Rational Attention in a Repeated Decision Problem*

Eugenio J. Miravete†

Ignacio Palacios-Huerta‡

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Abstract

Deliberation about an economic activity is costly. When information and deliberation costs are high relative to expected benefits, inertia and inattention may be rational. While the deliberation cost theme dominates the discussion in the literature that postulates deviations from unbounded rationality, no empirical evidence on the size of these costs is yet available. We address this question empirically using subscription data to local telephone service options where inertia is a pervasive feature. We show that appropriately accounting for individual unobserved heterogeneity due to past decisions is critical to account for the effect of consumer learning. We find that households learn rapidly to undertake optimal decisions, make no systematic mistakes, and react to potential savings of very small magnitude, typically about $5.00 per month.

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

JEL Codes: D42, D82, L96.

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†The University of Texas at Austin, Department of Economics, BRB 1.116, 1 University Station C3100, Austin, Texas 78712-0301; and CEPR, London, UK. Phone: 512-232-1718. Fax: 512-471-3510. E–mail: *miravete@eco.utexas.edu*; http://www.eco.utexas.edu/facstaff/Miravete

‡Department of Management G507, London School of Economics, Houghton Street, London WC2A 2AE, United Kingdom. Phone: +44 (0) 207-955-5167. E–mail: *i.palacios-huerta@lse.ac.uk*; http://www.econ.brown.edu/ipalacios

"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

There is no question that deliberation about an economic decision is a costly activity. Many economists, psychologists and other social scientists have expressed the idea that decision makers try to achieve a balance between the benefits of better decision making and the effort cost of decision. According to Stigler and Becker (1977), for instance,

> "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."

If finding the best action is costly, then the best way to decide on an action involves trading off decision-making costs with the benefits to improve the choice of the action. The purpose of this paper is to address this trade-off empirically and to infer the size of deliberation and estimation costs that may lead us to expect, or to separate, rational from irrational behavior within the context of a particular application. We study a specific individual decision making problem in a natural setting particularly well suited to address the effect of inertia and default choices that has recently attracted attention in the behavioral literature.[1] Thus, in this paper we turn the arguments toward the more constructive questions: When is bounded rationality likely to be important in a real environment? How large should the expected benefits from thinking and deliberating about a problem be if rational attention is to be expected from individuals? In other words, if deliberation and active cognition costs represent critical physiological limits on human cognition, when should we expect these limits to be important?

In our setting we have an individual decision making situation where strategy sets can be readily determined, individuals' choices can be precisely documented, incentives can be exactly computed, and where a rich panel data set on choices, rewards, and demographic characteristics is available to study how strong incentive effects need to be in order to induce learning and/or to overcome inertia and the (potentially adverse) effect of default choices. Customers of fixed telephony in Louisville, Kentucky, had

[1] See for instance Choi, Laibson, Madrian, and Metrick (2004), Cronqvist and Thaler (2004) or Madrian and Shea (2001).

the possibility of choosing between two alternative billing methods during the second half of 1986. They could either keep paying a fixed monthly fee of $18.70 per month with unlimited local calling, or choose an optional measured telephone service consisting of a $14.02 monthly fee, a $5.00 allowance, and a payment per call that depends on its duration, distance, time of the day, and day of the week. The basic problem that households face each month was to determine whether their expected demand for local phone service next month would exceed $19.02, as they would not be billed for the $5.00 allowance unless their usage level exceeds this limit. In addition, potential loses due to mistaken choices are limited as the maximum payment under the optional measured service is capped at $21.50 per month.[2]

Controlling for the effect of state dependence is the main econometric contribution of this paper. This has important economic consequences. Individuals are placed by default on the flat tariff option at the beginning of the tariff experiment. Then they choose different combinations of usage levels and tariff options as time goes by, allowing them to learn and become different from each other beyond observable household characteristics. As we observe the history of individual past decisions only partially, we need to use an econometric estimator that accounts for the effect of individual unobserved heterogeneity due to state dependence, *i.e.*, the accumulated level of experience acquired through past usage and tariff choices that cannot be accounted for by observable demographics. Our econometric analysis shows most strikingly, that failure to properly account for the effect of state dependence may lead to the wrong acceptance of inertia as the main explanation behind the choice of telephone usage level and tariff options.

Despite all the desirable features of the field experiment, the empirical analysis is far from trivial or straightforward. The reason is that we need to estimate a binary choice panel data model with predetermined variables and unobserved heterogeneity. There are several reasons that make the estimation of these models a difficult task. Thus for instance, 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, *e.g.*, Arellano and Honoré (2001). In linear models with additive effects the standard response is to consider instrumental-variables estimates that exploit the lack of correlation between lagged values of the variables and future errors in first differences. In non-linear models, however, very few results are available. For fixed effects the few methods available are case-specific, *e.g.*, the conditional maximum likelihood estimator of Chamberlain (1980) for the logit and Poisson regression. In the case of random effects, the main difficulty is the so-called "initial conditions

[2] 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 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 switcthing better explains the behavior of households.

problem," *i.e.*, if, as in our case, sampling begins after the process in question is already in progress, we need to isolate the effect of the first lagged dependent variable from the individual-specific effect and the distribution of the explanatory variables prior to the sample.

To illustrate the importance of state dependence in panel data modeling within the framework of our application consider that individual i at time t chooses the measured option:

$$y_{it} = \mathbf{1}\left\{\gamma y_{it-1} + \alpha_i + \varepsilon_{it} \geq 0\right\} . \tag{1}$$

In this model there are three sources of persistence that could be attributed to the effect of inertia: serial correlation in the error term, ε, unobserved heterogeneity, α_i, and true state dependence, γy_{it-1}. Even if the error terms are serially independent, the regularity conditions for conditional maximum likelihood estimation of a fixed effects logit model are not satisfied in the presence of a lagged dependent variable as ε_t is not independent of explanatory variables at $t-1$. The distinction between the sources of persistence, however, is very important for determining the underlying model of behavior.

In order to control for the effect of state dependence appropriately in our setting, we estimate the semiparametric, dynamic random effects, discrete choice, panel data model of Arellano and Carrasco (2003). These authors develop a *consistent* random effects estimator where: *(a)* explanatory variables are predetermined but not strictly exogenous, and where *(b)* individual effects are allowed to be correlated with explanatory variables. It contains a non-parametric conditional expectation of the effects given the predetermined variables, but it is otherwise parametric. This makes the estimation of the model affordable while not restricting the estimates of the effects by imposing an arbitrary distribution of the conditional expectation.

Results can be summarized briefly as follows. Accounting for individual experience gained through consecutive choices of usage and tariff options is critical to properly identify whether consumers engage actively in revising their choices or rather inertia dictates their future behavior. We find that while consumers facing the new consumption option may make mistakes initially, they actively engage in tariff switching in order to reduce the monthly cost of local telephone services. Thus, those mistakes are not systematic. As indicated earlier, incentives and motivation depend on the size of the costs of cognition and deliberation effort relative to expected gains. In our case, these incentives are quite low as the magnitude of the differences between the alternative tariff schemes is very small, typically about \$5.00-\$6.00 per month. Yet, the observed responses reacting to these potential savings are quite conclusive. No rational inattention is observed. Despite the small magnitude of the monetary differences across choices, we find no support for alternative models where consumers' responses are determined by habit, inertia, or impulsiveness. Results are robust across consumers with diverse demographic profiles.

The rest of the paper is organized as follows. Section 2 describes the features of the natural experiment, the data, and reports various descriptive statistics to frame the discussion of a simple theoretical model in Section 3. Section 4 discusses how to address the existence of unobserved heterogeneity due to state dependence and Section 5 presents the corresponding estimates. Section 6 concludes.

2 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,[3] and distinguished among setup, duration, peak periods, and distance.[4] 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.[5] 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.

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.[6] 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

[3] 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.

[4] 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.

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

[6] This is not the case of Della Vigna and Malmendier (2006), where health considerations or social networks within the workplace may induce a more or less intense use of the gym.

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.[7]

Some of the features of the tariff experiment are remarkable. The introduction of tariff options is recent and consumers have no previous experience with tariff plans that charge them according to their telephone usage. Learning, if any, is unlikely to have reached its full potential in just six months. And more importantly, monthly bills under the different tariff plans are very similar so that potential savings from switching are rather limited, *i.e.*, about $5.00-$6.00 per month. Thus, if under these circumstances we find that, conditional on their observable accumulated experience, consumers switch tariff plans to reduce their telephone bill we have to conclude that deliberation costs are quite low. This is indeed our main finding. Furthermore, we show that past experience is important and that it serves to correct previous mistakes rather to prolong them as a consequence of inertia that impedes individuals to revise their past choices.

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 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. Only active consumers were considered and a small number of observations with missing values for some variables were excluded.[8] These descriptive statistics initially suggest that individual heterogeneity in consumption and tariff subscription is important. Table 1 breaks down the sample into two groups according to their choice

[7] 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).

[8] The number of observations excluded is very small. Some households did not report their 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.

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 number of 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 number of 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	Potential savings 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	Head of household is between 15 and 34 years old	0.0632	(0.24)	0.0614	(0.24)	0.0676	(0.25)
AGE = 2	Head of household is between 35 and 54 years old	0.2686	(0.44)	0.2604	(0.44)	0.2880	(0.45)
AGE = 3	Head of household is above 54 years old	0.6682	(0.47)	0.6782	(0.47)	0.6444	(0.48)
COLLEGE	Head of household is at least a college graduate	0.2240	(0.42)	0.1821	(0.39)	0.3230	(0.47)
MARRIED	Head of household is married	0.5253	(0.50)	0.5342	(0.50)	0.5042	(0.50)
RETIRED	Head of household is retired	0.2433	(0.43)	0.2417	(0.43)	0.2471	(0.43)
BLACK	Head of household is black	0.1161	(0.32)	0.1295	(0.34)	0.0843	(0.28)
CHURCH	Telephone is used for charity and church purposes	0.1711	(0.38)	0.1785	(0.38)	0.1536	(0.36)
BENEFITS	Household receives some federal or state benefits	0.3095	(0.46)	0.3282	(0.47)	0.2654	(0.44)
MOVED	Head of household 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.

of tariff in October. In general Table 1 shows that consumers with very different demographics subscribe to FLAT and MEASURED tariffs. They do not only differ only in their level of local telephone usage —as captured by CALLS— but also in their expectations regarding future telephone usage and demographics (although to a lesser extent). Households subscribing to the optional FLAT service are on average larger, with teenagers, and with low level of education.

Despite all the remarkable features of the data, there are two concerns that need to be addressed econometrically. First, about 10% of consumers switched to the optional measured option when given that possibility, but our sample includes 30% of those customers. Choice-based sampling bias can easily be dealt with using well established methods surveyed by Amemiya (1985, §9.5). All estimates reported in this paper 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.

3 Theoretical Framework

This section describes the relevant theoretical background. A decision maker (DM) must choose an action a from a menu $\mathcal{A}$. He has a prior probability density $q(\theta)$ on state $\theta \in \Theta$ with full support. Action a yields von Neumann-Morgenstern utility $u(a, \theta)$ in state θ where $u : \mathcal{A} \times \Theta \to \boldsymbol{R}$. Without loss of generality, we consider a the binary-state, binary-action framework that corresponds to our empirical setting. There are two states $\Theta = \{\text{L}, \text{H}\}$ and two actions: $\mathcal{A} = \{\text{F}, \text{M}\}$. The two states may be interpreted as low (L) and high (H) demand respectively, and the two actions as the choice between flat tariff (F) and the measured tariff (M) options. Each tariff plan is the least expensive option for some usage level, *i.e.*, none of them is a dominated action. Thus, we assume:

$$
\begin{aligned}
u(\text{M}, \text{L}) &> u(\text{F}, \text{L}) \ , \\
u(\text{F}, \text{H}) &> u(\text{M}, \text{H}) \ ,
\end{aligned}
\tag{2}
$$

that is, tariff M is preferred if demand is low and tariff F if demand is high.

Before choosing an action, the DM observes the outcome of an n-sample $x^n = (x_1, ..., x_n) \in \boldsymbol{X}^n$ of experiments. Sampling may be the result, for instance, of searching, of explicit information purchases, or of thinking efforts. After observing x^n, the DM updates his prior beliefs and takes the action that maximizes his expected utility given the sample. Denote belief $q = Prob(\text{H})$. The DM optimally chooses action F if and only if $q \geq q^\star$ for some $q^\star \in (0, 1)$. Action M is selected if beliefs after observing x^n are:

$$
q_n = Prob\,(\text{H} \mid x^n) < q^\star = \frac{u(\text{M}, \text{L}) - u(\text{F}, \text{L})}{u(\text{M}, \text{L}) - u(\text{F}, \text{L}) + u(\text{F}, \text{H}) - u(\text{M}, \text{H})} \, . \tag{3}
$$

The expected payoffs in states L and H are:

$$
\text{L} : \; Prob\,(q_n < q^\star \mid \text{L})\, u(\text{M}, \text{L}) + Prob\,(q_n \geq q^\star \mid \text{L})\, u(\text{F}, \text{L}) \, , \tag{4a}
$$

$$
\text{H} : \; Prob\,(q_n \geq q^\star \mid \text{H})\, u(\text{F}, \text{H}) + Prob\,(q_n < q^\star \mid \text{L})\, u(\text{M}, \text{H}) \, . \tag{4b}
$$

Thus, the *ex-ante* payoff from sampling n observations can be written as:

$$V_{q,u}(n) = (1-q)\left[(1-\alpha_n)\, u\,(\textsc{m},\textsc{l}) + \alpha_n u(\textsc{f},\textsc{l})\right] + q\left[(1-\beta_n)\, u\,(\textsc{f},\textsc{h}) + \beta_n u(\textsc{m},\textsc{h})\right] , \tag{5}$$

where α_n and β_n denote error probabilities:

$$\alpha_n = \; Prob\,(q_n \geq q^\star \mid \textsc{l}) \;, \tag{6a}$$

$$\beta_n = \; Prob\,(q_n < q^\star \mid \textsc{h}) \;. \tag{6b}$$

We next introduce the costs of thinking and gathering information, which in our framework reduces to the (observable) sequence of past actions taken by consumers. We assume that the sampling of n demand realizations and choices of the past individual history leads to a flow cost $c(n) \geq 0$. In general, there are two classes of costs: *i)* the costs of buying and searching for information, and *ii)* the cognitive costs of processing this information and thinking about the problem. The former can be addressed by means of accounting for the past decisions of consumers while the latter falls among part of the individual unobserved heterogneity. Furthermore, in our setting, households are freely provided each month with a description of their consumption of telephone services. In this sense, the first class of costs are low, perhaps even zero for those who subscribed to the optional measured service as they immediately realize whether their bill exceeds the cost of the flat measured option. Behavior would then seem to be mostly, perhaps even exclusively, determined by cognitive and deliberation costs. Assume that the DM chooses n to maximize:

$$V_{q,u}(n) - c(n) \cdot n \,, \tag{7}$$

so that consumers will continue sampling and gathering information as long as the value of information exceeds its (low) cost of gathering it. How low can payoffs be so that deliberation and cognition activities are still cheap relative to payoffs in a given problem? Bayesian rational learning is to be expected in a context with active costs of cognition if these costs are small relative to the payoff stakes. We do not observe costs explicitly, but we observe the payoffs of the actions *and* the behavior of DMs in detail. Thus, from payoffs and the observed behavior of consumers we infer an upper bound on the size of the cognitive costs that may be involved if as predicted by this model, individuals behave in a way consistent with rational learning.

The model described above suggests that consumers with high demand will most likely choose the flat tariff option once they gather enough information. In an attempt to examine whether *ex-post* households tend to choose the 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.[9] We estimate the following reduced form model:

$$y_j^* = X\Pi_j + v_j\,, \qquad j = 1, 2, \tag{8}$$

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

$$(v_1, v_2) \sim N\left(\mathbf{0}, \boldsymbol{\Sigma}_v\right)\ ; \qquad \boldsymbol{\Sigma}_v = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}\ . \tag{9}$$

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 $19.02 when metered according to the measured tariff rates, so that a positive estimate of ρ can be interpreted as unobservable element inducing the appropriate tariff choice for each usage level. The model includes the same set of demographic variables in both equations to control for the effect of observable individual heterogeneity over the tariff choice and consumption decisions. Data also include household specific information from the Spring months to control, at least in part, for the accuracy of predictions of individual future usage. We thus include two dummies to indicate whether consumers significantly over or underestimated future consumption when marginal consumption was not priced at all.[10] Similarly, we construct an indicator of usage intensity for each household during the Spring months, LOW USAGE$_{Spring}$, which equals one when the usage 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 is perhaps the main result that we would like to emphasize. It indicates 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.

[9] 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.

[10] 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.

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.[11]

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

[11] 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.

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: Are the consumption levels, tariff choices, and the switching that we observe in the data sufficient to provide any significant evidence that consumers respond to potential savings? What is the role of previous tariff choices and demand realizations on the decision to subscribe to one of the two options? Do consumers simply stay on their previously chosen tariff because of inertia or rational inattention? 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 recent 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 leads to dramatically opposite results regarding the effect of experience on telephone usage and tariff choice, including switching plans to take advantage of potential savings.

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 and made different investments, which also affect 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:

$$y_{it} = \mathbf{1}\left\{\beta z_{it} + E\left(\eta_i \mid w_i^t\right) + \varepsilon_{it} \geq 0\right\}, \qquad \varepsilon_{it} \mid w_i^t \sim N\left(0, \sigma_t^2\right), \tag{10}$$

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.[12]

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 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:

$$Prob\left(y_{it} = 1 \mid w_i^t\right) = \Phi\left[\frac{\beta z_{it} + E\left(\eta_i \mid w_i^t\right)}{\sigma_t}\right]. \tag{11}$$

Since all our regressors are dichotomous variables, their support is a lattice defined by $2J$ nodes $\left\{\phi_1, ..., \phi_{2J}\right\}$. 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

[12] 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 did include 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.

representing the sequence of tariff choices and demand realizations for each individuals in the sample. Thus, the conditional probability can be rewritten as:

$$p_{jt} = Prob\left(y_{it} = 1 \mid w_i^t = \phi_j^t\right) \equiv h_t\left(w_i^t = \phi_j^t\right), \qquad j = 1, \ldots, (2J)^t . \tag{12}$$

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:

$$\sigma_t \Phi^{-1}\left[h_t\left(w_i^t\right)\right] - \sigma_{t-1}\Phi^{-1}\left[h_{t-1}\left(w_i^{t-1}\right)\right] - \beta\left(x_{it} - x_{i(t-1)}\right) = \xi_{it}, \tag{13}$$

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

$$E\left[\xi_{it} \mid w_i^{t-1}\right] = E\left[E\left(\eta_i \mid w_i^t\right) - E\left(\eta_i \mid w_i^{t-1}\right) \Big| w_i^{t-1}\right] = 0. \tag{14}$$

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:

$$\hat{h}_t\left(w_i^t\right) = \sum_{j=1}^{(2J)^t} \mathbf{1}\left\{w_i^t = \phi_j^t\right\} \cdot \hat{p}_{tj}, \tag{15}$$

which is used to define the sample orthogonality conditions of the GMM estimator:[13]

$$\frac{1}{N}\sum_{i=1}^{N} d_{it}\left\{\sigma_t \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right] - \sigma_{t-1}\Phi^{-1}\left[\hat{h}_{t-1}\left(w_i^{t-1}\right)\right] - \beta\left(x_{it} - x_{i(t-1)}\right)\right\} = 0, \qquad t = 2, \ldots, T, \tag{16}$$

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

[13] 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.

4.2 Alternative Approaches

Consumers may engage 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 consumers may differ by having taken a different sequence of decisions that we do not completely observe. It is important to realize 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 thus, they all share the same initial condition.

Assuming that in just three months consumers would have reached a stationary equilibrium is, in our opinion, unrealistic. If that was the case, the initial condition problem could be ignored. But since this is not the case, failing to address it will lead 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.[14]

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 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 we consider questionable.

[14] 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).

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.

5 Inertia and Learning Heterogeneity

In order to account for the dynamic nature of the learning process where individuals may invest time, deliberation 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 to point out the stark differences and opposite conclusions that we can obtain if we fail to address the 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

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	3,950
LOW INC = 1	−2.3919	(6.22)	1.1055	(2.87)	−20.0065	(52.02)	8	668
LOW INC = HIGH INC = 0	−1.9692	(7.35)	−5.5032	(20.54)	−6.0887	(22.73)	9	2,874
HIGH INC = 1	−2.1159	(5.00)	−6.2151	(14.68)	−12.4203	(29.34)	8	408
DINCOME = 1	−3.1042	(7.09)	−10.1293	(23.14)	−8.2131	(18.76)	7	683
DINCOME = 0	−1.8781	(7.46)	−3.5418	(14.06)	−8.1274	(32.26)	9	3,267
HHSIZE = 1	−1.2827	(3.64)	−3.2181	(9.13)	−4.3519	(12.35)	9	817
HHSIZE = 2	−1.6469	(5.16)	−6.5772	(20.60)	−11.5899	(36.29)	9	1,303
HHSIZE = 3	−2.6187	(6.82)	−5.4355	(14.16)	−6.3259	(16.48)	6	811
HHSIZE = 4	−2.3548	(5.86)	−11.4859	(28.57)	−16.0243	(39.86)	6	585
HHSIZE > 4	−3.4691	(6.82)	−13.4427	(26.44)	−31.7962	(62.54)	4	434
TEENS = 1	−3.1895	(7.63)	−25.6940	(61.46)	−25.8714	(61.89)	5	750
TEENS = 0	−1.8713	(7.41)	−2.9598	(11.72)	−7.3084	(28.93)	9	3,200
AGE = 1	−1.9711	(4.18)	−4.7308	(10.04)	−7.9214	(16.81)	6	235
AGE = 2	−1.9399	(5.79)	−4.1165	(12.28)	−5.6042	(16.71)	8	1,051
AGE = 3	−2.0563	(7.48)	−4.6915	(17.07)	−9.9864	(36.34)	9	2,664
COLLEGE = 1	−1.1912	(3.35)	−5.7461	(16.15)	−5.4816	(15.40)	8	792
COLLEGE = 0	−2.2028	(8.25)	−4.2893	(16.07)	−9.9372	(37.23)	9	3,158
MARRIED = 1	−1.6761	(5.42)	−11.7802	(38.08)	−15.1276	(48.91)	9	2,095
MARRIED = 0	−2.0548	(6.99)	−2.8714	(9.76)	−5.6511	(19.22)	9	1,855
RETIRED = 1	−1.9671	(5.63)	−5.5897	(15.99)	−12.6135	(36.09)	8	963
RETIRED = 0	−1.9684	(7.42)	−4.6514	(17.52)	−7.8735	(29.66)	9	2,987
BLACK = 1	−2.7295	(6.14)	−3.3922	(7.62)	−7.5027	(16.86)	6	494
BLACK = 0	−1.8738	(7.30)	−4.8573	(18.92)	−9.7249	(37.88)	9	3,456
CHURCH = 1	−2.1763	(5.56)	−5.3369	(13.63)	−4.7470	(12.13)	8	697
CHURCH = 0	−1.9526	(7.58)	−4.3052	(16.70)	−10.1812	(39.50)	9	3,253
BENEFITS = 1	−2.3831	(7.11)	−2.3833	(7.11)	−10.0434	(29.96)	8	1,265
BENEFITS = 0	−1.7939	(6.64)	−5.5373	(20.49)	−8.4938	(31.43)	9	2,685
MOVED = 1	−1.9123	(6.45)	−3.5743	(12.05)	−6.1390	(20.70)	9	1,554
MOVED = 0	−1.8605	(6.28)	−7.9804	(26.92)	−15.4823	(52.23)	9	2,396
OVEREST = 1	−3.1880	(8.00)	−8.4407	(21.17)	−20.5573	(51.56)	5	1,116
OVEREST = UNDEREST = 0	−1.7056	(6.48)	−2.3276	(8.85)	−6.1550	(23.40)	9	2,484
UNDEREST = 0	−2.6209	(5.21)	−7.5750	(15.07)	−28.5742	(56.84)	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.

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 GMM estimates that properly account for the existence of predetermined regressors. As this estimator relies on accounting for all potential paths of usage level and choice of tariffs over time, including several regressors reduces the chances that a particular cell, *i.e.*, a particular path of decisions for a cluster of individuals with identical observable demographics, includes any observations at all. 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

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	3,950
LOW INC = 1	−1.7328	(31.75)	0.3642	(2.91)	3.2571	(17.11)	369.992	668
LOW INC = HIGH INC = 0	−1.6912	(66.50)	0.5764	(9.59)	3.2276	(36.69)	1722.898	2,874
HIGH INC = 1	−1.7331	(24.92)	0.5619	(3.58)	3.1155	(14.58)	234.266	408
DINCOME = 1	−2.0408	(30.19)	0.7973	(6.11)	3.1935	(15.58)	260.263	683
DINCOME = 0	−1.6499	(70.87)	0.5048	(9.05)	3.2107	(39.93)	2050.425	3,267
HHSIZE = 1	−1.4620	(32.84)	0.3982	(4.65)	3.2386	(20.51)	648.485	817
HHSIZE = 2	−1.6579	(44.46)	0.6111	(7.25)	3.2278	(25.10)	823.698	1,303
HHSIZE = 3	−1.8118	(35.60)	0.1405	(1.08)	3.0371	(18.32)	395.571	811
HHSIZE = 4	−1.7839	(30.27)	−0.0466	(0.30)	3.3795	(15.08)	284.013	585
HHSIZE > 4	−2.1003	(24.49)	1.0141	(3.39)	3.5299	(11.53)	132.586	434
TEENS = 1	−2.0677	(32.49)	0.6782	(3.23)	3.3546	(16.04)	242.481	750
TEENS = 0	−1.6356	(69.51)	0.4885	(9.21)	3.1926	(39.77)	2062.152	3,200
AGE = 1	−1.6210	(18.73)	0.2697	(1.46)	2.9167	(11.34)	155.355	235
AGE = 2	−1.6259	(40.43)	0.5921	(6.04)	3.0474	(23.61)	694.975	1,051
AGE = 3	−1.7432	(63.64)	0.5488	(8.63)	3.3448	(33.70)	1473.016	2,664
COLLEGE = 1	−1.4680	(33.53)	0.4433	(4.63)	3.1072	(21.59)	622.282	792
COLLEGE = 0	−1.7707	(69.62)	0.5542	(9.15)	3.2418	(37.08)	1688.301	3,158
MARRIED = 1	−1.7238	(57.30)	0.6684	(8.77)	3.1634	(31.62)	1203.917	2,095
MARRIED = 0	−1.6768	(52.61)	0.4303	(6.14)	3.2856	(29.10)	1122.760	1,855
RETIRED = 1	−1.7400	(38.21)	0.7143	(6.99)	3.3179	(19.90)	544.966	963
RETIRED = 0	−1.6904	(67.77)	0.4762	(8.04)	3.1897	(38.11)	1782.296	2,987
BLACK = 1	−1.7978	(28.21)	1.1195	(5.49)	3.1317	(14.16)	255.872	494
BLACK = 0	−1.6886	(72.43)	0.4929	(9.26)	3.2324	(40.60)	2068.828	3,456
CHURCH = 1	−1.7209	(32.81)	0.5254	(4.27)	3.1127	(17.95)	403.143	697
CHURCH = 0	−1.6982	(70.56)	0.5413	(9.63)	3.2404	(39.15)	1925.785	3,253
BENEFITS = 1	−1.7931	(43.65)	0.4840	(5.12)	3.3164	(22.33)	646.447	1,265
BENEFITS = 0	−1.6630	(64.23)	0.5632	(9.22)	3.1765	(36.76)	1677.616	2,685
MOVED = 1	−1.6377	(48.57)	0.3136	(3.94)	3.2189	(27.50)	974.101	1,554
MOVED = 0	−1.7471	(60.65)	0.6934	(10.36)	3.2209	(33.00)	1348.630	2,396
OVEREST = 1	−1.9955	(41.00)	0.4503	(4.02)	3.0646	(18.91)	400.129	1,116
OVEREST = UNDEREST = 0	−1.5673	(59.79)	0.4145	(7.44)	3.3420	(34.15)	1722.032	2,484
UNDEREST = 0	−1.8784	(23.42)	0.4421	(1.98)	2.8298	(12.32)	159.640	350

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

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 3, 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 4 show this erroneous conclusion is only the result of ignoring the effect of unobserved heterogeneity associated to state dependence.

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 11. 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 subscription options does not necessarily mean that consumers will stay in the previously chosen tariff indefinitely.[15]

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 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 the results of the corresponding misspecified model that fails to address the endogeneity of lagged endogenous regressors. As in the previous section, results lead to opposite conclusions depending on whether we address the endogeneity of previous decisions or not.

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

[15] 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.

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.

mistake, *e.g.*, see Strotz (1956) and Laibson (2000). 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

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.

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.

5.3 Errare Humanum Est, In Errore Perservare Stultum

Before concluding, we pursue a bit further the result that mistakes are a transitory phenomenon, and compute the marginal effects associated with the transition among different states.[16] 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:

$$\hat{\triangle}_t = \frac{1}{N}\sum_{i=1}^{N}\left\{\Phi\left(\hat{\sigma}_t^{-1}\hat{\beta}\left(z^1 - z_{it}\right) + \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right]\right) - \Phi\left(\hat{\sigma}_t^{-1}\hat{\beta}\left(z^0 - z_{it}\right) + \Phi^{-1}\left[\hat{h}_t\left(w_i^t\right)\right]\right)\right\}. \quad (17)$$

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 of 1986.[17] 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 6.91% if consumers subscribed to the flat tariff option while the second row shows that this probability *decreases* by 1.22% had they subscribed to the measured tariff option. Thus, regardless of the choice of tariff, it is less likely, rather than more likely, that they make another mistake in their choice of tariffs.

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

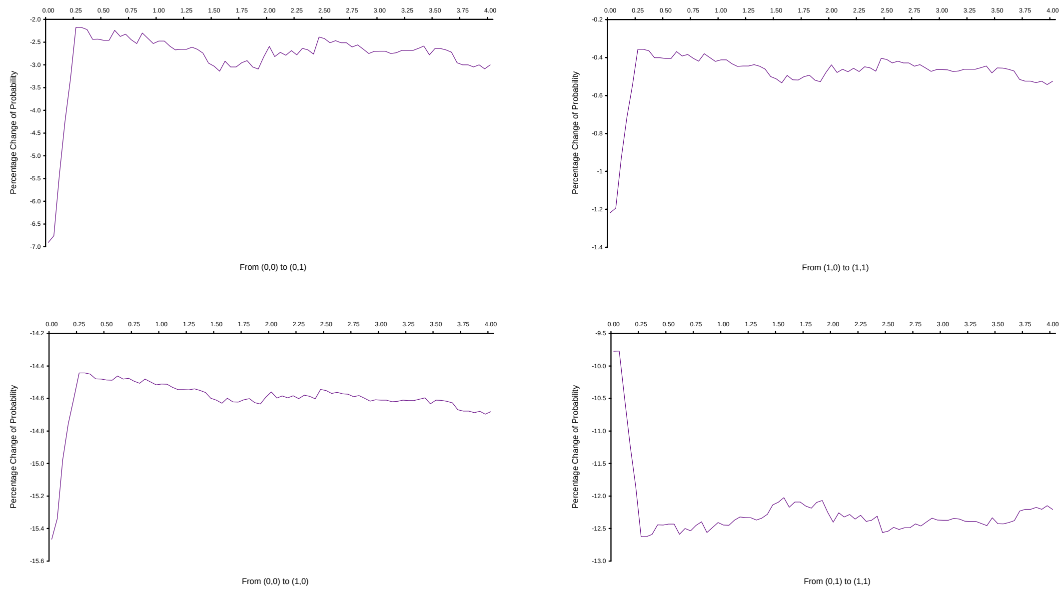
Percent change in the probability of choosing the current tariff option wrongly conditional on each transition among states.

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 15.47% if consumers subscribed correctly to the optional measured service in the previous month and by 9.78% if they subscribed wrongly to the optional measured service. Thus, consistent with the asymmetry in the

[16] 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.

[17] 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.

Figure 1: Marginal Effects at Different Mistake Thresholds



complexity of the problems discussed earlier, the probability of making a mistake is substantially lower after subscribing to the measured option. 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 one 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

To our knowledge, the effects of unobserved heterogeneity and unobserved investments in information, active cognition and deliberation costs in determining current choices have not been addressed before, neither in the experimental nor in the empirical learning and behavioral economics literature. The fact that the evidence turns out to be drastically different when predetermined variables and unobserved heterogeneity are appropriately treated indicates that they play an important role in the dynamic learning process.

Our empirical objective was quantitative in nature: When should we expect that deliberation and cognition activities represent critical limits to rationality or, alternatively, that they are cheap relative to payoffs? Since we can measure the magnitude of the cost difference between potential choices, the finding that households display rational attention lead us to infer an upper bound on the costs of active cognition and deliberation that are involved. This upper bound is low, and would even be lower if search and other costs were likely to be large. However, search costs are unlikely to be high since households receive every month a bill specifying their consumption behavior. Thus, we document that households who face the less complex, cognitively cheaper problem behave as predicted: they learn faster and are much less likely to make mistakes.

Our findings allow us to discard not only models of inertia and inattention, but also models where households are driven by impulsiveness or are time-inconsistent. Random choice of tariffs would simply imply no effect of lagged dependent variables (lagged choices and lagged outcomes), something that we do not observe in the data. Thus, our households are not impulsive. Neither they seem to be time-inconsistent. Households with non-constant temporal preferences, like those in models of myopic hyperbolic discounting would display a systematic overvaluation of the future: their actual consumption would systematically be above their planned consumption. Iff the optimal tariffs associated with these two consumption levels is different, then systematic *ex-post* mistakes should be observed in the data. Empirically, we find no evidence that households who systematically choose the measured service, expecting a low consumption level, systematically consume above the level that makes the measured tariff optimal *ex-post*. Thus, the evidence does not support the idea of time-inconsistent households.

Lastly, the generality of our results need not extend to problems that are more complex than the one we have studied. Yet, we have no way of assessing the degree of complexity of this problem relative to other problems that households may typically face. It is possible to argue, however, that the demand for many consumption goods and services is similar to this problem in that the optimal action often requires households, at the very least, to determine whether they expect their demand at a future date to be below or above a certain threshold. To the extent that this conjecture is correct, these results may gain a substantial deal of generality.

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