerman and Manski (1977) to obtain choice-based, heteroskedastic-consistent, standard errors. Second, when the tariff experiment began in July of 1986, all households were assigned the preexisting flat tariff as default option. Consumers may learn about their telephone usage profile as they switch tariff options, and thus, over time, they will differ in experience as summarized by the sequence of past tariff choices and usage levels. Testing the economic hypothesis of inertia in the choice of tariff options requires estimating the effect of past choices on the probability of choosing a particular tariff option. Obtaining a consistent estimate of these effects requires controlling for the possibility of unobserved individual heterogeneity due to unobserved sequences of past choices. To that end we use the semiparametric estimator suggested by Arellano and Carrasco (2003) in Section 4.

[10] Miravete (2002) documents that excluding households with missing information does not lead to biased results. The only variable with a substantial number of missings is income. In these cases we recoded the missing observations to the yearly average income of the population in Louisville and also included a dummy variable, DINCOME, to control for non-responses regarding household earnings.

In an attempt to examine whether households tend to choose the *ex-post* correct tariff option for their usage level, we first study the pattern of correlation among the decisions using a simple static model of simultaneous choice of tariff plan and usage level.[11] We estimate the following reduced form model:

$$y_j^* = X\Pi_j + v_j\,, \qquad j = 1, 2, \tag{1}$$

and where, conditional on observed demographics, we assume that:

$$(v_1, v_2) \sim N\left(\mathbf{0}, \mathbf{\Sigma_v}\right)\,; \qquad \mathbf{\Sigma_v} = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}\,. \tag{2}$$

These two reduced-form equations are estimated simultaneously as a bivariate probit model, thus providing an estimate of ρ. In this model $y_1 = 1$ if the household subscribes the measured tariff, M, and $y_2 = 1$ if the household realizes a *low usage* level L, defined as a consumption level below \$19.02 when metered according to the measured tariff rates, so that a positive estimate of ρ can be interpreted as unobservable element inducing the appropriate tariff choice for each usage level. The model includes the same set of demographic variables in both equations to control for the effect of observable individual heterogeneity over the tariff choice and consumption decisions. Data also include household specific information from the Spring months to control, at least in part, for the accuracy of predictions of individual future usage. We thus include two dummies to indicate whether consumers significantly over or underestimated future consumption when marginal consumption was not priced at all.[12] Similarly, we construct an indicator of usage intensity for each household during the Spring months, LOW USAGE$_{Spring}$, which equals one when the usage

[11] The approach is similar to Chiappori and Salanié (2000) and a significant correlation coefficient in this estimation supports the idea of the existence of asymmetric information beyond the observable demographics of our data.

[12] The UNDEREST dummy is equal to one if SWCALLS exceeds EXPCALLS by more than 50% of the standard deviation of SWBIAS. The OVEREST dummy is defined accordingly when EXPCALLS exceeds SWBIAS.

Table 2: Choice of Tariff and Usage Level

	MEASURED		LOW USAGE	
CONSTANT	−0.6763	(5.56)	−0.8099	(7.06)
LOW INC	−0.0604	(0.57)	0.0418	(0.46)
HIGH INC	−0.2317	(1.79)	−0.0320	(0.32)
DINCOME	−0.4846	(4.23)	−0.1144	(1.43)
HHSIZE = 2	−0.3548	(3.32)	−0.3128	(3.46)
HHSIZE = 3	−0.5645	(4.29)	−0.3979	(3.81)
HHSIZE = 4	−0.4854	(3.17)	−0.3866	(2.97)
HHSIZE > 4	−0.7187	(4.04)	−0.6709	(4.22)
TEENS	−0.1768	(1.27)	0.0115	(0.11)
AGE = 1	−0.0216	(0.14)	0.1761	(1.38)
AGE = 3	−0.0491	(0.53)	0.1707	(2.03)
COLLEGE	0.2910	(3.42)	0.0709	(0.93)
MARRIED	0.2301	(2.47)	−0.0509	(0.66)
RETIRED	0.0497	(0.43)	−0.1967	(2.24)
BLACK	0.0287	(0.26)	−0.1845	(1.72)
CHURCH	−0.0274	(0.30)	−0.0084	(0.11)
BENEFITS	−0.2189	(2.03)	−0.0360	(0.42)
MOVED	−0.0542	(0.64)	0.0915	(1.24)
OVEREST	−0.3548	(2.42)	−0.7881	(5.17)
UNDEREST	−0.4164	(4.14)	−1.1597	(9.70)
LOW USAGE$_{Spring}$	0.6418	(4.87)	1.4125	(11.26)
ρ	0.8408	(7.46)		
$\ln \mathcal{L}$	−2,463.197			
Observations	4,032			

The endogenous variable MEASURED equals one if the household subscribes the optional measured service during the current month. The UNDEREST dummy indicates that SWCALLS exceeds EXPCALLS by more than 50% of the standard deviation of SWBIAS. The OVEREST dummy is defined accordingly when EXPCALLS exceeds SWBIAS. The LOW USAGE dummy indicates whether the monthly consumption during the Spring months would have exceeded the $19.02 threshold if billed according to the optional measured tariff available during the second half of 1986. Estimates are obtained by weighted maximum likelihood (bivariate probit). Absolute, choice-biased sampling, heteroscedastic consistent, t-statistics are reported between parentheses.

level during Spring (at zero marginal charge) is less than $19.02 had it been metered according to the optional measured tariff that will later be in place during the Fall. We include this variable in order to account for any systematic effect of demographics not included in our data on usage. Table 2 reports the estimates of these reduced form parameters.

The positive estimate of ρ reflects the correlation between the choice of the measured service and a low demand realization. This suggests that consumers do not appear to make

systematic mistakes when choosing among optional tariffs. Furthermore, estimates of the effect of demographics suggest that when mistakes are made, it is more likely that they are made when a household subscribes to the measured service rather than to the flat tariff option. Consumers on the measured option enjoy a *de facto* negligible deliberation cost as they have just to compare their past monthly bill to the cost of the flat option to decide whether to switch tariff plans.

Thus, for instance, larger households tend to subscribe to the flat tariff option and to realize high usage levels, which is the less expensive option for the telephone usage profile. At the other end, households whose head holds a college degree are inclined to subscribe to the measured service option but, conditional on having subscribed the measured option, they are also more likely to realize a high demand and, thus, to have (incorrectly) chosen the measured option *ex-post*. A similar pattern arises for MARRIED couples.[13]

Finally, observe that households with a low usage profile during the Spring months are more likely to present a low usage pattern in the Fall months as well, and also to choose (correctly) the measured tariff option. Consumers that either over or underestimated their future telephone usage quite significantly are less likely to subscribe to the measured option, but are also far less likely to realize a low usage level. Thus, households who made the largest absolute forecast errors are among those with very high levels of demand, and thus, they are more likely to choose the right option by subscribing to the flat tariff.

After this descriptive evidence, we turn the arguments toward the more substantive questions: Do consumers simply stay on their previously chosen tariff because of inertia or rational inattention? Are the consumption levels, tariff choices, and the switching that we observe in the data sufficient to provide evidence that consumers are attentive and respond to potential savings? What is the role of previous tariff choices and demand realizations on the decision to subscribe to

[13] Consumers are classified as having chosen correctly or incorrectly each tariff option *ex-post* keeping the usage pattern unchanged, that is independently of price responses. This provides an approximate upper bound to the gains of switching to a different tariff option.

one of the two options? In order to answer these questions we need more sophisticated econometric methods that allow us to account for state dependence, unobserved heterogeneity, and dynamic learning. We study such model next.

4 Econometric Model

In this section we first present a semi–parametric, random effects, discrete choice model with predetermined variables based on the work by Arellano and Carrasco (2003) that controls for the effect of unobservable heterogeneity due to state dependence. We also discuss why this model offers useful advantages over the very few alternative approaches available in the literature. We then implement this model to study the choices of tariffs and consumption levels. Furthermore, we point out the importance of instrumenting for the lagged endogenous variables in this model appropriately, and how, ignoring endogeneity may lead to dramatically opposite results regarding the effect of experience on telephone usage and tariff choices.

4.1 A Dynamic Discrete Choice Panel Data Model

The model defines conditional probabilities for every possible sequence of realizations of state variables in order to deal with regressors that are predetermined but not exogenous, such as the previous choices of tariffs and the past realizations of demand in our setting. Then, the estimator computes the probability of subscribing to a given tariff along every possible path of past realizations of demand and subscription decisions. The panel data structure allows us to identify the effect of individual unobserved heterogeneity since consumers make different decisions even if they share the same history of realizations of state variables.

The probability of subscribing to a given tariff option, and hence the probability of switching tariffs in the future, depends on the particular sequence of past choices and past realizations of

demand for each consumer. As consumers choose differently, they accumulate different experiences and invest differently in information gathering and deliberation efforts. These experiences in turn change the information set upon which they decide in the future. For instance, consumers that have previously chosen the measured option may have learned that their demand is systematically high, so that in the future they will be more likely to subscribe to the flat tariff option. Consumers that have always remained on the flat tariff option have accumulated different experiences, which also affects their conditional probability of renewing their subscription to the flat tariff option. Given that their consumption was never priced at the margin in any range, these households may have much less knowledge of their own demand than those that at some point subscribe to the measured service. To be more specific, the probability of subscribing to a given tariff option may depend on some intrinsic characteristics of consumers, as well as on their expectation on the realization of demand. This can be written as follows:

$$y_{it} = \mathbb{1}\left\{\beta z_{it} + E\left(\eta_i \mid w_i^t\right) + \varepsilon_{it} \geq 0\right\}, \qquad \varepsilon_{it} \mid w_i^t \sim N\left(0, \sigma_t^2\right), \tag{3}$$

where $y_{it} = 1$ ($y_{it} = 0$) if the measured (flat) tariff option is subscribed; the set of predetermined variables z_{it} includes a constant plus the past realization of demand and the previous choices of tariffs, $y_{i(t-1)}$; $w_i^t = \{w_{i1}, ..., w_{it}\}$ is the history of past choices represented by a sequence of realizations: $w_{it} = \left\{x_{it}, y_{i(t-1)}\right\}$; and η_i is an individual effect whose forecast is revised each period t as the information summarized by the history w_i^t accumulates.[14]

In our case η_i is the future individual realization of demand. The conditional distribution of the sequence of expectations $E\left(\eta_i \mid w_i^t\right)$ is left unrestricted, and hence the process of updating expectations as information accumulates is not explicitly modeled. This is the only aspect that

[14] The specification of Arellano and Carrasco (2003) is more general in the sense that it also includes a time-varying component common to all individuals. With the exception of monthly indicators, all our available demographics are time-invariant. We also included these monthly indicators in our empirical analysis but they did not improve our estimations, even when interacted with past subscription decisions and past realizations of demand.

makes the model semi–parametric. While the assumption of normality of the distribution of errors is not essential, the assumption that the errors ε_{it} are not correlated over time is necessary for the estimation. Since errors are assumed to be normally distributed, conditional on the history of past decisions, the probability of choosing the measured option at time t for any given history w_i^t can be written as:

$$Prob\left(y_{it}=1 \mid w_i^t\right)=\Phi\left[\frac{\beta z_{it}+E\left(\eta_i \mid w_i^t\right)}{\sigma_t}\right] . \tag{4}$$

Since all our regressors are dichotomous variables, their support is a lattice defined by $2J$ nodes $\{\phi_1, ..., \phi_{2J}\}$. The $t \times 1$–vector of regressors $z_i^t = \{z_{i1}, ..., z_{it}\}$ has a multinomial distribution and may take up to J^t different values. Similarly, the vector w_i^t is defined on $(2J)^t$ values, for $j = 1, ..., (2J)^t$. Given that the model has discrete support, any individual history can be summarized by a cluster of nodes representing the sequence of tariff choices and demand realizations for each individuals in the sample. Thus, the conditional probability can be rewritten as:

$$p_{jt}=Prob\left(y_{it}=1 \mid w_i^t=\phi_j^t\right) \equiv h_t\left(w_i^t=\phi_j^t\right) , \qquad j=1, \ldots,(2J)^t . \tag{5}$$

In order to remove the unobserved individual effect we account for the proportion of customers with identical history up to time t that subscribe to the measured tariff option M at each time t. We then repeat this procedure for every history that exists in our data. For each history we compute the percentage of consumers that subscribe to M. This provides a simple estimate of the unrestricted probability $\hat{p}_{tj}$ for each possible history present in the sample. Then, by taking first differences of the inverse of the equation above we get:

$$\sigma_t \Phi^{-1}\left[h_t\left(w_i^t\right)\right]-\sigma_{t-1} \Phi^{-1}\left[h_{t-1}\left(w_i^{t-1}\right)\right]-\beta\left(x_{it}-x_{i(t-1)}\right)=\xi_{it} , \tag{6}$$

and, by the law of iterated expectations, we have:

$$E\left[\xi_{it} \mid w_i^{t-1}\right] = E\left[E\left(\eta_i \mid w_i^t\right) - E\left(\eta_i \mid w_i^{t-1}\right) \middle| w_i^{t-1}\right] = 0\,. \tag{7}$$

This conditional moment condition serves as the basis of the GMM estimation of parameters β and σ_t (subject to the normalization restriction that $\sigma_1 = 1$). Arellano and Carrasco (2003) show that there is no efficiency loss in estimating these parameters by a two–step GMM method where in the first step the conditional probabilities p_{tj} are replaced by unrestricted estimates $\hat{p}_{tj}$, such as the proportion of consumers with a given history that subscribe to the measured service. Then:

$$\hat{h}_t\left(w_i^t\right) = \sum_{j=1}^{(2J)^t} \mathbf{1}\left\{w_i^t = \phi_j^t\right\} \cdot \hat{p}_{tj}\,, \tag{8}$$

which is used to define the sample orthogonality conditions of the GMM estimator:[15]

$$\frac{1}{N}\sum_{i=1}^{N} d_{it}\Big\{\sigma_t \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right] - \sigma_{t-1}\Phi^{-1}\left[\hat{h}_{t-1}\left(w_i^{t-1}\right)\right] - \beta\left(x_{it} - x_{i(t-1)}\right)\Big\} = 0\,, \qquad t = 2, \ldots, T\,, \tag{9}$$

where d_{it} is a vector containing the indicators $\mathbf{1}\left\{w_i^t = \phi_j^t\right\}$ for $j = 1, ..., (2J)^{t-1}$.

4.2 Alternative Approaches

Consumers may, in principle, invest in all sort of non-observable effort to determine what is the right choice of tariff and usage profile for them. However, the econometrician only observes the sequence of usage levels and tariff choices that consumers make. Since consumer actions are likely to be conditioned by the individual history of choices, we need to control for state dependence as

[15] In practice the number of moment conditions is smaller than $\sum_t (2J)^{t-1}$ because we only consider clusters with at least 4 observations. Also, we use the orthogonal deviations suggested by Arellano and Bover (1995) rather than first differences among past values of the state variables.

consumers may differ by having taken a different sequence of decisions that we do not completely observe. It is important to note that households have already accumulated different individual experiences through their different choices during the July-September period. Since these pre-sample individual decision paths are not observable to us, we also have to deal with the "initial conditions problem" in the estimation of our econometric model. Had SCB collected data on tariff choices and usage decisions during the six months of the tariff experiment, we would not be facing this problem because *all* consumers in Louisville were priced according to the flat tariff option prior to the beginning of the experiment and, hence, they would all have shared the same initial condition.

The initial conditions problem could be ignored if there was a way of knowing with certainty that in just three months all consumers had reached a stationary equilibrium. But since this is probably not the case, failing to address the initial conditions problem likely leads to inconsistent estimates. One reason for this inconsistency is that initial conditions at the beginning of our sample become endogenous if errors are correlated. A potential solution is to consider that each unobserved individual path of discrete decisions prior to the initial month of data collection has an effect on the probability of subscribing to the measured option only through individual fixed effects. Unfortunately, and with few exceptions, discrete choice models with fixed effects cannot be consistently estimated with finite samples because of the well-known "incidental parameter problem.[16]

There are very few results in this literature. The only discrete choice models where the incidental parameter problem is not present is the conditional maximum likelihood estimator of Chamberlain (1980) for the logit and Poisson regression. In order to deal with the issue of state dependence, Honoré and Kyriazidou (2000) include one lagged dependent variable but require

[16] On the statistical problems originated by the initial conditions problem, including its relationship with the incidental parameter problem, see Heckman (1981). On the impossibility of obtaining consistent fixed-effect estimates with finite samples, see Neyman and Scott (1948) and Lancaster (2000).

that the remaining explanatory variables are strictly exogenous, thus excluding the possibility of a lagged dependent regressor. This rules out, for instance, that we condition tariff choices on past usage levels and monthly indicators. Honoré and Lewbel (2002) allow for additional predetermined variables but at the cost of requiring a continuous, strictly exogenous, explanatory variable that is independent of the individual effects, *i.e.*, we could condition tariff choice on past usage but not *vice versa* and estimates would be consistent only as long as individual effects are uncorrelated with telephone usage, an assumption that is highly questionable.

An alternative to the logit specification is the maximum score estimator of Manski (1987). However, in addition to the strict exogeneity of regressors this estimator also requires stationarity in order to avoid the initial conditions problem. But stationarity should not be expected in our sample which contains data collected just three months after the tariff experiment was launched.

In addition to fixed effects models, research has also contemplated random effects models in order to deal with unobserved heterogeneity in discrete choice problems, *e.g.*, Chamberlain (1984) and Newey (1994). However, beyond the common requirement of strict exogeneity of regressors, random effects models have the disadvantage that the identification of parameters depends critically on the arbitrary choice of the conditional distribution of individual effects by the econometrician. This is not the case in our model because, as pointed out earlier, the conditional distribution of the individual effect $E\left(\eta_i \mid w_i^t\right)$ is not explicitly modeled.

Finally, one additional reason in favor of choosing the approach of Arellano and Carrasco (2003) is that our short panel fits the identification requirements of their GMM estimator. Even if we were willing to impose the necessary additional assumptions, alternative fixed-effects approaches such as Honoré and Kyriazidou (2000) and Honoré and Lewbel (2002) are also far more demanding in terms of data. In particular, they require variation of the exogenous regressors over time, something that does not occur in our data set, and a minimum of a four period panel.

Table 3: Attention and Inertia in Tariff Subscription (GMM)

Sample:	CONSTANT		LOW USAGE$_{t-1}$		MEASURED$_{t-1}$		d.f.	Obs.
ALL	−1.9751	(7.99)	−4.4181	(17.88)	−8.9011	(36.02)	9	4,032
LOW INC = 1	−2.3919	(6.22)	1.1055	(2.87)	−20.0065	(52.02)	8	682
LOW INC = HIGH INC = 0	−1.9692	(7.35)	−5.5032	(20.54)	−6.0887	(22.73)	9	2,934
HIGH INC = 1	−2.1159	(5.00)	−6.2151	(14.68)	−12.4203	(29.34)	8	416
DINCOME = 1	−3.1042	(7.09)	−10.1293	(23.14)	−8.2131	(18.76)	7	697
DINCOME = 0	−1.8781	(7.46)	−3.5418	(14.06)	−8.1274	(32.26)	9	3,335
HHSIZE = 1	−1.2827	(3.64)	−3.2181	(9.13)	−4.3519	(12.35)	9	834
HHSIZE = 2	−1.6469	(5.16)	−6.5772	(20.60)	−11.5899	(36.29)	9	1,333
HHSIZE = 3	−2.6187	(6.82)	−5.4355	(14.16)	−6.3259	(16.48)	6	827
HHSIZE = 4	−2.3548	(5.86)	−11.4859	(28.57)	−16.0243	(39.86)	6	597
HHSIZE > 4	−3.4691	(6.82)	−13.4427	(26.44)	−31.7962	(62.54)	4	443
TEENS = 1	−3.1895	(7.63)	−25.6940	(61.46)	−25.8714	(61.89)	5	766
TEENS = 0	−1.8713	(7.41)	−2.9598	(11.72)	−7.3084	(28.93)	9	3,266
AGE = 1	−1.9711	(4.18)	−4.7308	(10.04)	−7.9214	(16.81)	6	240
AGE = 2	−1.9399	(5.79)	−4.1165	(12.28)	−5.6042	(16.71)	8	1,073
AGE = 3	−2.0563	(7.48)	−4.6915	(17.07)	−9.9864	(36.34)	9	2,719
COLLEGE = 1	−1.1912	(3.35)	−5.7461	(16.15)	−5.4816	(15.40)	8	808
COLLEGE = 0	−2.2028	(8.25)	−4.2893	(16.07)	−9.9372	(37.23)	9	3,224
MARRIED = 1	−1.6761	(5.42)	−11.7802	(38.08)	−15.1276	(48.91)	9	2,138
MARRIED = 0	−2.0548	(6.99)	−2.8714	(9.76)	−5.6511	(19.22)	9	1,894
RETIRED = 1	−1.9671	(5.63)	−5.5897	(15.99)	−12.6135	(36.09)	8	983
RETIRED = 0	−1.9684	(7.42)	−4.6514	(17.52)	−7.8735	(29.66)	9	3,049
BLACK = 1	−2.7295	(6.14)	−3.3922	(7.62)	−7.5027	(16.86)	6	504
BLACK = 0	−1.8738	(7.30)	−4.8573	(18.92)	−9.7249	(37.88)	9	3,528
CHURCH = 1	−2.1763	(5.56)	−5.3369	(13.63)	−4.7470	(12.13)	8	711
CHURCH = 0	−1.9526	(7.58)	−4.3052	(16.70)	−10.1812	(39.50)	9	3,321
BENEFITS = 1	−2.3831	(7.11)	−2.3833	(7.11)	−10.0434	(29.96)	8	1,291
BENEFITS = 0	−1.7939	(6.64)	−5.5373	(20.49)	−8.4938	(31.43)	9	2,741
MOVED = 1	−1.9123	(6.45)	−3.5743	(12.05)	−6.1390	(20.70)	9	1,586
MOVED = 0	−1.8605	(6.28)	−7.9804	(26.92)	−15.4823	(52.23)	9	2,446
OVEREST = 1	−3.1880	(8.00)	−8.4407	(21.17)	−20.5573	(51.56)	5	1,139
OVEREST = UNDEREST = 0	−1.7056	(6.48)	−2.3276	(8.85)	−6.1550	(23.40)	9	2,536
UNDEREST = 0	−2.6209	(5.21)	−7.5750	(15.07)	−28.5742	(56.84)	5	357

Consistent GMM random effects dynamic estimates of Arellano and Carrasco (2003) with predetermined regressors. Absolute, choice-biased sampling, heteroskedastic-consistent, t-statistics are reported in parentheses.

5 Inertia and Learning Heterogeneity

In order to account for the dynamic nature of the learning process where individuals may invest time, cognitive effort, and other resources to gain knowledge about their new options and about their own demand for telephone services, we estimate two dynamic discrete choice panel data

models with predetermined variables. These models control for the existence of state dependence and unobserved individual heterogeneity, as both of these aspects are likely to play a relevant role. In both cases we report the consistent GMM estimator of Arellano and Carrasco (2003) and the standard ML estimator that fails to address the endogeneity of lagged dependent variables. The comparison of these two estimators is useful to understand the role of unobserved heterogeneity due to state dependence.

5.1 Testing for Inertia in Tariff Choices

The first model studies whether households tend to remain subscribed to the same tariff option over time regardless of their past realized usage levels. The study of whether household choices can be characterized by habit and inertia in a natural environment is not only of interest *per se*, but also because it is a necessary condition for rational inattention.

Table 3 reports the results of our dynamic discrete choice model. Intuitively, as time elapses the effects of accumulated experiences, cognitive efforts, and investments take over through the updating process embodied in $E\left(\eta_i \mid w_i^t\right)$ in equation (4). In this sense, these effects should become a more important determinant of tariff choices over time. Interestingly enough, when we account for these effects the results are substantially different. The estimates of the predetermined variables LOW USAGE$_{t-1}$ and MEASURED$_{t-1}$ are both negative and very significant. The negative effect of LOW USAGE$_{t-1}$ captures the effect mistakes of consumers with low usage who remain on the flat tariff option or those with high enough usage that still sign up for the optional measured tariff. Similarly, the negative effect of MEASURED$_{t-1}$ indicates that consumers do switch tariffs significantly and that, contrary to the hypothesis of habit and inertia, automatic renewal of tariff

Table 4: Attention and Inertia in Tariff Subscription (ML)

Sample:	CONSTANT		LOW USAGE$_{t-1}$		MEASURED$_{t-1}$		$-\ln \mathcal{L}$	Obs.
ALL	−1.7022	(77.82)	0.5388	(10.54)	3.2177	(43.13)	2329.368	4,032
LOW INC = 1	−1.7328	(31.75)	0.3642	(2.91)	3.2571	(17.11)	369.992	682
LOW INC = HIGH INC = 0	−1.6912	(66.50)	0.5764	(9.59)	3.2276	(36.69)	1722.898	2,934
HIGH INC = 1	−1.7331	(24.92)	0.5619	(3.58)	3.1155	(14.58)	234.266	416
DINCOME = 1	−2.0408	(30.19)	0.7973	(6.11)	3.1935	(15.58)	260.263	697
DINCOME = 0	−1.6499	(70.87)	0.5048	(9.05)	3.2107	(39.93)	2050.425	3,335
HHSIZE = 1	−1.4620	(32.84)	0.3982	(4.65)	3.2386	(20.51)	648.485	834
HHSIZE = 2	−1.6579	(44.46)	0.6111	(7.25)	3.2278	(25.10)	823.698	1,333
HHSIZE = 3	−1.8118	(35.60)	0.1405	(1.08)	3.0371	(18.32)	395.571	827
HHSIZE = 4	−1.7839	(30.27)	−0.0466	(0.30)	3.3795	(15.08)	284.013	597
HHSIZE > 4	−2.1003	(24.49)	1.0141	(3.39)	3.5299	(11.53)	132.586	443
TEENS = 1	−2.0677	(32.49)	0.6782	(3.23)	3.3546	(16.04)	242.481	766
TEENS = 0	−1.6356	(69.51)	0.4885	(9.21)	3.1926	(39.77)	2062.152	3,266
AGE = 1	−1.6210	(18.73)	0.2697	(1.46)	2.9167	(11.34)	155.355	240
AGE = 2	−1.6259	(40.43)	0.5921	(6.04)	3.0474	(23.61)	694.975	1,073
AGE = 3	−1.7432	(63.64)	0.5488	(8.63)	3.3448	(33.70)	1473.016	2,719
COLLEGE = 1	−1.4680	(33.53)	0.4433	(4.63)	3.1072	(21.59)	622.282	808
COLLEGE = 0	−1.7707	(69.62)	0.5542	(9.15)	3.2418	(37.08)	1688.301	3,224
MARRIED = 1	−1.7238	(57.30)	0.6684	(8.77)	3.1634	(31.62)	1203.917	2,138
MARRIED = 0	−1.6768	(52.61)	0.4303	(6.14)	3.2856	(29.10)	1122.760	1,894
RETIRED = 1	−1.7400	(38.21)	0.7143	(6.99)	3.3179	(19.90)	544.966	983
RETIRED = 0	−1.6904	(67.77)	0.4762	(8.04)	3.1897	(38.11)	1782.296	3,049
BLACK = 1	−1.7978	(28.21)	1.1195	(5.49)	3.1317	(14.16)	255.872	504
BLACK = 0	−1.6886	(72.43)	0.4929	(9.26)	3.2324	(40.60)	2068.828	3,528
CHURCH = 1	−1.7209	(32.81)	0.5254	(4.27)	3.1127	(17.95)	403.143	711
CHURCH = 0	−1.6982	(70.56)	0.5413	(9.63)	3.2404	(39.15)	1925.785	3,321
BENEFITS = 1	−1.7931	(43.65)	0.4840	(5.12)	3.3164	(22.33)	646.447	1,291
BENEFITS = 0	−1.6630	(64.23)	0.5632	(9.22)	3.1765	(36.76)	1677.616	2,741
MOVED = 1	−1.6377	(48.57)	0.3136	(3.94)	3.2189	(27.50)	974.101	1,586
MOVED = 0	−1.7471	(60.65)	0.6934	(10.36)	3.2209	(33.00)	1348.630	2,446
OVEREST = 1	−1.9955	(41.00)	0.4503	(4.02)	3.0646	(18.91)	400.129	1,139
OVEREST = UNDEREST = 0	−1.5673	(59.79)	0.4145	(7.44)	3.3420	(34.15)	1722.032	2,536
UNDEREST = 0	−1.8784	(23.42)	0.4421	(1.98)	2.8298	(12.32)	159.640	357

Inconsistent ML estimates. Absolute, choice-biased sampling, heteroskedastic-consistent, t-statistics are reported in parentheses.

subscription options does not necessarily mean that consumers will stay in the previously chosen tariff indefinitely.[17]

GMM estimates of Table 3 properly account for the existence of predetermined regressors. As we explained in Section 4.1, the estimator accounts for all potential paths of usage level and

[17] Impulsiveness or random behavior, *e.g.*, consumers choosing tariffs by flipping a fair coin every month, would imply a coefficient for MEASURED$_{t-1}$ equal to zero.

choice of tariffs over time. If we further aim to distinguish these decision histories for clusters of individuals with identical observable demographics, we severely increase our chances of observing empty cells. If that is the case, moment conditions (9) become uninformative for the estimation as their value will depend on the very few non-empty cells scattered across a very large number of potential histories. Thus, we decided to repeat the analysis for every group of individuals as defined by each demographic indicator available one at a time. We report the (low) number of degrees of freedom available even with our approach of selecting samples based on a single demographic dimension. Table 4 repeats the same analysis with a standard probit regression that fails to address the endogeneity of lagged endogenous regressors.

Results are robust across different demographics and quite remarkably opposite to each other depending on the method of estimation employed. According to the results of the misspecified model of Table 4, consumers with low demand tend to subscribe to the optional measured service once and for all as the choice of tariff option also appears to be correlated over time. These results would support the idea that consumers are characterized by inertia, and that low demand consumers rightly chose the measured option and tended to stay there. Switching, if it existed, appears not to be important according to this misspecified model. However, as a simple comparison with the results of Table 3 show this erroneous conclusion is only the result of ignoring the effect of unobserved heterogeneity associated to state dependence.

We thus conclude that individual heterogeneity and state dependence are crucial to interpret the choice of tariff data, and that our consistent estimates do not support the idea that consumers' responses are determined by inertia or impulsiveness.

5.2 Rational Inattention in the Choice of Tariffs

The second model addresses the learning process directly by evaluating whether or not those households that made a mistake are more likely to continue making systematic mistakes in the

Table 5: Persistence in the Wrong Choice of Tariffs (GMM)

Sample:	CONSTANT		MEASURED_{t-1}		WRONG_{t-1}		d.f.	Obs.
ALL	−1.5233	(7.02)	−7.9160	(36.49)	−1.3889	(6.40)	9	3,950
LOW INC = 1	−1.5432	(4.42)	−10.4758	(30.03)	−1.8594	(5.33)	8	668
LOW INC = HIGH INC = 0	−1.5394	(6.59)	−7.4235	(31.77)	−1.2332	(5.28)	9	2,874
HIGH INC = 1	−1.6780	(4.30)	−6.2998	(16.13)	−3.0077	(7.70)	8	408
DINCOME = 1	−1.9619	(5.82)	−4.7247	(14.02)	−3.3609	(9.98)	7	683
DINCOME = 0	−1.4890	(6.56)	−7.7598	(34.18)	−1.0294	(4.53)	9	3,267
HHSIZE = 1	−0.7568	(2.54)	−5.3754	(18.07)	−1.2829	(4.31)	9	817
HHSIZE = 2	−1.4364	(5.13)	−5.4678	(19.51)	−0.9912	(3.54)	9	1,303
HHSIZE = 3	−2.0489	(5.98)	−7.3731	(21.53)	−1.8405	(5.37)	6	811
HHSIZE = 4	−2.0654	(5.43)	−13.2991	(34.96)	−2.1146	(5.56)	6	585
HHSIZE > 4	−2.8353	(5.92)	−20.5004	(42.84)	−12.1551	(25.40)	4	434
TEENS = 1	−2.5513	(6.42)	4.0823	(10.27)	−15.0762	(37.92)	5	750
TEENS = 0	−1.3811	(6.17)	−7.1850	(32.12)	−0.8616	(3.85)	9	3,200
AGE = 1	−1.3851	(3.33)	−1.4152	(3.40)	−1.3488	(3.24)	6	235
AGE = 2	−1.5545	(5.00)	−6.3919	(20.58)	−2.0171	(6.49)	8	1,051
AGE = 3	−1.5052	(6.30)	−9.1007	(38.08)	−1.8012	(7.54)	9	2,664
COLLEGE = 1	−0.7895	(2.27)	−5.2913	(15.18)	−5.9640	(17.11)	8	792
COLLEGE = 0	−1.6363	(7.10)	−9.2367	(40.09)	−1.0372	(4.50)	9	3,158
MARRIED = 1	−1.7349	(6.51)	−7.5556	(28.34)	−1.7565	(6.59)	9	2,095
MARRIED = 0	−1.3233	(5.30)	−7.4267	(29.72)	−1.3819	(5.53)	9	1,855
RETIRED = 1	−1.5378	(5.05)	−8.9728	(29.48)	−1.6826	(5.53)	8	963
RETIRED = 0	−1.5171	(6.48)	−7.3404	(31.37)	−1.5495	(6.62)	9	2,987
BLACK = 1	−2.3144	(5.70)	−7.1978	(17.73)	−1.7701	(4.36)	6	494
BLACK = 0	−1.4402	(6.48)	−7.7858	(35.04)	−1.4408	(6.48)	9	3,456
CHURCH = 1	−1.7183	(5.03)	−6.5395	(19.15)	−0.9614	(2.82)	8	697
CHURCH = 0	−1.4916	(6.57)	−7.8236	(34.47)	−1.7712	(7.80)	9	3,253
BENEFITS = 1	−1.6166	(5.58)	−11.3664	(39.27)	−1.3053	(4.51)	8	1,265
BENEFITS = 0	−1.4863	(6.23)	−6.7109	(28.12)	−1.4499	(6.07)	9	2,685
MOVED = 1	−1.4874	(5.58)	−6.7672	(25.41)	−0.5919	(2.22)	9	1,554
MOVED = 0	−1.5394	(6.12)	−8.6180	(34.27)	−2.2472	(8.94)	9	2,396
OVEREST = 1	−3.0922	(8.31)	−23.0542	(61.95)	4.9509	(13.30)	5	1,116
OVEREST = UNDEREST = 0	−1.1158	(4.86)	−5.5119	(24.01)	−0.4217	(1.84)	9	2,484
UNDEREST = 0	−2.4090	(4.81)	−25.6046	(51.07)	−4.2901	(8.56)	5	350

Consistent GMM random effects dynamic estimates of Arellano and Carrasco (2003) with predetermined regressors. Absolute, choice-biased sampling, heteroskedastic-consistent, t-statistics are reported in parentheses.

future. In Table 5 we study the extent to which *ex-post* mistakes are systematic. The endogenous variable equals one whenever household i chooses the wrong tariff option *ex-post*, that is, either the measured tariff and a relatively high usage level or the flat tariff and a relatively low usage level. The predetermined variables in this case are whether households made a wrong tariff choice in the previous period and whether they subscribed to the measured tariff option. Table 6 reports

Table 6: Persistence in the Wrong Choice of Tariffs (ML)

Sample:	CONSTANT		MEASURED_{t-1}		WRONG_{t-1}		$-\ln \mathcal{L}$	Obs.
ALL	−1.3560	(77.89)	0.8354	(15.90)	1.3827	(34.11)	4100.418	3,950
LOW INC = 1	−1.3614	(32.29)	0.7466	(5.30)	1.4310	(14.83)	694.868	668
LOW INC = HIGH INC = 0	−1.3563	(66.20)	0.8411	(14.12)	1.3514	(28.41)	2981.507	2,874
HIGH INC = 1	−1.3454	(25.28)	0.9418	(5.21)	1.5206	(11.69)	421.787	408
DINCOME = 1	−1.3812	(32.85)	0.8612	(5.74)	1.1121	(11.23)	682.776	683
DINCOME = 0	−1.3495	(70.62)	0.8126	(14.30)	1.4375	(32.20)	3410.681	3,267
HHSIZE = 1	−1.0573	(29.43)	0.4383	(5.27)	1.2120	(18.01)	1166.283	817
HHSIZE = 2	−1.2785	(43.34)	0.9422	(11.49)	1.1375	(16.98)	1477.969	1,303
HHSIZE = 3	−1.4939	(37.19)	0.7898	(4.49)	1.6838	(14.49)	682.011	811
HHSIZE = 4	−1.5722	(31.53)	1.2116	(6.67)	1.6317	(11.96)	446.790	585
HHSIZE > 4	−1.7703	(27.23)	1.0586	(2.92)	1.6733	(6.69)	239.488	434
TEENS = 1	−1.7098	(35.80)	0.3091	(1.21)	2.2813	(13.35)	452.514	750
TEENS = 0	−1.2896	(68.05)	0.8287	(15.56)	1.2905	(30.65)	3603.162	3,200
AGE = 1	−1.1530	(17.50)	0.5292	(2.73)	1.4017	(9.02)	293.859	235
AGE = 2	−1.3810	(40.53)	0.8353	(8.04)	1.5116	(18.35)	1049.965	1,051
AGE = 3	−1.3657	(64.14)	0.8578	(13.30)	1.3338	(27.24)	2748.582	2,664
COLLEGE = 1	−1.2466	(32.83)	0.6957	(6.95)	1.6055	(19.87)	924.480	792
COLLEGE = 0	−1.3828	(70.51)	0.8751	(14.10)	1.2943	(27.42)	3158.056	3,158
MARRIED = 1	−1.4388	(58.24)	1.0518	(13.76)	1.3041	(20.89)	1956.573	2,095
MARRIED = 0	−1.2715	(51.37)	0.6457	(8.93)	1.4106	(26.20)	2125.535	1,855
RETIRED = 1	−1.3772	(38.69)	0.9576	(9.58)	1.1225	(13.68)	990.614	963
RETIRED = 0	−1.3495	(67.57)	0.7849	(12.70)	1.4689	(31.31)	3100.573	2,987
BLACK = 1	−1.5838	(29.24)	0.9984	(4.57)	1.4243	(7.95)	368.718	494
BLACK = 0	−1.3274	(71.92)	0.8187	(15.12)	1.3666	(32.70)	3720.910	3,456
CHURCH = 1	−1.3834	(32.96)	0.9122	(7.25)	1.2699	(12.88)	700.132	697
CHURCH = 0	−1.3501	(70.56)	0.8196	(14.17)	1.4048	(31.58)	3398.716	3,253
BENEFITS = 1	−1.3851	(44.59)	1.0138	(10.57)	1.1353	(15.68)	1275.014	1,265
BENEFITS = 0	−1.3418	(63.83)	0.7387	(11.65)	1.5017	(30.40)	2812.217	2,685
MOVED = 1	−1.3168	(48.13)	0.7074	(8.30)	1.5454	(24.80)	1675.876	1,554
MOVED = 0	−1.3823	(61.16)	0.9286	(13.91)	1.2543	(23.43)	2412.525	2,396
OVEREST = 1	−1.9257	(42.41)	1.7689	(8.15)	0.9299	(4.15)	471.857	1,116
OVEREST = UNDEREST = 0	−1.1442	(55.42)	0.7105	(13.10)	1.2399	(29.08)	3237.562	2,484
UNDEREST = 0	−1.7267	(24.77)	0.9792	(3.23)	1.4056	(5.51)	216.562	350

Inconsistent ML estimates. Absolute, choice-biased sampling, heteroskedastic-consistent, t-statistics are reported in parentheses.

the results of the corresponding misspecified model that fails to address the endogeneity of lagged endogenous regressors.

The positive sign of MEASURED_{t-1} in Table 6 would be consistent, for instance, with a model where a household systematically thinks that it is going to consume below the threshold level but will systematically consume above it. A naïve hyperbolic discounter who subscribed to the optional

measured service as a commitment device to limit her time on the phone would exhibit this type of systematic mistake, *e.g.*, see Strotz (1956). However, once we control appropriately for the effects of individual heterogeneity associated to the accumulation of experience, investments, and information in Table 5, the results turn out to be drastically different. The sign of MEASURED_{t-1} becomes negative across all demographic strata. This result establishes that the switching of tariffs documented in Table 3 is not symmetric: consumers previously subscribed to the measured option are more likely to switch options than those subscribed to the optional flat tariff. This asymmetric behavior can easily be explained by different cognitive and deliberation costs across tariff choices. It is much easier for households that subscribe to the measured option to monitor whether they have made the wrong decision: they simply have to compare their actual bill with the $18.70 flat rate. On the contrary, households in the flat tariff would have to actively monitor their phone calls very carefully and make more complex calculations in order to ascertain whether or not they are paying too much for their local telephone service. Monitoring and cognitive costs are clearly much greater for them. The asymmetric switching behavior that we observe is thus perfectly consistent with these asymmetric differences in complexity and cognitive costs. This result supports the implication that households that face the less complex problem learn faster and incur in fewer mistakes.

The negative sign of WRONG_{t-1} in Table 5 indicates that mistakes are not permanent and that the switching between tariff options is aimed at reducing the cost of local telephone service. This finding is important, and is in sharp contrast with the positive sign of this variable in Table 6, which would incorrectly indicate that households made systematic mistakes. These mistakes, which would be characteristic of households driven by rational inattention, are not supported by our random effects dynamic model.

Table 7: Marginal Effects

Previous Transition	October	November	December	Fall
From (Flat,Right) to (Flat,Wrong)	−11.60	−6.52	−4.27	−7.46
From (Measured,Right) to (Measured,Wrong)	−0.01	−1.67	−2.13	−1.27
From (Flat,Right) to (Measured,Right)	−17.73	−17.82	−11.64	−15.73
From (Flat,Wrong) to (Measured,Wrong)	−6.13	−12.98	−9.49	−9.53

Percent change in the probability of choosing the current tariff option wrongly conditional on each transition among states.

5.3 Errare Humanum Est, In Errore Perservare Stultum

Before concluding, we pursue further the result that mistakes are a transitory phenomenon, and compute the marginal effects associated with the transition among different states.[18] Arellano and Carrasco (2003) show that the probability of subscribing to the wrong tariff plan when we compare two states $z_{it} = z^0$ and $z_{it} = z^1$ changes by the proportion:

$$\hat{\triangle}_t = \frac{1}{N}\sum_{i=1}^{N}\left\{\Phi\left(\hat{\sigma}_t^{-1}\hat{\beta}\left(z^1 - z_{it}\right) + \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right]\right) - \Phi\left(\hat{\sigma}_t^{-1}\hat{\beta}\left(z^0 - z_{it}\right) + \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right]\right)\right\}. \quad (10)$$

Since the evaluation depends on the history of past choices ω_i^t, these marginal effects are different for each month of the sample. Table 7 presents four marginal effects evaluated in October, November, December, as well as the average effect over the Fall.[19] The first two rows show the change in probability of choosing wrongly if consumers chose wrongly in the previous month. The first row indicates that this probability *decreases* on average by 7.46% if consumers subscribed to the flat tariff option while the second row shows that this probability *decreases* by 1.27% had they subscribed to the measured tariff option. Thus, regardless of the choice of tariff, it is less likely that they make another mistake in their choice of tariffs.

[18] The title of this section reads *"It is human to make a mistake, it is stupid to persist on it,"* L. A. Seneca, 4 BC – 65 AC.

[19] These four transitions exhaust the relevant effects to be reported. To compute the marginal effects of going in the opposite direction, just reverse the sign of the corresponding effect in Table 7.

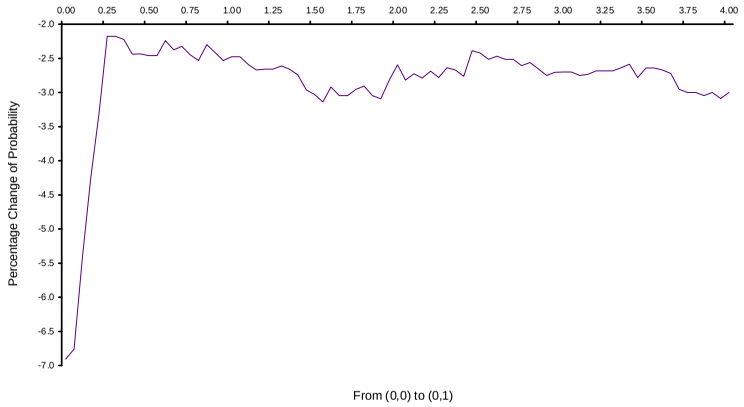
Similarly, the last two rows report the change in probability of choosing wrongly if consumers subscribed to the optional measured service in the previous month. This probability *falls* by 15.73% if consumers subscribed correctly to the optional measured service in the previous month and by 9.53% if they subscribed wrongly to the optional measured service. Thus, consistent with the asymmetry in the complexity of the problems discussed earlier, the probability of making a mistake is substantially lower after subscribing to the measured option than after subscribing to the flat tariff. This probability reduction is more important for those with low demand for which the measured service is the least expensive option than for those with an usage pattern above the threshold of $18.70.

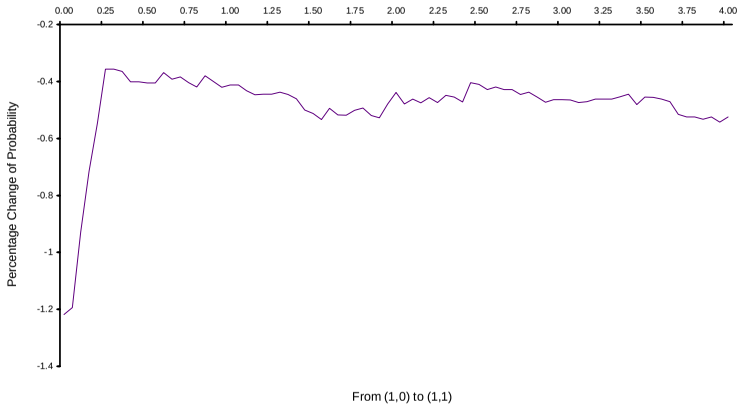
In analyzing these marginal effects, WRONG equals 1 when consumers pay any positive amount above the cost of the alternative option. We repeat the analysis for different thresholds in increments of 5 cents from $0.00 to $4.00 in order to measure whether this change in the probability varies significantly with the magnitude of the mistake. Figure 1 reports the average marginal effects for the Fall. Interestingly, marginal effects experience an abrupt jump in the first 25-30 cents and remain mostly constant once consumers realize a mistake above these 25-30 cents. Recall that under the measured service option consumers are not billed for the $5 allowance unless their usage is above $19.02. This is 32 cents more than the $18.70 cost of the flat tariff option. We find it remarkable that this amount is almost identical to 25-30 cents.

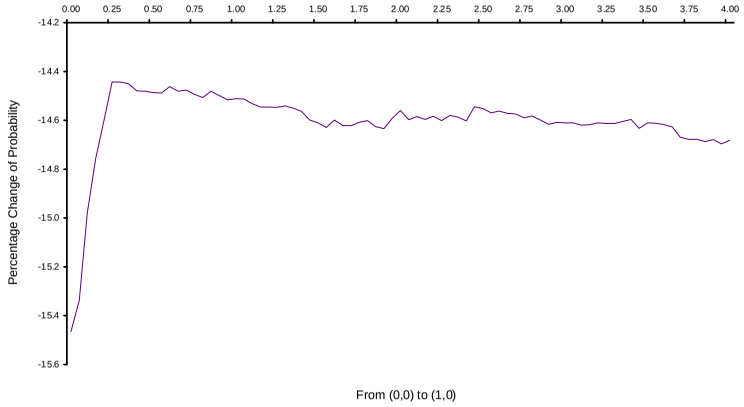
6 Concluding Remarks

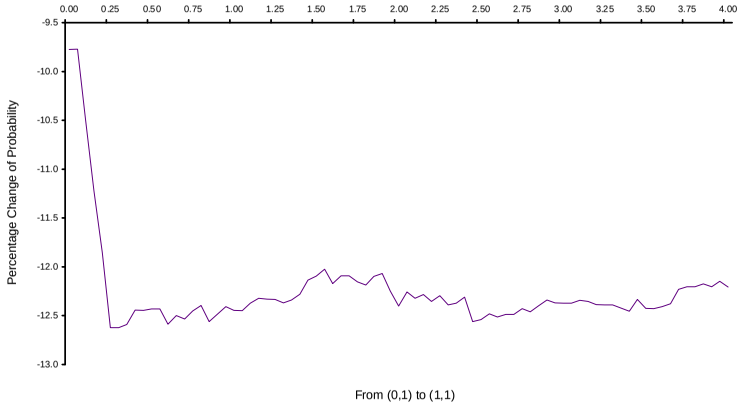
The systematic analysis of individual responses to changes in the environment is important for understanding the determinants of attention and inattention, and the extent and formation of rationality. The natural setting of the Kentucky tariff experiment and a rich panel dataset that is free from a number of critical obstacles that may explain the lack of empirical studies in the

Figure 1: Marginal Effects at Different Mistake Thresholds









literature have allowed us to uncover households' responses in isolation from a number of conflicting considerations which generally exist in other circumstances.

We find that households recognize that choices today affect their utilities in the future and actively react to a new option despite potential savings of very small magnitude. They make no systematic mistakes. Their reactions, however, are not symmetric. Households who face a more complex and cognitively more expensive tariff problem learn more slowly and are more likely to make mistakes than households that face a simpler tariff choice problem. The fact that the evidence turns out to be drastically different when lagged endogenous variables and unobserved heterogeneity are appropriately treated indicates that they play an important role in the dynamic learning process.

When and why people are attentive or inattentive and, when they are attentive, when and why people get it right or wrong, are fundamental questions for our understanding of human decision making.

We cannot claim, and we do not claim, that we should expect that the results we have obtained will systematically generalize to other settings. This is an empirical question whose answer depends on the degree of complexity, the costs of information, the size of incentives, and all other characteristics of the specific problem and environment under study. What we hope, however, is that the analysis in the current paper will begin to pave the way for an empirically based science of decision making which together with theoretical and experimental work on cognitive processes will significantly improve our understanding of *when* and *how* decision makers think about real life problems.

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