Three Essays on Macroeconomics and Laboratory Experiments

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Abstract

This dissertation examines two prominent macroeconomic models and their behavioral underpinnings in a laboratory setting. The first is that of state-dependent pricing models (i.e., “menu cost” models). Comparisons were made between laboratory results and a computer-simulated optimal behavior, and results indicate that subjects update prices too frequently resulting in statistically suboptimal profits due to subjects’ inability to clearly ascertain the optimal threshold at which to update prices. Second, the consumption predictions made under rational inattention theory were examined via a laboratory experiment. Results indicate that subjects’ behavior aligns well with predictions in that they consume stochastically, yet adjust their consumption and attention according to variations in the economic environment. Subjects also respond more quickly and in higher magnitude to negative income shocks compared to positive. Finally, the experiments provided two use cases that enabled the evaluation of how coding environments and demands on versatility of laboratory of experiments have evolved. Performance comparisons were made between two novel coding environments and the most commonly used experimental platform, z-Tree. Results indicate that while environments other than z-Tree offer substantially more flexibility and performance enhancements, these benefits can come at the cost of nontrivial software engineering resources.
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Dedication

To myself. For all of the countless days I spent alone in an office laboring. The missed Christmases. The missed Thanksgivings. The labor days. The memorial days. The birthdays, mine and others’. Saturdays. Sundays. Weeknights. Weekend nights. And everything in between and how far away this day felt.
Chapter 1
Introduction

The purpose of this body of work is to explore the microeconomic, or behavioral, underpinnings for two prominent macroeconomic models through the use of laboratory experiments. Further, this body of work links the work done herein back to the literature from an evaluation perspective with respect to resources required to develop said experiments – whether those resources be computational, time investment, or otherwise.

Chapter 1 articulates the formulation of an experiment to seeking to understand to what degree human subjects exhibit “state dependence” and the results found from implementing such an experiment. State dependent pricing models are commonly used in macroeconomics to understand the rigidity in price movements, but very little research has been done as to how closely the assumptions embedded in the model reflect human behavior. The experiment was devised to extend what had already been done in the literature to a more robust environment.

Chapter 2 examines to what degree consumers behave according to predictions made by the theory of the rational inattentive consumer. Again, an experiment was devised to test hypotheses derived from the rational inattention model that predicts distinct behaviors that changes in the economic environment would elicit from consumers.

Chapter 3 draws from the learnings from the first two chapters and considers the status of laboratory experiments in the context of a world that increasingly demands more functionality and robustness out of its experiments. Similar to how the field of economics as a whole has had to adapt to using computational methods to solve increasingly complex models, experimentalists are finding themselves having to move away from conventional methods of creating and implementing experiments due to the level of sophistication of hypotheses being tested. In this chapter, learnings from coding two custom environments are presented, and tradeoffs between three very common coding environments are presented to help researchers understand what environment might be best for their use case.
Chapter 1: State-Dependent Price Setting: An Experiment

1 Introduction

How firms make their pricing decisions is a fundamental question of macroeconomics. The degree of price rigidity is commonly considered the main factor affecting the transmission of monetary policy to the real economy and, not surprisingly, an extensive literature has explored this matter over the years. A growing body of empirical work has tried to quantify the degree of flexibility of prices. The most recent empirical evidence seems to unanimously support the conclusion that price adjustments occur at higher frequencies than initially believed (see, for instance, Klenow and Kryvtsov, 2008; Nakamura and Steinsson, 2007; Bils and Klenow, 2004, who find durations between four and eight months). At the same time, many competing theories have been proposed to provide a micro-foundation of price rigidity and embed it in macroeconomic models, from the more simple staggered price model of Calvo (1983) to the menu costs of Mankiw (1985) and the state dependent models (also known as “Ss”) introduced by Caplin and Leahy (1991). With so many alternative models still actively used in the theoretical work, economists seek to understand the intrinsic validity of the assumptions required by each, and controlled laboratory experiments provide a useful tool for doing so. In this paper, we investigate price rigidities relying on a fully experimental context. The purpose of our work is demonstrating to what extent a state dependent optimal pricing model has a legitimate behavioral basis.

Ss models hold at their core that agents should choose when to re-optimize solely based the state of the world (“state-dependent” adjustment) and not on time per se (i.e., “time-dependent” adjustment). In our experiment, we follow the approach of Magnani et al. (2015) to setup an experimental environment in which the subjects must adopt a state dependent pricing rule in order to maximize the profits of their production activity. The profits earned by the subjects are deleteriously affected by the deviation of the price charged
at a point in time from an optimal reference price, which depends on the underlying economic fundamentals. Prices can be reset to the optimal price at any time, but the subjects face an adjustment cost that determines the standard optimal threshold for price adjustments of the Ss models. We depart from Magnani et al. (2015)’s design in how we model the adjustment cost, which entails two fundamental differences with respect to their basic framework. First, in our experiment subjects must complete a real effort task in order to produce a unit of output and make profits. Second, the subjects must complete a time-consuming task in order to adjust the price they charge, thereby foregoing profits to do so. As a result of the introduction of these two tasks, the cost of updating in our model reflects a real trade-off between production and price setting, and it is not exogenously given to the subjects as in Magnani et al. (2015). The trade-off relies on the perception of time cost of the subjects, which is an individual specific cost. By internalizing the updating cost and examining how this affects subjects’ behavior, we are able to highlight a new dimension of the inability of price setters to accurately solve the problem facing them, which does not seem strictly related to economic uncertainty per se. This new dimension of the analysis provides additional insights on the role of attention and cognitive load in the price setting mechanism.

The main results of the experiment are essentially twofold. First, we find that subjects tend to respond in a way that is akin to state dependence. In each of the situations we examine, the average subject exhibits behavior consistent with the employment of a noisy decision rule for a threshold. They recognize the importance of an inaction region across treatments, although the precision of these regions is lower the higher the degree of uncertainty in the economy, and they exhibit some elements of time dependence in their pricing decisions. This first result is in line with the evidence documented by Magnani et al. (2015), who explain the large deviations of the subjects’ decisions from the optimal thresholds with a model of bounded rationality. In their model, the limitations to rationality increase as a function of the cognitive load fundamentally caused by the volatility of the economic environment.
in which subjects find themselves operating. Second, subjects in our experiment make an additional systematic error in the identification of the optimal thresholds due to our implicit adjustment cost. Comparing the experimental results to the optimal thresholds obtained by simulation of the profit-maximizing decision rules that a fully rational agent should have followed, we find that subjects adjust their prices too “early” with respect to the state, in the sense that they reset prices when the profit from a unit of output sold is still higher than the optimal threshold point. Since this type of error does not seem to depend on the uncertainty of the environment, we link it to the form of the adjustment cost used in our experiment.

The results obtained from the experiment suggest that a state dependent model is insufficient to fully capture the price setting behavior. Rather, an hybrid model that mixes, at the same time, state and time dependent elements would be necessary to adequately fit the data from the laboratory. Similar conclusions were suggested by the analysis of field data as well; for instance, Nakamura and Steinsson (2007) and Klenow and Kryvtsov (2008) show that the shape of the hazard functions of price adjustments in field microdata is not directly attributable to simple menu-cost models. Also Magnani et al. (2015) conclude that bounded rationality is the most plausible explanation of this hybrid dynamics of pricing decisions given their experimental setup. Subjects experience cognitive costs in assessing the state of the economy and formulating their optimal response, and higher cognitive loads decrease subjects ability to fully adopt state dependent rules by increasing these costs. While there are some exceptions, most studies have found negative effects of cognitive load on basic economic behavior (see Deck and Jahedi, 2015, for a recent survey).

We further explore this type of explanation with the introduction of the trade-off between production and price setting tasks. The purpose of this feature of our design is to have a simple and direct way to tamper with the cognitive capacity of the subjects; if the cognitive load explanation is valid, this additional burden should negatively affect the performance of
subject across treatments. The implicit cost of adjustment achieves this goal in two ways: the first is by forcing subjects to infer their cost of resetting prices in terms of forgone profits while they accomplish the updating task; the second is by adding a real source of distraction for the subjects that diverts their attention from screening the state of the economy to executing the production task. While it is hard to precisely disentangle the two channels, they both operate in the same direction by increasing the likelihood of optimization errors. This implicit trade-off provides a simple micro-foundation of the attention allocation mechanism in the laboratory, but it can be considered fairly realistic for a large group of firms in which one or only very few workers are employed. The Small Business Administration (SBA), for instance, reports that over 75% of businesses in the US have no employees. Hence for most companies, pricing, production, and other tasks likely fall to a single person. Whilst experimentally adequate in general, this trade-off would be clearly much less realistic for larger and more sophisticated enterprises in which separate divisions are in charge of production and pricing decisions. However, other distracting factors would likely impact the pricing divisions of larger firms and hence our experimental set-up likely has relevance for them as well.

The effect of the endogenous cost of adjusting prices is a uniform shift of thresholds across decision environments. Sellers, pressured to actively engage in production, seem to systematically underestimate the actual cost they must bare to update prices, and as a consequence they end up updating relatively more often than optimal. This behavioral pattern can be explained in two ways. First, the literature on cognitive load (Deck and Jahedi, 2015) has demonstrated that higher cognitive load leads people to be more impatient; subjects update more often in the short period in the experiment. Second, this pattern is also suggestive of models, as for instance rational inattention, in which information processing limitations trigger decisions about attention allocation. Intuitively, subjects focus more attention on production which, given a limited processing capacity, leads them to acquire a worse esti-
mate (signal) of their implicit costs. Finally, our experimental results offer some guidance also in interpreting some of the empirical evidence on price changes. Specifically, two observations are worth mentioning. First, the flat hazard rates found for the experimental updating decisions suggest that models with constant probability of updating, such as the Calvo pricing, might still have some practical utility in approximating the observed price changes in spite of the oversimplified mechanism of price setting they embed. Klenow and Kryvtsov (2008) find that flat hazard rates are typical with field data as well. The second observation is that the large differences in price change frequencies across sectors documented by both Nakamura and Steinsson (2007) and Klenow and Kryvtsov (2008) could find at least a partial explanation in our price setting trade-off mechanism. Price durations in the field could reflect the interaction between the price setting activity and other phases of the production and commercialization of a good.

The rest of the paper is laid out as follows: Section 2 elaborates on the relation between this paper and the literature in detail. Section 3 describes a simple theoretical model that provides us with some qualitative predictions on the relative movements of observations from treatment to treatment. In Section 4, we discuss the experimental design and implementation, and in Section 5 we present and discuss the results. Finally, in Section 6 we offer some discussion of our findings and some ideas for potential future work.

2 Related Literature

Given the complexity of bringing macroeconomic models into a laboratory setting, it is not especially common to use experiments to examine these models. With regard to price rigidity specifically, there is a particular gap in the literature. To the best of our knowledge, the only paper currently examining these issues is Magnani et al. (2015), who devise an experiment to test a state dependent pricing model. They find that adjustment distributions in the laboratory closely resembled field data, and that mean switching behavior
obeyed comparative static predictions, on average. In their paper, subjects face only a simple price adjustment task in which they must incur a fixed adjustment cost in order to reset the current price to the observable “optimal price.” We extend their framework by including real production and price updating tasks, which allows us to define an adjustment cost that reflects the implicit trade-off between the two tasks. This new mechanism negatively affects the decision process of the price setters by exposing the subjects to an increased cognitive load; in response to that, subjects systematically over-update.

The interpretation of our results is closely related to the literature on the negative effects of cognitive load on people’s economic decisions. Kahneman (2002, 2011) develops a dual system model of behavior.¹ In this framework, people have both an impulsive system (System 1) and a deliberate reasoning system (System 2). When faced with a choice, System 1 quickly arrives at a choice, which System 2 can override if brought to bear. However, when a person is placed under cognitive load, the ability of System 2 to counteract System 1 is reduced. Psychologists have recognized that people have limited cognitive resources since at least Miller (1956), who found that people can only hold seven items effectively in short term memory. As a result, much of the work on the negative effects of cognitive load involves examining choices people make while memorizing a 7 digit number. Using a within-subject design, Deck and Jahedi (2015) demonstrate that those whose math performance is most impacted by cognitive load are also the people who become more impatient and more risk averse when under cognitive load. This pattern of behavior provides strong evidence in favor of the dual system model. Relatedly, studies have found that people with high cognitive ability are less risk averse and more patient (see, for example, Dohmen et al., 2010; Benjamin et al., 2013).² Thus, increasing cognitive load effectively serves to reduce the cognitive ability of the decision maker. In our setting, the heightened cognitive load could explain two of the

¹Similar models have also been developed by Fudenberg and Levine (2006) and Mukherjee (2010).
²While cognitive load can be viewed as a state, cognitive ability is typically viewed as a trait.
behavioral patterns that we observe. First, because cognitive load leads people to be more impatient, they tend to update before they should. Second, because their cognitive ability is reduced, they have difficulty identifying a consistent threshold.

State dependent models are only one of the competing models of price rigidity currently in use. The evolution of these models dates back to the simplest mechanism of fixed contract durations, as proposed by Fischer (1977) and Taylor (1980). Both these papers develop rational expectation models wherein firms are locked into staggered contracts. Applying the model, Taylor finds that contracts of three or four quarters would be enough to explain unemployment rigidity. Staggered prices are introduced by Calvo (1983), which is considered the fountainhead of the New Keynesian approach to modeling price rigidity. Even though relatively more basic and stylized than a Ss model, this approach is still largely used in modern macroeconomic models due to the convenient tractability it offers. In his model, only a fixed fraction of firms are exogenously chosen to update their prices in each period, while the others are forced to keep the older price level. In a different approach, Mankiw (1985) suggests that prices are sticky because of “menu” costs. That is — real, explicit costs incurred by producers to update their prices. A firm observes the deviation of its current price from the optimal price level, and it decides to update its price when the possible increase in revenues from adjusting is higher than the explicit updating cost. State dependent models can be considered a logical progression of these last two models. Our paper, similarly to Magnani et al. (2015), shows that price setting fundamentally follows a state dependent rule, even though it also exhibits traits attributable to time dependence and elements of price rigidities typical of the Calvo pricing.

A parallel strand of the same literature unfolded around the relation between prices and information in the economy. Hayek (1945) argues that prices are the means by which economic changes are communicated across the entire economy to its various participants through a market mechanism. As a consequence, more price flexibility would reflect improve-
ments in the market communication system. Lucas (1973) proffers a model that derives a short-run Phillips Curve from the similar idea that prices convey noisy information about the stance of the economy and producers demand. More recently, stickiness in information propagation has been used by Mankiw and Reis (2002) to obtain sticky prices and inflation persistence in a way alternative to the New Keynesian Phillips Curve. Finally, Sims (2003) proposes a sophisticated model of price rigidities based on a limited information-processing capacity of the price-setting agents. This mechanism, known as rational inattention, improves on menu costs and Calvo/Ss models in that it can simultaneously generate the observed micro flexibility together with sufficient rigidity of prices on the macroeconomic level.

The essence of the rational inattention model was laid out in Matejka (2010). In his model, firms face a large number of noisy signals about factors that affect pricing decisions. However, they face a constraint on the total amount of attention that they may allocate over these signals, so they must decide the signals on which they will focus. Even though we do not explicitly explore the information channels in this paper, we identify an additional set of errors in the pricing decisions which, as discussed above, can be interpreted in terms of bounded rationality and the effects of the reduction of subjects’ attention on the optimal pricing due to the production activity.

Finally, our paper is related to the empirical literature that studies the characteristics of price adjustments from a more micro perspective at firm level. The main conclusion of this strand of literature is that prices are actually quite flexible and price adjustments occur relatively often, with a median frequency largely below one year. Blinder et al. (1998) surveys firm managers and concludes that prices change approximately only once per year; similar evidence is provided by Carlton (1986), Cecchetti (1986), Kashyap (1995), Levy et al. (1997), MacDonald and Aaronson (2001), and Kackmeister (2001). This was a commonly held belief in the 1990’s until Bils and Klenow (2004) studied frequency of price changes in 350 categories of goods and services representing approximately 70% of consumer spending.
and found that half of them had median frequency of price adjustments of approximately 4.3 months, substantially shorter than that reported by Blinder et al. (1998). Also Nakamura and Steinsson (2007) find that the typical price duration is below one year, around 7 – 8 months for median price adjustments, and that there were not gigantic differences across regular prices from the time periods of 1988-1997 as compared to 1988-2005. For these same periods, Klenow and Kryvtsov (2008) find qualitatively similar results for both posted prices (i.e., raw BLS data) and regular prices.³ Our experiment confirms that price setters understand and follow the right incentives to adjust prices relatively often, even in presence of hard costs of re-adjusting due to foregone production and profits. In this respect, the evidence we provide is broadly consistent with the conclusion that prices are indeed quite flexible as suggested by the most recent empirical literature.

3 Theoretical Framework

We first present a stylized theoretical model to help provide a structure for understanding the pricing decision faced by subjects in the experiment. As with Fischer (1977), Taylor (1980), Calvo (1983), and others, this model is an abstraction from the underlying decision making setting. That is, our research process began with the creation of an environment that mimics the phenomenon in the naturally occurring world that we wanted to investigate and then we constructed a model to forecast behavior. Thus, our experiment is not meant to be a strict test of this model. The model is a state contingent pricing model in discrete time in which a producer must make a decision on how to allocate time between the production of one unit of a single good and a task to optimize the price at which the good is sold. Time is the only input for both activities, and it takes one unit of time to produce one unit of

³Moreover, Klenow and Kryvtsov (2008) note that one of the primary issues that arise when assessing price rigidity is whether to focus on the median or the mean of the price change and they stress that it is also important to consider how one must account for “sales” (i.e., short-term reduction in prices, often used to lure customers into the store).
the good. The price setting task is time consuming, too; and it is also assumed to take one unit of time. The producer, then, faces a trade off between earning positive profits from the current production or suspending production for one period to update the current price and enjoy higher expected unit profits in the next period. The profit function, \( \Pi_t \), is specified in terms of the deviations of the optimal price, \( P_t^* \), from the current price, \( P_t \), as

\[
\Pi_t = \exp[-\mu \cdot |X_t|]
\]

where \( X_t = P_t - P_t^* \) and \( \mu > 0 \) is the parameter that regulates the rate of decay of profits. Given this profit function, we can express the Bellman equation of the producer’s recursive problem as

\[
V(X_t) = \max_{u_t} \{(1 - u_t) \Pi_t + \beta \mathbb{E}_t (V[X_{t+1} (X_t, u_t, \varepsilon_{t+1})])\}
\]

where \( u \) is a binary variable representing the decision of the producer to update the price \( (u = 1) \) or produce in the current period \( (u = 0) \), and \( \varepsilon \) is a zero mean, i.i.d. exogenous innovation to \( X \). The optimal price level \( P_t^* \) drifts away from current price \( P_t \). The price deviations are assumed to follow either a random walk process conditional to the decision of not updating or to be a simple white noise shock if the producer decides to update. Hence, the stochastic process of \( X_t \) can be expressed as

\[
X_{t+1} = \begin{cases} 
X_t + \varepsilon_{t+1} & \text{if } u_t = 0 \\
\varepsilon_{t+1} & \text{if } u_t = 1
\end{cases}
\]

The producer makes the production decision after observing the current \( X_t \). If the price is updated, \( \Pi_{t+1} \) is known up to the \( t + 1 \) realization of the random innovation \( \varepsilon_{t+1} \). Finally, future profits are discounted by the subjective discount factor \( \beta < 1 \).

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\(^4\text{There is no theoretical reason why the time cost of production and price setting need to be the same; but observed behavior in the experiment is consistent with this simplifying assumption.}\)

11
There are two important differences between this theoretical framework and our implementation of the experimental design below. First, the two tasks will last prolonged periods and not simply one. This implies a longer lag between the time when a task is started and the time when it is completed, with an increase of uncertainty that affects the updating and production decisions of the producers. In the model, this feature is only in part captured by the fact that $X_{t+1}$ after the price adjustment still has a stochastic component represented by $\varepsilon_{t+1}$. Second, it is possible that the time necessary for subjects to complete the two tasks may not be exactly the same and could vary over time. While the structure of the model is time invariant, the calibration of $\beta$ allows for some flexibility in the transformation rate between the two tasks since $\beta$ can be used to modify the relative value of updating even if the two tasks require the same input of one unit of time. Nevertheless, the model provides a very good guidance in calibrating the parameters of the experimental treatments and a set of very useful comparative statics to benchmark our experimental results.

The model is solved numerically by value function iteration for a discretization of the state space. The associated solution of the policy function $u_t = m(X_t)$ provides the inaction regions of the producers’ updating decisions (the set of values of $X_t$ for which it is more profitable to produce rather than updating the current price). In addition to the profit decay parameter $\mu$, the solution of the model depends on the uncertainty about the future economic circumstance that is embedded in the variability of $\varepsilon$. We model $\varepsilon_t$ as a discrete random variable which takes on two possible values with equal probability:

$$
\varepsilon_t = \begin{cases} 
h & \text{with } p = .5 \\
-h & \text{with } p = .5 
\end{cases}
$$

The step size $h$ of the increases (decreases) of $\varepsilon_t$ represents also the standard deviation of the innovation, and it can be thought of as a measure of the volatility of the underlying economic fundamentals on which $X_t$ depends. We set $\beta = .98$, and we study four calibrations
of \(\mu\) and \(h\) that are going to match the four treatments in our experiment.\(^5\) We choose fast/slow decay and small/large step size. Intuitively, the inaction region gets larger when the decay is faster because a faster decay of profits causes a reduction of the future value of updating; higher uncertainty affects the inaction region in a similar way. Figure 1 illustrates the inaction regions for the four combinations of the parameters; the proposed values are sufficient to obtain a substantial variation across different combinations.

![Figure 1: Inaction regions for the stylized theoretical model.](image)

From the numerical solutions, we were also able to obtain qualitative (i.e., directional) predictions concerning the relative magnitudes of total profits earned. Figure 2 shows that we should anticipate subjects earning the most profit in the slow decay/small steps treatment, approximately the same profits in the intermediate treatments, and the least amount of profit

\(^5\)We adopt a value of the discount factor that is fairly standard for macroeconomic applications. However, a direct mapping of \(\beta\) into the discount factor of the experimental subjects is not straightforward. Therefore, we check the robustness of the theoretical predictions of the model for a large set of alternative calibrations of \(\beta\), included between 0.80 and 0.99. Smaller values of \(\beta\) shifts the thresholds in Figure 1 to the left by only a few percentage points, while the comparative statics of the results are completely preserved. Since we are mostly interested in comparisons across treatments, different values of \(\beta\) would be equivalent.
In the fast decay/large steps treatment, the most volatile of the four. We were also able to obtain directional predictions on the relative fraction of tasks completed that would be price updates. Figure 3 shows what we would predict intuitively: That subjects would update relatively fewer times in the first treatment, approximately the same number of times in the intermediate treatments, and update prices most frequently in the last.

![Figure 2: Theoretical relative total profits for the stylized model. Values standardized to profits in Treatment 1.](image)

4 Treatment Design and Experimental Implementation

4.1 Experimental Framework

In order to examine the impact of implicit menu costs on price updating decisions, an experiment was devised to be administered in a laboratory setting. The experiment was developed using a combination of HTML5 and Javascript, and it was deployed using the Google Chrome internet browser.

The experiment put each subject in charge of a virtual pizzeria which sells its pizzas at
Figure 3: Theoretical share of price updates for the stylized model.

The subject earns profits (denoted in Experimental Lab Dollars, or ELD) by fulfilling incoming orders by assembling pizzas with the correct ingredients. The upper left-hand corner of Figure 4 shows the pane into which an order comes. The five boxes at the top of the ingredient columns indicate the five ingredients for that “order.” These boxes are populated by randomly and independently selecting an ingredient from a list of ten possible (note that this means that any single ingredient could possibly occur multiple times on a single “order”). Below each of these boxes is a column of the ten possible ingredients. The ingredients in each of the columns are randomly arranged for each new order.

To fulfill an order the subject must use a computer mouse to select the box next to the ingredient in the column that matches each of the five boxes above it. Once a subject has selected ingredients, the subject then clicks the “Submit Order” button and earns some profit

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6If it is common in experiments to avoid using particular contexts that bias behavior, in our case, framing the task in the context of a business aids in helping the subjects comprehend the task at hand without either directing subjects as to how they should behave or providing them with an informative reference.

71 ELD = 0.25 USD. The subjects were informed of the exchange rate before the experiment began.
if all of the ingredients are correct. The subject earns profits determined by the difference between his *Current Set Price* and the *Optimal Price*, $X_t$. The problem faced by the subject is that the *Optimal Price* changes according to a random walk every 0.2 seconds, where with probability 0.5 the *Optimal Price* increases or decreases by step size $h$. Made available to the subject is a pane, shown in the lower left-hand Figure 4, where the subject may observe the realtime movement of the *Optimal Price* (the green line) and its relationship to his *Current Set Price* (the red line) enabling the subject to better understand the variability in a given treatment. This allows a visual representation of the $X_t$ upon which each subject’s instantaneous profits depend. The instantaneous profit is also displayed on the screen, as is the “Cumulative Profit” the subject has earned that period.

The profit earned by the subject when the *Current Set Price* and the *Optimal Price* are equal (i.e., $X_t = 0$) is 1.00 ELD. Profit per pizza is calculated as $\Pi = \exp[-\mu \cdot |X_t|]$. If the subject determines that the per pizza profit that he is currently receiving is too low, then at any point that subject has the option to update his price—that is, set his *Current Set Price* equal to the *Optimal Price* at that moment in time. However, in order to do this, the subject must complete the task of entering an *Update Code* that is similar to the process of baking a pizza. The upper right-hand corner of Figure 4 shows the pane in which the *Update Code* is observed and entered by the subject. The top five boxes are populated randomly and independently with an integer in the set [0, 9]. The numbers in each of the columns below each of the five boxes are arranged randomly each time the “Update Price” button is clicked. Upon selecting the box next to each of the boxes next to the numbers that correctly correspond to the numbers in the five boxes, the *Current Set Price* is immediately set equal to the *Optimal Price* at that moment. To prevent a subject from selecting all of the ingredients in a pizza then updating his price and then submitting the already-prepared pizza, any time either the “Submit Order” or “Update Price” button was clicked, all of the

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8For ease of exposition, we refer to this action as “baking a pizza” throughout the paper.
Figure 4: A screenshot showing the user interface with which the subjects interacted.
checkboxes in each of the columns for both tasks were cleared. That is to say, completing one task—either correctly or incorrectly—completely reset both tasks.

It should be noted that the decision node for the subject is made immediately after the completion of the previous task (except for the very first task, of course). Because of this, there is uncertainty in the profit that will be subsequently earned by baking a pizza. That is to say, the time necessary for the subjects to complete a task introduces a delay between the moment a task is started and when the task is concluded. After the beginning of the task, the Current Set Price can drift several steps away from the initial observed price, following a richer distribution over time than the simple Bernoulli that governs the price change at each discrete step of time. This is illustrated for a few steps in the probability tree in Figure 5 and the full distribution is represented for ten seconds from the beginning of a task by the two panels of Figure 6. The bottom panel illustrates the three dimensional distribution as a function of time, expressed in seconds, and the deviations of the Optimal Price from the initial price, measured by the number of discrete steps away from the price observed at the beginning of the task. The top panel of the figure shows a two dimensional aerial projection of the same distribution to aid in interpreting the three dimensional distribution. The distribution is symmetric around the initial price and, as time passes, it spreads out becoming more similar to a Gaussian distribution. However, it is important to notice that the distribution fundamentally corresponds to a random walk, and it remains centered around the initial price at any point in time, making the conditional expected value for the subjects independent of the time required to complete the task.\footnote{The actual size of these steps depends then on the calibration of $h$ in each treatment, but this does not have any bearing on the shape of the distribution in Figure 6.}

Using the simulation data that we discuss in detail in Section 5.1, we can compute the average time that passes after someone updates their price until they face a decision at a particular instantaneous profit given they are following the optimal threshold. Figure 7 plots
Figure 5: Probability tree of the Optimal Price deviations from the initial Current Set Price for four price changes.

this relationship by treatment. One should keep in mind that time passes while engaging in baking so that the relevant instantaneous profit is the one they observed after, and not during, baking. Hence, one can encounter prices well below the instantaneous profit threshold if the previously encountered level was just above the threshold and the price continues to move further and further away from the optimal level while one is baking. The size of the markers in Figure 7 corresponds to the relative frequency of observing that instantaneous profit – higher profit levels are more commonly encountered. It is also worth noticing that these averages take into account that the probability distribution of the price movements experiences a truncation every time a subject crosses the optimal threshold. That is to say, starting at the optimal price, the instantaneous profit distribution has a precise theoretical form (derived from Figure 5) for any time horizon. However, the portion of this distribution actually spanned during a price spell will not always be the same because it depends on the
Figure 6: Distribution of price deviations from the initial price observed when an updating task is started.
duration of the spell itself.

As would be expected, a higher profit falloff rate and a larger price step size correspond to a faster profit decay and also a more rapid realization of the updating thresholds. The three horizontal red lines are the optimal thresholds in the four treatments. Treatment I has the highest threshold, but also a relatively smaller profit falloff than the other three treatments, which implies the largest average time to hit the threshold (about 80 seconds). We calibrate Treatments II and III to have nearly the same threshold and instantaneous profit time profile, while Treatment IV exhibits the lowest threshold and fastest time to reach it. Figure ?? in the online Appendix illustrates the full time distribution of profits in each treatment after a price update underlying the output in Figure 7. Consistent with this output, profit diffusion gets faster and falls deeper moving from the first to the last treatment.

4.2 Experimental Treatments

The experiment was conducted in the Behavioral Business Research Laboratory at the University of Arkansas. We employed a $2 \times 2$ within-subject design, and treatments were ordered so as to mitigate order effects. Table 1 shows the four treatments and their associated parameter values, also relating them to the qualitative language used when discussing the stylized model of slow/fast decay and small/large step size. Further, in order to eliminate the possibility of backward induction by subjects, they were told that each treatment period had random termination with an average expected duration of approximately five minutes. In a given session, every subject faced each of the four treatments, but the order of the treatments was determined by random assignment.\footnote{Four termination times were determined in advance. For every subject, the first period lasted 350 seconds. The second, third, and fourth periods experienced lasted 380, 260, and 300 seconds respectively. This ensured that fatigue and experience with the interface were held constant.}

Recall from Figure 1, that our theoretical predictions indicate that Treatment I—being the least volatile—should have the smallest inaction region. The most volatile treatment,
Figure 7: Average time at which a given level of instantaneous profit is expected to be observed by treatment. Simulation-based profit distributions, with ten seconds task duration. The size of the dots in each sequence of points indicates the relative frequency of realization of a profit level within each treatment. The horizontal lines indicate the optimal updating thresholds from simulation.

Treatment IV, should have the largest, and Treatments II and III should be very similar and fall in between the other two. This is directly related to Figure 3, where we see that Treatment I should have the fewest number of price updates, Treatment 4 should have the most, and Treatments II and III being very close to one another and falling in between the other two treatments. Finally, we see in Figure 2 that we would expect subjects to earn the highest profits in Treatment I, the least profits in Treatment IV, and intermediate profits in Treatments II and III.
Table 1: Profit falloff parameters and *Optimal Price* step size values for each of the four treatments.

<table>
<thead>
<tr>
<th>Profit Falloff Parameter ($\mu$)</th>
<th>Step Size ($h$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\mu = 0.25$ (slow decay)</td>
<td>$h = 0.16$ (small step)</td>
</tr>
<tr>
<td>$\mu = 0.50$ (fast decay)</td>
<td>$h = 0.32$ (large step)</td>
</tr>
<tr>
<td>Treatment I</td>
<td>Treatment II</td>
</tr>
<tr>
<td>Treatment III</td>
<td>Treatment IV</td>
</tr>
</tbody>
</table>

4.3 Experimental Implementation

Before beginning the experiment, the subjects worked through a series of instructions explaining the mechanics of the tasks they were being asked to perform.\footnote{These instructions have been included in Appendix A.} They were given the opportunity to read through these at their own pace and were also given the opportunity to ask questions for clarification. After reading through the instructions, the subjects were required to participate a practice round that lasted two minutes\footnote{The profit falloff parameter and step size used in the practice round were different from those parameters used in the actual experimental treatments ($\mu = 0.375$ and $h = 0.32$). The reason for this was that each of the parameters were at exactly the midpoint of the two extrema of the parameters that the subjects would encounter in the experiment. The concern was that if the parameters were too drastic in either direction, it would condition the subjects to expect a certain level of volatility. Setting the practice parameter values directly in the middle of the actual experiment parameters gave them a good idea of a baseline, so to speak, and so what they experienced was somewhat of a perturbation from this initial exposure.}

Upon completion of the practice round, the subjects were prompted to inform the experimenter. At this point, the subjects were required to complete a quiz\footnote{A copy of this quiz has been provided in the Appendix B.} to test their understanding of the experiment. Each quiz was evaluated by an experimenter, and for those questions that were answered incorrectly, the experimenter privately explained the correct
answer to the subject. The experimenter then entered a pass-code enabling the experiment to commence. Prior to the beginning of each treatment, the subject was given a 60 second forewarning. During this time, the subject was presented with information about the treatment he was about to face. For instance, for Order A, prior to Period 1 (which corresponds to the parameters used in Treatment 1) beginning the screen displayed the following:

Your first period will begin in 60 seconds.

In this period the Optimal Price will move relatively slowly, and the profit fall off will be relatively small.\(^{14}\)

Similar information was provided for 60 seconds before each period. At the 10 second mark the screen turned a bright red color and informed the subject that the next period began in 10 seconds.

Subjects were recruited from the undergraduate student body at the University of Arkansas, and a total of 70 subjects participated in the experiment over the course of 6 sessions in the month of June 2014. Subjects earned an average total profit, including a $5 show-up fee, of $18.04 USD. Each session lasted approximately 1 hour, which included working through the instructions, the practice round, taking the comprehension quiz, and participating in the actual experiment (i.e., all four treatments, each one lasting approximately 5 minutes). Payouts in excess of the show-up fee were determined by the subjects total cumulative profits across all four periods.

5 Results

In this section, we enumerate the main results of our experiment, and we discuss them in comparison to the quantitative predictions obtained from a benchmark simulation of the

\(^{14}\)We adopted the language of “period” rather than treatment when communicating with the subjects. “Period” simply indicates the position in which the subjects encountered the various treatments. For every subject, there were four periods experienced chronologically; these periods, depending on the Order, could have different treatment parameters.
experiment. The results can be summarized in three points. First, although the subjects seem to recognize the state-dependent nature of their price updating decision, they typically identify smaller than optimal inaction regions. Second, subjects do not follow purely state-dependent rules and the updating decisions exhibit some degree of time dependence as well. Third, we observe a large heterogeneity in the thresholds across individuals. However, conditional on an individual threshold, subjects tend to update their Current Set Price too frequently relative to the profit-generating production tasks. For ease of presenting results, we will throughout this section present results corresponding to each treatment in the following form: (Result₁, Result₁I, Result₁II, Result₁III, Result₁IV).

The predictions of the model are quite stark and it should not be surprising that people do not behave in strict accordance with those predictions. Rather the objective is to understand how people actually behave. Economic models are designed to be simplifications that capture core aspects of a decision problem and laboratory experiments provide a means of evaluating the predictive success of those models. In this, we are primarily concerned with the type of decision rules that people use to determine when they should update their prices and the behavioral responses to changes in the decision environment. We find that these aspects of the model predict the observed behavior reasonably well, although people’s decisions do exhibit some time dependence. That people behave in a consistent manner and respond to environmental changes in an intuitive manner, we take as evidence that the subjects understand their tasks and act in a meaningful way. While it is always true that the participants might eventually change to some other strategy, perhaps to the optimal one, we find no evidence of learning within our experiment.¹⁵

¹⁵As described in detail in section A of the online Appendix, we look for learning effects in two ways. The first is by comparing behavior in a treatment based on the order in which the subject experienced the treatment. Such an order effect, if it existed, would suggest that learning was being transferred across environments. Second, we compare a decision maker’s behavior during the first portion of a treatment to her behavior later in the same treatment to look for environment specific learning.
5.1 Simulation

After the experiment was conducted and the data analyzed, it was possible to simulate what the optimal behavior would be taking into account the actual time people need to complete tasks. A simulation is necessary because the experiment adds a significant amount of complexity beyond the stylized model presented in Section 3. The simulated outcome of the experiment provides accurate theoretical predictions fully consistent with the experimental design, which can be directly compared to the outcome of the actual experiment. Looking at the data we deduce the time it took for a subject to bake a pizza by taking the difference between when the “Submit Order” button was clicked and when the last event happened—either the clicking of “Update Price” or “Submit Order.” The data show that the mode is stark and approximately 10 seconds, with a mean baking task time of (12.9, 12.8, 12.6, 13.7) seconds. If we also look at the time subjects took in order to update their Current Set Price to match the Optimal Price, we find qualitatively similar results. The data show that yet again the mode is very pronounced around 10 seconds, with mean updating times of (10, 10.5, 10.5, 10.7) seconds. We found that updating and baking times were not always statistically similar from treatment to treatment, but we find that the differences are economically negligible.

For the simulation, we chose 10 seconds for both the pizza baking and the updating task.\textsuperscript{16} To simulate this decision mechanism a MATLAB code was written to produce an optimal price vector according to the same generating function as discussed in Section 3. The code initializes assuming that the first task an individual will perform is the baking of a pizza because every period in the experiment starts with the instantaneous profit at its maximum. The code then moves forward in “time” along the optimal price vector the amount

\textsuperscript{16}We looked at several variations of this task timing choice (mode, median, mean, and introducing noise). We decided to use 10 for ease of communication and because we found that the variation of the task time had little to no bearing on the outcome of the simulations.
of time it takes for the baking task to be performed. Then it calculates the instantaneous profits earned by baking the pizza at this “time” and adds it to total profits. Next, if the instantaneous profit is below a given threshold, then the code moves forward in “time” the amount of time it takes to perform the price updating task and resets the Current Set Price equal to the Optimal Price. If the instantaneous profit is above the given threshold, then a baking task is initialized, meaning the code moves forward in 10 seconds “time” along the optimal price vector, and the instantaneous profit at that point is added to the total profits. The code does this until 322 “seconds” have been simulated.\textsuperscript{17} This entire process is referred to as one simulation.\textsuperscript{18} The code collects data on the total profits, the number of pizzas baked, and the number of price updates in that simulation. The code calculated the average total profit across 10,000 simulations performed at each decision threshold in the range $[0.01, 0.02, \ldots, 0.99]$. The optimal inaction regions are defined by the thresholds that maximize the average total profits.

Table 2 summarizes the pertinent results of the simulation. Of particular note are the optimal decision thresholds, in ELD, of (0.52, 0.38, 0.38, 0.20). Further, the average number of price updates at the optimal threshold are (2.9, 4.3, 4.3, 5.3); the average number of pizzas baked in each of the four treatments are (29, 27.7, 27.7, 26.7); and the average total profits were (21.4, 16.8, 16.8, 11.6) in ELD.

5.2 Inferring Thresholds from the Data

While the updating thresholds are clearly defined in the simulations, the inaction regions adopted by the subjects in the experiment are less easy to identify due to the mistakes the subjects make in establishing the correct thresholds. Therefore, the subjective thresholds of these regions must be indirectly inferred from the observable decisions of the subjects, accounting for the uncertainty they face in making their decisions. Our strategy is to rely

\textsuperscript{17}This is the average duration of the four treatments.
\textsuperscript{18}An illustrative example of this process, and a pseudo-code have been provided in Appendix C.
on a comparison of the distributions of the profits observed by the subjects when they make a decision of either updating the price or baking a new pizza. Using only the distribution of the profits for the updating decisions would not be efficient because the distribution of profits for baking decisions provides valuable information about where it is more likely a subject places his implicit threshold. Intuitively, if the subjects understand they should follow a state dependent rule, they would start with a prior belief of the position of the threshold and search for the most profitable threshold as they conduct the experiment. Obviously, the process of uncertainty reduction is not going to be flawless, and we can suppose that the subjectively optimal threshold would lie inside a rather imprecise area of transition from the inaction region into the action region. Figure 8 illustrates the profit realizations (green line) and the baking/updating decisions (red and blue squares, respectively) for four of the subjects in the experiment (such plots for all 70 subjects may be found in Section ?? in the online Appendix). Each row represents a subject, columns are the treatments, the numeration above each panel reflects the order in which that subject observed the treatments, the dashed horizontal lines show the optimal thresholds found in the simulation stage. Instantaneous profits are on the vertical axis, and seconds are on the horizontal one. As expected, the baking decisions are typically clustered in the high-profit region, while the updating decisions occur in a region of lower profits; however, the two regions are only rarely completely disjointed. Updating decisions are often made for very high profits as well, and some baking decisions can occur at profits below the average point of price updating. This figure also illustrates well the point emphasized in Section 4.1 that there is unresolved uncertainty between the decision node and its completion. We see in Figure 8 that immediately after the completion of a given task (i.e., the decision node point), the optimal price begins to deviate from where it was when the decision was made. Looking at the completion of that action, we can see that the optimal price is as at a different level, showcasing the enduring uncertainty throughout the
duration of the chosen real effort task.\textsuperscript{19}

Figure 8: Price history (green line), price updating task decisions (blue dots), and baking task decisions (red dots) for a selected set of subjects. Each row represents a subject, columns are the treatments from T1 on the left to T4 on the right, the numeration reflects the sequence of the treatments for the subject, and the dashed horizontal lines show the optimal thresholds. Instantaneous profits are on the vertical axis and seconds on the horizontal one.

The characteristics of these plots suggest a simple approach to estimating the individual thresholds. First, we estimate the kernel density distributions of the two task decisions by using a normal kernel (that is, of the blue and the red squares in Figure 8) and compute the

\textsuperscript{19}For comparison with Figure 8 and the companion plots for the entire set of subjects in Section ?? the online Appendix, Figure A2 in Appendix D reports the histories of profit realizations and decisions made by seven virtual subjects under optimal thresholds simulation. Although the thresholds are obviously always respected by these virtual subjects, this Figure confirms the volatility of profits at the moment of the price updating decisions that reflect the lag between the beginning of a task and its completion.
respective cumulative distribution functions.⁴⁰ The survivor function of the baking decisions (defined as $1 - CDF$) would typically be greater than that of the price updating decision on the entire support. This can be interpreted as a sort of stochastic dominance which reflects the fact the density distribution of baking is shifted to the right of the $[0, 1]$ support of profit realizations relative to the updating distribution, but it does not exclude the possibility of overlapping of the two distributions. We illustrate this point in general terms for the aggregate set of decisions across subjects in Figures 9 and 10. At the level of individual subjects the same result typically holds, with the exception of the few cases in which a subject never updates during the experiment or updates only once. For these cases, the density of the updating decision degenerates, and we attribute the threshold simply to that single observation.

Finally, the threshold is estimated as the profit level that maximizes the distance between these two $CDF$s, which basically corresponds to the point where the two densities switch relative position and updating becomes more likely than baking (coming from the right hand side of the support). We report the thresholds computed based on the aggregate observations for the four treatments in Figure 11. It is important to note that the thresholds are indeed following the relative ordering found in the simulation, with the lowest threshold for the last treatment and similar thresholds for the intermediate treatments. Nevertheless, as discussed below, the subjects seem to perceive their thresholds above the simulated ones, uniformly across treatments.

Turning to the analysis at individual level, we observe a higher degree of heterogeneity across subjects. The relative ordering of the thresholds is not often exactly satisfied by the individual subjects and the distribution of thresholds is quite spread over the profit support as illustrated in Figure 12. As an illustrative example, Figure A3 and Figure A4 in Appendix D report the survivor functions and the estimated thresholds for the same set of

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⁴⁰Equivalent results are obtained when other popular kernel functions are adopted.
individual subjects for which Figure 8 is constructed.

We use the data of the optimal simulations to assess the validity of our approach to infer the thresholds from the distribution of the updating and baking decisions. The results of the assessment are reported in Section ?? of the online Appendix. The assessment confirms that our empirical strategy correctly recovers the thresholds used by the subjects in the experiment.

Figure 9: Empirical kernel distribution of the decisions of price updating and baking tasks. Gaussian kernel function for the aggregate set of decisions across subjects.

5.3 Experimental Results

We now turn to observed subject behavior. First, we examine the thresholds subjects use. Figure 11 shows histograms for the average decision threshold for all subjects in each treatment. The mean thresholds are (0.70, 0.57, 0.57, 0.38).\textsuperscript{21} All of these are highly statis-

\begin{footnote}
\textsuperscript{21}We test for the within-subject design by subtracting the average number of the relevant measured values for individual $i$ in Treatment $j$ to $i$’s average measured value in Treatment $k$. Then the average of those
\end{footnote}
Figure 10: Survivor functions (1-CDF) of the two types of decisions. The dashed vertical lines correspond to the simulated optimal thresholds; the solid vertical lines are the estimated thresholds implied by the empirical distributions of the two task decisions. Aggregate set of decisions across subjects.

Figure 11: Estimated thresholds in the four treatments; aggregate set of decisions across subjects.
As can be seen in Table 2, we find that in all four treatments, the average decision thresholds used in the experiment are in all cases substantially higher than those determined to be optimal by the simulations. Figure 12 shows a histogram of the subjects’ decision thresholds in each of the treatments. The vertical red line indicates the simulated optimal threshold. As can be seen, the mass of the distribution is well above the simulated optimal.

Figure 12: Dispersion of the individual estimated thresholds by treatment. Vertical dashed lines correspond to the simulated optimal thresholds.

differently different from one another ($p < 0.001$) except for when comparing Treatments II and III.

As can be seen in Table 2, we find that in all four treatments, the average decision thresholds used in the experiment are in all cases substantially higher than those determined to be optimal by the simulations. Figure 12 shows a histogram of the subjects’ decision thresholds in each of the treatments. The vertical red line indicates the simulated optimal threshold. As can be seen, the mass of the distribution is well above the simulated optimal.\textsuperscript{22}

\textsuperscript{22}We would ideally test for statistical difference in the variance of these distributions to test the hypothesis that more volatile underlying economic fundamentals would result in a greater variance in the individual differences was taken for all individuals, and a paired $t$-test was performed to test whether or not the mean of that difference vector was different from zero.
Table 2: Summary of pertinent data collected from the experiment and the simulation

<table>
<thead>
<tr>
<th></th>
<th>Treatment I</th>
<th>Treatment II</th>
<th>Treatment III</th>
<th>Treatment IV</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Experiment (means)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Price Updates</td>
<td>3.4</td>
<td>4.0</td>
<td>3.9</td>
<td>4.8</td>
</tr>
<tr>
<td>Number of Pizzas Baked</td>
<td>24.7</td>
<td>22.5</td>
<td>22.9</td>
<td>20.7</td>
</tr>
<tr>
<td>Decision Threshold [ELD]</td>
<td>0.70</td>
<td>0.57</td>
<td>0.57</td>
<td>0.38</td>
</tr>
<tr>
<td>Total Profits [ELD]</td>
<td>17.4</td>
<td>13.3</td>
<td>13.1</td>
<td>8.8</td>
</tr>
<tr>
<td><strong>Simulation (means)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of Price Updates</td>
<td>2.9</td>
<td>4.3</td>
<td>4.3</td>
<td>5.3</td>
</tr>
<tr>
<td>Number of Pizzas Baked</td>
<td>29</td>
<td>27.7</td>
<td>27.7</td>
<td>26.7</td>
</tr>
<tr>
<td>Decision Threshold [ELD]</td>
<td>0.52</td>
<td>0.38</td>
<td>0.38</td>
<td>0.20</td>
</tr>
<tr>
<td>Total Profits [ELD]</td>
<td>21.4</td>
<td>16.8</td>
<td>16.8</td>
<td>11.6</td>
</tr>
</tbody>
</table>

We find the average number of updates per treatments to be (3.4, 4, 3.9, 4.8). Relative to each other, these averages follow the predictions of our stylized model: Treatment I (slow price movement, small profit falloff) has fewer price updates, while Treatment IV (fast price movement, large profit falloff) has more price updates, with Treatments II and III being less extreme. Statistically, we find that there is significant difference ($p < 0.001$) between any two treatments except for Treatments II and III, which do not differ statistically. We find the mean number of pizzas baked to be (24.7, 22.5, 22.9, 20.7). Again, we find that these averages are statistically different to a very high degree of significance ($p < 0.001$) in all cases except for the comparison of Treatments II and III, which do not differ statistically.

Even though we find that subjects have a higher decision threshold in each of the treatments compared to the simulated optimal, the average number of updates in each treatment of the the experiment (3.4, 4.0, 3.9, 4.8) is quite similar to the number of price updates thresholds, but there are some limitations in our case. An $F$-test was performed, and the results showed that there were no statistical difference between any of the given treatments’ variances. This should be heavily caveated with the fact that the $F$-test is highly sensitive to the assumption that the data follow a Gaussian distribution. Moreover, it should also be emphasized that the $F$-test requires that observations be independent. Since each subject in our experiment experienced each of the four treatments, this assumption is not met.
per treatment in the simulations (2.9, 4.3, 4.3, 5.3). At the same time, as can be seen in Figure 13, we find that the subjects bake statistically significantly fewer pizzas relative to what the suggested optimal would be in each of the four treatments (24.7, 22.5, 22.9, 20.7 compared to the 29, 27.7, 27.7, 26.7 as implied as optimal by the simulations). As a consequence, the total number of tasks performed by the subjects falls several units short of the total number of tasks completed in the simulations. On average, subjects perform approximately 4 to 6 fewer tasks in each treatment. Finally, considering the fact that the subjects can complete a smaller number of tasks relative to the simulated optimal, it is not surprising that we find that subjects earned statistically significantly less total profits than the simulations (Figure 13). On average, we find that subjects earned (17.40, 13.30, 13.10, 8.80) in ELD. However, the total profits earned vary as predicted by the stylized model from one treatment to the next and these averages are statistically different to a very high degree of significance ($p < 0.001$) in all cases except for when we compare Treatments II and III, which do not differ statistically. Again, all of these data are summarized in Table 2.

Two main effects can explain this set of results. First of all, given the total number of tasks completed, the price updating decisions are relatively more frequent than expected. This reflects the higher inaction thresholds identified by the subjects, which induce people to update “too early” in state and more often than optimal. The implicit trade-off cost of price updating affects the subjects’ perception of the actual value of an updating decision, causing them to overestimate the benefits from updating. In other words, subjects do not realize the correct cost in terms of profit losses they face when updating and seem to put more importance on producing at higher rates per pizza, thus hastening the price adjustment. This result can be interpreted in light of the large experimental literature that show that increased cognitive loads make people more impatient and lead to poorer economic decisions (see Deck and Jahedi, 2015, for a more complete survey of this literature).
Second, the subjects are not as efficient at making decisions as machines, and they will naturally complete a smaller number of total tasks. However, their efficiency is also affected by the uncertainty of the environment as the increasing gap between simulated total tasks performed and actual tasks shows (this gap goes from 4 for Treatment I to 6.5 for Treatment IV). Magnani et al. (2015) document a large dispersion of subjects' actions around optimal thresholds, which increases in the volatility of the environment. In their setup the adjustment cost is known and the average thresholds are quite close to the optimal ones. We also find a large heterogeneity in the thresholds across individuals, but allowing for implicit adjustment cost we illustrate a limited ability of the subjects to correctly identify the thresholds. Furthermore, the mistake they make in assessing the thresholds is quite uniform across treatments and depends only marginally on the uncertainty of the economic environment. The new feature we introduce in our experimental design, namely the trade-off between production and price setting tasks, has a direct negative impact on the cognitive capacity of the subjects. The implicit cost of adjustment forces subjects to infer their cost of resetting prices in terms of forgone profits while they accomplish the updating task, heightening their cognitive cost. Similarly, the trade-off adds a real source of distraction for the subjects that diverts their attention from screening the state of the economy to executing the production task. While it is hard to precisely disentangle these two channels, they both operate in the same direction by increasing the likelihood of optimization errors even in the baseline treatment.

5.4 The Empirical Inaction Regions

A person who is following a state dependent strategy, should always update the price when the instantaneous profit is below the threshold value (the action region) and should never update the price when instantaneous profits exceed the threshold value (the inaction region), as shown in Figure 1. For each subject in each treatment, we have already empirically
Figure 13: Experimentally-determined Vs simulation-determined average number of baked pizzas per treatment (top), number of price updates (center), and total profits (bottom). Vertical bars show two standard deviations from the mean and are only suitable for comparison of means within treatment (but not across treatments).
identified the implied threshold value. Figure 14 examines how much variability each subject exhibits when making choices in their respective action and inaction regions. Specifically, the vertical axis indicates the frequency with which someone updated the price when the instantaneous profit indicates they were in the action region—that is, the vertical coordinate indicates the percentage of times a person updated their price when they should have updated it. The horizontal axis indicates the frequency with which someone updated their price when the instantaneous profit placed them in the inaction region—that is, the horizontal coordinate indicates the percentage of times a person updated their price when they should not have updated it. A person who perfectly follows a state dependent strategy would appear at the top left of Figure 14, while someone who is equally likely to update their price when they are in the action or inaction region would appear on the 45 degree line.

From Figure 14 it is clear that subjects generally update more frequently when they are in the action region than when they are in the inaction region as almost every point lies above the 45 degree line in all four plots. In fact, many subjects never update when they are in the inaction region as indicated by the concentration of points along the vertical axis. However, when subjects are in the action region with low instantaneous profits they frequently do not update even though they should. Overall, Figure 14 suggests that subjects behave as if they use a state dependent rule but have difficulty identifying and implementing a stark threshold. Also this result can be linked to the effects of cognitive load and bounded rationality that the subjects face while inferring the optimal thresholds under the endogenous opportunity cost of updating prices.

5.5 Time Dependence Analysis

The degree to which our subjects exhibited state-dependent or time-dependent behavior is of particular importance. We assess the presence of time-dependent elements in subjects’ decisions in two ways. The first way is by looking at the hazard function of price updates.
We construct the hazard function computing the probability that an individual will decide to perform a price updating task after $t$ periods, conditional on not having changed price for $t$ periods. In order to construct these hazard rates, we first compute the duration of price spells since the most recent updating task for all subjects in a treatment, and we sort them in one-minute intervals. The last interval is not bounded in order to accommodate the non-predetermined end of the experiment for each subject. The hazard rates are then defined as the ratio of updating decisions in an interval to the total number of updating decisions made in that and all the subsequent intervals.

As discussed in Nakamura and Steinsson (2007), the shape of the hazard function depends on the underlying pricing strategy followed by the price setters. In a menu cost model, the slope of the hazard function is related to the persistence of the shocks to the marginal cost. An upward sloping hazard function corresponds to non-stationary marginal cost, whereas less persistent shocks determine flatter (or even partially downward sloping) hazard rates. On the
contrary, a flat hazard function reflects a constant probability of updating in every period and would correspond to Calvo pricing. The analysis starts from the hazard functions obtained from the simulated data, which we compare to the empirical hazards based on the data of the experiment. Figure 15 illustrates that the simulation-based hazard functions are indeed upward sloping, as typically expected for this type of state dependent model. Although similar in magnitude to the theoretical ones, the empirical hazard rates reported in Figure 16 definitely exhibit more irregular shapes, with essentially flat profiles for Treatment III and IV and large drops for some of the rates in the other two treatments. Moreover, contrary to the theoretical hazards, there are no observations for the longest durations in Treatment II to IV. This result reveals the existence of clear deviations of the subjects’ decisions from the benchmark state dependent model and echoes the results in favor of time dependence found by Magnani et al. (2015) as well.

We further refine the empirical hazard measures by considering the updating decisions relative to the inaction threshold of each individual. This allows us to separately study the updating decisions made inside and outside the estimated action region of an individual, controlling for potential differences in state dependence for those actions that may occur earlier if done inside the inaction region. Similarly, using individual thresholds helps us eliminate the “survivor bias” that may arise when mixing flat individual hazards for subject with heterogeneous thresholds, as documented by Klenow and Kryvtsov (2008).23

Figure 17 reports the adjusted hazard functions for the four treatments of the experiment. The conclusions analyzed above are confirmed by the hazard functions of the decisions made inside the action regions, and time dependence does not seem to be determined by heterogeneity across subjects in our experiment. Furthermore, the hazard rates of the decisions made outside the action regions are downward sloping and, as expected, they show

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23 The survivor bias in this case would reflect the fact that individuals with lower subjective thresholds would take on average longer time to reach their action region compared to those with higher threshold values, causing a falling aggregate hazard function even when all individual hazards are flat.
that these actions correspond to shorter durations. Subjects who update inside the inaction region are likely also more impatient. Finally, time dependence in the hazard functions is not particularly affected by high volatility treatments (the two treatments on the right hand side column of these Figures). Magnani et al. (2015) note that time dependence “increases substantially” when the volatility of the underlying economy is increased, but this result is not corroborated by our study.

For the second part of this time dependence analysis, we study the ratio of updating decisions to the total number of decisions made at each point in time in function of the time elapsed since the last price update occurred. This function represents how the probability of making errors in performing a price updating changes over time. As for the hazard analysis, we sort the observations by one-minute intervals and we separate them between action and inaction regions based on the individual threshold estimates. If price setters are strictly state dependent, the function would be equal to one and completely flat inside the action region and zero outside the action region. Allowing for uncertainty and errors in the identification of the thresholds, we would expect an essentially flat shape in the two regions (although the function would not be equal one or zero anymore) if time does not have a systematic bearing on the subjects’ errors in making decision to update prices.

Figure 18 reports these error probability functions for the four treatments of the experiment, which strongly confirm the presence of time dependent traits in the behavior of the subjects of the experiment. The functions are clearly downward sloping in every treatment for the decisions in the action region. On the contrary, in the inaction region, the functions start with an initial spike in the first minute and then become quite small and flat in the following intervals, which is more in line with expectations under a state dependent strategy. These results suggest two interesting observations on the types of errors made by the subjects. First, the initial spikes in the functions outside the action region indicate that some subjects are underestimating their cost of updating, and this leads them to update prices
Figure 15: Hazard plots for each of the four treatments – simulated data.

Figure 16: Hazard plots for each of the four treatments – experimental data.
too “early” with respect to the state, when the deviation from the optimal price is not that large yet. As illustrated by the profits diffusion plots in Figure ?? of the online Appendix, the levels of profits for which this may occur are more likely observed at shorter durations. Second, the low persistence of the error functions inside the updating region represented by the steep negative slopes of the functions can be interpreted as evidence of habit formation in baking. Subjects become less likely to perform updating tasks as the price spells get longer, which will require subjects to be engaged in the production activity for longer durations. A prolonged focus on a series of baking tasks diverts the attention of the subjects from updating, and reduces the ability of the subjects to recognize the need of a price change even after entering the action region. This mechanism would be consistent with the view that the production task operates as a distracting factor, which increases the cognitive load of the subjects and the likelihood of mistakes in the updating process.

In conclusion, the evidence of time dependence elements in the updating rules of the subjects presented in this section are broadly consistent with the results of Magnani et al.
However, we find that these elements are mostly related to the cognitive load mechanism introduced by our experimental design, rather than to the underlying volatility of the economic environment. In relation to the empirical data from the field, our hazard functions have shapes more similar to the flat hazard rates computed by Klenow and Kryvtsov (2008) than the downward sloping hazard functions estimated by Nakamura and Steinsson (2007). This type of hazard function would primarily point to models in which the probability of updating is constant over time, such as the Calvo pricing, and therefore completely independent of the state of the economy. In this respect, our experiment suggests that, although this type of hazard function does not reflect a purely state dependent pricing rule, it might still come from subjects who recognize the validity of a state dependent approach to some extent, but do not always successfully follow it because they experience attention diversion or limited information processing capacity.
6 Conclusion

The issue of price setting has been an area of intense macroeconomic research for decades. With the abundance of possible explanations for observing macroeconomic price rigidity and the emergence of the use of laboratory testing to evaluate the validity and applicability of theoretical models, a logical endeavor is to evaluate the efficacy of these price setting models in a laboratory setting. This paper uses an experiment to understand if individuals behave in accordance with a strictly state-dependent pricing framework. We do not expect that subject behavior will perfectly match such a theoretical model. Rather, our objective is to identify how well an Ss model captures the decision rules that people actually use and how their responses vary with changes in the environment.

Our work extends Magnani et al. (2015) by internalizing the costs incurred by subjects to update their prices and thereby introducing an intrinsic element of additional cognitive demand on the subjects. In this richer environment, we find that in general subjects behave as if they recognize the importance of a state dependent pricing strategy, but they identify noisy inaction regions and they also exhibit a substantial degree of time dependence. As a result, subjects adopt decision thresholds higher than optimal and they end up updating relatively too often instead of engaging in production as compared to the simulated optimal behavior.

Our results are consistent with the conclusions of Magnani et al. (2015) and their explanation for time dependence based on cognitive or attention costs. However, we also highlight a second source of errors related to bounded rationality, which has a very large impact on the pricing decision of our subjects. When the updating cost is not imposed exogenously, people will tend to underestimate the actual cost they face to update and they systematically identify higher thresholds even in low uncertainty environments. The introduction of the real trade-off between production and price updating in our experimental design allows
us to further explore the link between attention and cognitive load in price setting on one side and some of the resulting errors on the other. We make a few interesting observations about this point.

The first is that the literature on the negative effects of cognitive load on the quality of economic decisions provides a possible interpretation of the main result. It has been shown that an increased cognitive load makes people more impatient; in our framework, early updating can be seen as the effect of impatience on subjects’ decisions. As a second point, our experimental design introduces a direct and simple micro-foundation of a source of distraction of the subjects’ attention through the production activity. It would be relatively easy to design modifications of this framework and study how pricing errors respond to changes in the type of cognitive cost in order to assess different theories of attention. For instance, the evidence of deviations from optimal price setting due to subjects’ limited ability to fully analyze the underlying economic conditions could possibly lend credence to the strand of models that focus on firms’ information processing bandwidth or even “rational inattention.” Specifically, such a model would have to entail explicit decisions about attention allocation across signals of different precisions, for example associated to updating tasks of different lengths, to infer the underlying marginal cost of production; one of the testable predictions of the model would be that the precision of the selected signal should increase in response to higher returns of an incoming order.

Related to the discussion of possible modifications of our design, the third observation we make is a caveat on the mechanism implemented by our approach. While our experimental design endogenizes the opportunity cost of updating prices, the updated price was always set optimally. In practice, if sellers are not given the optimal prices and have to invest effort to estimate the optimal price level, then the cognitive costs associated with updating prices would be relatively greater than in our design. This increased cost, coupled with the risk of earning sub-optimal profits as a result of an error in setting prices optimally, would likely
lead sellers to update prices less frequently than what we observe.

The last observation is that our research also provides some evidence on the behavioral support of different theoretical price models as well as of the empirical evidence obtained from field data on price changes. By directly testing subjects behavior in a state dependent environment, the first conclusion is that strictly state dependent models are not sufficient to represent pricing decisions; a relatively significant time dependent component emerges as well. From the flat hazard functions found for the experimental data, we can conclude that simpler staggered price models as in the Calvo pricing might still represent a valid alternative to other models to capture these deviations from full state dependence. Klenow and Kryvtsov (2008) find that flat hazard rates are typical with field data as well. Finally, as just discussed above, our entire analysis is impregnated by the relevance of cognitive and attention costs for price updating decisions and it calls for further, promising exploration in this direction.
7 References


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Appendix

A Instructions

Introduction

You are about to participate in an experiment in the economics of decision-making. If you follow these instructions carefully and make good decisions, you can earn a considerable amount of money, which will be paid to you in cash at the end of the experiment.

Your computer screen will display useful information. Remember that the information on your computer screen is private. To ensure best results for yourself and accurate data for the experimenters, please DO NOT COMMUNICATE with the other participants at any point during the experiment. If you have any questions, or need assistance of any kind, raise your hand and one of the experimenters will come to you.

In the experiment you will make decisions over four periods. At the end of the last period, you will be paid $5, plus earnings based on your profits earned in each period.

Task Details

You are in control of a company that bakes and sells pizzas at a Current Set Price. Your job is to fulfill incoming orders by assembling pizzas with the correct ingredients. When submitting an order, you will earn profits based on your Current Set Price and the Optimal Price. However, market conditions are constantly changing, so the Optimal Price is also constantly changing.

The further your Current Set Price is away from the Optimal Price (either too high or too low), the less money you make when submitting an order correctly. At any moment (and as often as you like) you can change your Current Set Price to the Optimal Price; but in order to do this, you must enter a code to update your price. After doing so, the Optimal
Price will continue to fluctuate just as before, possibly moving away from the Current Set Price again.

**Panel Details: Outstanding Order**

In the upper left-hand corner of your screen, you will find a panel that looks like the one above. This is where orders come into your pizzeria. In order to fulfill an order, you must select the button next to the ingredient that is in each box at the top of each column. Each column is populated with ten possible ingredients, and the ordering of each of these columns is random for every order. After you have correctly selected each ingredient, you will click the "Submit Order" button. Doing this will earn you profits and will bring in the next order. The more orders you complete correctly, the more profits you earn.

It is important to note that only one ingredient may be selected at a time. If you submit an order with more than one ingredient selected in a given column, you will not receive any profits, and you will have to try again.

**Panel Details: Optimal Price**

Remember that the market conditions are constantly changing, which means that the Optimal Price for your pizzas is also changing. The Optimal Price changes randomly 5 times per second. In the lower-left, you will find a panel that shows you the current Optimal Price, like the one in the image above. The green line indicates the Optimal Price, while the red line indicates your Current Set Price.

**Panel Details: Update Code**

Remember the further the Optimal Price deviates from your Current Set Price, the lower your profits per order. At any point you can update your Current Set Price. In the upper right-hand panel of your screen, you will see a 5-digit code that must be entered in order to
update your Current Set Price. Simply select the button next to the appropriate number in each column to match the number at the top of that column and click the "Update Price" button, and then your Current Set Price will equal the Optimal Price at that moment, thereby maximizing your profit per order. But remember: The Optimal Price is always moving, so it may be the case that shortly after updating your Current Set Price, your profit per order starts to change.

Again, only one number in each column may be selected in order to enter the code correctly. Also, the ordering of each column is randomized each time a new code is generated.

Panel Details: Profit Information

The fourth and final panel on your screen will give you information regarding your profits. On the left, you will find a field called "Instantaneous Profits." The number in this box will update in realtime, and it indicates the amount of profit you will earn if you were to submit an order at that exact moment. On the right you will see a box called "Cumulative Profits." This box will display the total profits you have earned throughout the period.

Period Details

There will be 4 paid periods. Each period’s length is determined randomly, but on average each one is approximately 5 minutes in length. There will be a 1 minute break between each period. Ten seconds before the next period begins, your screen will turn red. Each period will be similar, but can differ in two ways: (1) The movement of the Optimal Price over time can change; and (2) How instantaneous profit depends on the difference between the Optimal Price and the Current Set Price can change.

If you want to go back and get instructions, you may do so now by clicking the red "Back" button below. If you are comfortable with the layout of the experiment, you may proceed to a 2-minute practice round by clicking the blue "Start" button below. This practice round

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will have no impact on your total profit and it is intended entirely as a means by which you may familiarize yourself with the operations of the experimental interface.

Ready To Begin?

If you have any questions regarding the experiment, raise your hand now, and an experimenter will come to you and answer them.

If you are ready to begin, please raise your hand, and an experimenter will bring you a short comprehension quiz to ensure that you fully understand the experiment. Once you have successfully completed the quiz, an experimenter will enter the password to allow you to begin the experiment. Note that once the experiment begins, you cannot take a break.
B Comprehension Quiz

This will not affect your payoff in any way. It is designed to make sure that you understand the experiment before it begins. Please answer the following questions and then raise your hand so that a researcher can come check your answers. If you have a question at any point, please raise your hand and a researcher will approach you.

1. The only way you make money in this experiment is by making a pizza with the correct toppings.
   (a) True
   (b) False

2. How much money you earn making a pizza depends on
   (a) How long ago you updated your Current Set Price.
   (b) The Optimal Price when you last updated your Current Set Price.
   (c) The difference between your Current Set Price and the Optimal Price when you click Submit Order.

3. Your Current Set Price will adjust
   (a) Automatically every 0.2 seconds.
   (b) When you correctly enter the Update Price code.
   (c) Never

4. Each period will last
   (a) Exactly 5 minutes.
   (b) A random amount of time that on average will be about 5 minutes.
5. From period to period

(a) The Optimal Price will change at the same rate.

(b) The reduction in profit per pizza as Optimal Price and your Current Set Price drift apart is the same.

(c) Both (a) and (b) are correct.

(d) Neither (a) nor (b) is correct.
C Simulation Pseudo-code

Using the figure above, we will walk through a simple example of how the logic flows for one simulation.

1. The code initializes (point \( a \) in Figure A1) knowing that at point \( a \) the *Current Set Price* is equal to the *Optimal Price*. Because of this, it logically knows that it should bake a pizza.

2. The code then steps 10 seconds into the future (which corresponds to point \( b \) in Figure A1) because it takes time to bake the pizzas, the instantaneous profits earned by baking it are computed at this point.

3. The code then looks at the instantaneous profits available to it at point \( b \), and:

   (a) If the instantaneous profits are below the optimal decision threshold, then the
code would set the *Current Set Price* to the value of the *Optimal Price* at point $b$.

(b) Else, the code decides to bake another pizza.

4. Regardless of the outcome of the previous step, the code then moves 10 seconds along the optimal price vector to point $c$ in Figure A1, and

(a) If the previous action was a price update, then the code performs Step 3 again.

(b) Else, the previous action was the baking of a pizza, then the code computes the instantaneous profits for baking the pizza, and earns the value for point $c$.

5. The code then cycles through decision tree represented in Steps 3 and 4 for each point along an price vector that simulates 5 minutes.

This entire process is *one* simulation. The process then ran for 10,000 simulations.
D Additional Figures

We present here some additional Figures mentioned in the main body of the paper.

Figure A2: Price history (green line), price updating task decisions (blue dots), and baking task decisions (red dots) for a set of seven “subjects” from the optimal simulation. Each row represents a subject, columns are the treatments from T1 on the left to T4 on the right, and the dashed horizontal lines show the optimal thresholds. Instantaneous profits are on the vertical axis and seconds on the horizontal one.
Figure A3: Survivor functions (1-CDF) of the two types of decisions for the same subjects in Figure 8 with instantaneous profits on the vertical axis. See caption of that figure for details.

Figure A4: Estimated thresholds in the four treatments for the same subjects in Figure 8. Each panel represents a subject, bars correspond to the four treatments, and the numeration reflects the sequence of the treatments for the subject. Instantaneous profits are on the vertical axis and treatments on the horizontal one.
MEMORANDUM

TO: Justin LeBlanc  
    Cary Deck  
    Andrea Civelli  
    Klajdi Bregu

FROM: Ro Windwalker  
       IRB Coordinator

RE: New Protocol Approval

IRB Protocol #: 14-04-691
Protocol Title: Price Rigidity
Review Type: ☑ EXEMPT ☐ EXPEDITED ☐ FULL IRB

Approved Project Period: Start Date: 04/29/2014  Expiration Date: 04/28/2015

Your protocol has been approved by the IRB. Protocols are approved for a maximum period of one year. If you wish to continue the project past the approved project period (see above), you must submit a request, using the form Continuing Review for IRB Approved Projects, prior to the expiration date. This form is available from the IRB Coordinator or on the Research Compliance website (http://vpred.uark.edu/210.php). As a courtesy, you will be sent a reminder two months in advance of that date. However, failure to receive a reminder does not negate your obligation to make the request in sufficient time for review and approval. Federal regulations prohibit retroactive approval of continuation. Failure to receive approval to continue the project prior to the expiration date will result in Termination of the protocol approval. The IRB Coordinator can give you guidance on submission times.

This protocol has been approved for 120 participants. If you wish to make any modifications in the approved protocol, including enrolling more than this number, you must seek approval prior to implementing those changes. All modifications should be requested in writing (email is acceptable) and must provide sufficient detail to assess the impact of the change.

If you have questions or need any assistance from the IRB, please contact me at 210 Administration Building, 5-2208, or irb@uark.edu.
Chapter 2: Rationally Inattentive Consumer: An Experiment

1 Introduction

Cognitive limits in processing information have important implications in a variety of economic settings. Theoretical studies on stochastic choices (Matějka and McKay, 2015), investment decisions (see, among others, Mondria, 2010) and pricing decisions (Mackowiak et al., 2009), as well as empirical studies on consumers’ choices, financial markets and voting behavior (examples include DellaVigna and Pollet, 2007; Shue and Luttmer, 2009) highlight the role people’s limitations to paying attention to changes in the economic environment, even if these changes are relevant for their decisions. While there are several approaches to information acquisition (see Hellwig et al., 2012, for a survey), this paper is concerned with the theory of rational inattention pioneered by Sims (1998) and Sims (2003). The goal of the paper is to formally test the principles and implication of this theory for individual consumption decisions by using a laboratory experiment.

Sims postulates a model in which a decision-maker chooses optimally the amount of information for a given decision problem where the cost is based on Shannon’s mutual information between prior and posterior beliefs. Following this approach, Tutino (2013) builds a consumption-saving decision problem where consumers, aware of their cognitive limitations, change the amount of information and attention in response to changes in the economic environment. Tutino (2013) explicitly considers the cost implied by Shannon’s mutual information as a cognitive cost, i.e. a cost that captures the limits of people to map quickly and precisely all the available information about the economic environment into consumption choices. Moreover, the paper shows that for a given shadow cost of processing information, consumers react to exogenous economic changes by varying the informativeness of the signals.

1See, for instance, Chetty et al. (2009) for an application to sales taxes and de los Santos et al. (2012) for an application to e-commerce.
they require to select consumption choices.

This concept, known as *elastic* information processing capacity, implies that different economic stimuli correspond to different consumption behaviors, according to whether the decision-maker deems it beneficial to save processing effort by accepting lower consumption or she prefers to incur the cost of paying additional attention to increase the informativeness of signals and make sharper consumption choices. Tutino (2013) shows that taking into account cognitive limitations accounts for a number of empirical regularities in the consumption literature and, more importantly, documents novel findings pertaining to rational inattention theory on randomness of choices and asymmetric responses to economic changes that are hard to reconcile with standard choice theories.

This paper formally tests the predictions of a static version of the model in Tutino (2013) in a controlled laboratory experiment. In the basic task, a simple decision problem is faced by the participants, who make consumption decisions under income uncertainty. In each period, income is randomly drawn from a uniform distribution. Prior to selecting a consumption level, subjects can reduce the uncertainty about their income by acquiring signals that inform them of how much income they may have available for the period.

The precision of the signal is directly proportional to the cognitive effort participants need to exercise in order to extract information. We capture cognitive effort by requiring participants to solve logic puzzles, with more difficult puzzles demanding more effort to be solved by the subject. Since rational inattention is at its core a theory of how hard people think, the logical puzzles allow us to measure the effort participants choose. We relate the difficulty of the puzzles to the informativeness of the signals, with harder puzzles corresponding to more precise information about income (Civelli and Deck, 2018). Difficulties of the puzzles range from trivial (uninformative) to hardest (perfectly revealing). Upon completion of a task, participants update their prior beliefs about income and select their desired consumption level based on their informed posterior.
If the selected consumption level does not exceed the income drawn that period, the subject obtains the selected consumption level, otherwise the subject moves on to the next period with zero consumption. Participants are paid on the basis of their accumulated consumption over the course of the experiment. Other than the opportunity cost of processing information versus consumption, there is no time limit on any given period.\(^2\) By repeatedly exposing the subjects to each decision problem we can collect data on consumption choices as well as information/signal attempted and realized choices. These data are relevant in assessing whether the predictions of the rational inattention model are verified (see Caplin and Dean, 2015).

We focus on five main specific predictions from Tutino (2013) and design experimental treatments conducive to directly verifying whether laboratory evidence corroborates these predictions. The first two predictions we test concern the value of information and stochastic choices of consumption decisions. Consistent with the theory, the behavioral evidence reveals that, while on average a more informed subject receives a higher payoff, subjects often prefer to exercise low effort and process less information even if that choice implies lower consumption rewards. Moreover, unlike standard choice theories, we verify the prediction of randomness in choices by documenting stochastic behavior, with consumption outcomes generating a non-spurious posterior distribution.\(^3\)

The third prediction we verify concerns whether subjects respond to monetary incentives by varying their processing effort. To test this prediction, we modify our baseline experiment by varying the payoffs offered at specific intervals. In particular, we inform the subject that in this treatment the payoff will substantially increase every eleven periods while in other periods consumption values are a fraction of those in the baseline. We find that participants

\(^2\)The overall length of the task was random with an expected duration of 20 minutes. Subjects who neither acquired information nor consumed successfully experienced a 60 second reduction in the allotted task time as a penalty.

\(^3\)For other examples of this result in experimental data connected to rational inattention, see Dean and Neligh (2017), Khaw et al. (2016), Cheremukhin et al. (2015).
take advantage of the higher payoff by processing more information in periods where the consumption possibility are more sizable and reduce their cognitive effort with respect to the baseline treatment when consumption offers are small. Thus, unlike random utility model where attention is fixed, we document that subjects respond to incentives by varying information processing efforts when presented with lucrative alternatives. This finding is consistent with the notion of elastic capacity where individuals modulates their attention and cognitive effort according to what is at stake.

The fourth prediction of the theory posits that individuals tend to process less information and make less precise consumption decisions when the economic environment becomes more predictable. We test this prediction by implementing a treatment where the income process has persistence. In such an environment, we show that subjects change their consumption choice and signal precision infrequently. Generally, subjects fail to realize changes in their income possibilities as they process less information about income than they do in the baseline. In particular, we document that participants resolve the trade-off between cost of processing more precise signals with the benefit of higher payoffs by forgoing units of consumption rather than acquiring better information.

The fifth theoretical prediction we put to the empirical test is the asymmetric response of consumers to shocks, with negative shocks triggering faster and more sizable consumption reaction than positive shock. This prediction is a novel finding in Tutino (2013) which makes rational inattention theory observationally distinct from other theories of costly information acquisitions and attentiveness. We verify this prediction by feeding shocks of different size and sign to the predictable environment described for the third prediction. We found corroborating evidence to the finding of asymmetric consumption responses in the direction predicted by the theory. This finding is also supported by a host of empirical results on consumption demand (see, for instance, Abaluck and Adams, 2017; Shea, 2009), and the impact of taxation (Broda and Parker, 2014; Johnson et al., 2006).
This paper contributes to the literature concerning stochastic choices under uncertainty and to the recent and growing literature aimed at measuring the role of information acquisition in rationalizing economic outcomes. Recent studies test different models of attention and information acquisition (Khaw et al., 2016; Gabaix et al., 2006; Manzini and Mariotti, 2015). Within this literature, a subset of papers (see Pinkofskiy, 2009; Cheremukhin et al., 2015), experimentally test models of rational inattention using binary choices between gambles. Unlike these papers, our model is designed to directly quantify participants’ choices of cognitive effort by employing logic puzzles and to relate the effort to consumption choices. A closely related paper that experimentally checks the predictions of rational inattention against experimental data with perceptual tests is Dean and Neligh (2017). We differ from their approach in that we design the experimental setting to test a particular set of predictions of the rational inattention theory with respect to consumption behavior. This paper corroborates the empirical tests of Dean and Neligh (2017) regarding incentives and randomness in rational inattention models. Moreover, we complement their results with the empirical findings of delayed and asymmetric responses of consumption and attention to stimuli. To our knowledge, this is the first paper that explicitly test for asymmetry in response to shocks of rationally inattentive agents.

The rest of the paper is organized as follows. Section 2 formally presents the theoretical consumption decision problem under rational inattention. It discusses the properties of the problem’s solution and the testable predictions. Section 3 lays out the experimental setting and the treatments implemented to verify the theoretical prediction. A mapping between experimental set-up and the theoretical model is formally established. Section 4 shows the congruence of the experimental with the theoretical predictions. Finally, Section 5 offers some concluding remarks. Robustness checks are relegated to an Appendix.
2 Theoretical Framework

We briefly introduce the theoretical framework to provide a structure for understanding the consumption and attention allocation decisions faced by subjects in the experiment. Our experimental design implements a static version of the theoretical model by Tutino (2013). The model derives testable predictions of rationally inattentive agents’ behavioral responses to changes in the economic environment during their lifetime. We use this model as a guide to discipline our experiment and to contrast the theoretical predictions with the laboratory evidence.

The model describes an optimization problem in which an information processing capacity constraint is added to an otherwise standard consumption-saving problem. We focus here on the key aspects and implications of these limits to information processing for the consumer’s optimal decision. We formally outline the model with more technical details in Appendix A.

An agent with limited information capacity faces a consumption decision under uncertainty about her wealth. Since wealth is unknown, the agent cannot know precisely how much consumption she can afford. Hence, she must treat both wealth and consumption as random variables before deciding how much information about wealth she wants to process in order to consume. As a result, the optimization problem must be expressed and solved in terms of joint probability distributions over wealth and consumption. By contrast, in the infinite information capacity case, wealth is perfectly known and utility is directly maximized by choosing the optimal consumption stream.

More specifically, the optimization problem entails three main parts. First, the agent starts each period $t$ with a prior distribution on her initial level of wealth; although wealth is unknown, this prior distribution is not. Let $w_t$ indicate wealth, the prior is given by the distribution function $g(w_t)$. Before processing any information, consumption is also a random variable. This is because the uncertainty about wealth translates into a number of
possible consumption profiles with various levels of affordability.

Second, the agent chooses how much information about wealth she wants to process in order to make informed consumption choices. One way of thinking about the information processing decision is that the consumer chooses a noisy signal on wealth, where the noise can take on any distribution selected by the consumer consistent with the information processing capacity. In other words, the consumer allocates attention to forming a new probability distribution for $w$ functional to the consumption decision which improves on the prior $g(w)$.

In more precise terms, the reduction of uncertainty about wealth and the consumption choice, indicated by $c_t$, must be seen as two sides of the same utility maximization problem which occur simultaneously. Hence, when information cannot flow at infinite rate, the choice of the consumer is actually the joint distribution $p(w_t, c_t)$, as opposed to the stream of consumption $\{c_t\}_{t=0}^\infty$ we would have in the full information case. Given that the agent has a probability distribution over wealth, choosing the joint distribution $p(c_t, w_t)$ is akin to choosing a signal on wealth. This is easily illustrated by applying Bayes’ rule to the joint distribution

$$p(w_t|c_t) = \frac{p(c_t, w_t)}{\int p(c_t, w_t)dw_t}$$

where the type of signal chosen corresponds to the conditional probability $p(w_t|c_t)$.

The optimal choice of the joint distribution, $p^*(c_t, w_t)$, depends on the constraint on the amount of information the consumer can processes. $p^*(c_t, w_t)$ makes the distribution of consumption conditional on wealth as close to wealth as the limits imposed by Shannon capacity allow. Before processing any information, uncertainty about wealth is measured by the entropy of the prior of wealth $\mathcal{H}(w_t) \equiv -E[\log_2(g(w_t))]$. Since consumption and wealth are related in the consumer’s decision, knowledge of consumption provides information about wealth. The reduction in uncertainty about wealth that can be extracted by knowing

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\[\text{The conditional distribution } p(w_t|c_t) \text{ represents the signal of wealth the consumer receives after consumption is realized, where consumption is a draw from the optimally chosen } p(c_t, w_t).\]
consumption is expressed as the conditional entropy (or residual uncertainty) about wealth once consumption is known, $H(w_t|c_t)$. The information flow, $I(c_t, w_t)$, associated with a signal is the maximum reduction of entropy induced by the signal, which is bounded by the information that a selected signal conveys, $\kappa$, 

$$I(c_t, w_t) = H(w_t) - H(w_t|c_t) \leq \kappa.$$ 

The bound on the information flow depends on the effort the consumer chooses to exercise to track her wealth. Paying attention to reduce uncertainty requires spending some time and utility to process information. The task of thinking is modeled by augmenting the optimization problem with a Shannon channel. Limits in the capacity of the consumers to process information are captured by the fact that the reduction in uncertainty conveyed by the signal cannot be higher than a given number $\kappa_t$, which explicitly limits the set of available signals by introducing an upper bound to the feasible information flows.

The third part of the maximization problem regards the transition law from one period to the next, which in this setup corresponds to how the consumer’s information on wealth evolves over time rather than the standard law of accumulation of wealth. At the end of each period, after the realization of $c_t$, the consumer is endowed with a new income draw, $y_{t+1}$, from a known distribution which corresponds to the prior of the initial wealth $g(w_0)$, since $w_0 = y_0$ by construction. The law of accumulation of wealth reads 

$$w_{t+1} = R(w_t - c_t) + y_{t+1},$$

(1)

where $R$ is the interest on savings. The stochasticity of $w$ derives from the stochasticity of $y$.

The way beliefs about wealth transit across states is described by the update law of the prior $g(w_t)$ into its posterior $g(w_{t+1})$. This law takes into account how the choice in time
t, \( p(w_t, c_t) \), affects the distribution of the consumer’s belief after observing \( c_t \) as well as the stochastic accumulation of wealth in \( (1) \). Given the initial prior state \( g(w_t) \), the next period belief state \( g'(w_{t+1}|c_t) \) is determined by revising each state probability as displayed by the expression known as Bayesian conditioning

\[
g'(w_{t+1}|c_t) = \int \tilde{T}(w_{t+1}; w_t, c_t)p(w_t|c_t)dw_t,
\]

where \( \tilde{T}(\cdot) \) is the transition function embedding \( (1) \). \( g'(w_{t+1}|c_t) \) is used, in turns, as the new prior for the period \( t + 1 \), \( g(w_{t+1}) \).

Let \( u(c) \) be the utility of the household defined over consumption \( c \). Combining these three components, the program of the consumer under information frictions can be written as:

\[
\max_{p(w_t, c_t)\in D(w, c)} \mathbb{E}_0 \left\{ \sum_{t=0}^{\infty} \beta^t \int u(c_t)p(c_t, w_t)\mu(dc_t, dw_t)|\mathcal{I}_0 \right\}
\]

\[ \text{s.t. \hspace{1em} } \kappa_t = I_t(p(c_t, w_t)) \]

\[ \hspace{1em} g'(w_{t+1}|c_t) = \int \tilde{T}(w_{t+1}; w_t, c_t)p(w_t|c_t)dw_t \]

\[ p(c_t, w_t) \in \mathcal{D}(w, c) \]

\[ g(w_0) \text{ given} \]

where \( \mu(\cdot) \) is the Dirac measure that accounts for discreteness in the optimal choice of \( p(c, w) \), while \( \mathcal{D}(w, c) \) restricts the choice of the agent to be drawn from the set of distributions, \( \mathbb{E}_0 \) is the conditional expectation defined with respect to the \( \sigma \)-algebra \( \mathcal{I}_0 \).

Let \( \theta \) indicate the Lagrange multiplier associated with constraint \( (3) \), the total cost of an agent of choosing a signal and processing information is then given by \( \theta \kappa_t \). We allow \( \kappa_t \) to vary over time to capture the possibility that subjects in the experiment may want to process different amount of information within and between treatments. This formulation is

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known theoretically as subject’s elastic capacity of information processing. Subjects’ own $\theta$ is not directly observable in the experiment. However, we impose a structure of signals with different precision levels representing different information flows ($\kappa$), and let the subjects select among the alternatives according to how much time and effort they want to invest to sharpen their knowledge of wealth. In turn, this choice determines their optimal total cost.

The program in equations (2)-(6) is a well-posed mathematical problem, but with state and control variables that are infinite dimensional. However, Tutino (2013) shows that a discretization of this framework is also a well-posed problem and returns as a solution the ergodic distribution $p^*(c_t, w_t)$ which, from Bayes’ rule as well, can be represented as

$$p^*(c_t, w_t) = p^*(c_t|w_t)g(w_t),$$

where the marginal wealth distribution is equal to the prior, $\int p^*(c_t, w_t)dc_t = g(w_t)$, to satisfy model’s internal consistency. The conditional distribution $p^*(c_t|w_t)$ embeds the effects of more accurate information about wealth provided by the selected signal to sharpen consumption choices.

Our experiment studies the implications of the equilibrium solution of the static version of this model. The model is simplified in a static setting since savings are not possible, and the unused wealth is forgone. In fact, in a static setting $w_t = y_t \forall t$, and the evolution of beliefs about wealth (4) is simply replaced by the prior itself $g(w_t) = g(w_0) \forall t$; similarly, the objective function turns into the one-period version of (2).

In this case, the optimal solution is readily computed and can be expressed as:

$$p^*(c, w) = g(w) \left( e^{\left( \frac{u(c)}{\theta} \ln 2 \right)} - 1 \right),$$

which illustrates the solution depends on the shadow cost of processing information, $\theta$, the prior distribution of wealth, $g(w)$, and the functional form of utility, $u(c)$. 

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In Section 4, we discuss the main predictions obtained from the optimal solution of the static setting which can be tested with our experimental data.

3 Experimental Design and Implementation

This section discusses our experimental design. We first introduce the building blocks of the basic experiment and explicitly link them to the theoretical model in Section 2. We then describe the cognitive tasks the subjects engage with during the experiment and treatment variations we study.

3.1 Experimental design

We devise an experiment that aims to closely replicate in a laboratory setting the static version of Tutino (2013)’s model. Throughout the experiment, we present the subjects with a choice of 256 prizes whose values are expressed in experimental dollars. Prizes are sorted by increasing cost ranging from 1 to 256. The value of the prizes is directly related to its cost, i.e. how many experimental dollars it takes for the subject to claim a particular prize. The subjects attempt to maximize the total sum of prizes accumulated in the experiment which represents their final payoff. Subjects are endowed with a income in each period of the experiment. Each period, income is drawn from a distribution known to the subjects; however, the realization of the draw is unknown. Thus, the decision problem of the participants is to select prizes under income uncertainty. Thus, their decision problem boils down to selecting prizes under uncertainty about their available budget.

Subjects can reduce uncertainty about how big of a prize they can afford each period by choosing a signal of their endowment before selecting a prize. We provide them with an array of signals with varying levels of precision. The signals provide more information by narrowing the range from which the endowment was drawn. The precision of the signals is tightly linked to the cognitive effort the subjects need to exert in order to reveal the
signals’ information. The higher the precision of the signal is, the narrower the interval of possible endowments it provides, and the more significant the cognitive effort a subject must exert to achieve the signal. The precision levels available to the subjects and the associated average difficulty levels are illustrated in Table 1. More details about signals, intervals, and cognitive tasks are discussed in Section 3.2. With the introduction of these signals, the decision problem of the subjects becomes how much information they want to acquire before selecting the prizes.

We can explicitly map these features of experimental designs to the building blocks of the theoretical framework in Section 2. The cost of the prizes is the equivalent of the amount of consumption $c$ chosen by the optimizing agent in the model. The value of the prizes represents the experimental counterpart of the utility derived by the agents from consumption. Thus, choosing prizes is akin to choosing consumption. The endowment in the experiment is income $y$, which in the static setup is equal to wealth in any point in time, $y = w$. The prior on wealth is the known distribution over the income/consumption support from which endowments are drawn, $g(y)$. For the sake of concreteness, in the baseline treatment of the experiment $g(y)$ takes on the form of a discrete uniform distribution on the 1-256 interval.

To simplify the experimental design, the structure of the signals we provide belongs to the uniform family. In terms of the model, this simplification maps into the constraint (5) requiring that the distribution is chosen not from any family, but from the discrete distribution. We acknowledge the fact that a uniform distribution is unlikely to be the optimal for the problem in (2)-(6), since it is the distribution with the highest entropy. However, we decide to forgo optimality for tractability of the experimental set-up and, more importantly, to increase clarity for the subjects about the experiment thereby avoiding mistakes due to misunderstandings of the experimental structure. Under this additional constraint, subjects can choose any feasible signal precision consistent with their information processing limits. By allowing the subjects to choose the precision level of the signals, we can infer
subjects’ information processing shadow cost, $\theta$. As mentioned earlier, $\theta$ is not observed; however, subjects target their optimal total cost of information processing, $\theta \kappa_t$, by choosing the cognitive effort associated with a signal.

The interval revealed to the subjects by an acquired signal defines the corresponding optimal conditional distribution $p^*(c|w)$ in the theoretical framework. By narrowing the support of the income draw, subjects can modify their prior $g(y)$ with a more concentrated distribution reflecting lower uncertainty about income. For instance, in the baseline treatment, subjects would go from an initial discrete uniform prior on the full support 1-256 to a discrete uniform distribution over a restricted support, corresponding to a smaller interval. The observed realization of a particular prize is a draw from the optimal distribution $p^*(c|w)$.

Finally, since in the experiment periods are not statistically linked in the baseline given the i.i.d. nature of income $y$, the prior $g(y)$ is identical in each period, simplifying the constraint (4) to $g'(w_{t+1}) = g(w_t) = g(y)$.

The experiment is deployed in the laboratory using the interface shown in Figure 1. On the top portion of the screen, the subjects see the amount of time elapsed thus far in the treatment (in seconds) in the left corner, the period currently being played in the middle, and the accumulated total prizes on the right. The central part of the screen conveys information about the possible values of the prizes, while the bottom row of aqua blue buttons displays the nine levels of precision form the available signals from which the subject can choose. The buttons describe the size of the possible income intervals corresponding to each precision level, along with the difficulty level of the cognitive task expressed in terms of expected rate of success. For a signal to reveal the information about income, subjects must successfully complete the cognitive tasks associated to the signal selected. Both the expected success rates and the cognitive tasks will be described in details in Section 3.2.

We also use the screenshot in Figure 1 to visualize the mapping between theory and experiment. Subjects start each period with the grid of all potential prize values that reflects
Figure 1: Mapping from the theory to the experiment: a screenshot of the experimental interface over which we superimpose the theoretical framework.
the diffuse prior on income \( g(w) \). In this example illustrating the baseline, the value of prizes goes from 1 to 256 experimental dollars and there is a one to one transformation rate from the cost of prizes to their value. If subjects wants to proceed with no further information, they would select the first signal (with precision 0 in our classification) and the full set of prizes would turn yellow, indicating that the space of possible prize outcomes spans the whole support \([1, 256]\).

To make a concrete example, suppose a subject chooses a precision 3 signal and successfully completes the associated cognitive task. The signal reveals that income lies in the interval of 32 prizes from 193 to 224 and the two rows corresponding to this interval are in yellow. Thus, this particular signal selection allows the subject a reduction of income uncertainty of 1.5 bits – from the uninformative prior’s entropy of 2.4 bits to the posterior’s entropy of 0.9, as Table 1 illustrates.

The mapping between theory and experimental interface can be readily seen by interpreting the combined structure of prize grid and signal buttons as \( p(c, w) \) before any decision is made by the subject. The optimal policy function of the subject is a strategy that select a signal whose information is functional to acquiring the prizes. This strategy is defined as \( p^*(c, w) \), and in the experimental setting it encompasses both the choice of a particular signal on \( w \) and the information content about consumption possibilities revealed by the signal (as shown by the dashed red rectangle in Figure 1). Upon choosing the signal and successfully completing the cognitive task, the optimal posterior \( p^*(c|w) \) maps into the highlighted interval of prizes in the grid, corresponding to the narrower support for the prizes, or, equivalently, to the reduction of uncertainty about income provided by the signal. The chosen prize, 194, can be treated as a random draw from the optimal posterior \( p^*(c|w) \). If the prize drawn is smaller than the available income in that particular period, the prize box turns green and the prize value is added to the cumulative prize total.\(^5\)

\(^5\)Subjects are free to choose any prize even after receiving a signal. Knowing the more precise interval,
Table 1: Signal structure: precision levels, signal characteristics, expected success rate of the cognitive tasks, and information flows of the signals.

<table>
<thead>
<tr>
<th>Precision Level</th>
<th>Intervals Description</th>
<th>% Task Correct</th>
<th>Information Flow (bits)</th>
</tr>
</thead>
<tbody>
<tr>
<td>0</td>
<td>1 interval of length 256: [1 256]</td>
<td>99%</td>
<td>0</td>
</tr>
<tr>
<td>1</td>
<td>2 intervals of length 128: [1 128][129 256]</td>
<td>90%</td>
<td>0.3</td>
</tr>
<tr>
<td>2</td>
<td>4 intervals of length 64: [1 64] . . . [193 256]</td>
<td>80%</td>
<td>0.6</td>
</tr>
<tr>
<td>3</td>
<td>8 intervals of length 32: [1 32] . . . [225 256]</td>
<td>65%</td>
<td>0.9</td>
</tr>
<tr>
<td>4</td>
<td>16 intervals of length 16: [1 16] . . . [241 256]</td>
<td>50%</td>
<td>1.2</td>
</tr>
<tr>
<td>5</td>
<td>32 intervals of length 8: [1 8] . . . [249 256]</td>
<td>35%</td>
<td>1.5</td>
</tr>
<tr>
<td>6</td>
<td>64 intervals of length 4: [1 4] . . . [253 256]</td>
<td>20%</td>
<td>1.8</td>
</tr>
<tr>
<td>7</td>
<td>128 intervals of length 2: [1 2] . . . [255 256]</td>
<td>15%</td>
<td>2.1</td>
</tr>
<tr>
<td>8</td>
<td>256 intervals of length 1: [1] . . . [256]</td>
<td>5%</td>
<td>2.4</td>
</tr>
</tbody>
</table>

3.2 Signal characteristics and cognitive tasks

The information processing limits embedded in constraint (3) in the theoretical framework are implemented in the experiment through the structure of signals of different precisions that the subjects use to reduce uncertainty about income draws. We describe in this section the characteristics and information content of these signals.

As summarized in Table 1, subjects have access to 9 levels of signal precision. The levels start from a basic Level 0 signal, which is uninformative and simply replicates the prior of the income distribution, to a Level 8 signal, which is fully revealing about the income draw. Letting $j$ indicate the precision level, a signal identifies one of the $2^j$ intervals containing $2^{8-j}$ prizes into which the full support of prizes is partitioned.

We can compute the information flow of the signals by finding their variation in entropy starting from the uniform prior $g(y)$. The information flows of the signals are reported in the last column of Table 1. Since the signal structure is such that signals pertain to uniform distributions and the supports of the signals are proportional, our signals have the property though, subjects are expected, but not constrained, to choose from the prizes within the interval, as in the example discussed here. Section 3.3 describes how the experiment handles the cases in which the chosen prize is bigger than the income draw.
that the change in entropy is constant from one precision level to the next, and equal to 0.3 bits.

Subjects must successfully complete a cognitive task in order to acquire a signal. The precision of the signals is proportional to the cognitive difficulty of the task, and the subjects must exert a higher cognitive effort to obtain a better signal. We rely on the visual puzzles developed by Civelli and Deck (2018) to compose these tasks. These puzzles take on the form of a (3 × 3) graphical matrix in which eight images are provided and one is missing. Subjects must identify the missing image among a set of alternatives, after analyzing the patterns of attributes shared by the eight images in the matrix. The puzzles are generated in the spirit of the Raven’s Progressive Matrices, and their level of difficulty is correlated with the reasoning abilities of an individual. An example puzzle is illustrated in Figure 2, while the corresponding solution set with eight images is shown in Figure 3.

Each image in the puzzle has six attributes that could change: shape, size, shade of the filling, orientation, border style, and corner marker style. These attributes are allowed to change following six schemes of patterns: orthogonal - along rows and columns; diagonal - along main or counter-diagonal; and corners - from NW to SE and from SW to NE. The difficulty level of a matrix is determined by the number of attributes allowed to change. Given the puzzle calibration exercises conducted by Civelli and Deck (2018), we are able to create tasks of any desired cognitive difficulty by requiring someone to solve a series of puzzles of various difficulty levels. A task is usually composed of 2 puzzles, with the exception of the highest precision level that requires solving 3 very difficult puzzles. The reference expected difficulty level of the task of each signal is reported in Table 1 as well.

In the example of Figure 2, two attributes change: shape and size, while shade, orientation, border style, and corner marker style remain unchanged. Notice that along the diagonal from upper left to lower right, the size is the same. Further, notice that the shapes vary along the counter-diagonal. Given these observations about the patterns of attributes
Figure 2: An example of a logic puzzle faced by the subjects.

Figure 3: An example of a solution set for the logic puzzle in Figure 2.
in the puzzle matrix, one can deduce that option “A” in Figure 3 is the correct solution of the puzzle since it has the correct characteristics to complete the matrix.

3.3 Experimental Implementation

The experiment was written in Visual Basic, and it was deployed as a stand alone executable on the machines in the Behavioral Business Research Laboratory at the University of Arkansas. The experiment is a within-subject design, and it is broken into an un-paid practice round and 3 treatment rounds including the baseline. The practice round lasted 10 minutes and was otherwise identical to the baseline. Treatments are randomly ordered so as to mitigate order effects. Before beginning the practice round, the subjects worked through a series of instructions on their screen, and each subject was offered an opportunity to ask clarifying questions before beginning the actual experiment. These instructions have been included in Appendix B.

Subjects were recruited from the undergraduate student body at the University of Arkansas, and a total of 64 subjects participated in the experiment over the course of 6 sessions in the month of May 2017. Subjects earned an average total profit, including a $5 participation payment, of $25.30 USD. Each session lasted approximately 90 minutes, which included working through the instructions, the practice round, and participating in the actual experiment. Payouts in excess of the show-up fee were determined by the subjects total cumulative prizes across all three treatments divided by 500.

In each treatment, subjects face an indefinite number of periods during which they can try to earn as much money as possible. For each treatment, subjects are told that the allotted time is random, with an average of about 20 minutes. The unspecified ending time is designed to mitigate end of game effects on behavior. At the beginning of a period, subjects receive a new an endowment draw. In each period, subjects are allowed to attempt to gain as much additional information about the endowment from the signals as desired. If a subject
fails to successfully solve the task to obtain a more precise signal, she is free to attempt that same precision or another level of precision as many time as she wishes. However, once a precision level is successfully obtained, the subject is unable to reduce the precision of the signal.\textsuperscript{6}

Subjects accumulate earnings by successfully claiming prizes. Successfully claiming a prize requires the subject to claim a prize which costs less than or equal to the endowment drawn in that period. There are three possible outcomes when a subject attempts to claim a prize:

1. The subject claims a prize of value less than or equal to the drawn endowment. In this case the subject earns the value of the prize claimed and moves on to the next period.

2. The subject successfully obtains additional information about her endowment, but chooses a prize with a value that is greater than the endowment draw. In this case, the subject earns zero profit for the period and moves on to the next period.

3. The subject does not obtain additional information beyond what is known at the start of the period, simply that the endowment falls in the range of 1-256, and the subject picks a prize of greater value than the endowment draw. This results in zero earnings and a 60-second reduction in the length of the treatment that is enforced by adding 60 seconds to the elapsed timer.\textsuperscript{7} The theoretical model assumes an utility cost that the consumer incurs for zero consumption when it attempts $c > w$. This cost is embedded in the assumed functional form of the utility, CRRA family and log in its limit, which delivers negative infinite utility in case $c = 0$. This assumption is encoded in the experimental setting captures the well-known principle in information theory that information cannot be forgotten. In order to avoid subjects having to rely on cognitive effort to remember the more informative signals, we prevent them from choosing signals with lower precision than the ones successfully obtained.

\textsuperscript{6}This assumption encoded in the experimental setting captures the well-known principle in information theory that information cannot be forgotten. In order to avoid subjects having to rely on cognitive effort to remember the more informative signals, we prevent them from choosing signals with lower precision than the ones successfully obtained.

\textsuperscript{7}Incurring the penalty does not mean the subject has to sit idle for 60 seconds. It means the length of time over which the subject can try to earn money is reduced by 60 seconds.
made to prevent the decision maker from adopting a random consumption strategy without acquiring any information about income. In the experimental setting the 60-second penalty ensures that random consumption strategy is strictly dominated by an informed consumption strategy, preventing the subjects from utilizing a mechanical strategy in which they meaninglessly guess prizes a large number of times hoping to rapidly accumulate prizes.8

Choosing a prize concludes one period; as mentioned above, subjects encounter as many periods as they can within the timeframe of the treatment. At the beginning of each period, the subjects return to the state of only knowing that their income has been drawn from \( g(y) \), and they are again able to choose whether or not to pursue more precise information regarding the value of endowment.

### 3.4 Experimental Treatments

Up to this point the description of the task has focused on the baseline (Treatment 1). In Treatment 2, the endowment is drawn randomly from a uniform distribution on the support \([1, 256]\) as in Treatment 1; however, in Treatment 2 the value of prizes are increased by an order of magnitude every \((10^{th})\) period and decreased by an order of magnitude in all other periods. That is, in periods 11, 22, 33, etc., the value of every prize shown on the screen is multiplied by 10 in comparison to the prizes shown in Treatment 1. For the other periods the displayed prizes are one tenth the amount displayed in Treatment 1. The ratio of high payoff to low payoff periods is such that the expected payoff is the same across all treatments. In Treatment 3, the mapping of prize value to profit was the same as in Treatment 1, but the endowment is determined differently. For the first period, the endowment is drawn from the usual uniform distribution. For each subsequent period, there is an 80% probability that

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8In the experiment the penalty is levied to the participants who attempt \( c > w \) either without trying any signal or unsuccessfully trying a signal. The two cases are observationally equivalent in terms of information effectively acquired to reduce uncertainty with respect to the initial prior.
the endowment is the same as it was in the previous period and a 20% chance that the endowment is determined by a new draw from the uniform distribution.

4 Results

In this section we introduce five testable predictions we obtain from the comparative statics based on the solution of the static model in (7). We use the data from the laboratory to test these predictions in each of the sections below.

4.1 Information and consumption

The first testable prediction stemming from the model is that the more information agents acquire, the higher their consumption is. We test this prediction by examining the relationship between precision of information chosen by the participants and their realized consumption outcomes. In our experiment, precision of information relates to the cognitive tasks participants have to solve to know where their income lies. The harder the task is, the smaller the width of the interval revealing where the actual income is. We test whether a participant solving more difficult cognitive tasks enjoy higher profits than a participant who selects easier tasks.

Figure 4 illustrates the relation between successful consumption choices and the signal precision acquired, for all subjects in Treatment 1 of the experiment. The positive relation is clearly shown by the estimated OLS fitted line; the estimate slope coefficient is 16.9 and it is significant at 1%. Individuals with better information are also able to obtain higher successful realizations of consumption.

The figure also summarizes a couple of interesting properties of the consumption and precision decisions across individuals. First, we can clearly observe how the consumption choices are clustered at the intervals determined by each signal level. The points favor the lower portion of the intervals, as we would expect even for risk neutral subjects, but they
Figure 4: Scatter plot between consumption choices and the signal precision acquired. All subjects and all treatments.

Figure 5: Treatment 1: Scatter plot between average successful consumption and average signal precision acquired by each subject. The color-bar indicates the average time in seconds spent for information acquisition by the subject before a consumption choice is made.
are actually spread out and not concentrated on the lower extreme of the interval. We will go back to this point in Section 4.2 too. Second, the majority of the signals chosen by the subjects are for the first levels of precision up to level 4. Precision 5, 6, and 7 get progressively more infrequent as the cognitive cost of acquiring the signal gets higher. Level 7 is virtually never chosen and Level 8 is never successfully completed indicating it correctly approximates an infinite information capacity situation.

Similarly, Figure 5 shows the scatter plot between average precision acquired by each subject and average successful consumption in the baseline, Treatment 1. The color of the dots indicates the average time in seconds spent by the subject to acquire information before making the consumption choice, based on the scale on the right hand side of the figure. The plot confirms a strong positive relation between consumption and precision too, with an estimated coefficient of 14.1 (significant at 1%). As expected, we can also observe a general increase in the period length as the precision of the signal goes up.

4.2 Restricted consumption support

The second testable prediction of the model is that the optimal probability distribution $p^*(c, w)$ assigns positive probability to only an handful of values out of the full the support of $c$. A rationally inattentive consumer focuses only on a restricted subset of $c$, placing higher probability of selecting within that restricted support than everywhere else.

This prediction from the rational inattention model is observationally distinct from other theories of values where either the mean of the support is chosen with probability one or all the values in the support of $c$ are equally likely to be chosen. Moreover, unlike habit formation, where an ad-hoc shape of utility is needed to elicit the behavior predicted by the theory, rational inattention derives this prediction from people’s optimizing behavior of balancing cost and benefit of information gathering.

In the experimental setup, we test for this prediction by looking at the empirical dis-
Figure 6: Aggregate distributions of consumption choices for all subjects by signal precision. The intervals corresponding to a signal that spans different portions of the consumption space are superimposed to form a common distribution support.
tribution of consumption selections conditional on the signal received by the subjects. If the prediction holds, choices of consumption would display a restricted support; within that support, the consumption distribution would not take on the form of a uniform distribution since some outcomes will be more likely to be selected than others and some will not be selected at all, nor would it be a degenerate distribution with all probability concentrated on one particular value of the support.

We next discuss some evidence that corroborates this prediction. We begin with the simple histograms of the aggregate distributions of consumption choices made by all subjects in all treatments, reported in Figure 6. Each panel of the Figure refers to a precision level in which the different intervals corresponding to that precision level – disjoint, but of the same length – are superimposed. We focus on precision 1 to 6, since we do not have sufficient observations to generate reliable plots for higher precision levels.

The distributions typically exhibit higher densities on the lower portion of the intervals; however, we also find substantial distribution density on the rest of the segment. This evidence suggests that the empirical distribution of the subjects’ consumption choices is left-skewed – reflecting moderate risk aversion. However, the distribution is not spurious and it is not concentrated at a single point common to all subjects. Similarly, the optimal distributions of consumption are not uniform either, a difference from the prior.\footnote{The distribution of Precision 3 exhibits a density peak at position negative one. This position indicates a consumption choice outside the interval revealed by the signal, corresponding to the upper extreme of the interval preceding the revealed one. This concentration of the density reflects the behavior of an individual who acquired precision 3 signals multiple times and systematically chose one unit of consumption less than the minimum allotted by the revealed signal. There are three other subjects for which this behavior occurs episodically. We judge those instances as mistakes as opposite to deliberate choice and do not include them in Figure 6.}

These histograms help us show some properties of the observed distribution of consumption, but they are based on aggregate data for all subjects and hide individual choices. We consider distributions at the individual level, then, in order to fully assess the restricted support prediction. In fact, individual subjects might select only an handful of consumption
points, but if these points are distinct from one subject to the next then aggregation would cause overall distribution to look more dispersed. We assess this possibility by reporting in Figure 7 the Herfindahl-Hirschman Index (HHI) of the individual consumption distributions for each subject for signal precision 1 to 4.

The HHI is typically used to measure market concentration. It is calculated by summing the square of the market shares of competing firms in an industry, and it has a maximum value of 10,000 when the market is full concentrated in the hands of one single firm. In our situation, we first compute the frequencies of the consumption choices made by a subject over the support defined by the overlapped intervals corresponding to a certain signal precision. We then calculate the HHI of the consumption choices of an individual summing up the square of the frequencies of selected consumption levels. Finally, we normalize the HHI’s by 10,000 and order the normalized HHI values in decreasing order to report them in Figure 7.

The HHI is an intuitive and rapid way to provide information about the dispersion of consumption levels chosen by the subject although it is not always possible to uniquely map an HHI value into the exact number of points chosen. To this end, the horizontal red lines in Figure 7 provide some reference levels. For instance, when an HHI bar is equal to 1, the subject chose the same point every time he received a signal with that precision. The red line at a height of 0.5 denotes the normalized HHI for a subject who chooses each of two points with equal frequency. The following lines follow the same principle: 0.25 for four choices each with a $\frac{1}{4}$ share, .1 for 10 choices each with a $\frac{1}{10}$ share. The figure omits subjects that make only one consumption choice for which the HHI is trivially 1.\textsuperscript{10} We include subjects that make two choices to check whether their HHI differs from the trivial result of 0.5. These cases, HHI=1 with two choices, are displayed in red bars.

The subjects are ordered on the horizontal axis. Since the number of signals is endogenous the number of subjects varies by precision level. The histograms show that the majority

\textsuperscript{10}There are a total of 23 such cases out of 122 subject-precision observations in the sample of Figure 7.
Figure 7: Herfindahl-Hirschman Index (HHI) of the individual consumption distributions for all subjects for signal precision 1 to 4. The indexes are normalized by 10,000 and sorted in decreasing order. The horizontal lines indicate the HHI value that would occur if the subject were to choose the number of points indicated on the right vertical axis with the same frequency. Subjects who make a single consumption choice are omitted. Red bars indicate subjects with two consumption choices and HHI=1.
of subjects, do not focus exclusively on a single consumption level for a given precision. Further, almost half of the subjects spread their choices among four or more consumption choices within the revealed interval (HHIs fall below .25).

Overall, the evidence of this section strongly suggests both the transformation of the prior distribution into a non-uniform and non-degenerate optimal consumption distribution, and the concentration of consumption decisions on relatively small sets of choices, but not on a single choice.

4.3 Convex Payoffs and Information Processing

The third testable prediction from the model concerns the effects of incentives on subjects’ information acquisition and information processing. The prediction states higher payoffs are associated with more informed decisions. Intuitively, the prediction states that individuals pay more attention to the economic environment the higher the stakes are. We verify this prediction in Treatment 2, in which we vary the concavity of the payoffs by varying their magnitude across periods within the Treatment. As we explained in Section 3, the payoff variations occur at a pre-determined and known frequency: every eleventh period.

We assess the implications of payoff fluctuations in Figures 8 and 9. In these Figures, the red bars refer to signals pursued on the “eleventh” periods, when we impose higher payoffs in Treatment 2, while the blue bars correspond to the other periods, when we have lower payoffs in Treatment 2. It is worth keeping in mind that the payoffs differ by a factor of 100 between the high and low stakes periods of Treatment 2. Figure 8 illustrates the distribution of the maximum signal precision that subjects attempted to acquire before each consumption decision, regardless of the successful attainment of the signal. All subjects and all information acquisition instances are pooled together by treatment, and the distribution in Treatments 2 is compared to the baseline distribution in Treatment 1.

Two observations are noteworthy. First, as expected, there should not be differences
between the eleventh and off-eleventh period distributions in Treatment 1, since the subjects do not observe any change in the payoff structure in this Treatment. A KS test to compare the two distributions cannot reject the null that empirical distributions are the same (p-value of .88). In Treatment 2, the two distribution noticeably move apart from each other. The high payoff distribution shifts to the right, and reaches its peak for precision level 3. This shows how subjects seek better information in the high payoff periods in Treatment 2. The regular payoff distribution, on the contrary, shifts to the left, relative to the Treatment 1, with the highest mass concentration at precision 0. This reflects the optimal strategy of reducing attention in the low stakes periods, while increasing it in the high stakes moments. The difference in the two distributions is confirmed by the KS test too, which rejects the null at very high confidence level (p-value of .00).

Comparing the distribution across treatments, we find that the mean of the distributions in Treatment 1 is between precision 1 and 2; in Treatment 2 the mean of the high payoff distribution is between precision 2 and 3, while for low payoffs it is between 0 and 1. We formally test whether these distribution are the same across Treatments using the KS test again. The tests reject the null of same empirical distribution for both high and low payoffs states at extremely high level of confidence (p-value of .00).

As a robustness check, Figure 9 illustrates the distribution of the average maximum precision of the signals attempted by each individual by treatment. As before, the eleventh and off-eleventh period distributions are very similar in Treatment 1. There is no reason for subjects to change their behavior in this case since there is no material change in the economic environment from one period to the next in the baseline treatment. By contrast, in Treatment 2, the distribution of the high payoff periods shifts to the right, away from the least informative signal that dominates in the low payoff periods. Thus, this second set of figures corroborates the result that participants trade-off effort when the payoff is small for higher precision when the payoff is more substantial.
Figure 8: Distributions of the maximum precision of attempted signals – Treatment 1 Vs. Treatment 2. Period numbers that are a multiple of 11 in red while other periods are in blue. All subjects, all attempts.

Figure 9: Distributions of the average maximum precision of the signals attempted by each subject – Treatment 1 Vs. Treatment 2. Period numbers that are a multiple of 11 in red while other periods are in blue. All subjects, averages by subject.
As a consequence of the decision of allocating more attention to the economic environment in the high payoff state, consumption should be higher in the “eleventh” periods. We illustrate this point in Figure 10, where we estimate the non-parametric aggregate density distribution of consumption in the high and low payoff periods in Treatment 1 and 2 by using a normal kernel estimator.

In panel (a) of the Figure we see the two densities for Treatment 1 closely overlap. The density for the non-eleventh periods is less smooth, though, since the larger number of observations allows for a more precise estimation of the density. On the contrary, the densities in panel (b) for Treatment 2 exhibit two distinguishable density peaks. For the high payoff distribution the density maximum is achieved around 130 unit of consumption, while for the low payoff density this peak is shifted far to the left indicating a high density concentration at poor consumption levels.

A similar result is conveyed by Figure 11 where we plot the aggregate density distributions of unused income, as a percentage of the income draw – a measure of the “error” in consumption made by the subjects. Again, the two densities are the same for Treatment 1, while they have a completely different shapes in Treatment 2. In particular, we find a mass concentration at very low and very high levels of unused income respectively for the high and low payoff periods.

In summary, the results of this Section imply that participants with limited ability to process information choose more information about options that provide them with higher utility and, as a consequence, make better consumption decisions. This result is intuitive since a higher reward implies an higher benefit to process more precise information offsetting its cost. Consistent with these findings, the optimal probability distribution $p^* (c, w)$ is more informative the higher the payoff is.

\footnote{This is obviously the case if the attempted signal precision reported in Figure 8 lead to successful information acquisition too. In our data, we find that about 85\% of the signal attempts are successful.}
Figure 10: Aggregate density distribution of successful consumption choices – Treatment 1 Vs. Treatment 2. Estimation by normal kernel function. High payoff periods in red; low payoff periods in blue. All subjects.

Figure 11: Aggregate density distribution of unused income, as a percentage of the income draw – Treatment 1 Vs. Treatment 2. Estimation by normal kernel function. High payoff periods in red; low payoff periods in blue. All subjects.
4.4 Predictability of economic environment

The fourth prediction posits that a rationally inattentive person responds to a more predictable environment – higher persistence of $g(w)$ – by processing less information. That is, the optimal probability distribution of consumption conditional on the signal chosen by the participant, $p^*(c|w)$, is more dispersed in the case in which the environment is less uncertain, reflecting either a less precise signal acquired in the more predictable environment or more "mistakes" in consumption when learning about the environment turns out to be more costly than forgoing consumption units. We rely on Treatment 3 to test this prediction.

This prediction can have two different outcomes on the behavior of the subjects in the experiment. One the one hand, they could periodically increase signal precision, when they realize an income shock has occurred, to reset information about their income level. After acquiring more information, they would maintain a low precision signal relying on the high persistence of the process. On the other hand, if the cost of increasing precision level is sufficiently high, they could opt for a low precision level by default and use consumption choices to explore the income space. In this case, they would gropingly increase consumption after a positive income shock, trying to figure out the new higher income. Similarly, they would progressively reduce consumption after a negative shock, trying to detect the new lower income.

As we saw in Section 4.3, the cognitive cost of the signals necessary to acquire better information is relatively high in our experimental setup. In Treatment 2, for instance, the ten fold increase in the magnitude of payoffs was sufficient to move up precision of the signal by roughly only one notch. This tendency is confirmed by Treatment 3 as well, and the second type of behavior described above prevails. We illustrate this point in the two panels of Figure 12, where the average income, consumption, and precision time profiles in the neighborhood of an income shock are reported for all subjects in Treatment 3. The figures
Figure 12: Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.

are re-centered on the period in which the shock occurs, time 0 on the horizontal axis. The time series show the two periods before the shock and the three periods after it. We consider all episodes followed by at least three periods of constant income level. The left panel refers to the positive shocks, while the right panel to the negative ones.

While income jumps up with a positive shock (green line), the precision level minimally varies (blue line). However, consumption (red line) follows the direction of the shock and it is used by the subjects in place of a change in precision. In case of a negative shock, though, the response is more hybrid. Consumption drops at time 0, following income. Precision is increased in the next period, for just a period to provide one with a better sense of the new income level. After that, subjects start slowly moving up their consumption choices to learn more about income, as done for a positive shock.

We also find, however, the behavior of the subjects can be quite idiosyncratic. Some of them, probably those who have higher cognitive capacity, prefer to manipulate the precision level after a shock, as we document in Appendix C.
4.5 Asymmetry in delays

The fifth proposition deals with the differences in subjects’ responses to positive and negative income shocks. The theory predicts an asymmetric response to shocks of opposite sign. Although subjects naturally exhibit a delay in the response regardless of the type of shock, the response to bad income shocks should be faster and sharper than to good income shocks. Intuitively, this is because bad shocks elicit the allocation of more attention from the subjects.

This prediction, which is unique to RI with respect to other mainstream theories, can be tested with our results comparing subjects’ reaction to a period in which income falls with the reaction to a period in which income increases. Basically, a negative shock to income should be detected faster since previous consumption choices may no longer be available, whereas if the participant is sticky in its consumption choices an increase in income may go undetected for several periods.

Figure 13 replicates Figure 12 for two levels of shock intensity. The first row refers to shocks larger than two thirds of the income support (i.e. 162 units of income); the second row refers to shocks smaller than one third of the support (smaller than 94 units of income). Considering different sizes of the shocks helps us highlight differences in the speed of the responses.

We find that the responses of consumption and precision to income shock closely follow the predicted asymmetry. In the first row, a large negative shock causes an immediate correction of consumption that rapidly drops and remain low. On the contrary, subjects respond only gradually to a large positive shock. The adjustment process is indeed quite slow, and a faster correction would require subjects to increase the precision of the acquired signal. For small shocks, illustrated by the last row of the same Figure, the subjects’ response is less sharp and milder for the positive shock, while it is still quite rapid and deep for the
negative shock. Very interestingly, the negative shock induces a clear increase in the precision level the first period after the shock, which is totally missing for positive shocks. The negative shocks are perceived as more worrying, so that they also require an adjustment in the signal precision.

Overall, the size and timing of the reactions to income shocks is strongly supportive of this prediction of the model.
Figure 13: Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. Large shocks are larger than 162 units of income; medium shocks are between 94 and 162 units of income; small shocks are smaller than 94. All subjects in Treatment 3.
5 Concluding Remarks

The paper shows that subjects in a controlled laboratory experiment largely behave in accordance with the predictions of rationally inattentive consumption theory. Specifically, we find that subjects

- consume stochastically, with their consumption choices reflecting a draw from their optimal posterior distribution of consumption conditional on signals about income.

- respond to incentives by varying the informativeness of their optimal posterior toward higher consumption values, consistent with the postulates of elastic information processing capacity.

- react asymmetrically to shocks of income, with stronger and faster adjustment of consumption to negative shocks than positive ones.

The last of these findings is particularly noteworthy because, to the best of our knowledge, it is the first direct evidence of a pattern that is predicted by rational inattention, but not by other models.

We map the mutual information between prior and posterior beliefs by rigorously measuring attention and information processing using cognitive tasks and consumption choices. As research on behavioral modeling strategies focusing on information processing and attention advances, this paper constitutes a step towards bridging the gap between theoretical models and applied measurements.
6 References


Mel Win Khaw, Luminita Stevens, and Michael Woodford. Discrete adjustment to a changing


The theoretical model borrows from Tutino (2013). An agent faces a consumption decision under uncertainty about her wealth and acquires information about the distribution of her income under an information processing capacity constraint. To understand the implications of limits to information processing, we start with the standard, full information version of the problem.

Let \((\Omega, \mathcal{B})\) be the measurable space, where \(\Omega\) represents the sample set and \(\mathcal{B}\) the event set. States and actions are defined on \((\Omega, \mathcal{B})\). Let \(c_t, c_t,\) and \(w_t\) respectively indicate consumption, income, and wealth at time \(t\). Let \(\mathcal{I}_t\) be the \(\sigma\)-algebra generated by \(\{c_t, w_t\}\) up to time \(t\), i.e., \(\mathcal{I}_t = \sigma(c_t, w_t; c_{t-1}, w_{t-1}; \ldots; c_0, w_0)\). Then, the collection \(\{\mathcal{I}_t\}_{t=0}^{\infty}\) such that \(\mathcal{I}_t \subset \mathcal{I}_s \forall s \geq t\) is a filtration.

Let \(u(c)\) be the utility of the household defined over a consumption good \(c\). The consumer’s problem is:

\[
\max_{\{c_t\}_{t=0}^{\infty}} \mathbb{E}_0 \left\{ \sum_{t=0}^{\infty} \beta^t u(c_t) \mid \mathcal{I}_0 \right\} \tag{8}
\]

s.t. \(w_{t+1} = R (w_t - c_t) + y_{t+1} \) \(\tag{9}\)
\(w_0\) given \(\tag{10}\)

where \(\beta \in [0, 1)\) is the discount factor and \(R = 1/\beta\) is the interest on savings, \((w_t - c_t)\).

We assume that \(y_t \in Y \equiv \{y^1, y^2, \ldots, y^N\}\) follows a stationary Markov process with constant mean \(\mathbb{E}_t (y_{t+1} \mid \mathcal{I}_t) = \bar{y}\).

\footnote{We can assume that the utility belongs to the CRRA family, \(u(c) = c^{1-\gamma}/(1-\gamma)\) with \(\gamma\) the coefficient of risk aversion.}
Consider now a consumer who cannot process all the information available in the economy to track precisely her wealth. This not only adds a constraint to the decision problem, but fundamentally affects each constraint (9)-(10) in four important ways.

1. Since the consumer does not know her wealth, (10) no longer holds. Her uncertainty about wealth is given by the prior $g(w_0)$.

2. Before processing any information, consumption is also a random variable. This is because the uncertainty about wealth translates into a number of possible consumption profiles with various levels of affordability. It follows that to maximize lifetime utility, consumers need to jointly reduce uncertainty about wealth and choose consumption. The consumer chooses the joint distribution $p(c, w)$.

3. With respect to the program (8)-(10), there is a new constraint on the amount of information the consumer can process. The reduction in uncertainty conveyed by the signal depends on the attention allocated by the consumer to track her wealth. We append the Shannon channel in equation (11) to the constraint sets. The information flow available to the consumer is a function of the signal, i.e. the joint distribution $p(\cdot c_t, \cdot w_t)$. In formulae:

$$\kappa_t \geq I(p(\cdot c_t, \cdot w_t)) = \int p(c_t, w_t) \log \left( \frac{p(c_t, w_t)}{p(c_t) g(w_t)} \right) dc_t dw_t$$

(11)

4. The update of the prior replaces the law of motion of wealth using the budget constraint in (9). To describe the way individuals transit across states, define the operator $E_{w_t}(E_t(x_{t+1})|c_t) \equiv \hat{x}_{t+1}$, which combines the expectation in period $t$ of a variable in period $t + 1$ with the knowledge of consumption in period $t$, $c_t$, and the remaining uncertainty over wealth. Applying $E_{w_t}(E_t(\cdot)|c_t)$ to equation (9) leads to:
\[ \hat{w}_{t+1} = R (\hat{w}_t - c_t) + \tilde{y} \]  

where,

\[
\tilde{y} = E_{w_t} (E_t (y_{t+1}) | c_t) \\
\equiv E_{w_t} (E_t (y_{t+1} | \mathcal{L}_t) | c_t) + [E_{w_t} (E E_t (y_{t+1}) | c_t) - E_{w_t} (E_t (y_{t+1} | \mathcal{L}_t) | c_t)] \\
\overset{LIE}{=} \bar{y} + E_{w_t} [(E_t (y_{t+1}) | c_t) - (E_t (y_{t+1}) | c_t)] \\
= \bar{y}.
\]

Given the initial prior state \( g(w_0) \), the next belief state \( g'_{c_t}(w_{t+1}) \) is determined by Bayesian conditioning as

\[
g' (w_{t+1} | c_t) = \int \bar{T} (w_{t+1}; w_t, c_t) p (w_t | c_t) dw_t,
\]  

which is the transition equation we have in the main text too. In (13), the function \( \bar{T} \) is the transition function representing (12) and the belief state itself is completely observable. Bayesian conditioning satisfies the Markov assumption by keeping a sufficient statistics that summarizes all information needed for optimal control. Thus, (13) replaces (9) in the limited processing world.

Combining all these points, the problem of the household under rational inattention can be expressed by the program in equations (2)-(6) in Section 2.
B Instructions for the Laboratory Experiment

We report below the instruction sets that were provided to the subjects during the experiment:

A. General Instructions

B. Instructions for Treatment 1

C. Instructions for Treatment 2

D. Instructions for Treatment 3
General Instructions

You will be paid in cash at the end of today’s study based upon the decisions you make, so it is important that you understand the directions completely. If you have a question at any point, please raise your hand, but otherwise you should not talk or communicate with anyone.

Overview

This study involves 3 paid phases that each last about 20 minutes (the exact time for a part is random). During that time you can complete as many periods as possible. Each period you can earn money and you will be paid based on the total amount of money you earn from all three paid phases. Your payment in $US will equal your cumulative earnings from all three phases divided by 500. Each of the paid phases is slightly different and you will be given specific instructions about each phase just before it begins. Before the paid phases you will go through a practice phase to familiarize you with the decision process.

How You Earn Your Payoff

Each period in a phase involves the following sequence of events.

Step 1. The computer determines the maximum prize available that period, but this is not revealed to you.

Step 2. You decide what information you would like to receive about the maximum prize that period.

Step 3. You attempt to solve some logic puzzles to earn information.

Step 4. You can go back to Step 2 and get more information or...

Step 4. ... Go ahead and pick a prize.

ATTENTION: If you pick a prize less than or equal to the maximum prize you earn the amount of the prize you pick. But if the prize you pick is greater than the maximum prize, you earn 0 for the period. So picking a larger prize could increase your payment, but it is riskier. The maximum prize does not change no matter how many times Step 2 is repeated.

Step 5. The next period begins.
More Details About Each Step

You will use the interface (shown below) to interact with the computer. The interface tells you the current period, the elapsed time in seconds, and your accumulated prizes. Then, it shows a grid of possible prizes available in the current and a row of blue buttons to choose the information you want to receive about the prizes.

<table>
<thead>
<tr>
<th>Time</th>
<th>Period</th>
<th>My Prizes</th>
</tr>
</thead>
<tbody>
<tr>
<td>208</td>
<td>5</td>
<td>69</td>
</tr>
</tbody>
</table>

Possible Prizes

Step 1: Maximum Possible Prize

Each period there are 256 possible prizes. For instance, in the practice phase the prizes are 1, 2, ..., 255, 256. The computer will randomly determine the maximum possible prize each period. You will not be told the amount of the maximum prize, but it is the maximum amount you can earn in that period.
Step 2: Information about the Maximum Possible Prize

Gathering information allows you to find out more about the maximum prize in the current period. The blue buttons at the bottom of your screen let you pick the type of information you would like to know.

Select from 256 Prizes: You get no additional information and select from all 256 prizes.
Select from 128 Prizes: You learn if the maximum possible prize is in the first 128 prizes or the last 128 prizes.
Select from 64 Prizes: You learn if the maximum possible prize is in the first set of 64 prizes, the second set of 64 prizes, the third set of 64 prizes, or the fourth set of 64 prizes.

... Select from 1 Prize: You learn the maximum possible prize.

In the screen shot above, we picked Select from 32 Prizes and we found out the maximum possible prize is in the 7th set of 32 prizes: somewhere between 193 and 224.

Step 3: Solve Logic Puzzles

To receive the information selected in Step 2, you have to solve some logic puzzles which work as follows. A 3x3 table of images will be shown to you. Each image has a particular shape, direction, size, color, border edge, and border corner. Some of these characteristics will change from row to row, column to column, diagonally, or from corner to corner. You have to identify the image that belongs in the lower right corner. The blue buttons include information on how frequently people have successfully answered the required puzzles in a previous study. Below is an example of a puzzle. In this example, the shape changes row to row while the size changes from corner to corner.
General Instructions

The better information you want to receive the harder the logic puzzles you have to solve. Typically, you have to correctly solve 2 puzzles. One exception is that you have to solve 3 very difficult puzzles to Select from 1 Prize. The other exception is that you only have to answer a single easy puzzle if you want to Select from 256 Prizes. The blue buttons on your screen indicate how often people who have participated in this study before have been able to correctly solve the necessary puzzles.

The computer will not tell you if you answer a logic puzzle correctly or not; all you will observe is the yellow range for the maximum possible prize if you solve the task correctly. If you do not solve the task correctly, the full set of 256 prizes will be yellow.

Step 4: Picking a Prize

Once you have obtained all of the information you want then you can pick a prize.

To pick a prize you simple click on it. If the prize you pick is less than or equal to the maximum possible prize then the prize you pick is added to your payoff. Otherwise you earn 0 for the period.

In the example above we know the maximum possible prize is between 193 and 224. Suppose you picked a prize of 199. If the maximum prize was 211 then you would earn 199. But if the maximum prize was 194 then you would earn 0. The maximum prize is not revealed. You will simply see your prize turn green if you receive the prize and red if you do not.

ATTENTION – TRADEOFF: You face a tradeoff in that better information helps you make better decisions about what prize to claim in a period but acquiring less accurate information enables you to claim more prizes during that phase of the study.

ATTENTION - TIME PENALTY: Your time will be reduced by 60 seconds (technically 60 seconds are added to your elapsed time) if you pick from the full set of 256 prizes and are unsuccessful. This can occur either when you decide to Select from 256 Prizes because you did not try to get better information or when you tried for better information but did not correctly solve the required puzzles. When all of the prizes are yellow when you pick a prize the time penalty is in play.

Summary

1. Each period the computer will determine the maximum possible prize.
2. You can solve puzzles to get information about the maximum possible prize. Better information requires you to solve more and harder puzzles.
3. If you pick a prize less than or equal to the maximum possible prize then the prize you pick is added to your earnings. Otherwise you earn 0 for the period.
4. The faster you pick a prize the more periods you will have (but there is a 60 second penalty if you unsuccessfully pick from all 256 prizes).
5. You will complete three paid phases, each of which lasts about 20 minutes. You will be paid based upon your cumulative earnings in each of those phases.

If you, have a question raise your hand. Otherwise, you can press the start button to begin the practice phase.
**Instructions for Treatment 1**

The next phase of the study will count towards your payment.

This phase is the same as the practice phase you did initially.

1. The 256 prizes are just amounts 1, 2, 3, ..., 255, 256 (as in the practice phase).

2. Each period the maximum possible prize that you could claim is randomly determined and equally likely to be any of the show prize amounts (as in the practice phase).

If you have a question raise your hand. Otherwise, you can press the start button to begin this paid phase of the study. One you press start your time will begin and you cannot pause it.
Instructions for Treatment 2

The next phase of the study will count towards your payment.
This phase is different from the practice phase you did initially.

1. In most periods the prizes are 0.1, 0.2, 0.3, ..., 25.5, 25.6, which is one-tenth of what they were in the practice phase. However, every 11\textsuperscript{th} period (that is in period 11, period 22, period 33, and so on) the prizes are 10, 20, 30, ..., 2550, 2560, which is ten times what they were in the practice phase.

2. Each period the maximum possible prize that you could claim is randomly determined and equally likely to be any of the show prize amounts (as in the practice phase).

If you have a question raise your hand. Otherwise, you can press the start button to begin this paid phase of the study. One you press start your time will begin and you cannot pause it.
The next phase of the study will count towards your payment.
This phase is different from the practice phase you did initially.

1. The 256 prizes are just amounts 1, 2, 3, ..., 255, 256 (as in the practice).

2. In the first period, the maximum possible prize that you could claim is randomly determined and equally likely to be any of the show prize amounts. After that, in any period there is an 80% chance that the maximum possible prize does not change from one period to the next and only a 20% chance that a new maximum prize is randomly drawn. Below are two examples of this process.

Example 1

<table>
<thead>
<tr>
<th>Period</th>
<th>Maximum Prize</th>
</tr>
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<tbody>
<tr>
<td>1</td>
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</tr>
<tr>
<td>2</td>
<td>235</td>
</tr>
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<td>11</td>
<td>66</td>
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<tr>
<td>12</td>
<td>151</td>
</tr>
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<td>...</td>
<td>...</td>
</tr>
</tbody>
</table>

Example 2
**Instructions for Treatment 3**

<table>
<thead>
<tr>
<th>Period</th>
<th>Maximum Prize</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>117</td>
</tr>
<tr>
<td>2</td>
<td>117</td>
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<td>189</td>
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<tr>
<td>...</td>
<td>...</td>
</tr>
</tbody>
</table>

If you have a question raise your hand. Otherwise, you can press the start button to begin this paid phase of the study. Once you press start your time will begin and you cannot pause it.
C  Individual Results by Subject

Figure A1: INDIVIDUAL RESPONSES TO INCOME SHOCKS 1 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A2: INDIVIDUAL RESPONSES TO INCOME SHOCKS 2 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A3: INDIVIDUAL RESPONSES TO INCOME SHOCKS 3 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A4: INDIVIDUAL RESPONSES TO INCOME SHOCKS 4 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A5: INDIVIDUAL RESPONSES TO INCOME SHOCKS 5 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A6: INDIVIDUAL RESPONSES TO INCOME SHOCKS 6 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A7: INDIVIDUAL RESPONSES TO INCOME SHOCKS 7 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A8: INDIVIDUAL RESPONSES TO INCOME SHOCKS 8 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
Figure A9: INDIVIDUAL RESPONSES TO INCOME SHOCKS 9 - Average income, consumption, and precision in the neighborhood of an income shock, centered at time 0. Two periods before the shock and the three periods after the shock are showed. All subjects in Treatment 3.
MEMORANDUM

TO: Justin LeBlanc
    Andrea Civelli
    Cary Deck

FROM: Ro Windwalker
      IRB Coordinator

RE: PROJECT MODIFICATION

IRB Protocol #: 16-03-639
Protocol Title: Rational Inattention in the Laboratory
Review Type: ☑ EXEMPT  ☐ EXPEDITED  ☐ FULL IRB
Approved Project Period: Start Date: 09/16/2016 Expiration Date: 03/27/2017

Your request to modify the referenced protocol has been approved by the IRB. **This protocol is currently approved for 200 total participants.** If you wish to make any further modifications in the approved protocol, including enrolling more than this number, you must seek approval prior to implementing those changes. All modifications should be requested in writing (email is acceptable) and must provide sufficient detail to assess the impact of the change.

Please note that this approval does not extend the Approved Project Period. Should you wish to extend your project beyond the current expiration date, you must submit a request for continuation using the UAF IRB form “Continuing Review for IRB Approved Projects.” The request should be sent to the IRB Coordinator, 109 MLKG Building.

For protocols requiring FULL IRB review, please submit your request at least one month prior to the current expiration date. (High-risk protocols may require even more time for approval.) For protocols requiring an EXPEDITED or EXEMPT review, submit your request at least two weeks prior to the current expiration date. Failure to obtain approval for a continuation on or prior to the currently approved expiration date will result in termination of the protocol and you will be required to submit a new protocol to the IRB before continuing the project. Data collected past the protocol expiration date may need to be eliminated from the dataset should you wish to publish. Only data collected under a currently approved protocol can be certified by the IRB for any purpose.
Chapter 3: Evaluating Iodiosyncratic Experimental Environments

1 Introduction

It well known that economics has come a long way since its inception as a formal and distinct discipline. Indeed, the earlier economists – such as Adam Smith and his contemporaries – practiced their craft in a way quite different than modern economists. In the early stages most economics, like other sciences, took the form of an abstract thought exercise. In many respects, economics started as a philosophy rather than a science per se. That is, most of the understanding of economic phenomena was based on empirical observations from which economists made generalizations. These generalizations were then taken to be the foundations for logical derivations that were explanations for ancillary phenomena.

Such was the case for both the physical sciences and the social sciences for quite a long time. Obviously calculus and an avalanche of discoveries in the world of mathematics dramatically impacted the progress of physics and served to propel it forward at unprecedented rates. The rapid progress of economics experienced a very similar accelerator effect when mathematical rigor began to be incorporated. Economics now takes on a highly mathematized form where we start with a few fundamental assumptions, and then we derive predictions after wrapping a model around the world in an attempt to explain it. These predictions take the form of falsifiable hypotheses.

There are, generally speaking, two ways in which we approach the examination of these hypotheses. Firstly, where the data are available, we “take the model to the data” and employ various econometric techniques to test the ability of the model to accurately capture what we observe in the real world. Secondly, we have the ability to test these hypotheses with experiments.

Experimental techniques in economics and the social sciences take on two different forms: field experiments and laboratory experiments. Both of these techniques have grown increas-
ingly popular over the last six decades or so; and the level of sophistication, too, has increased. This rise in sophistication is accompanied by a simultaneous rise in the complexity and difficulty of implementation.

It is not surprising that the early days of experimental economics was very basic from a technological perspective. Experiments were devised in such a way that the experiment could be administered under the constraints at the time. Often experiments were administered using paper and pencil. As the field has developed, the types of questions that researchers have been taking to the laboratory to test have become increasingly demanding – not only theoretically, but also from a technical implementation perspective.

The advent of computer technology has fundamentally changed the world, and economics was not an exception. It has enabled economists to ask much more difficult questions, build much more sophisticated models, and provide richer insight into the world around us. However, when conducting these researchers are quickly confronted with a problem – as the level of complexity of the model increases, so does the amount of resources required to either attain a solution to the model or test the predictions of those models.

Economists have long used mathematical models to understand and explain economic phenomena. As the types of questions being asked have become more challenging, the level of sophistication of these models has increased as well. Because of the level of complexity of these models, often analytical solutions thereof are difficult, if not impossible, to attain. Numerical solutions methods have thus become a prominent subject in economics literature. Unsurprisingly, an important factor in the discussion is their costs – both in terms of computational resources as well as the effort needed in order to actually write the code.

Taylor and Uhlig (1990) compare several different solution techniques for nonlinear rational-expectation models, and as a result, they conclude that “researchers might want to be careful not to rely blindly on the results of any chosen numerical solution method in applied work” because there is such variance in the results from each. They also report the computation
time for each of the methods, with the fastest reported being 20 seconds, and the longest being 5 hours.

Parra-Alvarez (2013) also examines solution methods, except for solutions to dynamic stochastic general equilibrium (DSGE) models. Parra-Alvarez notes that analytical solutions to DSGE models are only attainable “under very restrictive and economically uninteresting assumptions.” Again, expansion of the complexity of these models and the relaxation of assumptions causes analytical solutions to quickly become unobtainable, and therefore requires the employment of numerical solutions. Finding that computation times can be as much as 40 times slower than the simplest case considered, Parra-Alvarez concludes that in order to find a good approximation, a good starting point for the numerical method is necessary. Because this exercise alone requires a substantial amount of computation time, he states that it is sufficient to undermine the viability of a some of the econometric methods he examined.

Aruoba and Fernandez-Villaverde (2014) consider the issue of computation time along a different dimension. Conditional upon the same code, they investigate the computation speed of several of the most widely used coding languages, both within and outside of the field of economics. They find that solutions can vary by up to 4 orders of magnitude merely as a function of language differences, and this is without altering the algorithm used for the testing to take advantage of idiosyncratic features of each of the languages. They also report differences across operating systems used to run the codes.

It is clear that there is a trade off between results and the computation time, but there is another factor that is potentially even more critical to the decision of which solution method to utilize: the difficulty of implementation. This is consistently a topic of discussion in the literature, and, unsurprisingly, a challenging input to measure. If the researcher has prior experience in one coding language over another, switching to what may be a faster language may not receive a sufficient return on investment in order to justify the time it would take
Researchers in the field of experimental economics are confronted with an analogous problem. Although computation time, *per se*, may not be of typical concern, the ability for a given coding language or environment to accommodate increasingly complex experimental setups is a very common issue. This also leads to the problem of implementation, and results in the fundamental question: What coding language is best for a given experiment, and what is the investment needed in order to accomplish the given ends? A search of the literature yields no substantive prior work on this matter.

This paper seeks to enumerate the advantages and disadvantages of some of the most commonly used environments for the deployment of experiments, and is structured as follows. Section 2 discusses some of the interfaces that are available for implementing experiments. In Section 3, I present two experimental interfaces that I developed along with comments and insights garnered from experience with those unconventional languages and interfaces. Section 4 presents a quantitative comparison between the two languages used to code the interfaces presented in Section 3. Section 5 concludes.

2 Interfaces for Experimental Studies in Economics

One of the most substantial contributions to the experimental literature — and the discipline at large — is the release of the freely available z-Tree (Zurich Toolbox for Ready-made Economics Experiments) software environment (Fischbacher, 2007). For quite a long time after its release, z-Tree became a sort of gold standard for experimental implementations. The authors of the code constructed it such that the end users had to have little to no knowledge of coding languages used to engineer a graphical user interface (GUI). Since it was first published in 2007, it has been cited thousands of times as the environment in which experimental economic studies have been conducted. z-Tree offered experimentalists something that had not been previously available — a pre-made environment in which an experiment 

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could be developed at an unprecedented rate. One of the most impactful features is that it allows for subjects to quickly and anonymously interact with one another in the lab. This was something that previously was extremely difficult to achieve. Timing, graphical displays, and an intuitive way to control global variables are all features that make z-Tree particularly appealing.

z-Tree is not without its faults, however. In particular, the two most characteristics that precluded it from being the environment of choice in LeBlanc et al. (2016) is the fact that “seconds” is the most granular unit of time in the environment’s movement between stages and/or updating graphics. We had a specific demand on our graphics that required them to be updated every 0.2 seconds (at maximum), which immediately made z-Tree a nonviable solution. Further, z-Tree is not particularly powerful when it comes to rendering graphics and can be relatively slow in that capacity. Even when attempting to use z-Tree at its fastest – 1 second intervals – the demand on the graphics was too high. Additionally, there are a very limited set of multimedia file types that z-Tree is able to consume: .jpg, .gif, .png, .bmp, .wmv, .mpg, .avi, .wav, and .mp3, and it exclusively uses Windows Media Player to deliver video content. Several of these file formats, especially the audio and video, are outdated, and some are only usable on Windows machines. With that in mind, another limitation of z-Tree is that it is only able to be used on Windows machines. With alternative operating systems such as Linux, Google Android, and Apple’s OSX and iOS gaining increasing share of the operating system market, a nontrivial consideration is an experiment being able to be deployed across a variety of devices.

Given that z-Tree has several rather substantial limitations. It is no surprise, then, that over the years there have been several alternatives that have also emerged, such as Redwood (Magnani et al., 2016), California Institute of Technology and UCLA’s Multistage, and University of Anchorage Alaska’s Python Experimental Economics Toolkit (PEET). While each of these (albeit not mutually exclusively) are able to provide some functionality
that may have been unavailable in the z-Tree world, each of them have technical hurdles and shortcomings of its own. While z-tree may be relatively painless to set up and use for development, this is not necessarily the case with the others. Redwood requires an extensive knowledge of HTML and Javascript and how it behaves on a server system. PEET requires that the experimenter have a background in the Python coding language, which I have found to be an uncommonly used coding language in the field of Economics in general, in experiments in particular.

An experimentalist is not, of course, limited to these options. He or she always has the option to engineer software specifically for one’s use case. The means by which this decision is most optimally made is ultimately an economic decision. What are the specific functions that the software needs to be able to perform for the experiment to address the research questions? Does the researcher have a rigorous coding background, let alone a software development background? If so, what is the “learning curve” in order to get an operational experiment constructed in a timely fashion? If not, what is the cost of a software engineer to complete the coding on one’s behalf?

3 Two Use Cases

The aforementioned is not an exhaustive list that a researcher must ask himself or herself when deciding what route to take in the implementation of an experiment, but they are certainly a few of the most impactful. In what follows, I discuss in detail specific needs for two different experimental platforms, and some of those platforms’ strengths and weaknesses. In providing some detail for the specific use cases that were encountered in this body of work, I illuminate why those in particular were chosen. I also discuss the benefits associated with each of them, as well as some of the most substantial challenges faced while developing the associated experiments. Finally, for each I conclude with recommendations for how to make an informed decision as to which environment is best for your experiment, and how to go
about developing an experiment efficiently and effectively.

3.1 Optimal Price Setting Using Javascript

Our price setting experiment was ultimately an extension of Magnani et al. (2016). It was therefore reasonable to think that a possible solution for the shortcomings left by z-Tree could be compensated for by the environment which they chose to use to administer their experiment. Working in a new environment called Redwood, Magnani, et al. ran their experiment using a tailored Javascript code. This seemed like a logical place to start, and Ryan Oprea generously assisted in providing us with resources, codes, and contacts to help get Redwood up and running at the University of Arkansas. Unfortunately, due to the fact that it was a very new software environment, there was not a lot of documentation or troubleshooting resources from which to draw understanding. Ultimately, after coordinating with our own campus information technologists and others at U.C. Santa Barbara, we were not able to get Redwood fully-functioning.

This does not speak to Redwood’s viability as a possible solution for implementing experiments, but it does, however, underscore the extent to which technological problems can substantially inhibit the ability of researchers to implement their experiments. After working for several weeks to try to adapt what had already been done in Magnani et al. (2016), there was no indication that the technical issues would be overcome in a timely fashion, which culminated in a decision that needed to be made: How were we going to get software made that had the functionality that we needed? The decision was finally made by myself to follow suit with Magnani, et al. and use Javascript and HTML, but instead of within the Redwood environment, the code was developed entirely independently by myself. This decision was made with a strong assumption that it was a viable option based on the work of Magnani et al., even though I personally had no prior experience with coding in HTML and Javascript.
3.1.1 Experimental Design

The goal of our experiment was to test the underlying behavioral assumptions of the commonly used price rigidity “menu cost” models. The interface was presented to the subject in the form of a virtual pizzeria which sells its pizzas at a current set price. Subjects earned real payoffs by fulfilling outstanding orders. To accomplish this, the subject needed to check the boxes next to five ingredients that correspond to those in the order and “submit the order” by clicking a button. (I will refer to this henceforth as “baking a pizza.”) If all of the ingredients of the pizza are correct, then the subject will earn some profit.

Profit is determined by the difference between the current set price and the optimal price. When the experiment initializes, the optimal price began to move according to a random walk, with equal probability of moving up or down every 0.2 seconds. Profit was maximized by selling the pizza at the optimal price, and it fell off according to an exponential decay. The subject was free at any point in time to change the current set price to equal the optimal price by entering an “update code,” which consisted of entering a random five-number code. The subjects had five minutes per treatment to accumulate as much profit as possible.

Figure 1 shows a screen shot of the experimental interface the subjects faced. The upper left-hand corner shows the pane where the subject fulfilled orders. The five boxes at the top correspond to the ingredients for that order and were populated randomly and independently, meaning that a particular ingredient could appear more than once on a given order. The columns below contain ten possible ingredients, and the sorting of these ingredients was changed randomly for every order.

The lower left-hand corner of the screen was a pane where the subject could observe the movement of the optimal price in real time and its relationship to the current set price. The lower right-hand corner of the screen provided the subject with information regarding the profit that could be earned if submitting a correct order at that moment in time, as well as
total profits accumulated during the treatment thus far.

The upper right-hand pane was where the subject would enter the update code. The five boxes containing the code were randomly populated with a number between 0 and 9, inclusive. The columns below contained all ten of those numbers, and the sorting of these columns were randomized in order to mirror the task of baking a pizza.

As a representation of level of effort for this experiment, altogether the price setting experiment required approximately 4,440 lines of code for the main body of the experiment. The instructions pages, transition pages, and final landing page where the summary of the subjects’ performance required more than an additional 500 lines of code.
Figure 1: A screenshot showing the user interface with which the subjects interacted.
3.1.2 Learnings

There are two things that made this experiment relatively simple to implement: (1) there was no need for subjects to interact with other subjects, and (2) there was no need to pass information between windows. The most challenging component in this experiment was the need to have the plot dynamically update at a very rapid refresh rate without it having deleterious effects on the performance of the software. Because HTML and Javascript are used ubiquitously for the development of webpages and other online platforms, it was known that this coding environment was capable of addressing our needs. Further, we knew that it was sufficient to support the work of Magnani, et al. Although their experiment was quite a bit simpler in structure than ours, the most demanding facet was the fact that again the graphics needed to be dynamic, fast, and light on hardware.

In this context, by far the most convenient aspect of HTML and Javascript is the ability to use cascading style sheets (CSS). These enable the developer to code a parametrized feature and use it in different cases. For instance, in 1, the ovals around the different panes can be coded into a CSS, and then the colors, width, and height can be altered. This saves a tremendous amount of coding efforts and reduces the code’s verbosity.

Another added benefit of using HTML and Javascript was the ease of deployment. There are a lot of things that can make the deployment of codes out to a number of machines somewhat problematic. When using software packages, it is essential to ensure that the operating system, the hardware requirements, and even system administration rights are all consistent with the needs of the program. Javascript and HTML are digestible and deployable using nearly any common internet browser. Because these tend to already be present on nearly every machine on a university campus, developing a code to be run in that environment has a relatively few points of failure from a implementation perspective. Additionally, developing with the aim of deploying to a browser such as enables experimenters
to take experiments off of the conventional laboratory setting with rows of desktops to being able to implement using alternative devices such as mobile phones or tablets.

3.2 Rational Inattention Using C#

The implementation of an experiment capable of examining the behavioral underpinnings of rational inattention was much more complex — and challenging — than the price setting experiment. Due to the need to have a fully customizable interface, a custom software application was engineered using C# (“C sharp”). There are many different integrated development environments (IDE) available at no cost, but arguably the most robust of these is Microsoft Visual Studio, which was the IDE of choice for this research endeavor.

The structure of the experiment was aimed to most closely represent in a laboratory setting the environment modeled in Tutino (2013). The general concept of the experiment is as follows. The treatment begins with the subject receiving an income draw, $i$, from a uniform distribution ranging from 1 to 256, inclusive. The subject then has the ability to acquire a signal that gives them information as to the magnitude of that income level. The precision or quality of this signal is determined by the successful completion task wherein they solve logic puzzles. Successful completion yields a signal indicating a bin of precision $p$ that indicates a bin in which the subject’s income is located. The subject then has the option to invest more time in order to get more precise information concerning their income, or the subject may choose to consume an amount, $c$. The level of consumption maps to a level of surplus, $s$, which is the quantity that determines the subjects actual payoff at the end of the experiment. If the consumption level $c$ is less than $i$, then the subject’s cumulative potential payoff, $\pi$, becomes $(\pi + s)$. 
3.2.1 Interface

As can be seen in Figure 2, the primary interface window consists of three separate panels. The upper left panel contains information about the various signals available to be acquired by the subject. Here a column indicates the precision level (0 for the easiest to acquire; 8 for the most difficult), and another indicates the width of the signal associated with that level. For example, a Precision 0 signal provides a trivial signal of 1-256. This signal is trivial because it is known by the subject that his or her income has been drawn randomly from a uniform distribution from [1,256]. A column is also provided to indicate the probability of successfully obtaining that particular signal. These probabilities were determined empirically by a number of calibration studies. Finally, a column indicating the number of puzzles that must be solved and what difficulty they are on a scale from 0 to 6 was provided.
Figure 2: A screenshot showing the primary user interface with which the subjects interacted.
The panel in the lower left is the area where the subject decides his or her level of consumption. In order to help the subject know their standing at any point in the experiment, a box is displayed dynamically. If the subject needs to acquire a signal in order to consume, a yellow box that says, “Please acquire a signal in order to attempt consumption is displayed.” Upon the successful acquisition of a signal, the box is replaced by a green box that says, “You are now ready to consume!” When this happens, the “Choose Consumption” button also appears. The subject then will use the scroll bar to adjust the level of consumption they would like to attempt to consume. Adjusting the scroll bar will indicate the level of consumption and, in realtime, the associated level of surplus will also be displayed. Once the preferred level of consumption is selected, the subject then clicks the “Choose Consumption” button.

The panel to the lower right shows the subject their earned cumulative profit thus far in the experiment. When the subject clicks “Choose Consumption,” if the subject has sufficient income, the cumulative surplus box will indicate this successful consumption by increasing by the appropriate amount, and the background color will turn green. In the event that the subject does not have enough income to purchase the consumption, then the background color of the box will turn red.

The upper right corner has a panel in which the subject may find information regarding the current state of the experiment. It shows a timer for the current treatment’s duration, and the period in which the subject is currently located.

In order to consume the subject must at least attempt a signal. This is done by solving a logic puzzle developed by Civelli and Deck (2017). An example of one of these puzzles may be seen in Figure 3. Unsuccessful attempts may not provide a signal, but the subject may still attempt to consume at his or her will. However, a penalty on an attempt to consume beyond one’s means was imposed. This was done for several reasons. First, it accounts for the possibility that the subject could move through the puzzle task without
any attempt to correctly answer. It could be imagined the the subject could set the chosen level of consumption extremely low, thereby increasing the chances that he or she would be successful at a consumption attempt. Doing this repeatedly would potentially exceed the potential payoffs obtained by carefully answering the puzzles in order to ascertain the true level of one's income in order to maximize total consumption every period.

Figure 3: A screenshot showing the user interface in which the subjects solved puzzles in order to obtain signals.

A period consists of the following.

1. Choose precision of signal to be attempted to acquire.

2. Answer the puzzles.
3. If successful in answering the puzzles, a signal will be provided. If unsuccessful, no signal will be provided. Here, the subject may choose to attempt to correctly solve the same level of precision again (if failed) or to choose a higher precision level.

4. Once the signal that the subject has been acquired, the subject attempts consumption. This process is called one “period,” and the subject is able to go through as many periods as possible in the duration of the treatment, and there were a total of three treatments.

Again, as a metric to indicate level of effort, the main experiment window (Figure 2) and all of its functionality consisted of approximately 3,110 lines of code. The logic puzzle task shown in Figure 3 required an additional 5,650 lines of code, while the starting page, an interactive walkthrough, and several pages of instructions required more than an additional 1,500. Altogether, the C# version of the Rational Inattention experiment well exceeded over 10,000 lines of code.

3.2.2 Learnings

Again, the needs for the rational inattention experiment were substantially more complex than in the price setting experiment. The demand on the graphics was not nearly as high, but the experiment was such that it needed multiple windows — or, forms — to pass information to other forms. There are a number of ways to do this, such as using a SQL database to store data from one form and using another forms to query that database to retrieve relevant data. Similar functionality could also be achieve using simple flat files (e.g., CSV or text files) stored to local hardware, and then again using the program to read/write again to that file in order to retrieve the data needed. Processes that require read/write typically add unnecessary overhead, and, as a rule of thumb, might be best to avoid to ensure stable performance of the program, not to mention efficient use of the hardware. Although employing these methods by no means would have resulted in a program too bulky
to operate properly, it *would* require a careful understanding of the timing and sequence of events in order to ensure that the relevant data are being stored properly before the program tries to read them from the database. One poorly-timed record could undermine the entire experiment’s data acquisition, not to mention could actually cause the code to crash if the data are essential to its operation.

Global variables can be a convenient way to deal with this need. Global variables can allow parameters to be perpetuated throughout the application without need to actually read/write the information intended to pass on from one form to another to disk. Of course, typically these data need to be recorded, but because ultimately the information needed to keep the experiment running are being maintained in real time in the background, the recording of data can be thought of as passive, and therefore not a critical function for making sure that the code is able to run without experiencing internal inconsistencies that might jeopardize the functionality of the code.

Further challenges were encountered due to the level of interactiveness desired for the application. Instructions that explained the behavior of the application in order to ensure that subjects understood the problem they were facing made the logic in the background extremely complex to code and very tedious to debug. Further, having a dynamic interface that only allowed certain actions or displays under certain conditions served to add to the points of failure in the code.¹ These complexities also make it difficult to consider “lines of code” as an accurate representation of the amount of effort that went into development. The operations to change the application’s performance, behavior, and displays based on decisions that the subject was making did not only require additional coding, but the implementation of the logic was extremely complex and required a great deal of attention to detail. An extremely important lesson was learned here: There is merit to the old adage “Perfection is

¹A sample of the part of the instructions prepared for this version of the experiment are included in the experiment as a means to demonstrate just how complex the application became.
the enemy of the good.” It is prudent to remember that while some sophisticated features might be *ideal*, sometimes that comes with a substantial cost, which in this case materialized in time to develop and a tremendous amount of points of failure making the debugging process extremely difficult.

4 Performance Comparisons

From a technical perspective, the best way to decide on the language to use for an experiment is dependent on what functions exactly the interface needs to be able to do. It is important to ensure that the end product will not be too slow or unstable to actually be able to use to collect data. Thus, having an understanding of the computational resources required for certain tasks is very useful. I have taken three operations that are common in experimental applications and executed them in both environments that I have implemented experiments, as well as in z-Tree for comparison purposes. These three operations are image loading/rendering, real time plotting, and data storage. As can be seen in Table 1, I find that for each operation, either C# or HTML/Javascript is clearly better from a computational resource perspective. For image loading and rendering, I find that between the three environments, one must choose to optimize either memory consumption or timing.
Table 1: Performance metrics for three tasks across two different coding environments.

<table>
<thead>
<tr>
<th>Task</th>
<th>HTML/Javascript</th>
<th>C#</th>
<th>z-Tree</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>RAM</td>
<td>Time [ms]</td>
<td>RAM</td>
</tr>
<tr>
<td>Image Loading</td>
<td>435KB</td>
<td>311</td>
<td>1.3MB</td>
</tr>
<tr>
<td>Realtime Plotting</td>
<td>4.4 MB</td>
<td>N/A</td>
<td>7.6MB</td>
</tr>
<tr>
<td>Data Storage</td>
<td>N/A</td>
<td>0.298</td>
<td>N/A</td>
</tr>
</tbody>
</table>

Figure 4: A screenshot showing Visual Studio’s diagnostic tools.

Data for the C# application performance were obtained using the native code evaluation tools in Microsoft Visual studio (part of which shown in Figure 4 and using custom-coded timing mechanisms. Data for the performance of the HTML and Javascript were obtained using Google Chrome’s Developer Tools (see Figure 5 for an example as to what these tools look like), which is an industry standard way to evaluate a program or processes. Evaluations
for z-Tree’s performance were used by custom coding in global timing variables and Window’s Resource Monitor (see Figure 6).

Figure 5: A screenshot showing one of the panes in Chrome’s Developer Tools.

4.1 Image Loading

Image loading is an extremely common function necessary for the administration of experiments. One could imagine a case where there would be the need for a very large number images to be read sequentially, so understanding demand on local memory is important to ensure that the application or machine does not crash. Further, if the refresh rate needs to be at a certain cadence, knowing the read time required for a given language is also imperative. For each language I wrote code to read in two images, one of file size 25KB with dimensions of $610\text{px} \times 410\text{px}$ and one of file size 20KB with dimensions of $480\text{px} \times 420\text{px}$. The C# application was already launched when it would load an image. Upon clicking of a button,
a new window would open, and it would read the images upon initialization. To mimic this, a simple “launch page” was coded in HTML with a simple button on the page. Clicking this button would launch a new tab and the images would be read in as it opened. This operation was mirrored closely in z-Tree by initially launching a “zLeaf,” which acts as a “launch page” before the experiment is started, which is equivalent to clicking the aforementioned buttons in C# and HTML.

I find that on average, reading in both of these images takes approximately 311 milliseconds in HTML using the Google Chrome browser, whereas the same operation took 0.114 milliseconds in C#. It was very difficult to measure the rendering speed using z-Tree. One may only call the backend timestamp using a “program,” and a “program” may only be executed at the beginning of a stage. Therefore, it was only possible to get that metric by transitioning into another stage, which cannot happen any more quickly than 1 second. However, it is worth noting that as soon as the stage begins, the images are virtually immediately present.

With respect to memory, I find that Google Chrome requires only 435KB of memory while C# requires 1.3MB. It is important to note here that in Google Chrome, as one opens
the new window and closes it, the local memory in the application resets automatically. That is, monitoring memory from the “launch page,” the new window only causes a brief increase in memory usage, and then will reset (often referred to as “garbage collecting” by developers). In the C# application, however, I find that the memory does not reset\(^3\) Image loading in z-Tree absorbed the most resources with a total of 1.8MB, but once moving to a new “stage,” the memory was reset automatically.

### 4.2 Real Time Plotting

Real time graphics are often a desired component to experiment, but as already discussed, a lot of coding environments in which most experiments are developed are not well suited to tackle such a task. z-Tree, for instance, is not capable of meeting this requirement, so therefore it was not possible to evaluate it. Both HTML and C#, however, are well-equipped for this; but knowing how each handles it from a load bearing perspective is critical. To evaluate the HTML use case, I used the same code that was used in the experiment in LeBlanc et. al (2014). Code specific for this task did not already exist in C#, so I developed a simple application to draw a line across the screen, using similar logic to that in the HTML code.

The Chrome developer tools include the Allocation Profiler, which allows you to record a session of a webpage and to analyze various dimensions of its performance as a function of time. Started the realtime plotting and recorded several profiles for 5 minutes in duration. I was then able to isolate the memory burden due to the plotting functions, and this consistently came out to be 4.4 MB. The Diagnostic Tools in Visual studio are quite different from, but the developer is still able to view the distribution of memory as a function of time. However, because there is an actual application running, there is intrinsically memory being

\(^3\)While it is certainly possible to have the code clean its memory upon the close of the window, I have not implemented that functionality in order to make the operations of the codes be as close to one another as possible. Since I did not specifically code for Chrome to reset, I chose not to do this in C# either.
consumed by that process. To account for this, I recorded the memory usage before the line began to be drawn and then let the program run for 5 minutes. I found that the difference in memory usage that can be attributed to the drawing process to be 7.6MB.

4.3 Storing Data

Saving data is an essential part of any experiment. How to construct the output is critical in the development process, and although it is not a glamorous aspect, improper data engineering could result in a number of problems, such as the inability to test various hypotheses that may arise once the experimental data have been collected\(^4\). On the other hand, careful consideration of how to record output can even enable researchers to reconstruction of a subject’s behavior throughout an entire experiment. But with the increasing complexity of experiments also contributes to an increase in the amount of data that will need to be stored, and if there are a significant number of actions to be recorded in rapid succession, it is important to understand the timing capabilities of how data are recorded.

I measured the time it took for Chrome to write a line to memory by inserting timing functions in the code. I found that on average, Chrome could write a line of data to memory in 0.298 milliseconds. Using a similar method to time it takes C# to write a line of data to memory, I found that it took no measurable amount of time. Writing a line to memory and storing the data at the conclusion of the experiment is not the only way to record data. It is common to have a program create a blank text file and then write lines to it when certain events occur. Using C#, this approach takes an average of 3.8 milliseconds to accomplish,

\(^4\)The fundamentals of the data engineering that is required to properly record an experiment is not the subject of this section, so I will no expound upon it in great detail here. However, I would encourage anyone constructing an experiment to think very carefully about every action that the subjects will be making and to consider how the codes that will be consuming the recorded data, and even possibly consult with a professional data engineer before collecting data. This can preempt a number of problems, two of which are most substantial being: (1) not being able to test hypotheses because sufficiently granular data were not recorded or were not recorded properly, and (2) it will reduce a great deal of coding necessary to reconstruct and clean data after the experiment is concluded.
while z-Tree was able to write data to a text file in less than a millisecond. To the best of my knowledge, HTML and Javascript are not well equipped for this approach, instead relying on outputting the data as a whole from memory once it has all been collected.

5 Conclusion

It is no small feat to publish a paper, and experimental economics is not exception. From the formulation of hypotheses, to discerning how to properly test those hypotheses with live subjects in a laboratory setting, to the data analysis and writing, it is an enormous undertaking, as any experimentalist may attest. The *implementation* of all of those ideas is often a stage that gets very little attention once the paper has come together, but it is a nontrivial portion to the entire process. Knowing which environment is most suitable to your experiment’s needs can not only save time and a lot of labor, but it can make the capabilities the researcher is able to implement richer and more robust.

The purpose of this work is to present two very different use cases and to provide insight to the strengths and weaknesses for each of the implementation methods. Further, this work finds a place in the literature alongside many other works written to help economists understand the computational advantages and limitations for some commonly used processes in the field of experimental economics. It is to be expected that there is not a “silver bullet” that solves the needs of every experiment, but the data presented here can help guide future researchers to a platform that is most suitable to their needs.

5It should be noted that z-Tree’s backend timing resolution is limited one millisecond.
6 References


Rational Inattention Instructions

This is where you can find information about the signals you can acquire, including difficulty level and probability of answering correctly. About the signals you can acquire, including difficulty level and probability of answering correctly.

This is where you find information about your cumulative payoff. It will turn **GREEN** if you had enough income to afford your cumulative payoff. It will turn **RED** if you could not afford the consumption you choose, and it will turn **RED** if you could not afford the consumption you choose.

This is where you find information about the signals you can acquire, including difficulty level and probability of answering correctly. About the signals you can acquire, including difficulty level and probability of answering correctly.
Cumulative Payoff | Pay Duration
---|---
1 | 00:00:09

Please select the precision level of your signal.

Please acquire a signal in order to attempt consumption.

You acquired a signal in the following range: NO SIGNAL

Line the slider bar to select the level of consumption you desire.
Once you have chosen a signal precision and completed the task, a green box appears to indicate that you are ready to consume! You are able to consume whether or not you actually acquired a signal. If you successfully acquired a signal, it appears here.

You are now ready to attempt to consume!

Use the scroll bar to choose your attempted consumption level and the corresponding potential payoff.

After acquiring a signal, you are allowed to also choose a signal of higher precision. You are not able to choose a signal of lower precision. You choose to acquire a signal of higher precision. You acquire a signal.
But be careful! If you didn’t successfully acquire a signal or choose precision level 0, and you try to consume more than your income, then you incur a 1 minute penalty! If you try to consume more than your income, then the cumulative payoff box will turn RED until your next consumption attempt. If you try to consume more than your income, then the cumulative payoff number will turn RED until your next consumption attempt.
Notice that the Choose Initial Consumption button disappears during the 60 second penalty. It will reappear after the 60 second penalty.

Notice that the Choose Initial Consumption button disappears during the 60 second penalty. It will reappear after the 60 second penalty.
Here a signal of precision Level 4 was successfully obtained. This tells you that your income is between 33 and 48, inclusive. This means that you should never attempt to consume less than 33 or more than 48. However, if you do attempt to consume greater than your income, you will not incur a penalty. Since you successfully acquired a signal, if you do attempt to consume greater than your income, you will not incur a penalty.
Conclusion

The purpose of this body of work has been to investigate the behavioral foundations of two of the most prominent macroeconomic models. Learnings from these experiments also provided insight into the advancement of experimental economics over the last few decades and to what increasing expectations might look like in the future.

Chapter 1 provides evidence that while subjects in general understand that state-dependence is the optimal approach for making decisions with regard to updating prices, they also find it difficult to ascertain a precise threshold. Chapter 2 provides experimental evidence that subjects exhibit behavior explicitly predicted by the rational inattention model, including adjusting the amount of information processed according to the underlying economics environment and responding asymmetrically to positive and negative shocks in income. Finally, Chapter 3 provides quantifiable metrics by which experimenters are able to understand the resources that common tasks require from a computational perspective. Moreover, insight is given into how the advancement in sophistication of experimental economics increasingly puts demands on economists from a software engineering perspective, very much akin to the way that the necessity for numerical methods for solving complex models put demand on economists from a computer science perspective.