Essays on Information, Cognition and Consumption

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ABSTRACT

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This dissertation examines how agents process information and update their beliefs in two different contexts. In the first two chapters we consider dynamic decision problems under perfect information. In the last chapter we consider static, strategic interactions with common knowledge but imperfect information. To tackle our first set of questions we design an experiment analogous to the dynamic consumption problem with stochastic income that households solve in standard macroeconomic models. In the first chapter we show that our subjects condition on past actions in the absence of informational frictions or switching costs. We argue that subjects do so to economize on scarce cognitive resources and develop a model of inattentive reconsideration that fits our data. An implication of our model is that inertia is state-dependent. In the second chapter we revisit the longstanding problem in empirical macroeconomics of excess sensitivity of consumption to income in our experimental data. We find that excess sensitivity arises from two distinct channels. The first channel is an overreaction of households to the arrival of income that is independent of their wealth level. The second is increased excess smoothness with respect to wealth when households receive news about future income. The third chapter examines the scope for persuasion in global games. We consider a central bank with a commitment technology that chooses a robustly optimal persuasion strategy. We show that such

a policy can reduce and even eliminate multiple equilibria in such games because it updates agents beliefs so that coordination motives become irrelevant. This suggests that central bankers are better served from influencing the markets through announcements rather than direct intervention.

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Acknowledgements

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Dedication

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Chapter 1

Deeper Habits

with Mel Win Khaw

Introduction

One of the most striking counterfactuals of economies populated by purely forward looking agents is the sensitivity of responses to shocks. As a result, a central concern of macroeconomic models in recent decades has been microfounding the presence of backward-looking terms—*inertia*—in the policy functions of agents. Although there has been a lot of progress, both empirical and theoretical, on the microfoundations of inertia on the firm side less attention has been paid to households. The purpose of this paper is to address the question of inertial behavior in consumption/savings decisions.

To achieve inertia in aggregate consumption, macroeconomists appeal to non-time-separable preferences: habits. Although estimates of habit parameters from aggregate data are positive and significant, there is little direct evidence for these types of preferences. Nor can the the consumption time series allow us to distinguish among different explanations for inertia. In fact, macroeconomists justify the use of habits in preferences *only* on the basis that they help to match empirical moments. As Nakamura and Steinsson (2018) put it, habits "allow the model to better fit the shapes of the impulse responses we have estimated in the data."

But evidence for inertia in consumption is not the same as evidence for habits in preferences. And distinguishing among the possible mechanisms that give rise to inertia is of particular interest to macroeconomics: understanding how agents *make choices* is crucial to evaluating the effects of alternative policies. Given the importance of teasing out this distinction and the difficulty of doing so with aggregate data, this paper runs a lab experiment in which we have subjects play a game that is analogous to a consumption/savings problem. We identify cognitive costs as a source of consumer inertia and develop a model to account for the inertia observed in our data. Importantly, agents in our model *choose* to condition on past choices optimally. We argue that these are *deeper* habits in the sense that they are not simply assumed—as is the case with non-time-separable preferences—but rather are a result of agents' economizing on cognitive resources. Consequently, these deeper habits are sensitive to changes in the state and therefore are not policy invariant.

Our experimental design has our subjects play a game that requires them to accumulate as many valuable assets as possible. Each period our subjects make a simple binary decision whether to accept or reject the asset on offer. The value of each asset is drawn from a known distribution. To pay for the assets our subjects are endowed with tokens which they receive randomly from time to time. In the case they decide to buy the asset on offer, they must pay one token. If they have no tokens, they cannot not buy the asset. To induce discounting, at the end of each period the game has a fixed probability of terminating. Once the game ends, subjects are paid only for the total value of all the assets they have bought; any residual tokens have no value. Although highly stylized, our framework captures the dynamic tradeoff at the heart of consumption/savings problem. Our subjects must decide whether to purchase an asset today versus the probability of being unable to buy a better offer in the future if they hit the credit constraint. In the same way that wealth can be turned into consumption, tokens can be turned into assets, but not the other way around. Tokens, like wealth, have value only because they represent future consumption.

By studying a consumption/savings problem in the lab, our paper makes two important empirical contributions to the consumption literature. First, we are able to find inertia in *choices* rather than simply as a feature of aggregate data. This first piece of evidence favors writing down models in which the households euler equation for consumption contains backward-looking terms. Further, we are also able to identify cognitive constraints as the source of this inertia in our subjects. This finding is important because it points toward a more rigorous and realistic microfoundation of households in general equilibrium models. A third contribution of this paper is theoretical. We propose a model of choice disciplined by the evidence in our data that can be adapted in a straightforward manner to DSGE models. An important advantage of our methodology is that lab-generated data can help us narrow the ways in which we model deviations from the rational expectations benchmark. In the model of rational inattention we propose, agents pay a fixed cognitive cost every time they "think through" a problem. As a result, they condition on past actions as a way to forego this thinking cost. Because our agents are rationally inattentive, they choose the probability of reconsideration conditioning on the state and their past actions. Our model also has the implication—at odds with the habits from preferences model—that the degree of inertia depends both on the policy rule and the realization of the state.

The fact that our experimental design shuts down any external motives to condition on past choices makes allows us to rule out some of the alternative explanations in the literature. We take care to eliminate external costs of switching, which is the mechanism that leads to inertia in models like Klemperer (1995). Under the commonsense assumption that our subjects seek to maximize their expected payoff from participating in our experiment their objective is time-separable. This feature rules out habits from preferences as in the models of Ravn, Schmitt-Grohe and Uribe (2004). Laibson (2001) develops a behavioral model were external cues may condition preferences and lead to habits. Like ours, his is a cognitive model, but the process he has in mind is quite different from ours. His model is in the spirit of Becker-Murphy rational addiction models. Our model is much more closely tied to the rational inattention literature, specifically, the model developed by Woodford (2009).

Although we model cognitive constraints broadly as a rational inattention problem our framework allows us to distinguish among several of the inattention models as explanations for our data. Our subjects have access to all of the information they need each period in order to make their choice and no time limit. Information is not sticky in the sense of Mankiw and Reis (2002) or Reis (2006); unlike their models were consumers forgo any new information for several periods the information is on the screen every turn. If anything it is harder to ignore than to read. Similarly, it is difficult to appeal to models of exogenous imperfect information such as Woodford (2002) and Lorenzoni (2009). Angeletos and Huo (2018) show that higher order beliefs can anchor outcomes on past behavior. This hinges on the joint assumption of imperfect information and strategic complementarity; our experiment has subjects solve a decision problem and thus there are no strategic considerations.

Our paper is most closely related to Matyskova *et al* (2018). Like us, they find evidence of state-contingent habits in an experimental setting. Their focus is somewhat different, yet complementary to ours. The purpose of their experimental design is to distinguish between two alternative cognitive explanations for habit formation. To that end their subjects are asked to identify the outcome of a binary random variable through time. In contrast we are interested in whether subjects display inertia in a specific context relevant to macroeconomics: while solving a consumption/savings problem where past actions are not directly payoff-relevant. Crucially, they conclude that habits in their choice data are not formed mechanically. Their findings that subjects condition on the past as a way to alleviate cognitive costs is in line with our own interpretation of the experimental results presented here.

To be clear, our experiment cannot rule out the existence of habits from preferences. We cannot take a stand either way on whether preferences are time-separable. Likewise, the fact that we make all relevant information available and eliminate any switching costs in the lab does not mean that we believe these conditions necessarily hold when households make consumption/savings choices. Rather than a weakness, however, this is precisely the strength of our approach. All of the channels that we shut down in our experiment are hard, if not impossible, to account for when looking at field data. The fact that the information set of the econometrician is not the same as that of the agents has long been an issue in the econometrics of rational expectations. This asymmetry is not present in the lab; we do not need any auxiliary assumptions about preferences, external constraints, or the stochastic nature of the data-generating process. Nor do we have to worry about measurement error of the relevant variables. Measuring the wealth of individuals in cross-section or panel data, for example, is notoriously difficult. By contrast, in our experiment it is trivial.

In a deeper sense, the strength of our experiment lies in that it sheds light on how subjects *make choices* when faced with a consumption/savings problem. We do not set out to measure inertia ex ante; actually, we make sure that there is no reason for our subjects to display it. The fact that they do is very strong evidence that cognitive limitations are at least part of the source of inertia in field data. Another way to interpret it is as a *lower bound* on the degree of inertia. Furthermore, our approach also uncovers an important result that is relevant for policy analysis: the degree of inertia depends on the stochastic process and the realizations of that process. Our model implies that inertia will be at its weakest when rare but large shocks occur. Consequently, models with state-independent inertia, like the standard habits-from-preferences models used in macroeconomics may overstate the degree of sluggishness of aggregate demand in “crisis” periods.

Section 2 outlines the experiment while section 3 solves the problem faced by our subjects. In Section 4 we establish our main empirical result—inertia. Section 5 introduces a model of inattentive reconsideration that matches our findings and within this framework we provide estimates for the cost of inertia. We consider some alternative explanations for our findings in section 6. Section 7 concludes.

Experimental Design

We describe the problem to our subjects as a real estate investment game where subjects’ objective is to accumulate as many “rents” from purchasing “properties” as possible. “Rents” in the context of our experiment are not the same as rents in the way economists usually understand them; we simply call them this because of how we frame the game to our subjects. In order to make the problem as simple as possible, the rental payments do not accrue over time so subjects do not have to calculate the discounted present value of each property. The “rent” on each property is paid to the subject at the moment it is bought and never again. If the subject buys the property on offer she receives the one-time rental payment but nothing otherwise. The rents of the properties range from 500 to 100,000 in increments of 500 and are drawn independently each period from a discretized log-normal distribution.

All properties have the same price of $1,000,000 in cash and subjects start each game with a cash stock of $11,000,000, which is the mean of the ergodic dis-

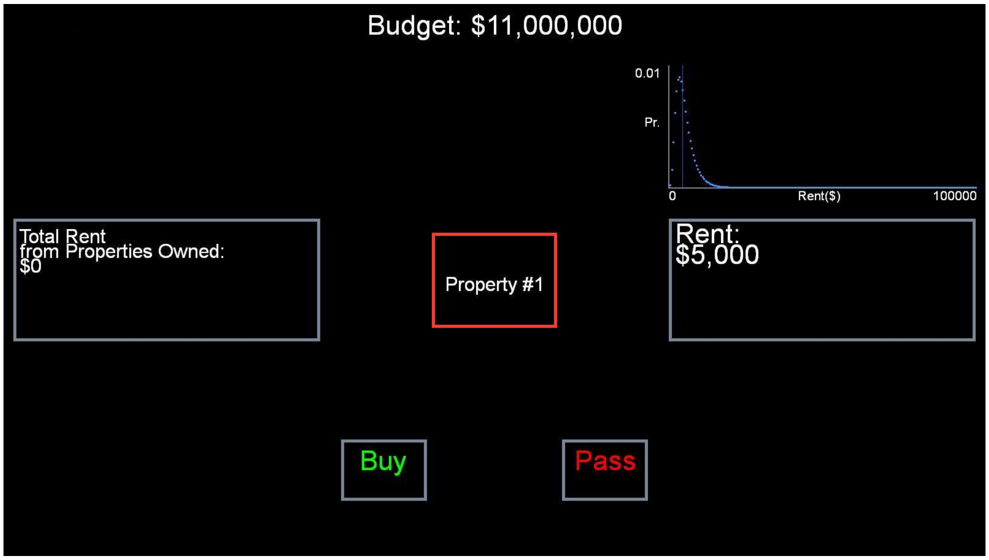


Figure 11: The computer screen during a single trial of our experiment. While subjects deliberate, we present to them information regarding the distribution of possible rent values, the current rent value, the accumulated rents of the subject, as well as the current budget available to be spent.

tribution of cash under the rational expectations policy. With a probability of 10 percent subjects receive –and are notified– of an income payment of $4,000,000 at the start of each turn. There is no interest nor any depreciation of cash so that the cash at the end of each turn carries unchanged into the following turn if the game does not terminate. At the end of each period the game ends with probability .002; this implies that the average game lasts 500 rounds. Upon termination of the game subjects are paid *only* for their total accumulated rents at an exchange rate of .00001. In dollar terms this implies that the rental payments on the properties range from half a cent to 1 dollar. Rents and cash, like consumption and wealth, are separate instruments. Subjects can turn cash into rents, but they cannot turn rents into cash.

Figure 11 shows a screenshot of the experiment in the first period of the game. Center top of the screen is the budget available to the subject. In the upper right hand corner is the discretized log-normal distribution of the rents with a vertical line that intersects the x-axis at the rental payment of the current property on offer. Below the graph of the probability mass function is the value of the rental payment associated with the property on offer. The center of the screen shows the number of turns played, inclusive. Center left is the running total of rental payments that subjects have received. Recall that each property offers only a one-time rental payment, so this number only increases when a new property is purchased.

Once subjects press a button, we do not ask them to confirm their choice, but we verify the consequences of that choice. Upon making a decision the corresponding deduction from their budget (-$1,000,000 or $0) along with the addition to their rental payments (current rental value on offer or $0) are displayed for 1.5 seconds. After this the game either continues onto the next round or terminates. At the beginning of each turn the mouse cursor is reset to the midpoint between the "Buy" and "Pass" buttons.

Figure 12 shows the other two screens that subjects see during the experiment. In the event that subjects receive income they see the left panel of Figure 12 for three second before being taken automatically to the interface in Figure 11. The screen displays both the income payment, which is always $4,000,000 and the budget with which they ended the previous period. This is the only notification of income. In the case of Figure 12, for example, once the subject is returned to the main interphase, their budget would display $13,000,000.

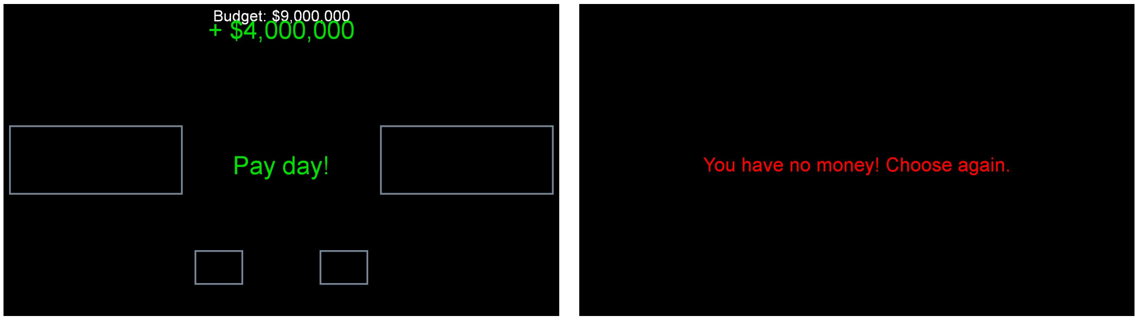


Figure 12: On trials where participants receive income, a notification of "Pay day" is displayed on screen for 3 seconds before subjects are allowed to make a decision (returning to the default interface in Fig.1). If subjects decide to purchase with no remaining money in their budget, subjects are displayed a notice in red for 1.5 seconds before they are returned to the decision screen.

When subjects exhaust their budget the main interphase continues to display, rental offers continue to be drawn and both the "Buy" and "Pass" buttons remain active. If subjects choose the "Buy" button, however, they are taken to the screen shown on the right panel of Figure 12. The notice displays for 1.5 second before subjects are taken back to the previous screen: the game *does not* continue. When credit constrained, subjects can only move on from their current round by pressing the "Pass" button.

Our experimental setup is designed to make it as easy as possible for subjects to enact the policy of a fully-informed agent with rational expectations. We provide agents with all of the necessary information to make their choice and make the two relevant variables – Rent and Budget – the same color and size. By resetting the cursor between the "Buy" and "Pass" buttons we remove any external costs of switching actions from period to period. We also allow our subjects as much time per round as they want to remove any possible external information processing constraints. Furthermore there are no time-keeping displays on the screen that might highlight to subjects the time it takes them to complete each round.

The main virtue of our experimental setup is that we can minimize the additional assumptions that are often needed to test hypotheses using field data, particularly in macroeconomics. We do not need to rely on conjectures about the objectives of our subjects, the information that is available to them, or the nature of the data-generating process. First, we have designed a game with an objective that rules out the need to condition on past choices. Second, our implementation of the experiment minimizes any external frictions that could impose additional costs or constraints beyond the rules of the investment game. This gives us confidence that problem misspecification on our part cannot account for possible deviations in our data from the full-information rational expectations policy. We can therefore interpret any such deviations as coming from cognitive constraints.

Agent's Problem

Consider an agent starting turn t with wealth $\tilde{x}_t$. She must choose sequences of actions $c \in \{0, 1\}$ contingent on each possible history of wealth and rental values that she may face in subsequent periods $t + k$. Formally, she seeks to maximize the following objective:

$$\begin{aligned}\max_{\{c_r(x_k)\}} \mathrm{E}_{f(x_t,r|\tilde{x_t})}[rc_r(x_t) + \delta \mathrm{E}_{f(x_{t+1},r|\tilde{x}_{t+1})}[rc_r(x_{t+1}) + \ldots \\ + \delta^k \mathrm{E}_{f(x_{t+k},r|\tilde{x}_{t+k})}[rc_r(x_{t+k}) + \ldots \qquad (1.1)\end{aligned}$$

where r is the rental value and the expectations at $t+k$ are taken conditional on each possible value of wealth at the end of period $t+k-1$, $\tilde{x}_{t+k}$. Because the rental draws are *iid*, the marginal distribution of r does not depend on k. In fact, the joint distribution $f(x_{t+k}, r|\tilde{x}_{t+k})$ is the product of the marginals. The distribution of $x_{t+k}|\tilde{x}_{t+k}$ is $\tilde{x}_{t+k}+4$ w.p. α and $\tilde{x}_{t+k}$ w.p. $1-\alpha$. The sequence of conditional distributions is stationary $f_k(r', x_{t+k}) = f(r', y') = \Pr(r = r')\Pr(y = y')$. Note that to economize on zeros we have deflated wealth by 1,000,000 so that income, y is either zero or four. This also helps economize on variables since it implies that the price of each property is normalized to one.

Her contingent plans are subject to the budget and no-borrowing constraints, both of which must be satisfied in every period $t+k$:

$$\tilde{x}_{t+k} = \tilde{x}_t + y - c_t \quad \forall c \in \{0,1\}, \quad \forall y \in \{0,4\} \qquad \text{(BC)} \qquad (1.2)$$

$$\tilde{x}_{t+k} \geq 0 \qquad \text{(CC)} \qquad (1.3)$$

Equation (2.1) looks like it can be decomposed into the value of the contingent choices in the current period t and the continuation value given by $\tilde{x}_{t+1}$. As in a standard dynamic consumption problem, we can rewrite the problem above as a bellman equation. We denote the value of each pair (x, r) by

$$\tilde{V}(x,r) = \begin{cases} \max_{c\in\{0,1\}} cr + \delta V(x-c) & \text{if } x > 0 \\ \delta V(x) & \text{if } x = 0 \end{cases} \qquad (1.4)$$

And denote the value of each level of wealth $\tilde{x}$ as

$$V(\tilde{x}) = \mathrm{E}_{f(y,r)}\left[\tilde{V}(x,r)\right] \qquad (1.5)$$

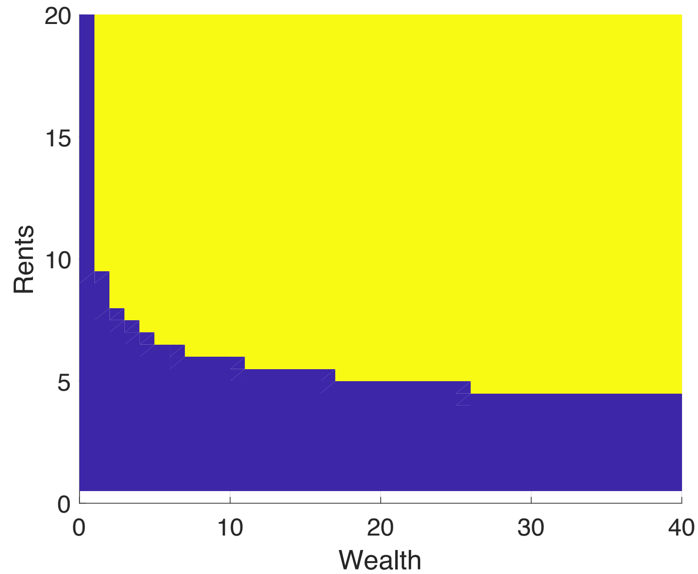


Figure 13: Threshold function $s(x)$. Agents buy in the yellow region and pass in the dark purple region.

Although we have described the problem as one of making a binary choice for each tuple (x, r) the solution to the agent's problem is a *threshold* policy $s(x)$ that depends only on x. Consider once again the agent starting the period with $\tilde{x}_t$. She does not need to wait until a property offer materializes in order to formulate her plan of action. Instead, for each level of wealth x she sets a reservation offer $s(x)$; she buys if and only if $r \geq s(x)$. Figure 13 shows the threshold policy. Higher wealth puts subjects farther away from the credit constraint so it also makes them more willing to accept lower offers.

We can write the problem directly as a choice of threshold. For tractability, the distribution from which we draw rental offers in the experiment is a discretized log-normal. Suppose we had instead allowed for the rental offers to be drawn from the positive reals. In that case, for every level of wealth x, the agent would choose a *unique* threshold s, similar to choosing a unique level of consumption for every level of wealth in a consumption/savings problem. The bellman equation for the problem of choosing $s(x)$ can be written:

$$V(\tilde{x}) = \mathrm{E}_{f(y)} \max_s \sum_{r=s(x)}^{\bar{r}} \Pr(r) r \\ + \delta \left(\sum_{r=\underline{r}}^{s(x)} \Pr(r) V(x+y) + \sum_{r=s(x)}^{\bar{r}} \Pr(r) V(x+y-1) \right)$$

In other words the agent is choosing an expected rent $u(s) \equiv \sum_s^{\bar{r}} \Pr(r) r$ along with its associated continuation value:

$$V(\tilde{x}) = \mathrm{E}_{f(y)} \left\{ \max_s u(s) + \delta \mathrm{E}_{f(r)} V\left(\tilde{x} + y - \mathbb{1}_{\{r \geq s\}}\right) \right\} \tag{1.6}$$

Even though our experiment requires to make a simple binary choice, the bellman equation above shows how this problem can be interpreted as an analogue to the familiar consumption/savings problem usually studied in macroeconomics. In our case, however, our subjects solve for savings rather than consumption. $V(\cdot)$ is a strong contraction and we can solve for $s(x)$ through value function iteration. Just as consumption is increasing in wealth, $s(\cdot)$ is weakly decreasing in x. Put another way, the probability of buying conditional only on r is increasing in r. Similarly, the probability of buying conditional only on x is also increasing in x.

Crucially, (x, r) are not only necessary, but also sufficient statistics for the agent to implement her optimal policy. In period t all the agent has to do is check whether $r_t \geq s(x_t)$. Previous choices only affect the current value of the problem through x_t. And previous offers contain no information about future offers. Our experimental design also ensures that we are not missing potential costs of switching actions that are not part of the game but may be present in the lab. Since the cursor is reset between the "buy" and "pass" buttons every period, there can be no motive to economize on switching the cursor from one button to the other. This result gives us a straightforward testable prediction: the probability of buying in any given period should depend only on (x, r).

Results

We collected data for 24 subjects. Each subject played on average just under three games, 69 games in all. [1]We have a total of 27,638 observations. Because the length of the game is stochastic, some subjects played only one game while the maximum number of games played by a subject was four. If subjects played more than one game then one of the games was chosen at random for payment and this was explained to the subjects. On top of the earnings from playing the game, all subjects received $10 for showing up. We have several hundreds of data for each subject. The subject with the fewest data played 839 rounds over four games while the subject with the most data played 2,762 rounds over two games. We do not believe that fatigue is a major concern because although subjects played many hundreds of rounds, each round took subjects on average only 1.3 seconds to complete with a standard deviation of 2.35 seconds.

Figure 14 shows subject 20's probability of buying conditional on (x, r).

[1]One game ended after a single round

The black line corresponds to the rational expectations threshold $s(x)$. Even though subject 20 does not buy only and always above the line the probability of buying is higher above the line than below it for each level of wealth x. This pattern is typical of our subjects and translates into our aggregate data. Since we observe stochasticity at the subject level we interpret stochasticity in our pooled data as a feature of behavior rather than as the result of aggregation. Another salient feature of Figure 14 is how little of the state space is actually covered. Although we have a lot of data, our state space is countably infinite. The maximum rental draw in our experiment was 36,000 and the maximum wealth level was 87 million. Even if we were to truncate the state space at these values, it would still have a cardinality of 6,336. Which, if uniformly distributed, would correspond to an average of four observations per (x, r) pair. Of course they are not evenly distributed which explains the white spaces in Figures 14 and 15. Due to the size of our state space, looking at aggregate data has the benefit of allowing for more precise estimates of the policy function and adds power to our formal hypotheses tests. All our results and figures are for our pooled data unless otherwise specified. We will revisit our data subject by subject as a robustness check of our main result in section 4.3.

Subjects Condition on the State

Before showing that our subjects condition their current choices on past choices we first show that they also respond to (x, r). Without this feature we would not be able to rule out that our choice data are simply noise. As a first pass we can compare the

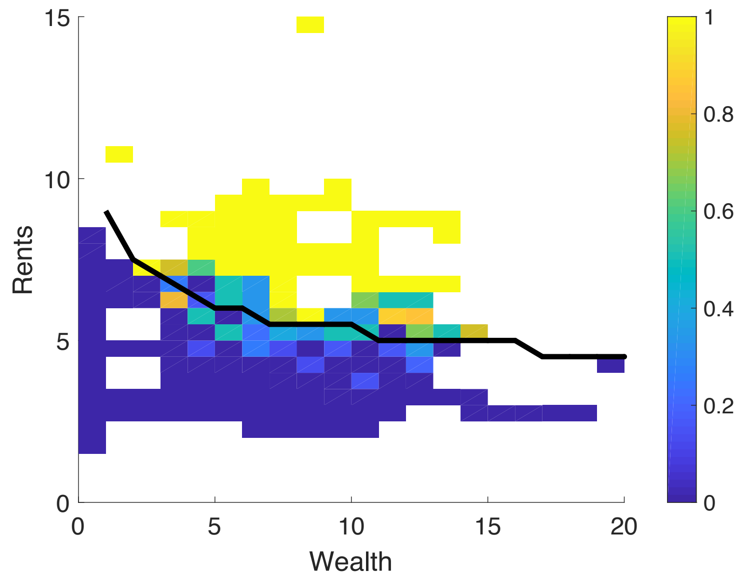


Figure 14: Empirical probability of buying conditional on the state (x, r) for subject 20. Rents are in thousands and wealth in millions. The threshold function $s(x)$ is in black. Probability scale on the right.

average earnings of our subjects with how much they would have earned under three alternative state-independent rules of thumb. On average our subjects earned \$12.63. If our subjects had used a fair coin to decide their action they would have earned on average only \$9.31. If they had naively tried to buy every property they were offered while not being credit constrained, average earnings would have been \$9.65. Finally we can look at how our subjects would have done if they had randomized using their actual unconditional probability of buying. In that case, on average, they would have earned \$8.68. The fact that our subjects earn so much more than if they bought with the same unconditional probability of buying suggests that they were conditioning on the state rather than just clicking away.

Figure 15 shows the probability of buying conditional on (x, r) with the threshold function $s(x)$ in black. Although we have truncated the figure at $x = 40$

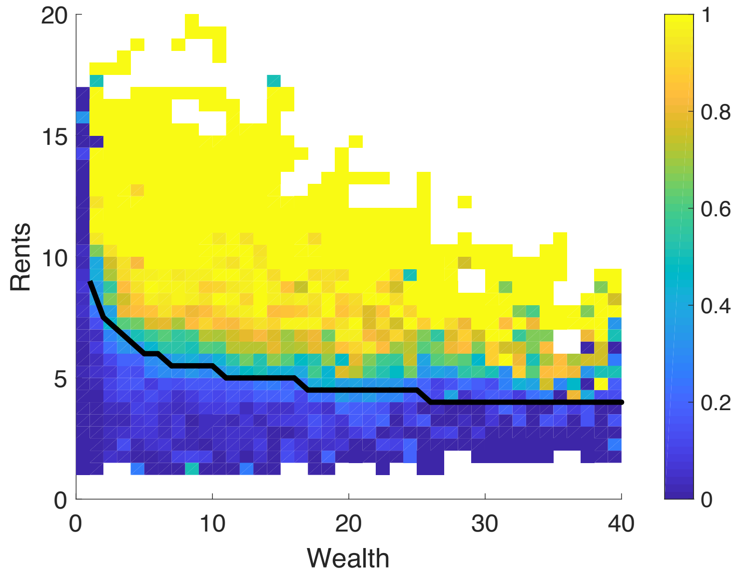
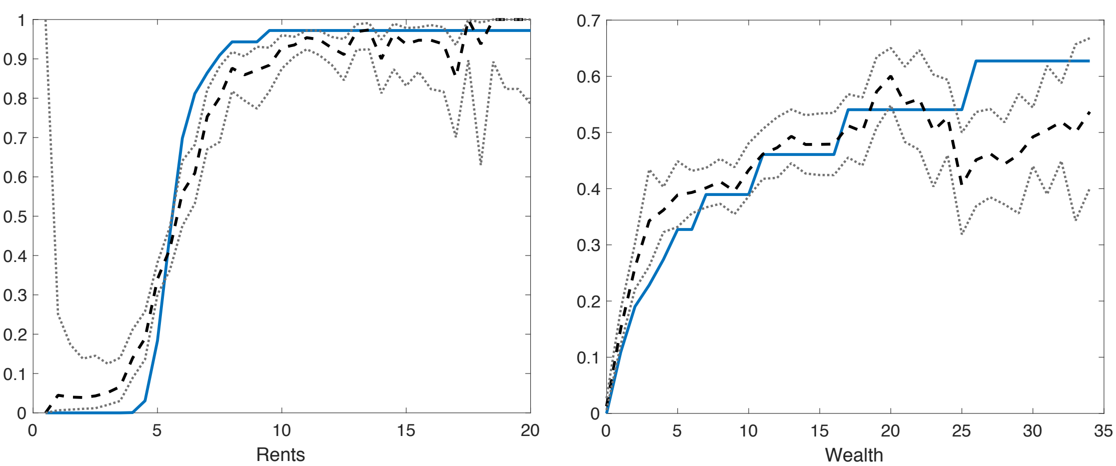


Figure 15: Empirical probability of buying conditional on the state (x, r) for all subjects. Rents are in thousands, wealth in millions. The threshold function $s(x)$ is in black.



(a) Prob(Buy$|r$)

(b) Prob(Buy$|x$)

Figure 16: RE probability (solid blue); empirical probability (dashed black). 95 percent confidence intervals (dotted gray) are clustered by subject. Rents are in thousands, wealth in millions.

million and $r = 20$ thousand, 95 percent of our data fall within these bounds. Outside of these bounds the conditional probability estimates are quite noisy and not very informative. Our data appear to be most noisy around the rational expectations threshold; although our subjects cannot implement the full rational expectations policy, they seem to have a fairly sophisticated understanding of the incentive structure of the game.

Figure 16 depicts more clearly the degree of sophistication of our subjects; panel (a) displays the probability of buying conditional on rents while panel (b) displays the probability of buying conditional on wealth. The probability of buying is increasing in both rents and wealth, respectively. There is also a pattern to the deviations from the rational expectations solution: subjects tend to overbuy for low levels of wealth and rents, but underbuy for high levels of both. Our data display more sophistication than simple "rule of thumb" behavior on the part of our subjects. We can rule out, for example, a single threshold s independent of x. This would imply a vertical line in Figure 16(a), a hypothesis that is clearly rejected by the data.

It is worth highlighting the fact that the probabilities implied by the rational expectations policy (in blue) are *below* the 95 percent confidence intervals (dotted gray) for low values of both r and x and similarly *above* for high values. This implies that the empirical and theoretical probabilities must cross; they do so around the means of each distribution, which are 5,600 for rents and 11 million for wealth. Our data display sophistication in the sense that the probability of buying not only *increases* as the state increases but the degree to which it does means that the *level* remains tethered to the one implied by the rational expectations policy. Subjects do not systematically underbuy or overbuy for all levels of wealth or all rental values.

Subjects Condition on Past Choices

We begin discussion of our main empirical result by considering the odds ratio. We first focus on the odds ratio because it allows for a more straightforward interpretation of the degree of inertia than regression coefficients. We of course follow this discussion with formal hypotheses testing of inertia in our data.

Let $\pi(\cdot)$ denote the probability of buying. Our analysis of the problem in Section 3 showed that π should depend *only* on (x, r); we can actually write the rational expectations policy as $\pi\left(r \geq s(x)\right) = 1$. The odds ratio, in turn, is defined as

$$\rho \equiv \frac{\pi}{1-\pi}$$

Like π, ρ should only depend on (x, r). In particular, it should be independent of the past action, c_{-1}. Similarly to Mayskova *et al* (2018), we say that our subjects condition on past choices if $\rho(x, r, c_{-1}) \neq \rho(x, r)$. While we find that ρ *does* depend on the state as implied by Figures 15 and 16, we find it *also* depends on c_{-1}. Moreover, the conditioning is *habitual.* The odds of buying go up if subjects have bought in the previous period: they display inertia.

Since rental draws are exogenous we can integrate over r and, abusing notation, denote the odds ratio in terms of wealth and the previous choice

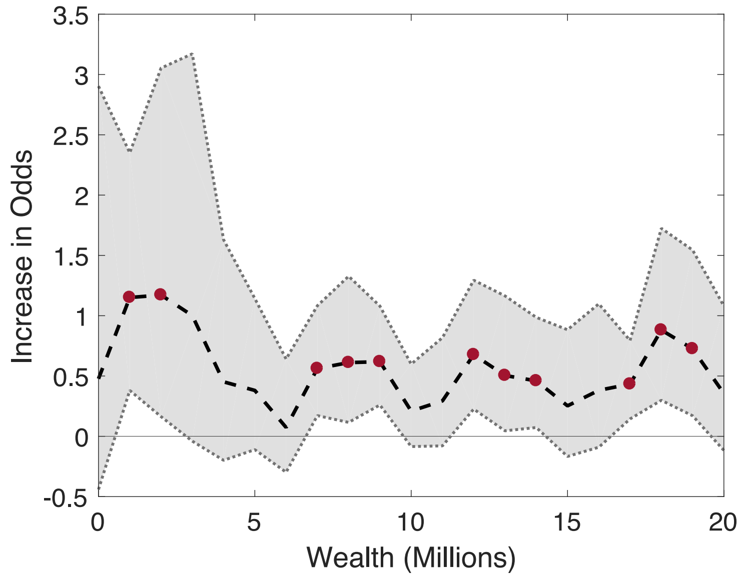


Figure 17: Increase in odds ratio. Wealth in millions. Red points denote significance at a five percent level. Bonferroni correction applied to hypotheses tests.

$\rho(x, c_{-1}) = \mathrm{E}_{f(r)}[\rho(x, r, c_{-1})]$. Figure 17 shows the increase in the odds of buying when subjects have bought in the previous period for wealth levels ranging between zero and 20. In any turn t, wealth is of course correlated with previous actions. However, c_{-1} contains no additional relevant information beyond x. So for any given level of wealth, the odds of buying should not change given the previous action. Instead, we see that for the 21 levels of wealth in figure 17 the point estimates (dashed black line) are always above zero and nine of them significantly so (red dots). On average, the odds of buying increase by around 50 percent if the previous action was "buy."

It is important to highlight the fact that this result rules out the interpretation that our subjects are simply behaving as if they were implementing $s(x)$ plus some noise. If subjects were only able to implement a stochastic choice rule there is still no reason why it would depend on anything other than (x, r), even if they internalize the stochasticity of their choice rule when designing a policy.

To test this formally we estimate a linear probability model with fixed effects. We do not have to worry about consistency because the failure of consistent estimators from panel data arises when T is held fixed. Our panel, however, does not have this shortcoming. Therefore the usual asymptotic results apply. Our regression includes x and r as well as the previous period's action c_{-1}. Under the null, the coefficient on this last variable should be zero. We also include a time polynomial for each subject $[t_i, t_i^2]$ to control for both learning and fatigue. This ensures that the inertia we observe in the data is not driven by these—very plausible—factors. We collect the fixed effects and time polynomials in vector z and estimate the following regression:

$$c = \beta_0 + \beta_1 r + \beta_2 x + \beta_3 x^2 + \beta_4 c_{-1} + \Phi z \tag{1.7}$$

Our choice to include higher order polynomials of wealth is informed by the well known theoretical result from dynamic programing that the value function should be increasing and concave in wealth. We report our estimates in the first column of Table 11. We also estimate a logistic regression under random effects for comparison. The first thing to notice in our results is that subjects condition on the state. the coefficients on wealth and rents are all significant at the .1 percent level. We also estimate the subjective value of wealth to be increasing and concave: β_2 is positive while β_3 is small and negative. Higher order polynomials are not significant and do not change our estimates. We therefore exclude those results. Importantly, we find that the lag coefficient β_4 is *positive* and *significant* although the level of significance is lower in the linear regression with fixed effects. All standard errors are clustered

	Linear (FE)	Logit (RE)
Lag	0.1272	1.409
	(0.0401)**	(0.2642)***
Rents	10.317	123.46
	(0.4209)***	(20.539)***
Wealth	0.0219	0.214
	(0.0019)***	(0.0296)***
$Wealth^2$	-0.0002	-0.002
	(0.00003)***	(0.0003)***

$^{*}p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Standard errors clustered by subject. N=27,560.

Table 11: Linear and Logistic Models

by subject. Finally, qualitative results are invariant to our regression specification: both the linear and logistic regressions offer the same picture.

One may worry that the logistic regression results are biased since we are estimated a random effects regression with endogenous regressors that are potentially correlated with individual characteristics. Although this is not a concern in our linear regression specification, we may wonder how meaningful the p-value on the lag coefficient in the logistic regression is. To overcome this we also estimate a dynamic probit with unobserved effects. The non-linear model we would like to estimate is:

$$\Pr(c = 1) = G(r, x, c_{-1}, d^i) \tag{1.8}$$

where d^i is a subject fixed effect indexed by i. In linear models, the incidental parameters problem that arises from the fact that the coefficients on d^i cannot be estimated consistently is addressed by differencing out the fixed effects. In nonlinear models we need to integrate over the d^i which requires a way to deal with the initial observation $c_{t=1}$. This is known as the initial conditions problem.

To address this problem we follow Wooldridge (2010) and estimate a random effects augmented probit that includes as regressors the initial choice of each subject, i, $c^i_{t=1}$ for every t. Our specification differs from Wooldridge in that he proposes including every pair (x^i_t, r^i_t) as a regressor. A large T makes this is infeasible since it would require a vector of regressors $z^i = (x^i_t, r^i_t)^T_{t=1}$ for each subject.[2] Instead we include leads and lags of (x^i_t, r^i_t). Finally, as further check we replace the initial condition $c^i_{t=1}$ with the average condition $\bar{c}^i$ as proposed by Chamberlain (1980). All our specifications include time polynomials. None of these alternative specifications affect our conclusions.

We estimate the following random effects probit:

$$\Pr(c = 1) = G(\beta_0 + \beta_1 r + \beta_2 x + \beta_3 x^2 + \beta_4 c_{-1} + controls) \tag{1.9}$$

where *controls* accounts for the incidental regressors discussed in the previous paragraph.

Table 12 reports our results. The headings in bold refer to the number of lags and leads of (x, r). Specification (A) follows Wooldridge and includes the initial condition $c^i_{t=1}$ while specification (B) follows Chamberlain and include the average condition $\bar{c}^i$. The first row reports our estimates of the coefficient on c_{-1} (β_4). The

[2] A large T also ameliorates the worry that the initial choice c_1 is correlated with the majority of the subsequent choices. Like the incidental parameters problem, the initial conditions problem is acute when T is small but less severe as T increases. Given our very large T, we are confident our results are not driven by this problem.

	4 Lags and Leads		10 Lags and Leads		20 Lags and Leads	
	(A)	(B)	(A)	(B)	(A)	(B)
Lag Action	0.844	0.843	0.840	0.838	0.837	0.835
	(0.188)	(0.188)	(0.186)	(0.186)	(0.200)	(0.199)
Rent	64.709	64.700	66.108	66.099	67.547	67.554
	(9.005)	(9.030)	(9.198)	(9.209)	(9.616)	(9.626)
Wealth	0.667	0.667	0.670	0.671	0.682	0.683
	(0.056)	(0.056)	(0.060)	(0.060)	(0.060)	(0.060)
$Wealth^2$	-0.004*	-0.004*	-0.004*	-0.004*	-0.004*	-0.004*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Cons	-4.671	-5.533	-4.246	-5.220	-4.030	-5.239
	(0.543)	(0.746)	(0.540)	(0.731)	(0.529)	(0.761)
N	27152	27152	26348	26348	25103	25103

$^*p < 0.05$, $p < 0.001$ otherwise. Standard errors clustered by subject.

Table 12: Probit Model

first thing to notice is that our point estimates are robust to either our choice of specification (A) or (B) as well as the lag and lead length. The average over our estimates is 0.840 with the smallest being 0.835 and the largest 0.844. Standard errors are clustered by subject and all six estimates are significant at the .001 percent level.

One of the challenges of estimating non-linear regressions is the *economic* interpretation of the parameters. We do not intend to give a structural interpretation to the probit model in (2.7). The aim of the formal statistical analysis is to get qualitative rather than quantitative results. The meaningful result is that inertia is a feature of choice. Quantifying the degree of inertia is left for future research. We are thus more interested in the *signs* of our estimates than in their values. The main takeaway from Table 12 is that it confirms the claims of the two subsections thus far. First, the coefficients on rent and wealth are all positive at a significance level of .001: *subjects condition on the state.* Second, and the central claim of this paper, the coefficient on the lag action is positive and significant at a .001 level: *subjects condition on past actions.*

Robustness Checks

To check the severity of the bias in our estimates we conduct Monte Carlo simulations on the counterfactual were subjects do not condition on c_{-1}. For each of our subjects we estimate a policy function $\pi^i = g(\beta_0^i + \beta_1^i r + \beta_2^i x + \beta_3^i x^2)$. We do not have to worry about the size of our 24 subsets of data since each contains on average more than 1000 observations. By construction, these estimated choice rules do not condition on past actions.

For each of the 69 games played we draw one of the choice rules from $\{\pi^i\}_{i=1}^{24}$ with replacement and generate a new history of play. So for each of the games the vectors of exogenous variables—income, rental and continuation—remain the same and only the wealth and choices are simulated. This generates a dataset with the same number of observations as our original dataset but with simulated choice and wealth data. Importantly, our simulated choice data depend only on wealth, rents, and a subject fixed effect but not on past actions.

For each of 1200 Monte Carlo simulations we estimate regression (2.7) and

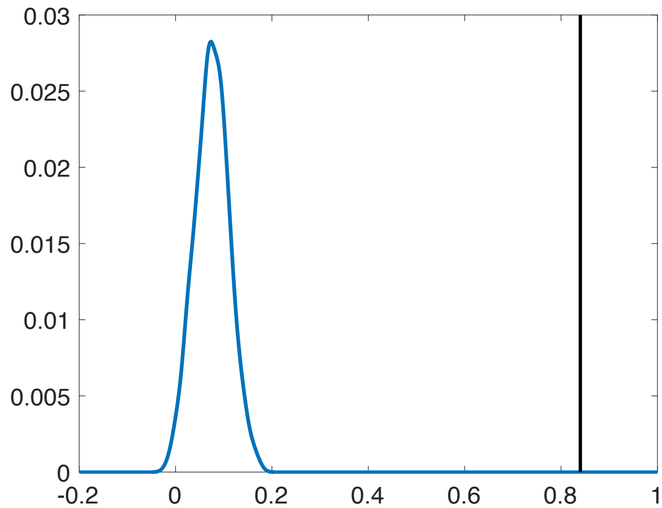


Figure 18: Kernel Density of $\hat{\beta}_4$ (blue). Average value of $\hat{\beta}_4$ from table 1 (black).

generate a distribution of estimated parameters $\hat{\beta}_4$.[3] Figure 18 shows the kernel density estimate of $\hat{\beta}_4$. Although the estimates are upward biased, the average bias of .075 is an order of magnitude smaller than the estimates reported in Table 12. The 95 percent confidence interval ranges from .01 to .14. The black vertical line in figure 18 intersects the x-axis at .84, the average of the $\hat{\beta}_4$ reported in Table 12. The standard deviation of the kernel density is .036, which puts the black line nearly 20 standard deviations away from the upper boundary of the confidence interval. Even after we apply this bias correction to our estimates they remain significant at the same .001 level.

As a second robustness check we can run time series probit regressions for each of our 24 subjects. Table 13 reports the estimates of β_4 for each of the 24 subjects. All but two of the estimates are positive. The two negative point estimates are extremely small and not significant; in both cases the *p-value* is over 50 percent.

[3] Given the stability of our estimates we only report the specification with 4 lags and leads.

(1)	(2)	(3)	(4)	(5)	(6)
0.454	0.614***	0.035	0.430	0.350	1.407***
(0.568)	(0.135)	(0.323)	(0.201)	(0.242)	(0.222)
(7)	(8)	(9)	(10)	(11)	(12)
1.375***	-0.156	1.998***	0.396	1.138***	-.033
(0.221)	(0.216)	(0.117)	(0.202)	(0.192)	(0.200)
(13)	(14)	(15)	(16)	(17)	(18)
1.226***	0.661	0.246	0.776***	0.109	0.604
(0.257)	(0.246)	(0.186)	(0.162)	(0.183)	(0.358)
(19)	(20)	(21)	(22)	(23)	(24)
0.322	0.133	0.501	0.583	0.369	0.776*
(0.115)	(0.214)	(0.249)	(0.237)	(0.399)	(0.253)

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Robust standard errors.

Table 13: Lag Action Coefficient by Subject.

Seven of the remaining 22 point estimates are significant at the .1 percent level and one is significant at the five percent level.[4] About a third of our subjects convincingly display inertia while we estimate a positive residual autocorrelation in actions for over 90 percent of them.

One of the drawbacks of disaggregating our data is that our state space is very large so the coefficient on the lag term is not very precisely estimated. We are interested in average behavior rather than individual heterogeneity. We only want to consider the results in 13 as a robustness check that the unobserved effects d^i are not driving the estimates we report in table 12.

We have already argued at length that our experimental design allows us to

[4]Since we are testing multiple hypotheses, we apply the Bonferroni correction before testing for significance.

rule out preferences or external costs as the sources of inertia. Having firmly established inertia as a feature of behavior, we can only conclude that it must come from cognitive biases or limitations. Our experimental design allows us to determine that the problem as laid out in equations (1.4)-(2.5) is *not* the problem that subjects are solving. Although we cannot observe cognitive processes directly, we can nonetheless use our choice data to discipline our modeling of those processes. In what follows, we will argue that inertia is best understood as a way of *economizing on cognitive resources* rather than as a behavioral bias that creates attachment to goods or actions.

Inattentive Reconsideration

Why would our subjects condition on past choices when x and r are freely available to them at all times? Our preferred explanation is consistent both with introspection and models of cognition from psychology and neuroscience: thinking hard about a problem is slow and requires effort. It is well known that there is no single mechanism through which the brain makes decisions.[5] Choices from deliberation, which requires a lot of cognitive resources, are slow compared to *habitual* actions. Subjects might therefore condition on previous actions as a way to economize on thinking through the problem every round. In some rounds they may choose to simply continue doing what they did before, while in others they may decide it is worth it for them to think more carefully before choosing.

[5] See, for example, Sanfrey and Chang (2008).

The hypothesis we have sketched above introduces an interim choice. We are not proposing that the conditioning on past actions is automatic. Quite the opposite, we are proposing that subjects take account of the state when they choose whether to reconsider. Alternatively, we can interpret this interim choice as a decision whether to *continue* deliberating. What we have in mind is a subject who sees her budget and the rental offer and then decides whether to just choose as she did in the previous period or keep thinking through whether to buy or not. If she continues to think she may very well choose the same action as she has in the past. However, changes in actions necessarily imply that she has reconsidered.

We find suggestive evidence of this process in our reaction time data. Reaction times for action switches are longer than reaction times when actions remain the same. Recall that the mouse cursor is reset each turn so there are no external adjustment reasons for this time delay. We estimate a random effects regression of reaction times on an indicator of action switches

$$rt = \gamma_0 + \gamma_1 \mathbb{1}_{\{c \neq c_{-1}\}} + controls$$

Our controls are a third order polynomial for the number of turns as well as the state (x, r). Since neither wealth nor rents affect reaction times or have a meaningful effect on our estimate of γ_1 we drop them from the specification we report here. A fixed effects model yields nearly identical results.

The point estimate $\hat{\gamma}_1$ is .08, the 95 percent confidence interval, clustered by subject, is [.04 .12]. The associated *p-value* is less than .1 percent. As a comparison

$\hat{\gamma}_0 = 1.72$. The interpretation is that action switches on average lead to reaction times that are 80 milliseconds longer than the 1.72 seconds that it takes to choose the same action is before.

To be sure, this is only a small delay. However, the 1.72 seconds include processes which subjects must undergo regardless of their choice, such as reading the screen and moving the mouse. It is therefore hard to directly compare time spent thinking. Reaction times are not the focus of this paper so we need to be careful not to over-interpret $\hat{\gamma}_1$. While reaction times are not a direct test of whether our subjects are "thinking more," the fact that subjects do take longer in rounds where they change their choice from the previous round is evidence in favor of the hypothesis for which we have argued above.

Model

In the following sections we formalize the story sketched out above in a model of *inattentive reconsideration*. We begin with a discussion of two important details of our modeling strategy. We then proceed to write down the model and solve for the policy function of our cognitively-constrained agents. This model follows a model proposed by Woodford (2009) to explain *pricing* behavior. A variant of this model was also used by Khaw *et al* (2017) to account for pricing-like behavior in an experimental setting. The model combines rational inattention with the two-step decision of whether to reconsider and, conditional on reconsideration, whether to buy.

First proposed by Sims (2003), the rational inattention model captures cognitive limitations in a reduced form as informational costs. Rather than conditioning on the state, agents must now condition on signals of the state. There is an information cost function that makes it costlier for signals to be more precise and therefore agents must weigh the benefit of making choices with more precise signals against the cognitive cost of those signals. Importantly, however, agents are free to choose whatever signal structure they want.

In a dynamic environment—as is the case in our experiment—one must consider whether agents would also choose to condition their actions on the history of signals they have received rather than simply the current signal. If memory itself is costly, accessing the history of signals up to time t is not free and should itself be transmitted by an informative, yet not perfectly revealing signal. But then it is better for the agent to get a more precise signal today than to devote cognitive resources on accessing the history of signals up to today. In settings where knowing the state is very difficult, it makes sense to model agents as conditioning their signals on beliefs rather than on the state itself. This is not the case in our framework. Agents do not have to guess what their current wealth is or what the rents on offer are in any given period. Both are displayed on the screen and so it would be strange for subjects to devote time and effort recalling *past* values of the given that the only reason they would do so would be to update their beliefs about the current values of the state, which they can easily read on a computer screen in front of them. We therefore find it sensible to assume that signals will be conditioned only on the current state and not on past signals. Further, as shown by Caplin and Dean (2015) and Steiner *et* *al* (2017), agents will only choose at most one signal for each available action. It is without loss of generality that we restrict the signal space to one of recommended actions.

Information costs generally lead to stochastic rather than deterministic choice rules. Although this is certainly in line with what we observe in our data, we have to think carefully about the implications of hitting the credit constraint. To do this we have to distinguish between actions and consequences. For the fully informed rational agent of Section 3 there is no distinction. For an inattentive agent this is also the case everywhere except when $x = 0$.

Recall that the experiment allows subjects to click "buy" even when they are credit constrained. The rational agent would never click on "buy," unlike our subjects whose probability of trying to buy when they are credit constrained is 1.3 percent while .6 percent is the lower bound of the 95 percent confidence interval. This is the experimental equivalent behavior of having your credit card declined; it doesn't happen often, it doesn't happen to everyone, but it happens from time to time. We want to capture this feature of behavior in our data because it is behavior that we observe outside of the lab. To do this we decouple the action "buy" from the consequence of buying at $x = 0$ by introducing a fixed cost κ of trying to buy when credit constrained. In the context of our experiment κ can be interpreted as the wasted time that comes from not clicking "pass."

To Think or not to Think

In our model agents choose whether to reconsider and pay a fixed cost. If they do not reconsider they repeat the previous period's action. If they decide to reconsider they now choose whether to "buy" or "pass". Importantly, this choice is subject to an information cost. In addition to wealth and rents, the agent's previous choice is now relevant to her decision and becomes another state variable. The value of the triple (x, r, c_{-1}) is given by:

$$\tilde{V}(x, r, c_{-1}) =$$
$$\begin{cases} \max_{\mu}(1-\mu)(rc_{-1} + \delta V(x - c_{-1}, c_{-1})) + \mu(\bar{V}(x, r) - \gamma^{rec}) - \phi^{-1} I(\mu) & \text{if } x > 0 \\ \max_{\mu}(1-\mu)(-\kappa c_{-1} + \delta V(x, c_{-1})) + \mu(\bar{V}(x, r) - \gamma^{rec}) - \phi^{-1} I(\mu) & \text{if } x = 0 \end{cases} \tag{1.10}$$

Equation (1.10) is the analogue of the rational expectations $\tilde{V}(\cdot)$ in equation (1.4). The value function, $V(\cdot)$ now depends on c_{-1} and, similarly, is the analogue of equation (2.5):

$$V(\tilde{x}, c_{-1}) = \mathrm{E}_{f(r,y)}\left[\tilde{V}(x, r, c_{-1})\right] \tag{1.11}$$

where $\mu(x, r, c_{-1})$ is the probability of reconsideration, ϕ parametrizes the marginal cost of information, $I(\cdot)$ is an information cost function, γ^{rec} is the fixed cost of reconsidering and $\bar{V}(\cdot)$ is the value of reconsidering. In this section we want to focus on the choice to reconsider so for now we take $\bar{V}(\cdot)$ as given. In the next section we focus on how this value is determined.

Equation (1.10) says that the agent must choose a probability of reconsideration for each (x, r, c_{-1}) by balancing the benefit of the more precise signal (the first terms in brackets) against the cost of more precision (the last term). If she does not reconsider she faces the value of that outcome in the first term inside the brackets: she takes her past action, c_{-1} and carries it forward into the next period. If she does reconsider, she pays a fixed cost but gains the value of reconsideration given the state, $\bar{V}(\cdot)$. Notice that the value of reconsidering does not depend on c_{-1}; the only reason for the agent to condition on c_{-1} is to avoid paying the cost γ^{rec}. Once she incurs the cost and commits her cognitive resources, she only pays attention to the payoff-relevant variables (x, r). Our agent is forward-looking, but optimally chooses to condition on c_{-1}.

The second line in equation (1.10) captures the cost of being credit constrained. Suppose the agent hits her credit constraint and does not reconsider. If she does not buy her payoff is zero and her continuation value is $V(0, 0)$. If she does click "buy," however, she pays a cost κ independent of the rental value on offer. Even though the agent incurs this cost if she tries to buy at $x = 0$ it does not necessarily mean that she will reconsider with probability one when $x = 0$ and $c_{-1} = 1$. She must balance this agains the cost of discriminating the state perfectly.

To operationalize our model we follow the literature of rational inattention and assume that the cost of information is the mutual information function. The mutual information function is the expectation taken with respect to the joint distribution of the action and the state of the log of likelihood ratio of the joint over the marginals. Simple algebra shows that this implies $I(\cdot)$ is of the following form:

$$I = \mu \log\left(\frac{\mu}{M}\right) + (1-\mu)\log\left(\frac{1-\mu}{1-M}\right) \tag{1.12}$$

where μ is the conditional probability of reconsideration while M is the unconditional probability. $I(\cdot)$ captures the tradeoff of paying attention by penalizing deviations from expected behavior. There is no penalty to choosing $\mu = M$. There is only a cost if you deviate from what you do in expectation.

Taking the first order conditions we can write the solution to problem (1.10)-(1.12) recursively as: [6]

$$\mu(x, r, c_{-1}) = \frac{M' \exp\{\phi \bar{V}(x, r)\}}{\Delta_\mu(x, r, c_{-1})} \tag{1.13}$$

$$\begin{aligned}\Delta_\mu(x, r, c_{-1}) = &M' \exp\{\phi \bar{V}(x, r)\} + \\ &(1 - M') \exp\{\phi(rc_{-1} + \delta V(x - c_{-1}, c_{-1})\}\end{aligned} \tag{1.14}$$

$$V(\tilde{x}, c_{-1}) = \frac{1}{\phi} \mathrm{E}_{f(r,y)} \left[\log \Delta_\mu(x, r, c_{-1})\right] \tag{1.15}$$

where $M' \equiv M \exp\{-\phi\gamma^{rec}\}/(M \exp\{-\phi\gamma^{rec}\} + [1-M])$. Conditional on ϕ, γ^{rec} pins down M'. We can dispense with γ^{rec} and interpret M' as the reference probability of reconsideration so that it is costly to choose probabilities that differ from M' rather than M. Under that interpretation it is costly for agents to deviate from what they would *prefer* to do (reference probability M') instead of what they expect to do (unconditional probability M).

[6] We omit the case of $x = 0$ for brevity. in that case we would simply replace r in the last term of equation (1.14) with $-\kappa$ and c_{-1} in the first argument of the value function with zero.

Equation (1.13) says that the agent rescales her reference probability of reconsidering, M', by the relative value of reconsidering given the state: $\exp\{\phi\bar{V}(\cdot)\}/\Delta_\mu(\cdot)$. This ratio is the value of discriminating the state with high precision. If this ratio is larger than one she sets her probability of reconsideration above her reference probability and vice versa. For example, if for some reason there was a pair (x, r) for which the value of reconsidering were the same as the value of not then the agent would reconsider with probability M'. Note, also, that the value of her choice is scaled by the marginal cost of information. When ϕ is high, the marginal cost of information is low and so the term in the exponent rises. Because these terms are exponentiated, ϕ amplifies the differences between the values of reconsidering versus not. This is exactly the intuition of the information cost: the lower the cost, the cheaper it is to distinguish between the two choices and the more sensitive μ is to the state rather than to the reference probability M'.

To Buy or not to Buy

We now turn to the agent's choice of action conditional on her having reconsidered. We assume that this choice is also subject to an information cost. We also allow for a bias γ^{buy}; if positive it captures a bias toward spending while if negative it captures a bias toward saving. Although not crucial to our results, we include it to get cleaner estimates of the information cost parameters.

$$\bar{V}(x,r) =$$

$$\begin{cases} \max_\pi \pi(r+\delta V(x-1,1)-\gamma^{buy}) + (1-\pi)\delta V(x,0) - \theta^{-1}I(\pi,\Lambda) & \text{if } x>0 \\ \max_\pi \pi(-\kappa+\delta V(x,1)-\gamma^{buy}) + (1-\pi)\delta V(x,0) - \theta^{-1}I(\pi,\Lambda) & \text{if } x=0 \end{cases} \tag{1.16}$$

where Λ is the probability of buying conditional on reconsidering and θ is the marginal cost of attention. Agents now choose the probability of buying $\pi(x,r)$ taking into account that they will condition on their action in the future. We can write the solution recursively as:

$$\pi(x,r) = \frac{\Lambda' \exp\{\theta(r+\delta V(x-1,1))\}}{\Delta_\pi(x,r)} \tag{1.17}$$

$$\Delta_\pi(x,r) = \Lambda' \exp\{\theta(r+\delta V(x-1,1))\} + (1-\Lambda')\exp\{\theta\delta V(x,0)\} \tag{1.18}$$

$$\bar{V}(x,r) = \frac{1}{\theta}\log\Delta_\pi(x,r) \tag{1.19}$$

where $\Lambda' \equiv \Lambda\exp\{-\theta\gamma^{buy}\}/(\Lambda\exp\{-\theta\gamma^{buy}\}+[1-\Lambda])$ has a similar interpretation as M'; it represents the reference probability of buying, conditional on reconsideration. Equations (1.17)-(1.19) should look familiar. They are the counterparts of equations (1.13)-(1.15) for a different binary choice: whether to "buy" or "pass." Equations (1.13)-(1.19) characterize the solution to the full two-stage decision problem. We can solve for the policy functions $\mu(x,r,c_{-1})$, $\pi(x,r)$ via value function iteration.

The policy equations (1.13)-(1.14) and (1.17)-(1.18) also suggest an alternative interpretation of the attention costs $I(\cdot)$. In equation (1.10), for example, the natural interpretation of ϕ is as the marginal cost of reducing the noise with which agents can observe the state. Similarly for θ in equation (1.16). This is the traditional interpretation of the Shannon information cost in the rational inattention literature. In the policy functions for $\mu(\cdot)$ and $\pi(\cdot)$, however, it is more natural to interpret ϕ and θ as governing the ease of distinguishing the values associated with each action. Under the first view, the cost function captures an inability to process inputs (the value of the state) without adding noise; under the latter—and, we would argue, more appealing—view, the cost function captures an inability to process the outputs (the value of the action) without noise. This is a story in which agents cannot always tell which action is more valuable, even if they know the state perfectly. This is why we argue that ours is a reduced-form model of cognitive constraints rather than a model of costly information acquisition.

We should underscore how this model is consistent with the evidence on reaction times. According to (1.10) action switches must be the result of reconsideration. Although we cannot observe agents' interim decision, we can isolate cases when we know the outcome of that decision. And in those cases, we see evidence of deliberation in the form of slower reaction times.

Estimating the Model

We estimate the five parameters of our model jointly via maximum likelihood. We report the estimates in Table 14. Since the estimates are in the arbitrary units of rental values in our experiment we convert them to U.S. cents. For example, the estimate of the cost of buying when credit constrained, $\hat{\kappa}$, is .012 or just over one hundredth of a cent.

The estimated reference probability of reconsidering, $\hat{M}'$ is less than one, which implies there is a cognitive cost of thinking. If γ^{rec} were zero the reference and unconditional probabilities of reconsideration would both be equal to one. The intuition is that by reconsidering subjects now get the value $\bar{V}(\cdot)$ without having to pay any cost γ^{rec}. We know that $\bar{V}(\cdot)$ is at least as high as the value of not reconsidering because, upon reconsideration, agents can always choose the same action as before. Therefore agents always reconsider. Given our empirical results in Section (4), it is unsurprising that the estimates of our structural model imply that agents condition on past actions with positive probability.

The estimated information cost of reconsidering $\hat{\phi}^{-1}$ is just over twice as large as the estimated information cost of choosing whether to buy $\hat{\theta}^{-1}$. Remarkably, this is roughly the same ratio that Khaw *et al* (2017) find in their experimental data as well as Stevens (2015) in her parametrization of micro price data. Three seemingly unrelated datasets generated by different subjects in different settings have now all estimated that the cognitive cost of whether to reconsider is around twice as high as the cognitive cost of what to do upon reconsidering. That the estimate of this ratio remain stable across these three different decision problems executed by different people is significant. It suggests that this model *is* capturing part of the cognitive process that culminates in choice. This points to an avenue for further research to more rigorously measure the ratio of these two attention costs.

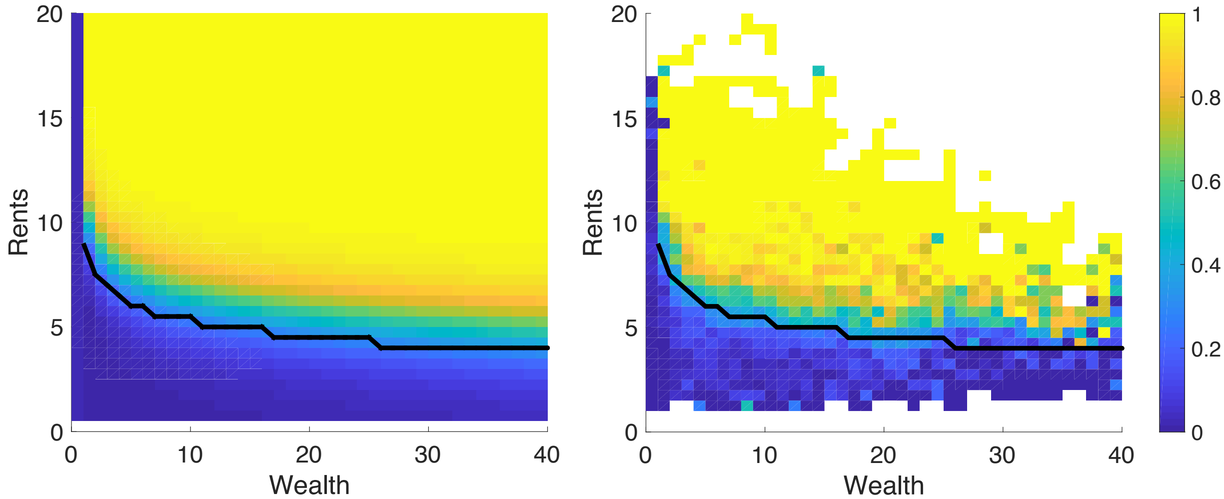
Table 14: Estimated Parameters

$\hat{\theta}$	$\hat{\phi}$	$\hat{\Lambda}'$	$\hat{M}'$	$\hat{\kappa}$
114.3	49.65	.103	.878	.012

The estimated reference probability of buying upon reconsidering is only 10 percent. This is low relative to the unconditional probability of buying in our data which is 40.3 percent. This implies that subjects are conservative in the sense that they are slightly biased toward not buying. This conservatism, however, does not imply a counterfactually low probability of buying. The unconditional probability of buying implied by our model is 39.89 percent. As a reference, the unconditional probability of buying had our subjects implemented the rational expectations policy is 40.4 percent.

Overall, our model is successful in capturing the main features of our pooled data. Figure 19 contrasts the empirical probability of buying conditional on (x, r) in panel (b) with the probability of buying conditional on (x, r) implied by our estimated model in panel (a). Our estimated model matches the data particularly well for levels of wealth up to around 25 million. At higher levels of wealth the empirical probability of buying properties with rents at or below 5000 is very close to zero whereas in our model they are closer to 40 percent. This, however, is likely due from fewer data; 88.9 percent of all our observations occur for wealth levels at or below 25 million.

To compare the degree of inertia implied by our model to the one we estimate from the data we simulate 1200 datasets using the estimated policy functions of the model $\hat{\mu}(\cdot)$ and $\hat{\pi}(\cdot)$. These simulations take the histories $\{r_t^i, y_t^i\}$ as given so that



(a) Model (b) Empirical

Figure 19: Probability of buying conditional on wealth and rents. Threshold function $s(x)$ in black. Probability scale on the right.

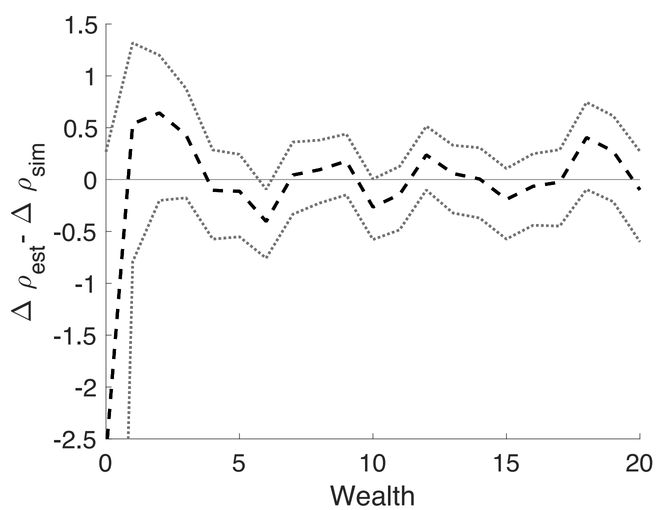
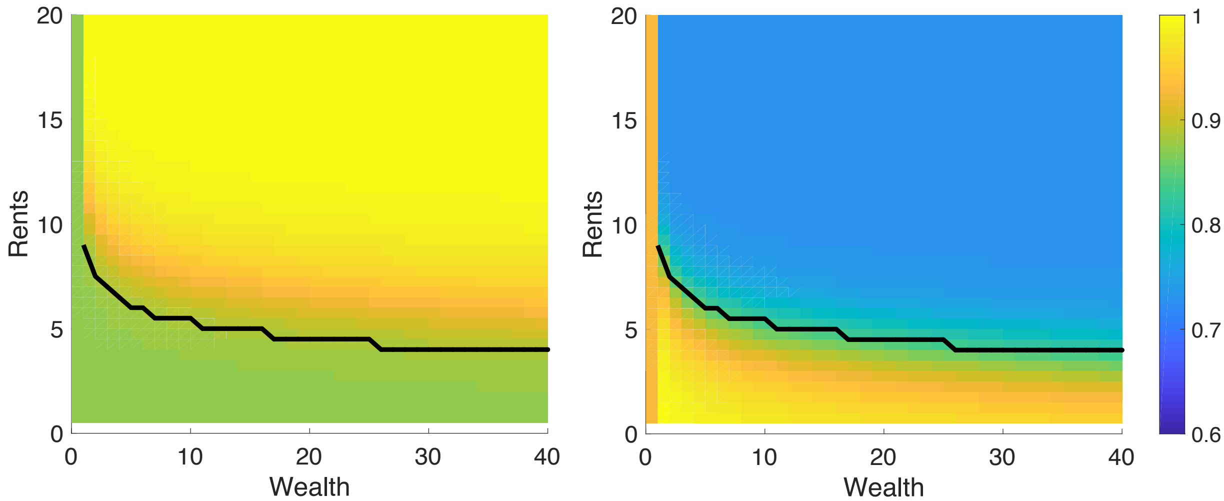


Figure 110: Difference between estimated increase in odds and simulated increase in odds. Average difference in dashed black. 95 percent confidence intervals in dotted gray. Wealth scale in millions.

only wealth and actions differ from our real dataset. For each of the 1200 simulations we estimate the increase in the odds ratio $\Delta\rho(x, c_{-1})$ for all levels of wealth in the same way as we did to construct Figure 17. This yields a sequence of distributions $(\Delta\hat{\rho} - \Delta\rho)(x, c_{-1})$ indexed by x where $\Delta\hat{\rho}$ is the estimated increase in the odds shown in figure 17. Figure 110 shows the mean (dashed black line) along with the 95 percent confidence interval (dotted gray) of these distributions. Our model captures the degree of inertia in the data extremely well. We cannot reject the hypothesis that the degree of inertia implied by our model is different from that in our data. Only at $x = 0$ is the increase in the odds predicted by the model qualitatively higher from the one implied by our data. The level of uncertainty around this estimate is also high, however, and thus not statistically significant. At $x = 0$ our model, on average, implies a much higher increase in the odds of buying than for other levels of x. This is due to the credit constraint. As shown in Figure 111(b), the probability of reconsidering at the credit constraint is actually relatively low because the estimated costs of buying when credit constrained $\hat{\kappa}$ are not very high. Though the average difference is high, we cannot reject the null that they imply the same degree of inertia.

Beyond matching the *degree* of inertia in our data, our model implies that inertia is *state dependent*. Figure 111 displays the estimated probability of reconsideration conditional on the state and past actions $\hat{\mu}(x, r, c_{-1})$. Panel (a) shows the probability of reconsideration when the past action is "pass" whereas in panel (b) the past action is "buy." Unsurprisingly, this probability is nearly one for very high rents



(a) $\hat{\mu}(x, r, 0)$

(b) $\hat{\mu}(x, r, 1)$

Figure 111: Probability of Reconsideration. Panel (a): previous action is "pass". Panel (b): previous action is "buy". Rents are in thousands, wealth in millions. Threshold function $s(x)$ in black. Probability scale on the right.

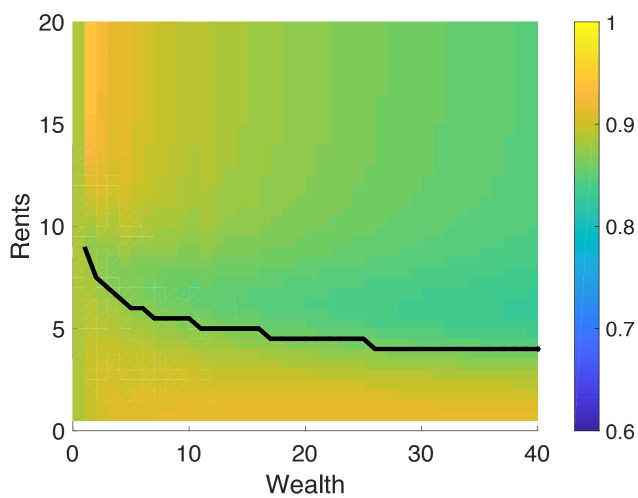


Figure 112: Probability of reconsidering conditional on wealth and rents only: $\hat{\mu}(x, r)$. Rents are in thousands, wealth in millions. Threshold function $s(x)$ in black. Probability scale on the right.

when the previous action is "pass" and for very low rents when the previous action is "buy."

In macroeconomics we are especially interested in how inertia changes with the state. Figure 112 shows the probability of reconsidering conditional only on (x, r). The probability of reconsidering is highest close to the axes, forming a kind of interrupted L shape. The intuition is simple: the probability of reconsidering is highest where action switches are most likely to be valuable. For low levels of wealth agents are more likely to "pass" than to "buy." So when rare, but very high rental offers materialize, agents' expected losses from not reconsidering are high. These are states worth being able to distinguish precisely—hence the high probability of reconsideration. A similar argument applies for the lowest rental offers. These are properties that the agent would not want to buy, regardless of her level of wealth. Therefore it is also valuable for the agent to distinguish these states precisely so that she can make sure not to buy these properties when they go on offer. Inertia is thus highest in the non-extreme regions of the state space, which are also the most likely. Recall that the average rent is around 5,600; it is just above this value, especially as wealth increases, that we see the highest degree of inertia.

This contrasts sharply with standard habits models. In those models the stock of habits is predetermined so inertia is independent of the state. The result is an inelastic term in the household's consumption rule. Our model has no such term. The deeper habits that we propose will be strongest when the gains from reconsideration are small; but they will almost disappear when changes in the state make it too costly to act habitually. This has an important implication for the propagation mechanism of aggregate shocks. In our model, the larger the shock, the more responsive aggregate demand will be to that shock. This suggests, for example,

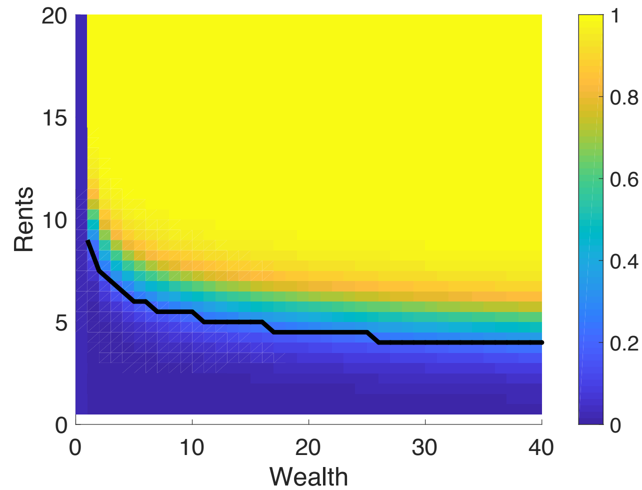


Figure 113: Probability of buying upon reconsideration $\hat{\pi}(x, r)$. Rents are in thousands, wealth in millions. Threshold function $s(x)$ in black. Probability scale on the right.

that impulse responses with non-time-separable preferences overstate the degree of inertia in aggregate demand to large and infrequent shocks.

Figures 111 and 112 also show that the level of inertia implied by our parameter estimates is not too severe. The probability of reconsideration ranges from .729 to .996. This is not surprising given the environment in which our subjects completed the task. They had no outside distractions competing for their attention and could see both x and r on the screen at all times. It seems sensible that most of the time they focused on the problem and occasionally found it preferable to act "habitually." The unconditional probability of not reconsidering is 12.54 percent. And yet even with this modest degree of inertia Figure 17 shows that the odds of buying go up by around 50 percent if the subject bought in the previous period.

The high probability of reconsideration also means that the bulk of stochasticity in our data is explained by the probability of buying conditional on reconsideration. Figure 113 shows this probability, $\hat{\pi}(x, r)$. Qualitatively it looks very similar to the probability of buying conditioning on only the state (x, r) as shown in Figure 19. This highlights the importance of information costs at both stages of choice. The information cost is higher for the reconsideration choice, and so agents do not distinguish among states too finely. The lower the attention costs, the wider the range of $\mu(\cdot)$. In the limit, if attention costs were taken to zero, $\mu(\cdot)$ would become a threshold; agents would reconsider if and only if the value of reconsideration exceeded the fixed cost. By contrast, in the case of the buying decision, where we estimate a lower attention cost, we see that the probabilities vary much more with the state, ranging from close to zero to close to one.

Costs of Inertia

It is difficult to understand the magnitude of the attention costs by looking at $\hat{\phi}$ and $\hat{\theta}$ alone. To get a better sense for two costs we decompose the gap between the average earnings from behaving according to our estimated policy functions and the rational expectations counterfactual. The average earnings implied by our model are \$12.63, which is exactly the same as the actual average earnings of our subjects. The average earnings from implementing the rational expectations policy would have been \$13.32. This is a small gap in dollar terms, but amounts to over five percent of earnings.

Our objective is to decompose this 69 cent gap into the costs of reconsideration and the costs of attention once subjects decide to reconsider. For clarity, we will refer to the first as the cost of inertia and the second as the cost of inattention. To achieve this decomposition we solve for the policy functions of two auxiliary models. In both models we use the parameter estimates reported in Table 14 while shutting down one of the two types of cognitive costs present in our full model.

In the first auxiliary model we get rid of inertia so that our subjects reconsider every period. This is equivalent to setting $M' = 1$, which implies $\gamma^{rec} = 0$. This counterfactual answers the question: how much more would our subjects have earned *given* $[\hat{\theta}, \hat{\Lambda}', \hat{\kappa}]$ if they did not have any reconsideration costs?

The second model shuts down the inattention costs upon reconsideration. This is equivalent to $\theta \to \infty$. In this case subjects do not reconsider every period, but when they do they behave according to a threshold policy. Importantly, this will not be the same as $s(x)$ since subjects now take into account the fact that they may be locked into this choice in future periods. This counterfactual asks: how much more would our subjects have earned *given* $[\hat{\phi}, \hat{M}', \hat{\kappa}]$ if they did not have any inattention costs?

For each auxiliary model we run 1200 Monte Carlo simulations to estimate average earnings.[7] We estimate that 13 out of the 69 cents are due to inertia while the remaining 56 come from insensitivity. In other words, around one fifth of the gap between the rational expectations payoff and the actual payoff can be attributed to inertia while four fifths can be attributed to inattention. Inertial behavior cost our subjects around one percent of expected earnings. This amount is small, but

[7]These are done in the same exact way as we have already described in previous sections.

significant, especially considering that it comes from cognitive costs *alone*. To put it in perspective, one percent is the the average growth rate of real consumption expenditures in the United States.

Alternative Explanations

We conclude with a discussion of alternative explanations for our data. Since alternative models have different numbers of parameters, we use the Bayes Information Criterion (BIC) to score our models. The BIC weighs improvements in the log likelihood against a penalty for additional parameters. It is defined as $BIC \equiv -2LL + K\log(n)/n$. Where LL is the log likelihood, K is the number of parameters and n is the number of data. Model (a) would be selected over model (b) according to the BIC if it had a lower score. As an example, the rational expectations model has a BIC of infinity; because although it is a zero parameter model the likelihood that our data are generated by that model is zero. Table 15 reports the BIC scores for a few alternative models that cannot be otherwise rejected as possible explanations. The right hand column reports the BIC for models that do not condition on past actions (No Inertia), while all the models in the left hand column condition on c_{-1}. Our model of inattentive reconsideration (IR) at the bottom of the left-hand column has to lowest score among the models we look at.

We include models that do not condition on past actions to allow for the possibility that capturing this feature of the data leads to overfitting. We consider two econometric models—probit and logit—the rational expectations model, the rational

Table 15: Model Selection: Bayes Information Criterion

	INERTIA	NO INERTIA
Logit	19,819	20,551
Probit	20,064	20,799
RE		∞
RE+noise		23,516
RI		20,419
RI+switch cost	19,851	
IR	19,623	

expectations model with additive noise and a rational inattention model like the one proposed by Sims (2003). The probit and logit models include only x, x^2, and r as regressors as well as a constant. Since the deterministic threshold function $s(x)$ will obviously be rejected by the data we relax this strong assumption by allowing for additive noise in the implementation of the RE policy, this is the RE+noise model. Just like the RE model, however, only x and r enter into the policy function. In the RI model subjects choose the probability of buying subject to the mutual information cost function. The solution is a logistic distribution that once again only depends on the state. The RI model does best among this class of models. It is interesting how poorly the RE+noise model does. It is even beat by statistical models. This suggests that subjects are not just trembling in their implementation of the threshold $s(x)$. None of these models, however, have higher scores than any of the models that condition on past choices. In particular, both the logit and probit models' score improves by including c_{-1} as a regressor. Yet another piece of evidence pointing toward the importance of inertia in our data. We now turn to alternative explanations of this fact.

One could argue that habits result from cognitive biases that we would otherwise avoid if we could help it. If this were the case, then the fact that our experiment makes the objective time separable would not mitigate this type of behavior. Consider the case of external habits. Under external habits, agents derive utility from the quasi difference of their own consumption and *others'* consumption. It may be that agents would prefer not take other's consumption into consideration but simply cannot help feeling envy. Our subjects could not have conditioned on each others' outcomes. They did not observe each other's payoffs or history of play.

Habits could also be formed by subject's own past consumption. The fact that our subjects only got paid at the end makes this implausible. Yet it could be that the same biases that would lead subjects to become attached to goods make them somehow attached to the rental offers in our game. Suppose that in any given turn our subjects did not consider r_t but rather $r_t - \lambda R_t$ where the stock of habits, R_t is a linear combination of the elements in the history of rents accrued $\{c_{t-k} r_{t-k}\}_k$. Let $R_t = r_{t-1} c_{t-1}$. This is the simplest form of habits; the habit stock depends only on the previous period's consumption. These types of preferences would *not* lead to inertial behavior. The reason is that habits from preferences relies on *two* assumptions: non-linear utility and non-time-separable preferences. If the objective is linear, as in our experimental setup, previous consumption is a sunk cost. The disutility $r_{t-1} c_{t-1}$ is independent of the choice and therefore not relevant for today's decision. We can also test this in the data by including a the term $r_{t-1} c_{t-1}$ in our regression. This term is not significant.

Under a richer specification, R_t may depend on further lags of accrued rentals. In this case inertia would be generated because R_t is part of the state. In the previous case, only today's choice carries forward so past actions do not enter into continuation value of today's choice. If R_t has more lags then past actions will now be part of the continuation value of today's choice. R_t is still a sunk cost, but inertia is embedded in the continuation value of the bellman equation. The intuition is that the agent anticipates how the habit stock will affect future utility and takes that into account when making choices today. If the habit stock going forward contains past choices, then through R_t those will influence today's choice. For simplicity, let R_t be the total rents accrued up to time t. During the experiment the screen displays this total, making it easy for subjects to potentially condition their decisions on this statistic. We can test this by including R_t as a regressor in equation (2.7). The coefficient on cumulative rents is not significant, with a *p-value* of .8. The point estimate is $-2.2 \cdot 10^{-8}$ with a standard error of $8.8 \cdot 10^{-8}$. Meanwhile the estimates for the other coefficients remain unchanged.

One could alternatively argue that though there are no external switching costs, there is a behavioral attachment to past actions that is costly to break. This is similar to the habits model described above, except that the stock of habits is built over actions rather than over rental payments. In this case a subject would pay a fixed cost λ every time her actions in period t are different from her actions in period $t-1$. We combine these costs in with rational inattention—*RI+switch cost* in Table 15—and estimate the model via maximum likelihood. The key difference between this and our model of inattentive reconsideration is that inertia here is not state-dependent. In this model the costs of switching create a wedge in the odds ratio equal to $\exp\{\theta\lambda\}$, where θ is the marginal cost of attention. It has a higher score, and thus a worse fit, than our model of state-dependent inertia even though it has one fewer parameter. The key is state-dependence; the model with fixed switching costs will either over-predict inertial behavior in states were it should not matter, as shown in Figure 112, or under-predict it everywhere else.

Conclusion

We have run an experiment to replicate the consumption problem studied in macroeconomics. We have concluded that the failure of people to implement the rational expectations policy must come from cognitive limitations. We have identified two such types of limitations: costs of reconsideration and, conditional on reconsidering, costs of deliberation. The first leads to inertia while the second to—partial—insensitivity to the state. Although our data are generated in highly stylized environment, they nonetheless display these two features which have been documented not only on aggregate consumption data, but increasingly at the household level as well.

Our estimates show that even small cognitive costs can have stark behavioral implications. While state-dependent habitual behavior may have cost players less than one percent of the winnings they would have had without it, inertia of this type nonetheless has crucial implications for the reactions of aggregate demand to shocks. State-dependent inertia implies that aggregate consumption will be more forward-looking in times of crisis than during normal times.

At the micro level, our model also makes predictions about individual household consumption choices. It suggests that these choices may be akin to how firms set prices. In our model, inertia in aggregate consumption comes from the extensive margin. Individual consumption in our model need not react to shocks even if aggregate consumption does. This is, of course, an empirical question and an obvious direction for future research.

Finally, we want to highlight how our findings contribute to a growing literature that models short-run macroeconomic fluctuations as arising from some type of "bounded rationality." One of the truisms associated with this literature is that while there is only one way to behave rationally there are an infinite number of ways to deviate from the rational expectations benchmark. Our paper takes this concern seriously and makes use of experimental data to discipline the way we model bounded rationality. Going forward, experiments may prove a useful tool for macroeconomists as a way to discriminate among behavioral models. The fact that we adapt a model originally developed to explain *pricing* behavior is also significant. Calibration exercises of general equilibrium models usually involve adding many different ingredients ad hoc to match the inertia we observe in the data. Our paper is yet another piece of evidence that perhaps we can move toward a more parsimonious model where the same cognitive limitations—present in all agents—can be responsible for price sluggishness and consumption inertia.

Chapter 2

Experimental Tests of Excess Sensitivity

Introduction

Understanding how consumers react to anticipated events has been a central question in macroeconomics ever since Flavin's (1981) seminal paper. In the standard model of intertemporal consumption used by macroeconomists, absent external frictions, consumption depends *only* on the households' expected present discounted value of wealth. This, in turn, leads to a testable prediction that has generated an entire literature in macroeconomics: consumption should not react to *anticipated* changes in wealth. Using both the time series properties of income and, more recently, anticipated government payments, economists have found ample evidence that consumption *does* react to anticipated income; or in the jargon of the literature: found *excess sensitivity* of consumption to income.

Although field data have convincingly established excess sensitivity as en empirical fact, they have had less success answering the following two important and related questions:

1. *What is the mechanism that leads to excess sensitivity?*

2. *Does excess sensitivity arise from an underreaction to news, an overreaction to income or both?*

The first question is about *what* kind of models generate excess sensitivity of which there are two broad classes in the literature: models with external frictions and models of bounded rationality. In the former consumers are rational but constrained even when they have positive wealth. Kaplan and Violante (2014) consider consumers whose portfolio includes high return assets with a fixed cost of liquidation. Chetty and Szeidl (2015) consider "consumption commitments" such as housing. These are goods whose level of consumption can only be changed by paying a fixed cost.

Models with bounded rationality relax the assumptions of perfect information and costless attention and cognition of the standard model. In Reis (2006) some consumers choose to be hand-to-mouth while others update their information sets only infrequently. Gabaix (2014) proposes a model where consumers do not pay attention to certain state variables and thus do not react to any changes in those variables. More recently, Azeredo da Silveira and Woodford (2018) generate excess sensitivity through a model of imperfect recall.

The second question is about *how* excess sensitivity comes about. In models with external constraints, excess sensitivity results from an underreaction to news. In these models agents would change their consumption upon receiving news if they were not constrained. In behavioral models either underreaction to news or overreaction to income are possible. In Gabaix (2014), for example, consumers do not react sufficiently when news arrives and therefore continue to adjust when income arrives. Whereas in Azeredo da Silveira and Woodford (2018) consumers overreact to the arrival of income.

In this paper we argue that these two questions are difficult to answer using field data because of the informational disadvantage of the econometrician relative to the subjects who generate the data. To overcome this disadvantage we design a novel experiment in which we have subjects complete a task analogous to an intertemporal consumption problem with stochastic income and news. Importantly, our subjects always have all of the information required to implement the optimal consumption plan. Our experimental data offer the following answers: First, we find evidence of excess sensitivity in the absence of credit constraints; this suggests that bounded rationality is at play. Second, we find evidence of *both* underreaction to news and overreaction to income.

Although it may seem as if this is a simple exercise in model selection, these two questions are relevant to several literatures. Recently there has been a growing literature that uses field data to distinguish among competing explanations of excess sensitivity (*Q1*). Kueng (2018) uses transaction data to test how consumption in Alaska responds to the annual dividend payment of the Alaska permanent fund. He argues that because the payments of the Alaska permanent fund are paid out every year in October and the value of the payment is widely reported and searched online, any increases in consumption due to the dividend payment cannot capture "surprises." He finds that consumption in Alaska does not react in the months prior to the disbursement of the dividend and that even households that are not credit constrained increase their consumption of nondurables in October. Ganong and Noel (2019) consider changes in spending during and after unemployment benefits. They find sharp reductions in consumption the month that unemployment benefits expire even though this is not random. Models of credit constrained consumers like Kaplan and Violante cannot account for excess sensitivity when income falls.

Our findings are in line with those of Kueng and Ganong and Noel, but because our data are generated in a lab, our identification is cleaner. Although Ganong and Noel's results seem to suggest a behavioral explanation for excess sensitivity, this is only under the assumption that expectations about future employment prospects do not change. Either perfectly rational agents with imperfect information or boundedly rational agents could account for the findings of Ganong and Noel. In contrast, we can rule out imperfect information as a source of excess sensitivity in our experimental results.

Our paper also contributes to a literature that sets out to measure agents reaction to news directly (*Q2*). Landier *et al* (2017) study how subjects forecast random variable in an experimental setting. They find evidence of both extrapolation and underreaction although in their setting extrapolation tends to dominate. Similarly we find evidence for both – underreaction to the announcement but overreaction to the event. Bordalo *et al* look at professional forecasts of macroeconomic and financial series and find evidence that forecasts overreact to news. An important distinction is that our experiment measures actions versus expectations.

Finally, our findings are relevant to the question of forward guidance in monetary economics. The uncomfortable result in standard New-Keynesian models that the power of forward guidance explodes as promises are made further into the future has led to a cottage industry of papers attempting to taper the reaction of consumers to news. As Werning (2015), shows, however, incomplete markets and credit constraints alone need not diminish the power of forward guidance. Both behavioral models such as Fari and Wening (2017) and Woodford (2018) as well as models without common knowledge as in Angeletos and Lian (2016) have been proposed to solve the so called forward guidance puzzle. The puzzle, remains, however, a purely theoretical one. As even though it seems highly unlikely to expect the type of response to aggregate demand predicted by the puzzle, we have no observations about policy commitments in the unboundedly far future. The results in this paper that subjects *underreact to news* provide some indirect *empirical* evidence that this is indeed a puzzle. Our findings suggest that the standard consumption model that leads to the explosive dynamics associated with forward guidance *overestimates* the degree to which agents react to news and therefore that the reaction of consumption to news predicted by the forward guidance puzzle is counterfactual. Furthermore, the lack of coordination motives in our experiment means that this underreaction is behavioral and not the result of coordination games without common knowledge.

The rest of the paper is organized as follows. In the following section we argue why experimental data are better suited than field data to answer our questions and briefly deal with the issue of external validity. In section 3 we describe the experiment and section 4 derives the rational expectations solution to the problem. Section 5 presents our main results. Section 6 concludes.

The Usefulness of Experimental Data

Experimental data allow us to overcome the information asymmetry that we face as econometricians when analyzing field data along three important dimensions. First, in the lab we are able to control our subjects' objective. Under the commonsense assumption—which we test in our data—that subjects want to maximize their expected earnings from participating in our experiment they have a time-separable objective with no free parameters. This means we can solve for the optimal policy function that maximizes those objectives.

Although the prediction that consumption should only react to income "shocks" does not depend on the shape of the utility function, it does matter when we set out to measure excess sensitivity in the data. As an example consider Kueng's main result that consumption of nondurables jumps in October when households receive the dividend from the Alaska Permanent Fund. If most of the extra spending occurs on *social* activities, then the timing of the payment could serve as a sunspot variable similar to holidays that allows households to overcome a coordination problem.

The second informational asymmetry that we can overcome in the lab is the amount of information that agents have at the time they make decisions. By giving our subjects all of the information they need in order to implement the optimal policy and clearly providing "news" to our subjects about future income we can clearly distinguish between choices before and after the arrival of news and income. Measures of excess sensitivity depend crucially on whether changes in income are anticipated *by the household* even if they are by the econometrician. Whether the drops in consumption that follow the end of unemployment benefits documented by Ganong and Noel qualify as excess sensitivity depend entirely on this assumption. The reason is that even if households are as aware as the econometricians about the length of their unemployment benefits, the econometricians are not as aware as the the household about their expected length of their unemployment spell. If, for example, upon becoming unemployed individuals expect to regain employment *before* their benefits run out then Ganong and Noel's findings are in fact evidence in *favor* rather than against the standard consumption model.

Finally, in the lab we are able to observe our subject's entire financial position at the time they make each choice. Importantly, so do our subjects. Measuring wealth and the degree to which households are credit constrained is notoriously difficult, if not impossible, using field data. Even detailed financial statements offer only an incomplete picture of a households' access to credit and insurance against idiosyncratic income risk. Even if households find it costly to liquidate some of their assets as in Kaplan and Violante, they may nonetheless have access to non-market sources of credit through family and other social ties. In fact, Chiappori *et al* (2014) find strong evidence of full insurance among households in Thai villages.

Knowledge of our subjects' objective, information, and wealth allow us not only to compute their policy function but also *evaluate* for every choice that they make. This provides us with a clean identification strategy because we can compare their choices against those of the optimal policy. Our subjects display excess sensitivity if they react in excess of what the optimal policy would have prescribed. It also allows us to look at levels rather than just changes – which is crucial in order to distinguish between overreaction to income and underreaction to news. Because our data allow us to compare levels against those predicted by the model we can also spot additional deviations that may otherwise wash out by taking differences. We find, for example, that at higher wealth, consumption levels are lower than those predicted by the theory even though our parametric estimates of excess sensitivity imply that it is decreasing in the level of wealth.

The degree of control in the lab also allow us to narrow the explanations for the excess sensitivity in our data. Neither credit constraints nor consumption commitments can possibly generate excess sensitivity in our setup. Also, all of the relevant information that our subjects need is always on display in the screen when they make their choices. This rules out imperfect information as a possible source. Our experiment allows us to isolate *cognitive* limitations as the only source for the disparity between observed behavior and the behavior predicted by the optimal policy function.

Our results are therefore, positive. The purpose of our experiment is not to reject credit constraints, consumption commitments, coordination motives, or imperfect information as possible sources of excess sensitivity. Rather, our findings assert that cognitive limitations *do* in fact give rise to the types of puzzles observed in the data on consumption.

The advantages of experimental data are, of course, tempered by concerns of external validity. A common concern about choices generated in a lab is that even if they are incentive compatible, if the stakes are low enough subjects will not bother to exert effort. In the "real world", so the argument goes, the quality of choices should be better because the stakes are higher. This criticism, however, strengthens the underlying hypothesis of this paper: that cognition is costly and therefore the degree to which subjects deviate from optimal behavior depends on the incentives they face. And even if they ultimate monetary payout from participating in our experiment is relatively low, we have designed our experiment so that the stakes *within* the game they play are high. Responding to the incentive structure of the game–the arrival of income, the arrival of news–has a big effect on the percentage increase in their payouts. The expected payout of participating induced by the optimal policy is over 1.5 times higher than the expected payout associated with an unresponsive policy. Empirically, we also find that subjects do react to the incentive structure of the game.

Moreover, we find evidence that measures of the subjective value of wealth are broadly in line with the predictions of the theory of dynamic programing. One of the advantages of our experimental setup is the ability to estimate the value function of wealth by successive polynomial approximations. One of the standard results in dynamic programing is that the value function should be increasing and concave in wealth. Up to a second order [1] our data show that the value of wealth is indeed increasing and concave.

Our data also display two important features of the consumption data both at the aggregate and household level: excess sensitivity – the topic of this paper – and excess smoothness. The fact that the two most important puzzles in the consumption literature pop up in our data suggests that not only is our experimental design analogous to the dynamic consumption model used in workhorse macro but also that the data it yields are analogous to consumption choices made outside of the lab. And therefore useful in answering the questions posed in this paper.

Experimental Setup

The central tradeoff in the dynamic consumption savings problem is the choice between consumption in the present versus consumption in the future subject to the law of motion of wealth. The key features of this problem are an exponential discount factor and a value function that depends on expectations of future wealth conditional on all information available at any given time. This problem can then be solved in a straightforward manner using dynamic programing techniques. The solution is a *time-invariant* policy function that solves a bellman equation and the value function is concave in total wealth. Finally, any test of excess sensitivity requires us to specify a stochastic process of income and news. Our experimental design includes all of

[1] higher orders are not statistically significant

these features.

We describe the problem to our subjects as a real estate investment game. It is, in effect, a version of the Hasbro game MONOPOLY made simpler by removing the uncertainty associated with the returns on each investment. To fix ideas, at the beginning of every period a single property is offered to the subjects and they have to decide whether to buy it or pass. If a subject decides to buy the property she receives a one-time "rental" payment. At the end of the experiment subjects are paid a dollar amount proportional to the total rents they managed to accrue during the game.

Rents.—The rents on the properties are drawn from a discretized log normal distribution and range from 500 to $100,000$ separated by increments of 500. The draws are independent across periods. The distribution, the value of the draw and the place in the distribution from which it was drawn are all known to the subject before she makes a decision each period. It is important to underscore that when subjects buy a property they receive the rental payment only in the period in which they buy the property. Although we call them "rents" these payments are not rents as economists usually think of rents. Because these are one-time payments subjects do not need to solve a present value problem when deciding the return to buying a particular property. Also, because rents are *iid* they are *not* a state variable of this problem.

Wealth.—Subjects pay for the properties on offer with "cash." Subjects start each game with a cash endowment of 11 million “dollars.”[2] All properties have the same price of one million dollars, which is deducted from subjects’ cash budget if they choose to buy. The interest rate is zero. Crucially, cash and rents are separate instruments. The rents payments are sunk in the sense that once they are received they are added to the running total. They cannot be turned back into cash. Furthermore this running total *alone* determines the payment that subjects receive from participating in the experiment at an exchange rate of 100,000 to 1 US dollar.[3] Subjects are not paid for any leftover cash they may have when the game ends. In our set up cash is the analogue of wealth and rents are the analogue of consumption in the standard dynamic consumption problem. Cash has no value beyond its ability to be turned into rents in the future through the purchase of properties. Similarly wealth is only valuable because it can be turned into consumption in the future.

Discounting.—So far we have have talked about turns without defining the length of the game. Importantly, we do not want to have a deterministic number of periods since this would be at odds with the time-stationarity of the bellman equation in the standard consumption problem. To induce exponential discounting the game continues after each period with probability $\delta = .998$. To ensure that our subjects understand the meaning of δ we not only tell them this probability but also explain that this means the game is expected to last 500 more rounds after every round.

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[2] We chose this amount because it is the average cash balances of the ergodic distribution of wealth implied by the rational expectations solution.

[3] The smallest rent represents a payment of half a penny while the highest rent pays out the equivalent of one dollar.

Income.—If subjects did not receive any income, their cash wealth would be exhausted only after 11 purchases. To replenish their wealth, subjects receive an income of 4 million with an unconditional probability of α=10 percent. Additionally, we announce the arrival of income to our subjects one period in advance. To incorporate news about income into an experimental design that admits a time-invariant solution, income and news are described by a three-state Markov process. In any given period subjects may find themselves in a "no news" state denoted by n_0, a "news" state, denoted n_1, or an "income" state, n_2. If subjects find themselves in n_0 they will, with probability $\lambda = .1114$ receive news; otherwise they remain in n_0. If subjects receive news then with probability $\beta = 1$ they will receive income in the following period as long as the game continues. This is an important distinction; news only pertains to future income and contains no information about the probability of continuation, which remains constant. Once they receive income they either receive news once again with probability λ or receive no news with probability $1 - \lambda$. Notice that this means that income is *always* announced in period in advance and therefore is always anticipated. We extend the restriction that income is always announced to the initial period of the game. There is a 90 percent probability that subjects start the game without news and 10 percent probability that they start with news.[4]

Borrowing Limit.—Finally, as in all consumption problems we have to define a limit on borrowing. We follow Aiyagari (1994) and define the natural debt limit

[4]Note that the restrictions on the ergodic distribution of the markov chain along with the transition probability between news and income pin down the value of λ.

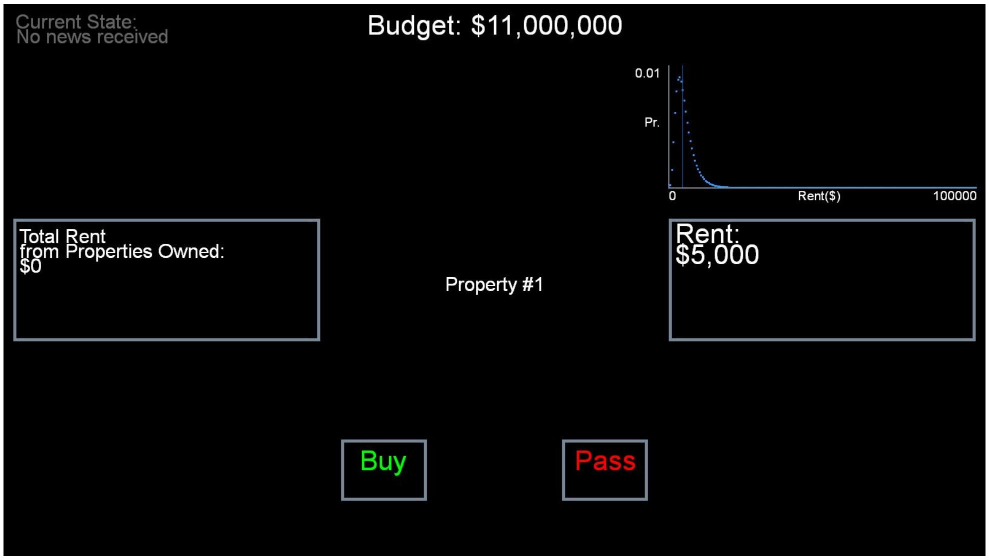


Figure 21: The computer screen during a single trial of our experiment. While subjects deliberate, we present to them information regarding the distribution of possible rent values, the current rent value, the accumulated rents of the subject, the number of trials, their current wealth (Budget) as well as news about income.

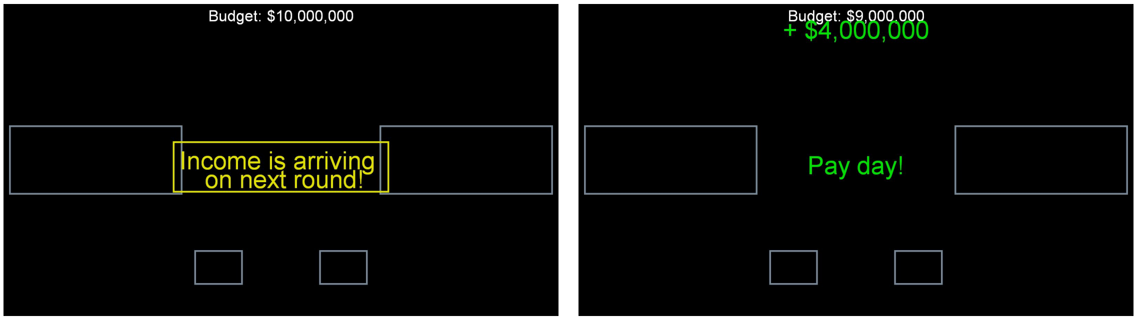
as the largest amount of debt that can be repaid *ex ante with probability one*. This means that the natural debt limit in our setting is given by the infimum over the sums of all income sequences that occur positive probability. Since *ex ante* it is possible, if extremely unlikely, to never receive any income, the infimum over this set is zero. This rules out debt entirely: our subjects cannot borrow in order to buy properties. Although we impose a no-borrowing constraint this constraint is not tighter than the natural debt limit.

Figure 21 displays a screenshot of the experiment in the first period of the game. At the top left subjects can see whether the signal about future income has arrived. In this case no news about income has been received. In the top center, subjects can see their total wealth, labeled "Budget." The top right graph is the probability mass function of the discretized log-normal distribution. The vertical line running through the graph represents the particular draw from the distribution of rentals, which in this case is 5000, as shown just below the graph. In this way subjects know both the rental amount and, visually, the probability of receiving a worse offer.

In addition, we give subjects two additional pieces of information. The center of the screen displays the number of rounds that have elapsed. Since this is a screenshoot of the first round, this is Property #1. On the middle left we keep track of the running total of rental payments that subjects have thus far accrued. Subject's payment from participating in the experiment depends only on this number. Recall that rents are one-time payments so this number is only updated when subjects buy a property by the amount on the right of the screen.

The bottom of the screen displays the choice buttons. To illustrate the evolution of the game, consider a possible history if the player facing the screen in figure 21 chooses to buy. One million will be deducted from her budget and since she has not received any news about income with probability one her budget in period 2 will be 10 million. The total rent from properties owned on the left hand side will go from \$0 to \$5,000. With probability λ the subject will receive news that income is coming.

Suppose that in period 2 the subject does jump from n_0 to n_1. In this case, before the screen updates to the second round, the subject would see the screen in Figure 22(a). Notice that the Budget has already been updated to illustrate the



(a) News (b) Income

Figure 22: Screenshots displaying news about future and current income.

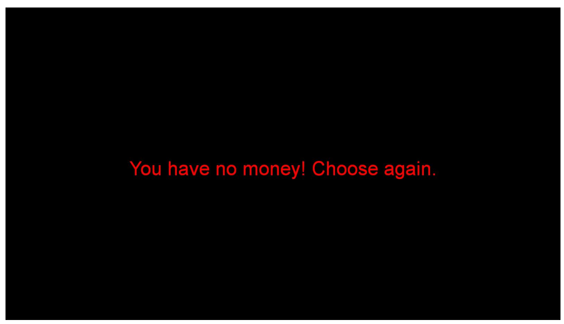


Figure 23: No Borrowing Allowed.

$1,000,000$ cost of buying the property. This notification is displayed for three second before returning the subject to a screen like the one displayed in Figure 21. If the subject where to choose to buy yet again then she would see the screen in Figure 22(b) announcing the arrival of income. This screen would again display for three seconds before returning our subject to the main interphase. Her budget would then display $13,000,000$ and she would see “No news received” in the upper left hand corner.

We do not ask subjects to confirm their choice once they press either “Buy” or “Pass.” Once they make a choice they see a screen for 1.5 seconds that displays the changes to their budget (0 or -1 million) and their total rental payments (0 or rental payment on offer). After this the game either continues or terminates.

It is also worth mentioning the information we do not give, and the restrictions we do not place, on our subjects. Once they are in the main interphase they have to time limits to make their choice. We do not want to impose any of the *external* cognitive constraints which would inevitably come from a time limit. In this spirit we do not display any time keeping devices. We also reset the mouse cursor halfway between the "Buy" and "Pass" buttons to remove any external costs of switching that might otherwise introduce correlation of choices between the "news" and "income" states. Finally, we do not restrict our subjects' choices when they are credit constrained. They can press either "Buy" or "Pass" even when they have exhausted their Budget constraint. If they choose "Buy", however, they are taken to the the display in Figure 33. This design allows us to decouple actions from consequences.

Rational Expectations Solution

In any given period an agent in our experiment must ask herself, "are the rental gains from buying the property currently on offer worth increasing my chances of finding myself credit constrained in the future when a much better offer is on the table?" To evaluate that tradeoff she needs to know the rental on offer, her budget, and the probability of receiving income in the future, all of which are available to her in our experimental setup.

Consider an agent at the very beginning of period t. She starts the period with the wealth, signal pair $(\tilde{x}, \tilde{n})_t$. Given this pair she must choose sequences of actions $c \in \{0, 1\}$ contingent on each possible evolution of wealth, x, and rental values, r, she may face in subsequent periods $t+k$. Formally, she seeks to maximize the following recursive objective:

$$\max_{\{c_r((x,n)_k)\}} \mathrm{E}_{f((x,n)_t,r|(\tilde{x},\tilde{n})_t)}[rc_r((x,n)_t)+\delta\mathrm{E}_{f((x,n)_{t+1},r|(\tilde{x},\tilde{n})_{t+1})}[rc_r((x,n)_{t+1})+...$$
$$+\delta^k\mathrm{E}_{f((x,n)_{t+k},r|(\tilde{x},\tilde{n})_{t+k})}[rc_r((x,n)_{t+k})+... \tag{2.1}$$

subject to the budget constraint (BC)

$$\tilde{x}_{t+k} = \begin{cases} \tilde{x}_t - c_t & \forall c \in \{0,1\}, \quad \text{if } \tilde{n}_t = \{n_0, n_2\} \\ \\ \tilde{x}_t - c_t + 4 & \forall c \in \{0,1\}, \quad \text{if } \tilde{n}_t = \{n_1\} \end{cases} \tag{2.2}$$

and the no borrowing constraint (CC)

$$\tilde{x}_{t+k} \geq 0 \tag{2.3}$$

both of which must be satisfied in every period $t+k$. We economize on zeros by deflating the units of wealth by one million so that the price of each property is normalized to one and income is now $y \in \{0,4\}$.

The key to understanding the objective is the conditional distribution over which the expectations operator is evaluated. The distribution f conditions on the state variables $(\tilde{x},\tilde{n})$. Because rentals are drawn iid we can rewrite $f(\cdot)$ as the product of the marginals $f = g(x,n|\tilde{x},\tilde{n})h(r)$.

In order to characterize the conditional distribution g it is helpful to notice that the state (x,n_3) is equivalent to the state (x,n_0). This means that we can reduce the Markov chain that describes news and income from three states to two. There are only three cases to characterize. If the agent starts the period in the state $(\tilde{x}, \tilde{n}_0)$ then when she makes her buying decision she will either remain in that same state with probability $g(\cdot) = 1 - \lambda$ or else will have moved into the news state (x, n_1) with probability $1 - \lambda$. If, on the other hand, she starts the period in state $(\tilde{x}, \tilde{n}_1)$ then when she makes her decision she will have moved to state $(\tilde{x} + 4, n_0)$ with probability one.

The recursive nature of problem (2.1)-(2.3) allows us to consider instead the related dynamic programming formulation, which Lucas and Stokey assure us will have the same solution. We can write the value of each choice given the triple (x, n, r) as

$$\tilde{V}(x, n, r) = \begin{cases} \max_{c \in \{0,1\}} cr + \delta V(x - c, n) & \text{if } x > 0 \\ \\ \delta V(x, n) & \text{if } x = 0 \end{cases} \tag{2.4}$$

The value function is then the conditional expectation of $\tilde{V}(\cdot)$ given by f.

$$V(\tilde{x}, \tilde{n}) = \begin{cases} \mathrm{E}_{h(r)}\ \lambda\tilde{V}(\tilde{x}, n_1, r) + (1 - \lambda)\tilde{V}(\tilde{x}, n_0, r) & \text{if } \tilde{n} = n_0 \\ \\ \mathrm{E}_{h(r)}\ \tilde{V}(\tilde{x} + 4, n_0, r) & \text{if } \tilde{n} = n_1 \end{cases} \tag{2.5}$$

Let R denote the set of all rental values, X the set of all possible wealth levels (the integers), and $N = (n_0, n_1)$ the two possible news states. Equations (2.4)-(2.5) define a contraction mapping that can be solved via value function iteration. This process then yields a solution $c : X \times N \times R \longrightarrow \{0, 1\}$.

Initially, it may seem puzzling that we are attempting to measure excess sensitivity in a setting with binary choices. However, the following proposition characterizes an alternative solution to the bellman equation.

Proposition 1. *The solution to equations (2.4)-(2.5) is a threshold rule given by*

$$s : X \times N \longrightarrow R$$

where the agent buys if and only if $r \geq s(x, n)$*. Furthermore,* $s(\cdot)$ *is weakly decreasing in* x *and* n*.*

To see why the first part is true, assume that the solution is not a threshold. That means there exists at least one case where $r' > r''$ and $c(x, n, r') = 0$ while $c(x, n, r'') = 1$. Equation (2.4) implies the following two inequalities: $r'' + \delta V(x - 1, n) > \delta V(x, n)$ and $r' + \delta V(x - 1, n) < \delta V(x, n)$. But $r' > r''$ so both can't be true simultaneously. The second part follows from the same kind of argument.

The importance of this result is that it establishes the *behavioral equivalence* under the hypothesis of rational expectations between the simple binary choice problem that our subjects face in the lab with the richer and potentially more complicated problem of choosing a threshold $s(\cdot)$. Although we ask our subjects to choose whether to buy a property given (x, n, r) we could have equivalently asked them to choose an expected rental given (x, n) defined by the threshold function $s(\cdot)$. The binary choices in our experiment are not themselves similar to choosing a *level* of consumption or, equivalently, savings. A threshold, on the other hand, *is* a level. Experimental practicality requires that we discretize the rental space. We could, however, impose finer and finer discretizations until the set of rental values R becomes dense in the non-negative reals $\mathbb{R}_+$.

The analogy between the choice of threshold in our setup and the choice of savings in the dynamic consumption problem goes beyond the fact that they are both levels. Like savings, $s(\cdot)$ depends on wealth (x) and all of the information about future wealth (n). And, like savings, it is decreasing in x and n. Recall that h is the discretized log-normal distribution of rentals. Let H be the cumulative mass function fo this distribution. Then we can write the probability of buying as a function of $s(x, n)$:

$$\pi(x, n) = 1 - H(s(x, n)) \tag{2.6}$$

Since $s(\cdot)$ is weakly decreasing in its arguments and $H(\cdot)$ is weakly increasing, $\pi(x, n)$ is weakly increasing in wealth and news. This probability is our experimental analogue of consumption.

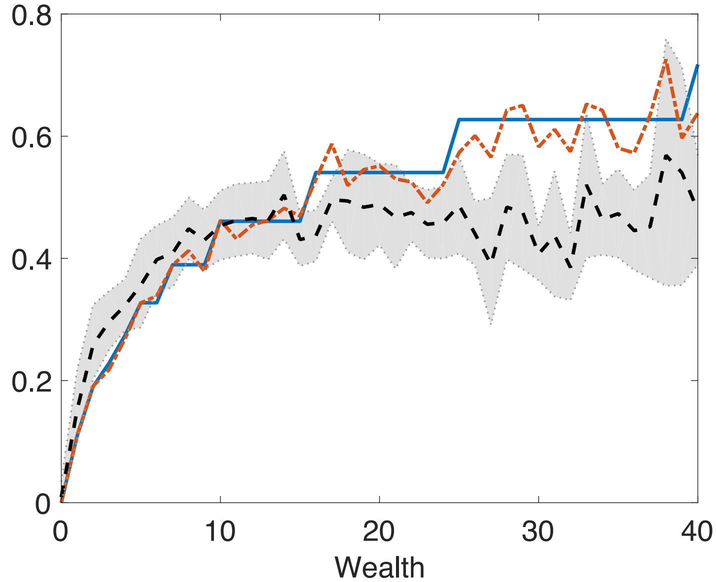
Equation (2.6) nicely summarizes the benefits of the experimental approach that we advocated in Section 2. First, $\pi(\cdot)$ depends only on wealth and news – two quantities that are nearly impossible to measure cleanly using field data but are trivially observed in our experiment. Second, we do not need to assume or else appeal to circumstantial evidence in order to establish that agents condition their choices on x and n. Not only do we know that they know both of these when choosing, but we can also test whether they pay attention to these values at all (they do).

Finally, we know the constrained optimization problem that agents face and therefore the *solution* to that problem. This gives us a clean counterfactual against which to compare behavior. It also allows us to go beyond measuring *changes* in behavior, in this case, the reaction of consumption to the arrival of anticipated income. We can also measure the degree to which the empirical probability of buying $\hat{\pi}(x, n)$ matches the theoretical one.

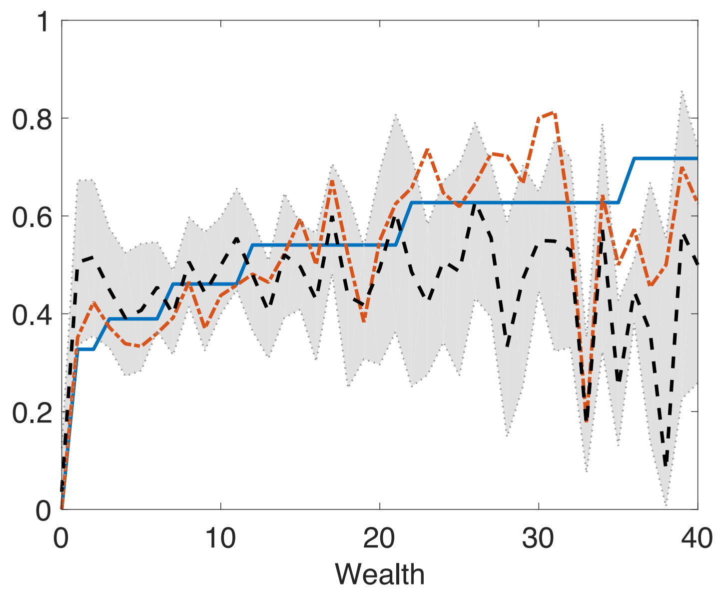
Results

We collected a total of 24,502 observations from 23 subjects at Columbia University. On average each subject played just over two games. Eight subjects played only one game while the subject who played the most games played five. Because the length of the games is stochastic, however, this does not correspond to the number of observations per subject. The subject with the fewest observations only played 436 rounds over three games whereas the subject with the most observations played a total of 1,835 rounds over two games. If subjects played more than one game then one of the games was chosen at random for payment and this was explained to the subjects. On top of the earnings from playing, subjects received ten dollars for showing up.

We do not believe that fatigue is a factor because each round was surprisingly fast. Subjects took, on average only 1.5 seconds per choice, for an average of only 12.5 minutes for the average game. The average length of each game was longer, however, because of the additional screens which more than doubled the time it took to complete each game: around half an hour.



(a) Prob(buy$|x, n_0$)



(b) Prob(buy$|x, n_1$)

Figure 24: Probability of buying conditional on wealth and news. Wealth scale is in millions. Dashed black line is the empirical probability $\hat{\pi}(x, n)$. Confidence intervals, clustered by subject are shaded in gray. Solid blue line is the theoretical probability $\pi(x, n)$. Dashed red line is the probability given the realized rental draws $\bar{\pi}(x, n)$.

Levels

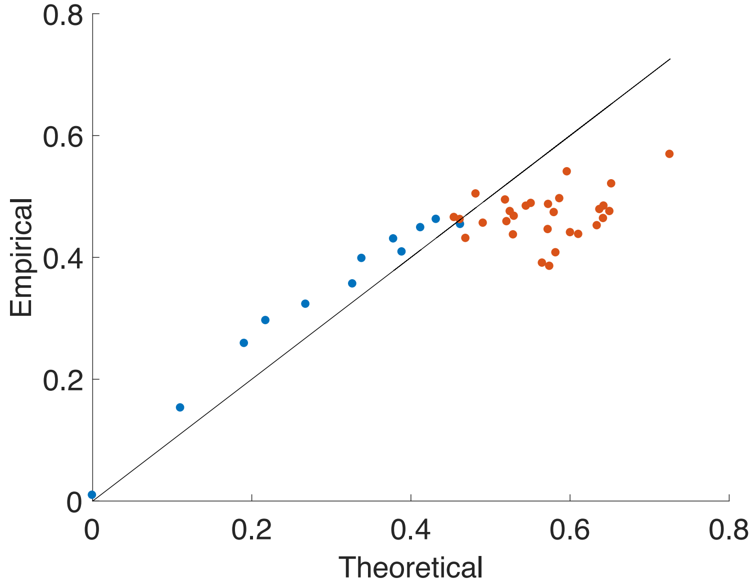
We begin by comparing the empirical probability of buying $\hat{\pi}(x, r)$ with the probability implied by the rational expectations policy. Since we are working with panel data and dealing with probabilities close to zero computing the empirical probabilities and their standard errors cannot be done by taking the sample mean. Instead we run logistic regressions on a constant and an indicator variable for news for each level of wealth. Since both the constant and news are necessarily independent of any subject specific characteristics, we expect the estimates under random effects to be unbiased. The logistic regression yields estimates of the log odds and their respective confidence intervals under clustered standard errors. We then transform these into probabilities.

There are two potential counterfactuals for our estimates, $\hat{\pi}$. The first is the theoretical probability as defined in equation (2.6). This would certainly be the probability we would expect to observe from fully rational agents as our dataset grows. In a finite dataset, however, the probability of buying following the threshold rule $s(x, n)$ might differ from $\pi(x, n)$. To make the distinction between these two, we define the optimal probability of buying in finite sample as $\bar{\pi}(x, n) \equiv \pi(x, n|\{r\})$ where $\{r\}$ represents the history of draws in our experimental data.

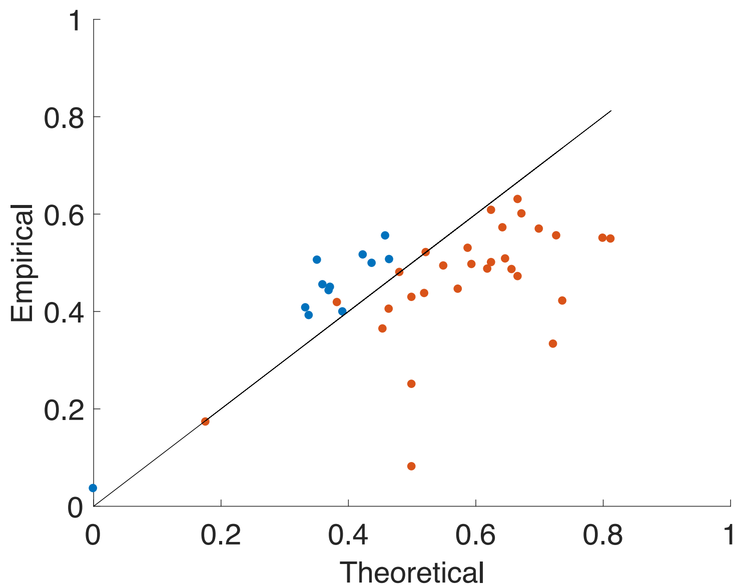
At first one might not expect $\pi(x, n)$ and $\bar{\pi}(x, n)$ to differ too much given how many data we have collected. Recall, however, that our state space is countably infinite. And since most of the mass of the ergodic distribution of wealth lies below $x = 20$ we expect this discrepancy to grow larger for higher values of wealth. We also expect this discrepancy to be larger in the news state since subjects spend on average only ten percent of the rounds in that state.

Figure 24 displays π in blue, $\bar{\pi}$ in red and $\hat{\pi}$ in black. The top panel shows the probability of buying as function of wealth in the no news state while the bottom shows the probability of buying as a function of wealth in the news state. Unsurprisingly we see that in the top panel, for low levels of wealth, π and $\bar{\pi}$ are right on top of each other. While π is a useful benchmark it is not the relevant counterfactual. We want to compare how our subjects did relative to what synthetic fully rational subjects would have done *given the same finite history of draws.* Not how they did relative to what synthetic fully rational subjects would have done *given an infinitely long history of draws.* Therefore, unless otherwise specified when talking about the theoretical probability of buying we mean $\bar{\pi}$.

There are two salient features in both panels of Figure 24. The first is that the dashed black line tracks the dot and dashed red line; subjects' response to changes in wealth is clearly correlated with the responses of the optimal policy. The second is that the deviations, both with and without news, are systematic. For low levels of wealth the empirical probability is higher, while for levels of wealth beyond the median(11 million), the empirical probability is lower than its theoretical counterpart. For low levels of wealth this difference is often within the 95 percent confidence region, however, for higher levels of wealth the empirical probability of buying is often significantly below the dashed red line even though the confidence bands are wider.



(a) No News



(b) News

Figure 25: Empirical probability of buying versus Theoretical probability of buying. Low wealth in blue, high wealth in red. 45 degree line in black.

Yet this discrepancy between the levels does not on its own imply an over or under reaction of subjects either to wealth or to news. There must certainly be an overreaction between $x = 0$ and $x = 1$, otherwise, $\hat{\pi}$ could not be above $\bar{\pi}$. But after $x = 1$ it is not clear that this trend continues. In fact, as wealth increases there must be an underreaction since $\bar{\pi}$ eventually surpasses $\hat{\pi}$.

Figure plots $\hat{\pi}$ versus $\bar{\pi}$ distinguishing between wealth up to the mean in blue and wealth above the mean in red. If behavior matched the rational expectations policy, all the points would lie along the 45 degree line. Remarkably, regardless of their information, subjects overspend below the median and underspend above it. But more importantly the degree to which they overspend when their wealth lies below the median is much less than the degree to which they underspend when their wealth is above it. This fact will drive our analysis of excess smoothness in subsection 5.3.

Subjects Condition on Income

One of the key predictions of the rational expectations solution is that (x, n) are both necessary and sufficient statistics to pin down $\pi(\cdot)$. Though income would surely be informative about the probability of buying if we could not observe wealth, it would only be informative to the degree that is contains information *about* wealth. If we were in the field and could not observe x cleanly, we might not be surprised that income would be correlated with $\hat{\pi}$. However, once we control for wealth, income should have no explanatory power.

To test this hypothesis we run the following linear regression:

$$c = \beta_0 + \beta_1 r + \beta_2 x + \beta_3 x^2 + \beta_4 n + \beta_5 y + \Phi z \tag{2.7}$$

Φz controls for subject fixed effects as well as learning and fatigue. For each subject, i we include as regressors the number of rounds t and t^2: $\Phi z_i = \phi_0^i + \phi_1^i t + \phi_2^i t^2$.

A well known result in Panel Data analysis is that for large N and finite T, the regression we have specified above will not be consistent since it includes the endogenous regressors x. This is usually overcome through first differencing as advocated by Arellano and Bond. In our case, however, we have large T and so standard asymptotic theory applies. Fixed effects regression for non-linear models is not always possible and the endogenous regressor x is almost certainly correlated with individual characteristics. Chamberlain (1980) and Wooldridge (2014), offer solutions to this problem for probit regressions but these are once again meant to be implemented with small T. Finally, since we are interested in the *sign* of β_5 rather than its magnitude, a linear model suffices to test our hypothesis.

We report our estimates in Table 21. For completeness we also report estimates of random effects probit and logit regressions. We report regressions both with and without including income as a regressor. In all cases β_5 is positive and significant, at the 1 percent level in the linear regression and at the .1 percent level in the probit and logit regressions. It is also important to notice that neither the estimates for the coefficients nor their standard errors are affected by including income in the regression. This suggests that income is just another variable on which subjects condition

	$Cons$	$Rents$	$Wealth$	$Wealth^2$	$News$	$Income$
Linear	-0.4191	11.091	0.0138	-0.0002	0.0503	
(FE)	(0.0453)***	(0.1961)***	(0.0027)***	(0.00002)***	(0.0142)**	
	-0.4232	11.089	0.0135	-0.0002	0.0536	0.0075
	(0.0450)***	(0.1963)***	(0.0026)***	(0.00003)***	(0.0146)***	(0.0021)**
Probit	-6.2237	84.2897	0.0953	-0.0009	0.3446	
(RE)	(0.4541)***	(6.2762)***	(0.0178)***	(0.0002)***	(0.0947)**	
	-6.2572	84.3997	0.0937	-0.0009	0.3665	0.0456
	(0.4536)***	(6.2603)***	(0.0175)***	(0.0002)***	(0.0994)***	(0.0139)***
Logit	-11.5397	157.695	0.1749	-0.0017	0.6132	
(RE)	(0.8930)***	(12.7115)***	(0.0335)***	(0.0003)***	(0.1728)**	
	-11.5982	157.874	0.1722	-0.0017	0.6518	0.0787
	(0.8941)***	(12.6982)***	(0.0329)***	(0.0003)***	(0.1816)***	(0.0246)***

$^{*}p < 0.05$, $^{**}\ p < 0.05$, $^{***}\ p < 0.001$. Standard errors clustered by subject. N=24,451

Table 21: Income

when deciding whether to buy.

The regression specified in equation (2.7) is a test of behavior. Because we control for wealth and news, and the estimates on their coefficients do not change when we include income as a regressor, we know that β_5 is not loading relevant information about the state but rather additional information on which subjects choose to condition their decisions.

This result speaks to both of our motivating questions. First, it establishes that there is excess sensitivity in our experiment and that to the degree that it arises it does from cognitive rather than external constraints on our subjects. It also speaks to our second question. This test of excess sensitivity in levels rather than in differences. It does not rely on the difference in probabilities of buying between the arrival of news and the arrival of income. Rather it looks directly at whether the arrival of income alone increases subject's probability of buying: there is an overreaction to income.

This result also informs the behavioral literature on consumption. For example, it is inconsistent with models of rational inattention where subjects can choose signals from an unrestricted (or at least a sufficiently rich) set. A rational inattentive agent of this type would *not* condition on income, choosing instead to receive a direct signal that depends only wealth, news and rents. Our experimental design also rules out imperfect information explanations since all of the information is freely available to our subjects at all times.

Subjects Underreact to News

Recall that in Figure 25 we plot the empirical probabilities of buying on the y axis against the theoretical probabilities of buying along the x axis. A clean test of the rational expectations hypothesis is to estimate the slope of the line of best fit in that figure and test whether it is equal to one. Formally we estimate the following regression:

$$\hat{\pi}(x,n) = \alpha + \beta_0 \bar{\pi}(x,n_0) + \beta_1 \bar{\pi}(x,n_1) + \gamma^n \mathbb{1}\{n_1\} + \gamma^y \mathbb{1}\{y=4\} + \varepsilon \tag{2.8}$$

We allow for different slopes β_0 versus β_1 as well as different intercepts γ^n between receiving news and not. And, given our results in the previous section we also allow for a different intercept when income arrives γ^n. What are these different coefficients measuring? The intercepts measure excess sensitivity. Under the null $\alpha = \gamma^m = \gamma^y = 0$. The slopes measure excess smoothness. Under the null $\beta_0 = \beta_1 = 1$. If either are less than one, we say that our subjects are excessively smooth – they *underreact* with respect to their fully rational counterparts given changes in wealth.

This parametric test of rational expectations improves on the local nonparametric tests in Figure (24) because it uses information for all wealth levels. However, estimating equation (2.8) via OLS is inefficient because it ignores a lot of information. The regressions in the previous section used over 24 thousand data points, this regression uses only 51.[5]

To leverage this information we estimate (2.8) via Feasible Generalized Least Squares (FGLS) under the null hypothesis of rational expectations. To do this, we rewrite (***) under the null and take the conditional variance

$$\begin{aligned} \mathrm{Var}\left(\hat{\pi}(x,n)|\bar{\pi}(x,n)\right) &= \mathrm{Var}\left(\bar{\pi}(x,n)+\varepsilon|\bar{\pi}(x,n)\right) \\ &= \mathrm{Var}\left(\varepsilon|\bar{\pi}(x,n)\right) \end{aligned}$$

Now note that, for any given history $\{r\}$, $\bar{\pi}$ is not a random variable. So we can simplify the lefthand side: $\mathrm{Var}\left(\hat{\pi}(x,n)|\bar{\pi}(x,n)\right) = \mathrm{Var}(\hat{\pi}(x,n))$. Let κ denote the vector of the inverse standard errors $[\sigma(\hat{\pi}(x,n))^{-1}]$. If we multiply equation (***) by κ the resulting equation has spherical errors $u \equiv \kappa\varepsilon$ under the null. By the Gauss-Markov Theorem we have that the OLS estimator of this regression is BLUE.

By weighting each observation by its precision, the FGLS estimator corrects for what we observe in Figures 24 and 25: that while the probabilities of buying differ

[5]We do not include estimates for values above 51 because they are extremely noisy, extremely rare, and heavily reliant only a few subjects. This truncation preserves 96.6 percent of all our data.

α	γ^n	γ^y	β_0	β_1
0.022	0.089	0.063	0.805	0.649
$(0.022)^{***}$	$(0.035)^{*}$	$(0.019)^{***}$	$(0.020)^{***}$	$(0.072)^{***}$
$\beta_0 = 1$	$p = 0.000$			
$\beta_1 = 1$	$p = 0.000$			
$\beta_0 = \beta_1$	$p = 0.039$			
$\gamma^n = \gamma^y$	$p = 0.530$			

$^{*}p < 0.05$, $^{**}\ p < 0.05$, $^{***}\ p < 0.001$. $N = 50$. *Adj.* $R^2 = 0.934$

Table 22: Parametric tests of excess smoothness and excess sensitivity.

more from their theoretical counterparts at high wealth, they are also less precisely estimated.

We report our results in Table 22. The first thing to notice is that the rational expectations solution does a good job of fitting the data; the adjusted R^2 is 0.934. However, our data deviate in systematic—and familiar—ways from the rational expectations predictions. Our main results are as follows: our data test positive both for excess sensitivity and excess smoothness.

We find that there is excess sensitivity to *both* income and news and we cannot reject the null that they have same magnitude. This is a surprising and sheds light on the determinants of excess sensitivity. Assume, for the moment that $\beta_1 = \beta_0 = 1$. In that case even though subjects overreact to both income and news, traditional tests of excess sensitivity—which cannot properly account for wealth—would not measure *any* excess sensitivity even though, subjects are overreacting relative to the rational expectations benchmark.

We now turn to excess smoothness. We strongly reject the null that $\hat{\pi}(x, n)$

moves one-for-one with $\bar{\pi}(x, n)$. The *p-values* for both hypothesis tests are less than .001 percent. But we also reject the null that the degree of excess smoothness is the same when subjects receive news than when they don't. In fact, we find that the subjects display more excess smoothness when they receive news than when they do not. This result provides a fuller answer to our second motivating question. Excess smoothness to news shows up as excess sensitivity to income. To see this, consider the qualitative implications of our results. Since we cannot reject $\gamma^n = \gamma^y$ we have that $\hat{\pi}(x+y, n_0) - \hat{\pi}_(x, n_1) = \beta_0\bar{\pi}(x+y, n_0) - \beta_1\bar{\pi}(x, n_1)$. But recall that we reject the null in favor of $\beta_0 > \beta_1$. Since the probability of buying given income and no news is weakly higher than the probability of buying given news, we have the following sequence of inequalities:

$$
\begin{aligned}
\mathrm{E}\ \hat{\pi}(x+y, n_0) - \hat{\pi}(x, n_1) = \mathrm{E}\ \beta_0\bar{\pi}(x+y, n_0) - & \beta_1\bar{\pi}(x, n_1) \\
& \geq \mathrm{E}\ \bar{\pi}(x+y, n_0) - \bar{\pi}(x, n_1)
\end{aligned}
$$

Although our result comes from an experimental setting, and only applies to "good" news—there is no bad news in our design—to the best of our knowledge this is the first empirical evidence that excess smoothness is exacerbated by information. This empirical finding is directly relevant to the literature on the power of forward guidance. Up to now, the forward guidance puzzle has been less a puzzle and more an uncomfortable theoretical implication. Puzzles, after all, are only puzzles if there is some empirical evidence at odds with the theory. Our finding that $\beta_1 < \beta_0$ offers some indirect empirical evidence for this puzzle. It does so because it calls into question the modeling assumption that gives rise to the power of forward guidance: the extreme forward-lookingness of rational consumers.

Conclusion

Is there a behavioral basis for excess sensitivity? And if so, what are the deviations from full rationality that give rise to this puzzle? Our paper has set to answer these two questions by generating and analyzing experimental data. By designing a simple game that is analogous to the dynamic consumption problem studied in macroeconomics we can easily answer the first question. We place our subjects in a setting where they *could* implement the rational expectations solution and find that they do not. Furthermore, we find that our subjects deviate from their fully rational counterparts in systematic ways. Nonetheless, they are "near" rational. In our setting, the failure to implement the optimal policy must arise from cognitive constraints as there are no *external* constraints that would prevent subjects from doing so.

We found several answers for the second question. First, we found evidence that subjects overreact to income controlling for wealth, but we also found that subjects tend to overreact to news. Finally we found evidence for *differential* excess smoothness to wealth. Both excess sensitivity to news and differential excess smoothness are novel facts that suggest additional puzzles that may be uncovered using field data.

Chapter 3

Persuasion and Coordination in Global Games

Introduction

Consider the canonical global coordination game of regime change: a unit mass of agents must each simultaneously decide whether to attack the status quo. Attacking is costly to the agents but profitable conditional on regime change. The strength of the regime — the fundamentals — can fall in three categories. They can be weak, in which case the attack of a single agent brings down the regime. They can be strong, in which case the regime will survive regardless of the measure of agents who attack. Or they can be somewhere in the middle, in which case the regime falls if and only if a large enough mass of agents attack.

This class of games has been extensively studied in the literature because the equilibria tend to be highly sensitive to the information structure of the agents. Under different assumptions about *what* information is available as well as *how* that information is generated the game described above can admit a unique or multiple equilibria. Under common knowledge, for example, the game exhibits coordination failures: there are multiple equilibria when the fundamentals are commonly known to be intermediate. In one equilibrium all agents attack and the regime falls, whereas in the other none of the agents attack and the regime survives.

Multiplicity is problematic for mainly two related reasons. First, it is not clear which equilibrium will be selected. This indeterminacy makes it challenging to conduct comparative statics exercises, without which it is difficult to prescribe policy interventions.

Morris and Shin (1998, 2001, 2003) obtain uniqueness by relaxing the assumption of common knowledge. Instead of commonly knowing the fundamental, Morris and Shin consider a game where agents receive exogenous private and public signals. Under this alternative information structure multiplicity becomes a knife-edge outcome that disappears even with an arbitrarily small amount of noise. They conclude, therefore that "multiplicity is the unintended consequence of common knowledge."

In a series of subsequent papers, Angeletos and his coauthors (2006a, 2006b, 2007) argue that Morris and Shin's uniqueness result hinges on the assumption of *exogenous* public information. In particular, Angeletos, Hellwig and Pavan (2006b), henceforth AHP, interpret the global game as a currency attack. They keep the exogenous private signals of Morris and Shin but introduce as the public signal the policy actions of the central bank. In their set-up, the central bank can take costly and *publicly observable* actions to change the cost of attacking for the agents. AHP highlight the fact that actions by the central bank to shore up weak fundamentals might be counterproductive because they can reveal to agents that the fundamentals are weak in the first place. This type of policy trap leads to multiplicity. One can summarize this result as "uniqueness is the unintended consequence of exogenous public information."

This paper argues that neither common knowledge nor endogenous public information necessarily imply multiple equilibria. Further, policy traps arise in AHP only because they consider a narrow definition of central bank policy. The paper develops a model with an alternative policy-as-endogenous-public-signal structure: *purposeful* communication by the central bank. The power of forward guidance in helping central banks achieve their monetary policy aims has highlighted the importance of communication as part of the policy tools available to the central bank when setting interest rates. As this paper shows, the central bank's ability to speak is also important when dealing with currency crises.

Recall, for example, the summer of 2012 when the fate of the Euro seemed as if it could go two ways. Investors might lose faith, launch a definitive attack on the single currency's weakest members and bring about the end of the monetary union. Alternatively, investors might decide that the European Central Bank (ECB) would act as a backstop to preserve the Euro, which would dissuade them from attacking and thus ensure the survival of the single currency. Then on July 6th, Mario Draghi, the head of the ECB, said "The ECB is ready to do whatever it takes to preserve the Euro... believe me, it will be enough." The rates on the sovereign bonds of Portugal, Italy, Ireland, Greece and Spain – the so-called PIIGS of Europe – immediately fell and the crisis was abated. Famously, this speech was all it took and the ECB never had to actually intervene in the markets.

To capture the effects of the central bank's communication policy, this paper develops a variant of the global game thus far considered with an *incomplete* but *commonly* known and *endogenous* information structure that admits a unique equilibrium. The structure of the game is as follows. First, a central bank who shares a prior with the agents about the fundamental chooses an experiment and credibly commits to revealing the outcome of that experiment to the agents. After the fundamental is drawn the bank receives a signal which it credibly relays to the agents. The agents then update their beliefs on the fundamental and decide whether to attack. Since there is no dispersed information, I consider only symmetric perfect bayesian equilibria in which all agents take the same action.

It is important to highlight three departures from the AHP setup. First, unlike AHP, the central bank is allowed commitment power. AHP consider the case of a discretionary central bank that chooses an optimal policy *after* observing the fundamental. Yet it is not immediately clear that the central bank should not be able to commit to a policy ex ante. When considering "optimal" policy rules by the central bank, the literature almost always is referring to commitment. If the bank can commit in the conduct of monetary policy, why not here? It is true that regime changes are rare events in the sense that their timing is random as opposed to interest rate decisions which are taken at regular intervals. It is therefore possible — though not necessarily true — that the commitment technology available to the central bank when setting monetary policy might be unavailable when dealing with currency crises. Yet even if that is the case, considering the optimal policy under commitment is still a useful benchmark that the literature has not yet fully considered.

My second departure from AHP, as already mentioned, concerns the definition of policy. AHP work with a Spence signaling game where the bank can take costly action to alter the payoffs of the agents. The agents observe these actions and can thus update their prior on the fundamental. Here I consider a central bank that engages in Bayesian Persuasion, as formalized by Kamenica and Gentzkow (2011). The bank can only commit to a signaling structure but cannot otherwise directly affect the payoffs of the agents. AHP model policy as the ability to alter payoffs, I model policy here as the ability to alter information. Policy, of course, is both; central banks can speak as well as act. An interesting extension, beyond the scope of this paper, would look at the informational effects of a bank that has access to both types of policy tools.

Finally, in both Morris and Shin and AHP agents have an improper prior over the real line and receive private signals centered around the true value of the fundamental. The model developed here introduces a proper prior and shuts down the private signals. The proper prior is necessary in order for the bank to choose and commit to a signal structure. Without a prior the bank has no way of evaluating its *own* expected payoffs, and agents have no way of applying Bayes' Rule to "make sense" of the bank's signals. Having an exogenous prior distribution plus private noise would make the informational structure of this model isomorphic to that of Morris and Shin. Allowing for the central bank to commit to the conditional distribution of a second public signal would not alter or contribute much to the uniqueness result of Morris and Shin. By shutting down private information, this paper shows that *even* when all agents have the same information, the bank – choosing an optimal signal given the prior distribution over the fundamental – can not only improve its own payoffs but induce a unique equilibrium.

Uniqueness is obtained through the interaction of two channels: the central bank's commitment power, and its beliefs about how agents will coordinate upon updating their information. Unsurprisingly, there are priors under which multiplicity will arise. In these cases, the Bayesian Persuasion problem requires the central bank to form beliefs on which equilibrium will be played. This highlights an important feature of the decision-making process faced by policymakers: ambiguity. Policymakers must often choose policy without having an objective distribution over the possible outcomes of such a policy. This notion rings especially true when dealing with currency attacks. This paper shows that ambiguity need not imply multiplicity. And further, that a policymaker's attitude toward ambiguity can act as a mechanism for selecting equilibrium play by agents.

The rest of the paper is organized as follows. Section 2 sets up the model. Section 3 discusses the Bayesian Persuasion problem of the central bank and derives the equilibrium. Section 4 considers the problem when the fundamental has support on the real line. Section 5 concludes.

Model

In the canonical global game of regime change the strength of the fundamental is parametrized by $\theta \in \mathbb{R}$. In the model analyzed here, by contrast, the fundamental can only take three values: $\Theta := \{\theta_L, \theta_M, \theta_H\}$. Recall there is a unit mass of agents. If the state is $\theta_L \leq 0$ the regime will fall if *anyone* attacks. If the state is $\theta_M \in (0, 1)$ the regime falls if and only if the measure of agents who attack is larger than θ_M. Finally, if the state is $\theta_H \geq 1$ the regime will survive even if *everyone* attacks.

Under the assumption of common information restricting the model to only three states is, as will be shown in section 4, without loss of generality. However, by working with only three states, the analysis can be carried out diagrammatically and the intuition behind the results is sharper. The natural interpretation of the discrete space is this. In the continuum case, the real line can be partitioned into three relevant regions: low fundamentals, intermediate fundamentals (where multiplicity arises), and high fundamentals. The "values" of θ in this model correspond to the "regions" that θ can be in when it can take any value in $\mathbb{R}$.

Agents.— There is a unit mass of agents indexed by i with two actions available to them: attack ($a_i = 1$) or not ($a_i = 0$). They must pay a fixed cost c to attack. If they do not attack, their payoff is normalized to zero. Agent's payoffs are given by the following table:

	$R = 1$	$R = 0$
$a_i = 1$	$1 - c$	$-c$
$a_i = 0$	0	0

where $R = 1$ denotes a change in the regime and $R = 0$ denotes a survival of the status quo. Let $A \equiv \int_0^1 a_i \, di \in [0, 1]$ denote the the mass of agents who attack. If the fundamental is weak then $\forall A > 0$, $R = 1$: the regime falls. If the fundamental is strong then $\forall A$, $R = 0$: the regime survives. If the fundamental is intermediate then $R = 1$ if and only if $A > \theta_M$.

The agents share a common prior with each other and the central bank over the fundamental: $\mu_0(\theta) \in \Delta(\Theta)$. Following the signal s from the central bank, agents must form updated beliefs about two things: the fundamental θ and the actions of other agents. In a setting with private information, higher order beliefs would come into play on the latter as players try to coordinate on the public signal given their private information. Let $\pi_{A|i} \equiv \text{prob}_i(A > \theta_M | \theta_M)$ be agent i's belief that $A > \theta_M$. $\pi_{A|i}$ is the belief that the regime falls conditional on the fundamental being intermediate. Having specified beliefs and payoffs, agent i will attack if and only if

$$c < \mu_i(\theta_L | \ s) + \mu_i(\theta_M | \ s)\pi_{A|i} \tag{3.1}$$

This condition has a straightforward interpretation. Recall that the value of attacking is one if the regime falls and zero otherwise, but the agent has to pay a cost c in order to attack. The optimality condition above simply says that the agent will only attack if the fixed cost of attacking is strictly lower than the expected payoff from attacking.

Notice that $\pi_{A|i}$ captures the coordination motive of the agents; the expected payoff from the attack depends not only on agents' beliefs about the fundamental, but their beliefs of each others' actions.

Central Bank.— The central bank wants the regime to survive. The central bank's payoffs are normalized as follows:

$$u(R) = \begin{cases} 0 \text{ if } R = 1 \\ \\ 1 \text{ if } R = 0 \end{cases}$$

This is a different objective from that specified in AHP, where θ is a preference parameter of the central bank and its objective is the difference between the size of the attack and θ: $u(A;\theta) = \theta - A$. In this model θ parametrizes the strength of the regime, not the bank's preferences. Although in global games of regime change it is standard to interpret θ as regime's "type," it is not obvious that it should enter directly in the central bank's objective. Considering that this is a static game, why would it matter to the bank whether the fundamentals are high or low as long as the regime survives? Of course, in equilibrium $R = R(A, \theta)$, so the indirect utility of the bank will depend on the fundamental.

In order to deploy the machinery of Kamenica and Gentzkow we need to rewrite the bank's objective as a function only of beliefs. Since agents share a prior and observe a common signal they will also share posterior beliefs $\mu_i(\theta|s) = \mu(\theta)$, which in turn means they will take symmetric actions so that A can only be either 0 or 1. Applying Proposition 1 of Kamenica and Gentzkow, from the Bank's perspective it must choose a straightforward signal structure $\pi(s|\theta)$ that induces either outcomes

$A = 1$ or $A = 0$.[1] We can now write the bank's utility as a function of beliefs:

$$v(\mu(\theta)) = \sum_{\theta \in \Theta} \mu_0(\theta) \int_0^1 a_i(\mu(\theta))\, di = \sum_{\theta \in \Theta} \mu_0(\theta) A(\mu(\theta)) \tag{3.2}$$

where the μ_0 (with the nought subscript) represents the prior while the μ (without a subscript) represents the posterior. When it is apparent we are referring to outcomes the i subscript is dropped because all agents hold the same beliefs and take the same action in equilibrium. It is important to highlight, as is clear from equation 3.1 that a_i also depends on $\pi_{A|i}$, and hence so does v. From the bank's perspective, however, $\pi_{A|i}$ is the same for all agents and so it is simply a parameter of v which is omitted for clarity.

Choosing the signal structure $\pi(s|\theta)$ is equivalent to choosing a distribution τ over the posterior beliefs $\mu(\theta|s)$. The only restriction that Bayesian agents impose on the central bank's problem is that the expectation of their posteriors must equal the prior. This is what Kamenica and Gentzkow call "bayes plausibility." The central bank's problem is thus to choose a τ to maximize its expected utility, $v(\cdot)$, subject to bayes plausibility. Formally:

$$\max_{\tau} \mathrm{E}_\tau[v(\mu(\theta))]$$

$$\text{s.t. } \mathrm{E}_\tau[\mu(\theta)] = \mu_0(\theta)$$

[1] We are essentially rewriting the central bank's objective as one that depends on A instead of R. One must be careful here because, as discussed above in the description of the agents' problem, A depends both on the beliefs $\mu_i(\theta)$ *and* $\pi_{A|i}$. The bank can persuade agents to update the former belief but not the latter. This issue is important, and discussed at length in the next section. Here, for ease of exposition and to fix ideas, I gloss over it and apply Proposition 1 from Kamenica and Gentzkow, but I will return to it in section 3.1.

Timing.— The central bank chooses a distribution τ to solve its constrained optimization problem. This induces a joint distribution $\pi(s,\theta) \in \Delta(S \times \Theta)$, which agents observe. A tuple (s,θ) are drawn but agents only observe s. Agents then update their prior on θ according to Bayes' Law as well as form a belief $\pi_{A|i}$. Each agent then chooses a_i. The equilibrium concept is a bank-preferred symmetric PBE; whenever agents are indifferent between actions they choose the action preferred by the central bank.

Equilibrium

Definition 1. An equilibrium consists of a distribution over posterior beliefs $\tau \in \Delta(\Delta(\Theta))$ and its associated signal structure $\pi(s|\theta)$ (recall that each signal is associated with a unique posterior belief), a symmetric strategy $a : \Delta(\Theta) \times \{0,1\} \to \{0,1\}$, and a common set of beliefs $(\pi_A,\ \mu(\theta)) \in \{0,1\} \times \Delta(\Theta)$ that satisfy

- *Strategies*

  a. $\tau \in \underset{\mathrm{E}_\tau[\mu(\theta)]=\mu_0(\theta)}{\arg\max}\ \mathrm{E}_\tau[v(\mu(\theta))]$

  b. $a(s) = \mathbf{1}\{c < \mu(\theta_L|s) + \mu(\theta_M|s)\pi_A\}$, and $A = \int_0^1 a(s)\ di$

- *Beliefs*

  c. $\mu(\theta|s) = \frac{\pi(s|\theta)\mu_0(\theta)}{\tau(s)}\ \ \forall\ \theta \in \Theta$, where $\tau(s) \equiv \sum_{\theta\in\Theta} \pi(s|\theta)\mu_0(\theta)$

d. $\pi_A = \begin{cases} 1 \text{ if attacking is the strictly dominant behavioral strategy given } \mu(\theta) \\ 0 \text{ if not attacking is the strictly dominant behavioral strategy given } \mu(\theta) \\ \{0,1\} \text{ otherwise} \end{cases}$

Conditions a and b are the optimality conditions of the central bank and agents, respectively, while condition c imposes bayesian rationality. All three conditions were discussed at length in the previous section. The first two cases of condition d also impose bayesian rationality. They say that if an agent finds it uniquely optimal to take an action *regardless* of what other agents do, he must put probability one on all agents taking that same action. The last case of condition d, however, says something different. It anticipates a feature that appears throughout the global games literature: multiplicity can never be entirely whipped out. Morris and Shin do not show that there is a unique equilibrium for *any* information structure. Even in their setup the set of distributions that admit multiplicity is nonempty. Rather, the importance of their result lies in the fact that those distributions that are in some sense "close" to the full information benchmark yield a unique equilibrium. Likewise, the model developed here cannot rule out multiplicity in *all* cases. Instead, it *shrinks* the set of priors that admit multiple equilibria.

We begin with the full information and common information benchmarks without persuasion. These are basic results and the proposition is stated without a proof. They are presented here as a contrast to the case where there is a central bank that can engage in persuasion. They are collected both as a proposition, and represented graphically.

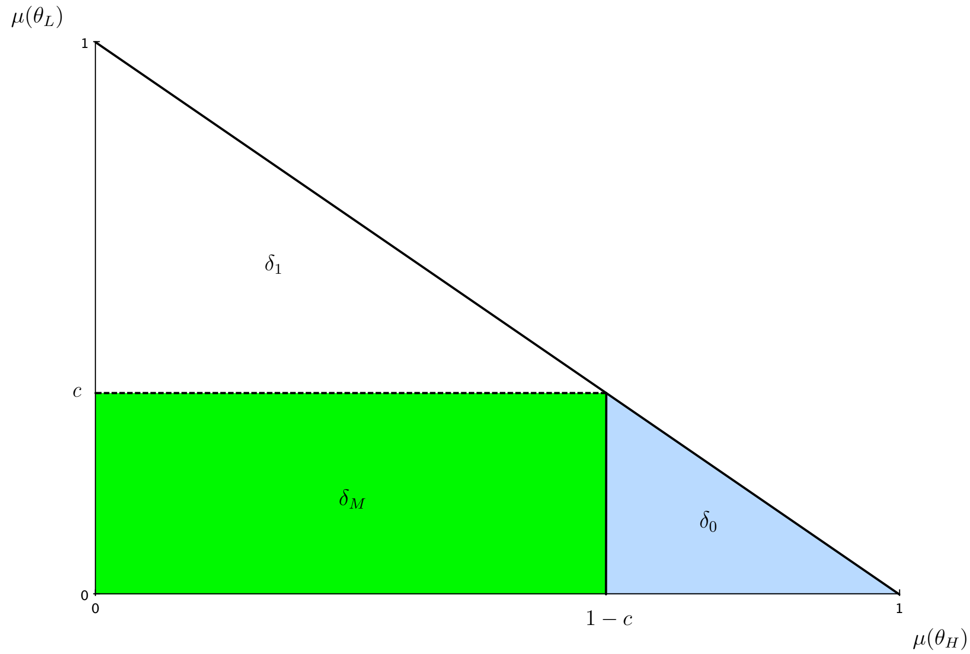


Figure 31: $\Delta(\Theta)$

Proposition 2. *Let agents share a commonly known prior* $\mu_0 \in \Delta(\Theta)$ *and partition the simplex* $\Delta(\Theta)$ *into the following three regions:* $\delta_0 \equiv \{\mu(\theta) : \mu(\theta) \in \Delta(\Theta),\ \mu(\theta_H) \geq 1-c\}$, $\delta_1 \equiv \{\mu(\theta) : \mu(\theta) \in \Delta(\Theta),\ \mu(\theta_L) > c\}$, *and* $\delta_M \equiv \Delta(\Theta) \setminus (\delta_0 \cup \delta_1)$.

1. *If* $\mu_0 \in \delta_0$ *there is a unique equilibrium:* $\pi_A = a = 0$.

2. *If* $\mu_0 \in \delta_1$ *there is a unique equilibrium:* $\pi_A = a = 1$.

3. *If* $\mu_0 \in \delta_M$ *there are multiple equilibria. In one equilibrium* $\pi_A = a = 0$*. In the other* $\pi_A = a = 1$.

The sets δ_0 and δ_1 contain those beliefs under which agents will never attack and always attack, respectively. The beliefs in these sets are so strong relative to the cost of attacking, c, that they swamp the coordination motive of agents. In the case of δ_0, recall that agents will not attack if and only if $c \geq \mu(\delta_L) + \mu(\delta_M)\pi_A$. By setting $\pi_A = 1$, we isolate those beliefs for which agents find it optimal not to attack even when *everybody else* attacks. Similarly, by setting $\pi_A = 0$ in equation 3.1 we isolate those beliefs for which it is optimal to attack even when no other agents attack. For all other beliefs—those in δ_M—the coordination motive matters and we have multiplicity.

Figure 1 above gives a graphical representation of proposition 1 using a Machina Triangle in $(\mu(\theta_H) \times \mu(\theta_L))$ space. The blue (δ_0) and white (δ_1) regions contain beliefs that admit a unique equilibrium while the green region (δ_M) contains those beliefs that give rise to multiplicity. Notice that the full information benchmarks are contained in their respective regions of the triangle. The origin, which represents $\mu(\theta_M) = 1$ is in δ_M. The beliefs that are consistent with a commonly known weak fundamental—$\mu(\theta_L) = 1$—are depicted in $(0, 1) \in \delta_1$. And correspondingly $\mu(\theta_H) \in \delta_0$ for the strong fundamental.

The Central Bank's Problem

Figure 31 also highlights the problem faced by the central bank when it engages in persuasion. In Kamenica and Gentzkow's model the persuader has an objective function from the probability space to the reals. The presence of multiplicity in this case, however, means that the central bank does not have a payoff function as suggested in equation (2.2) but rather a payoff correspondence $v : \Delta(\Theta) \rightrightarrows \mathbb{R}$.[2] More precisely, the set of payoffs over δ_M is not a singleton.

$$v(\mu) = \begin{cases} \mu_0(\theta_H) \text{ if } \mu \in \delta_1 \\ \\ \{\mu_0(\theta_H), 1\} \text{ if } \mu \in \delta_M \\ \\ 1 \text{ if } \mu \in \delta_0 \end{cases} \tag{3.3}$$

This raises the question of how to proceed in solving the bank's persuasion problem when its payoff is a correspondence. Clearly the central bank is facing a game of incomplete information where agents are of two "types": $\{\pi_A = 1,\ \pi_A = 0\}$. What is less clear is how to transform the game into one of imperfect information. What kind of beliefs should the bank have, or form, about the two types of agents of agents it might face? Introducing an objective prior $q \equiv \Pr(\pi_A = 1)$ over the the type space a la Harsanyi is not particularly satisfactory. For one, it is equivalent to introducing a sunspot variable that agents can use as a coordination device. But then one could always introduce the sunspot variable in a global game with incomplete information to resolve the issue of multiplicity. There is also no reason to think that the bank and the agents would share beliefs about the equilibrium that would be selected in the case of multiplicity. And more practically, how does one interpret q in the context of a currency crisis?

[2]Notice that correspondence 3 makes more precise the payoffs specified in equation 2. The term π_A is not explicitly written in equation 2, even though it also matters for the bank's payoff. For clarity of exposition, equation 2 simply expresses the bank's payoffs as a function of *only* those beliefs which it can control. Since the issue of multiplicity arises for beliefs outside its control, the fact that $v(\cdot)$ is a correspondence and not a function was not treated in section 2, and only addressed now.

Ultimately, applying the Harsanyi trick misses the point—it solves the problem of multiplicity by assuming it away. Yet central bankers face the issue of how their words might be interpreted when different outcomes are possible. The bank's challenge is that it must solve a decision problem under ambiguity and not under risk. In reality, central bankers cannot generally rely on a sunspot as an equilibrium-selecting mechanism. Therefore imposing an exogenous probability q deprives the model of its ability to offer policy solutions.

What we're after, then, is a model that incorporates the ambiguity faced by the central bank into its decision problem. Following Hansen and Sargent (2008) and Woodford (2010), I consider the equilibrium outcomes under a central bank that conducts *robustly optimal policy*. By robustly optimal I mean a bank that solves a maximin problem—the policy is robust in the sense that it is optimal (max) under the worst possible scenario (min). Specifically in the context studied here, the robustly optimal policy is one that is optimal when, in the face of multiplicity, agents coordinate on "attack."

The notion of robustness adopted here has both normative and positive content. From a normative standpoint, it is appealing for policymakers to enact policies that are optimal under the worst-case scenario if they are unsure about how likely each alternative scenario is. A policy that can do relatively well in *all* cases has the flavor of being more prudent than one which can do very well in some situations but also very poorly in others. The central banker knows that when beliefs fall in δ_M the regime can either fall or survive but can go no further and assign probabilities to each outcome. It seems sensible for her to design a policy that is robust to agents attacking in the face of multiplicity; certainly more so than for her to "hope for the best" and assume that agents will not attack only to find herself unprepared if they do.

Instead of interpreting the robustly optimal policy as one that the central bank *ought* to adopt, it can also be interpreted as the policy that an ambiguity-averse bank *will* adopt. The Ellsberg paradox is perhaps the sharpest example that there is an economically meaningful difference between attitudes toward ambiguity and attitudes toward risk; it shows that agents dislike facing unknown odds. The model here applies to an ambiguity averse – as opposed to an expected utility maximizing – central bank. There are several ways to axiomatize ambiguity aversion; these lead to different objective functional forms. However, models of ambiguity aversion tend to agree on the fact that agents behave conservatively in that they solve a maximin problem. Hansen and Sargent and Woodford consider an objective function that includes entropy as an additively separable convex loss. Here, the central bank is modeled as a Gilboa-Schmeidler ambiguity averse agent. Instead of a single prior q – recall that q is the bank's belief that the "attack" equilibrium is selected – the bank has a set of priors $Q \equiv [\underline{q}, \bar{q}]$ and chooses μ to maximize $\min_{q \in Q}\{\mathrm{E}_q(v(\mu))\}$. The next section shows how the bank can evaluate its ex ante utility by persuading. By applying the results from the next section tedious algebra shows that if $\bar{q} \geq (1-c)/(\mu_0(\theta_H) + (1-c))$ then the central bank will want to choose the *robustly optimal* policy. To sum up, the central bank presented here can be interpreted as an

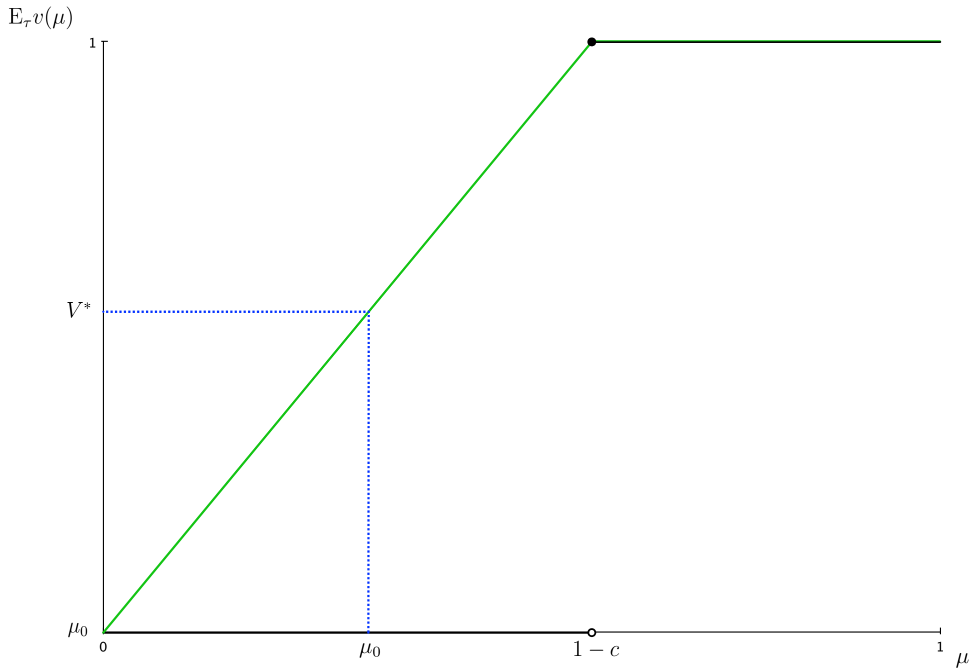


Figure 32: Bayesian Persuasion

ambiguity averse agent that believes there is at least a "high enough" chance that in the face of multiplicity agents will attack in equilibrium.

Robustly Optimal Persuasion

One of the features of the robustly optimal policy is that it collapses the dimensionality of the problem. The bank is designing policy by assuming that agents will attack under any beliefs that would imply multiplicity. The bank's problem therefore divides the simplex into only two regions. The first region contains those beliefs that imply an aggregate attack: $\mu \in \delta_1 \cup \delta_M$. The second are those that imply no attack: $\mu \in \delta_0$. This reduces the simplex from two dimensions to one, and allows us to graph the bank's payoff as a function of $\mu(\theta_H)$ in Figure 22. We then proceed by applying the tools from Kamenica and Gentzkow to solve the problem graphically.

Figure 32 graphs the bank's payoff (in black) and its concave closure (in green) as a function of $\mu(\theta_H)$. The first thing to notice is that for all beliefs $\mu(\theta_H) \geq 1-c$ the bank's payoff is one. Referring back to figure 1, it is clear all such beliefs fall in the set δ_0 under which agents do not attack regardless of the state. Since the regime never falls in this region, the bank receives a payoff of one. For all beliefs not in δ_0, the bank assumes agents will attack. But then the bank's ex ante payoff is simply the probability of withstanding the attack, which is the prior on the state being high $\mu_0(\theta_H)$.

Clearly the bank will not engage in persuasion if the prior lies in δ_0 since the regime survives with probability one. Figure 32 shows that when the prior does not lie in δ_0 the bank can choose an appropriate convex combination between beliefs $\mu(\theta_H) = 1-c$ and $\mu(\theta_H) = 0$ with its expectation at the prior, $\mu_0(\theta_H)$, to achieve an ex ante payoff of $V^* > \mu_0(\theta_H)$. As Kamenica and Gentzkow show, the bank can achieve this by choosing two signals; one which says "the fundamental is high," and another one which says "the fundamental is not high." When agents hear the former they update their belief to $\mu(\theta_H) = 1-c$: just enough to leave them indifferent between attacking and not, right on the boundary of set δ_0. Recall that our bank-preferred equilibrium concept makes $\delta_0 \in [0,1] \times [0,1]$ a compact set so agents do not attack. When agents hear the latter message, by contrast, they are sure that the fundamental is not strong, i.e. $\mu(\theta_H) = 0$.

Let s_1 and s_2 refer to the "high" and "not high" signals, respectively. Bayes plausibility imposes the following constraint on the distribution over posterior beliefs

τ:

$$\tau\mu(\theta_H|s_1) + (1-\tau)\mu(\theta_H|s_2) = \mu_0(\theta_H) \tag{3.4}$$

Yet Figure 32 suggests that $\mu(\theta_H|s_2) = 0$ which in turn implies that $\mu(s_2|\theta_H) = 0$ by bayesian updating and $\mu(s_1|\theta_H) = 1$ by the law of total probability. Combining this with the the fact that $\mu(\theta_H|s_1) = 1-c$ and substituting into equation (3.4) gives us that the unconditional probability of observing s_1 is $\tau = \mu_0(\theta_H)/(1-c)$. Equating this result with the definition of unconditional probability $\tau = \mu_0(\theta_H)/(1-c) = \mu(s_1|\theta_H)\mu_0(\theta_H) + \mu(s_1|\{\theta_M \cup \theta_L\})(1-\mu_0(\theta_H))$ we can solve for the conditional distribution of s_1 to get the following signal structure:

$$\mu(s_1|\theta_H) = 1 \qquad \mu(s_1|\{\theta_M \cup \theta_L\}) = \left(\frac{c}{1-c}\right)\left(\frac{\mu_0(\theta_H)}{1-\mu_0(\theta_H)}\right)$$

$$\mu(s_2|\theta_H) = 0 \qquad \mu(s_2|\{\theta_M \cup \theta_L\}) = \frac{1-(c+\mu_0(\theta_H))}{(1-c)(1-\mu_0(\theta_H))}$$

It is worth noting, given the dependence of the signal structure on the cost of attacking, c, that $\mu(s_1|\{\theta_M \cup \theta_L\})$ remains well-defined even as c gets close to one. The reason is that even though the first term, which depends on c goes to infinity, the second term goes to zero. Figure 31 depicts how the locus of beliefs $\mu(\theta_H) = 1-c$ divides the simplex between the sets that correspond to "attack" and "not attack." As c gets larger, intuitively, the set of beliefs that would lead agents to attack must shrink since attacking has become more costly. But then the central bank would only feel the need to engage in persuasion if the prior beliefs on the state being θ_H are sufficiently close to zero.

The "fundamental is high" signal is vague in the sense that agents do not know with certainty whether the fundamental is actually θ_H. That is, the bank will be vague when it comes to averting an attack, but precise when it comes to inducing it. Notice, also, that even though there are three types of fundamentals, the bank only uses two signals. The reason is that there are only two actions and it is the actions – not the state of the world – that matter to the central bank. Nor would a third signal be able to resolve the multiplicity in the central bank's favor. Consider a third message along the lines of "the fundamental is θ_M but no one else is going to attack." In equilibrium, it must be the case that agents *optimally* follow the central bank's recommendation. The problem with such a message is that agents could just as easily use the information that the fundamental is intermediate and coordinate on attacking the regime. The reason why the bank can send credible signals about the fundamentals but not π_A is simple: the bank can use backward induction for the former, but must rely on forward induction for the latter.

Having solved the robust bank's problem, I now turn to how agents interpret the signals. The case of s_1 is trivial because it induces beliefs that lead all agents to not attack. But now suppose that an agent hears s_2 instead. In that case he knows for sure that the state is *not* θ_H but nothing beyond that. Although the central bank makes no attempt to differentiate between θ_M and θ_L, agents still update those beliefs after observing s_2. It is precisely this updating that gives us the following result:

Proposition 3. *For any cost $c \in (0, 1)$ persuasion shrinks the set of priors that admit multiple equilibria.*

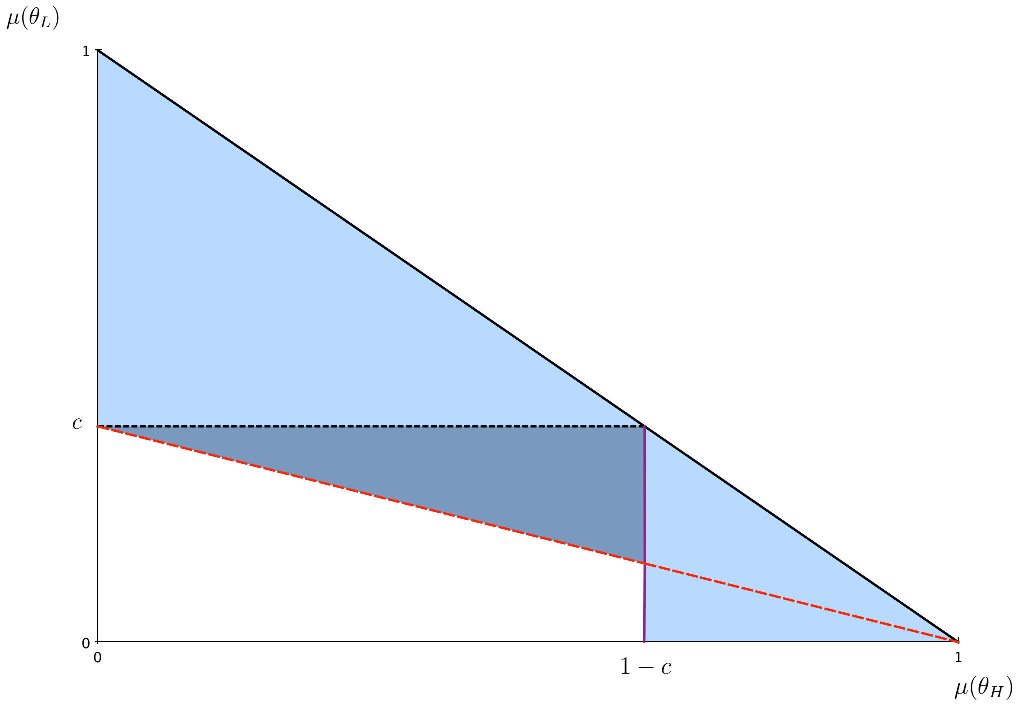


Figure 33: Multiplicity and Uniqueness

Proof. The proof is exceedingly simple and it comes from equation 1. Concern for robustness means that signal 1 is such that $\mu(\theta_L|s_1) + \mu(\theta_M|s_1) = 1 - \mu(\theta_H|s_1) = c$. So we know that regardless of the prior, agents will never attack after observing s_1. After observing signal 2, however, agents update according to bayes' rule as follows:

$$\mu(\theta_L|s_2) = \frac{\mu_0(\theta_L)}{1-\mu_0(\theta_H)} \qquad\qquad \mu(\theta_M|s_2) = 1 - \mu(\theta_L|s_2) \tag{3.5}$$

We are now after those prior beliefs that will lead agents to attack after observing signal 2. To do this we substitute the updates from equation (3.5) into equation 1 and set $\pi_A = 0$. That is, we want to isolate those priors under which it is optimal to attack even when nobody else does after observing s_2. This gives us the following condition:

$$\mu_0(\theta_L) > c\,(1-\mu_0(\theta_H)) \tag{3.6}$$

The first thing to note is that the entire set δ_1 satisfies condition (3.6). That means that the set of priors that admit multiplicity under persuasion must be a subset of the set of priors that admit multiplicity without persuasion. We now show that it is, in fact, a strict subset. This can be done by noting that $\forall\ c \in (0,1) \Rightarrow c^2 < c$. But then there are priors that satisfy both condition (3.6) and lie in set δ_M. This set is shaded in dark blue in figure 33. $\square$

Figure 3 shows the set of priors that lead to a unique equilibrium under persuasion: the regions in both dark and light blue. The priors above the red line satisfy condition 6. The top light blue triangle is set δ_1. The light blue triangle in the bottom right is set δ_0. The reason why the beliefs that lie below the red line in δ_0 still lead to a unique equilibrium is that the bank does not engage in persuasion for any priors that lie in δ_0. The interesting region in figure 33 is the dark blue triangle which hold priors that would lead to multiplicity in the absence of persuasion. Under persuasion, posterior beliefs will fall on two locci. The first is the vertical axis, which contains all the points that correspond to the origin in figure 32. The second is the purple vertical line through $1-c$, which contains all the points that correspond to the point $(1-c,\ 0)$ in figure 32. For all of the priors in the dark blue triangle, updating on signal s_2 will lead to posteriors on the vertical axis above c, which are in the set δ_1. The central bank, therefore, is able to break multiplicity not by managing higher order beliefs – which it never attempts to control – but by rendering them irrelevant through its robustly optimal signal structure.

Even in the white region of figure 33, where multiplicity prevails it is still diminished. Without persuasion, any prior belief in the white region leads to multiplicity with probability one. But under persuasion, agents will receive signal s_1 with probability $\tau \geq 0$. Since s_1 leads to posterior beliefs that imply a unique equilibrium where agents do not attack for *any* prior, priors in the white region of figure 33 lead to multiplicity only with probability $1 - \tau \leq 1$. The only priors under persuasion that lead to multiple equilibria with probability one are those on the vertical axis of figure 3 between the origin and $(0,\ c)$. For these priors, agents know with certainty that the fundamental is not high; the bank can persuade but not fool, so $\tau = 0$ for beliefs on the vertical axis. To sum up, for *almost all* priors multiplicity will arise with probability less than one. For many of them, of course, that probability goes all the way down to zero.

The Fundamental on a Continuum

The model developed thus far differs from the classic global game of regime change in restricting the values of the fundamental. This section shows that under the assumption of common information this restriction has no bite. Symmetric equilibria under common information requires all agents to take the same action. Since the fundamental does not directly enter into the payoffs of agents or the central bank its actual value does not matter—only the region under which the fundamental falls.

If we allow θ to take any value in $\mathbb{R}$ trivially nothing changes if agents can observe θ. Now consider the case where agents only have a prior cdf $F(\theta)$. When

$\theta \in \mathbb{R}$, equation 1 becomes

$$c < \int_{-\infty}^{0} dF + \int_{0}^{1} \pi_{A|i} \, dF$$

But common information means that $\pi_{A|i}$ is independent of θ, so it can be taken out of the integral, which yields: $c < F(0) + [F(1) - F(0)] \, \pi_{A|i}$. This condition is essentially the same as equation 1. As an example, take the region $\theta \geq 1$. From an agents' perspective, it is irrelevant whether the fundamental has a value of two or three. What matters is the threshold $\theta = 1$ after which attacking becomes futile. In the continuous case with common information only $F(0)$ and $F(1)$ matter, so all we need to consider are the sets $\theta_L \equiv \{\theta \in \mathbb{R} : \theta < 0\}$, $\theta_M \equiv \{\theta \in \mathbb{R} : \theta \in [0, 1)\}$ and $\theta_H \equiv \{\theta \in \mathbb{R} : \theta \geq 1\}$. The first set has measure $\mu(\theta_L) \equiv F(0)$, the second has measure $\mu(\theta_M) \equiv F(1) - F(0)$ and the last set has measure $\mu(\theta_H) \equiv 1 - F(1)$. Importantly, this simplifies the case of $\theta \in \mathbb{R}$ because it allows us to conduct the analysis in the two-dimensional simplex as was done in sections 2 and 3.

By extending the values that the fundamental can take to $\mathbb{R}$ the uniqueness results obtained here can be compared more directly with those in Morris and Shin and AHP. Both Morris and Shin and AHP begin with an improper uniform prior with full support on the real line. Since, as was mentioned before, the central bank's bayesian persuasion problem requires a well-defined prior, I begin here with a uniform distribution over the interval $[0, 1]$ and allow its variance, σ to diverge. This distribution on $\mathbb{R}$ is equivalent to starting with probability $\mu(\theta_M) = 1$ in the discrete case. Graphically, this corresponds to the origin in figures 1 and 3. As $\sigma \to \infty$, any finite interval on the real line will have measure zero so $\mu(\theta_M) \to 0$. Because we approach the limit by flattening the uniform distribution symmetrically, $\forall \sigma > 0$ $\mu(\theta_H) = \mu(\theta_L)$. This sequence of priors traces a path along the 45 degree line from the origin to the hypotenuse in figures 1 and 3. In this case, in the limit we have $\mu(\theta_H) = \mu(\theta_L) = 1/2$ and $\mu(\theta_M) = 0$.

Without persuasion if $c < 1/2$ the sequence of uniform priors will eventually cross the threshold $\mu(\theta_L) = c$ and the improper uniform lies in δ_1. In contrast, if $c > 1/2$ the sequence of priors will end up crossing the threshold $\mu(\theta_H) = 1 - c$ and the improper uniform lies in δ_0. When $c = 1/2$, though, the sequence of priors is *always* in the interior of δ_M and we get uniqueness in the limit as a knife-edge condition: any prior with *finite* support over $\mathbb{R}$ leads to multiplicity. This is not the case under robustly optimal persuasion.

Consider figure 3 once again. Because the improper prior will put measure zero on θ_M, we know that it will lie on the hypotenuse of the triangle; where on the hypotenuse, however, will depend on what sequence $\{F_n\}$ we use to obtain the improper uniform in the limit. As both the example of the uniform distribution above as well as figure 1 show, in the absence of persuasion, it is possible to approach the improper uniform without ever leaving set δ_M. Figure 3 makes it clear that under robustly optimal persuasion that is not possible—the hypotenuse of the triangle is in the *interior* of the set of uniqueness.

We can now compare the results obtained here directly with those of Morris and Shin as well as those of AHP. If we begin with $\theta \in \mathbb{R}$, as we approach the improper uniform on $\mathbb{R}$ – the distribution with which both of those papers begin – the endogenous public signal chosen by the central bank achieves a unique equilibrium regardless of the cost of attacking. With respect to Morris and Shin, this result suggests that common knowledge alone is not the cause of multiplicity. We can begin with a sequence of priors that always imply multiplicity and yet the endogenous public signal will still break this multiplicity even though information is common across agents. With respect to AHP, endogenous public information need not lead to multiple equilibria. Further, the central bank can avoid the pitfalls of policy traps if it focuses on its words rather than its actions.

Conclusion

This paper has presented a currency attack model with common information and an endogenous public signal. The public signal is purposeful communication by the central bank about the strength of the fundamental θ. The central bank considered here is both ambiguity averse – it chooses a *robustly* optimal policy – and has access to a commitment technology so that it can engage in Bayesian Persuasion.

By choosing its optimal signal structure, the bank can with positive probability save the regime from falling even when the prior would lead to an attack with probability one. Persuasion by the bank also shrinks the set of prior beliefs under which multiple equilibria arise. Importantly, the set of multiplicity will shrink even with no private information. And as the prior becomes more diffuse, persuasion by the bank will lead to a unique equilibrium for any cost of attacking even when that would not be the case without persuasion. The fact that policy-as-communication can lead to uniqueness and to a lowered ex ante probability of the regime falling contrasts sharply with the multiplicity that arises with policy-as-action considered by AHP. Especially with their finding of an inactive-policy equilibrium in which the bank does not attempt to save the regime. The central bank in this model will always attempt to persuade except when agents know the value of the fundamental with certainty. The results here suggest that perhaps central banks should rely more on speaking than actions and agrees, at least anecdotally with the response of markets to announcements by credible central banks versus actions, as the example of the ECB shows. It also highlights the importance of credibility. The fact that the bank can commit to a signaling policy is key. Without this, uniqueness would not be obtained.

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